When Can We Ignore Measurement Error in the Running Variable?*

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Abstract

In many empirical applications of regression discontinuity designs, the running variable used by the administrator to assign treatment is only observed with error. We show that, provided the observed running variable (i) correctly classifies the treatment assignment, and (ii) affects the conditional means of the potential outcomes smoothly, ignoring the measurement error nonetheless yields an estimate with a causal interpretation: the average treatment effect for units with the value of the observed running variable equal to the cutoff. To accommodate various types of measurement error, we propose to conduct inference using recently developed bias-aware methods, which remain valid even when discreteness or irregular support in the observed running variable may lead to partial identification. We illustrate the results for both sharp and fuzzy designs in an empirical application.

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1 Introduction

The key characteristic of regression discontinuity (RD) designs is that assignment of units to treatment is determined by whether the value of a particular covariate $X^*$, called the running variable, exceeds a fixed threshold $c$. Under weak continuity conditions, comparing units on either side of the threshold identifies the average treatment effect (ATE) for units with $X^* = c$. However, in many cases, researchers do not observe the underlying running variable directly, and only observe $X$, a noisy version of $X^*$. Most commonly, the noise arises due to rounding, such as when researchers observe age in years, income reported in income brackets, or unemployment duration in months, while the administrator assigning treatment uses the exact birthdate, exact income, or exact unemployment duration. A rounded or coarsened version of a running variable that could be measured more finely features in 30 out of 158 applications of RD designs that we surveyed.\footnote{Our survey focused on papers published between 2005 and 2020 in one of 7 leading journals (American Economic Journal: Applied Economics, American Economic Journal: Economic Policy, American Economic Review, Quarterly Journal of Economics, Journal of Political Economy, Review of Economics and Statistics, and Review of Economic Studies).} Measurement error in the form of a recall or reporting error is also a concern whenever $X$ comes from survey data.

The prevalence of this problem has given rise to a growing literature on adapting standard RD techniques to allow for measurement error (see, among others, Barreca et al., 2016; Bartalotti et al., 2021; Davezies and Le Barbanchon, 2017; Dong, 2015; Hullegie and Klein, 2010; Pei and Shen, 2017; or Dieterle et al., 2020). As this literature points out, ignoring the measurement error can lead to inconsistent estimates of the usual RD estimand, the ATE for units with $X^* = c$.\footnote{An exception is Battistin et al. (2009), who, in the context of a fuzzy RD design, assume that $X$ is a contaminated version of $X^*$, and give conditions under which ignoring the measurement error nonetheless yields a consistent estimate of a fuzzy RD analog of this estimand.} These papers propose a variety of alternative estimation and inference procedures to recover this treatment effect, with solutions that depend on the exact auxiliary assumptions about the form of the measurement error, or the availability of auxiliary datasets.

This paper makes the simple point that under easily interpretable conditions, existing RD techniques provide inference for a slightly different estimand, the ATE for units with $X = c$. For instance, suppose $X^*$ is birthdate, and we only observe the year of birth $X$. While standard RD analysis does not yield valid inference for the usual RD estimand, the ATE for individuals born on the cutoff date, it does provide valid inference for the ATE for individuals born in the cutoff year.\footnote{Equivalently, this estimand is a weighted average of ATEs conditional on $X^*$, with weights given by the density of the measurement error conditional on $X = c$. The estimand in the example is a weighted average of ATEs for individuals born on each day of the cutoff year, weighted by the relative birthdate frequencies.} If one did not know that $X$ was mismeasured (say we
thought the administrator used year or birth, not birthdate, for treatment assignment), the
ATE for units with $X = c$ would, however, be the natural parameter to focus on. If we
subsequently learn that $X$ contains measurement error, we can thus keep the analysis the
same, with the same interpretation as if $X$ were the true running variable.

This result relies on two key conditions: (i) using $X$ as a running variable correctly
classifies the treatment assignment, and (ii) the conditional means of the potential outcomes
are smooth in $X$. The first condition holds automatically for certain types of rounding error;
more generally it may involve removing observations at or in the immediate vicinity of the
threshold, resulting in a “doughnut design”. For the second condition, we give a formal
result showing it holds under weaker conditions than those needed for inference on the ATE
conditional on $X^* = c$, if we were to observe $X^*$: intuitively the measurement error smooths
out kinks or other irregularities in the conditional mean of the outcome given $X^*$. Inference
can be conducted using recently developed bias-aware inference methods (e.g. Armstrong &
Kolesár, 2018, 2020; Noack & Rothe, 2021), which automatically adapt to the potentially
irregular support of $X$. In particular, we can ignore the measurement error in the sense
that, after possible doughnut trimming to ensure that condition (i) holds, estimation and
inference on the trimmed data can proceed as if $X$ were the running variable used by the
administrator to assign treatment.

An appealing feature of focusing on the ATE for units with $X = c$ is that valid inference
relies only on assumptions about the smoothness of the conditional mean of the potential
outcomes given $X$. Such assumptions are easy to interpret, and are (partially) testable. Fur-
thermore, inference is standard in that we can directly apply existing methods. In contrast,
for inference on the average treatment effect for units with $X^* = c$, one needs to either make
specific assumptions about latent objects, such as the distribution of the measurement error
given $X$ or $X^*$, or have access to auxiliary data; furthermore, the form of the estimator
depends on the exact form of these specific assumptions.

The rest of the paper proceeds as follows. Section 2 gives the setup and the main results.
Section 3 illustrates the results in an empirical application. Section 4 concludes. Auxiliary
results appear in the appendix.

## 2 Setup and main result

We first set up the sharp RD model. Section 2.1 outlines our proposed approach to estimation
and inference. Section 2.2 discusses conditions for its validity. Section 2.3 extends the results
to fuzzy RD settings.

We are interested in the effect of a treatment $T$ on an outcome $Y$. Let $Y(t)$ denote the
potential outcomes, $t \in \{0, 1\}$. The observed outcome is given by $Y = Y(0) + T(Y(1) - Y(0))$. An administrator assigns individuals to treatment if their running variable $X^*$ crosses a threshold $c$, which we normalize to $c = 0$. Let $Z = I\{X^* \geq 0\}$ denote the indicator for treatment assignment. In a sharp RD design, all individuals comply with the treatment assignment, so that $T = Z$. Let $g_t^*(x) := E[Y(t) \mid X^* = x]$ denote the conditional mean of the potential outcomes given $X^*$, and let $g^*(x) := E[Y \mid X^* = x] = I\{x \geq 0\}g^*_1(x) + I\{x < 0\}g^*_0(x)$ denote the conditional mean of the observed outcome.

If the functions $g_t^*$ are continuous at 0, the jump in $g^*(x)$ at 0 identifies an average treatment effect for units at the threshold (Hahn et al., 2001),

$$\tau^* := E[Y(1) - Y(0) \mid X^* = 0] = \lim_{x \downarrow 0} g^*(x) - \lim_{x \uparrow 0} g^*(x).$$

If $X^*$ were observed, and we strengthen the continuity assumption by placing nonparametric smoothness assumptions on $g^*$, several methods for estimation and inference on $\tau^*$ are available (see, e.g., Armstrong & Kolesár, 2018, 2020; Calonico et al., 2014; Imbens & Kalyanaraman, 2012; Imbens & Wager, 2019). Under parametric restrictions on $g^*$, we can leverage standard parametric regression methods for estimation and inference.

We do not observe $X^*$ directly, however; instead we observe $X = X^* - e$, where $e$ is measurement error. We’ll focus on the case where $e$ represents rounding error as our leading example. For instance, if we observe a rounded-down version of $X^*$, then $e = X^* - \lfloor X^* \rfloor$. However, we allow the measurement error to take other forms; $X$ may be discrete, continuous, or mixed; $e$ may correspond to a classical or Berkson measurement error. As a number of papers point out (see, among others, Barreca et al., 2016; Dong, 2015; Hullegie and Klein, 2010; or Pei and Shen, 2017), ignoring the measurement error, and employing the parametric or nonparametric methods cited above generally yields inconsistent estimates of $\tau^*$. These papers also show how one can combine parametric or nonparametric smoothness assumptions on $g^*$ with various independence and distributional assumptions on $e$ to obtain consistent estimates of $\tau^*$. The form of the estimator and its consistency properties will generally depend on the particular assumptions imposed on $e$.

2.1 Estimation and inference with measurement error

Our approach is based on the observation that any variable $X$ can serve as a running variable, provided that it correctly classifies the treatment assignment, and provided that the

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4Our setting is distinct from the setting studied in Eckles et al. (2022), where the running variable $X^*$ used by the administrator is the same the variable $X$ observed by the researcher, but $X^*$ can be thought of a noisy measure of some latent variable that affects potential outcomes.
conditional mean functions of the potential outcomes given $X$ are continuous at the cutoff:

(C1) $\{X \geq 0\} = Z$ almost surely.

(C2) $g_t(x) := E[Y(t) \mid X = x]$ is continuous at 0 for $t = 0, 1$.

We give a detailed discussion of these conditions in our setting—where $X$ is a mismeasured version of $X^*$ in Section 2.2 below. Note that condition (C2) is essentially the same as the standard RD continuity condition from Hahn et al. (2001), but applied to $X$ rather than $X^*$. While, as discussed in Remark 3 below, our approach does allow the measurement error to affect the potential outcomes, so long as it does so smoothly, to link the estimand to the usual RD estimand, it is useful to rule this possibility out and assume:

(C3) $E[Y(1) - Y(0) \mid X^* = x, X = 0] = E[Y(1) - Y(0) \mid X^* = x]$.

This condition is slightly weaker than the requirement that the measurement error be non-differential, i.e., independent of $(Y(1), Y(0))$ given $X^*$ (Carroll et al., 2006, Chapter 2.6); this is a common assumption in the literature on RD with measurement error (e.g. Bartalotti et al., 2021; Davezies & Le Barbanchon, 2017; Pei & Shen, 2017). For instance, rounding errors and classical measurement error are both non-differential.

To state our result, let $\tau(x) := E[Y(1) - Y(0) \mid X^* = x]$, so that $\tau = \tau(0)$.

Lemma 2.1. Suppose that $T = Z$ and conditions (C1) and (C2) hold. Then the jump in the conditional mean function $g(x) := E[Y \mid X = x]$ at 0 identifies the ATE for units with $X = 0$,

$$\tau := E[Y(1) - Y(0) \mid X = 0] = \lim_{x \downarrow 0} g(x) - \lim_{x \uparrow 0} g(x).$$

If, in addition, condition (C3) holds, then $\tau = \int \tau(e) dF_{e \mid X}(e \mid 0)$, where $F_{e \mid X}(e \mid x)$ is the conditional distribution of $e = X^* - X$ given $X = x$.

Proof. Under condition (C1) $g(x) = I\{X \geq 0\}g_1(x) + I\{X < 0\}g_0(x)$, so that $\lim_{x \downarrow 0} g(x) - \lim_{x \uparrow 0} g(x) = \lim_{x \downarrow 0} g_1(x) - \lim_{x \uparrow 0} g_0(x)$, which equals $g_1(0) - g_0(0)$ by condition (C2). Under condition (C3), by iterated expectations, $\tau = E [E [Y(1) - Y(0) \mid X^*] \mid X = 0]$, which gives the second claim. \qed

By Lemma 2.1, we can effectively “ignore” the measurement error in the observed running variable $X$, in that we can conduct the analysis as if $X$ were the running variable used by the administrator to assign treatment, provided that we align the target of inference accordingly, setting it to $\tau$. Under condition (C3), we can interpret $\tau$ as a weighted average of ATEs conditional on $X^*$.
Remark 1 (Comparison of $\tau$ and $\tau^*$). The parameter $\tau^*$ corresponds to an ATE for units with the latent running variable $X^*$ equal to 0. If $X^*$ is the birthdate of an individual, for instance, then $\tau^*$ is the ATE for those born at the cutoff date. If $X$ is month of birth, then $\tau$ corresponds to the ATE for those born in the same month as the individuals born at the cutoff date. $\tau$ can be expressed as a weighted average of ATEs $\tau^*(x)$ for individuals born on day $x$, and the same month the individuals born at the cutoff date. The exact weights depend on the distribution of births in that month. If, for instance, birthdate is uniformly distributed within the month, then the weights are uniform. The estimands $\tau$ and $\tau^*$ are generally different unless $\tau^*(x)$ is constant on the support of $e$. For example, if $\tau^*(x) = a + bx$, and $E[e | X = 0] = 1/2$ (say when $e$ is uniform), which appears to be consistent with the results of our empirical application, then $\tau^* = a$, while $\tau = a + b/2$. Which parameter is more policy relevant depends on the particular policy counterfactual one has in mind.\(^5\)

For estimation and inference, we need to strengthen condition (C2) by assuming that $g$ satisfies appropriate parametric or nonparametric smoothness conditions. As a simple parametric approach, one may assume that $g(x)$ takes the form of a polynomial of degree $q$ on either side of the threshold for values of $x$ within distance $h$ of the threshold. Then one could estimate $\tau$ by a local polynomial regression of $Y$ onto $m_q(X) = (I\{X \geq 0\}, I\{X \geq 0\}X, \ldots, I\{X \geq 0\}X^q, 1, \ldots, X^q)$ (a polynomial in $X$ interacted with treatment assignment), using ordinary least squares (OLS).\(^6\) Specifically, given a sample $\{Y_i, X_i\}_{i=1}^n$, the estimator is defined as

$$\hat{\tau}_{h,q}^Y = (1, 0, \ldots, 0)' \left( \sum_{i=1}^n I\{|X_i| \leq h\}m_q(X_i)m_q(X_i)' \right)^{-1} \sum_{i=1}^n I\{|X_i| \leq h\}m_q(X_i)Y_i. \quad (3)$$

Under i.i.d. sampling, inference can be conducted using Eicker-Huber-White (EHW) standard errors, provided that $X$ has at least $q+1$ support points on either side of the threshold (see Remark 4 and footnote 10 below).

A limitation of the parametric approach is that if $g(x)$ is not exactly polynomial inside the estimation window, the estimator will be biased; consequently, confidence intervals (CIs) based on EHW standard errors will undercover $\tau$. To address this issue, as the preferred inference method, we propose to use the bias-aware (or “honest”) inference approach developed in Armstrong and Kolesár (2018, 2020) and Kolesár and Rothe (2018). This approach

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\(^5\)One may object that the parameter $\tau$ is reverse-engineered in that Lemma 2.1 shows $\tau$ is the parameter that our analysis happens to identify. The same criticism may be leveled at the result of Hahn et al. (2001) that RD analysis in the absence of measurement error identifies $\tau^*$. We view both results as useful in separating the internal and external validity of the analysis.

\(^6\)While this covers a global approach by setting $h = \infty$, as discussed in Gelman and Imbens (2019), such an approach may perform poorly relative to local approaches.
enlarges the CIs by taking into account the potential finite sample bias of the estimator. In particular, letting $\hat{\sigma}(\hat{\tau}_{h,q}^Y)$ denote the standard error, a CI with level $1 - \alpha$ takes the form

$$\hat{\tau}_{h,q}^Y \pm cv_\alpha \left( B(\hat{\tau}_{h,q}^Y/\hat{\sigma}(\hat{\tau}_{h,q}^Y)) \cdot \hat{\sigma}(\hat{\tau}_{h,q}^Y) \right).$$

(4)

Here $cv_\alpha(t)$ is the $1 - \alpha$ quantile of a folded normal distribution $|N(t, 1)|$, and $B(\hat{\tau}_{h,q}^Y)$ is a bound on the finite-sample conditional (on $X$) bias of the estimator. As a baseline assumption to bound the potential bias, we replace the parametric assumption that $g$ is polynomial with the weaker non-parametric assumption that $g \in F_{RD}(M)$, where

$$F_{RD}(M) = \{ f_1(x) I\{x \geq 0\} + f_0(x) I\{x < 0\} : \|f'_0\|_{C1} \leq M, \|f'_1\|_{C1} \leq M \}.$$  

(5)

Here $\|f\|_{C1} = \sup_{x \neq x'} |f(x) - f(x')|/|x - x'|$ is the Lipschitz constant of $f$ (if $f$ is differentiable, then the constant is the maximum of its derivative, $\|f\|_{C1} = \sup_{x} |f'(x)|$). The parameter space for $g$ thus corresponds to the (closure of) family functions that are twice differentiable on either side of the cutoff, with the second derivative bounded in absolute value by $M$, but are potentially discontinuous at 0. Under this assumption, it is optimal to run a local linear regression, i.e. use the estimator $\hat{\tau}_{h,1}^Y$. The (conditional on $X$) bias of the estimator is maximized at the function $h(x) = Mx^2(I\{x < 0\} - I\{x \geq 0\})/2$, so that $B(\hat{\tau}_{h,1}^Y)$ is given by eq. (3), with $Y_i$ replaced by $h(X_i)$. See Armstrong and Kolesár (2020) and Kolesár and Rothe (2018) for details. An appealing feature of the bias-aware CI is that because it accounts for the exact finite-sample bias of the estimator, it is valid under any bandwidth sequence, including using a fixed bandwidth; for example, the bandwidth $h$ may be selected to minimize the (worst-case over $F_{RD}(M)$) mean squared error, or the length of the resulting confidence interval.\(^7\)

2.2 Conditions for validity of proposed approach

Unlike existing approaches that seek to do inference on $\tau^*$ even in presence of measurement error (e.g. Davezies and Le Barbanchon, 2017; Dong, 2015; Hullegie and Klein, 2010; or Pei and Shen, 2017), we do not impose specific assumptions on the measurement error distribution or require auxiliary data. Instead, our approach is based on the observation that we can use existing parametric or nonparametric methods for inference on $\tau$ provided that condition (C1) holds, and we strengthen condition (C2) by assuming that the conditional mean functions $g$ is smooth (in the sense that it is exactly polynomial inside the estimation

\(^7\)See Armstrong and Kolesár (2020), Kolesár and Rothe (2018) and Imbens and Wager (2019) for more discussion, and for a discussion of implementation issues, including the choice of $M$. We implement this method in the empirical application in Section 3.
window, or else \( g \in \mathcal{F}_{RD}(M) \). Arguably, these assumptions are high-level in that they are exactly the conditions needed to interpret the RD design with the observed running variable as a valid RD design. In the following remarks, we discuss in detail sufficient conditions for these assumptions, showing that these assumptions hold in a variety of measurement error settings. We also discuss related practical issues.

**Remark 2** (Correct classification of treatment assignment). When \( X \) corresponds to \( X^\ast \) rounded down to the nearest integer, and the threshold \( c \) is an integer, condition \((C1)\) holds automatically. However, in general, measurement error may induce misclassification of the treatment assignment for values of \( X \) equal to the cutoff or in its immediate vicinity. In such cases, condition \((C1)\) requires dropping observations with such values of \( X \), resulting in a “doughnut” design (e.g. Almond et al., 2011; Barreca et al., 2011). A sufficient condition for \((C1)\) is that (i) conditional on \( X \), \( e \) has bounded support that is contained in the support of \( X \), and (ii) one removes observations with possibly misclassified treatment. While our approach does not apply if the support of \( e \) is unbounded, such applications appear to be relatively rare, in contrast to the ubiquity of rounding or coarsening that we found in our survey discussed in the Introduction.

The exact form of such doughnut trimming depends on the support of the measurement error. For ordinary rounding or rounding up to the nearest integer or a multiple of 10, for instance, this requires removing observations with \( X = 0 \) (the cutoff may itself be similarly rounded). If \( e \) has bounded support \([s_0, s_1]\), with \( s_0 \leq 0 \leq s_1 \), we need to remove observations with \( X \in [-s_1, -s_0) \).\(^8\) Under interval measurement, where, given a set of intervals we observe whether \( X \) belongs to, we need to remove \( X \) that falls into the interval containing 0 (one could define \( X \) to be the midpoint or one of the endpoints of the interval).

**Remark 3** (Smoothness of \( g \)). Our proposed approach requires smoothness of \( g \) in the sense that \( g \in \mathcal{F}_{RD}(M) \). To discuss this condition, let \( g_t^\ast(X^\ast, e) = E[Y(t) \mid X^\ast, e] \) denote the conditional mean of the potential outcome given both the true value of the running variable and the measurement error, and suppose that condition \((C1)\) holds, so that we may write 
\[
g(x) = I\{x \geq 0\} g_1(x) + I\{x < 0\} g_0(x),
\]
where, by iterated expectations,
\[
g_t(x) = E[Y(t) \mid X = x] = E[g_t^\ast(X^\ast, e) \mid X = x]. \tag{6}
\]

In Lemma A.1 in Section A.1, we give a formal result showing that if (a) the distribution of the measurement error \( F_{e \mid X}(e \mid x) \) is smooth in \( x \); and (b) the effect of the measurement error on potential outcomes is smooth, so that \( g_t^\ast(X^\ast, e) \) is smooth in the second argument, then

\(^8\)Assuming that \( s_0 \leq 0 \leq s_1 \) is without loss of generality, since if, \( s_0 > 0 \), subtracting \( s_0 \) from \( X \) mitigates the measurement error issue; similarly, if \( s_1 < 0 \), we can subtract \( s_1 \) from \( X \).
the condition \( g \in \mathcal{F}_{RD}(M) \) is \textit{weaker} than the analogous smoothness requirement needed for inference on \( \tau^* \) if \( X^* \) were observed: the smoothness of \( g_t^* \) in \( X \) is \textit{greater} than the smoothness of \( g_t(X^*, e) \) in \( X^* \). Consequently, eq. (5) will hold for \( g \) even in settings where it may not hold for \( g^*(x) = \mathbb{1}\{x \geq 0\} g_1^*(x) + \mathbb{1}\{x < 0\} g_0^*(x) \).

To gain intuition for this result, suppose first that the measurement error is non-differential (as is the case under rounding error or coarsening), so that condition (b) above holds trivially because \( g_t^*(X^*, e) = g_t(X^*) \), and we may write eq. (6) as \( g_t(x) = \mathbb{E}[g_t^*(X^*) | X = x] \). Lemma A.1 then formalizes the notion that the conditional expectation \( \mathbb{E}[\cdot | X = x] \) “smooths out” non-linearities in \( g_t^* \). For example, if \( g_t^* \) contains kinks (so that it has smoothness index 1 and eq. (5) fails for \( g^* \)), these kinks will be smoothed out by the measurement error provided that the conditional density of \( e \) given \( X \) is continuous with a bounded slope (in which case \( g_t \) will have smoothness index 2, and eq. (5) will hold for \( g \)). The result also goes through under differential measurement error, provided the error affects the potential outcomes smoothly—this is analogous to the result that \( g_t^*(X^*) \) remains to be smooth in \( X^* \) even if agents can manipulate the value of their running variable, so long as the manipulation is not perfect (Lee, 2008).

For the parametric approach, if condition \((C1)\) holds, a sufficient condition for \( g \) to be polynomial of degree \( q \) on either side of the threshold is that \( g_0^*(X^*, e) \) and \( g_1^*(X^*, e) \) are multivariate polynomials of degree \( q \), and \( \mathbb{E}[e^j | X] \) for \( j = 1, \ldots, q \) are polynomials of degree \( j \). This follows directly from the binomial theorem.

While conditions (a) and (b) in Remark 3 are relatively mild, both conditions fail when there is “heaping” in the running variable. For example, consider using birthweight as a running variable to identify the effects of hospital care on infant health (Almond et al., 2010). Suppose that some (but not all) hospitals report rounded rather than precise birthweight—then condition (a) fails since the distribution \( F_{e|X}(e | x) \) changes discontinuously as at round values of \( x \). Furthermore, suppose that hospitals with fewer resources are more likely report rounded rather than precise birthweight, as argued in Barreca et al. (2016). Then condition (b) also fails, since \( g_t^*(X^*, e) \) is then potentially discontinuous at \( e = 0 \) due to different hospital composition under rounded vs exact reporting. On the other hand, the assumption that \( g \) is smooth plausibly holds once we drop rounded values of \( X \) from the analysis, as suggested by Barreca et al. (2016).

Since the conditional mean function \( g(x) \) is identified over the support of \( X \), smoothness

\[ \text{See, for example, Newey (2013, Section 4) for a discussion in the context of nonparametric instrumental variables regression, where } X^* \text{ plays the role of an endogenous variable, and } X \text{ plays the role of an instrument. One consequence of this smoothing in the current context, as discussed for example in Battistin et al. (2009), is that the measurement error may smooth out the discontinuity of } g^* \text{ at the cutoff, making } g \text{ continuous, and causing condition } (C1) \text{ to fail unless we restrict the sample as discussed in Remark 2.} \]
assumptions such as \( g \in \mathcal{F}_{\text{RD}}(M) \) are testable. Problems such as heaping are often apparent from simple plots of undersmoothed binned averages of the outcome against \( X \) (see, e.g., Figure 1 in Barreca et al., 2011), and one can also conduct more formal specification tests (see, e.g., Kolesár & Rothe, 2018, Appendix S.3).

**Remark 4** (Irregular support of \( X \)). The measurement error may result in a coarsening of the support of the observed running variable \( X \) relative to \( X^* \). In the case of rounding, for example, the support of \( X \) may be discrete even if \( X^* \) is continuously distributed. Furthermore, there may be a gap in the support around 0 due to a “doughnut design” (see Remark 2). In such “irregular” cases, since conditional mean functions are only well-defined over the support of the conditioning variable, following Kolesár and Rothe (2018) and Imbens and Wager (2019), we interpret smoothness assumptions such as eq. (5) to mean that there exists a function \( g(x) \in \mathcal{F}_{\text{RD}}(M) \) with domain \( \mathbb{R} \) such that \( E[Y | X] = g(X) \) with probability one. With discrete support for \( X \) or under a doughnut design, there will be multiple functions \( g \) satisfying this condition, and the parameter \( \tau \) will only be partially identified.

An advantage of the bias-aware inference approach is that the estimator and confidence interval construction remains the same whether the support of \( X \) is continuous or irregular, and whether \( \tau \) is point or partially identified. Under irregular support of \( X \), the finite-sample bias of the estimator may be large, but this will be reflected in the confidence interval through the larger critical value (in such cases, the interval will converge to the identified set as the sample size \( n \to \infty \)).\(^{10}\) Under the bias-aware approach, we thus do not need to distinguish between cases where the distribution of the observed running variable \( X \) is continuous, or discrete or otherwise irregular. We illustrate these points in the empirical application in Section 3, where we show that under rounding error, confidence intervals for \( \tau \) tend to be longer than confidence intervals for \( \tau^* \) that one would obtain using the same construction if \( X^* \) were observed.\(^{11}\)

### 2.3 Fuzzy designs

In fuzzy RD designs, only a subset of the individuals complies with the treatment assignment, so that \( T \neq Z \). In this case, Hahn et al. (2001) show that the fuzzy RD parameter can be

\(^{10}\)See Armstrong and Kolesár (2018), and Imbens and Wager (2019) for a more detailed discussion of these issues. Under the parametric assumption discussed above that \( g \) is exactly polynomial of degree \( q \) inside the estimation window, the parameter \( \tau \) remains point identified provided there are at least \( q + 1 \) support points in each interval \([−h,0)\) and \([0,h]\). However, such parametric extrapolation results are sensitive to misspecification.

\(^{11}\)For other types of measurement error under which \( \tau \) remains point identified, the CIs for \( \tau \) may be narrower or wider than the corresponding CIs for \( \tau^* \) if \( X^* \) were observed. A general relative efficiency comparison is difficult, as it depends on the smoothness of \( g \) vs \( g^* \), the relative efficiency of using the local linear estimator under the given smoothness, and the heteroskedasticity of the regression residual.
interpreted as a local average treatment effect for individuals who comply with the treatment assignment. Let us reconsider their argument when we use a variable $X$ as the running variable, not necessarily equal to the running variable $X^\ast$ used by the administrator.\footnote{12}{The original argument in Hahn et al. (2001) involved defining potential treatments under counterfactual values of the running variable. In many cases, however, the running variable is not manipulable (such as when $X^\ast$ corresponds to birthdate, or birthweight). We therefore use a slightly different argument, based on manipulation of the treatment assignment. The treatment assignment is typically manipulable, say by moving the cutoff.}

Let $T(1)$ denote the potential treatment status of the individual if they are assigned to treatment, and let $T(0)$ denote their status if they are not assigned to treatment. The observed treatment is given by $T = T(Z)$, and the observed outcome is given by $Y = Y(T(Z)) = Y(0) + T(Z)(Y(1) - Y(0))$. Let $\mathcal{C}$ denote the event that an individual is a complier, that is $T(1) > T(0)$. Finally, in analogy to the conditional means $g$ and $g_t$, let $p(x) = E[T \mid X = x]$ and $p_z(x) = E[T(z) \mid X = x]$ for $z \in \{0, 1\}$.

We replace the sharp RD condition that all individuals comply with the treatment assignment ($T = Z$) with the weaker condition that a non-zero fraction of individuals complies with it, and that nobody defies the treatment assignment (in analogy with the monotonicity condition in Imbens and Angrist, 1994):

\begin{equation}
(F1) \quad P(T(1) \geq T(0) \mid X = 0) = 1, \text{ and } P(T(1) > T(0) \mid X = 0) > 0.
\end{equation}

Next, we replace the continuity assumption $(C2)$ with a continuity assumption on the first stage and reduced form regression functions:\footnote{13}{Analogous to an instrumental variables regression that uses $Z$ as an instrument, these are (non-parametric) regressions of $T$ and $Y$, respectively, onto $Z$ and $X$.}

\begin{equation}
(F2) \quad p_z(x) \text{ and } E \left[ Y (T (z)) \mid X = x \right], z = 0, 1, \text{ are continuous at 0}.
\end{equation}

Intuitively, if treatment eligibility $Z$ did not change at the cutoff but was instead fixed, this condition implies that the observed outcome $Y = Y(T(Z))$ would be continuous at 0. As a result, any discontinuity must be due to change in treatment eligibility, which allows for identification of causal effects. Conditions $(F1)$ and $(F2)$ are analogous to the standard fuzzy RD assumptions, but applied to $X$ rather than $X^\ast$.

Finally, to interpret the estimand as a weighted average of treatment effects conditional on $X^\ast$, we replace condition $(C3)$ with a condition ensuring that the measurement error has no effect on the compliance probability or the ATE for compliers once we control for $X^\ast$.

\begin{equation}
(F3) \quad P(\mathcal{C} \mid X^\ast = x, X = 0) = P(\mathcal{C} \mid X^\ast = x) \text{ and } E \left[ Y(1) - Y(0) \mid \mathcal{C}, X^\ast = x, X = 0 \right] = E \left[ Y(1) - Y(0) \mid \mathcal{C}, X^\ast = x \right].
\end{equation}
This is a slightly weaker requirement that the measurement error $e$ be non-differential, i.e. independent of $(Y(1), Y(0), T(1), T(0))$ given $X^*$. In analogy to condition $(C3)$ in the sharp case, condition $(F3)$ is helpful for interpreting the estimand, but it is not necessary for validity of our approach.

With this setup, we obtain a fuzzy RD analog of Lemma 2.1.

**Lemma 2.2.** Suppose that conditions $(C1)$, $(F1)$, and $(F2)$ hold. Then

$$
\tau_F := E[Y(1) - Y(0) \mid \mathcal{C}, X = 0] = \frac{\lim_{x \downarrow 0} g(x) - \lim_{x \uparrow 0} g(x)}{\lim_{x \downarrow 0} p(x) - \lim_{x \uparrow 0} p(x)}.
$$

If, in addition, condition $(F3)$ holds, then $\tau_F = \int \tau_F^e(e)\omega(e)dF_{e|X}(e \mid 0)$, where $\tau_F^e(x) := E[Y(1) - Y(0) \mid \mathcal{C}, X^* = x]$, and $\omega(e) = \frac{P(\mathcal{C}|X^* = e)}{\int P(\mathcal{C}|X^* = e)dF_{e|X}(e|0)}$.

**Proof.** Observe that

$$
\lim_{x \downarrow 0} g(x) - \lim_{x \uparrow 0} g(x) = \lim_{x \downarrow 0} E[Y(T(1)) \mid X = x] - \lim_{x \uparrow 0} E[Y(T(0)) \mid X = x]
$$

$$
= E[Y(T(1)) - Y(T(0)) \mid X = 0]
$$

$$
= E[(Y(1) - Y(0))(T(1) - T(0)) \mid X = 0]
$$

$$
= E[Y(1) - Y(0) \mid X = 0, \mathcal{C}]P(\mathcal{C} \mid X = 0),
$$

where the first equality uses the fact that $Y = Y(T(Z))$, and that by condition $(C1)$, $T = T(1)$ for individuals with $X \geq 0$, and $T = T(0)$ for those with $X < 0$, the second equality uses condition $(F2)$, the third uses $Y(T(z)) = Y(0) + T(z)(Y(1) - Y(0))$, and the last equality uses iterated expectations and condition $(F1)$. By analogous arguments, $\lim_{x \downarrow 0} p(x) - \lim_{x \uparrow 0} p(x) = P(\mathcal{C} \mid X = 0)$. The second claim follows by applying iterated expectations to the numerator and denominator of $\tau_F = \frac{E[Y(T(1)) - Y(T(0))|\mathcal{C}, X = 0]}{E[T(1) - T(0)|\mathcal{C}, X = 0]}$, and using condition $(F3)$. \qed

Under perfect compliance, $T = Z$, Lemma 2.2 reduces to Lemma 2.1. In analogy to the sharp case, any variable satisfying conditions $(C1)$, $(F1)$, and $(F2)$ can be used as a running variable. With $X = X^*$, we obtain the standard result that

$$
\tau_F^* := \tau_F^*(0) = \frac{\lim_{x \downarrow 0} g^*(x) - \lim_{x \uparrow 0} g^*(x)}{\lim_{x \downarrow 0} p^*(x) - \lim_{x \uparrow 0} p^*(x)},
$$

where $p^*(x) = E[T \mid X^* = x]$.

Unless the local average treatment effects $\tau_F^*(x)$ are constant on the support of $e$, $\tau_F \neq \tau_F^*$. Since $\tau_F^*(x)$ is given by the ratio of the reduced form effect $E[Y(T(1)) - Y(T(0)) \mid X^* = x]$,
to the first stage effect \( E[T(1) - T(0) \mid X^* = x^*] \), whether \( \tau_F^*(x) \) is locally constant depends on heterogeneity in both the reduced form and the first stage conditional mean functions. Our empirical results in Section 3, for example, are consistent with the reduced form effect being approximately constant, while the first stage effect is approximately linear, i.e., \( E\{Y(T(1)) - Y(T(0)) \mid X^* = x\} \approx a \) and \( E\{T(1) - T(0) \mid X^* = x\} \approx b + cx \); further the measurement error \( e \) at \([0, 1]\). So \( \tau_F \) is approximately given by \( a/(b + c/2) \), while \( \tau_F^* \approx a/b \).

Similarly to the sharp case, if we assume that the conditional mean functions \( g(x) \) and \( p(x) \) are polynomial inside a window \( h \) of the threshold, then we can estimate \( \tau_F \) as a ratio of local polynomial estimators

\[
\hat{\tau}_{h,q} = \frac{\hat{\tau}_Y^{Y}}{\hat{\tau}_T^{T}},
\]

with \( \hat{\tau}_Y^{Y} \) defined in eq. (3), and \( \hat{\tau}_T^{T} \) defined analogously.\(^{14}\) If there are at least \( q + 1 \) support points for \( X \) on either side of the threshold and inside the estimation window, then under i.i.d. sampling, standard errors for \( \hat{\tau}_{h,q} \) can be constructed based on the EHW covariance matrix for \( (\hat{\tau}_Y^{Y}, \hat{\tau}_T^{T}) \) using the delta method.

Our preferred approach weakens the polynomial assumptions on \( g(x) \) and \( p(x) \) by instead assuming that \( g \in \mathcal{F}_{RD}(M_y) \), and \( p \in \mathcal{F}_{RD}(M_t) \). While under this assumption, we may lose point identification if the support of \( X \) is irregular (see Remark 4), we can use the bias-aware inference approach for constructing CIs that are asymptotically valid whether \( \tau_F \) is point identified, identified, or unidentified, and whether the support of \( X \) is regular or irregular.\(^{15}\) In particular, following Noack and Rothe (2021), we can test the hypothesis \( H_0: \tau_F = \tau_{F,0} \) by checking whether 0 is in the bias-aware confidence interval based on \( \hat{\tau}_{h,1}^{Y - \tau_{F,0}T} \), and noting that the smoothness assumptions on \( g \) and \( p \) imply \( E\{Y - \tau_{F,0}T \mid X = x\} \in \mathcal{F}_{RD}(M_y + |\tau_{F,0}| M_t) \).

The confidence set for \( \tau_F \) is constructed by collecting all values of \( \tau_{F,0} \) that are not rejected, similar to the construction of Anderson and Rubin (1949) confidence set in standard linear instrumental variables model.\(^{16}\)

### 3 Empirical Application

In this section, we use data from Holbein and Hillygus (2016) to estimate the impact of preregistration on youth turnout in an election. Holbein and Hillygus (2016) leverage the

\(^{14}\)Equivalently, as noted in Hahn et al. (2001), the estimator can be computed as a two-stage least squares estimator in a regression of \( Y \) onto \( T \) using \( I\{X \geq 0\} \) and instrument, and the remaining elements of \( m_q(X) \) as exogenous covariates, using observations inside the estimation window.

\(^{15}\)The parameter \( \tau_F \) is unidentified if the instrument \( I\{X \geq 0\} \) is irrelevant in the sense that \( P(T(1) > T(0) \mid X = 0) = 0 \). Since the expression for \( \tau_F \) in Lemma 2.2 not well-defined in this case, one can define \( \tau_F \) in an arbitrary way.

\(^{16}\)See Noack and Rothe (2021) for a detailed discussion of implementation issues, including the choice of constants \( M_t \) and \( M_y \). We implement this method in our empirical application in Section 3.
fact that in Florida, individuals who were ineligible to vote in the 2008 election (those born after November 4, 1990) were nonetheless eligible to preregister to be added to the voter rolls for the next election (those born before November 4, 1990 are already eligible to register regularly and vote in 2008). This motivates a fuzzy RD design, where the treatment \( T \) is an indicator for preregistering, the outcome \( Y \) is an indicator for voting in the 2012 election, and the running variable \( X^* \) is the proximity to the eligibility cutoff in days.

To illustrate the effects of measurement error in the running variable, we compare this design to a fuzzy RD design in which we (pretend to) only observe the month of birth of the individuals, and hence use proximity to November 1990 in months, \( X \), as a running variable. We discard individuals born in November 1990, since their eligibility cannot be determined by month of birth alone (see Remark 2). We show that, consistent with the discussion in Remarks 1 and 4, (i) using proximity in days vs months yields different estimates, reflecting the impact of the rounding error on the estimand, and (ii) using month of birth generally leads to wider CIs.

We first visualize the two versions of the RD design. In each case, the sample size is 186,575, consisting of individuals born within 6 months of the eligibility cutoff. Figure 1 presents the first stage, plotting preregistration rate against proximity in days (panel (a)) or in months (panel (b)). For ineligible individuals, the preregistration rate is essentially 0, while for eligible individuals, the preregistration rate is downward slopping: those born

![Figure 1: Effect of proximity on preregistering.](image-url)
further away from the cutoff preregister with lower probability. There is a clear jump in the registration rate at the eligibility threshold in either panel.

Figure 2 shows the reduced form, plotting the proportion who voted in the 2012 election against proximity to eligibility. In both panels, there is a small jump in the voting probability at the cutoff.

We use five specifications to compute the fuzzy RD estimator in eq. (7), the sharp RD estimators of the first stage and reduced form effects, and the associated confidence intervals. For ease of comparison across specifications, all specifications use a uniform kernel and local linear regression ($q = 1$). The first specification follows Holbein and Hillygus (2016), and uses bandwidth set to $h = 60$ days (or $h = 2$ months), and the confidence intervals to not account for the potential bias of the estimator. The second specification differs only in that it uses a slightly larger bandwidth, $h = 90$ days (or $h = 3$ months).\(^\text{17}\) The third specification uses the robust bias correction (RBC) method of Calonico et al. (2014). For proximity in days, we use the default “MSE optimal” bandwidth provided by their software package; for proximity in months we use $h = 3$.\(^\text{18}\)

\(^{17}\)These specifications can be interpreted as imposing a parametric linear functional form inside the estimation window. Alternatively, one can justify them by an “undersmoothing” argument: the specifications implicitly assume that the constants $M_y$ and $M_t$ are small enough so that the bias is negligible at these bandwidth choices.

\(^{18}\)The formal arguments justifying this method and the default bandwidth selector require the running
The last two methods implement the bias-aware approach. We use confidence intervals given in eq. (4) for the first stage and reduced form effects; for inference on the fuzzy RD estimand, we use the Noack and Rothe (2021) construction. Implementing these methods requires a choice of smoothness bounds for the first stage \((M_t)\) and the reduced form \((M_y)\). The fourth specification uses a rule of thumb (ROT) proposed by Armstrong and Kolesár (2020), which fits a global quartic regression on either side of the cutoff, and computes the largest (in absolute value) second derivative of the fitted line. The fifth specification uses the ROT of Imbens and Wager (2019), which uses a global quadratic regression instead, and, additionally, multiplies the largest second derivative of the fitted line by some moderate factor, taken here to be 2. To assess these rules, we use the visualization approach proposed in Noack and Rothe (2021), described and implemented in Section A.2. These visualizations suggest that the Armstrong and Kolesár (2020) ROT is quite conservative, while the second ROT delivers more optimistic smoothness bounds that generate reasonably smooth conditional mean functions. To make the smoothness constants comparable across the specifications, we report the implied smoothness constants after rescaling the running variable to have support \([-1, 1]\) (which amounts to multiplying the original smoothness constants by \(\max_i X_i^2 \) and \(\max_i (X_i^*)^2\), respectively). Given a choice of the smoothness constants, the bandwidth is selected so that the point estimate defined in eq. (3) minimizes the worst-case (over the chosen smoothness class) finite-sample MSE of the estimator. This is a well-defined object whether the parameter of interest is point or set-identified.

3.1 Results

Table 1 presents the first stage estimates, the effect of preregistration eligibility on the preregistration. The estimates are stable across the specifications, in the range of 38–40% when using proximity in days; the estimates using proximity in months are slightly lower, at around 37%, but still indicating a clear jump in the registration rate at the eligibility threshold. This is consistent with our theory, discussed in Remark 1, and reflects the difference between the parameters \(E[T(1) - T(0) | X^* = 0]\) (the effect for those born on the cutoff date, November 4) and \(E[T(1) - T(0) | X = 0]\) (the effect for those born in November, the cutoff month). In variable to be continuous, which is technically not the case in either design. When proximity is measured in months, this causes issues implementation issues with the default “MSE optimal” bandwidth calculations. Both rules of thumb can be formally justified by assumptions relating local and global smoothness of \(g\) and \(p\). See Armstrong and Kolesár (2020) for details.

Armstrong and Kolesár (2020) show that, if the support of \(X\) is regular, the resulting estimator is asymptotically nearly minimax over class of all estimators, not just local linear estimators. Imbens and Wager (2019) propose an alternative estimator that numerically approximates the finite-sample-minimax linear estimator. When the support of \(X\) is irregular, their estimator has lower worst-case MSE, in finite samples and also asymptotically.
Table 1: First stage estimates: effect of eligibility on preregistration.

<table>
<thead>
<tr>
<th></th>
<th>OLS</th>
<th>RBC</th>
<th>Bias-aware inference</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>(1)</td>
<td>(2)</td>
<td>(3)</td>
</tr>
<tr>
<td>Panel A: Proximity in days</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Estimate</td>
<td>0.384</td>
<td>0.379</td>
<td>0.393</td>
</tr>
<tr>
<td>SE</td>
<td>0.006</td>
<td>0.005</td>
<td>0.008</td>
</tr>
<tr>
<td>95% CI</td>
<td>(0.373,0.396)</td>
<td>(0.370,0.388)</td>
<td>(0.378,0.409)</td>
</tr>
<tr>
<td>Bandwidth</td>
<td>60</td>
<td>90</td>
<td>43</td>
</tr>
<tr>
<td>Eff. obs</td>
<td>63,220</td>
<td>94,118</td>
<td>43,538</td>
</tr>
<tr>
<td>Rescaled $M_t$</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Panel B: Proximity in months</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Estimate</td>
<td>0.365</td>
<td>0.363</td>
<td>0.368</td>
</tr>
<tr>
<td>SE</td>
<td>0.009</td>
<td>0.006</td>
<td>0.017</td>
</tr>
<tr>
<td>95% CI</td>
<td>(0.348,0.382)</td>
<td>(0.351,0.375)</td>
<td>(0.335,0.402)</td>
</tr>
<tr>
<td>Bandwidth</td>
<td>2</td>
<td>3</td>
<td>3</td>
</tr>
<tr>
<td>Eff. obs</td>
<td>64,011</td>
<td>94,662</td>
<td>94,662</td>
</tr>
<tr>
<td>Rescaled $M_t$</td>
<td>0.865</td>
<td>0.092</td>
<td></td>
</tr>
</tbody>
</table>

Notes: Column (1) uses local linear regression with bandwidth equal to 60 days (panel A) or 2 months (panel B), without any bias corrections. Column (2) is analogous, but uses bandwidth equal to 90 days or 3 months. Column (3) uses the RBC procedure, with the default “MSE optimal” bandwidth in panel A, and bandwidth equal to 3 months in panel B. Columns (4) and (5) report bias-aware confidence intervals, with bandwidth chosen to minimize the worst-case MSE. Column (4) uses the ROT of Armstrong and Kolesár (2020) to choose the smoothness constant $M_t$, while column (5) uses the ROT of Imbens and Wager (2019). Eff. obs refers to the number of observations inside the estimation window. The smoothness constants are reported after rescaling the running variable to have support $[-1,1]$.

Table 2 presents the reduced form estimates, the effect of preregistration eligibility on voting. The point estimates are about 3% for both designs and stable across specifications. The CIs are produced by the bias-aware specifications are wider when using proximity in months, reflecting the loss of point identification. This is in line with the discussion in Remark 4.

Table 3 presents the fuzzy RD estimates of the effect of preregistration on voting. When eligibility is measured in months, the smaller first stage estimates in panel B of Table 1...
Table 2: Reduced form estimates: effect of eligibility on voting.

<table>
<thead>
<tr>
<th></th>
<th>OLS</th>
<th>RBC</th>
<th>Bias-aware inference</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>(1)</td>
<td>(2)</td>
<td>(3)</td>
</tr>
<tr>
<td><strong>Panel A: Proximity in days</strong></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Estimate</td>
<td>0.028</td>
<td>0.027</td>
<td>0.032</td>
</tr>
<tr>
<td>SE</td>
<td>0.008</td>
<td>0.007</td>
<td>0.012</td>
</tr>
<tr>
<td>95% CI</td>
<td>(0.012, 0.044)</td>
<td>(0.014, 0.040)</td>
<td>(0.008, 0.056)</td>
</tr>
<tr>
<td>Bandwidth</td>
<td>60</td>
<td>90</td>
<td>38</td>
</tr>
<tr>
<td>Eff. obs</td>
<td>63,220</td>
<td>94,118</td>
<td>39,195</td>
</tr>
<tr>
<td>Rescaled $M_y$</td>
<td>1.401</td>
<td>0.099</td>
<td></td>
</tr>
<tr>
<td><strong>Panel B: Proximity in months</strong></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Estimate</td>
<td>0.037</td>
<td>0.034</td>
<td>0.042</td>
</tr>
<tr>
<td>SE</td>
<td>0.013</td>
<td>0.009</td>
<td>0.025</td>
</tr>
<tr>
<td>95% CI</td>
<td>(0.012, 0.062)</td>
<td>(0.017, 0.051)</td>
<td>(-0.007, 0.090)</td>
</tr>
<tr>
<td>Bandwidth</td>
<td>2</td>
<td>3</td>
<td>3</td>
</tr>
<tr>
<td>Eff. obs</td>
<td>64,011</td>
<td>94,662</td>
<td>94,662</td>
</tr>
<tr>
<td>Rescaled $M_y$</td>
<td>1.818</td>
<td>0.121</td>
<td></td>
</tr>
</tbody>
</table>

Notes: See Table 1.

translate to larger estimates of the effect of preregistration on voting, around 10%, compared to 7–8% when eligibility is measured in days. When eligibility is measured in months, the fuzzy RD estimand, $\tau_F$, is the ATE for compliers born in November 1990, and thus averages over individuals born further away from the cutoff than the estimand $\tau_F^*$ when eligibility is measured in months, which corresponds to the ATE for compliers born on November 4, 1990. If the treatment effect for compliers born $x$ days from the eligibility threshold, $\tau_F^*(x) = E[Y(1) - Y(0) \mid C, X^* = x]$, is increasing in $x$, then $\tau_F$ will be larger than $\tau_F^*$, which is consistent with the results in Table 3. However, the bias-aware confidence intervals are fairly wide, and also consistent with $\tau_F^*(x)$ being constant. As discussed in Remark 1, the policy relevance of $\tau_F$ vs $\tau_F^*$ will depend on the particular counterfactual one has in mind.

## 4 Summary and conclusions

Measurement error—especially in the form of coarsening or rounding—is a common feature of RD applications. We show that after possible doughnut trimming to ensure that the observed running variable $X$ correctly classifies the treatment assignment, such measurement
error does not have deleterious effects on the validity of existing inference methods, provided one uses methods that account for the potentially irregular support of the running variable, and loss of point identification that the measurement error may cause. We argue that bias-aware inference methods are well-suited for this purpose, as they automatically adapt to the observed support of the running variable.

Care needs to be taken when interpreting the estimand: it corresponds to the ATE for units with the observed running variable $X$ equal to the cutoff, rather than the usual parameter, the ATE for units with the latent running variable $X^*$ equal to the cutoff. We illustrate this point in an empirical application.
References


Appendix A  Auxiliary results

A.1 Effect of measurement error on smoothness of conditional mean

We now formalize the notion that measurement error smooths out non-linearities in $g_t^*$. To this end, first we introduce some definitions. For a real-valued function on a bounded set in $\mathbb{R}^d$, and a multi-index $\alpha = (\alpha_1, \ldots, \alpha_d)$, let $D^\alpha f = \partial^{\sum_{j=1}^d \alpha_j} f / \partial x_1^{\alpha_1} \cdots \partial x_d^{\alpha_d}$. For an integer $k$, let $\|f\|_{C^{k+1}} = \sum_{|\alpha| \leq k} \sup_x |D^\alpha f(x)| + \sum_{|\alpha| = k} \sup_{x \neq y} |f^{(k)}(x) - f^{(k)}(y)| / \|x - y\|$ denote the Hölder norm, with the convention that $\|f\|_{C^0} = \sup_x |f(x)|$, and that $\|f\|_{C^k} = \infty$ if $f$ is not $(k - 1)$-times differentiable. We say that $f$ has Hölder smoothness index $k$ if $\|f\|_{C^k} < \infty$ (cf. e.g. van der Vaart & Wellner, 1996, Section 2.7.1). This quantifies the “smoothness” of $f$ ($k$ is also called the Hölder exponent; for simplicity we focus attention on exponents that are integers). In other words, $f$ has smoothness $k$ if it is $k$ times differentiable almost everywhere, with the derivatives bounded.

The next result shows that if the conditional density of $f(e;x)$ of $e$ given $X = x$ is sufficiently smooth in the second argument (this condition holds automatically for Berkson measurement error, under which $f$ doesn’t depend on $x$), and $g_t^*(x,e)$ is also sufficiently smooth in the second argument (which holds trivially for non-differential measurement error), then the smoothness of $g_t$ is given by the sum of the smoothness indices of $x \mapsto g_t^*(x,e)$ and that of $e \mapsto f(e;x)$. This makes precise the notion that measurement error “smooths out” the non-linearities in $g_t^*$. 


Lemma A.1. Suppose that \((X, e)\) has bounded support, and that the distribution of \(e\) given \(X = x\) is continuous with bounded density \(f(e; x)\) such that \(\|\partial^s f / \partial x^s\|_{C^r} < \infty\) for some non-negative integers \(r, s\). Let \(h(x, e)\) be a function such that \(\text{sup}_e \|\partial^r h(\cdot, e) / \partial e^r\|_{C^s} < \infty\). Then \(g(x) := E[h(X + e, e) \mid X = x]\) has smoothness \(s + r\).

\textbf{Proof.} Since the lower-order derivatives \(D^{(0,k-1)} f(e; x)\) and \(D^{(k-1,0)} h(x + e, e)\) exist and are Lipschitz continuous for \(k \leq s\), by dominated convergence theorem, we can take a derivative under the integral sign using the Leibniz product rule, so that, for all \(x\),

\[
g^{(s)}(x) = \int \sum_{k=0}^{s} \binom{s}{k} D^{(k,0)} h(x + e, e) D^{(0,s-k)} f(e; x) de.
\]

Thus,

\[
g^{(r+s)}(x) = \sum_{k=0}^{s} \binom{s}{k} \frac{d^r}{dx^r} \int D^{(k,0)} h(y, y - x) D^{(0,s-k)} f(y - x; x) dy
\]

\[
= \sum_{k=0}^{s} \sum_{u=0}^{r} \sum_{v=0}^{s-k} \frac{1}{u!} \binom{s}{k} \binom{r}{u} (-1)^{r-u+v} \int D^{(k,r-u)} h(y, y - x) D^{(v,s-k+u-v)} f(y - x; x) dy,
\]

where the first line follows by change of variables, and the second line by the dominated convergence theorem and Leibniz product rule. Since \(D^{(k,r-u)} h(y, y - x)\) and \(D^{(v,s-k+u-v)} f(y - x; x)\) are bounded, it follows that \(g^{(r+s)}\) is bounded. \(\square\)

A.2 Visualization of smoothness constants

Here we assess the smoothness constants suggested by the ROTs using the visualization approach proposed in Noack and Rothe (2021). To explain the approach, suppose that we are interested in a sharp RD regression of an outcome \(\tilde{Y}_i\) on a running variable \(\tilde{X}_i\), and make the assumption that the conditional mean satisfies \(E[\tilde{Y}_i \mid \tilde{X}_i] \in \mathcal{F}_{RD}(M)\). To assess the plausibility of the smoothness bound \(M\), we regress \(\tilde{Y}_i\) on a basis function transformation \(g(\tilde{X}_i)\) of \(\tilde{X}_i\), and the interaction of \(g(\tilde{X}_i)\) with \(1\{\tilde{X}_i \geq 0\}\). To ensure that the estimated regression function lies in \(\mathcal{F}_{RD}(M)\), we minimize the sum of squared residuals subject to the constraint that the second derivative of the estimated regression function be no larger than \(M\), and equal \(M\) at the cutoff. If the basis is sufficiently flexible, the estimated regression function will tend to overfit the data, and therefore represent an extremal element of \(\mathcal{F}_{RD}(M)\). If the estimated regression function appears relatively smooth, this thus is an indicator that the choice of \(M\) is quite optimistic; if we are clearly overfitting the data, it signals that the choice of \(M\) is conservative—it is unlikely that \(E[\tilde{Y}_i \mid \tilde{X}_i]\) lies outside \(\mathcal{F}_{RD}(M)\).
Figure A.1: Visualization of extreme conditional mean functions in the class \( \mathcal{F}_{RD}(M_t) \), for different choices of the first stage smoothness constant \( M_t \).

Notes: Orange dotted line visualizes the choice of \( M_t \) based on the ROT proposed by Imbens and Wager (2019). Blue solid line visualizes \( M_t \) based on the ROT proposed by Armstrong and Kolesár (2020). Values of these constants are given in columns (4) and (5) of Table 1. In panel (a), proximity is measured in days, and each point corresponds to an average of 1,000 individuals. In panel (b), proximity is measured in months, and each point corresponds to an average across all individuals born in a given month.

We use this method to assess the plausibility of the ROTs that we used to calibrate the bounds \( M_y \) and \( M_t \) in the first stage and reduced form sharp RD regressions. In the former, the outcome \( \tilde{Y}_i \) corresponds to the treatment variable \( T_i \), while in the latter, \( \tilde{Y}_i = Y_i \). To implement the method, as a basis function, we use a quadratic spline with 21 knots on each side of the cutoff when proximity is measured in days, and with 6 knots when it is measured in months.

Figure A.1 visualizes the choices for the first stage smoothness constant \( M_t \), as estimated by the ROTs proposed by Armstrong and Kolesár (2020) and Imbens and Wager (2019). Both choices ROTs appear reasonable based on the figure. Figure A.2 gives an analogous visualization for the choices for the reduced form smoothness constant \( M_y \). Here the ROT proposed by Armstrong and Kolesár (2020) is quite conservative, while the Imbens and Wager (2019) ROT is more optimistic.
Figure A.2: Visualization of extreme conditional mean functions in the class $\mathcal{F}_{RD}(M_y)$, for different choices of the reduced form smoothness constant $M_y$.

Notes: Orange dotted line visualizes the choice of $M_y$ based on the ROT proposed by Imbens and Wager (2019). Blue solid line visualizes $M_y$ based on the ROT proposed by Armstrong and Kolesár (2020). Values of these constants are given in columns (4) and (5) of Table 2. In panel (a), proximity is measured in days, and each point corresponds to an average of 1,000 individuals. In panel (b), proximity is measured in months, and each point corresponds to an average across all individuals born in a given month.