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BOUNDS ON TREATMENT EFFECTS IN REGRESSION DISCONTINUITY DESIGNS UNDER MANIPULATION OF THE RUNNING VARIABLE, WITH AN APPLICATION TO UNEMPLOYMENT INSURANCE IN BRAZIL

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Bounds on Treatment Effects in Regression Discontinuity Designs under Manipulation of the Running Variable, with an Application to Unemployment Insurance in Brazil
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ABSTRACT

A key assumption in regression discontinuity analysis is that units cannot affect the value of their running variable through strategic behavior, or manipulation, in a way that leads to sorting on unobservable characteristics around the cutoff. Standard identification arguments break down if this condition is violated. This paper shows that treatment effects remain partially identified under weak assumptions on individuals' behavior in this case. We derive sharp bounds on causal parameters for both sharp and fuzzy designs, and show how additional structure can be used to further narrow the bounds. We use our methods to study the disincentive effect of unemployment insurance on (formal) reemployment in Brazil, where we find evidence of manipulation at an eligibility cutoff. Our bounds remain informative, despite the fact that manipulation has a sizable effect on our estimates of causal parameters.

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1. INTRODUCTION

In a regression discontinuity (RD) design, a unit is assigned to treatment if and only if the value of an observed running variable exceeds a known cutoff. This structure makes it possible to identify and estimate causal effects by comparing the outcomes of units on either side of the cutoff as long as the distribution of units' unobservable characteristics varies smoothly with the running variable (e.g. Hahn, Todd, and Van der Klaauw, 2001). In many empirical settings, however, units can influence their value of the running variable through strategic behavior, or manipulation. Manipulation can break the comparability of units on different sides of the cutoff, and the RD design no longer identifies the causal effect of the treatment in this case. Manipulation of the running variable is thus an important practical issue, and it is an issue that has been documented in many empirical contexts.[1]

In an influential paper, McCrary (2008) argues that a jump in the density of the running variable at the cutoff is a strong indication that a RD design is impacted by manipulation. It has therefore become standard practice in the applied literature to address concerns about manipulation by testing the null hypothesis that the density of the running variable varies smoothly around the cutoff. If this null hypothesis is not rejected, researchers typically proceed with their empirical analysis under the assumption that no manipulation occurs. In contrast, the cutoff is often no longer used for inference on treatment effects if this null hypothesis is rejected.[2] This practice is problematic for at least two reasons. First, a non-rejection may not be due to the absence of manipulation but to a lack of statistical power, e.g.

[1]For instance, Urquiola and Verhoogen (2009) document that schools manipulate enrollment to avoid having to add an additional classroom when faced with class-size caps in Chile. Other examples abound in the education literature (e.g. Card and Giuliano, 2014; Dee, Dobbie, Jacob, and Rockoff, 2014; Scott-Clayton, 2011) as well as in other fields (e.g. Camacho and Conover, 2011). Manipulation of running variables around discontinuities (or "notches") in tax and transfer systems has even generated its own literature in public finance (Kleven and Waseem, 2013).

[2]There is a small number of papers that develop solutions tailored to very specific empirical settings. For examples, see Bajari, Hong, Park, and Town (2011) or Davis, Engberg, Epple, Sieg, and Zimmer (2013). Another strategy to address manipulation that is sometimes put forward in the literature is the so-called "doughnut hole" approach. This method excludes observations around the cutoff, and then relies on extrapolation outside of the range of the remaining data to recover estimates of treatment effects at the cutoff. Of course, ignoring all data close to the cutoff is very much against the nonparametric spirit of the RD design.

due to a small sample size. Units just to the left and right of the cutoff could still differ in their unobservable characteristics in this case, and estimates ignoring this possibility may be severely biased. Second, even if one correctly rejects the null hypothesis of no manipulation, the extent of the problem could still be modest, and the data thus remain informative.

In this paper, we propose a partial identification approach (Manski, 2003, 2009) to dealing with the issue of potentially manipulated running variables in RD designs. We avoid making a binary decision based on a statistical test regarding whether manipulation occurs or not. Instead, our approach involves working with a general model which allows for the possibility of manipulation, and lets the data decide about its extent. This strategy leads to bounds on causal effects instead of delivering point identification, but these bounds can be informative and rule out non-trivial candidate values for the parameter of interest. It also leads to confidence intervals that are valid irrespective of the extent to which manipulation occurs.

Since manipulation can come in various forms and shapes, we consider a general setup in which there are two unobservable types of units: *always-assigned* units for which the realization of the running variable is always on one particular side of the cutoff (which we normalize to be the right side), and *potentially-assigned* units that behave as postulated by the standard assumptions of a RD design. This setup imposes only weak restrictions on individuals' actions, and thus covers a wide range of patterns of strategic behavior. The most immediate is one where always-assigned units have control over the value of the running variable and can ensure a realization that is to the right of the cutoff. However, we also discuss several other concrete examples of strategic behaviors that fit into our framework.

Our main identification analysis then focuses on the causal effect of the treatment on the mean and the quantiles of the outcomes among potentially-assigned units. First, taking the argument of McCrary (2008) one step further, we use the magnitude of the discontinuity in the density of the running variable at the cutoff to identify the proportion of always-assigned units among all units close to the cutoff. Second, we use this information to bound treatment effects by finding those "worst case" scenarios in which the distribution of outcomes among always-assigned units takes its "highest" and "lowest" feasible value (in a stochastic dominance sense). In the case of a sharp RD design, this leads to a simple bound based on trimming the tails of the outcome distribution among units just to the right of the cutoff.[3] For fuzzy RD designs, we derive more elaborate bounds that exploit the various shape restrictions implied by our model. To the best of our knowledge, these types of bounds are new to the literature. Our main identification results are then extended in several ways. We show that the bounds can be sharpened by using covariate information, or by imposing further assumptions about the behavior of economic agents. We also show that one can identify the distribution of covariates among always-assigned and potentially-assigned units at the cutoff. Finally, we derive bounds for treatment effects among the subpopulation at the cutoff that includes both potentially-assigned and always-assigned units.

To implement our approach in practice, we describe computationally convenient sample analogue estimators of our bounds, and confidence intervals for the causal parameters of interest based on recent methods from the literature on set inference (e.g. Imbens and Manski, 2004; Stoye, 2009; Andrews and Soares, 2010). We recommend the use of such confidence intervals in applications irrespective of the outcome of McCrary's (2008) test in order to ensure that inference is robust against the possibility of manipulation.

Lastly, we illustrate the usage of our approach by applying it to estimate the effect of unemployment insurance (UI) around an eligibility cutoff in Brazil. This application is also of empirical relevance in itself. First, UI programs typically specify minimum requirements, such as a minimum number of months of prior employment or since the last UI spell, for displaced workers to be eligible. Yet, the welfare effects of changes in such requirements have not been a focus of the optimal UI literature (Chetty and Finkelstein, 2013). Second,

[3]This result shares some similarities with that of Lee (2009) on bounding treatment effects in randomized experiments under sample selection; and several applied papers have used heuristic arguments to arrive at such a strategy (e.g. Card, Dobkin, and Maestas, 2009; Sallee, 2011; Anderson and Magruder, 2012; Schmieder, von Wachter, and Bender, 2012). Our contribution with regard to the sharp design is thus mainly to formalizing this approach. We also remark that Chen and Flores (2015) extend Lee (2009) to sample selection in randomized experiments with imperfect compliance; and Kim (2012) and Dong (2016b) extend Lee (2009) to sample selection in RD designs.

UI programs have been adopted or considered in a growing number of developing countries, but there is still limited evidence on their impacts (Gerard and Gonzaga, 2016). We find strong evidence of manipulation around the eligibility cutoff. Yet, we are able to infer that UI takeup increases the average paid UI duration and the average time it takes to return to a formal job by at least 35.4 and 42.9 days, respectively. We also show that bounds for quantile treatment effects are often more narrow than bounds on average treatment effects, because they are less sensitive to the tails of the outcome distribution. Together, our results imply that the efficiency cost of a policy that relaxes the eligibility condition by marginally changing the location of the cutoff amounts to at least 30% of its mechanical cost (absent behavioral responses). This figure implies a lower bound on the need for insurance among newly eligible workers for the policy to increase welfare.

The remainder of the paper is organized as follows. Section 2 introduces a framework for RD designs with manipulation. Section 3 contains our main partial identification results for treatment effects in both Sharp and Fuzzy RD designs, and Section 4 discusses several useful extensions. Sections 5 then describes our proposed methods for estimation and inference, which are applied to our empirical setting in Section 6. Finally, Section 7 concludes. Proofs and additional material can be found in the Appendix.

2. GENERAL FRAMEWORK FOR MANIPULATION IN RD DESIGNS

In this section, we first review the basic RD design, and then explain how we formally introduce manipulation into the setup. We discuss a number of examples that fit into our framework, and clarify the definition and interpretation of our parameters of interest.

2.1. **The Basic RD Design**

Suppose that we observe a random sample of n units, indexed by $i = 1, \ldots, n$, from some large population. Our interest is in the causal effect of a binary treatment on an outcome variable. The treatment effect is potentially heterogeneous among observational units, which could be individuals or firms for instance. Following Rubin (1974), each unit is therefore characterized by a pair of potential outcomes, $Y_i(1)$ and $Y_i(0)$, which denote the outcome of unit i with and without receiving the treatment, respectively. Out of these two potential outcomes, we only observe the one corresponding to the realized treatment status. Let $D_i \in \{0,1\}$ denote the treatment status of unit i, with $D_i = 1$ if unit i receives the treatment, and $D_i = 0$ if unit i does not receive the treatment. The observed outcome can then be written as $Y_i = D_i Y_i(1) + (1 - D_i) Y_i(0)$.

In an RD design, the treatment assignment is a deterministic function of a so-called running variable X_i that is measured prior to, or is not affected by, the treatment. Let $Z_i \in \{0,1\}$ denote the treatment assignment of unit i, with $Z_i = 1$ if unit i is assigned to receive the treatment, and $Z_i = 0$ if unit i is not assigned to receive the treatment. Then $Z_i = \mathbb{I}(X_i \geq c)$ for some fixed cutoff value c. Let the potential treatment status of unit i as a function of the running variable be $D_i(x)$, so that the observed treatment status is $D_i = D_i(X_i)$. Also define the limits $D_i^+ = D_i(c^+) \equiv \lim_{x \downarrow c} D_i(x)$ and $D_i^- = D_i(c^-) \equiv \lim_{x \uparrow c} D_i(x)$.[4] The extent to which units comply with their assignment distinguishes the two types of RD designs that are commonly distinguished in the literature: the Sharp RD design and the Fuzzy RD design. In a sharp design, compliance with the treatment assignment is perfect, and thus $D_i^+ = 1$ and $D_i^- = 0$ for all units i. In a fuzzy design, on the other hand, values of D_i^+ and D_i^- differ across units, but the conditional treatment probability $\mathrm{E}(D|X = x)$ is discontinuous at $x = c$.

2.2. **Manipulation**

Identification in standard RD designs relies on the assumption that the conditional distribution of units' unobservable characteristics given the running variable does not change in a discontinuous manner at the cutoff. Units on different sides of the cutoff are thus comparable except for their treatment assignments, and treatment effects can be identified by comparing outcomes (and treatment probabilities) of units on different sides of the cutoff. This approach

[4]Throughout the paper, we use the notation that $g(c^+) = \lim_{x \downarrow c} g(x)$ and $g(c^-) = \lim_{x \uparrow c} g(x)$ for a generic function $g(\cdot)$. We also follow the convention that whenever we take a limit we implicitly assume that this limit exists and is finite. Similarly, whenever an expectation or some other moment of a random variable is taken, it is implicitly assumed that the corresponding object exists and is finite.

to identification may break down if at least some units behave strategically and thereby influence the value of their running variable. Throughout the paper, we refer to any pattern of behavior that fits this broad description as manipulation.

Manipulation by itself does not necessarily break the comparability of units on different sides of the cutoff. For example, if students take a test, the presence of a pass/fail cutoff may increase effort among those who expect that their score will be close to the cutoff relative to those who are confident that they will pass. Despite such clear strategic behavior, the distribution of exerted effort should be the same among students just to the left and right of the cutoff in this case, and thus the causal effect of passing the test remains identified for units at the cutoff.[5] This result holds in fact more generally: an RD design identifies a meaningful causal effect as long as there is no manipulation that leads to a discontinuous change in the distribution of units' unobservable characteristics at the cutoff.

In this paper, we study settings where manipulation creates two unobservable types of units: *always-assigned* units whose value of the running variable only takes values on one particular side of the cutoff, which we normalize to be the right side without loss of generality; and *potentially-assigned* units who can potentially be observed on either side of the cutoff. Such a structure can arise from several types of behavior and is generally a problem from an identification point of view. The most immediate case is one where some units have control over the value of the running variable to the extent that they can ensure a realization to the right of the cutoff (and assignment to treatment is desirable for all units). Such a structure applies more broadly, however, and we provide concrete examples below of alternative mechanisms that also fit our framework.

More formally, let $M_i \in \{0, 1\}$ denote an indicator for the unobserved type of unit i, with $M_i = 1$ if unit i is always-assigned and $M_i = 0$ if unit i is potentially-assigned. We then impose three assumptions for our analysis. The first one implies that the standard conditions from the RD literature are satisfied among potentially-assigned units.

[5] Formally, this is the causal effect for a setup where units are aware of the existence of the cutoff. The causal effect may well be different in a hypothetical setting where students are unaware of its existence.

Assumption 1. *(i)* $\mathrm{P}(D = 1|X = c^+, M = 0) > \mathrm{P}(D = 1|X = c^-, M = 0)$*; (ii)* $\mathrm{P}(D^+ \geq D^-|X = c, M = 0) = 1$*; (iii)* $\mathrm{P}(Y(d) \leq y|D^+ = d^1, D^- = d^0, X = x, M = 0)$, $\mathrm{E}(Y(d)|D^+ = d^1, D^- = d^0, X = x, M = 0)$, $\mathrm{P}(D^+ = 1|X = x, M = 0)$ *and* $\mathrm{P}(D^- = 1|X = x, M = 0)$ *are continuous in x at c for $d, d^0, d^1 \in \{0, 1\}$ and all y; (iv) $F_{X|M=0}(x)$ is differentiable in x at c, and the derivative is strictly positive.*

This assumption is stated here for the general case of a Fuzzy RD design; many of its conditions simplify considerably if the RD is sharp.[6] Assumption 1(i) requires that the treatment probability changes discontinuously at the cutoff value of the running variable, with the direction of the change normalized to be positive. Assumption 1(ii) is a monotonicity condition stating that the response of treatment selection to crossing the cutoff is monotone for every unit. Assumption 1(iii) is a continuity condition which roughly speaking requires the distributions of potential outcomes and potential treatment status to be the same on both sides of the cutoff. Finally, Assumption 1(iv) implies that the running variable has a positive density at the cutoff, and thus that there are potentially-assigned units close to the cutoff on either side. Note that Assumptions 1(i)-(iii) simplify to the condition that $\mathrm{E}(Y(d)|X = x, M = 0)$ is continuous in x at c for $d \in \{0, 1\}$ for the special case of a Sharp RD design.

Assumption 2. *The derivative of $F_{X|M=0}(x)$ is continuous in x at c.*

Assumption 2 is a weak regularity condition on the distribution of the running variable among potentially-assigned units. Together with Assumption 1(iv), this assumption implies that the density of X_i among potentially-assigned units is smooth and strictly positive over some open neighborhood of c. Continuity of the running variable's density around the cutoff is a reasonable condition in applications, and is generally considered to be an indication for the absence of manipulation in the applied literature (McCrary, 2008).

[6] We define the RD design in terms of continuity conditions on the distributions of potential outcomes and treatment states as in Frandsen, Frölich, and Melly (2012), Dong (2016a) or Bertanha and Imbens (2016). This leads to the same identification results as directly imposing the local independence condition that the treatment effect is independent of the treatment status conditional on the running variable near the cutoff, as in Hahn, Todd, and Van der Klaauw (2001).

Assumption 3. *(i)* $\mathrm{P}(X \geq c|M = 1) = 1$*, (ii)* $F_{X|M=1}(x)$ *is right-differentiable in* x *at* c*.*

Assumption 3 is the only restriction we impose on the properties of always-assigned units in our setup. Its first part, which is key to our analysis, is the defining property of this group. Together with Assumption 1, it implies that the running variable only takes on values to the right of the cutoff among those units that are problematic for the validity of the standard RD design. This encompasses the setup discussed by McCrary (2008), but is more general since it does not require that always-assigned units have perfect control over the value of the running variable; see the discussion below for details. The second part rules out mass points in the distribution of X_i around the cutoff. In particular, it rules out that the running variable is exactly equal to the cutoff among always-assigned units. However, the distribution of X_i is allowed to be arbitrarily highly concentrated close to c. In view of Assumption 1(iv), this condition implies that a unit's type cannot simply be inferred from the value of its running variable (without such a condition the analysis would be trivial). It also implies that in the full population, which contains both always-assigned and potentially-assigned units, the observed running variable X_i is continuously distributed, with a density that is generally discontinuous at c. Moreover, Assumption 1(iv) and 3 together imply that none of the units observed to the left of the cutoff are of the always-assigned type, i.e. $\mathrm{P}\left(M = 1|X = c^-\right) = 0$, whereas to the right of the cutoff we observe a mixture of types.

2.3. **Discussion of Manipulation Setup**

Many different types of strategic behavior can generate subgroups of always-assigned and potentially-assigned units. To illustrate this point, consider the case of an income transfer program for which eligibility is based on a cutoff value of a poverty score X_i, and the formula used to calculate the score takes as inputs household characteristics recorded during home visits by local administrators. Programs of this type are found in many developing countries, and various types of manipulation have been documented in this context (Camacho and Conover, 2011). The following examples of strategic behavior all fit into our setup.

Example 1 (Usual RD setup)**.** Suppose that the formula for the poverty score is not publicly known. Then neither households nor local administrators can ensure program assignment through misreporting of input variables within reasonable bounds. There may still be some manipulation of the running variable, but there are no always-assigned households in this case. Every household is potentially-assigned and a standard RD design is valid.

Example 2 (Perfect control)**.** Suppose that some households know the formula for the poverty score, and local administrators turn a blind eye when the households report inaccurate information. Those households can report combinations of variables such that X_i is to the right of the cutoff. They are thus always-assigned, while all other households are potentially-assigned.[7]

Example 3 (Ex-post definition)**.** Alternatively, suppose that some local administrators refuse to collaborate such that only a fraction of households is able to carry out its intended manipulation. Only those households that succeed in manipulating the running variable would then be always-assigned. The subset of households whose manipulation efforts fail would be counted as potentially-assigned along with those households that never made a manipulation attempt.

Example 4 (Passive manipulation)**.** Suppose that households report information truthfully, but local administrators fill in combinations of variables such that X_i is to the right of the cutoff if a household strongly supports local elected officials. Such households are always-assigned, even though they are not engaging in any manipulation themselves.

Example 5 (Legitimate behavior)**.** Suppose that households can request a second home visit after learning the outcome of the first one, and that only the most recent score is relevant for program eligibility. Let X_{ji} be the poverty score based on the jth visit of household i, and suppose that households request a second visit if and only if they were ineligible based on the

[7]Misreporting households should have an incentive not to report information in such a away that their poverty score is exactly equal to the cutoff in order to avoid detection by e.g. central administrators. This makes the assumption of a continuously distributed running variable among always-assigned units palatable.

first visit. Then the observed running variable is $X_i = X_{1i} \cdot \mathbb{I}(X_{1i} \geq c) + X_{2i} \cdot \mathbb{I}(X_{1i} < c)$. All households with $X_{1i} \geq c$ are always-assigned, whereas all households that receive a second visit are potentially-assigned. Here "manipulation" occurs even though nobody is doing anything that is illegal or against the terms of the program.

One can easily construct further variants of these examples that also fit into our general setup. For instance, misreporting household information or requesting a second home visit may be costly, with the cost depending on the distance between the cutoff and the true or initial poverty score, respectively. Moreover, these examples have natural analogues in other contexts. Consider for instance an educational program for which students are eligible if their score in a test falls to the right of some cutoff. Teachers could then directly manipulate test scores, or students could retake the test if their score falls to the left of the cutoff. Our setup thus applies to a wide range of empirical settings.

2.4. **Parameters of Interest**

The framework that we use in this paper is very general and covers a wide range of patterns of strategic behavior. This is possible because we are rather agnostic about the exact mechanism through which manipulation occurs. On the flip-side, this also means that our model does not specify, for example, which value the running variable would take in the absence of manipulation.[8] Our identification analysis therefore focuses on causal parameters in subgroups of the population defined by the realized value of the running variable, and not by some hypothetical one that would have been observed in the absence of manipulation.

In RD designs without manipulation, the parameter of interest is commonly the difference of some feature of the distribution of the two potential outcomes among compliers at the cutoff. Such parameters can be written as

$$\theta\left(F_{Y(1)|X=c,D^+>D^-}\right) - \theta\left(F_{Y(0)|X=c,D^+>D^-}\right),$$

[8] More generally, no notion of "absence of manipulation" is well-defined in our framework. In particular, it follows from the discussion in the previous subsection that the absence of always-assigned units is not equivalent to the absence of strategic behavior.

where the notation $\theta(F)$ describes a real-valued parameter of a generic c.d.f. F. Examples of such parameters include the (local) average treatment effect, where $\theta(F) = \int y dF(y)$, and (local) quantile treatment effects, where $\theta(F) = F^{-1}(u)$ for some quantile level $u \in (0, 1)$. Under manipulation, however, some caution is needed since the function

$$x \mapsto \theta\left(F_{Y(d)|X=x,D^+>D^-}\right)$$

is generally not continuous at $x = c$ due to the possible shift in the composition of units at the cutoff. The value of this function at $x = c$ is thus not necessarily a meaningful object. We therefore consider parameters based on left and right limits of this function at the cutoff as our parameters of interest. In particular, we consider the (local) average treatment effects

$$\Gamma_- \equiv \mathrm{E}(Y(1) - Y(0)|X = c^-, D^+ > D^-) \quad \text{and}$$
$$\Gamma_+ \equiv \mathrm{E}(Y(1) - Y(0)|X = c^+, D^+ > D^-),$$

and the corresponding quantile treatment effects

$$\Psi_-(u) \equiv Q_{Y(1)|X=c^-,D^+>D^-}(u) - Q_{Y(0)|X=c^-,D^+>D^-}(u) \quad \text{and}$$
$$\Psi_+(u) \equiv Q_{Y(1)|X=c^+,D^+>D^-}(u) - Q_{Y(0)|X=c^+,D^+>D^-}(u),$$

where $u \in (0, 1)$ is the desired quantile level. Quantile treatment effects are interesting parameters to consider as they allow one to study causal effects on different parts of the outcome distribution. Quantile treatment effects are also less sensitive than average treatment effects to variation in the outer tails of the outcome distribution.

The parameters Γ_- and $\Psi_-(u)$ can be interpreted as causal RD parameters in the usual sense for the subgroup of potentially-assigned compliers. To see this, consider the case of the (local) average treatment effect, and note that by Assumption 1 the function

$$x \mapsto \mathrm{E}(Y(1) - Y(0)|X = x, D^+ > D^-, M = 0)$$

is continuous at $x = c$. Since there are only potentially-assigned units to the left of the cutoff in our setup, it follows that

$$\Gamma_- = \mathrm{E}(Y(1) - Y(0)|X = c, D^+ > D^-, M = 0),$$

and thus Γ_- can be interpreted as the "usual" RD parameter in the subgroup of potentially-assigned units. An analogous statement applies to the (local) quantile treatment effect $\Psi_-(u)$. These parameters are thus natural objects of interest, and, as we argue in the following subsection, are also a policy relevant quantity in many settings. Our main identification analysis below therefore focuses on Γ_- and $\Psi_-(u)$. Identification of Γ_+ and $\Psi_+(u)$ is a conceptually more involved issue, as these are causal parameters corresponding to a subpopulation that includes a group that is only observed on one side of the cutoff. We consider these parameters in an extension in Section 4.3 of this paper.

2.5. **Policy Relevance**

A common criticism of identification strategies that recover a local average (or quantile) treatment effect is that the corresponding subpopulation is not necessarily of particular interest from a policy point of view (e.g. Heckman and Urzua, 2010). One reason for the popularity of the RD design is that it mostly avoids this criticism in settings where every unit is potentially-assigned. In this case, the usual RD parameters capture the causal effect for the subpopulation of units whose treatment status would directly change following a marginal change in the level of the cutoff, which is often a feasible policy option.[9]

Our parameter Γ_- (and its quantile analogue) typically retains a similar sense of policy relevance in the presence of always-assigned units. To see this, it is useful to consider the various examples laid out in Section 2.3 above. It is clear that marginal changes in the level

[9] One may add the qualifier that, in settings where units can influence their value of the running variable (but not to the extent that they can ensure being assigned to the treatment, and thus break the comparability of units on different sides of the cutoff), a cutoff change may also affect the treatment status of units away from the cutoff. To see this, consider the educational program example at the end of Section 2.3, but assume that a test score is only the result of a student's true effort. Low-ability students, who may otherwise be discouraged, may think that they have a better chance to qualify if the cutoff value decreases, and exert more effort in response. As a result, some of them may improve their test score above the new cutoff value, and be assigned to the treatment. RD estimates of course do not provide any information regarding the causal effect of the treatment for this subpopulation. This limitation also applies in the cases that we consider.

of the cutoff affect program eligibility for potentially-assigned households in all examples, as the standard RD framework holds for this subpopulation. The parameter Γ_- therefore always captures a policy-relevant effect. The degree of its policy relevance, however, depends on the extent to which the treatment status of always-assigned units changes in response to a small change in the cutoff value. In those examples involving active manipulation, it may be reasonable to assume that always-assigned units have the ability to remain above cutoff, and maintain their treatment status, when the cutoff changes. Γ_- captures the full policy relevant effect in that case. Yet, even with active manipulation, some always-assigned units may fall below the cutoff in response to a small change in its value, and Γ_- then only captures part of the policy relevant effect. This would be the case for example under a model where active manipulation is costly, and the cost is increasing in the difference between a "true" (or "unmanipulated") level of the running variable and the cutoff. Always-assigned units may also change their treatment status in the example involving a second home visit.[10]

Our parameter Γ_+ (and its quantile analogue), in contrast, is typically of minor policy relevance. As is clear from the above discussion, it only captures the full policy relevant effect when *every* always-assigned unit changes its treatment status in response to a cutoff change. This appears to be an unrealistic prospect in all of the examples that we consider.

3. IDENTIFICATION UNDER MANIPULATION: MAIN RESULTS

Since we cannot infer whether any given unit is always-assigned or potentially-assigned, the parameters of interest are generally not point identified. In this section, we therefore derive bounds on both mean and quantile effects. We first obtain some preliminary results on the proportion of always-assigned units at the cutoff. We then use these results to derive bounds for the special case of a Sharp RD design, and finally extend the analysis to the general case of a Fuzzy RD design.

[10] If the cutoff changes from c to $\bar{c}$, households with $X_{1i} \in [c, \bar{c})$ and $X_{2i} < \bar{c}$, which used to be always-assigned, no longer receive the treatment.

3.1. **Proportion of Always-Assigned Units**

We begin by studying the identification of two important intermediate quantities: the proportion of always-assigned units among all units just to the right of the cutoff, and the proportion of always-assigned units among units with treatment status $d \in \{0, 1\}$ just to the right of the cutoff. We denote these quantities by

$$\tau \equiv \mathrm{P}(M = 1|X = c^+) \quad \text{and} \quad \tau_d \equiv \mathrm{P}(M = 1|X = c^+, D = d), \quad d \in \{0, 1\}, \tag{3.1}$$

respectively. While we cannot observe or infer the type of any given unit, under our assumptions we can point identify τ from the size of the discontinuity in the density f_X of the observed running variable at the cutoff.

Lemma 1. *If Assumptions 1–3 hold, then* $\tau = 1 - f_X(c^-)/f_X(c^+)$ *is point identified.*

In contrast, the two probabilities τ_1 and τ_0 are not point identified but only partially identified under our assumptions. To see this, note that there are two logical restrictions on the range of their plausible values. By the law of total probability and our monotonicity condition in Assumption 1(i), any pair of candidate values for $(\tau_1, \tau_0) \in [0, 1]^2$ has to satisfy the following two conditions:

$$\tau = \tau_1 \cdot \mathrm{E}(D|X = c^+) + \tau_0 \cdot (1 - \mathrm{E}(D|X = c^+)) \quad \text{and}$$
$$\mathrm{E}(D|X = c^+) \cdot \frac{1 - \tau_1}{1 - \tau} > \mathrm{E}(D|X = c^-).$$

With $\mathcal{T}$ denoting the set containing those $(\tau_1, \tau_0) \in [0, 1]^2$ that satisfy these two restrictions, we have the following result.

Lemma 2. *If Assumptions 1–3 hold, the set* $\mathcal{T}$ *is the sharp identified set for* (τ_1, τ_0).

Geometrically, the set $\mathcal{T}$ is a straight line in the unit square. For our following analysis, it is notationally convenient to represent this set in terms of the location of the endpoints of the line. That is, we can write

$$\mathcal{T} = \{(\eta_1(t), \eta_0(t)) : t \in [0,1]\} \quad \text{with} \quad \eta_d(t) = \tau_d^L + t \cdot (\tau_d^U - \tau_d^L)$$

for $d \in \{0,1\}$, where

$$\tau_1^L = \max\left\{0, 1 - \frac{1-\tau}{g^+}\right\}, \qquad \tau_1^U = \min\left\{1 - \frac{(1-\tau)\cdot g^-}{g^+}, \frac{\tau}{g^+}\right\},$$
$$\tau_0^L = \min\left\{1, \frac{\tau}{1-g^+}\right\}, \qquad \tau_0^U = \max\left\{0, \tau - \frac{(1-\tau)\cdot(g^+ - g^-)}{1-g^+}\right\},$$

using the shorthand notation that $g^+ = \mathrm{E}(D|X = c^+)$ and $g^- = \mathrm{E}(D|X = c^-)$.

3.2. Treatment Effects Among Potentially-Assigned Units

Using the above results, we now derive sharp lower and upper bounds on the parameters Γ_- and $\Psi_-(u)$. To simplify the exposition, we define the following subpopulations for $m \in \{0,1\}$:

- $C_m = \{D^+ > D^-, M = m\}$, the compliers;
- $A_m = \{D^+ = D^- = 1, M = m\}$, the always-takers;
- $N_m = \{D^+ = D^- = 0, M = m\}$, the never-takers.

Our main parameters of interest can thus be written as

$$\Gamma_- \equiv \mathrm{E}(Y(1) - Y(0)|X = c, C_0) \quad \text{and} \quad \Psi_-(u) \equiv Q_{Y(1)|X=c,C_0}(u) - Q_{Y(0)|X=c,C_0}(u),$$

respectively. Our general strategy is to first obtain sharp lower and upper bounds, in a first-order stochastic dominance sense, on the c.d.f. $F_{Y(d)|X=c,C_0}$, for $d \in \{0,1\}$. That is, we derive c.d.f.s F_d^U and F_d^L that are feasible candidates for $F_{Y(d)|X=c,C_0}$, i.e. they are compatible with our assumptions and the population distribution of observable quantities, and that are such that $F_d^U \succeq F_{Y(d)|X=c,C_0} \succeq F_d^L$, where $\succeq$ denotes first-order stochastic dominance.[11] Once such F_d^U and F_d^L have been obtained, it follows from Stoye (2010, Lemma 1) that sharp upper

[11]For two generic c.d.f.s A and B, we say that $A \succeq B$ if and only if $A(y) \leq B(y)$ for all y.

and lower bounds on Γ_- are given, respectively, by

$$\Gamma_-^U \equiv \int y dF_1^U(y) - \int y dF_0^L(y) \quad \text{and} \quad \Gamma_-^L \equiv \int y dF_1^L(y) - \int y dF_0^U(y),$$

whereas sharp upper and lower bounds on $\Psi_-(u)$ are given, respectively, by

$$\Psi_-^U(u) \equiv Q_1^U(u) - Q_0^L(u) \quad \text{and} \quad \Psi_-^L(u) \equiv Q_1^L(u) - Q_0^U(u),$$

with $Q_d^j(u) = \inf\{y \in \mathbb{R} : F_d^j(y) \geq u\}$ denoting the inverse of F_d^j for $d \in \{0,1\}$ and $j \in \{U, L\}$. It is instructive to first consider a Sharp RD design before studying the more general case of a Fuzzy RD design. For notational convenience, all results in this section are stated for the special case of a continuously distributed outcome variable (see Appendix B for an extension to outcomes whose distribution has mass points).

Sharp RD Designs. In a Sharp RD design every unit is a complier, and thus receives the treatment if and only if its value of the running variable is to the right of the cutoff. Since every unit just to the left of the cutoff is potentially-assigned in our setup, the distribution of Y in this subpopulation coincides with the distribution of $Y(0)$ among potentially-assigned compliers (C_0) at the cutoff:

$$F_{Y(0)|X=c,C_0}(y) = F_{Y|X=c^-}(y).$$

We therefore only need to bound the distribution of $Y(1)$ among potentially-assigned compliers at the cutoff. Information about $Y(1)$ is only contained in the subpopulation of treated units, which contains potentially- and always-assigned compliers (C_0 and C_1). The share of the latter type of unit is given by

$$\mathrm{P}(C_1|X = c^+) = \tau$$

in our setting. Since $\tau = 1 - f_X(c^-)/f_X(c^+)$ is point identified (see Lemma 1), we can obtain bounds using a strategy similar to that in Lee (2009) for sample selection in randomized experiments. In particular, a sharp upper bound on $F_{Y(1)|X=c,C_0}(y)$, in a first-order stochastic dominance sense, is obtained by truncating the distribution $F_{Y|X=c^+}(y)$ below its τ-quantile, and a sharp lower bound is obtained analogously by truncating $F_{Y|X=c^+}(y)$ above its $(1-\tau)$-quantile. That is, the bounds on $F_{Y(1)|X=c,C_0}(y)$ are given, respectively, by

$$F^U_{1,SRD}(y) = F_{Y|X=c^+,Y\geq Q_{Y|X=c^+}(\tau)}(y) \quad \text{and} \quad F^L_{1,SRD}(y) = F_{Y|X=c^+,Y\leq Q_{Y|X=c^+}(1-\tau)}(y).$$

These bounds correspond to the two "extreme" scenarios in which the proportion $1-\tau$ of units just to the right of the cutoff with either the highest or the lowest outcomes are the potentially-assigned units. These bounds are sharp because both of these "extreme" scenarios are empirically feasible. The following theorem translates these findings into explicit bounds on Γ_- and $\Psi_-(u)$.

Theorem 1. *Suppose Assumptions 1–3 hold, that $P(D^+ > D^-) = 1$, and that $F_{Y|X=c^+}(y)$ is continuous in y. Then sharp lower and upper bounds on Γ_- are given by*

$$\begin{aligned}
\Gamma^L_{-,SRD} &= \mathrm{E}(Y|X=c^+, Y \leq Q_{Y|X=c^+}(1-\tau)) - \mathrm{E}(Y|X=c^-) \quad \text{and} \\
\Gamma^U_{-,SRD} &= \mathrm{E}(Y|X=c^+, Y \geq Q_{Y|X=c^+}(\tau)) - \mathrm{E}(Y|X=c^-),
\end{aligned}$$

respectively; and sharp lower and upper bounds on $\Psi_-(u)$ are given by

$$\begin{aligned}
\Psi^L_{-,SRD}(u) &= Q_{Y|X=c^+}((1-\tau)u) - Q_{Y|X=c^-}(u) \quad \text{and} \\
\Psi^U_{-,SRD}(u) &= Q_{Y|X=c^+}(\tau + (1-\tau)u) - Q_{Y|X=c^-}(u),
\end{aligned}$$

respectively, for every quantile level $u \in (0,1)$.

Fuzzy RD Designs. In a Fuzzy RD design, the population might contain always-takers and never-takers in addition to compliers, and each unit is either potentially assigned or always-assigned. Overall, there are thus six different types of units; and there are also four possible combinations of treatment assignments and treatment decisions. The relationship between these groups is given in Table 1. Recall that we want to derive bounds on the distributions of the two potential outcomes among potentially-assigned compliers (C_0) at the cutoff. To do so, we first obtain bounds for the hypothetical case in which the true values of

Table 1: Allocation of Units' Types in the Fuzzy RD Design

Subset of population	Types of units present
$X = c^+, D = 1$	$C_0,\ C_1,\ A_0,\ A_1$
$X = c^-, D = 1$	A_0
$X = c^+, D = 0$	$N_0,\ N_1$
$X = c^-, D = 0$	$C_0,\ N_0$

Note: See the beginning of Section 3.2 for a definition of units' types.

τ_1 and τ_0, defined in (3.1), are actually known, and not only partially identified. In a second step, we then extend the result to our actual setting in which we only know that $(\tau_1, \tau_0) \in \mathcal{T}$.

Step 1. We begin by considering bounds on $F_{Y(1)|X=c,C_0}$. Information about the distribution of $Y(1)$ is only contained in the data on treated units. From Table 1, we see that the subpopulation of treated units just to the left of the cutoff consists exclusively of potentially-assigned always-takers (A_0). The c.d.f. $F_{Y(1)|X=c,A_0}$ is therefore point identified:

$$F_{Y(1)|X=c,A_0}(y) = F_{Y|X=c^-,D=1}(y).$$

Using simple algebra, we also find that the proportion of potentially-assigned always-takers (A_0) among treated units just to the right of the cutoff, which we denote by κ_1, is point identified in our setting as well:

$$\kappa_1 \equiv \mathrm{P}(A_0|X = c^+, D = 1) = (1 - \tau) \cdot \frac{\mathrm{E}(D|X = c^-)}{\mathrm{E}(D|X = c^+)}. \tag{3.2}$$

It then follows from the law of total probability that the c.d.f. $F_{Y(1)|X=c,C_0 \cup C_1 \cup A_1}$, which we denote by G to simplify the notation, is also point identified:

$$G(y) \equiv F_{Y(1)|X=c,C_0 \cup C_1 \cup A_1}(y) = \frac{1}{1-\kappa_1}\left(F_{Y|X=c^+,D=1}(y) - \kappa_1 F_{Y|X=c^-,D=1}(y)\right).$$

The c.d.f. $F_{Y(1)|X=c,C_0}$ can now be bounded sharply by considering the two "extreme" scenarios in which potentially-assigned compliers (C_0) are those units just to the right of the cutoff in the subpopulation $C_0 \cup C_1 \cup A_1$ with either the highest or the lowest outcomes. The share of

C_0 units in this subpopulation is

$$\mathrm{P}(C_0|X = c^+, C_0 \cup A_1 \cup C_1) = 1 - \frac{\tau_1}{1-\kappa_1}.$$

Given knowledge of τ_1, we therefore obtain a sharp upper bound on $F_{Y(1)|X=c,C_0}$, in a first-order stochastic dominance sense, by truncating the distribution G below its $\tau_1/(1-\kappa_1)$ quantile, and we analogously obtain a sharp lower bound by truncating G above its $1-\tau_1/(1-\kappa_1)$ quantile. With some algebra, these bounds on $F_{Y(1)|X=c,C_0}$ given knowledge of (τ_1, τ_0) can be written, respectively, as

$$\begin{aligned} F^U_{1,FRD}(y, \tau_1, \tau_0) &= \frac{(1-\kappa_1)\cdot G(y) - \tau_1}{1-\kappa_1-\tau_1} \cdot \mathbb{I}\left\{y \geq G^{-1}\left(\frac{\tau_1}{1-\kappa_1}\right)\right\} \qquad \text{and} \\ F^L_{1,FRD}(y, \tau_1, \tau_0) &= \frac{(1-\kappa_1)\cdot G(y)}{\tau_1} \cdot \mathbb{I}\left\{y \leq G^{-1}\left(1-\frac{\tau_1}{1-\kappa_1}\right)\right\}, \end{aligned}$$

Next, we consider bounds on $F_{Y(0)|X=c,C_0}$. Information about the distribution of $Y(0)$ is only contained in the data on untreated units. From Table 1, we see that untreated potentially-assigned compliers (C_0) cannot be observed in isolation, but only together with potentially-assigned never-takers (N_0) in the subpopulation of untreated units just to the left of the cutoff. Given knowledge of τ_0, the share of the latter type of units, which we denote by $\kappa_0 \cdot (1-\tau_0)$, is point identified:

$$\mathrm{P}(N_0|X = c^-, D = 0) = \kappa_0 \cdot (1-\tau_0), \qquad \kappa_0 = \frac{1}{1-\tau} \cdot \frac{1-\mathrm{E}(D|X=c^+)}{1-\mathrm{E}(D|X=c^-)}. \tag{3.3}$$

If we were to use only information from untreated units just to the left of the cutoff, we could therefore obtain lower and upper bounds on $F_{Y(0)|X=c,C_0}(y)$ by truncating the distribution $F_{Y|X=c^-,D=0}(y)$ below its $\kappa_0\cdot(1-\tau_0)$ quantile and above its $1-\kappa_0\cdot(1-\tau_0)$ quantile, respectively. However, such bounds are generally not sharp. This is because they correspond to "extreme" scenarios in which potentially-assigned never-takers (N_0) have either the highest or the lowest outcomes among untreated units just to the left of the cutoff. By Assumption 1, however, the c.d.f. $F_{Y(0)|X=x,N_0}(y)$ varies continuously in x around the cutoff, and thus these two "extreme" scenarios might be at odds with the distribution of outcomes that we observe among untreated units just to the right of the cutoff.

From Table 1, we see that the subpopulation of untreated units just to the right of the cutoff consists of potentially- and always-assigned never-takers (N_0 and N_1), and the share of the former type of units is

$$\mathrm{P}(N_0|X = c^+, D = 0) = 1 - \tau_0.$$

This means that we can write the density $f_{Y(0)|X=c,N_0}(y)$ in two different ways (assuming that $\kappa_0 > 0$ and $\tau_0 < 1$):

$$f_{Y(0)|X=c,N_0}(y) = \frac{f_{Y|X=c^-,D=0}(y) - (1 - \kappa_0 \cdot (1 - \tau_0))f_{Y(0)|X=c,C_0}(y)}{\kappa_0 \cdot (1 - \tau_0)} \quad \text{and} \tag{3.4}$$

$$f_{Y(0)|X=c,N_0}(y) = \frac{f_{Y|X=c^+,D=0}(y) - \tau_0 f_{Y(0)|X=c,N_1}(y)}{1 - \tau_0}. \tag{3.5}$$

To be compatible with the distribution of Y among untreated units on either side of the cutoff, any candidate for $f_{Y(0)|X=c,N_0}(y)$ has to be bounded from above, for every value y, by

$$s(y) \equiv \frac{\min\left\{f_{Y|X=c^-,D=0}(y)/\kappa_0, f_{Y|X=c^+,D=0}(y)\right\}}{1 - \tau_0}.$$

This is because otherwise either $f_{Y(0)|X=c,C_0}(y)$ or $f_{Y(0)|X=c,N_1}(y)$ would have to take a negative value in order for equations (3.4)–(3.5) to be satisfied, which is of course not possible since they are density functions. The most "extreme" feasible candidates for $F_{Y(0)|X=c,N_0}(y)$, which put as much probability mass as possible to one of the tail regions of the support of the outcome variable, are then given by

$$F^U_{Y(0)|X=c,N_0}(y) = \int_{-\infty}^{y} s(t)\mathbb{I}\left\{t \geq q_U\right\} dt \quad \text{and} \quad F^L_{Y(0)|X=c,N_0}(y) = \int_{-\infty}^{y} s(t)\mathbb{I}\left\{t \leq q_L\right\} dt,$$

respectively, where q_U and q_L are constants such that

$$\int_{q_U}^{\infty} s(t)dt = \int_{-\infty}^{q_L} s(t)dt = 1.$$

Figure 3.1 illustrates this construction. Note that we leave the dependence of $s(y)$, q_U and q_L on τ_0 implicit in our notation. The "extreme" candidates for $F_{Y(0)|X=c,N_0}(y)$ directly correspond

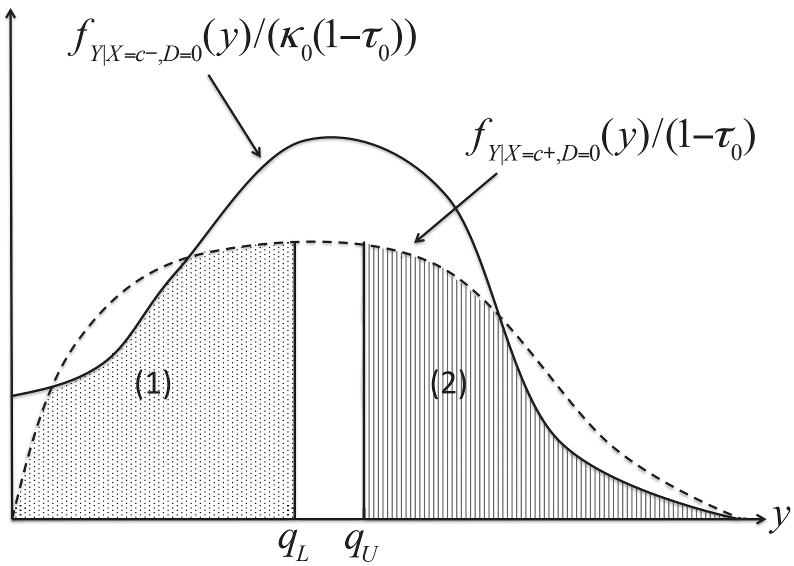

Figure 3.1: Construction of upper and lower bounds for $F_{Y(0)|X=c,N_0}$

The figure illustrates the construction of upper and lower bounds on $F_{Y(0)|X=c,N_0}$. The solid and dotted lines represent the graph of the functions $f_{Y|X=c^-,D=0}(y)/((1-\tau_0)\kappa_0)$ and $f_{Y|X=c^+,D=0}(y)/(1-\tau_0)$, respectively. The function $s(y)$ is the pointwise minimum of these to functions. The upper contours of the shaded areas (1) and (2) then correspond to the densities of $F^L_{Y(0)|X=c,N_0}$ and $F^U_{Y(0)|X=c,N_0}$, respectively, as the constants q_L and q_U are chosen such that the surface of the shaded areas is equal to 1.

to "extreme" candidates for the density $f_{Y(0)|X=c,C_0}(y)$ through the relationship (3.4), which in turn yields the following sharp upper and lower bounds, in a first-order stochastic dominance sense, on the c.d.f. $F_{Y(0)|X=c,C_0}$ given knowledge of (τ_1, τ_0):

$$F^U_{0,FRD}(y,\tau_1,\tau_0) = \frac{F_{Y|X=c^-,D=0}(y) - \kappa_0 \cdot (1-\tau_0)F^L_{Y(0)|X=c,N_0}(y)}{1-\kappa_0 \cdot (1-\tau_0)} \quad \text{and}$$
$$F^L_{0,FRD}(y,\tau_1,\tau_0) = \frac{F_{Y|X=c^-,D=0}(y) - \kappa_0 \cdot (1-\tau_0)F^U_{Y(0)|X=c,N_0}(y)}{1-\kappa_0 \cdot (1-\tau_0)}.$$

In the special case that $s(\cdot)$ is a proper density function these two bounds coincide, and thus the c.d.f. $F_{Y(0)|X=c,C_0}$ is point identified. The function $s(\cdot)$ is a density if $\tau_0 = 0$ or $\mathrm{E}(D|X = c^+) = 1$, for example.

Step 2. The above analysis shows that if we knew the values of τ_1 and τ_0, sharp upper and lower bounds on the local average treatment effect Γ_- would be given by

$$\begin{aligned}\Gamma^U_{-,FRD}(\tau_1,\tau_0) &\equiv \int y dF^U_{1,FRD}(y,\tau_1,\tau_0) - \int y dF^L_{0,FRD}(y,\tau_1,\tau_0) \quad \text{and} \\ \Gamma^L_{-,FRD}(\tau_1,\tau_0) &\equiv \int y dF^L_{1,FRD}(y,\tau_1,\tau_0) - \int y dF^U_{0,FRD}(y,\tau_1,\tau_0),\end{aligned} \tag{3.6}$$

respectively. Similarly, sharp upper and lower bounds on the local quantile treatment effect $\Psi_-(u)$ would be given by

$$\begin{aligned}\Psi^U_{-,FRD}(u,\tau_1,\tau_0) &\equiv Q^U_{1,FRD}(u,\tau_1,\tau_0) - Q^L_{0,FRD}(u,\tau_1,\tau_0) \quad \text{and} \\ \Psi^L_{-,FRD}(u,\tau_1,\tau_0) &\equiv Q^L_{1,FRD}(u,\tau_1,\tau_0) - Q^U_{0,FRD}(u,\tau_1,\tau_0).\end{aligned} \tag{3.7}$$

These bounds are not practically useful by themselves since τ_1 and τ_0 are, following the result in Lemma 2, only partially identified in our setup. However, we can find sharp bounds on Γ_- and $\Psi_-(u)$ by finding those values of $(\tau_1,\tau_0)\in\mathcal{T}$ that lead to the most extreme values of the quantities defined in (3.6) and (3.7). These bounds are sharp because they are based on assigning "worst case" distribution of the potential outcomes to each of the six groups mentioned in Table 1 that satisfy our assumptions and are compatible with the distribution of observables. The next theorem formally states the main finding of our identification analysis.

Theorem 2. *Suppose that Assumptions 1–3 hold, and that $F_{Y|XD}(y|c^+,d)$ and $F_{Y|XD}(y|c^-,d)$ are continuous in y for $d\in\{0,1\}$. Then sharp lower and upper bounds on Γ_- are given by*

$$\Gamma^L_{-,FRD} = \inf_{(t_1,t_0)\in\mathcal{T}} \Gamma^L_{-,FRD}(t_1,t_0) \quad \text{and} \quad \Gamma^U_{-,FRD} = \sup_{(t_1,t_0)\in\mathcal{T}} \Gamma^U_{-,FRD}(t_1,t_0),$$

respectively; and sharp lower and upper bounds on $\Psi_-(u)$ are given by

$$\Psi^L_{-,FRD}(u) = \inf_{(t_1,t_0)\in\mathcal{T}} \Psi^L_{-,FRD}(u,t_1,t_0) \quad \text{and} \quad \Psi^U_{-,FRD}(u) = \sup_{(t_1,t_0)\in\mathcal{T}} \Psi^U_{-,FRD}(u,t_1,t_0),$$

respectively, for every quantile level $u\in(0,1)$.

If our model is incorrect, in the sense that some of the provisions in Assumption 1–3 do not hold, the quantities in (3.6) and (3.7) might not be well-defined. For example, the function $G(y)$ might not be a proper c.d.f., or the function $s(y)$ might not integrate to a number greater than or equal to one. In this case the identified set for Γ_- and $\Psi_-(u)$ is simply the empty set, as there is no candidate value that is compatible with the assumptions and the distribution of observables.

3.3. Adding Behavioral Assumptions in Fuzzy RD Designs

The bounds derived in Theorem 2 can be made more narrow by imposing stronger assumptions on the units' behavior. Such additional behavioral restrictions often arise naturally in certain empirical contexts. Consider for instance a setting where always-assigned units obtain values of the running variable to the right of the cutoff by misreporting some information. Since such units actively choose to be eligible for the treatment, it seems plausible to assume that their probability of actually receiving the treatment conditional on being eligible is relatively high in some appropriate sense. One might be willing to assume, for example, that always-assigned units are at least as likely to get treated as eligible potentially-assigned units. The following theorem studies the implications of this assumption.

Theorem 3. *Suppose that the conditions of Theorem 2 hold, and that* $\mathrm{E}(D|X = c^+, M = 1) \geq \mathrm{E}(D|X = c^+, M = 0)$*. Then* $\mathcal{T}_a \equiv \{(t_1, t_0) : (t_1, t_0) \in \mathcal{T} \text{ and } t_1 \geq \tau\}$ *is the sharp identified set for* (τ_1, τ_0)*; sharp lower and upper bounds on* Γ_- *are given by*

$$\Gamma^L_{-,FRD(a)} = \inf_{(t_1,t_0)\in\mathcal{T}_a} \Gamma^L_{-,FRD}(t_1, t_0) \quad \text{and} \quad \Gamma^U_{-,FRD(a)} = \sup_{(t_1,t_0)\in\mathcal{T}_a} \Gamma^U_{-,FRD}(t_1, t_0),$$

respectively; and sharp lower and upper bounds on $\Psi_-(u)$ *are given by*

$$\Psi^L_{-,FRD(a)}(u) = \inf_{(t_1,t_0)\in\mathcal{T}_a} \Psi^L_{-,FRD}(u, t_1, t_0) \quad \text{and} \quad \Psi^U_{-,FRD(a)}(u) = \sup_{(t_1,t_0)\in\mathcal{T}_a} \Psi^U_{-,FRD}(u, t_1, t_0),$$

respectively, for every quantile level $u \in (0, 1)$*.*

In some cases, it may be reasonable to drive this line of reasoning further and consider the identifying power of the assumption that always-assigned units *always* receive the treatment, which is equivalent to assuming that no always-assigned unit is a never-taker. The following theorem provides expressions for the bounds under this assumption.

Theorem 4. *Suppose that the conditions of Theorem 2 hold, and that* $\mathrm{E}(D|X=c^+, M=1)=1$*. Then* $\tau_1=\tau/\mathrm{E}(D|X=c^+)$ *and* $\tau_0=0$ *are point identified; sharp lower and upper bounds on* Γ_- *are given by*

$$\Gamma^L_{-,FRD(b)}=\Gamma^L_{-,FRD}\left(\frac{\tau}{\mathrm{E}(D|X=c^+)},0\right) \quad \text{and} \quad \Gamma^U_{-,FRD(b)}=\Gamma^U_{-,FRD}\left(\frac{\tau}{\mathrm{E}(D|X=c^+)},0\right),$$

respectively; and sharp lower and upper bounds on $\Psi_-(u)$ *are given by*

$$\begin{aligned}\Psi^L_{-,FRD(b)}(u)&=\Psi^L_{-,FRD}\left(u,\frac{\tau}{\mathrm{E}(D|X=c^+)},0\right) \quad \text{and}\\ \Psi^U_{-,FRD(b)}(u)&=\Psi^U_{-,FRD}\left(u,\frac{\tau}{\mathrm{E}(D|X=c^+)},0\right),\end{aligned}$$

respectively, for every quantile level $u\in(0,1)$*.*

Comparing the first part of the Theorem 3 with the result in Lemma 2, we see that the additional behavioral restriction increases the lowest possible value of τ_1 from $\max\{0,1+(\tau-1)/\mathrm{E}(D|X=c^+)\}$ to τ, and correspondingly decreases the largest possible value for τ_0 from $\min\{1,\tau/(1-\mathrm{E}(D|X=c^+))\}$ to τ. This follows from a simple application of Bayes' Rule, and means that $\mathcal{T}_a\subset\mathcal{T}$. We then obtain bounds that are (weakly) more narrow, because optimization is carried out over a smaller set. Under the conditions of Theorem 4, the set of plausible values of (τ_1,τ_0) shrinks to a singleton, which means that sharp bounds on our parameter of interest can be defined without invoking an optimization operator. Moreover, due to the absence of always-assigned never-takers under the conditions of Theorem 4, the distribution $F_{Y(0)|X=c,C_0}$ is point identified in this case

4. IDENTIFICATION UNDER MANIPULATION: EXTENSIONS

The results in the previous section can be extended in various ways. In this section, we show that the distribution of covariates among always-assigned and potentially-assigned units is point identified in our setup, that covariates can be used to tighten the bounds on Γ_- and $\Psi_-(u)$, and how to obtain bounds of the alternative causal parameters Γ_+ and $\Psi_+(u)$.

4.1. **Characteristics of Always- and Potentially-Assigned Units**

It is not possible to determine whether any given unit belongs to the group of always-assigned units or to the group of potentially-assigned units in our setup. This does not mean, however, that it is impossible to give any further characterization of these two groups. In particular, suppose the data include a vector W of covariates that are measured prior to treatment assignment, and whose distribution (conditional on units' type and the running variable) does not change discontinuously at c. It is then possible to identify the distribution of these covariates among always-assigned and potentially-assigned units. This information could be useful, for instance, for targeting policies aimed at mitigating manipulation. The following corollary formally states this result.

Corollary 1. *Suppose that Assumptions 1–2 hold, and that* $\mathrm{P}(W \leq w|X = x, M = 0)$ *is continuous in* x *at* c*. Then*

$$\begin{aligned}\mathrm{P}(W \leq w|X = c^+, M = 1) &= \frac{1}{\tau}(\mathrm{P}(W \leq w|X = c^+) - \mathrm{P}(W \leq w|X = c^-)) \\ &\quad + \mathrm{P}(W \leq w|X = c^-) \quad \text{and} \\ \mathrm{P}(W \leq w|X = c^+, M = 0) &= \mathrm{P}(W \leq w|X = c^-).\end{aligned}$$

Of course, identification of the distribution of W immediately implies identification of moments, quantiles and related summary statistics. For example, the corollary implies that $\mathrm{E}(W|X = c^+, M = 1) = (\mathrm{E}(W|X = c^+) - \mathrm{E}(W|X = c^-))/\tau + \mathrm{E}(W|X = c^-)$; and that $\mathrm{E}(W|X = c^+, M = 0) = \mathrm{E}(W|X = c^-)$.

4.2. **Using Covariates to Tighten the Bounds**

Following arguments similar to those in Lee (2009), covariates that are measured prior to treatment assignment can also be used to narrow the bounds on causal effects we derived above. Let W be a vector of such covariates, and denote its support by $\mathcal{W}$. The main idea then is that if the outcome distribution or the proportion of always-assigned units varies with the value of W, trimming units based on their position in the outcome distribution conditional on W leads to a smaller number of units with extreme outcomes being trimmed overall, which narrows the bounds. For the Sharp RD design, this reasoning leads to the following sharp upper and lower bounds on $F_{Y(1)|X=c,C_0}$:

$$
\begin{aligned}
F^U_{1,SRD(W)}(y) &= \int F_{Y|X=c^+,W=w,Y\geq Q_{Y|X=c^+,W=w}(\tau(w))}(y)dF_{W|X=c^-}(w) \quad \text{and} \\
F^L_{1,SRD(W)}(y) &= \int F_{Y|X=c^+,W=w,Y\leq Q_{Y|X=c^+,W=w}(1-\tau(w))}(y)dF_{W|X=c^-}(w).
\end{aligned}
$$

Note that the integral in the previous two equations is with respect to the covariate distribution among potentially-assigned units. Here $\tau(w) = \mathrm{P}(M = 1|X = c^+, W = w)$ is a conditional version of τ defined as in (3.1), and this quantity is point identified as $\tau(w) = 1 - f_{X|W}(c^-, w)/f_{X|W}(c^+, w)$ through arguments analogous to those used in the proof of Lemma 1, conditioning on $W = w$ throughout. The next corollary gives the resulting sharp lower and upper bounds on the average treatment effect Γ_- and quantile treatment effect $\Psi_-(u)$.

Corollary 2. *Suppose that the assumptions of Theorem 1 hold, mutatis mutandis, with conditioning on the covariates W. Then sharp lower and upper bounds on Γ_- are given by*

$$
\begin{aligned}
\Gamma^L_{-,SRD(W)} = &\int \mathrm{E}(Y|X = c^+, W = w, Y \leq Q_{Y|X=c^+,W=w}(1 - \tau(w)))dF_{W|X=c^-}(w) \\
&- \mathrm{E}(Y|X = c^-) \quad \text{and} \\
\Gamma^U_{-,SRD(W)} = &\int \mathrm{E}(Y|X = c^+, W = w, Y_i \geq Q_{Y|X=c^+,W=w}(\tau(w)))dF_{W|X=c^-}(w) \\
&- \mathrm{E}(Y|X = c^-),
\end{aligned}
$$

respectively; and sharp lower and upper bounds on $\Psi_-(u)$ are given by

$$
\begin{aligned}
\Psi^L_{-,SRD(W)}(u) &= Q^L_{1,SRD(W)}(u) - Q_{Y|X=c^-}(u) \quad \text{and} \\
\Psi^U_{-,SRD(W)}(u) &= Q^U_{1,SRD(W)}(u) - Q_{Y|X=c^-}(u),
\end{aligned}
$$

respectively, for every quantile level $u \in (0, 1)$.

To state a similar result for the Fuzzy RD design, we need to define conditional versions of

τ_1, τ_0, $\mathcal{T}$, κ_1 and κ_0, which we denote by $\tau_1(w)$, $\tau_0(w)$, $\mathcal{T}(w)$, $\kappa_1(w)$ and $\kappa_0(w)$, respectively. These objects are defined as in (3.1), (3.3) and (3.2) and Lemmas 1 and 2 by conditioning on $W = w$ throughout. We then define conditional versions of $F^U_{d,FRD}(y, \tau_1, \tau_0)$ and $F^U_{d,FRD}(y, \tau_1, \tau_0)$, denoted by $F^U_{d,FRD|W=w}(y, \tau_1(w), \tau_0(w))$ and $F^U_{d,FRD|W=w}(y, \tau_1(w), \tau_0(w))$, respectively, for $d \in \{0, 1\}$. These objects are constructed following the steps in the previous section by conditioning on $W = w$ throughout. We also define the set $\mathcal{T}_{\mathcal{W}} = \{(t_1(\cdot), t_1(\cdot)) : (t_1(w), t_1(w)) \in \mathcal{T}(w)$ for all $w \in \mathcal{W}\}$. Finally, we denote the proportion of potentially-assigned compliers (C_0) conditional on $W = w$ just to the left of the cutoff by

$$\begin{aligned}
\mathrm{P}(C_0|X = c^-, W = w) &= \frac{1 - \tau_1(w)}{1 - \tau(w)}\mathrm{E}(D|X = c^+, W = w) - \mathrm{E}(D|X = c^-, W = w) \\
&\equiv \Pi_{-,W=w}(\tau_1(w), \tau_0(w)).
\end{aligned}$$

With this notation, we can then construct sharp upper and lower bounds on $F_{Y(1)|X=c,C_0}$ and $F_{Y(0)|X=c,C_0}$ given knowledge of the function $w \mapsto (\tau_1(w), \tau_0(w))$. These bounds are given by

$$\begin{aligned}
F^U_{d,FRD(W)}(y, \tau_1(\cdot), \tau_0(\cdot)) &= \int F^U_{d,FRD|W=w}(y, \tau_1(w), \tau_0(w))\omega(w, \tau_1(w), \tau_0(w))dF_{W|X=c^-}(w) \\
F^L_{d,FRD(W)}(y, \tau_1(\cdot), \tau_0(\cdot)) &= \int F^L_{d,FRD|W=w}(y, \tau_1(w), \tau_0(w))\omega(w, \tau_1(w), \tau_0(w))dF_{W|X=c^-}(w),
\end{aligned}$$

for $d \in \{0, 1\}$, where

$$\omega(w, \tau_1(w), \tau_0(w)) \equiv \frac{\Pi_{-,W=w}(\tau_1(w), \tau_0(w))}{\int \Pi_{-,W=w}(\tau_1(w), \tau_0(w))dF_{W|X=c^-}(w)}.$$

The resulting sharp upper and lower bounds on the local average treatment effect Γ_- given (hypothetical) knowledge of the function $w \mapsto (\tau_1(w), \tau_0(w))$ are given by

$$\begin{aligned}
&\Gamma^U_{-,FRD(W)}(\tau_1(\cdot), \tau_0(\cdot)) \\
&\qquad \equiv \int y dF^U_{1,FRD(W)}(y, \tau_1(\cdot), \tau_0(\cdot)) - \int y dF^L_{0,FRD(W)}(y, \tau_1(\cdot), \tau_0(\cdot)) \quad \text{and} \\
&\Gamma^L_{-,FRD(W)}(\tau_1(\cdot), \tau_0(\cdot)) \\
&\qquad \equiv \int y dF^L_{1,FRD(W)}(y, \tau_1(\cdot), \tau_0(\cdot)) - \int y dF^U_{0,FRD(W)}(y, \tau_1(\cdot), \tau_0(\cdot)),
\end{aligned}$$

respectively. Similarly, the resulting sharp upper and lower bounds on the local quantile treatment effect $\Psi_-(u)$ for known values of $\tau_1(w)$ and $\tau_0(w)$ are given by

$$\Psi^U_{-,FRD(W)}(u,\tau_1(\cdot),\tau_0(\cdot)) \equiv Q^U_{1,FRD(W)}(u,\tau_1(\cdot),\tau_0(\cdot))) - Q^L_{0,FRD(W)}(u,\tau_1(\cdot),\tau_0(\cdot)) \quad \text{and}$$
$$\Psi^L_{-,FRD(W)}(u,\tau_1(\cdot),\tau_0(\cdot)) \equiv Q^L_{1,FRD(W)}(u,t\tau_1(\cdot),\tau_0(\cdot)) - Q^U_{0,FRD(W)}(u,\tau_1(\cdot),\tau_0(\cdot))$$

respectively. The following corollary gives the feasible sharp bounds on Γ_- and $\Psi_-(u)$ given that the function $w \mapsto (\tau_1(w),\tau_0(w))$ is only partially identified.

Corollary 3. *Suppose that the assumptions of Theorem 2 hold, mutatis mutandis, with conditioning on the covariates W. Then sharp lower and upper bounds on Γ_- are given by*

$$\Gamma^L_{-,FRD(W)} = \inf_{(t_1(\cdot),t_0(\cdot))\in\mathcal{T}_\mathcal{W}} \Gamma^L_{-,FRD}(t_1(\cdot),t_0(\cdot)) \quad \text{and}$$
$$\Gamma^U_{-,FRD(W)} = \sup_{(t_1(\cdot),t_0(\cdot))\in\mathcal{T}_\mathcal{W}} \Gamma^U_{-,FRD}(t_1(\cdot),t_0(\cdot)),$$

respectively; and sharp lower and upper bounds on Ψ_- are given by

$$\Psi^U_{-,FRD(W)} = \inf_{(t_1(\cdot),t_0(\cdot))\in\mathcal{T}_\mathcal{W}} \Psi^U_{-,FRD}(u,t_1(\cdot),t_0(\cdot)) \quad \text{and}$$
$$\Psi^L_{-,FRD(W)} = \sup_{(t_1(\cdot),t_0(\cdot))\in\mathcal{T}_\mathcal{W}} \Psi^L_{-,FRD}(u,t_1(\cdot),t_0(\cdot)),$$

respectively, for every quantile level $u \in (0,1)$.

4.3. **Causal Effects for Units Just to the Right of the Cutoff**

The parameters Γ_- and $\Psi_-(u)$ that we considered so far correspond to causal effects among compliers whose realization of the running variable is just to the left of the cutoff. As pointed out in Section 2.4, alternative parameters of interest are

$$\Gamma_+ \equiv \mathrm{E}(Y(1)-Y(0)|X=c^+,D^+>D^-), \quad \text{and}$$
$$\Psi_+(u) \equiv Q_{Y(1)|X=c^+,D^+>D^-}(u) - Q_{Y(0)|X=c^+,D^+>D^-}(u),$$

which are causal effects among compliers whose realization of the running variable is just to the right of the cutoff. For reasons outlined above, these parameters are often less important in empirical applications than their "left side" counterparts, but they are nevertheless interesting to study. The main conceptual difficulty for an identification analysis is that by definition there is no always-assigned complier that does not receive the treatment. Any bounds analysis therefore must rely on some additional assumption, at least for the average treatment effect, that specifies a "worst case" scenario for the outcome variable in this counterfactual scenario. To make progress, we impose the assumption that the outcome variable has bounded support conditional on the running variable in some neighborhood of the cutoff. This type of assumption is commonly used in the partial identification literature (cf. Manski, 1990) and is natural for binary outcomes, for example. However, it is restrictive in general and difficult to justify for some outcomes commonly studied in economics, like wages.

Assumption 4. *There are constants* Y^L *and* Y^U *such that* $\mathrm{P}(Y^L \leq Y(0) \leq Y^U | X = x) = 1$ *and* $\mathrm{P}(Y^L \leq Y(1) \leq Y^U | X = x) = 1$ *for every* x *in some open neighborhood of the cutoff.*

We now study identification of Γ_+ and $\Psi_+(u)$ under this additional assumption. Paralleling the discussion in Section 3, we begin with the Sharp RD design before turning to the more general Fuzzy RD setup. Note that, using notation from Section 3, we have that $\{D^+ > D^-\} = C_0 \cup C_1$, and thus the parameters of interest can be written as $\Gamma_+ = \mathrm{E}(Y(1) - Y(0)|X = c^+, C_0 \cup C_1)$ and $\Psi_+(u) = Q_{Y(1)|X=c^+,C_0\cup C_1}(u) - Q_{Y(0)|X=c^+,C_0\cup C_1}(u)$.

Sharp RD Designs. In the Sharp RD design every unit just to the right of the cutoff is a complier, and thus the distribution of Y_i given $X_i = c^+$ coincides with the distribution of $Y_i(1)$ among compliers (C_1 or C_0) just to the right of the cutoff:

$$F_{Y(1)|X=c^+,C_0\cup C_1}(y) = F_{Y|X=c^+}(y).$$

On the other hand, we have that

$$F_{Y(0)|X=c^+,C_0\cup C_1}(y) = \tau F_{Y(0)|X=c^+,C_1}(y) + (1-\tau) F_{Y(0)|X=c^+,C_0}(y).$$

Since there exist no untreated always-assigned compliers, we can only deduce from Assumption 4 that the potential outcome $Y_i(0)$ of always-assigned compliers is bounded between Y^L and Y^U. This, and the continuity conditions on potentially-assigned units in Assumption 1, then lead to the following sharp bounds on $F_{Y(0)|X=c^+,C_0\cup C_1}(y)$:

$$F^U_{0,+,SRD}(y) = (1-\tau)F_{Y|X=c^-}(y) + \tau\mathbb{I}\left\{y \geq Y^U\right\} \quad \text{and}$$
$$F^L_{0,+,SRD}(y) = (1-\tau)F_{Y|X=c^-}(y) + \tau\mathbb{I}\left\{y \geq Y^L\right\}.$$

The following corollary gives the resulting sharp lower and upper bounds on the average treatment effect Γ_+ and quantile treatment effect $\Psi_+(u)$.

Corollary 4. *Suppose Assumptions 1–4 hold, that $P(D^+ > D^-) = 1$. Then sharp lower and upper bounds on Γ_+ are given by*

$$\Gamma^L_{+,SRD} = \mathrm{E}(Y|X=c^+) - (1-\tau)\mathrm{E}(Y|X=c^-) - \tau Y^U \quad \text{and}$$
$$\Gamma^U_{+,SRD} = \mathrm{E}(Y|X=c^+) - (1-\tau)\mathrm{E}(Y|X=c^-) - \tau Y^L,$$

respectively; and sharp lower and upper bounds on $\Psi_+(u)$ are given by

$$\Psi^L_{+,SRD}(u) = Q_{Y|X=c^+}(u) - \mathbb{I}\left\{u < 1-\tau\right\} Q_{Y|X=c^-}\left(\frac{u}{1-\tau}\right) - \mathbb{I}\left\{u \geq 1-\tau\right\} Y^U \quad \text{and}$$
$$\Psi^U_{+,SRD}(u) = Q_{Y|X=c^+}(u) - \mathbb{I}\left\{u \geq 1-\tau\right\} Q_{Y|X=c^-}\left(\frac{u-\tau}{1-\tau}\right) - \mathbb{I}\left\{u < \tau\right\} Y^L,$$

respectively, for every quantile level $u \in (0,1)$.

The formulas in Corollary 4 highlight that our additional assumption of bounded support always matters for Γ_+, but only matters for Ψ_+ if $u < \tau$ or $u \geq 1-\tau$. The bounds on $\Gamma_+(u)$ are thus finite even in the absence of support restrictions on the outcome distribution for most quantile levels, as long as the degree of manipulation is moderate.

Fuzzy RD Designs. For the Fuzzy RD design, our strategy is to first derive bounds for the hypothetical case in which the true values of (τ_1, τ_0) and $\lambda \equiv \mathrm{P}(A_1|X=c^+, D=1, M=1)$, the proportion of always-takers among the treated always-assigned units just to the right of the cutoff, are known. In a second step, we then extend this result to our actual setting in which we only know that $(\tau_1, \tau_0) \in \mathcal{T}$ and that $\lambda \in [0, 1]$.

We begin by considering the c.d.f. $F_{Y(1)|X=c^+,C_0\cup C_1}(y)$. Recall from Section 3.2 that we can point identify the c.d.f. $G(y) \equiv F_{Y(1)|X=c^+,C_0\cup C_1\cup A_1}(y)$ from the data on treated units, and note that $\mathrm{P}(A_1|X = c^+, C_0 \cup C_1 \cup A_1) = 1 - \lambda\tau_1/(1 - \kappa_1)$. By truncating the distribution $G(y)$ appropriately, we thus arrive on the following sharp upper and lower bounds on $F_{Y(1)|X=c^+,C_0\cup C_1}(y)$:

$$F^U_{1,+,FRD}(y, \tau_1, \tau_0, \lambda) = \frac{(1 - \kappa_1)G(y) - \lambda\tau_1}{1 - \kappa_1 - \lambda\tau_1} \cdot \mathbb{I}\left\{y \geq G^{-1}\left(\frac{\lambda\tau_1}{1 - \kappa_1}\right)\right\} \quad \text{and}$$
$$F^L_{1,+,FRD}(y, \tau_1, \tau_0, \lambda) = \frac{(1 - \kappa_1)G(y)}{\lambda\tau_1} \cdot \mathbb{I}\left\{y \leq G^{-1}\left(1 - \frac{\lambda\tau_1}{1 - \kappa_1}\right)\right\}.$$

Now consider the c.d.f. $F_{Y(0)|X=c^+,C_0\cup C_1}(y)$, which can be written as

$$F_{Y(0)|X=c^+,C_0\cup C_1}(y) = s(\tau_1, \lambda)F_{Y(0)|X=c^+,C_0}(y) + (1 - s(\tau_1, \lambda))F_{Y(0)|X=c^+,C_1}(y)$$

where

$$s(\tau_1, \lambda) \equiv \mathrm{P}(C_0|X = c^+, C_0 \cup C_1) = \frac{(1 - \tau_1)\mathrm{E}(D|X = c^+) - (1 - \tau)\mathrm{E}(D|X = c^-)}{(1 - \lambda\tau_1)\mathrm{E}(D|X = c^+) - (1 - \tau)\mathrm{E}(D|X = c^-)}.$$

is the proportion of potentially-assigned units among all compliers just to the right of the cutoff. The term $F_{Y(0)|X=c^+,C_0}(y) = F_{Y(0)|X=c^-,C_0}(y)$ can then be bounded as in Section 3.2, and bounds on $F_{Y(0)|X=c^+,C_1}(y)$ follow from Assumption 4:

$$F^U_{0,+,FRD}(y, \tau_1, \tau_0, \lambda) = s(\tau_1, \lambda)F^U_{0,FRD}(y, \tau_1, \tau_0) + (1 - s(\tau_1, \lambda))\mathbb{I}\left\{y \geq Y^U\right\} \quad \text{and}$$
$$F^L_{0,+,FRD}(y, \tau_1, \tau_0, \lambda) = s(\tau_1, \lambda)F^L_{0,FRD}(y, \tau_1, \tau_0) + (1 - s(\tau_1, \lambda))\mathbb{I}\left\{y \geq Y^L\right\},$$

Taken together, the sharp bounds on the local average treatment effect Γ_+ for known values of τ_1, τ_0 and λ are

$$\Gamma^U_{+,FRD}(\tau_1, \tau_0, \lambda) \equiv \int y dF^U_{1,+,FRD}(y, \tau_1, \tau_0, \lambda) - \int y dF^L_{0,+,FRD}(y, \tau_1, \tau_0, \lambda),$$
$$\Gamma^U_{+,FRD}(\tau_1, \tau_0, \lambda) \equiv \int y dF^L_{1,+,FRD}(y, \tau_1, \tau_0, \lambda) - \int y dF^U_{0,+,FRD}(y, \tau_1, \tau_0, \lambda);$$

and analogous bounds for the local quantile treatment effect $\Psi_+(u)$ are

$$\Psi^U_{+,FRD}(u,\tau_1,\tau_0,\lambda) \equiv Q^U_{1,+,FRD}(u,\tau_1,\tau_0,\lambda) - Q^L_{0,+,FRD}(u,\tau_1,\tau_0,\lambda),$$
$$\Psi^L_{+,FRD}(u,\tau_1,\tau_0,\lambda) \equiv Q^L_{1,+,FRD}(u,\tau_1,\tau_0,\lambda) - Q^U_{0,+,FRD}(u,\tau_1,\tau_0,\lambda).$$

We can then give sharp bounds on Γ_+ and $\Psi_+(u)$ by finding those values of $(\tau_1,\tau_0) \in \mathcal{T}$ and $\lambda \in [0,1]$ that lead to the most extreme values of the just-defined quantities.

Corollary 5. *Suppose Assumptions 1–4 hold, and that* $F_{Y|XD}(y|c^+,d)$ *and* $F_{Y|XD}(y|c^-,d)$ *are continuous in* y *for* $d \in \{0,1\}$*. Then sharp lower and upper bounds on* Γ_+ *are given by*

$$\Gamma^L_{+,FRD} = \inf_{(t_1,t_0,l)\in\mathcal{T}\times[0,1]} \Gamma^L_{+,FRD}(t_1,t_0,l) \quad \text{and}$$
$$\Gamma^U_{+,FRD} = \sup_{(t_1,t_0,l)\in\mathcal{T}\times[0,1]} \Gamma^U_{+,FRD}(t_1,t_0,l),$$

respectively; and sharp lower and upper bounds on $\Psi_-(u)$ *are given by*

$$\Psi^L_{+,FRD}(u) = \inf_{(t_1,t_0,l)\in\mathcal{T}\times[0,1]} \Psi^L_{+,FRD}(u,t_1,t_0,l) \quad \text{and}$$
$$\Psi^U_{+,FRD}(u) = \sup_{(t_1,t_0,l)\in\mathcal{T}\times[0,1]} \Psi^U_{+,FRD}(u,t_1,t_0,l),$$

respectively, for every quantile level $u \in (0,1)$*.*

Following the reasoning laid out in Section 3.3, these bounds can be tightened by imposing additional behavioral assumptions. Consider, for example, the setup of Theorem 4, which imposes that every always-assigned unit just to the right of the cutoff receives the treatment. In many empirical contexts in which this assumptions is plausible, one might also be willing to assume that none of the always-assigned unit just to the right of the cutoff would have been able to receive the treatment had they not been assigned to the treatment group. Taken together, these two conditions imply that every always-assigned unit just to the right of the cutoff is a complier. The following corollary shows the implications of this assumption on the bounds on causal parameters.

Corollary 6. *Suppose that the conditions of Corollary 5 hold, and that* $\Pr(D^+ > D^-|M =$

1) = 1. *Then* $(\tau_1, \tau_0, \lambda) = (\tau/\mathrm{E}(D|X = c^+), 0, 0)$ *is point identified, and sharp upper and lower bound on* Γ_+ *are given by*

$$\Gamma^L_{+,FRD} = \Gamma^L_{+,FRD}\left(\frac{\tau}{\mathrm{E}(D_i|X_i = c^+)}, 0, 0\right) \quad \text{and}$$
$$\Gamma^U_{+,FRD} = \Gamma^U_{+,FRD}\left(\frac{\tau}{\mathrm{E}(D_i|X_i = c^+)}, 0, 0\right),$$

respectively; and sharp upper and lower bound on $\Psi_+(u)$ *are given by*

$$\Psi^L_{+,FRD}(u) = \Psi^L_{+,FRD}\left(u, \frac{\tau}{\mathrm{E}(D_i|X_i = c^+)}, 0, 0\right) \quad \text{and}$$
$$\Psi^U_{+,FRD}(u) = \Psi^U_{+,FRD}\left(u, \frac{\tau}{\mathrm{E}(D_i|X_i = c^+)}, 0, 0\right),$$

respectively, for every quantile level $u \in (0, 1)$*.*

4.4. **Remarks on Exploiting Selection Assumptions**

We conclude this section by noting that additional behavioral restrictions on the form of selection taking place at the cutoff can further tighten our bounds on Γ_-. Suppose for example that one is willing to assume that always-assigned units have higher (average) potential outcomes under the treatment than potentially-assigned units. A new upper bound on the value of Γ_- in the Sharp RD design is then a naive estimate that ignores selection concerns:

$$\Gamma_- \leq \mathrm{E}(Y|X = c^+) - \mathrm{E}(Y|X = c^-).$$

Instead, if one is willing to assume that always-assigned units have higher average treatment effects than potentially-assigned units, this implies that $\Gamma_+ \geq \Gamma_-$; which holds because

$$\Gamma_+ = (1 - \tau) \cdot \Gamma_- + \tau \cdot \mathrm{E}(Y(1) - Y(0)|X = c^+, M = 1).$$

Thus, an upper bound on Γ_+ automatically becomes an upper bound on Γ_-. Similar arguments apply to the fuzzy RD design, and to quantile treatment effects.

5. ESTIMATION AND INFERENCE

In this section, we explain how to conduct estimation and inference based on the identification results derived above. Software to implement these methods is available from the authors' websites.

5.1. **Estimation of the Bounds**

The bounds that we obtained in Sections 3–4 can be estimated through a "plug-in" approach that replaces unknown population quantities with suitable sample counterparts. Following the recent RD literature, we focus on flexible nonparametric methods, and in particular local polynomial smoothing (Fan and Gijbels, 1996), for the construction of these sample counterparts. To simplify the exposition, we use the same polynomial order p, bandwidth h and kernel function $K(\cdot)$ in all intermediate estimation steps in this paper. We also use the notation that $\boldsymbol{\pi}_p(x) = (1/0!, x/1!, x^2/2!, \ldots, x^p/p!)'$ and $K_h(x) = K(x/h)/h$ for any $x \in \mathbb{R}$, and define the $(p+1)$-vector $\mathbf{e}_1 = (1, 0, \ldots, 0)'$. The data available to the econometrician is an independent and identically distributed sample $\{(Y_i, D_i, X_i), i = 1, \ldots, n\}$ of size n.

Proportion of Always-Assigned Units. Following the result in Lemma 1, estimating τ requires estimates of the right and left limits of the density at the cutoff. There are a number of nonparametric estimators that can be used to estimate densities at boundary points; see for example Lejeune and Sarda (1992), Jones (1993), Cheng (1997) or Cattaneo, Jansson, and Ma (2015). Here we use a minor variation of the procedure in Cheng (1997), which also forms the basis for the McCrary (2008) test, and estimate $f_X(c^+)$ and $f_X(c^-)$ by

$$\widehat{f}^{+} = \mathbf{e}_1' \operatorname*{argmin}_{\beta \in \mathbb{R}^{p+1}} \sum_{i=1}^{n} (\widehat{f}(X_i) - \boldsymbol{\pi}_p(X_i - c)'\beta) K_h(X_i - c) \mathbb{I}\{X_i \geq c\}, \text{ and}$$
$$\widehat{f}^{-} = \mathbf{e}_1' \operatorname*{argmin}_{\beta \in \mathbb{R}^{p+1}} \sum_{i=1}^{n} (\widehat{f}(X_i) - \boldsymbol{\pi}_p(X_i - c)'\beta) K_h(X_i - c) \mathbb{I}\{X_i < c\},$$

respectively, where $\widehat{f}(X_i) = (1/n) \sum_{j=1}^{n} K_b(X_j - X_i)$ and b is another bandwidth. Since by assumption the proportion of always-assigned units among units just to the right of the cutoff has to be non-negative, our estimate of τ is then given by

$$\widehat{\tau} = \max\{\widetilde{\tau}, 0\}, \quad \text{with} \quad \widetilde{\tau} = 1 - \widehat{f}^- / \widehat{f}^+.$$

Conditional Expectation, Distribution, and Density Functions. Using standard nonparametric regression techniques, and writing $g^+ = \mathrm{E}(D_i | X_i = c^+)$ and $g^- = \mathrm{E}(D_i | X_i = c^-)$, we estimate the conditional treatment probabilities on either side of the cutoff by

$$\widehat{g}^+ = \mathbf{e}_1' \operatorname*{argmin}_{\beta \in \mathbb{R}^{p+1}} \sum_{i=1}^{n} (D_i - \boldsymbol{\pi}_p (X_i - c)'\beta)^2 K_h(X_i - c) \mathbb{I}\{X_i \geq c\}, \text{ and}$$
$$\widehat{g}^- = \mathbf{e}_1' \operatorname*{argmin}_{\beta \in \mathbb{R}^{p+1}} \sum_{i=1}^{n} (D_i - \boldsymbol{\pi}_p (X_i - c)'\beta)^2 K_h(X_i - c) \mathbb{I}\{X_i < c\},$$

respectively (Fan and Gijbels, 1996). Next, we estimate the conditional c.d.f.s $F_{Y|X=c^+,D=d}(y)$ and $F_{Y|X=c^-,D=d}(y)$ by

$$\widehat{F}_{Y|X=c^+,D=d}(y) = \mathbf{e}_1' \operatorname*{argmin}_{\beta \in \mathbb{R}^{p+1}} \sum_{i=1}^{n} (\mathbb{I}\{Y_i \leq y\} - \boldsymbol{\pi}_p (X_i - c)'\beta) K_h(X_i - c) \mathbb{I}\{X_i \geq c\}, \text{ and}$$
$$\widehat{F}_{Y|X=c^-,D=d}(y) = \mathbf{e}_1' \operatorname*{argmin}_{\beta \in \mathbb{R}^{p+1}} \sum_{i=1}^{n} (\mathbb{I}\{Y_i \leq y\} - \boldsymbol{\pi}_p (X_i - c)'\beta) K_h(X_i - c) \mathbb{I}\{X_i < c\},$$

respectively, which for every $y \in \mathbb{R}$ corresponds to a local polynomial regression with $\mathbb{I}\{Y_i \leq y\}$ as the dependent variable (Hall, Wolff, and Yao, 1999). Finally, we estimate the conditional p.d.f.s $f_{Y|X=c^+,D=d}(y)$ and $f_{Y|X=c^-,D=d}(y)$ by

$$\widehat{f}_{Y|X=c^+,D=d}(y) = \mathbf{e}_1' \operatorname*{argmin}_{\beta \in \mathbb{R}^{p+1}} \sum_{i=1}^{n} (K_b(Y_i - y) - \boldsymbol{\pi}_p (X_i - c)'\beta) K_h(X_i - c) \mathbb{I}\{X_i \geq c\}, \text{ and}$$
$$\widehat{f}_{Y|X=c^-,D=d}(y) = \mathbf{e}_1' \operatorname*{argmin}_{\beta \in \mathbb{R}^{p+1}} \sum_{i=1}^{n} (K_b(Y_i - y) - \boldsymbol{\pi}_p (X_i - c)'\beta) K_h(X_i - c) \mathbb{I}\{X_i < c\}$$

respectively, which for every $y \in \mathbb{R}$ corresponds to a local polynomial regression with $K_b(Y_i - y)$ as the dependent variable, where b is another bandwidth (Fan, Yao, and Tong, 1996).

Final Bounds Estimates. We illustrate the construction of our final bounds estimates for the case of the local average treatment effect Γ_- under the conditions of Theorem 2. The construction is analogous for all other settings in Sections 3–4. Using the representation of the set $\mathcal{T}$ given after Lemma 2, and dropping the "FRD" subscript to simplify the notation, the bounds on Γ_- from Theorem 2 can be written as

$$\Gamma_-^L = \inf_{t\in[0,1]} \Gamma_-^L(\eta_1(t), \eta_0(t)) \quad \text{and} \quad \Gamma_-^U = \sup_{t\in[0,1]} \Gamma_-^U(\eta_1(t), \eta_0(t)).$$

This reparametrization is convenient because it ensures that the area over which optimization takes place does not depend on nuisance parameters that have to be estimated. Next, we put

$$\widehat{\Gamma}_-^j(t_1, t_0) = \int y d\widehat{F}_1^j(y, t_1, t_0) - \int y d\widehat{F}_0^j(y, t_1, t_0), \quad j \in \{U, L\}, \tag{5.1}$$

where for $j \in \{U, L\}$ and $d \in \{0, 1\}$ the function $\widehat{F}_d^j(y, t_1, t_0)$ is a sample analogue estimator of the function $F_{d,FRD}^j(y, t_1, t_0)$ defined in Section 3. Specifically, we put

$$\widehat{F}_1^U(y, t_1, t_0) = \frac{(1-\widehat{\kappa}_1)\widehat{G}(y) - t_1}{1 - \widehat{\kappa}_1 - t_1} \cdot \mathbb{I}\left\{y \geq \widehat{G}^{-1}\left(\frac{t_1}{1-\widehat{\kappa}_1}\right)\right\},$$
$$\widehat{F}_0^U(y, \tau_1, \tau_0) = \frac{\widehat{F}_{Y|X=c^-,D=0}(y) - \widehat{\kappa}_0 \cdot (1-t_0)\widehat{F}^L_{Y(0)|X=c,N_0}(y)}{1 - \kappa_0 \cdot (1-t_0)},$$

and similarly define $\widehat{F}_1^L$ and $\widehat{F}_0^L$. These expressions use the notation that

$$\widehat{G}(y) = \frac{\widehat{F}_{Y|X=c^+,D=1}(y) - \widehat{\kappa}_1\widehat{F}_{Y|X=c^-,D=1}(y)}{1-\widehat{\kappa}_1}, \quad \widehat{F}^L_{Y(0)|X=c,N_0}(y) = \int_{-\infty}^{y} \widehat{s}(u)\mathbb{I}\left\{u \geq \widehat{q}_L\right\} du$$
$$\widehat{s}(y) = \frac{\min\left\{\widehat{f}_{Y|X=c^-,D=0}(y)/\widehat{\kappa}_0, \widehat{f}_{Y|X=c^+,D=0}(y)\right\}}{1-t_0}, \quad \widehat{\kappa}_1 = \frac{(1-\widehat{\tau})\widehat{g}^-}{\widehat{g}^+}, \quad \widehat{\kappa}_0 = \frac{1-\widehat{g}^+}{(1-\widehat{\tau})(1-\widehat{g}^-)},$$

with $\widehat{q}_L$ the value that satisfies $\int_{-\infty}^{\widehat{q}_L} \widehat{s}(y)dy = 1$. Note that the integrals in (5.1) have to be evaluated numerically. We then define the functions

$$\widehat{\eta}_d(t) = \widehat{\tau}_d^L + t \cdot (\widehat{\tau}_d^U - \widehat{\tau}_d^L), \quad d \in \{0, 1\},$$

where for $j \in \{U, L\}$ and $d \in \{0, 1\}$ the term $\widehat{\tau}_d^j$ is a sample analogue estimator of the point τ_d^j that describes the shape of the set $\mathcal{T}$. Finally, our estimates of the lower and upper bounds on Γ_- are given, respectively, by

$$\widehat{\Gamma}_-^L = \inf_{t\in[0,1]} \widehat{\Gamma}_-^L(\widehat{\eta}_1(t), \widehat{\eta}_0(t)) \quad \text{and} \quad \widehat{\Gamma}_-^U = \sup_{t\in[0,1]} \widehat{\Gamma}_-^U(\widehat{\eta}_1(t), \widehat{\eta}_0(t)).$$

We use grid search to solve the two optimization problems in the previous equation. While a full analysis of the statistical properties of these estimators is beyond the scope of this paper, we remark that they generally exhibit finite sample bias due to the use of smoothing estimators and the presence of the sup and inf operators in the definition of the bounds (see Hirano and Porter, 2012, for details on the latter issue).

5.2. **Inference**

In order to quantify sampling uncertainty about the various parameters of interest, we construct "manipulation-robust" confidence intervals that are valid irrespective of the true value of τ, under certain regularity conditions.[12] To explain our approach, we focus again on the case of Γ_- under the conditions of Theorem 2, as the procedures work analogously in other settings.

The first conceptual complication is due to the presence of an optimization operator in the definition of the bounds, which we address using the intersection-union testing principle of Berger (1982).[13] The main idea is the following. Suppose that for every $t \in [0,1]$ we had a $1-\alpha$ confidence interval $\mathcal{C}_{1-\alpha}^{FRD}(t)$ for Γ_- that was valid if the true value of (τ_1, τ_0) was equal to $(\eta_1(t), \eta_0(t))$. Then the intersection-union principle implies that $\mathcal{C}_{1-\alpha}^{FRD} = \cup_{t\in[0,1]}\mathcal{C}_{1-\alpha}^{FRD}(t)$ is a $1-\alpha$ confidence interval for Γ_-. That is, a candidate value for Γ_- is outside of $\mathcal{C}_{1-\alpha}^{FRD}$ if and only if it is outside of $\mathcal{C}_{1-\alpha}^{FRD}(t)$ for all $t \in [0,1]$. An important feature of this approach is that both the "fixed t" and the overall confidence interval have level $1-\alpha$: there is no need for a multiplicity adjustment to account for the fact that we are implicitly testing a continuum of hypotheses. Moreover, Berger (1982) also shows that this approach has strong power properties.

[12]These conditions include standard smoothness assumptions commonly found in the literature on local polynomial smoothing, as well as technical restrictions on the magnitudes on of the bandwidths involved. We only sketch the main line of the arguments here in order to maintain this paper's focus on identification results. A full econometric analysis will be developed in a companion paper.

[13]Our problem differs from the one in Chernozhukov, Lee, and Rosen (2013), who study inference on *intersection bounds* of the form $[\sup_v \theta(v), \inf_v \theta(v)]$. It is more accurately described as an example of *union bounds*, as the role of the inf and the sup operator in the definition of the identified set is reversed. We are not aware of any existing general results on inference for union bounds, but the intersection-union testing principle provides a straightforward solution.

The second conceptual complication involves the construction of a "fixed t" confidence interval. If the estimates $\widehat{\Gamma}^L_-(\widehat{\eta}_1(t), \widehat{\eta}_0(t))$ and $\widehat{\Gamma}^U_-(\widehat{\eta}_1(t), \widehat{\eta}_0(t))$ were jointly asymptotically normal irrespective of the true value of τ, one could use the approach proposed by Imbens and Manski (2004) and Stoye (2009) for this purpose. However, our bound estimates are only jointly asymptotically normal (under appropriate regularity conditions) if $\tau > 0$. For $\tau = 0$, their limiting distribution is non-Gaussian, as the estimated level of manipulation $\widehat{\tau} = \max\{0, 1 - \widehat{f}^-/\widehat{f}^+\}$ fails to be asymptotically normal in this case.[14] A Gaussian approximation to the distribution of the "fixed t" estimates is thus typically poor in finite samples if τ is not well-separated from zero, and the standard bootstrap is unable to provide a remedy in this case (Andrews, 2000).

We therefore propose an approach similar to moment selection in the moment inequality literature (e.g. Andrews and Soares, 2010; Andrews and Barwick, 2012). We first test the hypothesis that $\tau = 0$, and if this is not rejected estimate the limiting distribution of the bounds estimates for a level of manipulation that is slightly tilted away from zero, the least favorable direction in this case (note that the distributions of $\widehat{\Gamma}^L_-(\widehat{\eta}_1(t), \widehat{\eta}_0(t))$ and $\widehat{\Gamma}^U_-(\widehat{\eta}_1(t), \widehat{\eta}_0(t))$ are increasing in τ in a stochastic sense). If the hypothesis that $\tau = 0$ is rejected, no such tilting occurs. As in the moment inequality literature, the pre-test is designed such that an incorrect rejection is "very unlikely" in large samples.

For convenience, we implement this approach via the bootstrap. Specifically, we construct a bootstrap distribution under which the bootstrap analogue of $\widetilde{\tau} = 1 - \widehat{f}^-/\widehat{f}^+$ is centered around $\max\{\widehat{\tau}, \kappa_n \widehat{\sigma}_{\widetilde{\tau}}\}$, where $\widehat{\sigma}_{\widetilde{\tau}}$ is the standard error of $\widetilde{\tau}$, and κ_n is a sequence of constants that slowly tends to infinity. Following much of the moment inequality literature, we choose $\kappa_n = \log(n)^{1/2}$ in this paper. The algorithm for our bootstrap is as follows.

1. Generate bootstrap samples $\{Y_{i,b}, D_{i,b}, X_{i,b}\}_{i=1}^n$, $b = 1, \ldots, B$ by sampling with replacement from the original data $\{Y_i, D_i, X_i\}_{i=1}^n$; for some large integer B.
2. Calculate $\widetilde{\tau}^*_b = 1 - \widehat{f}^-_b/\widehat{f}^+_b$, and put $\widehat{\sigma}_{\widetilde{\tau}}$ as the sample standard deviation of $\{\widetilde{\tau}^*_b\}_{b=1}^B$.

[14] Under standard regularity conditions we have that $\sqrt{nh}(\widehat{\tau} - \tau) \xrightarrow{d} \max\{0, Z\}$ if $\tau = 0$, where Z is a Gaussian random variable with mean zero.

3. Calculate $\tilde{\tau}_b = \tilde{\tau}_b^* - \tilde{\tau} + \max\{\hat{\tau}, \kappa_n \hat{\sigma}_{\tilde{\tau}}\}$ and $\hat{\tau}_b = \max\{\tilde{\tau}_b, 0\}$.
4. For $j \in \{U, L\}$, calculate $\hat{\Gamma}_-^j(\hat{\eta}_1(t), \hat{\eta}_0(t))$ using the redefined estimate $\hat{\tau}_b$ from the previous step, and put $\hat{\sigma}^j(t)$ as the sample standard deviation of $\{\hat{\Gamma}_-^j(\hat{\eta}_1(t), \hat{\eta}_0(t)\}_{b=1}^B$.

Now define $\hat{\Gamma}_-^{L*}(t)$ and $\hat{\Gamma}_-^{U*}(t)$ exactly as $\hat{\Gamma}_-^L(\hat{\eta}_1(t), \hat{\eta}_0(t))$ and $\hat{\Gamma}_-^U(\hat{\eta}_1(t), \hat{\eta}_0(t))$, with the exception that $\hat{\tau}^* = \max\{\tilde{\tau}, \kappa_n \hat{\sigma}_{\tilde{\tau}}\}$ is used instead of $\hat{\tau}$. Following Imbens and Manski (2004) and Stoye (2009), our "fixed t" confidence interval for Γ_- with level $1-\alpha$ is then given by

$$\mathcal{C}_{1-\alpha}^{FRD}(t) = \left[\hat{\Gamma}_-^{L*}(t) - r_\alpha(t) \cdot \hat{\sigma}^L(t),\ \hat{\Gamma}_-^{U*}(t) + r_\alpha(t) \cdot \hat{\sigma}^U(t)\right],$$

where $r_\alpha(t)$ is the value that solves the equation

$$\Phi\left(r_\alpha(t) + \frac{\hat{\Gamma}_-^{U*}(t) - \hat{\Gamma}_-^{L*}(t)}{\max\{\hat{\sigma}^L(t), \hat{\sigma}^U(t)\}}\right) - \Phi(-r_\alpha(t)) = 1 - \alpha,$$

and $\Phi(\cdot)$ is the CDF of the standard normal distribution. The final intersection-union confidence interval for Γ_- is then given by

$$\mathcal{C}_{1-\alpha}^{FRD} = \left[\inf_{t \in [0,1]} \left(\hat{\Gamma}_-^L(t) - r_\alpha(t) \cdot \hat{\sigma}^L(t)\right), \sup_{t \in [0,1]} \left(\hat{\Gamma}_-^U(t) + r_\alpha(t) \cdot \hat{\sigma}^U(t)\right)\right].$$

We remark that the construction of this confidence interval does not account for the fact that the limiting distribution of the "fixed t" estimates is not only discontinuous at $\tau = 0$, but also at those values of τ under which one of the various max and min operators in the definition of the function $\eta_d(\cdot)$ becomes binding.[15] The construction also implicitly assumes that the two functions involved in the definition of the term $s(y)$ cross at a finite number of points.[16]

5.3. Illustrating the Potential Impact Of Manipulation

Suppose that a researcher obtains a point estimate of τ that is close to zero but has a large standard error. In this case the confidence intervals proposed in the previous subsection

[15] In practice, our confidence interval should work well as long as either $\tau < 1 - \mathrm{E}(D|X = c^+)$ or $\tau > 1 - \mathrm{E}(D|X = c^+)$, as this rules out the issue. Both conditions appear reasonable for many applications, including the one we study below. To keep the exposition simply, we therefore do not include any "safeguards" against such cases into our bootstrap procedure.

[16] If this assumption was considered to be too restrictive for the empirical context, a construction analogous to that in Anderson, Linton, and Whang (2012) could be used to remove the resulting bias.

are typically rather wide, reflecting the high level of uncertainty about the true level of manipulation. If the institutional setting is such that the researcher strongly suspects that manipulation is either absent or at least quite rare, these confidence intervals might seem overly pessimistic. We therefore consider an alternative way to illustrate the potential impact of manipulation in such settings. The basic idea is to compute a confidence interval for Γ_- under the assumption that the value of τ is known, and then to investigate how the confidence interval changes with the value of τ.

To understand how to interpret the result of such an exercise, suppose that researcher's main goal is testing the hypothesis that $\Gamma_- = 0$ against the alternative that $\Gamma_- \neq 0$. Let $\mathcal{C}_{1-\alpha}(\tau^*)$ be a confidence interval for Γ_- that is derived under the assumption that $\tau = \tau^*$, where τ^* is a constant chosen by the researcher (we explain in Appendix D how such a confidence interval can be constructed). For $\tau^* = 0$, this yields the usual "no manipulation" confidence interval, and generally $\mathcal{C}_{1-\alpha}(\tau^*)$ becomes wider as τ^* increases. The researcher can then plot the upper and lower boundary of $\mathcal{C}_{1-\alpha}(\tau^*)$ as a function of τ^*, and check graphically for which levels of manipulation the value of 0 is contained in the confidence interval. For example, suppose that $0 \notin \mathcal{C}_{1-\alpha}(0)$, but that $0 \in \mathcal{C}_{1-\alpha}(\tau^*)$ for $\tau^* \geq 0.1$. Then the researcher can report that in his preferred "no manipulation" specification the null hypothesis $\Gamma_- = 0$ is rejected at the critical level α, and that at least a 10% level of manipulation around the cutoff would be needed to reverse this result. We believe that such an exercise is a useful robustness check for every RD study, and a reasonable way to visualize the impact of potential manipulation.

6. EMPIRICAL APPLICATION

In this section, we apply the methods developed above to bound treatment effects of unemployment insurance (UI) on (formal) reemployment around an eligibility cutoff in Brazil.

This is a good example of a empirical question for which our approach is relevant and useful. An extensive literature, dating back to at least Katz and Meyer (1990), has studied the effect of UI on the time it takes for displaced workers to find a new job. UI programs typically feature discontinuities in potential UI benefits (level or duration) based on the value of some running variable, such as the number of months of employment prior to layoff, and thus RD designs are natural empirical strategies to estimate this effect. At the same time, the possibility that manipulation of the running variable could invalidate the standard assumption for a RD design is a serious concern in the UI context, which is discussed explicitly in prominent papers in the literature (e.g. Card, Chetty, and Weber, 2007; Schmieder, von Wachter, and Bender, 2012). Employers may put some workers on temporary layoff once they are eligible for UI (Feldstein, 1976). Some workers may also provoke their layoff or ask their employer to report their quit as a layoff once they are eligible for UI.[17] This concern may be particularly severe in developing countries with high labor market informality, such as Brazil. The utility costs of being formally laid off when eligible for UI may be relatively low for some workers if they can work informally while drawing UI benefits. Finally, our key identifying assumption ("one-sided manipulation") is likely to apply: all displaced workers are likely to have at least a weak preference for being eligible for UI benefits (they always have the choice to not take up UI).

Our empirical exercise is also relevant beyond illustrating the applicability of our approach. The effect of UI on functions of the non-formal-employment duration (i.e., the time between two formal jobs), for which we estimate bounds below, captures the usual moral hazard problem with UI – that it distorts incentives to return to a formal job. As such, it is an important input to the evaluation of the optimal design of UI programs (e.g. Chetty, 2008).[18] Specifically, as we show below, the treatment effect at an eligibility cutoff is a key input for the welfare effect of marginal changes in the location of the cutoff. Moreover, UI programs have been adopted or considered in a number of developing countries with high informality.

[17] Alternatively, workers laid off with a value of the running variable to the left of the relevant cutoff may lobby their employers to lay them off on a later date. This latter behavior could be modeled similarly to the example of a second home visit in Section 2.3. The manipulation in our empirical application is likely the result of a combination of these different types of behaviors (and possibly others).

[18] The optimal UI literature typically refers to the effect on non-employment duration because it considers countries where all jobs are assumed to be formal.

Yet, the existing evidence for such labor markets remains limited.

6.1. **Institutional Details, Data, and Sample Selection**

Our empirical exercise focuses on an eligibility cutoff in the Brazilian UI program. In the interest of space, we present the institutional details and the data succinctly. For more details, see Gerard and Gonzaga (2016), which study other aspects of the Brazilian UI program.

Institutional Details. In Brazil, a worker who is reported as involuntarily laid off from a private-sector formal job is eligible for UI under two conditions. First, she must have at least six months of continuous job tenure at layoff. Second, there must be at least 16 months between the date of her layoff and the date of the last layoff after which she applied for and drew UI benefits. We focus on the eligibility cutoff created by the second condition. The 16-month cutoff is more arbitrary and thus less likely to coincide with other possible discontinuities.[19] It may also be more relevant for the moral hazard problem. Both conditions end up restricting eligibility for workers with limited prior formal employment, but the second condition also restricts eligibility for workers cycling in and out of formal employment, who may be more responsive to UI incentives. For instance, Gerard and Gonzaga (2016) show that the moral hazard problem is more severe in labor markets where displaced formal workers return faster to a formal job.

Workers who satisfy the two conditions can withdraw monthly UI payments after a 30-day waiting period and until they are formally reemployed or exhaust their potential UI duration. The potential UI duration is equal to three, four, or five months of UI benefits if workers accumulated more than 6, 12, or 24 months of formal employment in the 36 months prior to layoff, respectively. The benefit level depends on workers' average wage in the three months prior to layoff. The replacement rate is 100% at the bottom of the wage distribution but is already down to 60% for a worker who earned three times the minimum wage (the benefit schedule is shown in the Appendix). Finally, UI benefits are not experience-rated in Brazil.

[19] For instance, six months of job tenure may be a salient milestone for evaluating employees' performance. Note that there is evidence of manipulation around the six-month cutoff as well (available upon request).

Data. Our empirical analysis relies on two administrative datasets. The first one is a longitudinal matched employee-employer dataset covering by law the universe of formal employees. Every year, firms must report all workers formally employed at some point during the previous calendar year. The data include information on wage, tenure, age, gender, education, sector of activity, and establishment size. Importantly, the data also include hiring and separation dates, as well as the reason for separation. The second dataset is the registry of all UI payments. Individuals can be matched in both datasets as they are identified through the same ID number. Combining the datasets, we know whether any displaced formal employee is eligible for UI, how many UI payments she draws, and when she is formally reemployed. We have both datasets from 2002 to 2010.

Sample selection. Our sample of analysis is constructed as follows. First, we consider all workers, 18 to 55 years old, who lost a private-sector full-time formal job between 2004 and 2008. We start in 2004 because we are interested in workers who were displaced from another formal job about 16 months earlier. We end in 2008 because we want to observe at least two years after the layoff. Second, we keep workers who had more than six month of job tenure at layoff, which is the other eligibility condition. Third, we restrict attention to workers who were previously displaced from another formal job and for whom the difference between the previous layoff date and the new layoff date fell within 50 days of the 16-month eligibility cutoff. Fourth, we limit the sample to workers who took up UI and exhausted their UI benefits after the previous layoff. This is to make sure that the change in eligibility at the 16-month cutoff is sharp. Indeed, workers who find a formal job before exhausting their benefits are entitled to draw the remaining benefits after a new layoff, even if it occurred before the 16-month cutoff. To implement this fourth step, we select workers who drew the maximum number of UI benefits after the previous layoff (about 40% of cases). This is because we can only measure the number of UI benefits a worker is eligible for imprecisely in our data. Finally, we drop workers whose previous layoff date fell after the 28$^{\text{th}}$ of a month. Policy rules create bunching in the layoff density at the 16-month cutoff even in the absence of manipulation among these workers.[20]

Our sample ultimately consists of 169,575 workers with a relatively high attachment to the formal labor force, high turnover rate, and high ability to find a new formal job rapidly.[21] These are not the characteristics of the average displaced formal employee or UI taker in Brazil, but characteristics of workers for whom the 16-month cutoff may be binding.

6.2. **Connecting our empirical application to the optimal UI literature**

In this subsection, we further motivate our empirical application by relating the treatment effects that we estimate to the optimal UI literature. In particular, we show that they constitute important inputs for the evaluation of the welfare effect of a marginal decrease in the location of the cutoff. Readers primarily interested in an empirical illustration of our identification results can skip ahead to Section 6.3.

Context. The recent UI literature Chetty and Finkelstein (cf. 2013) has considered the optimality of a series of policy parameters for UI programs, such as the benefit level, the potential benefit duration, or the profile of the benefit level over the potential benefit duration. These studies investigate the welfare effects of marginal changes in those policy parameters, but they typically hold fixed the rules determining UI eligibility. In particular, they do not investigate the welfare effects of marginal changes in the set of displaced formal workers who are deemed eligible for UI to begin with. This is somewhat surprising because many (if not all) UI programs restrict eligibility in some ways, indicating that eligibility rules are important policy parameters to consider.

A common eligibility rule is to require that displaced formal workers have a minimum number of months of formal employment prior to layoff or a minimum number of months

[20] For instance, all workers laid off between October 29th and 31st in 2007, became eligible on February 28th in 2009, because there are only 28 days in February.

[21] They were eligible for five months of UI after their previous layoff, so they had accumulated 24 months of formal employment within a 36-month window. They were laid off again within 16 months and they had accumulated at least six months of continuous tenure at layoff. Therefore, they found a job relatively quickly after their previous layoff. Indeed, about 50% of workers eligible for five months of UI benefits remain without a formal job one year after layoff.

since the last UI spell. Let us define this "running" variable, X, and the cutoff value c. In a Baily-Chetty-type framework, which is a canonical partial-equilibrium framework in the optimal UI literature (Baily, 1978; Chetty, 2008), there are three main reasons why a government may want to restrict eligibility to workers with $X \geq c$. First, workers with $X < c$ may have a lower need for insurance. Insurance is typically most valuable for events that are consequential and relatively rare. Workers who have been formally employed for a shorter period of time may not have fully adjusted to their employment status and may thus experience a smaller utility loss following layoff. Moreover, the hazard of layoff is often decreasing over the formal employment spell, making layoff a more likely event for these workers.[22] Second, the usual moral hazard problem may be particularly severe for workers with $X < c$. Workers who have fewer months of formal employment prior to layoff or since the last UI spell may be disproportionally composed of workers who are cycling in and out of relatively low-quality formal jobs. Their job-search and reemployment choices may be particularly sensitive to UI eligibility if it is relatively easy for them to find a new formal job and their utility gains from formal reemployment (absent UI benefits) are limited. Third, the probability that a layoff occurs may be particularly sensitive to UI eligibility among workers with fewer months of formal employment as they are less attached to their job. This is the mechanism considered by Hopenhayn and Nicolini (2009), who argue that UI benefits should increase with prior (formal) employment history for this reason. The optimal UI literature often abstracts from this mechanism, however, because it is thought to be best tackled by the experience-rating of UI benefits, and not by changing UI eligibility rules or benefits.

Welfare formula. We obtain a formula for the welfare effect of a marginal decrease in the cutoff value c in a Baily-Chetty framework, by following the derivation in Gerard and Gonzaga (2016), which adapts Chetty (2008) for the presence of informal job opportunities. In the interest of space, we do not detail the standard assumptions in these papers, which are

[22] Of course, their need for insurance may instead be higher as they would not have had the time to accumulate much savings to self-insure against such an event.

necessary for such a welfare formula to hold. In a nutshell, workers are assumed to internalize all consequences of their choices, except on the government budget, and the incidence of taxes and benefits are assumed to fall on workers.

At time 0, a set of formal workers of mass 1 is laid off. Assume that the variable X is continuously distributed among these workers, with c.d.f. F_X when the cutoff is located at some level c. The share $1 - F_X(c)$ of these workers is eligible for UI benefits and can draw a benefit level b in each period (e.g. each month) until they are formally reemployed or until they reach the potential UI duration B. Upon formal reemployment, they pay a UI tax in each period to fund the UI system (tax). The UI program must thus (in expectations) satisfy the budget constraint $(1 - F_X(c))) \cdot b \cdot D^B_{X \geq c} = tax \cdot D^F$, where $D^B_{X \geq c} = \sum_0^{B-1} \mathrm{P}(NotFormal_{i,t} = 1 | X \geq c)$ is the average paid UI duration among eligible workers, $\mathrm{P}(NotFormal_{i,t} = 1 | X \geq c)$ is their survival rate in non-formal-employment in each period t after layoff, and D^F is the average number of months spent formally employed after layoff among all displaced workers.[23]

We derive the welfare effect of a marginal decrease in the cutoff value c through a perturbation argument. Such a reform will first lead to a *mechanical effect*. Workers with values of the running variable just to the left of the cutoff, with density $f_X(c^-)$, will become eligible mechanically, without changing their behavior. The associated cost will be: $f_X(c^-) \cdot b \cdot D^B_{X=c^-}$, where $D^B_{X=c^-}$ is the average paid UI duration among these mechanical beneficiaries (the target of the reform), absent behavioral responses. This cost amounts to a transfer between mechanical beneficiaries and formal employees. It constitutes the possible source of welfare gain with UI programs when insurance markets are incomplete.

[23] A departure from our framework is that UI is financed by a .65% tax on firms' sales in Brazil. We consider instead the case of a tax on formal workers, which is the main source of funding for UI in other countries, including developing countries (Velásquez, 2010). A tax on formal workers is the interesting case conceptually. They are the beneficiaries of the program and UI aims at providing insurance, not at redistribution. The incidence of a tax on formal workers is also likely to fall on those workers, and is certainly more likely to do so than a sales tax. We thus use Brazil as an empirical setting to estimate and illustrate the efficiency cost of changes in a UI eligibility cutoff as derived in a benchmark framework. The odd financing of the Brazilian program is unlikely to invalidate this objective. A 2.5% payroll tax would be sufficient to fund UI (UI expenditures/total eligible payroll $\simeq$ .025) and it is unlikely that the composition of the formal labor force would be very different substituting such a tax for the existing one.

The associated welfare effect is thus: $f_X(c^-) \cdot b \cdot D^B_{X=c^-} \cdot (u'_M - u'_F)$, where u'_M and u'_F are the average marginal value of \$1 for mechanical beneficiaries and formal employees, respectively.

A marginal decrease in the cutoff value c will also lead to *behavioral effects*. Workers with values of the running variable just to the left of the cutoff may change their behaviors in response to their new UI eligibility. In particular, they may delay formal reemployment by remaining unemployed or working informally. As a result, their paid UI duration will increase, increasing the cost of the UI program by: $f_X(c^-) \cdot b \cdot dD^B_{X=c^-}$, where $dD^B_{X=c^-}$ is the change in average paid UI duration due to behavioral effects among these workers. They may also reduce their contribution to the funding of the UI system, by spending fewer periods formally employed and paying the UI tax. This potential loss in UI revenues will amount to: $f_X(c^-) \cdot tax \cdot dD^F_{X=c^-}$, where $dD^F_{X=c^-}$ is the change in the average number of periods formally employed after layoff among these workers. A standard envelope argument implies that there is no first-order utility gain from these (and other) behavioral responses. However, there is a first-order utility loss because the associated costs must be paid for. The welfare effect is thus: $-u'_F \cdot f_X(c^-) \cdot \left[b \cdot dD^B_{X=c^-} + tax \cdot dD^F_{X=c^-}\right]$.

Finally, changes in UI eligibility rules may also affect the layoff probability and the distribution of X among displaced formal workers. An envelope argument would again imply that there is no first-order utility gain from such behavioral responses. However, any impact on the UI budget (costs or revenues) must be paid for. The welfare effect would thus be: $-u'_F \cdot dBudget(dX)$, in which $dBudget(dX)$ captures all such impacts. We use a coarse notation for this last element because it is not the focus of our empirical analysis.

Putting everything together, we obtain a formula for the overall welfare effect of a marginal decrease in the eligibility cutoff. It is common in public economics to measure welfare per unit impact on affected agents, so we normalize the welfare effect (dW) by the mechanical effect. We also divide by the average marginal utility of \$1 for formal employees to express welfare in a money metric (Chetty, 2008):

$$\begin{aligned}&\frac{dW}{f_X(c^-)\cdot b\cdot D^B_{X=c^-}\cdot u'_F}\\&\quad=\left(\frac{u'_M-u'_F}{u'_F}\right)-\left(\frac{dD^B_{X=c^-}}{D^B_{X=c^-}}+\frac{tax\cdot dD^F_{X=c^-}}{b\cdot D^B_{X=c^-}}+\frac{dBudget(dX)}{f_X(c^-)\cdot b\cdot D^B_{X=c^-}}\right)\end{aligned} \tag{6.1}$$

Equation (6.1) specifies the usual trade-off with UI between insurance and efficiency. The first parenthesis captures the *marginal value of insurance*, the relative welfare gain from transferring \$1 from formal employees to mechanical beneficiaries. The second parenthesis, the ratio of the behavioral effects to the mechanical effect, captures the *efficiency cost*, or the resources lost per \$1 reaching mechanical beneficiaries. It is common in public economics for the efficiency cost of a policy to be captured by such a “leakage” ratio.

Equation (6.1) provides a local welfare test: the welfare effect of the reform is positive if the marginal value of insurance exceeds the efficiency cost. Estimating the marginal value of insurance is always challenging because marginal utilities are not easily measured. However, an estimate of the efficiency cost already allows some welfare statements. Suppose we estimate an efficiency cost of 30 cents per \$1 reaching mechanical beneficiaries. The welfare effect would then only be positive if the average marginal utility of \$1 was at least 30% higher for mechanical beneficiaries than for formal workers. Such a bound could be informative in some settings, for instance when we have some priors about the need for insurance.

Connecting the theory back to the empirical application. The first two components of the efficiency cost in equation (6.1) relate to treatment effects among potentially-assigned workers. We investigate such effects for the 16-month cutoff in Brazil. The third component relates to the possibility of manipulation in the running variable. We do not investigate it below because it cannot be estimated without variation in the location of the cutoff (at least without strong parametric assumptions). The presence of always-assigned units at the cutoff, however, imply that we will only be able to recover bounds for the treatment effects among potentially-assigned units. Lower bounds will be particularly relevant in our case given that we are only able to estimate part of the efficiency cost. In particular, they will determine a minimum value for the marginal value of insurance in order for welfare effects to be positive.

6.3. **Graphical Evidence**

Figure 6.2 displays graphically some of the patterns in our data. Observations are aggregated by day between the layoff date and the 16-month eligibility cutoff. Panels A and B provide some evidence of manipulation of the running variable. The density jumps up by about 6.5% at the cutoff. Moreover, the average UI replacement rate (benefit/wage) increases discontinuously at the cutoff, highlighting the possibility of sample selection at the cutoff. Panel C suggests that workers were at least partially aware of the eligibility rule, a necessary condition for manipulation to take place. The share of workers applying for UI benefits jumps by about 40%-points at the cutoff. Panel D shows that the eligibility rule was enforced. The share of workers drawing some UI benefits is close to zero to the left of the cutoff, but takeup jumps to about 73% at the cutoff. Eligible workers drew on average 3.1 months of UI benefits (panel E), implying that UI takers drew on average 4.25 months of UI benefits (= 3.1/.73). Finally, Panel F shows that the non-formal-employment duration (censored at two years after layoff) jumps from about 220 days to 280 days at the cutoff.[24] This discontinuity could be due to a treatment effect, but also to a selection bias. Workers on each side of the cutoff may have different potential outcomes in the presence of manipulation. The estimators developed above allow us to bound treatment effects, despite the possibility of selection effects.

Next, Figure 6.3 compares our RD sample to other displaced formal employees in Brazil. Specifically, we drew a 5% random sample of formal employees laid off over the same period; we restricted attention to workers with at least six months of tenure at layoff (the other eligibility condition); but we did not impose any sample restriction related to the 16-month eligibility cutoff. This random sample includes 1,098,745 workers.

Panels A and B display UI collection patterns for this random sample and for workers on the right of the cutoff in the RD sample (those eligible for UI). They show the share taking

[24] The distribution of outcomes to the left and right of the cutoff is shown in the Appendix.

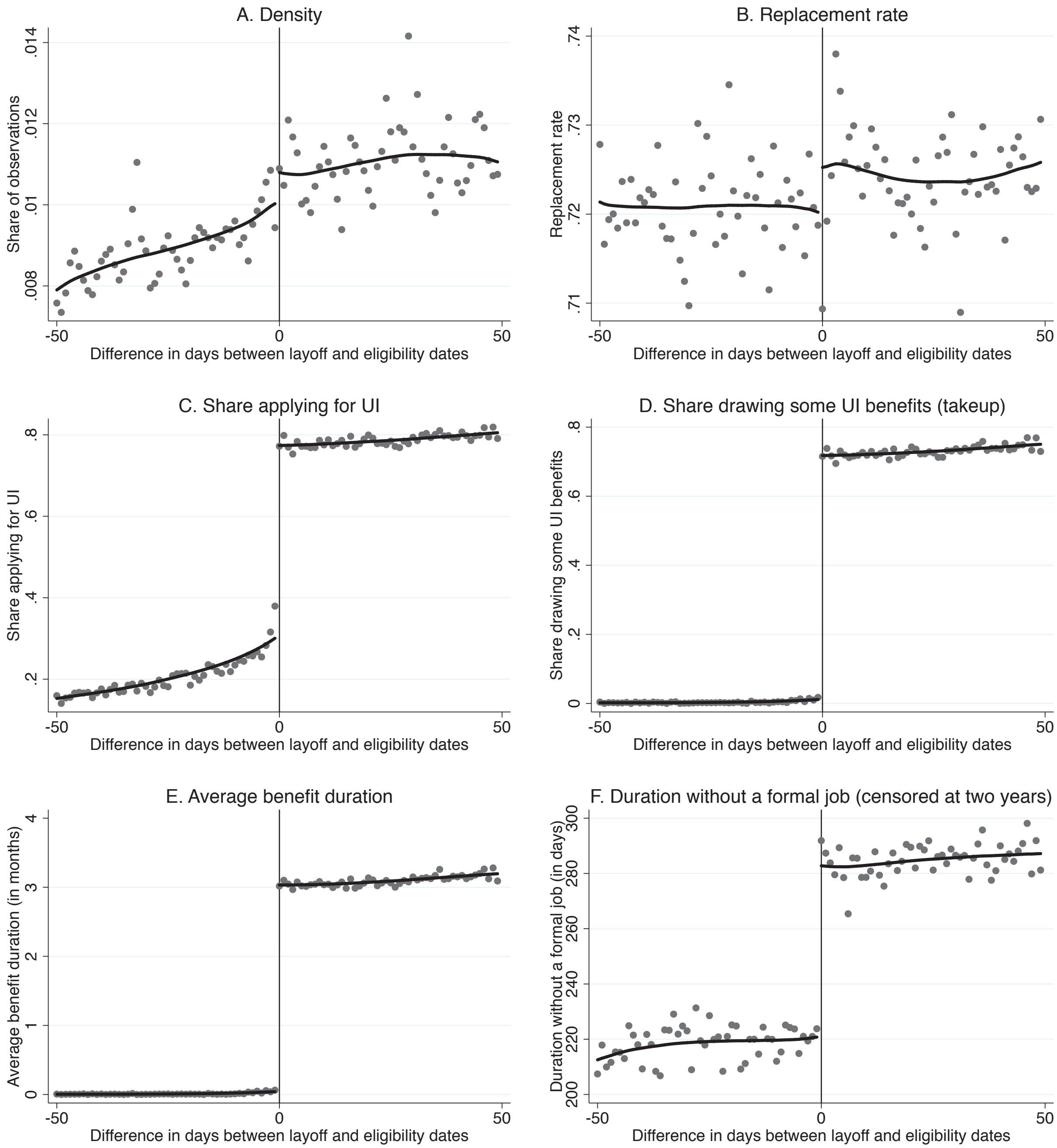

Figure 6.2: Graphical evidence for our empirical application

The figure displays the mean of different variables on each side of the cutoff by day between the layoff and eligibility dates, as well as local linear regressions on each side of the cutoff using an edge kernel and a bandwidth of 30 days. The figure is based on a RD sample of 169,575 displaced formal workers.

up UI (i.e. drawing a first UI payment) and drawing some UI payment in each month since layoff. The 30-day waiting period appears strictly enforced and most UI takers draw their first UI payment in the second and third month after layoff. As a result, most UI takers would exhaust their potential duration by month 5 to 7 after layoff. Accordingly, the share drawing some UI payment decreases sharply after month 5 since layoff. These patterns are similar in the two samples. The overall takeup rate is smaller in the RD sample (.73 vs. .79), but the average paid UI duration among UI takers is larger (3.09/.73 = 4.25 vs. 3.29/.79 = 4.16).

Panels C and D display formal reemployment patterns. Panel C displays survival rates in non-formal-employment in each month since layoff for all workers on the left and on the right of the cutoff, separately. It also displays survival rates for the subset of UI takers on the right of the cutoff. Panel D displays survival rates for all workers and for UI takers in the random sample. Survival rates remain higher for workers on the right than on the left of the cutoff in the RD sample. This is consistent with the evidence on panel F in Figure 6.2. A year after layoff, 21% and 29% of workers remain without a formal job among workers on the left and on the right of the cutoff, respectively. Survival rates remain even higher among UI takers on the right of the cutoff. This is particularly the case during the first few months after layoff, before the end of the potential UI duration. Survival rates start decreasing faster after month 5 since layoff. Yet, the share without a formal job remains higher among UI takers even a year after layoff (about 35%). These patterns are qualitatively similar in the random sample, but displaced workers return even slower to a formal job in this sample. This is true even among UI takers, despite the fact that the average paid UI duration is actually lower among UI takers in the random sample. A year after layoff, 40 % of the displaced workers and 46% of the UI takers remain without a formal job in this sample.

In sum, the RD sample includes workers who have a comparable paid UI duration, but who return much faster to a formal job once they stop drawing UI benefits than typical displaced workers in Brazil. This echoes a concern that the moral hazard problem may be more severe for workers around the eligibility cutoff.

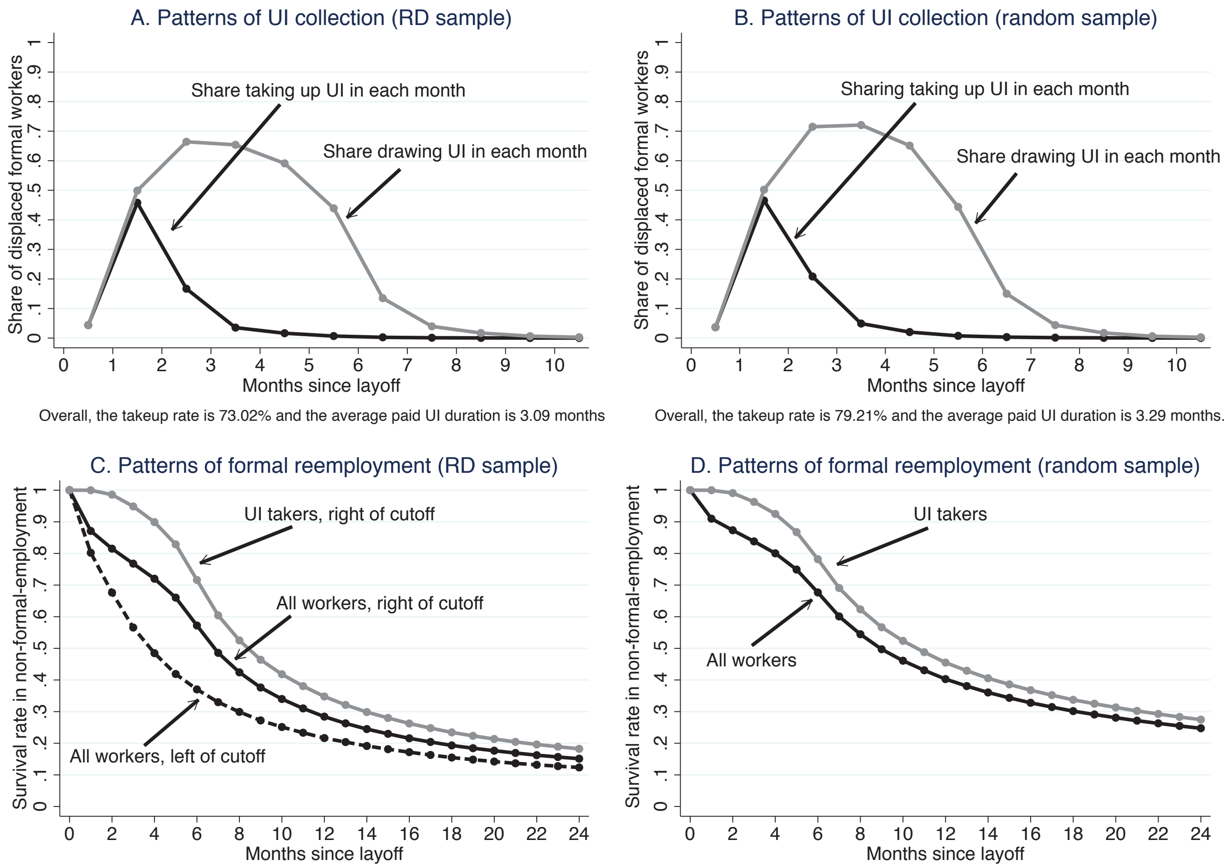

Figure 6.3: Comparing patterns of UI collection and formal reemployment in the RD sample and in a random sample of displaced formal workers (not selected around 16-month cutoff)

The figure displays the share of displaced formal workers taking up UI (i.e. drawing a first UI payment) and drawing some UI payment in each month after layoff. The figure also displays the share of displaced formal workers who remain without a formal job in each month after layoff, separately for all displaced formal workers and for those who took up UI. The figure compares those patterns for displaced formal workers in our RD sample and in a 5% random sample of all displaced formal workers with at least 6 months of tenure at layoff (the other eligibility condition for UI, next to the 16-month cutoff). The RD sample includes 169,575 displaced formal workers; the random sample includes 1,098,745 workers. UI outcomes are only displayed for workers on the right of the 16-month cutoff for the RD sample. Formal reemployment patterns are displayed separately for workers on the left and on the right of the 16-month cutoff.

6.4. Estimates

We implement our estimation and inference procedures for treatment effects on the non-formal-employment duration. We present three sets of results, in which the outcome is censored at 6, 12, and 24 months after layoff, respectively. This allows us to illustrate how our bounds for average treatment effects are affected by long tails in the distribution of the outcome variable. It also allows us to illustrate the usefulness of looking at quantile treatment effects, as these are rather insensitive to long tails. Furthermore, it allows us to connect our empirical analysis to the first two components of the efficiency cost in equation (6.1). The first component includes the behavioral effect on the average paid UI duration among potentially-assigned units: $dD^B_{X=c^-}$. We estimate this behavioral effect by considering treatment effects on the non-formal-employment duration censored at six months after layoff.[25] The second component includes the behavioral effect on the number of months formally employed after layoff. We follow most of the optimal UI literature (e.g. Chetty, 2008; Schmieder, von Wachter, and Bender, 2012) and approximate it by considering the overall effect on the non-formal-employment duration. For sample size reasons, we only consider the duration up to two years after layoff. We acknowledge that we may thus underestimate this effect. In practice, however, this second component matters much less for the efficiency cost than the first one. It is scaled by the ratio tax/b, which Gerard and Gonzaga (2016) argue can be approximated by the number of UI beneficiaries per private formal employee.[26] They estimate it to be around .086 over a similar sample period in Brazil.

[25] If all UI takers were taking up UI immediately and the potential UI duration was the same for all workers, say 5 months, we would look at changes in the non-formal-employment duration censored at four months ($D^B_{X=c^-} = \sum_0^{B-1} \mathrm{P}(NotFormal_{i,t} = 1 | X = c^-)$). However, UI takeup typically takes place in the second or third month after layoff in our case (see Figure 6.3). Moreover, displaced workers in our sample are eligible for 4 to 5 months of UI. We thus chose to consider the duration censored at 6 months because most of our UI takers have exhausted their benefits by month 6 after layoff (see Figure 6.3). We run the risk of underestimating the behavioral effect on average paid UI duration by considering a shorter duration, as many workers may still be eligible for UI beyond the censoring point. We run the risk of overestimating the behavioral effect on paid UI duration by considering a longer UI duration, as many workers will no longer be eligible for UI beyond the censoring point.

[26] This scaling factor is approximated by the unemployment rate in studies that consider labor markets with limited informal employment (e.g. Chetty, 2008; Schmieder, von Wachter, and Bender, 2012).

We present results for an edge kernel (Cheng, Fan, and Marron, 1997) and a bandwidth of 30 days around the cutoff, which gives non-zero weights to 102,791 observations of displaced formal workers.[27] For bounds in the Fuzzy RD case that involve numerical optimization, we use a grid search to look for the infimum and supremum using 51 values for $t \in [0,1]$ and for $\lambda \in [0,1]$. Confidence intervals are based on 500 bootstrap samples.[28]

Results. Results for average and quantile treatment effects are displayed in Tables 2 and 3. We present quantile effects for the outcome censored at 24 months after layoff and we consider lower percentiles of the distribution (10th, 30th, and 50th percentiles) because the treatment is more likely to affect workers who would have returned rapidly to a formal job.

Panels A of Tables 2 and 3 report estimates of key inputs for our bounds. Always-assigned units are estimated to account for 6.5% of observations just to the right of the cutoff and UI takeup is estimated to increase by 71%-points at the cutoff. Note that the value of τ appears well-separated from zero, so that the safeguards that ensure uniform validity of the confidence intervals for our bounds in case of small values of τ are not of any practical importance here.

Panels B and C then report results from two types of exercises. First, we consider a Sharp RD design in which *UI eligibility* is defined as the treatment of interest (panel B). The causal effect on the outcome can be interpreted as an intention-to-treat (ITT) parameter in this case. Second, we consider the usual Fuzzy RD design with *UI takeup* as the treatment (panel C). Naive RD estimates that assume no manipulation yield an average increase in non-formal-employment duration from being eligible for UI (ITT/SRD) of 29.4, 48.6, and 61.9 days for censoring points of 6, 12, and 24 months, respectively. The corresponding figures are 41.6, 68.8, and 87.7 days for the effect of UI takeup (LATE/FRD). As expected, naive treatment effects at the median are much larger, at 86 days (SRD) and 99 days (FRD;

[27] We do not have theoretical results on the optimal bandwidth for the estimation of our bounds. Our estimates are similar if we use bandwidths of 10 or 50 days (available upon request).

[28] Due to the censoring of the outcome variable, we use identification results for non-continuously distributed outcomes described in the appendix.

Table 2: Average treatment effects of UI on non-formal-employment duration

	Duration without a formal job censored at:					
	6 months		12 months		24 months	
	Estimate	95% CI	Estimate	95% CI	Estimate	95% CI
A. Basic Inputs						
Share of always-assigned workers	0.065	[0.040;0.087]	0.065	[0.040;0.087]	0.065	[0.040;0.087]
Increase in UI takeup at the cutoff	0.706	[0.698;0.713]	0.706	[0.698;0.713]	0.706	[0.698;0.713]
B. ITT/SRD estimates						
Ignoring manipulation	29.4	[27.6;31.0]	48.6	[44.9;51.9]	61.9	[55.3;67.5]
Bounds for Γ_-	[26.4;38.8]	[24.2;42.6]	[37.9;62.9]	[32.1;69.2]	[31;81.2]	[16.8;90.7]
Bounds for Γ_- using covariates	[26.4;38.7]	[24.0;42.9]	[37.9;62.8]	[31.7;69.7]	[31;81.1]	[15.4;91.4]
Bounds for Γ_+	[24.7;36.5]	[22.2;39.5]	[35.4;59.0]	[29.4;63.9]	[29;76.2]	[15.1;83.9]
B. LATE/FRD estimates						
Ignoring manipulation	41.6	[39.4;43.7]	68.8	[64.0;72.9]	87.7	[78.5;95.5]
Bounds for Γ_-	[35.4;51.7]	[32.6;55.7]	[52.3;84.4]	[44.8;91.3]	[42.9;110.0]	[23.3;121.7]
Bounds for Γ_-: Refinement from Th. 3	[37.7;50.0]	[34.7;53.3]	[53.9;84.1]	[46.0;91.0]	[43.2;110.0]	[23.3;121.7]
Bounds for Γ_-: Refinement from Th. 4	[38.5;48.2]	[35.6;51.0]	[55.1;83.3]	[47.4;89.8]	[45.1;110.0]	[24.9;121.7]
Bounds for Γ_- using covariates	[35.7;51.7]	[32.7;56.1]	[52.4;84.4]	[44.5;91.9]	[41.9;109.7]	[20.3;122.3]
Bounds for Γ_+	[34.9;51.9]	[31.5;56.0]	[50.1;84.4]	[41.6;91.4]	[40.3;110.0]	[20.6;121.8]
Bounds for Γ_+: Refinement from Cor. 6	[34.9;51.6]	[31.5;55.8]	[50.1;83.5]	[41.6;90.2]	[41.0;107.8]	[21.3;118.6]

Notes: Total number of observations within our bandwidth of 30 days around the cutoff: 102,791 displaced formal workers. Confidence intervals have nominal level of 95% and are based on 500 bootstrap samples. Bounds that use covariates only use a dummy variable for having a wage at layoff above/below the median.

Table 3: Quantile treatment effects of UI on non-formal-employment duration

	Duration without a formal job censored at 24 months					
	Percentile 10%		Percentile 30%		Percentile 50%	
	Estimate	95% CI	Estimate	95% CI	Estimate	95% CI
A. Basic Inputs						
Share of always-assigned workers	0.065	[0.04;0.087]	0.065	[0.040;0.087]	0.065	[0.040;0.087]
Increase in UI takeup at the cutoff	0.706	[0.698;0.713]	0.706	[0.698;0.713]	0.706	[0.698;0.713]
B. ITT/SRD estimates						
Ignoring manipulation	10	[7.6;12.4]	74	[69.4;78.7]	86	[80.9;91.1]
Bounds for $\Psi_-(u)$	[8;35]	[5.6;47.1]	[64;95]	[57.4;102.6]	[75;99]	[67.7;106.8]
Bounds for $\Psi_-(u)$ using covariates	[8;35]	[5.5;48.2]	[64;95]	[56.3;102.6]	[75;99]	[66.7;107.0]
Bounds for $\Psi_+(u)$	[9;17]	[6.7;19.1]	[69;87]	[64.4;94.2]	[75;101]	[65.1;108.7]
B. LATE/FRD estimates						
Ignoring manipulation	90	[86.3;93.7]	96	[91.0;101.0]	99	[91.0;107.0]
Bounds for $\Psi_-(u)$	[84;130]	[76.4;149.1]	[87;114]	[81.8;121.9]	[67;120]	[52.4;131.2]
Bounds for $\Psi_-(u)$: Refinement from Th. 3	[84;121]	[77.4;130.8]	[87;111]	[81.8;118.8]	[76;120]	[60.6;131.2]
Bounds for $\Psi_-(u)$: Refinement from Th. 4	[84;121]	[77.6;130.8]	[88;111]	[82.5;117.8]	[77;120]	[66.2;131.2]
Bounds for $\Psi_-(u)$ using covariates	[83;130]	[75.1;148.8]	[87;114]	[81.1;122.4]	[65;120]	[49.4;132.5]
Bounds for $\Psi_+(u)$	[84;131]	[76.3;151.3]	[87;114]	[81.6;121.9]	[67;120]	[51.3;131.6]
Bounds for $\Psi_+(u)$: Refinement from Cor. 6	[85;121]	[77.4;126.8]	[89;114]	[83.6;121.8]	[68;119]	[52.9;131.5]

Notes: Total number of observations within our bandwidth of 30 days around the cutoff: 102,791 displaced formal workers. Confidence intervals have nominal level of 95% and are based on 500 bootstrap samples. Bounds that use covariates only use a dummy variable for having a wage at layoff above/below the median.

outcome censored at 24 months). All those estimates, confound treatment effects and selection bias. Tables 2 and 3 therefore also provide estimates of our bounds for the treatment effects, using the Sharp RD formulas (resp. the Fuzzy RD formulas) for the ITT (resp. the LATE).

The following points are useful to note for the behavior of our bounds in this application. First, the bounds for the average treatment effects among potentially-assigned units (Γ_-) are relatively tight for the non-formal-employment duration censored at 6 months after layoff. The lower bounds, in particular, are close to the naive RD estimates, with point estimates of 26.4 days (ITT) and 35.4 days (LATE) for the standard lower bounds. Second, the bounds for the average treatment effects become wider on both sides of the naive estimates when we consider higher censoring points. This difference comes from the fact that when increasing the censoring threshold the distribution of the outcome becomes more dispersed and has less probability mass at the censoring point. Third, comparing estimates in Table 3 and in the last two columns of Table 2 shows that bounds for quantile treatment effects can be tighter than bounds on average treatment effects in these cases. This is because bounds for quantile treatment effects are less sensitive to tails of the outcome distribution. For instance, when we censor the outcome at 24 months, we obtain bounds for the average treatment effect between 42.9 and 110 days, but between 67 and 120 days for the treatment effect at the median (FRD). Bounds are even tighter at other percentiles of the distribution; for instance they are between 87 and 114 days at the 30th percentile. Fourth, estimates that use behavioral assumptions to tighten our Fuzzy RD bounds are often similar to estimates for the standard bounds in our application. Yet, assuming that all always-assigned units take up the treatment (refinement B) closes half of the gap between our lower bound and the naive RD estimate when we censor the outcome at 6 months after layoff. Fifth, estimates that use covariates (here, a dummy for a replacement rate above/below the median) to tighten our Fuzzy RD bounds have no meaningful identifying power.[29] Sixth, and lastly, bounds for the average

[29] Bounds that use covariates are sometimes even wider than standard bounds. Despite our identification results, nothing guarantees that the bounds will actually be tighter in finite samples. In particular, we split the sample in two when estimating effects for the two categories, leading to less precise estimates.

treatment effect among units just to the right of the cutoff (Γ_+) are very similar to bounds for the potentially-assigned units. This is partly because the distributions of our outcome variables have a lot of probability mass at the extreme values of their support.

The estimates in Table 2 also allow us to provide a lower bound for the efficiency cost in equation (6.1). We estimate a lower bound for the behavioral effect on paid UI duration among UI takers to be around 35.4 days. This figure is only the numerator of the statistic of interest: $dD^B_{X=c^-}/D^B_{X=c^-}$. An estimate of the denominator will also be affected by manipulation. We thus estimated bounds for this ratio as well, yielding point estimates of .266 and .376 for the lower and upper bounds, respectively (95% CI: [.24;.49]). The point estimate for the lower bound increases to .29 (resp. .30) with our refinement in Theorem 3 (resp. Theorem 4) and the plausible assumption that always-assigned workers are at least as likely to take up the treatment (resp. are all taking up the treatment) as potentially-assigned workers. For completeness, we also estimated bounds for the second component of the efficiency cost in equation (6.1), approximating the change in subsequent formal employment by the change in the non-formal-employment duration censored at 24 months. We again estimated bounds taking into account that the denominator $D^B_{X=c^-}$ will also be affected by manipulation. We obtained a lower bound of .028 using the scaling factor $tax/b = .086$ (refinements in Theorems 3 and 4 have no meaningful influence on these figures). This second component thus only marginally affects our lower bound for the efficiency cost. Overall, these estimates indicate that at least 29.4 to 32.8 cents are lost to behavioral responses for each $1 reaching mechanical beneficiaries. To provide some perspective, the comparable figure in Gerard and Gonzaga (2016) is only around 20 cents for increases in the potential UI duration among workers with two years of continuous tenure at layoff. The marginal value of insurance must thus be significantly higher to justify lowering the 16-month eligibility cutoff in our case than to justify increasing the potential UI duration in their case.

Finally, we present the results of two additional exercises. First, we illustrate the alternative strategy for inference that we recommend when researchers have strong prior beliefs that

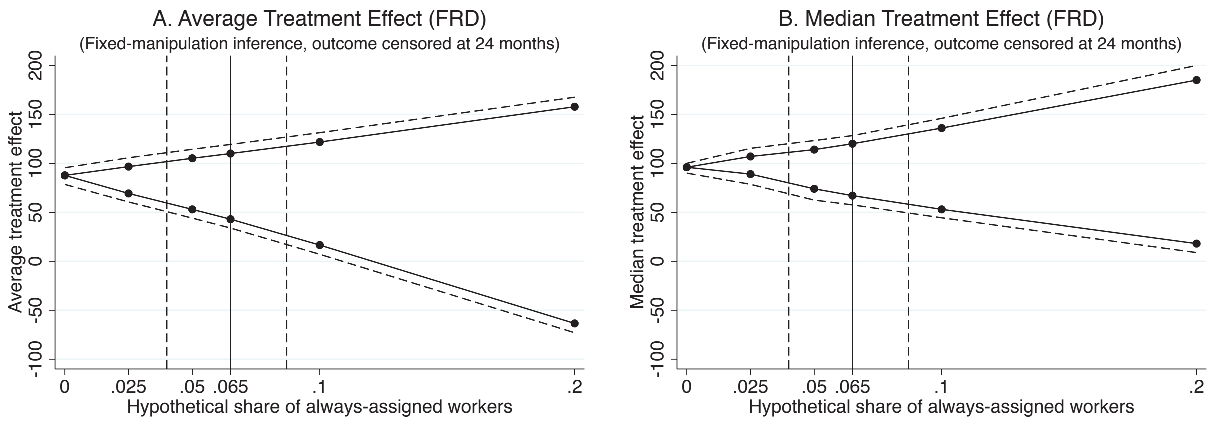

Figure 6.4: Fixed-manipulation inference for our empirical application

The figure displays point estimates and confidence intervals for our bounds for fixed levels of the degree of manipulation. We consider LATE/FRD estimates (standard bounds) for the average treatment effect and the quantile treatment effect at the 30th percentile for the outcome censored at 24 months. The solid vertical line (resp. dashed vertical lines) corresponds to our point estimate (resp. confidence interval) for the extent of manipulation (see Table 2).

manipulation is either absent or very rare in their setting. Figure 6.4 displays point estimates and confidence intervals for our bounds in the Fuzzy RD cases for various fixed levels of the extent of manipulation (hypothetical share of always-assigned units). Panel A shows that inference on the average treatment effect (censoring the outcome at 24 months) can be quite sensitive to the extent of manipulation. For instance, the width of the confidence intervals more than doubles when we assume a small degree of manipulation (a share of always-assigned units of 2.5%) rather than no manipulation. This illustrates the importance of taking into account the possibility of manipulation even when the McCrary (2008) test fails to reject the null hypothesis of no manipulation. The width of the confidence intervals grows quickly with larger degrees of manipulation. Panel B shows that inference on quantile treatment effects are also sensitive to the extent of manipulation. However, inference may remain meaningful in this case, even for rather large degrees of manipulation.

Second, we estimate the characteristics of potentially-assigned and always-assigned workers. This could in theory be useful to target policies aimed at mitigating manipulation in the timing of layoff. Table 4 displays the estimated difference in the mean of workers' characteristics at the cutoff (column 1), as would be typically presented in a RD analysis. The associated graphs are presented in the Appendix. We find significant evidence of selection in terms of wage and thus replacement rate, and sector of activity. Columns (2) and (3) then display the estimated means for potentially-assigned and always-assigned workers. Always-assigned workers earned on average .24 log point less, and were 30%-points less likely to come from the service sector than the potentially-assigned workers. The large difference in wages and thus replacement rates motivated the choice of using a dummy for replacement rates above/below the median to construct bounds in Tables 2 and 3.

Table 4: Characteristics of always- and potentially-assigned workers

	Difference at the cutoff	Potentially-assigned	Always-assigned
Share male	-0.0031	0.714	0.665
	[-0.0168;0.0105]	[0.704;0.724]	[0.439;0.892]
Average age	-0.0729	32.475	31.345
	[-0.3091;0.1633]	[32.304;32.645]	[27.627;35.063]
Average years of education	0.0011	9.104	9.121
	[-0.0803;0.0825]	[9.049;9.160]	[7.836;10.406]
Average tenure	0.0103	8.802	8.961
	[-0.0418;0.0623]	[8.771;8.833]	[8.100;9.821]
Average log wage	-0.016	6.704	6.456
	[-0.0308;-0.0012]	[6.693;6.716]	[6.208;6.704]
Average replacement rate	0.0051	0.720	0.800
	[0.0005;0.0098]	[0.717;0.724]	[0.722;0.878]
Share from commercial sector	0.0071	0.355	0.465
	[-0.0059;0.02]	[0.346;0.365]	[0.264;0.665]
Share from construction sector	0.0073	0.106	0.218
	[-0.0015;0.0161]	[0.099;0.112]	[0.079;0.358]
Share from industrial sector	0.0061	0.225	0.319
	[-0.006;0.0182]	[0.216;0.234]	[0.131;0.507]
Share from service sector	-0.0204	0.314	-0.002
	[-0.0332;-0.0077]	[0.305;0.324]	[-0.201;0.197]
Share from small firm	0.0083	0.367	0.496
(<10 employees)	[-0.0057;0.0224]	[0.357;0.377]	[0.268;0.730]

Notes: Total number of observations within our bandwidth of 30 days around the cutoff: 102,791 displaced formal workers. Numbers in square brackets are 95% confidence intervals calculated by adding $\pm 1.96\times$standard error to the respective point estimate,where standard errors are calculated via the bootstrap with 500 replications.

In sum, we find significant evidence of manipulation and selection at the cutoff. Our bounds imply that the magnitude of naive RD estimates may be heavily affected by selection, but that we can still draw useful conclusions from this empirical exercise.

7. CONCLUSIONS

In this paper, we propose a partial identification approach to deal with the issue of potentially manipulated running variables in RD designs. We show that while the data are unable to uniquely pin down treatment effects if manipulation occurs, they are generally still informative in the sense that they imply bounds on the value of interesting causal parameters in both sharp and fuzzy RD designs. Our main contribution is to derive and explicitly characterize these bounds. We also propose methods to estimate our bounds in practice, and discuss how to construct confidence intervals for treatment effects that have good coverage properties. The approach is illustrated with an application to the Brazilian unemployment insurance (UI) system. We recommend the use of our approach in applications irrespective of the outcome of McCrary's (2008) test for manipulation.

Our approach can also be useful for RD designs where running variables are not manipulated per se. Suppose for example that the probability of missing outcomes changes discontinuously at the cutoff. This could be the case if outcomes are based on surveys, and units are easier to locate and survey if they were assigned to the program. Our approach could be used to partially identify causal effects of the program at the cutoff for units whose outcomes would not be missing in the absence of program assignment. Suppose instead that one is interested in the causal effect of a treatment at some cutoff in a self-selected subpopulation. For instance, suppose that every displaced worker is eligible for some UI benefits but that the level of UI benefits changes discontinuously at some cutoff. One may be interested in the causal effect of a higher benefit level among UI takers. The density of the running variable in this subpopulation will be discontinuous at the cutoff, however, if displaced workers are more likely to take up UI when the benefit level is higher. Our approach could be used to partially identify causal effects of the program at the cutoff for units who would take up UI even when they are not eligible for the higher benefit level.[30]

[30]Card, Dobkin, and Maestas (2009) faced essentially the same problem by studying the effect of Medicare on mortality in the subpopulation admitted to a hospital around the 65 years old eligibility cutoff.

A. ADDITIONAL TECHNICAL RESULTS FOR IDENTIFICATION ANALYSIS

A.1. Proof of Lemma 1

Since the density of the running variable is continuous around the cutoff c among potentially-assigned units by Assumption 2, we have that $f_{X|M=0}\left(c^{-}\right) = f_{X|M=0}\left(c^{+}\right)$, and therefore $f_X\left(c^{+}\right) = \left(1 - \mathrm{P}\left(M=1\right)\right) f_{X|M=0}\left(c^{-}\right) + \mathrm{P}\left(M=1\right) f_{X|M=1}\left(c^{+}\right)$. Since $f_{X|M=1}(x) = 0$ for $x < c$ by Assumption 3, we also have that $f_X\left(c^{-}\right) = \left(1 - \mathrm{P}\left(M=1\right)\right) f_{X|M=0}\left(c^{-}\right)$. Hence $(f_X\left(c^{+}\right) - f_X\left(c^{-}\right))/f_X\left(c^{+}\right) = f_{X|M=1}(c^{+})\mathrm{P}(M=1)/f_X(c^{+}) = \tau$, where the last equality follows from Bayes' Theorem. □

A.2. Proof of Lemma 2

By Assumption 1(i) and the law of total probability, our model implies that $\mathrm{E}(D_i|X_i = c^{+}) \cdot (1-\tau_1)/(1-\tau) > \mathrm{E}(D_i|X_i = c^{-})$ and $\tau = \tau_1 \cdot \mathrm{E}(D_i|X_i = c^{+}) + \tau_0 \cdot (1 - \mathrm{E}(D_i|X_i = c^{+}))$. By construction, any point $(\tau_1, \tau_0) \notin \mathcal{T}$ is incompatible with at least one of these two restrictions. It thus remains to be shown that any point $(\tau_1, \tau_0) \in \mathcal{T}$ is compatible with our model and the observed joint distribution of (Y, D, X). Note that it suffices to consider the latter distribution for $X \in (c-\epsilon, c+\epsilon)$ for some small $\epsilon > 0$, as our model has no implications for the distribution of observables outside that range. Let $(\tilde{Y}(1), \tilde{Y}(0), \tilde{D}^{+}, \tilde{D}^{-}, \tilde{M}, \tilde{X})$ be a random vector taking values on the support of $(Y(1), Y(0), D^{+}, D^{-}, M, X)$, and define $\tilde{D}$ and $\tilde{Y}$ analogous to D and Y in our Section 2.1. We now construct a particular joint distribution of $(\tilde{Y}(1), \tilde{Y}(0), \tilde{D}^{+}, \tilde{D}^{-}, \tilde{M}, \tilde{X})$. For $x \in (c-\epsilon, c+\epsilon)$, let

$$f_{\tilde{X}}(x) = f_X(x) \quad \text{and} \quad \mathrm{P}(\tilde{M} = 1|\tilde{X} = x) = \begin{cases} 1 - f_X(c^{-})/f_X(x) & \text{if } x \geq c \\ 0 & \text{if } x < c. \end{cases}$$

Moreover, let

$$
\begin{aligned}
P(\tilde{D}^- = 0, \tilde{D}^+ = 1|\tilde{X} = x, \tilde{M} = 0) &= \begin{cases} P(D = 1|X = x) \cdot \frac{1-\tau_1}{1-\tau} - P(D = 1|X = c^-) & \text{if } x \geq c, \\ P(D = 1|X = c^+) \cdot \frac{1-\tau_1}{1-\tau} - P(D = 1|X = x) & \text{if } x < c, \end{cases} \\
P(\tilde{D}^- = 1, \tilde{D}^+ = 1|\tilde{X} = x, \tilde{M} = 0) &= \begin{cases} P(D = 1|X = c^-) & \text{if } x \geq c, \\ P(D = 1|X = x) & \text{if } x < c, \end{cases} \\
P(\tilde{D}^- = 0, \tilde{D}^+ = 0|\tilde{X} = x, \tilde{M} = 0) &= 1 - P(\tilde{D}^- = 0, \tilde{D}^+ = 1|\tilde{X} = x, \tilde{M} = 0) \\
&\quad - P(\tilde{D}^- = 1, \tilde{D}^+ = 1|\tilde{X} = x, \tilde{M} = 0), \\
P(\tilde{D}^- = 1, \tilde{D}^+ = 0|\tilde{X} = x, \tilde{M} = 0) &= 0,
\end{aligned}
$$

and

$$
\begin{aligned}
P(\tilde{D}^- = 0, \tilde{D}^+ = 1|\tilde{X} = x, \tilde{M} = 1) &= \begin{cases} P(D = 1|X = x) \cdot \frac{\tau_1}{\tau} - h(x) & \text{if } x \geq c, \\ P(D = 1|X = c^+) \cdot \frac{\tau_1}{\tau} - h(c^+) & \text{if } x < c, \end{cases} \\
P(\tilde{D}^- = 1, \tilde{D}^+ = 1|\tilde{X} = x, \tilde{M} = 1) &= \begin{cases} h(x) & \text{if } x \geq c, \\ h(c^+) & \text{if } x < c, \end{cases} \\
P(\tilde{D}^- = 0, \tilde{D}^+ = 0|\tilde{X} = x, \tilde{M} = 1) &= 1 - P(\tilde{D}^- = 0, \tilde{D}^+ = 1|\tilde{X} = x, \tilde{M} = 1), \\
&\quad - P(\tilde{D}^- = 1, \tilde{D}^+ = 1|\tilde{X} = x, \tilde{M} = 1), \\
P(\tilde{D}^- = 1, \tilde{D}^+ = 0|\tilde{X} = x, \tilde{M} = 1) &= 0,
\end{aligned}
$$

where $h(\cdot)$ is an arbitrary continuous function satisfying that $0 \leq h(x) \leq P(D = 1|X = x) \cdot \tau_1/\tau$. With these choices, the implied distribution of $(\tilde{D}, \tilde{X})|\tilde{X} \in (c-\epsilon, c+\epsilon)$ is the same as that of $(D, X)|X \in (c-\epsilon, c+\epsilon)$ for every $(\tau_1, \tau_0) \in \mathcal{T}$. It thus remains to be shown that one can construct a distribution of $(\tilde{Y}(1), \tilde{Y}(0))$ given $(\tilde{D}^+, \tilde{D}^-, \tilde{X}, \tilde{M})$ that is compatible with our assumptions and such that the distribution of $\tilde{Y}$ given $(\tilde{D}, \tilde{X})$ for $\tilde{X} \in (c-\epsilon, c+\epsilon)$ is the same as the distribution of Y given (D, X) for $X \in (c-\epsilon, c+\epsilon)$ for every $(\tau_1, \tau_0) \in \mathcal{T}$. But this is always possible because our model encompasses the setting in which the label "always-assigned unit" is randomly assigned with probability τ_d to units with treatment status d and running variable to the right of the cutoff. Put differently, the distribution of $(Y(1), Y(0))$ given (D^+, D^-, X, M) implies no restrictions on the values of τ_1 and τ_0. $\square$

B. BOUNDS FOR NON-CONTINUOUSLY DISTRIBUTED OUTCOMES

Theorem 1 and 2 are stated for the case in which the outcome variable is continuously distributed. This is for notational convenience only, and our results immediately generalize to the case of a discrete outcome variable, which occur frequently in empirical applications. Suppose that supp(Y) is a finite set. Then in the case of a Sharp RD design our sharp upper and lower bounds on $F_{Y(1)|X=c,C_0}$ are

$$F^U_{1,SRD}(y) = (1-\theta^U)F_{Y|X=c^+,Y>Q_{Y|X=c^+}(\tau)}(y) + \theta^U \mathbb{I}\left\{y \geq Q_{Y|X=c^+}(\tau)\right\} \quad \text{and}$$
$$F^L_{1,SRD}(y) = (1-\theta^L)F_{Y|X=c^+,Y<Q_{Y|X=c^+}(1-\tau)}(y) + \theta^L \mathbb{I}\left\{y \geq Q_{Y|X=c^+}(1-\tau)\right\},$$

where

$$\theta^L = \frac{\mathrm{P}(Y \geq Q_{Y|X=c^+}(1-\tau)|X=c^+) - \tau}{1-\tau} \quad \text{and} \quad \theta^U = \frac{\mathrm{P}(Y \leq Q_{Y|X=c^+}(\tau)|X=c^+) - \tau}{1-\tau}.$$

The following Corollary uses these bounds to obtain explicit sharp bounds on the average treatment effect Γ_- and the quantile treatment effect $\Psi_-(u)$.

Corollary 7. *Suppose that the assumptions of Theorem 1 hold, and that supp(Y) is a finite set. Then sharp lower and upper bounds on Γ_- are given by*

$$\begin{aligned}
\Gamma^L_{-,SRD} &= (1-\theta^L)\mathrm{E}(Y|X=c^+, Y<Q_{Y|X}(1-\tau|c^+)) + \theta^L Q_{Y|X}(1-\tau|c^+) \\
&\quad - \mathrm{E}(Y|X=c^-) \quad \text{and} \\
\Gamma^U_{-,SRD} &= (1-\theta^U)\mathrm{E}(Y|X=c^+, Y>Q_{Y|X}(\tau|c^+)) + \theta^U Q_{Y|X}(\tau|c^+) \\
&\quad - \mathrm{E}(Y|X=c^-),
\end{aligned}$$

respectively; and sharp lower and upper bounds on $\Psi_{-}(u)$ are given by

$$\Psi^{L}_{-,SRD}(u) = Q_{Y|X=c^+}((1-\tau)u) - Q_{Y|X=c^-}(u) \quad \text{and}$$
$$\Psi^{U}_{-,SRD}(u) = Q_{Y|X=c^+}(\tau + (1-\tau)u) - Q_{Y|X=c^-}(u),$$

respectively, for every quantile level $u \in (0,1)$.

In a Fuzzy RD design, we modify the expressions for the sharp upper and lower bounds on $F_{Y(1)|X=c,C_0}$ and $F_{Y(0)|X=c,N_0}$ for known values of τ_1 and τ_0 as follows:

$$F^{U}_{1,FRD}(y,\tau_1,\tau_0) = (1-\theta^{U}_{1})G_{Y|Y>Q_G\left(\frac{\tau_1}{1-\kappa_1}\right)}(y) + \theta^{U}_{1}\mathbb{I}\left\{y \geq Q_G\left(\frac{\tau_1}{1-\kappa_1}\right)\right\} \quad \text{and}$$
$$F^{L}_{1,FRD}(y,\tau_1,\tau_0) = (1-\theta^{L}_{1})G_{Y|Y<Q_G\left(1-\frac{\tau_1}{1-\kappa_1}\right)}(y) + \theta^{L}_{1}\mathbb{I}\left\{y \geq Q_G\left(1-\frac{\tau_1}{1-\kappa_1}\right)\right\},$$

where

$$\theta^{U}_{1} = \frac{\mathrm{P}_G\left(Y \leq Q_G\left(\frac{\tau_1}{1-\kappa_1}\right)\right) - \frac{\tau_1}{1-\kappa_1}}{1-\frac{\tau_1}{1-\kappa_1}}\theta^{L}_{1} = \frac{\mathrm{P}_G\left(Y \geq Q_G\left(1-\frac{\tau_1}{1-\kappa_1}\right)\right) - \frac{\tau_1}{1-\kappa_1}}{1-\frac{\tau_1}{1-\kappa_1}}.$$

The modified expressions for bounds on $F_{Y(0)|X=c,N_0}$ are given by

$$F^{U}_{Y(0)|X=c,N_0}(y) = \int_{-\infty}^{y} s(t)\mathbb{I}\left\{t \leq q_U\right\}dt + \theta^{U}_{0}\mathbb{I}\left\{y > q_U\right\} \quad \text{and}$$
$$F^{L}_{Y(0)|X=c,N_0}(y) = \int_{-\infty}^{y} s(t)\mathbb{I}\left\{t \geq q_L\right\}dt + \theta^{L}_{0}\mathbb{I}\left\{y > q_L\right\}.$$

where

$$\theta^{U}_{0} = 1 - \int_{-\infty}^{q_U} s(t)\mathbb{I}\left\{t \leq q_U\right\}dt \quad \text{and} \quad \theta^{L}_{0} = 1 - \int_{q_L}^{\infty} s(t)\mathbb{I}\left\{t \geq q_L\right\}dt,$$

and

$$q_L = \inf\{y \in \operatorname{supp}(Y) : \int_{q_L}^{\infty} s(t)dt \leq 1\} \quad \text{and} \quad q_U = \sup\{y \in \operatorname{supp}(Y) : \int_{-\infty}^{q_U} s(t)dt \leq 1\}.$$

We then obtain the following expressions for sharp bounds on the average treatment effect

Γ_- and the quantile treatment effect $\Psi_-(u)$ given knowledge of τ_1 and τ_0:

$$\Gamma^U_{-,FRD}(\tau_1,\tau_0) \equiv \int y dF^U_{1,FRD}(y,\tau_1,\tau_0) - \int y dF^L_{0,FRD}(y,\tau_1,\tau_0),$$
$$\Gamma^U_{-,FRD}(\tau_1,\tau_0) \equiv \int y dF^L_{1,FRD}(y,\tau_1,\tau_0) - \int y dF^U_{0,FRD}(y,\tau_1,\tau_0),$$
$$\Psi^U_{-,FRD}(u,\tau_1,\tau_0) \equiv Q^U_{1,FRD}(u,\tau_1,\tau_0) - Q^L_{0,FRD}(u,\tau_1,\tau_0),$$
$$\Psi^L_{-,FRD}(u,\tau_1,\tau_0) \equiv Q^L_{1,FRD}(u,\tau_1,\tau_0) - Q^U_{0,FRD}(u,\tau_1,\tau_0).$$

The following Corollary finally states the sharp bounds on Γ_- and $\Psi_-(u)$ given that the values of τ_1 and τ_0 are only partially identified.

Corollary 8. *Suppose that the assumptions of Theorem 2 hold, and that supp(Y) is a finite set. Then sharp lower and upper bounds on Γ_- are given by*

$$\Gamma^L_{-,FRD} = \inf_{(t_1,t_0)\in\mathcal{T}} \Gamma^L_{-,FRD}(t_1,t_0) \quad \text{and} \quad \Gamma^U_{-,FRD} = \sup_{(t_1,t_0)\in\mathcal{T}} \Gamma^U_{-,FRD}(t_1,t_0),$$

respectively; and sharp lower and upper bounds on $\Psi_-(u)$ are given by

$$\Psi^L_{-,FRD}(u) = \inf_{(t_1,t_0)\in\mathcal{T}} \Psi^L_{-,FRD}(u,t_1,t_0) \quad \text{and} \quad \Psi^U_{-,FRD}(u) = \sup_{(t_1,t_0)\in\mathcal{T}} \Psi^U_{-,FRD}(u,t_1,t_0),$$

respectively, for every quantile level $u \in (0,1)$.

C. ADDITIONAL TABLES AND GRAPHS

We present here some supporting graphs. Figure C.5 displays the distribution of our outcome variable (duration without a formal job, censored at two years after layoff) on the left and on the right of the cutoff (30-day window around the cutoff). Figure C.6 displays the distribution of our outcome variable on the right of the cutoff for workers with wages at layoff above/below the median (and thus replacement rates below/above the median). Figure C.7 displays the full schedule of the UI benefit level, which is a function of a beneficiary's average monthly wage in the three years prior to her layoff. Figure C.8 displays the mean of different covariates on each side of the cutoff by day between the layoff and eligibility dates.

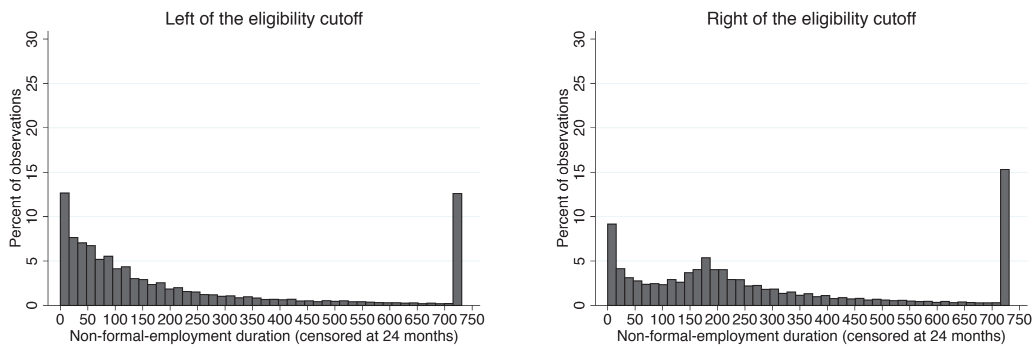

Figure C.5: Distribution of our outcome variable on each side of the cutoff

The figure displays the distribution of our outcome variable (duration without a formal job, censored at two years after layoff) on the left and on the right of the cutoff (30-day window on each side of the cutoff). The figure is based on a sample of 102,791 displaced formal workers.

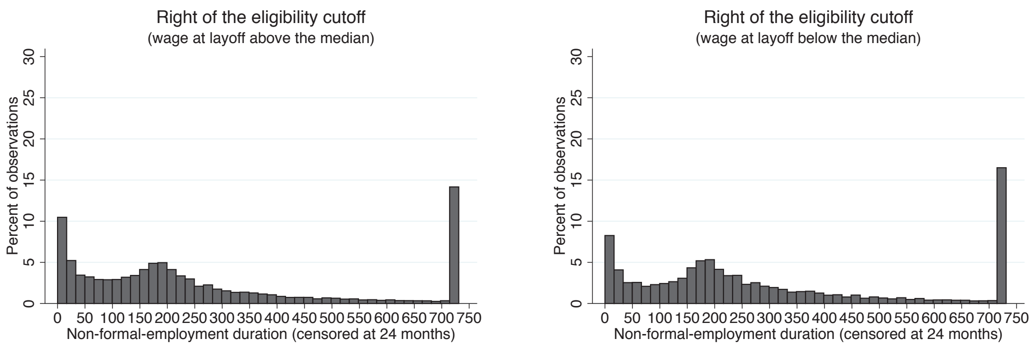

Figure C.6: Distribution of our outcome variable on the right side of the cutoff by wage at layoff

The figure displays the distribution of our outcome variable (duration without a formal job, censored at two years after layoff) on the right of the cutoff (30-day window on each side of the cutoff) for workers with wages at layoff above/below the median (and thus replacement rates below/above the median). The figure is based on a sample of 102,791 displaced formal workers.

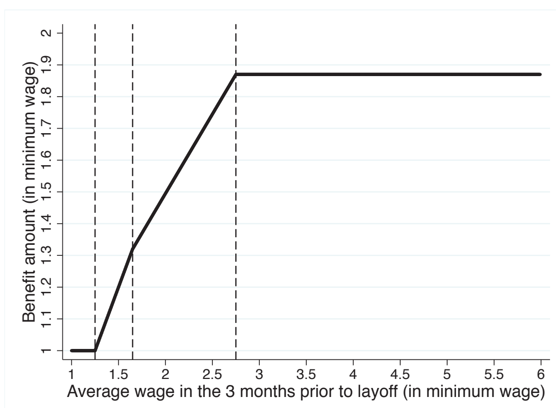

Figure C.7: Monthly UI benefit amount

The figure displays the relationship between a UI beneficiary's average monthly wage in the three months prior to her layoff and her monthly UI benefit level. All monetary values are indexed to the federal minimum wage, which changes every year. The replacement rate is 100% at the bottom of the wage distribution as the minimum benefit level is equal to one minimum wage. The graph displays a slope of 0% until 125% of the minimum wage, then of 80% until 165% of the minimum wage, and finally of 50% until 275% of the minimum wage. The maximum benefit level is equal to 187% of the minimum wage.

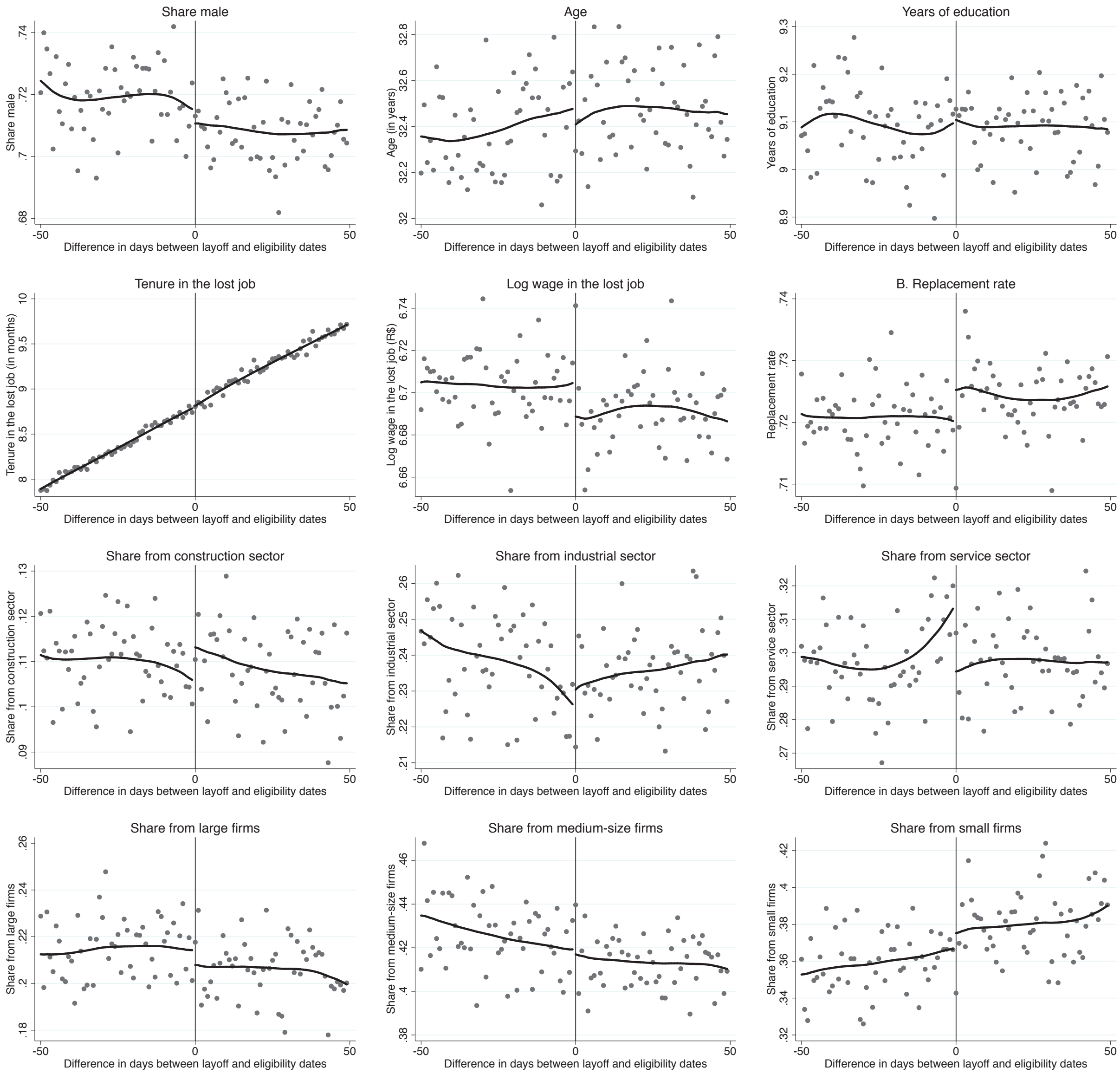

Figure C.8: Graphical evidence for the characteristics of always-assigned units in our empirical application

The figure displays the mean of different covariates on each side of the cutoff by day between the layoff and eligibility dates, as well as local linear regressions on each side of the cutoff using an edge kernel and a bandwidth of 30 days. The figure is based on a RD sample of 169,575 displaced formal workers.

D. ILLUSTRATING THE POTENTIAL IMPACT OF MANIPULATION

In this Appendix, we describe how to construct confidence intervals for Γ_- under the assumption that the level of manipulation τ is assumed to be equal to some known constant τ^*. The following modified bootstrap algorithm delivers the desired confidence interval.

1. For $\tau^* \in [0,1]$ and $t \in [0,1]$, define $\widehat{\Gamma}_-^L(\tau^*, t)$ and $\widehat{\Gamma}_-^U(\tau^*, t)$ exactly as $\widehat{\Gamma}_-^L(\widehat{\eta}_1(t), \widehat{\eta}_0(t))$ and $\widehat{\Gamma}_-^U(\widehat{\eta}_1(t), \widehat{\eta}_0(t))$, with the exception that τ^* is used instead of $\widehat{\tau}$.
2. Generate bootstrap samples $\{Y_{i,b}, D_{i,b}, X_{i,b}\}_{i=1}^n$, $b = 1, \dots, B$ by sampling with replacement from the original data $\{Y_i, D_i, X_i\}_{i=1}^n$; for some large integer B.
3. For $j \in \{U, L\}$, calculate $\widehat{\Gamma}_{-,b}^j(\tau^*, t)$, and put $\widehat{\sigma}^j(\tau^*, t)$ as the sample standard deviation of $\{\widehat{\Gamma}_{-,b}^j(\tau^*, t)\}_{b=1}^B$.
4. Compute the $1-\alpha$ confidence interval

$$\mathcal{C}_{1-\alpha}^{FRD}(\tau^*) = \left[\inf_{t\in[0,1]} \left(\widehat{\Gamma}_-^L(\tau^*, t) - r_\alpha(\tau^*, t) \cdot \widehat{\sigma}^L(\tau^*, t)\right), \sup_{t\in[0,1]} \left(\widehat{\Gamma}_-^U(\tau^*, t) + r_\alpha(\tau^*, t) \cdot \widehat{\sigma}^U(\tau^*, t)\right)\right],$$

where $r_\alpha(\tau^*, t)$ is the value that solves the equation

$$\Phi\left(r_\alpha(\tau^*, t) + \frac{\widehat{\Gamma}_-^U(\tau^*, t) - \widehat{\Gamma}_-^L(\tau^*, t)}{\max\{\widehat{\sigma}^L(\tau^*, t), \widehat{\sigma}^U(\tau^*, t)\}}\right) - \Phi(-r_\alpha(\tau^*, t)) = 1 - \alpha.$$

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