Abstract

The regression discontinuity (RD) design is one of the most popular quasi-experimental methods for applied causal inference. In practice, the method is quite sensitive to the assumption that individuals cannot control their value of a “running variable” that determines treatment status precisely. If individuals are able to precisely manipulate their scores, then point identification is lost. We propose a procedure for obtaining partial identification bounds in the case of a discrete running variable where manipulation is present. Our method relies on two stages: first, we derive the distribution of non-manipulators under several assumptions about the data. Second, we obtain bounds on the causal effect via a sequential convex programming approach. We also propose methods for tightening the partial identification bounds using an auxiliary covariate, and derive confidence intervals via the bootstrap. We demonstrate the utility of our method on a simulated dataset.

1 Introduction

The regression discontinuity (RD) design is a vital analytic tool for social scientists. First introduced by [Thistlethwaite and Campbell (1960)], the RD design gained popularity due to its applicability to a wide variety of non-experimental settings. RD designs have been used to infer the effect of health insurance on neonatal hospital stays (Almond and Doyle [2011]), the effect of college quality on students’ postsecondary enrollment choices (Cohodes and Goodman [2014]), and the effect of incumbency on U.S. House election outcomes (Lee [2008]), to name just a few.

The method exploits scenarios in which each unit has an associated score (the “running variable”), and treatments are assigned based on whether the
score falls above or below a threshold. Under the assumption that units are unable to precisely manipulate their score near the threshold, treatment assignment is as good as random in a narrow window around this cutoff. This quasi-randomization can be exploited to infer local average treatment effects without the necessity of running a randomized experiment.

The RD design relies on the no-precise-manipulation assumption, but this can be problematic in practice. Gerard et al. (2015) document numerous scenarios in which plausible regression discontinuities show evidence of manipulation. These include teachers manipulating student test scores to meet performance standards in New York City (Dee et al., 2016), and students manipulating credits in order to be eligible for a college scholarship in West Virginia (Scott-Clayton, 2011).

If perfect manipulation is present, we can no longer assume that there is no systematic difference between units just above and just below the threshold. As a result, point identification of a local average treatment effect is lost. But if the population is composed of some manipulators and some non-manipulators – and we estimate the prevalence of each – we can still hope to do valid causal inference on the sub-population of non-manipulators.

In this paper, we propose a partial identification approach in RD designs where manipulation is present. Our approach is designed for the case in which the running variable is discrete. We propose to first generate an estimate of the “un-manipulated” density, using a technique proposed by Diamond and Persson (2016). Under stated assumptions, we can subsequently derive the relative densities of manipulators and non-manipulators. We then pose the problem as an optimization, in which we can derive treatment effect bounds as the best- and worst-case treatment effect estimates consistent with these densities. Our method draws on other partial identification approaches proposed in the literature.

The remainder of this paper proceeds as follows. In Section 2, we review relevant literature on regression discontinuity designs and partial identification of causal effects. Our proposed procedure is described in detail in Section 3. In Section 4, we propose methods for tightening the partial identification bounds by making use of auxiliary covariates. We demonstrate the utility of these methods via simulations in Section 5. Section 6 concludes.

2 Literature Review

Since its introduction in the mid-twentieth century, the RD design has yielded a broad literature covering both applications and methodological developments. Perhaps the most fundamental question is how to estimate conditional means just below and just above the cutoff. Local polynomial regression approaches have received substantial attention in recent decades (Hahn et al., 2001; Imbens and Lemieux, 2008), with particular focus on the selection of tuning parameters such as the choice of kernel and the smoothing bandwidth. More recently, Imbens and Wager (2018) proposed a minimax linear estimator, which obviates
the need for some of these choices. Yet, the regression-based methods remain appealing due to their simplicity and interpretability.

Violations of the RD design assumptions have received comparatively less attention. McCrary (2008) introduced an intuitive test for identifying the presence of manipulation, based on examination of the running variable density. Suppose the treatment is assigned to those with scores above the cutoff, and the treatment is desirable. In the absence of manipulation, we would expect to see a continuous running variable density at the cutoff. However, if manipulation were occurring, we would expect to see a discontinuity, resulting from individuals just below the cutoff manipulating their scores to just above the cutoff in order to secure the treatment. McCrary suggests an empirical hypothesis test for this density discontinuity, making use of a density estimator originally proposed by Cheng et al. (1997). Alternative density estimators have subsequently been proposed by both Otsu et al. (2013) and Cattaneo et al. (2015). Frandsen (2017) extended this approach by developing an alternative test for manipulation for the case of a discrete running variable. These methods have been widely adopted in the applied literature as a falsification test for checking the assumption of no manipulation.

There is comparatively little empirical work describing how to proceed when these tests indicate the presence of manipulation. Diamond and Persson (2016) consider Swedish math test data in which there is evidence of teachers inflating students’ grades. They develop an estimator to determine the causal effect of the score manipulation on future educational attainment and earnings. While their focus is on a different causal effect than the one we consider, the paper develops several useful methods that will be incorporated here.

We pursue the “partial identification” approach, popularized by Manski and later by Tamer (see e.g., Manski et al. 1989; Manski and Tamer 2002; Haile and Tamer 2003). The core idea is that, in scenarios in which a treatment effect cannot be point identified (even with an infinite sample size), it can still sometimes be bounded. These bounds might be very informative in practice – for example, allowing us to rule out negative or positive treatment effects.

Gerard et al. (2015) also take a partial identification approach to analyzing RD designs in the presence of manipulation. Their method posits the existence of subpopulations of manipulators and non-manipulators and defines the causal effect on the non-manipulators as the inferential target. We adopt the same framework but differ in our estimation technique. Particularly, Gerard and co-authors extend McCrary’s result to estimate the proportion of manipulators at the cutoff \( c \). They then propose a “polynomial truncation” approach to estimation, which implicitly assumes all manipulated units lie at the top or bottom of the distribution of outcomes within a bandwidth \( h \) above the threshold. We avoid making such an assumption by estimating the manipulator counts at all values of the running variable and explicitly assuming that it is discrete.
3 Proposed Procedure

Our contribution is a novel optimization procedure for estimating partial identification bounds on causal effects in RD designs with a discrete running variable. We require two preliminary steps: testing for the presence of manipulation, and estimating the un-manipulated density to derive the counts of manipulators and non-manipulators. These steps can be accomplished using existing methods.

3.1 Notation and Initial Assumptions

Our data consists of $i = 1, \ldots, N$ units. We associate with each unit $i$ a pair of unseen potential outcomes $(Y_i(0), Y_i(1)) \in \mathbb{R}^2$, corresponding to the value of the outcome if unit $i$ does not or does receive the treatment, respectively. We also associate with each unit an observed value of the running variable $\tilde{X}_i$ and a true, unobserved value of the running variable $X_i$.

We have a treatment assignment $W_i \in \{0, 1\}$ and a running variable cutoff value $c$ such that $\tilde{X}_i \geq c$ implies $W_i = 1$. In other words, this is a sharp, rather than fuzzy, RD design, but treatment assignments are based on the observed running variable rather than the true running variable. We observe

$$Y_i = W_i Y_i(1) + (1 - W_i) Y_i(0)$$

the outcome for each unit. Our estimand of interest is

$$\tau(c) = E(Y_i(1) - Y_i(0) \mid X_i = c)$$

where the expectation is with respect to the super-population from which our data is sampled.

We define an indicator variable $Z_i \in \{0, 1\}$ such that $Z_i = 1$ if $X_i = \tilde{X}_i$ and $Z_i = 0$ otherwise ($Z_i$ defines whether this is an “honest” subject as opposed to a manipulator). Both $X_i$ and $\tilde{X}_i$ lie in the set $\mathcal{X} = \{x_1, x_2, \ldots, x_n\}$, a discrete set of running variable values, where $c \in \mathcal{X}$. We also denote the counting functions

$$T(x) = \sum_{i=1}^{n} I(X_i = x) \quad \tilde{T}(x) = \sum_{i=1}^{n} I(\tilde{X}_i = x)$$
$$M(x) = \sum_{i=1}^{n} I(X_i = x, Z_i = 0) \quad \tilde{M}(x) = \sum_{i=1}^{n} I(\tilde{X}_i = x, Z_i = 0)$$
$$H(x) = \sum_{i=1}^{n} I(X_i = x, Z_i = 1) \quad \tilde{H}(x) = \sum_{i=1}^{n} I(\tilde{X}_i = x, Z_i = 1)$$

where $T(x) = M(x) + H(x)$ and $\tilde{T}(x) = \tilde{M}(x) + \tilde{H}(x)$. Here, $M(\cdot)$ represents the manipulator distribution and $H(\cdot)$ the “honest” (non-manipulator) distribution. $H(x)$ is assumed to evolve smoothly with $x$ while there is no such assumption on $M(x)$.

We also define the quantities

$$N_{\ell} = \sum_{x < c} \tilde{T}(x), \quad N_r = \sum_{x \geq c} \tilde{T}(x)$$

observing $N = N_{\ell} + N_r$. For convenience, we assume the indices are assigned such that $\tilde{X}_i < c$ for $i = 1, \ldots, N_{\ell}$ and $\tilde{X}_i \geq c$ for $i = N_{\ell} + 1, \ldots, N$. 

4
We make a somewhat restrictive shape assumption about the shape of the density of the \( X_i \).

**Assumption 1** The un-manipulated density is log-concave, and the densities of the missing and excess mass (to the left and right of each threshold) are log-concave and monotonic.

This assumption appears in Diamond and Persson (2016), who argue that it is necessary for an approximate recovery of the un-manipulated density. Without it, any observed bunching in the density could simply be attributable to a bumpy un-manipulated density rather than the effect of the manipulation. Fortunately, the assumption is still quite general and allows for the density to follow many commonly used probability distributions, such as the normal, Gumbel, gamma, beta, and logistic (Diamond and Persson, 2016).

### 3.2 Testing for Manipulation

Many methods exist for validating the RD design, including density tests and tests for covariate balance just above and below the threshold (Lee and Lemieux, 2010). Rejections of the null in these tests can be used to falsify the RD design by identifying behavior that would be implausible in the case of treatment randomization near the threshold value of \( X \).

Our methods are only valid for the setting in which the un-manipulated density is recoverable from the observed density. Unsurprisingly, then, we rely on settings where evidence of the manipulation is manifest in the shape of the observed density. In cases where manipulation is plausible, we thus suggest the use of a density test as the first step in our procedure. The McCrary test (McCrary, 2008) is the standard approach, though the test advocated by Frandsen (2017) may be preferable for a discrete running variable \( X_i \in X \) if \( X \) contains only a moderate number of distinct values.

If the null hypothesis is rejected by the density test, we suggest conducting further confirmatory falsification tests. A standard approach is to use baseline covariates as placebo outcomes and determine whether a causal effect would have been estimated at the cutoff for these outcomes (see e.g., Sekhon and Titunik, 2016). If an abnormally high number of covariates show statistically significant discrepancies across the threshold, this further indicates the plausibility of manipulation.

### 3.3 Estimation of the True Density and the Non-Manipulator Counts

Once manipulation is established, we seek to estimate the density of the true running variable \( X_i \) from the observed running variable \( \tilde{X}_i \). Multiple methods could be deployed for this task.

So-called “bunching strategies” for analyzing manipulated distributions have ample precedent in the economics literature. For example, various models have
been proposed to assess underlying income distributions when reported incomes are manipulated (Kleven and Waseem, 2012; Chetty et al., 2013). The statistics literature also provides methods for recovering the underlying distributions. Lindsey’s Method (Efron, 2012), in which a Poisson regression is fit to histogram heights in the un-manipulated section of the distribution, is a simple and intuitive technique.

We are partial to the technique used by Diamond and Persson (2016), which has the advantage of simultaneously estimating the width of the “manipulation region” (the radius around the cutoff in which manipulation takes place) and also the un-manipulated density. Their method performs a grid search over the possible widths of the manipulation region. For each potential value, non-linear least squares is used to estimate the un-manipulated distribution as a linear combination of exponentiated Bernstein polynomials of the running variables, with a linear inequality constraint on the coefficients to enforce log-concavity of the un-manipulated density. A cross-validation procedure is used to identify the optimal width and optimal polynomial degrees based on out-of-sample predictions.

Whether by the Diamond and Persson technique or another method, the output should be \( \hat{T}(x) \), an estimate of the counting function \( T(x) \). To identify the count of non-manipulators at each value of \( x \), we make a further assumption.

\[ \text{Assumption 2} \quad M(x) > 0 \implies \hat{M}(x) = 0 \quad \text{and} \quad \hat{M}(x) > 0 \implies M(x) = 0. \]

This assumption is somewhat restrictive. It implies that if there is some \( x \) at which there are manipulators for whom \( X = x \), there cannot be any manipulators for whom the manipulated \( \tilde{X} = x \). Similarly, if there are some manipulators for whom \( \tilde{X} = x \), then there are no manipulators for whom \( X = x \). In practical settings, this will look like a monotonicity constraint i.e., any manipulator for whom \( X_i < c \) will successfully manipulate such that \( \tilde{X}_i \geq c \) and no manipulator will manipulate in the opposite direction (for a desirable treatment); or vice versa (for an undesirable treatment).

Assumption 2 is very similar to Assumption 3 in Gerard et al. (2015), who note that this kind of “one-sided” manipulation is plausible as long as the treatment is unambiguously desirable or undesirable. We show an example of manipulation satisfying our assumptions in Figure 1.

Under Assumption 2, observe that

\[
\min(T(x), \tilde{T}(x)) = \min(H(x) + M(x), H(x) + \hat{M}(x)) \\
= H(x) + \min(M(x), \hat{M}(x)) \\
= H(x)
\]

Thus, we can estimate the non-manipulator count at each value \( x \) by computing

\[
\hat{H}(x) = \min(\hat{T}(x), \tilde{T}(x)).
\]
3.4 Optimization Problem

Many RDD causal effect estimators can be written as

$$\hat{\tau}(c) = (c^\star)^T \left( (D_r^T W_r D_r)^{-1} D_r^T W_r Y_r - (D_\ell^T W_\ell D_\ell)^{-1} D_\ell^T W_\ell Y_\ell \right),$$

where $D_\ell \in \mathbb{R}^{N_\ell \times p}$ and $D_r \in \mathbb{R}^{N_r \times p}$ are concatenated basis expansion of the running variables $X_i$ for units to the left and right of the cutoff, respectively; $c^\star \in \mathbb{R}^p$ is an analogous basis expansion of the cutoff $c$; and $W_\ell \in \mathbb{R}^{N_\ell \times N_\ell}$ and $W_r \in \mathbb{R}^{N_r \times N_r}$ are diagonal matrices representing unit-level weights. The popular local polynomial regression approach ([Hahn et al., 2001]) can be expressed in this form, as can spline formulations (see e.g. [Lemieux and Milligan, 2008]) and simpler unweighted regressions. Suppose we are using any such method for inference.

Our goal is to put partial identification bounds on $\hat{\tau}$. Observe that, were we to know the values of the $Z_i$ indicators, we could recover an estimate of the causal effect free from bias due to the manipulation. For bookkeeping, we collect the $Z_i$ values into vectors $Z_\ell \in \{0, 1\}^{N_\ell}$ and $Z_r \in \{0, 1\}^{N_r}$ such that $Z_{\ell i} = Z_i$, where $Z_{\ell i}$ is the $i^{th}$ entry in $Z_\ell$, and $Z_{r i} = Z_{i-N_\ell}$.

In practice, $Z_\ell, Z_r$ are unknown to the researcher, but we can impose certain constraints that they must satisfy. If we additionally knew the true non-manipulator counts $H(x)$, then for any choice of $x < c$ we would know

$$\sum_{i : X_i = x} Z_{\ell i} = H(x)$$
and for any choice of $x \geq c$

\[ \sum_{i: X_i = x} Z_{ri} = H(x). \]

We could collect these equalities into matrix equalities

\[ \mathbf{A}_\ell \mathbf{Z}_\ell = \mathbf{H}_\ell \quad \mathbf{A}_r \mathbf{Z}_r = \mathbf{H}_r. \]

where e.g.

\[ \mathbf{A}_\ell = \begin{pmatrix}
I(X_1 = x_1) & I(X_2 = x_1) & \cdots & I(X_{N_\ell} = x_1) \\
I(X_1 = x_2) & I(X_2 = x_2) & \cdots & I(X_{N_\ell} = x_2) \\
\vdots & \vdots & \ddots & \vdots \\
I(X_1 = x_{n_\ell}) & I(X_2 = x_{n_\ell}) & \cdots & I(X_{N_\ell} = x_{n_\ell})
\end{pmatrix} \]

and

\[ \mathbf{H}_\ell = (H(x_1), H(x_2), \ldots, H(x_{n_\ell}))^T \]

with analogous definitions for $\mathbf{A}_r, \mathbf{H}_r$. In practice, these equalities have to be approximated using the output of Section 3.3

\[ \mathbf{A}_\ell \mathbf{Z}_\ell = \hat{\mathbf{H}}_\ell \quad \mathbf{A}_r \mathbf{Z}_r = \hat{\mathbf{H}}_r. \]

where $\hat{\mathbf{H}}_\ell, \hat{\mathbf{H}}_r$ are the approximated analogues of $\mathbf{H}_\ell, \mathbf{H}_r$.

We can now write down the first iteration of our optimization problem. We solve for the upper partial identification bound via:

**Optimization Problem 1 Initial Formulation**

\[
\begin{align*}
\text{maximize} & \quad (c^*)^T \left( (D_\ell^T W_\ell^* D_\ell)^{-1} D_\ell^T W_\ell^* Y_\ell - (D_r^T W_r^* D_r)^{-1} D_r^T W_r^* Y_r \right) \\
\text{subject to} & \quad W_\ell^* = W_\ell \, \text{diag}(\mathbf{Z}_\ell), \quad W_r^* = W_r \, \text{diag}(\mathbf{Z}_r), \\
& \quad \mathbf{A}_\ell \mathbf{Z}_\ell = \hat{\mathbf{H}}_\ell, \quad \mathbf{A}_r \mathbf{Z}_r = \hat{\mathbf{H}}_r, \\
& \quad \mathbf{Z}_\ell \in \{0,1\}^{N_\ell}, \quad \mathbf{Z}_r \in \{0,1\}^{N_r}
\end{align*}
\]

Here, observe that $W_\ell^*$ is a diagonal weight matrix resulting from the product of weight matrix $W_\ell^*$, representing the weights used by our regression estimator (e.g. kernel weights), and $\text{diag}(\mathbf{Z}_r)$, our optimization variables. Since the $Z_i$ are boolean, any value $Z_i = 0$ has the effect of “turning off” observation $i$ under the assumption that it is a manipulator. Analogous definitions apply for $W_r^*$.

### 3.5 Solving the Optimization Problem

Optimization Problem 1 is a boolean optimization problem with a non-convex objective. To have any hope of solving this problem, we must relax the final two constraints to make them convex, via:
Optimization Problem 2 Relaxed Formulation

\[
\begin{align*}
\text{maximize} & \quad (c^\star)^T \left( (D_r^TW_r^*D_r)^{-1} D_r^TW_r^*Y_r - (D_r^TW_r^*D_r)^{-1} D_r^TW_r^*Y_r \right) \\
\text{subject to} & \quad W_r^* = W_r \text{ diag}(Z_r), \quad W_r^* = W_r \text{ diag}(Z_r), \\
& \quad A_r Z_r = \mathbb{H}_r, \quad A_r Z_r = \mathbb{H}_r, \\
& \quad Z_r \in [0, 1]^{N_r}, \quad Z_r \in [0, 1]^{N_r}
\end{align*}
\]

where the coordinates of $Z_r, Z_r$ are now confined to the unit interval rather than $\{0, 1\}$. Note that any solution of the relaxed problem for which the entries of $Z_r, Z_r$ lie in $\{0, 1\}$ will also be a solution to the original problem.

Define $Z = [Z_r, Z_r]$ as our optimization variable, the concatenation of $Z_r, Z_r$. Denote the objective as $f(Z)$. The non-convexity of $f(Z)$ poses a substantial challenge. We propose a computationally efficient method based on sequential convex programming [Fleury, 1989]. The algorithm can be described in a few simple steps.

1. Find a feasible point $Z$ satisfying all the constraints.

2. Repeat until convergence:

- Compute the linearization of $f$ at $Z$:

  \[
  f^*(Z) = f(Z) + \nabla f(Z)^T (Z - Z)
  \]

- Solve the linearized convex optimization problem

  \[
  \begin{align*}
  \text{maximize} & \quad f^*(Z) \\
  \text{subject to} & \quad W_r^* = W_r \text{ diag}(Z_r), \quad W_r^* = W_r \text{ diag}(Z_r), \\
& \quad A_r Z_r = \mathbb{H}_r, \quad A_r Z_r = \mathbb{H}_r, \\
& \quad Z_r \in [0, 1]^{N_r}, \quad Z_r \in [0, 1]^{N_r}
  \end{align*}
  \]

and denote its solution as $Z^*$.

- Set $Z \leftarrow Z^*$

The linearization in the above procedure can be computed efficiently by observing that

\[
\begin{align*}
\nabla f(Z) = \left(Y_r \circ D_r \left( D_r^TW_r^*D_r \right)^{-1} - Y_r \circ D_r \left( D_r^TW_r^*D_r \right)^{-1} \right) c^* - \\
\left( \left(D_r \left( D_r^TW_r^*D_r \right)^{-1} \circ D_r \left( D_r^TW_r^*D_r \right)^{-1} \right) c^*1^T \right) D_r^TW_r^*Y_r - \\
\left( \left(D_r \left( D_r^TW_r^*D_r \right)^{-1} \circ D_r \left( D_r^TW_r^*D_r \right)^{-1} \right) c^*1^T \right) D_r^TW_r^*Y_r
\end{align*}
\]

where $\circ$ denotes the Hadamard product and $1$ is the length $p$ vector of ones. Note also that because the optimization variable appears only in convex inequalities and affine equalities, we need not maintain a trust region [Duchi et al., 2018].
Sequential convex programming is a heuristic and typically yields feasible points with good, but not necessarily optimal, objective values. Hence, the bounds generated by our procedure may be conservative. Such intervals are still informative if, for example, they cross zero. Additionally, in simulations, we frequently see that the solution has no sensitivity to the starting point, providing some evidence that the solution may be a global, rather than local optimum. Finding methods for solving Optimization Problem 3 with stronger theoretical guarantees remains a direction for future research.

3.6 Confidence Sets

The intervals generated by our procedure will account for the worst-case estimation bias resulting from the manipulation, but not the variance in the data. We would like to provide a data-driven interval \( I = [L, R] \) such that the true causal effect \( \tau(c) \) lies asymptotically within \( I \) with probability \( 1 - \alpha \) for some small value \( \alpha \).

The development of valid confidence sets for regression discontinuity estimation remains an active area of research (Calonico et al., 2014; Bartalotti et al., 2016). In our setting, we have multiple sources of uncertainty: the estimation of the un-manipulated density as well as the estimation of the best- and worst-case treatment effects. The most straightforward way to account for these sources is to use the percentile bootstrap (Efron, 1992). A discussion of the statistical issues involved in using the bootstrap in concert with an optimization procedure to yield valid intervals can be found in Zhao et al. (2019), though we do not attempt a robust treatment here.

For \( b = 1, \ldots, B \) replicates, we draw samples with replacement from the original \( N \) units. We then repeat the estimation of the un-manipulated density and the optimization procedure, yielding lower- and upper-bound estimates \( (\hat{\tau}^{(b)}_L, \hat{\tau}^{(b)}_U) \). We construct a confidence interval as

\[
L = Q_{\alpha/2} \left( \hat{\tau}^{(b)}_L \right) \quad \text{and} \quad R = Q_{1-\alpha/2} \left( \hat{\tau}^{(b)}_U \right)
\]

where \( Q_{\nu}(\cdot) \) represents the \( \nu \) quantile of the distribution.

4 Bound Tightening via Covariates

This framework can be easily extended to incorporate covariates, which may help to tighten the partial identification bounds. Suppose we have access to some non-manipulable covariates whose expected values we assume evolve smoothly with the running variable \( X \). Under our assumptions, the subset of honest subjects should be randomized locally about the threshold \( c \), while the manipulators are definitionally not randomized. If we solve Optimization Problem 3 and find that the solution implies unreasonably large changes in the average covariate value at \( c \) or at any other point throughout the manipulation region, this indicates our worst-case or best-case samples contain manipulators.
In practice, we can use this insight to tighten the bounds by imposing further constraints in the optimization procedure. Let us suppose also that we have access to some non-manipulable $Q_i, i = 1, \ldots, N$ (e.g. age), and that we have used the Diamond and Persson procedure to identify a symmetric running variable manipulation region given by $(c - \Delta, c + \Delta)$. We consider the typical case in which all the manipulators are on one side of the threshold (to the left of $c$, let’s say). We assume also that the expected value of $Q_i$ evolves smoothly as a function of the running variable $X$.

We can estimate the conditional mean of $Q_i$ as a function of $X$ using data to the left of $c$ and above $c + \Delta$. This can be done using any of the methods we are using for the causal effect estimation, e.g. local polynomial regression or spline regression. Using the model, we then construct simultaneous confidence intervals for the observed mean of $Q_i$ at points $(c, c+1, \ldots, c+\Delta)$. An example is provided in Figure 2. A quadratic polynomial fit is used to estimate the dependence of $Q_i$ on $X_i$ and Bonferroni-corrected intervals for the mean observed value of $Q_i$ at each value of the running variable in the manipulation region (given here by $X_i \in \{10, 11, 12, 13\}$).

These bounds can then be easily integrated into the optimization procedure as additional affine constraints on $Z_r$. A constraint for a single value $x$ within the manipulation region looks like:

$$Q_{x,\text{lwr}} < \frac{1}{H(x)} \sum_{i : X_i + N_i = x} Z_{ri} Q_i < Q_{x,\text{upr}}$$

for derived bounds $Q_{x,\text{lwr}}, Q_{x,\text{upr}}$. These constraints can simply be appended to the existing optimization problem. Using the one-directional manipulation example above, our problem would look like
Optimization Problem 3 Covariate-Tightened Formulation

\[
\text{maximize } \quad (c^*)^T \left( (D_r^T W_r^* D_r)^{-1} D_r^T W_r^* Y_r - (D_l^T W_l^* D_l)^{-1} D_l^T W_l^* Y_l \right)
\]

subject to

\[W_r^* = W_r \, \text{diag}(Z_r),\]
\[A_r Z_r = \hat{H}_r,\]
\[Z_r \in [0,1]^{N_r},\]
\[Q_{x,lwr} < \frac{1}{H(x)} \sum_{i: X_i + N_i = x} Z_{ri} Q_i < Q_{x,upr} \quad \forall x \in \{c, c+1, \ldots, c+\Delta\}\]

A note of caution: these constraints are taking a probabilistically unlikely event and using them to define a constraint that must be satisfied. The unlikely event is also defined using a model that may itself be biased if we guess an erroneous functional form. Lastly, if constraints are imposed based on several different covariates, then it may be that any one constraint violation is highly unlikely but that collectively at least one violation is plausible.

Based on these considerations, the analyst should be cautious when imposing these constraints. Levels of \(\alpha\) should be chosen low and Bonferroni corrections made when appropriate. Covariates should also be chosen that have reasonably clear functional forms to avoid underfitting bias.

5 Simulated Example

We step through an illustrative example to demonstrate the utility of our method. We sample a running variable \(X_i\) from a Poisson distribution with mean 20 and suppose \(N = 5000\). The cutoff is chosen to be 25, and we suppose that 10% of individuals with \(X_i = 22\), 20% of individuals with \(X_i = 23\) and 30% of individuals with \(X_i = 24\) manipulate their scores. Of these manipulators, 50% manipulate exactly to the threshold of 25, 30% to 26, and 20% to 27. Outcomes \(Y_i\) are sampled as

\[Y_i = 2 + 0.1 X_i + 0.02 X_i^2 + \epsilon\]

where \(\epsilon \sim N(0, 2)\) are independent Gaussian errors. Note that the outcomes are independent of the treatment assignment, indicating no treatment effect.

We begin by using a modified version of Diamond and Persson’s method to recover an estimate of the un-manipulated distribution. We fit a Poisson model with coefficients corresponding to the un-manipulated, missing, and excess mass. A Bernstein basis expansion and linear constraint are used to ensure the un-manipulated distribution is log-concave, and the design matrix is constructed such that the missing and excess densities only contribute within the manipulation region below and above the cutoff respectively. We grid search over the order of the polynomials (two through five) and the width of the manipulation
Figure 3: The un-manipulated counts (in gray) are approximately recovered by our procedure, which yields the estimated values in red.

region (one through six units). Cross-validated MSE is used to choose the optimal model, where the folds are defined by individual points on the histogram and we try to predict the height at each point).

The optimal model chosen by the algorithm uses a fourth-order Bernstein polynomial. The optimal radius for the manipulation region is correctly chosen to be four units. The estimated counts shown by the red line in Figure 3 nonetheless recover the true counts above the cutoff reasonably well. The estimated and true non-manipulator counts at each value of the running variable are provided in Table 1.

Table 1: Estimated and true non-manipulator counts within the manipulation region at and above the threshold.

| Running Variable | Observed Count | Estimated Non-Manipulators | True Non-Manipulators |
|------------------|----------------|----------------------------|-----------------------|
| 25               | 370            | 240                        | 241                   |
| 26               | 328            | 189                        | 201                   |
| 27               | 189            | 143                        | 117                   |
| 28               | 136            | 104                        | 100                   |

Suppose we are using a third-order polynomial to estimate the conditional means to the left and right of the threshold. The manipulation is such that if we naively fit the model without accounting for the manipulation, we will obtain a causal estimate of -0.98. We might mistakenly think this is a real effect: a bootstrap confidence interval with 500 replicates covers $(-1.42, -0.55)$, lending credence to the view that the treatment has a negative causal effect.

We run our optimization procedure to obtain partial identification bounds.
Table 2: Lower and upper bounds averaged over five runs of the bound-tightening procedure at different values of the correlation between covariate $Q_i$ and true running variable $X_i$.

| Correlation | Lower Bound | Upper Bound |
|-------------|-------------|-------------|
| 0.0         | −2.70       | 0.70        |
| 0.2         | −2.70       | 0.70        |
| 0.4         | −2.69       | 0.70        |
| 0.6         | −2.66       | 0.68        |
| 0.8         | −2.47       | 0.60        |

on the causal effect with this manipulation table. The results are summarized in Figure 4. Each plot contains a scatterplot of the outcomes vs. the observed running variable at values near the threshold. We show the model fit to the data below the cutoff in purple and the model fit to the data above the cutoff in blue; a vertical gray line denotes the cutoff.

In the first panel, we can see how a standard analysis would yield a negative causal estimate. In the “Lower Bound” panel, we highlight in red the points identified as worst-case manipulators by our algorithm, and the resultant model fit to the data excluding these points in the dashed blue line. We see the analogous results in the “Upper Bound” panel. The “Together” panel shows all three model fits on one scatterplot.

Several details are immediately noticeable. The bounds now extend between $−2.70$ and $0.70$ – and, crucially, the upper bound lies above zero, casting doubt on the presumed negative causal effect. We see the manipulators identified in the second and third panels are always among the highest or lowest values of $y$ at each value of the running variable, but they swap between the values of 26 and 27 so as to optimize the curvature of the polynomial fit.

If we have access to additional covariates, the identified best- and worst-case manipulators may not strictly lie among the highest and lowest values of $y$ at each value of the running variable. We simulate the case where we have access to an additional covariate $Q_i$, and we generate different versions such that each has the same variance as the true running variable $X_i$ but correlations ranging from 0 to 0.8 (and a linear relationship with $X_i$ when the correlation is nonzero). We then follow the procedure laid out in Section 4 with an alpha level of 0.05 and a (correctly specified) model of $Q_i$ as a function of $X_i$.

In Table 2, we show the average of the bounds over five runs of the bound-tightening procedure. We can immediately see that the correlation must be quite high to see any meaningful tightening. At $\rho = 0.8$, we do see a modest decline in the interval width. It is plausible that the presence of multiple variables highly correlated with the running variable would yield tighter partial identification bounds.

We can also see in Figure 5 the set of individuals identified as manipulators for the lower tightened bound imposed for a covariate highly correlated with the running variable. Notably, the set of identified manipulators no longer
Figure 4: Results of the optimization procedure. Third-order polynomials are fit to the data to the left (in purple) and right (in blue) of the threshold. The top right and bottom left panels show the result of the optimization procedure in identifying manipulators (in red) and excluding them from the polynomial fit. The bottom right panel shows all three fits simultaneously. Crucially, observe that the interval between the best- and worst-case estimates now contains the true effect of zero.
adheres strictly to the highest and lowest individuals at each value of the running variable.

Lastly, we bootstrap the procedure to estimate full confidence bounds (without covariate tightening). We draw 200 bootstrap replicates from the data and compute upper and lower confidence bounds within each replicate. Taking the 2.5% quantile on the lower bounds and the 97.5% quantile on the upper bounds yields a final interval of $(-3.68, 1.71)$, about one unit wider than our bounds on the point estimates. Again, it bears emphasizing that these intervals cover the true effect of zero, while bootstrap intervals not accounting for the manipulation are bounded away from zero.

6 Conclusions

We have considered a common problem in the analysis of the regression discontinuity design: the presence of manipulators, or individuals who are able to exhibit precise control over their value of the running variable. The presence of such individuals undermines a key assumption in the RD design: that of local randomization about the threshold. Point identification on the causal effect is thus lost. In its absence, we propose a two-stage method for instead estimating partial identification bounds when the running variable is discrete.

In the first stage, we estimate the un-manipulated distribution of the running variable, making use of a log-concavity assumption and a method proposed in
Combined with an assumption that manipulation may occur to or from a value – but not both – this allows us to estimate the distribution of non-manipulators in our data. We use this distribution to define an optimization problem to bound the treatment effect, and propose to solve the (non-convex) problem via sequential convex programming. We also propose methods to tighten the bounds using auxiliary covariates, and we suggest a bootstrap procedure for obtaining confidence bounds.

Our method is quite general and can be used with several common models leveraged in the analysis of the RD design, including local polynomial and spline regressions. We think this method will be of use to applied researchers when analyzing RD designs with clear manipulation. If the bootstrapped bounds share the same sign, this provides particularly strong evidence of a directional causal effect even under the most adversarial manipulation.

There are several obvious next steps for this work. While we believe the current heuristic method is providing optimal or near-optimal solutions, we hope to derive certificates of optimality when solving Optimization Problem 3. This would allow us to drop the “conservative” modifier from our partial identification bounds. Relaxing the log-concavity assumption on the un-manipulated density would also help to generalize these methods to new settings.

The extension of our methods to continuous running variables is reasonably straightforward: we need only bin the running variable values. The “trick” lies in choosing appropriate bandwidths for binning. We hope to determine data-driven approaches for binning the data and thus easily extend these techniques to the common case of a continuous \( X \).

Lastly, in this manuscript we discuss only briefly the possibility of using covariates to identify plausible manipulators better. This approach shows some promise in simulations. We hope to extend these methods to incorporate richer covariate data in the future.

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