Crime and Mismeasured Punishment:
Marginal Treatment Effect with Misclassification∗

Vitor Possebom†

Sao Paulo School of Economics - FGV

First Draft: May 2021; This Draft: July 2022
Please click here for the most recent version

Abstract

I partially identify the marginal treatment effect (MTE) function when the treatment variable is misclassified. To do so, I explore three sets of restrictions on the relationship between the instrument, the misclassified treatment and the correctly measured treatment, allowing for dependence between the instrument and the misclassification decision. If the signs of the derivatives of the correctly measured propensity score and the mismeasured one are the same, I identify the sign of the MTE function at every point in the instrument’s support. If those derivatives are close to each other, I bound the MTE function. Finally, by imposing a functional restriction between those two propensity scores, I derive sharp bounds around the MTE function and any weighted average of the MTE function. To illustrate the usefulness of my partial identification method, I analyze the impact of alternative sentences — e.g., fines or community services — on recidivism using random assignment of judges within Brazilian court districts. In this context, misclassification is an issue when the researcher measures the treatment based solely on trial judge’s rulings, ignoring that the Appeals Court may reverse sentences. I show that, when I use the trial judge’s rulings as my misclassified treatment variable, the misclassification bias may be as large as 10% of the MTE function, which can be estimated using the final ruling in each case as my correctly measured treatment variable. Moreover, I show that the proposed bounds contain the MTE function in this empirical example.

∗I would like to thank Santiago Acerenza, Fernando Ramon Machado de Andrade, Nathan J. Canen, Xiaohong Chen, Bruno Ferman, John Finlay, Paul Goldsmith-Pinkham, Renata Hirota, John Eric Humphries, Hugo Jales, Désiré Kédagni, Helena Laneuville, Julian Martinez-Iriarte, Marcela Mello, Michael Mueller-Smith, Yusuke Narita, Ahyan Panjwani, Renato Caetano de Almeida Silva, Julio Trecenti, Takuya Ura and Edward Vytlacil, the institutional support of Associação Brasileira de Jurimetria, and seminar participants at Yale University, O’Neill School of Public and Environmental Affairs (Indiana University), 2021 Latin American Meeting of the Econometric Society, 2021 Causal Data Science Meeting, 43rd Meeting of the Brazilian Econometrics Society, 2022 Texas Economics of Crime Workshop, New York Camp Econometrics XVI, Sao Paulo School of Economics - FGV, Federal University of Pernambuco, Universidad ORT Uruguay, PUC-Rio, Insper and School of Time Series and Econometrics for helpful suggestions.

†Email: vitor.possebom@fgv.br
Keywords: Misclassification, Instrumental Variable, Partial Identification, Alternative Sentences, Recidivism

JEL Codes: C31, C36, K42
1 Introduction

Evaluating a policy with a misclassified treatment variable is theoretically challenging (Ura, 2018; Calvi et al., 2021; Acerenza et al., 2022) and practically relevant (Millimet, 2011). Moreover, this type of problem is widespread in empirical economics. For example, when analyzing the effect of incarceration or alternative sentences in Crime Economics, the treatment variable will be misclassified if the researcher has information only about the trial judge ruling. In this context, measurement error is created by the appealing process because Appeals Court judges may reverse the trial judge’s ruling (Green and Winik, 2010). Furthermore, education attainment (Black et al., 2003; Card, 2001; Kane et al., 1999), immigration status (Bernstein et al., 2019), unionization (Card, 1992; Bollinger, 1996) and welfare program participation (Hernandez et al., 2007; Hernandez and Pudney, 2007; Meyer et al., 2015, 2018; Meyer and Mittag, 2019a,b; Bruckmeier et al., 2021; Celhay et al., 2022) are likely to be mismeasured. Moreover, with the increasing availability of large data sets, prediction methods are being used to infer the treatment status in a variety of empirical questions (Black et al., 2020; Arellano-Bover, 2020). Since no prediction algorithm perfectly classifies the treatment status, the observed treatment variable is misclassified.

In this paper, I provide easy-to-compute bounds around the marginal treatment effect (MTE) function when the treatment variable is misclassified. To do so, I propose three partial identification strategies under increasingly restrictive sets of assumptions, extending the MTE framework (Heckman and Vytlacil, 1999) to scenarios with a misclassified treatment variable.

The MTE is a function that captures the effect of a treatment for the individual who is indifferent between taking the treatment or not. In the Crime Economics example, the MTE function captures the effect of being punished with an alternative sentence on recidivism for the defendant who is at the margin of being punished conditional on her judge’s leniency level. By analyzing this treatment effect parameter at different margins of judge’s leniency, we may find a negatively sloped MTE function, implying that individuals who would be punished even by very lenient judges are harmed by their alternative sentences while individuals who
would be punished only by very strict judges benefit from them. Therefore, understanding the unobserved heterogeneity of alternative sentences’ impact is key to understanding its benefits and costs. In particular, a policymaker may want to train judges to only use sentencing criteria that reduce recidivism according to the MTE function.  

My partial identification strategy for the MTE function with a misclassified treatment variable offers a menu of estimates based on three sets of assumptions. These assumptions simultaneously address endogenous selection into treatment and non-classical measurement error. Since these sets gradually add stronger assumptions to tighten the bounds around the MTE function, the researcher can build a menu of estimates and transparently analyze the informational content of each assumption as recommended by Tamer (2010). This type of analysis is also advocated by Manski (2011), who refers to it as a layered policy analysis.

Four assumptions are common to all sets of assumptions used in this paper. First, the instrument is independent of the potential outcomes and the latent heterogeneity variable that defines the true treatment. Second, the instrument is strong, impacting the correctly measured and mismeasured probabilities of receiving the treatment. Third, the latent heterogeneity variable that defines the treatment is continuous. Finally, the potential outcomes’ first moments are finite. These assumptions are standard in the literature about instrumental variables as illustrated by Heckman et al. (2006) and are used here to address the problem of endogenous selection into treatment.

Under these four assumptions, I show that the Local Instrumental Variable (LIV) estimand is biased relative to the MTE function when the treatment variable is misclassified. This bias depends on the instrument’s value and may move the LIV estimand in any direction. Consequently, predicting the direction of the misclassification bias is challenging even with expert knowledge. Moreover, when using a misclassified treatment variable, the standard instrument validity tests (Frandsen et al., 2019, and Heckman and Vytlacil, 2005) may fail, and the IV weights may not integrate to one even when all weights are positive.

To account for misclassification of the treatment variable, I add three increasingly strong

---

1Designing this policy would be impossible if the researcher had access only to a single summarizing measure of the distribution of treatment effects such as the average treatment effect, the average treatment effect on the treated or the IV estimand, which is a weighted average of the MTE function (Heckman et al., 2006).
assumptions to those four common assumptions.

First, I impose that the impact of the instrument on the correctly measured probability of treatment has the same sign as the impact of the instrument on the mismeasured probability of treatment. Using this assumption, I can identify the sign of the MTE function at any point in the instrument’s support.

Second, I impose that the impact of the instrument on the mismeasured probability of treatment is close to the impact of the instrument on the correctly measured probability of treatment. Using this assumption, I can uniformly bound the magnitude of the MTE function at any point and analyze the bounds’ sensitivity to the degree of misclassification.

Third, I impose a functional relationship between the mismeasured probability of treatment and the correctly measured probability of treatment. Using this assumption and imposing that the outcome variable is binary, I can derive sharp uniform bounds around the MTE function. Consequently, I can sharply bound any treatment effect that can be written as a weighted integral of the MTE function, such as the Average Treatment Effect (ATE), the Average Treatment Effect on the Treated (ATT) and the Average Treatment Effect on the Untreated (ATU).

To estimate the aforementioned bounds around the MTE function, I suggest a parametric model. I impose a polynomial model for the correctly measured propensity score and for the conditional expectation of the treatment effect as a function of the latent resistance to treatment, implying that the outcome equation’s reduced-form model is a polynomial function too. By combining this object with a reduced-form polynomial model for the misclassified treatment variable, I can estimate the mismeasured LIV estimand and use it to estimate the MTE function’s sign and bounds.

To exemplify the identification tools proposed in this work, I evaluate the effect of alternative sentences on recidivism in the State of São Paulo, Brazil, between 2010 and 2019.\footnote{São Paulo is the largest state in Brazil, with a population above 41 million people according to the Brazilian Census in 2010. Moreover, analyzing the impact of judicial policies on criminal behavior in this state is relevant due to its relatively high criminality. For example, according to São Paulo Public Safety Secretary, there were 6,481 murders, 878.83 thefts and 490.23 robberies per 100,000 inhabitants in 2020. Importantly, theft is one of the most common crimes in my sample.} To do so, I observe trial judge’s full sentences and divide them into two groups: (i) punished...
(treated group), containing defendants who were fined or sentenced to community services, and (ii) not punished (untreated group), containing defendants who were acquitted or whose cases were dismissed. To measure recidivism, I check whether the defendant’s name appears in any criminal case within two years after the final sentence date.

This context illustrates my framework in three dimensions. First, we need an instrumental variable because the econometrician does not observe all the variables that influence the defendant’s future criminal behavior and are used by the trial judge to decide the defendant’s punishment. To address endogenous selection into punishment, I use the trial judge’s leave-one-out rate of punishment (or “leniency rate”) as an instrument for the trial judge’s decision (Bhuller et al., 2019; Agan et al., 2021). Second, this instrument is continuous, allowing me to estimate the MTE function flexibly. Third, in Brazil, defendants will only fulfill their sentences after their judicial case is closed, implying that they will only be punished after the Appeals process or after they inform the Court System that they will not appeal. Consequently, using only trial judges’ rulings to define which defendants were punished with an alternative sentence introduces a natural misclassification problem.

To better understand my methods’ ability to identify the correctly measured MTE function, I also collect data on Appeals Court’s decisions. By doing so, I can use the results based on the correctly measured punishment decision (each case’s final ruling) to evaluate the bias in an analysis that ignores the misclassification problem. Moreover, I can compare these results with the results derived from the proposed set identification methods.

I find that the misclassification bias can be relatively large and complex. For example, I estimate that this type of bias can be as small as 2.4% or as large as 11.3% of the target parameter depending on observable characteristics. Moreover, the misclassification bias can

3This instrumental variable is independent of the defendant’s counterfactual criminal behaviors because trial judges’ are randomly assigned to each case conditional on court districts according to state law in São Paulo.

4Within the empirical judge fixed effect literature, some authors construct their treatment variables based on trial judges’ rulings — e.g., Kling (2006, California’s dataset); Green and Winik (2010); Loeffler (2013); Aizer and Doyle (2015); Bhuller et al. (2019) — while others construct their treatment variables based on the final ruling or on the actual sentence served by the defendant — e.g., Kling (2006, Florida’s dataset); Dobbie et al. (2018); Arteaga (2019). Although the first group of authors is careful when interpreting their results as the impact of trial judge’s decisions on future criminal and labor outcomes, we may affirm that there would be a misclassification problem if their focus had been on identifying the effect of final rulings.
be either positive or negative depending on the geographic location and it can change its sign depending on the instrument’s value. Consequently, even an expert may have difficulties theorizing about the misclassification bias’ direction and magnitude. For this reason, it is useful to adopt methods that account for a possibly misclassified treatment variable.

Moreover, I find that the proposed partial identification strategies work in practice. When estimating the sign of the MTE function, my method achieves the correct conclusion for more than 98% of the evaluated points. When bounding the MTE function, the estimated sets cover the estimated MTE function entirely for the four largest districts where I focus my analysis.

Finally, using each case’s final rulings as my treatment variable, I find that the effect of alternative sentences on recidivism is likely small. Moreover, the observable geographic heterogeneity across court districts appears to matter more than the unobservable heterogeneity across defendants’ resistance to treatment.

Concerning its theoretical contribution, my work is inserted in the literature about identifying treatment effect parameters with measurement error. As illustrated by the survey articles by Schennach (2016) and Hu (2017), this literature is vast and growing. The studies in this literature differ with respect to four important aspects: whether the true treatment variable is endogenous, whether treatment effects are heterogeneous, which variable is mismeasured (outcome, treatment or instrument), and the target parameter.

Similarly to my work, four recent papers focused on identifying treatment effect parameters with heterogeneous effects, endogenous selection into treatment and misclassification of the treatment variable: Ura (2018), Calvi et al. (2021), Tommasi and Zhang (2020) and Acerenza et al. (2022).

These papers differ, for example, with respect to their target parameters. While the first three papers focus on discrete instruments and the Local Average Treatment Effect (LATE) parameter (Imbens and Angrist, 1994), Acerenza et al. (2022) and I focus on continuous instruments and the MTE function.\footnote{Yanagi (2019) also focus on the LATE parameter with a misclassified treatment variable, but, differently from the work mentioned here, he requires an extra exogenous covariate to point-identify his parameter of interest. Moreover, Acerenza (2021) extends the method proposed by Acerenza et al. (2022) to cover the case...}
Moreover, unlike these four papers, I do not assume that the instrument is independent of the potential misclassified treatment variables or the misclassification decision. This flexibility is relevant in a variety of applied examples as documented by Bound et al. (2001) in a Labor Economics context and Haider and Stephens (2020) in a Health Economics context.

Allowing for co-dependence between the instrument and the misclassification decision is particularly important in my empirical application. Since sentences by extreme trial judges may be more frequently reversed than sentences by median trial judges, my instrument may be correlated with the misclassification decision. In fact, I find that stricter trial judges are more likely to have their sentences reversed in my empirical application (Subsection 5.1).

Even when Ura (2018, Subsection 3.4) and Acerenza et al. (2022, Appendix G) extend their main bounds to allow for dependence between the instrument and the potential misclassified treatment variables, our strategies still complement each other. While I provide easy-to-derive bounds that can be used in a sensitivity analysis with respect to the degree of misclassification, they focus on worst-case bounds.

Concerning its empirical contribution, my work is inserted in the literature about the effect of alternative sentences on future criminal behavior. Three recent papers in this field were written by Huttunen et al. (2020), Giles (2021) and Klaassen (2021). While the first group of authors uses data from Finland and uses a judge fixed-effect strategy, the second uses data from Milwaukee (a city in the State of Wisconsin in the U.S.) and leverages a state law change as a source of exogenous variation in a regression discontinuity design. Both sets of authors find that alternative sentences increase recidivism. Differently from them, Klaassen (2021) finds that alternative sentences decrease recidivism in North Carolina (a state in the U.S.) by using a judge fixed-effect strategy. Unlike these previous studies, my estimated treatment effect parameters are small and rarely statistically different from zero. Given the large amount of observable geographic heterogeneity in my estimated results, the difference between the recent literature and my findings may be due to different contexts in Finland, Milwaukee, North Carolina and São Paulo. A deeper understanding of the mechanisms behind these differences is beyond the scope of this work, even though they deserve further investigation.
This paper is organized as follows. Section 2 presents the structural model and the misclassification mechanism, and discusses the identifying assumptions. In Section 3, I provide the identification results for the MTE bounds with a misclassified treatment variable under each set of assumptions. Moreover, Section 4 briefly explains how to estimate the objects that are necessary to implement the identification strategy described in the previous section. Finally, Section 5 describes the data and discusses the empirical results, while Section 6 concludes.

This paper also contains an online supporting appendix. All proofs are detailed in Appendix A. Appendices B and C explain two problems generated by misclassification in the MTE framework. In Appendix D, I discuss simple economic models that ensure that the identifying assumptions in Section 2 hold. Furthermore, in Appendix E, I extend my model to encompass the cases with more than one instrumental variable or more than one misclassified treatment variable, while, in Appendix F, I extend my analysis to a binary instrument and the LATE parameter, complementing the work done by Ura (2018) and Calvi et al. (2021). Moreover, in Appendix G, I provide details on how to connect the bounds around the MTE function to bounds around common treatment effect parameters. In Appendix H, I provide detailed information on the estimation method discussed in Section 4 and analyze the proposed estimators’ performance in a Monte Carlo exercise. Finally, Appendices I and J provide further details about my empirical application.

2 Econometric Framework

In this section, I assume that I do not observe the final ruling in each case and develop an econometric method that addresses endogenous selection-into-treatment and misclassification simultaneously. To analyze the Marginal Treatment Effect (MTE) when the treatment variable is mismeasured, I start with the standard generalized selection model (Heckman and Vytlacil, 1999), described in the potential outcome framework:

\begin{align}
Y &= Y_1 \cdot D + Y_0 \cdot (1 - D) \tag{1} \\
D &= 1 \{U \leq P_D(Z)\} \tag{2}
\end{align}
where $Z$ is an observable instrumental variable (trial judge’s leniency rate) with support given by a set $Z \subset \mathbb{R}$, $P_D: Z \to \mathbb{R}$ is an unknown function, $U$ is a latent heterogeneity variable (defendant’s resistance to treatment or the amount of criminal evidence in her favor) and $D$ is the correctly classified treatment status (indicator that the defendant received some type of punishment — non-prosecution agreement or conviction — in her case’s final ruling).\(^6\)

Equation (2) models how the agent self-selects into treatment and imposes monotonicity (Imbens and Angrist, 1994; Vytlacil, 2002). Variable $Y$ is the realized outcome variable (recidivism indicator), while $Y_0$ and $Y_1$ are the potential outcomes when the agent is untreated (not punished) and treated (punished), respectively.

I augment this model with the possibly misclassified treatment status indicator $T$ (trial judge’s sentence). Note that the binary nature of the treatment variable implies that the measurement error $(T - D)$ is non-classical, i.e., $Cov(T - D, D) < 0$. The misclassified treatment is relevant because the researcher observes only the vector $(Y, T, Z)$, while $Y_1$, $Y_0$, $D$ and $U$ are latent variables.\(^7\)

Following Heckman et al. (2006), I impose four assumptions.

**Assumption 1** The latent variables $(Y_0, Y_1, U)$ are independent of the instrument $Z$, i.e., $Z \perp \perp (Y_0, Y_1, U)$.

Assumption 1 is an exogeneity assumption and is common in the literature about instrumental variables. In my empirical application, this assumption holds conditional on the court district because, in the State of São Paulo, Brazil, trial judges are randomly assigned to cases within each court district. Moreover, this type of assumption is common in the judge fixed-effect literature (Bhuller et al., 2019; Agan et al., 2021; Norris et al., 2021), Education (Carneiro et al., 2011, 2017; Cornelissen et al., 2018), Development Economics (Dupas, 2014; \(^6\)My framework can be adapted to binary instruments. This case, that bounds the LATE parameter, is detailed in Appendix F and complements the work developed by Ura (2018) and Calvi et al. (2021).

\(^7\)For brevity, I drop exogenous covariates from the model even though all results derived in the paper hold conditionally on covariates. Moreover, I assume that there is only one instrument even though all results hold with partial derivatives when there is a vector of continuous instrumental variables. For an extended model with more than one instrument, see Appendix E. This appendix also discuss a model with more than one misclassified treatment variable. Such an extension is useful to empirical applications whose treatment variable is defined based on a prediction algorithm (Black et al., 2020; Arellano-Bover, 2020).
Mogstad et al., 2018), Family Economics (Black et al., 2005; Brinch et al., 2017) and many other applied fields.

**Assumption 2** The derivatives of function $P_D$ and of the mismeasured propensity score are always different from zero, i.e., $\frac{dP_D(z)}{dz} \neq 0$ and $\frac{dP_T(z)}{dz} \neq 0$ for every $z \in \mathcal{Z}$, where $P_T: \mathcal{Z} \rightarrow \mathbb{R}$ is defined as $P_T(z) = \mathbb{P}[T = 1|Z = z]$ for any $z \in \mathcal{Z}$.

Assumption 2 is a rank condition, intuitively imposing that the instrument is locally strong everywhere. Note also that Assumption 2 is stronger than the rank condition usually imposed in the literature about marginal treatment effects (Heckman et al., 2006, Assumption A-2). In particular, since the correctly measured treatment variable is not observed, it is impossible to directly test that $\frac{dP_D(z)}{dz} \neq 0$ for every $z \in \mathcal{Z}$. However, since the mismeasured propensity score is trivially identified, it is possible to test that $\frac{dP_T(z)}{dz} \neq 0$ for every $z \in \mathcal{Z}$. Observe also that Assumption 2 implies that $0 < \mathbb{P}[D = 1] < 1$, a support condition that is required for any evaluation estimator.

**Assumption 3** The distribution of the latent heterogeneity variable $U$ is absolutely continuous with respect to the Lebesgue measure.

Assumption 3 is a regularity condition that allows me to normalize the marginal distribution of $U$ to be the standard uniform. Consequently, I have that the true propensity score $\mathbb{P}[D = 1|Z = z]$ satisfies $P_D(z) = \mathbb{P}[D = 1|Z = z]$ for any $z \in \mathcal{Z}$. However, due to misclassification of the treatment variable, the correctly measured propensity score is not identified.

**Assumption 4** The potential outcome variables have finite first moments, i.e., $\mathbb{E}[|Y_0|] < \infty$ and $\mathbb{E}[|Y_1|] < \infty$.

Assumption 4 is a regularity condition that allows me to apply standard integration theorems and ensures that average treatment effects are well-defined.

Due to misclassification of the treatment variable, Assumptions 1-4 are not sufficient to identify the marginal treatment effect function (Proposition 1). To address this problem, I propose a layered policy analysis (Manski, 2011) and gradually impose three increasingly
strong assumptions that allow me to derive increasingly strong identification results in Section 3. Consequently, I offer a menu of estimates whose credibility can be assessed by each reader based on the plausibility of each assumption (Tamer, 2010). Intuitively, these assumptions restrict the amount of measurement error by constraining the relationship between $D$, $T$ and $Z$.

To derive my first partial identification result (Corollary 1), I require a weak sign restriction on the impact of $Z$ on the true treatment variable $D$ and on the misclassified treatment variable $T$. Under Assumption 5, it is possible to identify the sign of the marginal treatment effect.

**Assumption 5** The derivative of the correctly measured propensity score has the same sign as the derivative of the mismeasured propensity score, i.e., $\text{sign} \left( \frac{dP_D(Z)}{dz} \right) = \text{sign} \left( \frac{dP_T(Z)}{dz} \right)$ for every $z \in Z$.

In my empirical application, Assumption 5 imposes that tougher trial judges also increase the probability of receiving some type of punishment according to each case’s final ruling. Intuitively, this assumption holds if tougher trial judges write more compelling rulings that are more likely to be affirmed by the Appeals Court. Alternatively, if only a small share of defendants appeal, this assumption may hold even if tougher trial judges’ rulings are more likely to be reversed. Moreover, as discussed in Example 1, this assumption holds if trial judges and Appeals judges have the same sentencing criteria, but face different information sets.

**Example 1 (Collecting New Evidence in a Criminal Case)** Let $U_T$ be the amount of evidence in favor of the defendant when the trial judge is making her decision and $U$ be the amount of evidence in favor of the defendant in the Appeals Court. I assume that either the district attorney or the defendant’s lawyers can collect new evidence between the trial judge’s decision and the Appeals judge’s analysis, and I define $V := U - U_T$ as the new evidence that was collected. Moreover, I assume that the trial judge and the Appeals judge are equally strict.

---

8 Sign restrictions have been used previously in the misclassification literature to identify the treatment effect parameter in a linear model with homogeneous treatment effects (Haider and Stephens, 2020, Assumption 5).

9 For another example about investment decisions, see Appendix D.1. Moreover, in Appendix D.4, I impose restrictions on the model’s primitives that imply Assumptions 5-7.
i.e., their sentences only differ because of the new evidence.\footnote{Assuming that two judges are equally strict is plausible. For example, consider the case when the defendant is arrested before the trial and is sent to prison while awaiting trial. In Brazil, if a lawyer petitions the court for a writ of 	extit{habeas corpus} multiple times and brings new evidence each time, the same trial judge will analyze each petition.} Mathematically, I write that

\[ D \equiv 1 \{ U \leq P_D (Z) \} \]

and

\[ T \equiv 1 \{ U_T \leq P_D (Z) \} = 1 \{ U - V \leq P_D (Z) \} . \]

Assuming that \( Z \independent (U_T, U) \) and denoting \( F_{U_T} \) as the distribution function of \( U_T \) and \( f_{U_T} \) as the density function of \( U_T \), I can show that, for any \( z \in Z \),

\[ P_T (z) := P [ T = 1 \mid Z = z ] = F_{U_T} (P_D (z)) , \]

implying, by the chain rule, that

\[ \frac{dP_T (z)}{dz} = f_{U_T} (P_D (z)) \cdot \frac{dP_D (z)}{dz} . \]

Consequently, Assumption 5 holds because a density function is always positive.

To derive a stronger partial identification result (Proposition 2), I impose not only that the impact of \( Z \) on \( D \) and \( T \) have the same sign, but also that those impacts are not arbitrarily different from each other. Under Assumption 6, it is possible to uniformly bound the marginal treatment effect function.

\textbf{Assumption 6} The ratio between the derivatives of the correctly measured propensity score and of the mismeasured propensity score is bounded, i.e., there exists a known \( c \in [1, +\infty) \) such that

\[ \frac{dP_D (z)/dz}{dP_T (z)/dz} \in \left[ \frac{1}{c}, c \right] \text{ for any value } z \in Z. \]

Note that using a smaller \( c \) imposes a stronger restriction on the relationship between \( D \), \( T \) and \( Z \).\footnote{Note that imposing that \( c \) is a constant instead of assuming that \( c \) is a nontrivial function of the instrument is a restriction that is overly strong in practice. In practice, the researcher may be uncertain about the value of \( c \). After deriving...}
Proposition 2, I explain how to choose the largest value of \( c \) that is compatible with the data and the other model assumptions. Alternatively, the researcher can follow a sensitivity analysis strategy (Cinelli and Hazlett, 2019) and present results for different values of \( c \).\(^{12}\)

In my empirical application, Assumption 6 imposes that decreasing the trial judge’s leniency increases the punishment probabilities according to the trial judge’s ruling and according to the final ruling by similar amounts. As explained in Section 5.2, the minimum valid \( c \) in my empirical application is estimated to be equal to 1.02.

To illustrate the theoretical plausibility of my framework in my empirical application, Example 2 describes a simple model of Appeals Courts reversing trial judges’ rulings.\(^{13}\)

**Example 2 (Reversing Trial Judges’ Rulings)** Let \( Z \) be the punishment rate of the trial judge with \( Z \in [0, 1] \). Let \( D \) be the final ruling in each case and \( T \) be the trial judge’s ruling. The trial judge’s ruling is reversed if \( V > P_{NR}(z) \), where \( P_{NR} : [0, 1] \to [0, 1] \) captures the quality of the trial judge’s ruling and is continuously differentiable with \( P_{NR}(z) > \frac{1}{2} + \epsilon \) and \( \frac{dP_{NR}(z)}{dz} > 0 + \epsilon \) for some \( \epsilon \in (0, 1/3) \) and for any \( z \in [0, 1] \), and \( V \sim \text{Uniform}[0, 1] \) captures the minimum trial judge’s ruling’s quality that Appeals Court finds acceptable with \( V \perp (Z, D) \). Mathematically, we have that

\[
T = 1 \{ V \leq P_{NR}(z) \} \cdot D + 1 \{ V > P_{NR}(z) \} \cdot (1 - D) .
\]

Moreover, define \( P_{D} : [0, 1] \to [0, 1] \) such that \( P_{D}(z) = \mathbb{P}[D = 1 | Z = z] \) for any \( z \in [0, 1] \) and assume that \( P_{D} \) is continuously differentiable with \( P_{D}(z) > \frac{1}{2} + \epsilon \) and \( \frac{dP_{D}(z)}{dz} > 0 + \epsilon \) for

\(^{12}\)If the researcher observes the correctly classified and the misclassified treatment variable for a subsample of her sample, she can use this subsample to estimate \( c \) and use the estimated \( c \) in Assumption 6 to partially identify the MTE function in her entire sample. Without this special subsample, Acerenza et al. (2022, Section 4) shows that Assumption 6 holds and that the constant \( c \) can be estimated from the data if the researcher assumes that the misclassification decision is independent from the instrument and from the latent heterogeneity variable.

\(^{13}\)To illustrate the broad applicability of my theoretical framework, Appendix D.2 explains that Assumption 6 may also be plausible when analyzing returns to education if having a college degree is randomly miscoded in a survey.
any \( z \in [0, 1] \).

Note that, for any \( z \in [0, 1] \)

\[
P_T(z) := \mathbb{P}[T = 1|Z = z]
\]

\[
= \mathbb{P}[T = 1|Z = z, D = 1] \cdot \mathbb{P}[D = 1|Z = z] + \mathbb{P}[T = 1|Z = z, D = 0] \cdot \mathbb{P}[D = 0|Z = z]
\]

\[
= \mathbb{P}[V \leq P_{NR}(z)|Z = z, D = 1] \cdot P_D(z) + \mathbb{P}[V > P_{NR}(z)|Z = z, D = 0] \cdot (1 - P_D(z))
\]

\[
= P_{NR}(z) \cdot P_D(z) + (1 - P_{NR}(z)) \cdot (1 - P_D(z)),
\]

implying that

\[
\frac{dP_T(z)}{dz} = (-1 + 2 \cdot P_{NR}(z)) \cdot \frac{dP_D(z)}{dz} + (-1 + 2 \cdot P_D(z)) \cdot \frac{dP_{NR}(z)}{dz}.
\]

Observe that, given the functional form assumptions made in this example, \( \frac{dP_T(z)}{dz} > 4 \cdot \epsilon^2 > 0 \). Consequently, we can define \( g : [0, 1] \to \mathbb{R}_{++} \) such that, for any \( z \in [0, 1] \),

\[
g(z) = \frac{dP_D(z)/dz}{dP_T(z)/dz} = \frac{1}{-1 + 2 \cdot P_{NR}(z)} - \left(\frac{-1 + 2 \cdot P_D(z)}{-1 + 2 \cdot P_{NR}(z)}\right) \cdot \frac{dP_{NR}(z)/dz}{dP_T(z)/dz}.
\]

Observe that, given the functional form assumptions made in this example, \( g \) is positive and continuous. Since \( Z = [0, 1] \) is compact, \( g \) attains its maximum and minimum, implying that there exists a constant \( c \in [1, +\infty) \) such that Assumptions 6 holds.

To derive my strongest partial identification results (Corollary 2, Proposition 3 and Corollary 3), I impose a restriction on the functional relationship between the correctly measured propensity score and the mismeasured propensity score in the sense that it connects the level and all the derivatives of those two objects. Under Assumption 7, it is possible to derive sharp uniform bounds around the MTE function and to sharply bound a variety of treatment effect parameters that can be written as a weighted integral of the MTE function.\(^{14}\)

Assumption 7 For a known \( c \in [1, +\infty) \), a known set \( \mathcal{A} \subseteq \mathcal{G} \times \mathbb{R} \) and an unknown pair

\(^{14}\)My definition of sharpness imposes that I can recover a derivative of the data distribution instead of the level of the data distribution. For a more detailed discussion, see Remark 1.
\((\alpha, b) \in A\), the function \(P_D\) is invertible and \(P_D(z) = \int_{-\infty}^{z} \alpha(\tilde{z}) \cdot \frac{dP_T(\tilde{z})}{dz} d\tilde{z} + b\), where

\[
\mathcal{G} := \left\{ g: \mathbb{Z} \to \left[\frac{1}{c}, c\right] \right\}.
\]

If \(A = \mathcal{G} \times \mathbb{R}\), then Assumption 7 only adds an invertibility condition to Assumption 6. An easy-to-interpret version of Assumption 7 imposes that the mismeasured propensity score is a rescaled version of the true propensity score by defining \(A := \{ g \in \mathcal{G} \text{ and } g \text{ is constant.} \} \times \{0\}\).\(^{15}\) Once more, using a smaller \(c\) imposes a stronger restriction on the relationship between \(D\), \(T\) and \(Z\).

In my empirical application, I impose that the mismeasured propensity score is a rescaled version of the true propensity score. As explained in Section 5.2, the estimated bounds around the ATE, the ATT and the ATU based on this assumption contain the estimated treatment effect parameters based on the final ruling of each case.

To illustrate the broad applicability of my theoretical framework, Example 3 explains that Assumption 7 may also be plausible when analyzing welfare benefits if misclassification is unidirectional.\(^{16}\)

**Example 3 (Misreporting due to Stigma)** Hernandez and Pudney (2007) documents that welfare participation is measured with error. I am interested in evaluating the marginal treatment effect of welfare participation on children’s health outcomes. Since people are likely to lie about welfare participation because of social stigma, I assume that the measurement error is unidirectional, i.e., the person will truthfully report no participation \((T = 0)\) if they do not receive welfare benefits \((D = 0)\), but the person may report participation \((T = 1)\) or no participation \((T = 0)\) if they receive welfare benefits \((D = 1)\). Formally, I assume that

\[
T = 1 \{ V \leq 0.95 \} \cdot D,
\]

where \(V \sim Uniform[0,1]\) captures how much the individual cares about others’ opinions and

\(^{15}\)While Assumption 7 in its general form can be seen as a high-level condition that is weaker than the restrictions imposed by Acerenza et al. (2022, Section 4), its more intuitive version is more restrictive than the method proposed by these authors.

\(^{16}\)For two other examples in a dynamic setting, see Appendix D.3. Moreover, in Appendix D.4, I impose restrictions on the model’s primitives that imply Assumption 7.
\( V \perp (Z, D) \). In this case, I have that, for any \( z \in \mathcal{Z} \), \( P_T(z) = 0.95 \cdot P_D(z) \), implying that, if \( P_T \) is invertible, then Assumption 7 holds for \( c = \frac{20}{19} \), \( b = 0 \) and \( \alpha(z) = c \) for any \( z \in \mathcal{Z} \). Moreover, if \( P_T \) is not invertible, then Assumption 7 cannot hold in this context.

3 Partial Identification of the MTE with a Misclassified Treatment

My goal is to derive partial identification results for the Marginal Treatment Effect as a function of the value of the instrument (MTE function). Formally, I define this object as \( \theta: \mathcal{Z} \rightarrow \mathbb{R} \) such that, for any \( z \in \mathcal{Z} \),

\[
\theta(z) = \mathbb{E}[Y_1 - Y_0 | U = P_D(z)].
\]

Intuitively, this definition of the MTE function captures the effect of a treatment for the individual who is indifferent between taking the treatment or not, where the margin of indifference is defined by the value of the individual’s instrument.

In my empirical application, the MTE function captures the effect of being punished with an alternative sentence on future criminal behavior for the defendant who is at the margin of being found guilty given her judge’s leniency levels. Analyzing the MTE function at different margins of judge’s leniency is important because being punished may harm individuals who would be punished even by very lenient judges, but may benefit individuals who would be punished only by very strict judges. Consequently, understanding the heterogeneity of the impact of alternative sentences is key to understand its benefits and costs.

Note that, while Heckman and Vytlacil (2005) define the MTE as a function of the latent heterogeneity \( U \), I define the MTE as a function of the instrument.\(^{17}\) Consequently, different instrumental variables are associated with different MTE functions. Although the function \( \theta(\cdot) \) is not policy-invariant according to the definition of Heckman and Vytlacil (2005), I can still use it to compute interesting policy relevant treatment effect parameters (PRTE).

\(^{17}\)In my work, I do not use the standard definition of the MTE function and its classic identification result \( \left( \mathbb{E}[Y_1 - Y_0 | U = p] = \frac{d\mathbb{E}[Y | P_D(Z) = p]}{dp} \right) \) because I cannot point-identify \( P_D \) due to misclassification of the treatment variable.
For example, in my empirical application, I can still compute the treatment effect of making all judges as strict or as lenient as the strictest or most lenient judges. Moreover, under Assumption 7, there is a one-to-one map between my definition of the MTE function and its standard definition, allowing me to easily derive bounds around common treatment effect parameters (Corollary 3).

To partially identify the MTE function, I analyze the consequences of a misclassified treatment variable on the Local Instrumental Variable (LIV) estimand and, then, derive increasingly strong identification results based on Assumptions 5-7. Analyzing the misclassification bias of the LIV estimand is important, because this estimand is traditionally used to identify the MTE function in the previous literature.

If the researcher ignores that the treatment variable is misclassified, she can compute the LIV estimand using the misclassified treatment variable $T$ as if it was the actual treatment variable. In this case, the LIV estimand is defined as $f: \mathcal{Z} \to \mathbb{R}$ such that, for any $z \in \mathcal{Z}$,

$$ f(z) = \frac{dE[Y|Z=z]}{dz} \cdot \frac{dE[T|Z=z]}{dz} $$

(3)

following Chalak (2017) and as $\tilde{f}: \mathcal{P} \to \mathbb{R}$ such that, for any $p$ in the support $\mathcal{P}$ of $P_T(Z)$,

$$ \tilde{f}(p) = \frac{dE[Y|P_T(Z)=p]}{dp} $$

(4)

following Heckman et al. (2006). The next proposition analyzes which object is identified by both definitions of the LIV estimand and clarifies two negative consequences of ignoring misclassification.

**Proposition 1 (LIV Estimand)** Under Assumptions 1-4, the LIV estimand $f$ satisfies

$$ f(z) = \frac{dP_D(z)/dz}{dP_T(z)/dz} \cdot \theta(z) $$

(5)

for any $z \in \mathcal{Z}$.

Moreover, if Assumptions 1-4 hold and the function $P_T$ is invertible, then the LIV estimand
\[ \hat{f} \text{ satisfies} \]
\[ \frac{d\mathbb{E}[Y \mid P_T(Z) = p]}{dp} = \frac{dP_T (P_T^{-1}(p))/dz}{dP_Y (P_Y^{-1}(p))/dz} \cdot \theta (P_T^{-1}(p)) \]  

for any \( p \) in the support of \( P_T(Z) \).

**Proof.** The proof of this proposition follows Heckman et al. (2006) and is detailed in Appendix A.1.

The first negative consequence shown by Proposition 1 is that, when misclassification is ignored, two different definitions for the LIV estimand do not identify the MTE function. Similarly to the comparison between the LATE and the Wald Estimator when the treatment variable is misclassified (Calvi et al., 2021; Tommasi and Zhang, 2020), there is a scaling factor connecting the LIV estimand and the MTE function. Differently from the work done by Calvi et al. (2021) and Tommasi and Zhang (2020), this scaling factor is a function, suggesting that the bias may be positive, negative or even zero depending on the point where the LIV estimand is evaluated. As illustrated by my Monte Carlo exercise (Subsection H.4) and by my empirical application (Section 5.2), this phenomenon complicates an intuitive analyzes of the misclassification bias where the researcher tries to guess its direction based on expert knowledge.

The second negative consequence shown by Proposition 1 is that IV validity tests may fail if misclassification is ignored. For example, when the outcome variable has a compact support, Frandsen et al. (2019, Section 3) proposes to test the monotonicity condition (Equation (2)) by testing whether the function \( d\mathbb{E}[Y \mid P_T(Z) = p] \) is bounded between the smallest and the largest possible treatment effects. Proposition 1 shows that this test is not valid when the treatment variable is mismeasured because the scaling factor \( \frac{dP_T (P_T^{-1}(p))/dz}{dP_Y (P_Y^{-1}(p))/dz} \) in Equation (6) is possibly unbounded without extra assumptions. A misclassified treatment variable also renders the monotonicity test proposed by Heckman and Vytlacil (2005, Theorem 1) invalid as detailed in Appendix B. Moreover, the mismeasured propensity score may not satisfy index sufficiency as explained in Appendix C.

Furthermore, ignoring misclassification hardens the interpretation of the usual IV estimand. Following Heckman et al. (2006, Section III.B), I can show that the naive IV estimand
satisfies

\[ \frac{\text{Cov}(Z,Y)}{\text{Cov}(Z,T)} = \int_0^1 \omega(u) \cdot \mathbb{E}[Y_1 - Y_0 | U = u] \, du, \]

where \( \omega(u) = \frac{\mathbb{E}[Z - E[Z] | P_D(Z) \geq u] \cdot \mathbb{P}[P_D(Z) \geq u]}{\text{Cov}(Z,T)}. \) Unless \( \text{Cov}(Z,T) = \text{Cov}(Z,D), \) the weights \( \omega(\cdot) \) do not integrate to one and the IV estimand do not identify a proper weighted average of the MTE even when the weights are positive. Moreover, since \( \text{Cov}(Z,T) = \text{Cov}(Z,D) \) is equivalent to \( \text{Cov}(Z,T - D) = 0, \) this condition intuitively imposes a testable restriction in my empirical application: sentences by extreme trial judges are as likely to be reversed as sentences by median trial judges. Since I find that stricter trial judges are more likely to have their sentences reversed by the Appeals Court (Subsection 5.1), the naive IV estimand will have weighting problems in my empirical application.

Now, to derive increasingly strong identification results for \( \theta(\cdot), \) I add Assumptions 5, 6 and 7. The first identification result (Corollary 1) shows that I can identify the sign of the MTE function under a weak assumption about the signs of the derivatives of the correctly measured propensity score function and of the mismeasured propensity score function.

**Corollary 1 (Identifying the sign of the MTE function)** Under Assumptions 1-4 and 5, the sign of \( \theta(z) \) is identified for any \( z \in Z. \)

**Proof.** In Equation (5), the scaling function that multiplies the MTE function \( \theta(\cdot) \) is strictly positive under Assumptions 2 and 5. Consequently, I have that \( \text{sign}(\theta(z)) = \text{sign}(f(z)) \) for any \( z \in Z. \)

Knowing the sign of the MTE function \( \theta(z) \) at a point \( z \in Z \) is important. If the instrument is policy-relevant, this result can be used to ensure that the benefit of every treated person is positive. For example, in my empirical application, the policy maker can re-educate judges whose punishment rates are related to a positive effect on recidivism to change their punishment criteria to points \( \theta(z) \) that are associated with a negative effect. Even when the instrument is not policy-relevant, knowing whether the MTE function \( \theta(\cdot) \) is mostly positive or negative is useful to evaluate the pros and cons of a treatment.

The second identification result (Proposition 2) shows that, under an assumption about the
ratio between the derivatives of the correctly measured propensity score function and of the
mismeasured propensity score function, I can uniformly bound the MTE function. Moreover,
the distance between the true MTE function and any function in this set is bounded above
by an identifiable constant under Assumptions 1-4 and 6.

**Proposition 2 (Uniform Outer Set for the MTE function)** Suppose Assumptions 1-4
and 6 hold. I have that \( \theta \in \Theta_1 \), where

\[
\Theta_1 := \left\{ \tilde{\theta} : \mathcal{Z} \to \mathbb{R} \mid \text{For any } z \in \mathcal{Z}, |\tilde{\theta}(z)| \in \left[ \frac{1}{c} \cdot |f(z)|, c \cdot |f(z)| \right] \right\}.
\]

Moreover, for any \( \tilde{\theta} \in \Theta_1 \) and \( d \in (0, +\infty] \), \( \|\theta - \tilde{\theta}\|_d \leq \left( \frac{c^2 - 1}{c} \right) \cdot ||f||_d \), where \( ||\cdot||_d \) is
the \( L_d \)-norm if \( d < \infty \) and \( ||\cdot||_\infty \) is the sup-norm if \( d = \infty \).

**Proof.** The proof of the first part of Proposition 2 uses Equation (5) and the bounds imposed
on the ratio \( \frac{dP_0(z)/dz}{dP_T(z)/dz} \) by Assumption 6 to uniformly bound the MTE function, while the proof
of the second part of Proposition 2 uses the definition of the norm \( ||\cdot||_d \). The details are in
Appendix A.2.

Proposition 2 is strictly stronger than Corollary 1 in the sense that not only it identifies
the sign of \( \theta(z) \) at any point \( z \in \mathcal{Z} \), but it also provides the largest possible effect and the
smallest possible effect for each value of the instrument. If the instrument is policy-relevant,
this result can be used to ensure that the benefit of every treated person is larger than the
cost of treatment. For example, in my empirical application, the policy maker can re-educate
judges whose punishment rates are related to effects that are not large enough to compensate
for punishment costs to change their punishment criteria to points associated with effects that
pass a cost-benefit analysis. Even when the instrument is not policy-relevant, bounding the
MTE function \( \theta(\cdot) \) is useful to know whether most points pass a cost-benefit analysis.

Due to Proposition 2, I can also adapt the test proposed by Frandsen et al. (2019) to test
Assumptions 1-4 and 6 when the support of \( Y_0 \) and \( Y_1 \) is bounded. To do so, I check whether
\( \sup_{\tilde{\theta} \in \Theta_1} \sup_{z \in \mathcal{Z}} \tilde{\theta}(z) \) is smaller than the largest possible effect and whether \( \inf_{\tilde{\theta} \in \Theta_1} \inf_{z \in \mathcal{Z}} \tilde{\theta}(z) \)
is larger than the smallest possible effect. If that is the case, I do not reject Assumptions 1-4

21
and 6. Note that this test can be used to define the largest non-falsified value of $c \in [1, +\infty)$. To implement it, find $\tau$ and $\tilde{z}$ that respectively maximizes and minimizes the function $f(\cdot)$ and, then, find the smallest $c$ that falsifies the model by comparing $\frac{1}{c} \cdot f(\tau)$, $c \cdot f(\tau)$, $\frac{1}{c} \cdot f(\tilde{z})$ or $c \cdot f(\tilde{z})$ against the largest and smallest possible effects.

The third identification result (Corollary 2) derives a stricter uniform bound for the MTE function under Assumption 7.

**Corollary 2 (Uniform Outer Set for the MTE Function with Assumption 7)** Suppose Assumptions 1-4 and 7 hold. I have that $\theta \in \Theta_2$, where

$$\Theta_2 := \left\{ \tilde{\theta} : \mathcal{Z} \to \mathbb{R} \mid \begin{array}{l}
\text{For some } (a, \tilde{b}) \in \mathcal{A} \text{ such that } \hat{P}_D := \mathcal{Z} \to [0, 1] \text{ is invertible, } P_D(z) = \int_{-\infty}^{z} a(\tilde{z}) \cdot \frac{dP_T(\tilde{z})}{dz} d\tilde{z} + \tilde{b}, \\
\text{and } \tilde{\theta}(z) = \frac{1}{a(z)} \cdot f(z) \text{ for any } z \in \mathcal{Z}.
\end{array} \right\}.$$ 

**Proof.** Under Assumption 7, there exists $(\alpha, b) \in \mathcal{A}$ such that $P_D(z) = \int_{-\infty}^{z} \alpha(\tilde{z}) \cdot \frac{dP_T(\tilde{z})}{dz} d\tilde{z} + b$. Consequently, Equation (5) implies that $\theta(z) = \frac{1}{\alpha(z)} \cdot f(z)$ for any $z \in \mathcal{Z}$. I can, then, conclude that $\theta \in \Theta_2$. 

Corollary 2 is stronger than Proposition 2 in the sense that it imposes more discipline in the acceptable MTE functions given the data restrictions. For example, Proposition 2 stills accepts MTE functions that are equal to $f(z)$ times a non-constant scaling factor, while those functions are ruled out in Corollary 2 when $\mathcal{A} := \{g \in \mathcal{G} \text{ and } g \text{ is constant.}\} \times \{0\}$. Importantly, if $f(z)$ has different signs for different values of $z$, the upper contour and the lower contour of the bounds in Proposition 2 are not included in the set $\Theta_2$ when $\mathcal{A} := \{g \in \mathcal{G} \text{ and } g \text{ is constant.}\} \times \{0\}$. As a consequence, treatments that pass a cost-benefit analysis under Proposition 2 may not pass the same analysis under Corollary 2.

Although outer sets — as the ones described in Proposition 2 and Corollary 2 — provide reliable information about the true value of the function $\theta(\cdot)$, it is theoretically interesting to investigate when an outer set exhaust all the information about $\theta(\cdot)$ based on the data and the
model restrictions. Following this line of inquiry, the fourth identification result (Proposition 3) shows that the uniform bound in Corollary 2 is sharp when the outcome variable is binary.

**Proposition 3 (Sharp Identified Set for the MTE Function)** Suppose Assumptions 1-4 and 7 hold. In addition, suppose that the outcome variable $Y$ is binary, $\sup_{\tilde{\theta} \in \Theta_2} \sup_{z \in Z} \tilde{\theta}(z) \leq 1$ and $\inf_{\tilde{\theta} \in \Theta_2} \inf_{z \in Z} \tilde{\theta}(z) \geq -1$. Then, $\Theta_2$ is the sharp identified set for $\theta$ in the sense that, for any $\tilde{\theta} \in \Theta_2$, there exist candidate random variables $(\tilde{U}, \tilde{Y}_0, \tilde{Y}_1, \tilde{D})$ and a function $P_{\tilde{D}}: Z \rightarrow [0, 1]$ such that

1. $\tilde{D}$ is monotonic with respect to $Z$ and its index is given by $P_{\tilde{D}}$, i.e.,

$$
\tilde{D} = 1\{\tilde{U} \leq P_{\tilde{D}}(Z)\}; \quad (7)
$$

2. $(Z, \tilde{U}, \tilde{Y}_0, \tilde{Y}_1)$ and $P_{\tilde{D}}$ achieve the candidate target parameter, i.e.,

$$
\tilde{\theta}(z) = \mathbb{E}\left[\tilde{Y}_1 - \tilde{Y}_0 \Big| \tilde{U} = P_{\tilde{D}}(z)\right] \quad (8)
$$

for any $z \in Z$;

3. $(Z, \tilde{U}, \tilde{Y}_0, \tilde{Y}_1)$ and $P_{\tilde{D}}$ satisfy the data restriction given by

$$
\frac{d\mathbb{E}[Y|Z = z]}{dz} = \frac{d\mathbb{E}[\tilde{Y}|Z = z]}{dz} \quad (9)
$$

for any $z \in Z$, where $\tilde{Y} = \tilde{Y}_1 \cdot \tilde{D} + \tilde{Y}_0 \cdot (1 - \tilde{D})$;

4. $\tilde{Y}_0$ and $\tilde{Y}_1$ satisfy the support condition given by

$$
\tilde{Y}_0 \subseteq \mathcal{Y}_0 = \{0, 1\} \text{ and } \tilde{Y}_1 \subseteq \mathcal{Y}_1 = \{0, 1\}, \quad (10)
$$

where $\tilde{Y}_0$, $\mathcal{Y}_0$, $\tilde{Y}_1$ and $\mathcal{Y}_1$ are the support of $\tilde{Y}_0$, $Y_0$, $\tilde{Y}_1$ and $Y_1$, respectively;

5. $(Z, \tilde{U}, \tilde{Y}_0, \tilde{Y}_1, \tilde{D})$ satisfy Assumptions 1-4 and Assumption 7.
Proof. To prove that the set $\Theta_2$ is the sharp identified set for $\theta$, I construct, for each $\tilde{\theta} \in \Theta_2$, the candidate random variables that satisfy the five restrictions of Proposition 3. The details of this proof are in Appendix A.3.

Remark 1 The definition of sharpness in Proposition 3 is weaker than the definition of sharpness proposed by Canay and Shaikh (2017) because it does not ensure that the data $(Z, \tilde{Y})$ generated by the candidate random variables $(\tilde{U}, \tilde{Y}_0, \tilde{Y}_1, \tilde{D})$ has the same distribution of the data $(Z, Y)$ that is generated by the true latent variables $(U, Y_0, Y_1, D)$. However, my definition still uses all the restrictions (Equation (9)) directly imposed by the data on the MTE function.

Remark 2 Assuming that the outcome variable $Y$ is binary is not restrictive. If $Y$ is a continuous variable, I can analyze the dummy variable $1\{Y \leq y\}$ for any $y \in \mathbb{R}$ and derive point-wise sharp bounds for the distributional marginal treatment effect function.

Corollary 2 and Proposition 3 are useful because they provide easy-to-derive bounds for any treatment effect parameter that can be written as a weighted integral of the marginal treatment effect. Corollary 3 summarizes this result. In Appendix G, I provide explicit formulas for the weights associated with the Average Treatment Effect (ATE), the Average Treatment Effect on the Treated (ATT), the Average Treatment Effect on the Untreated (ATU) and any Policy Relevant Treatment Effect (PRTE, Heckman and Vytlacil, 2001).

Corollary 3 (Bounds on Treatment Effects Parameters) Suppose Assumption 1-4 and 7 hold. Define a treatment effect parameter $TE_\omega$ as

$$TE_\omega := \int_0^1 \tilde{\theta} \left( P_D^{-1} (u) \right) \cdot \omega (u, P_D) \, du,$$

where $\omega : [0, 1] \times \mathbb{R} \rightarrow \mathbb{R}$ is a known weighting function.

Then, the treatment effect parameter $TE_\omega$ is bounded by

$$\inf_{(a, b, P_D, \tilde{\theta}) \in \mathcal{A}} \int_0^1 \tilde{\theta} \left( P_D^{-1} (u) \right) \cdot \omega (u, P_D) \, du \leq TE_\omega.$$
\[
\leq \sup_{(a, b, P_{\tilde{D}}, \tilde{\theta}) \in \tilde{A}} \int_{0}^{1} \tilde{\theta} \left(P_{\tilde{D}}^{-1}(u)\right) \cdot \omega(u, P_{\tilde{D}}) \, du;
\]

where \(\tilde{A} := \left\{ (a, \tilde{b}, P_{\tilde{D}}, \tilde{\theta}) \in A \times \{g : \mathcal{Z} \to [0, 1]\} \times \{g : \mathcal{Z} \to \mathbb{R}\} \text{ such that } P_{\tilde{D}} \text{ is invertible}, \right. \]

\[
P_{\tilde{D}}(z) = \int_{-\infty}^{z} a(\tilde{z}) \cdot \frac{dP_{\tilde{T}}(\tilde{z})}{dz} \, d\tilde{z} + \tilde{b} \text{ and } \tilde{\theta}(z) = \frac{1}{a(z)} \cdot f(z) \text{ for any } z \in \mathcal{Z} \left. \right\}.
\]

If, additionally, the outcome variable \(Y\) is binary, \(\sup_{\tilde{\theta} \in \tilde{\Theta}} \tilde{\theta}(z) \leq 1\) and \(\inf_{\tilde{\theta} \in \tilde{\Theta}} \inf_{z \in \mathcal{Z}} \theta(z) \geq -1\), then the above bounds are sharp in the sense defined in Proposition 3.

**Proof.** Note that, under Assumption 7, \(\theta \left(P_{\tilde{D}}^{-1}(u)\right) = \mathbb{E}[Y_1 - Y_0|U = u]\), implying that Corollary 3 is a direct consequence of Corollary 2 and Proposition 3.

Corollary 3 illustrates the unifying force of the marginal treatment effect: identification results about the MTE function can be extended easily to many interesting treatment effects. Consequently, bounding the MTE function offers a flexible and off-the-shelf tool to answer many research questions that rely on other treatment effect parameters. For numeric convenience, I also rewrite Corollary 3 imposing that \(A := \{g \in \mathcal{G} \text{ and } g \text{ is constant.}\} \times \{0\}\) in Appendix G.

Moreover, when interest lies exclusively on an uni-dimensional treatment effect parameter (e.g., the ATE or some PRTE), Corollary 3 can be used to implement the breakdown analysis proposed by Kline and Santos (2013), Masten and Poirier (2018) and Masten and Poirier (2019). To do so, it is necessary that some desired conclusion (e.g., ATE is positive) holds when \(c = 1\). Then, I increase the value of \(c \in [1, +\infty)\) in Assumption 7 until the desired conclusion ceases to hold for some values in the identified set of the parameter of interest. This value of \(c\) that starts breaking down the desired conclusion is known as the breakdown point.

### 4 Estimation

In this section, I briefly explain how to estimate the MTE function’s sign (Corollary 1) and its bounds (Proposition 2). To estimate these objects, I need to estimate two objects: the mismeasured propensity score and the outcome equation’s reduced-form model. Importantly,
in my empirical application, these objects depend not only on the value \( z \) of the instrument but also on the value \( x \) of the covariates. These extra variables contain a full set of court district dummies and are included because, in São Paulo, trial judges are randomly allocated to criminal cases only after conditioning on the court district.

To estimate the mismeasured propensity score, I treat it as a purely reduced-form object. Consequently, I model the misclassified treatment variable’s conditional expectation as a separable function between the instrument and the covariates, depending on a polynomial of the instrument and a full set of court district dummies. Under these parametric assumptions, this model can be estimated by OLS.

To estimate the outcome equation’s reduced-form model, I treat it as an object derived from the economic model’s primitives. Specifically, the correctly measured propensity score function and the conditional expectation of the treatment effect as a function of the latent resistance to treatment are separable between the instrument and the covariates, depending on a polynomial of the instrument and a full set of court district dummies. Consequently, the outcome equation’s reduced-form model is a polynomial function that depends on the interaction between the instrument and the court district dummies. Under these parametric assumptions, this model can be estimated by OLS.

By combining the parameters from these two OLS regressions, I can estimate the mismeasured LIV estimand (Equation (3)) and use it to estimate the MTE function’s sign and bounds.

Moreover, in my empirical application, I also need to estimate the correctly measured MTE function to use it as a benchmark against the analysis that ignores misclassification. To do so, I need to estimate the LIV estimand that uses the correctly measured propensity score in its denominator. This object’s estimator is very similar to the mismeasured LIV estimand’s estimator. The only difference is that I now use a parametric polynomial approximation for the conditional expectation of the correctly classified treatment variable.

To have a deeper understanding of the estimation methods and their performance in a Monte Carlo exercise, see Appendix II.
5 Empirical Application

In my empirical application, I answer the question: “Do alternative sentences impact recidivism?” To answer this question, I collect data from all criminal cases brought to the Justice Court System in the State of São Paulo, Brazil, from 2010 to 2019. In Subsection 5.1, I briefly explain my dataset and provide descriptive statistics. For a detailed explanation on how I constructed my dataset, see Appendix I. Finally, in Subsection 5.2, I describe the results of my empirical analysis.

5.1 Data and Descriptive Statistics

I collect data from all criminal cases brought to the Justice Court System in the State of São Paulo, Brazil, between January 4th, 2010, and December 3rd, 2019. I restricted my sample to cases that started between 2010 and 2017, because the last two years are used only to define my outcome variable. Moreover, I focus on the criminal cases whose maximum prison sentence is less than 4 years because, according to Brazilian Law, these cases must be punished with alternative sentences. Due to this sample restriction, the most common crime types in my sample are theft and domestic violence. After those two restrictions, my dataset contain 51,731 case-defendant pairs.

In my dataset, I observe the defendant’s full name, the defendant’s court district, the case’s starting date, the assigned trial judge’s full name, the trial judge’s full sentence, the trial judge’s sentence’s date, whether the case went to the Appeals Court, the Appeals’ Court’s ruling if there is one, and the Appeals’ Court’s ruling’s date if there is one. Based on those variables, I define my outcome variable \((Y = \text{“recidivism within 2 years of the final sentence”})\), my misclassified treatment variable \((T = \text{“trial judge’s decision”})\), my correctly classified treatment variable \((D = \text{“final ruling”})\), my instrument \((Z = \text{“trial judge’s leniency rate”})\) and my covariates \((X = \text{“full set of court district dummies”})\).

My misclassified treatment variable \(T\) is based only on trial judge’s sentences and divides them into two groups. The first group (treated) receives a punishment, i.e., its defendants were fined or sentenced to community services because they were either convicted or signed
a non-prosecution agreement. The second group (control) did not receive a punishment, i.e., its defendants were acquitted or its cases were dismissed.

To define this variable, I proceed in three stages. First, I manually classify 325 sentences between the two aforementioned groups. Then, I use 216 of those sentences to train a L1-Regularized Logistic Regression that uses a pre-selected bag of informative words to predict which defendants were punished or not.\footnote{This pre-selected bag of informative words contain expressions such as “found guilty”, “acquitted” and specific articles in the Brazilian Criminal Code. For more information, see Appendix I.} When I use the remaining 109 sentences to validate my prediction algorithm, I find that this algorithm classified 98.8% of those sentences correctly. Finally, I use this trained prediction tool to classify all trial judge’s sentences in my dataset.

My correctly classified treatment variable $D$ also divides the case-defendant pairs into the groups “punished” and “not punished”. However, it considers the final ruling in each case. If the case did not go to the Appeals Court, this variable is equal to the misclassified treatment variable $T$. However, if the case went to the Appeals Court, this variable is possibly different from $T$ because it also considers the Appeals Court’s decision. If the trial sentence was affirmed, then $D$ is equal to $T$. However, if the trial sentence was reversed, then $D$ is equal to $1 - T$.

To identify which trial sentences were affirmed or reversed by the Appeals Court, I proceed in three steps. First, I manually classify 500 sentences from the Appeals Court. Then, I use 300 hundred of those sentences to train a L1-Regularized Logistic Regression that uses a pre-selected bag of informative words to predict which trial sentences were affirmed or reversed.\footnote{This pre-selected bag of informative words contain expressions such as “affirmed” and “reversed”. For more information, see Appendix I.} When I use the remaining 200 sentences to validate my prediction algorithm, I find that this algorithm classified 96.2% of those sentences correctly. Finally, I use this trained prediction tool to classify all Appeals Court’s decisions in my dataset.

Although my prediction algorithms may make classification errors, I ignore this source of misclassification in my empirical analysis. Consequently, I focus exclusively on the misclassification problems generated by the appeals process. This source of misclassification is arguably more interesting because it is embedded in the economic nature of the problem instead of
being mechanically created by a prediction algorithm.\textsuperscript{20}

Table 1 shows the joint distribution of the correctly treatment variable $D$ ("Punishment according to the Final Ruling") and the misclassified treatment variable $T$ ("Punishment according to the Trial Judge’s Ruling"). Since most cases (67.3\%) do not go the Appeals Court, most cases are correctly classified (95.6\%) as described in the main diagonal. The other two cells describe the cases that are misclassified when I ignore the Appeals Court’s decisions. First, I find that 3.5\% of the defendants were punished by the Trial Judge and were able to reverse their sentences in the Appeals Court. Moreover, in 0.9\% of the cases, the defendant was not punished by the Trial Judge, but the prosecutor was able to appeal and reverse the decision.

Table 1: Joint Distribution: Trial Judge’s Ruling v. Final Ruling

|                | Final Ruling |
|----------------|-------------|
|                | Not Punished | Punished   |
| Trial Judge’s Ruling | 45.8\%       | 0.9\%      |
| Punished        | 3.5\%        | 49.8\%     |

Note: Most cases (67.3\%) do not go the Appeals Courts. In those cases, the trial judge’s ruling and the final ruling must be equal by construction.

My instrument $Z$ is the trial judge’s leniency rate. This variable is equal to the leave-one-out rate of punishment for each trial judge, where the defendant’s own decision is excluded from this average. To do so, I only use the 639 judges who analyzed more than 20 cases during my sample period.

Subfigure 1a shows the histogram of the number of case-defendant pairs analyzed by each judge. Note that it is not uncommon to find judges who analyzed more than 100 cases. For this reason, I treat the instrument $Z$ as the correctly measured leniency of the trial judges, ignoring any estimation error from this stage in my estimation and inference methods.

Subfigure 1b shows the histogram and the smoothed density of the trial judge’s leniency

\textsuperscript{20}Alternatively, I could focus on the misclassification error generated by the prediction algorithms. The method proposed in this paper partially identifies the $MTE$ function even if the final sentence is possibly misclassified. The drawback of this approach is the impossibility of estimating the misclassification bias because I would not observe the correctly measured final sentence if I took prediction errors into account. If I were to follow this approach, I could define one misclassified treatment variable for each prediction algorithm (e.g., random forest or logistic LASSO) and use the methods proposed in Appendix E.2 for the case with more than one misclassified treatment variable.
rate. Observe that there are judges who are very lenient, punishing less than 25% of their cases, and judges who are very strict, punishing more than 75% of their cases. This large dispersion of leniency rates is useful for my empirical strategy because it strengthens my instrument.⁴¹

Having described the treatment and instrumental variables, I can now discuss their relationship. Figure 2 shows three conditional probabilities where the conditioning variable is the instrument \( Z \). The orange line is the share of defendants who were initially found not guilty by the trial judge and had their sentences reversed by the Appeals Court conditional on the punishment rate of the trial judge. The dark blue line is the share of defendants who were initially found guilty by the trial judge and had their sentences reversed by the Appeals Court conditional on the punishment rate of the trial judge. Finally, the light blue line is the share of defendants who had their sentences reversed by the Appeals Court conditional on the punishment rate of the trial judge. The dotted lines are robust bias corrected 95%-confidence intervals (Calonico et al., 2019).

Figure 2 illustrates the importance of having more than one alternative set of assump-

---

⁴¹When I semiparametric estimate the propensity score functions (Appendix J.4), I find that the correctly measured propensity score and the mismeasured propensity score are nontrivial functions of the instrument. Their steep inclines also suggest that my instrument is strong.
I, now, describe how I define my outcome variable ($Y$ = “recidivism within 2 years of the final sentence”). A defendant $i$ in a case $j$ recidivated ($Y_{ij} = 1$) if and only if defendant $i$’s full name appears in a case $\tilde{j}$ whose starting date is within 2 years after case $j$’s final sentence’s date. Importantly, case $\tilde{j}$ can be about any type of crime, including more severe crimes whose maximum sentence is greater than four years, while case $j$ has to about a crime whose maximum sentence is at most 4 years. To match defendants’ names across cases, I use the Jaro–Winkler similarity metric (Winkler, 1990) and I define a match if the similarity
between full names in two different cases is greater than or equal to 0.95.\textsuperscript{22}

Even though this fuzzy matching algorithm may misclassify the outcome variable of some case-defendant pairs, I assume that this type of error is negligible and do not account for it in my empirical analysis. This assumption is plausible because Brazilian names are frequently long, containing four or more words. For instance, in my dataset, 59.4\% of the case-defendant pairs have names with four or more words, and only 6.7\% of them have names with only two words.

Table 2 shows the recidivism rate by punishment group. Overall, I find that 33.6\% of the case-defendant pairs in my sample recidivated within two years of the final sentence in their cases. Conditioning on treatment status, approximately 36\% of the punished defendants and 31\% of the non-punished defendants recidivate. Consequently, the comparison between these two groups suggests that receiving an alternative sentence increases the probability of committing crimes in the future if I ignore endogenous selection-into-treatment. As discussed in Subsection 5.2, this naive conclusion is not supported by an analysis that takes endogeneity into account.

Table 2: Recidivism Rate by Punishment Group

\begin{center}
\begin{tabular}{|c|c|c|c|c|}
\hline
 & Overall & Trial Judge's Ruling & & Final Ruling \\
 & & Punished & Not Punished & Punished & Not Punished \\
\hline
33.6\% & 36.2\% & 30.6\% & 36.1\% & 31.0\% \\
\hline
\end{tabular}
\end{center}

Note: Recidivism is defined using a fuzzy matching algorithm based on the Jaro–Winkler similarity metric.

My covariates contain a full set of court district dummies. Figure 3 shows the histogram of the number judges per court district. Note that most districts have at least two judges during my sampling period even though the modal district has only one judge. Since my identification strategy leverages the random allocation of judges to criminal cases, I only use the 193 districts with two or more judges.

In Section 5.2, I estimate my results using the entire sample, but I focus my discussion in Sections 5.2.\textsuperscript{22} Abramitzky et al. (2019) match full names in historical Censuses in the U.S. and Norway. They define a match between two individuals if the Jaro–Winkler similarity between their names is greater than or equal to 0.90 and if their dates of birth match exactly. Since I do not observe defendants’ dates of birth, I adopt a stricter Jaro-Winkler similarity threshold to define a match in my dataset.

\textsuperscript{22} Abramitzky et al. (2019) match full names in historical Censuses in the U.S. and Norway. They define a match between two individuals if the Jaro–Winkler similarity between their names is greater than or equal to 0.90 and if their dates of birth match exactly. Since I do not observe defendants’ dates of birth, I adopt a stricter Jaro-Winkler similarity threshold to define a match in my dataset.
on the districts with the largest number of judges: Campinas, Ribeirão Preto, Guarulhos and Santos with 14, 9, 8 and 7 judges, respectively. These districts are the third, seventh, second and thirteenth largest cities in the State of São Paulo.

At the end, I impose one final restriction in my dataset: common support between the treatment and control groups. To do so, I impose that the minimum and maximum values of the instrument $Z$ are the same across both treatment arms. My final sample has 43,468 case-defendant pairs when I use the correctly classified treatment variable $D$ and 43,461 case-defendant pairs when I use the misclassified treatment variable $T$.

Moreover, I can also find the defendant’s sex based on the defendant’s first name. This extra covariate is not used in my main analysis, but can be used to check whether the trial judge’s leniency rate is correlated with the defendant’s sex given the court district. To do so, I use the R package genderBR (Meireles, 2021) to define which names are typically male (37,304 defendants), female (4,936) or unisex (1,228) based on the Brazilian 2010 Census. Using only names that are typically male or female, I regress a sex dummy on the trial judge’s leniency rate controlling for court district fixed effects and find a coefficient equal to 0.02 with a standard error equal to 0.03 when I cluster it at the court district level. This result indirectly suggests that trial judges are randomly allocated to criminal cases as mandated by law.

\footnote{The state capital, São Paulo city, is divided in many court districts that are smaller than the aforementioned districts. For example, the largest district in São Paulo city is located in the Barra Funda neighborhood and has only three judges.}
5.2 Empirical Results

First, I present the results of the first stage regression in my empirical analysis. In my model, the correctly classified treatment variable $D$ ("final ruling") is a function of instrument $Z$ ("trial judge’s punishment rate") and court district fixed effects. Following Section 4 and Appendix H.3, I use a polynomial series to approximate the correctly measured propensity score and report the estimated coefficients of linear, quadratic and cubic models in Columns (1)-(3) in Table 3. Note that, in all three models, the only statistically significant coefficient is the one associated with the linear term. For this reason, I use a linear propensity score in my analysis.\(^{24}\)

| Table 3: First Stage Results |
|-----------------------------|
|                             | Final Rulings | Trial Judge’s Ruling |
|                             | (1)          | (2)          | (3)          | (4)          |
| Linear                     | 0.765***     | 0.688***     | 0.833*       | 0.777***     |
|                            | (0.027)      | (0.132)      | (0.485)      | (0.026)      |
| Quadratic                  | 0.070        | -0.216       |              |              |
|                            | (0.117)      | (0.934)      |              |              |
| Cubic                      |              |              | 0.177        |
|                            |              |              | (0.574)      |
| District FE                | ✓            | ✓            | ✓            | ✓            |
| F-statistic                | 827          | 827          | 832          | 868          |
| Sample Size                | 43,468       | 43,461       |              |              |

Note: In Columns (1)-(3), I regress the final punishment in each case on a polynomial series of the trial judge’s leave-one-out punishment rate and court district fixed effects. In Column (4), I regress the trial judge’s ruling in each case on the trial judge’s leave-one-out punishment rate and court district fixed effects. The F-statistic is the test statistic of a $\chi^2$-test whose null hypothesis is that all the coefficients in the associated column are equal to zero. Heteroskedasticity-robust standard errors (HC3, Long and Ervin, 2000) are reported in parenthesis. Significance levels are indicated by *$p \leq 0.10$, **$p \leq 0.05$ and ***$p \leq 0.01$.

Moreover, my instrument is strong according to the F-statistic of the first stage regression. This result implies that the first part of Assumption 2 is valid under the assumption that the correctly measured propensity score is linear, i.e., $\frac{dP_D(z,x)}{dz} \neq 0$ for every value $z$ of the

\(^{24}\)In Appendix J.4, I estimate the correctly measured propensity score semiparametrically (Robinson, 1988; Calonico et al., 2019). These results suggest that the correctly measured propensity score is linear.
instrument and every value $x$ of the covariates.\textsuperscript{25}

Furthermore, when I assume that $D$ is not observable, my partial identification strategy (Section 3) relies on the mismeasured propensity score ($P_T(z, x) = \mathbb{E}[T \mid Z = z, X = x]$) to capture features of the MTE function $\theta(z, x)$. Following Section 4 and Appendix H.1, I use a linear model to approximate the mismeasured propensity score and report the estimated coefficient in Column (4) in Table 3. Note that this coefficient is highly significant, implying that the second part of Assumption 2 is valid under the assumption that the mismeasured propensity score is linear, i.e., $\frac{dP_T(z, x)}{dz} \neq 0$ for every value $z$ of the instrument and every value $x$ of the covariates.\textsuperscript{26}

More importantly, Table 3 shows that Assumptions 5 and 6 are valid. Since $\frac{dP_D(z, x)}{dz} = 0.765$ and $\frac{dP_T(z, x)}{dz} = 0.777$, I have that the derivatives of the correctly measured and mismeasured propensity scores have the same sign and that their ratio is bounded above by 1.02. For this reason and to be conservative, I impose that Assumption 6 holds with $c = 1.1$ in my empirical analysis (Figure 5).

When I semiparametrically estimate the correctly measured propensity score and the mismeasured propensity score, I find that Assumption 6 holds with $c = 1.19$. Even though my main results impose a smaller value ($c = 1.1$ in Figure 5), my sensitivity analysis results impose a much larger value ($c = 1.72$ in Figure 6). This difference illustrate the importance of conducting a sensitivity analysis (Cinelli and Hazlett, 2019) where we gradually increase $c$ to understand the impact of allowing for a more intense misclassification problem.

Second, I report the estimated signs of the MTE function (Corollary 1 and Equation (H.7)) in Figure 4. Each bar represents the MTE function $\theta(\cdot, x)$ for district $x$ and indicates which points $z$ of the MTE function $\theta(z, x)$ are estimated to be positive ($\hat{s}(z, x)$ in Equation (H.7)). For example, the MTE function is positive from 0 to 0.89 for the district of Suzano and from

\textsuperscript{25}In Appendix J.4, I estimate $P_D(\cdot, \cdot)$ semiparametrically (Robinson, 1988; Calonico et al., 2019). The steep inclines of the estimated functions suggest that the instrument is strong for every value $z$ of the instrument and every value $x$ of the covariates, i.e., Assumption 2 holds.

\textsuperscript{26}In Appendix J.4, I estimate $P_T(\cdot, \cdot)$ semiparametrically (Robinson, 1988; Calonico et al., 2019). The steep inclines of the estimated functions suggest that the instrument is strong for every value $z$ of the instrument and every value $x$ of the covariates, i.e., Assumption 2 holds. Moreover, these results suggest that the mismeasured propensity score is linear too.
0 to 0.27 for the district of Carapicuiba.

![Figure 4: Sign of the MTE function $\theta(\cdot, \cdot)$ — Corollary 1](image)

Notes: Each bar represents the MTE function $\theta(\cdot, x)$ for district $x$ and indicates which points $z$ of the MTE function $\theta(z, x)$ are estimated to be positive ($\hat{s}(z, x)$ in Equation (H.7)). The districts are ranked by the amount of evaluation points $z$ that are positive and ties are broken by alphabetical order. In the X-axis, I highlight the first district whose MTE function is not entirely positive (Suzano’s MTE function is positive from 0 to 0.89), the last district whose MTE function is not entirely negative (Carapicuiba’s MTE function is positive from 0 to 0.27), the four main analyzed districts (Campinas, Ribeirão Preto, Guarulhos and Santos), the first district by alphabetical order (Adamantina) and the last district by alphabetical order (Votuporanga).

Since I observe the correctly classified treatment variable $D$ (“each case’s final ruling) in my dataset, I can also estimate the sign of the true MTE function (Appendix H.3). When I compare the estimated sign of the MTE function (Figure 4) against the sign of the correctly estimated MTE function, I find that my method reaches the right conclusion for 98.7% or 99.9% of the evaluated points depending on whether I condition on court districts whose correctly estimated MTE function changes its sign or not.

As explained in Subsection 5.1, Figure 4 also highlights the results for the cities of Campinas, Ribeirão Preto, Guarulhos and Santos. Note that MTE functions $\theta(\cdot, x)$ are estimated to be always positive for Campinas and Ribeirão Preto and always negative for Guarulhos and Santos, implying that alternative sentences may increase or decrease recidivism depending on cities’ contexts. This result illustrates the importance of accounting for geographic heterogeneity when discussing the effect of alternative sentences on recidivism.

Third, I report, in Figure 5, the estimated MTE function $\theta(\cdot, \cdot)$, the estimated misclassified LIV estimand (Equation (3)) and the estimated upper and lower bounds of the set $\Theta_1$.
(Proposition 2) for Ribeirão Preto, Campinas, Santos and Guarulhos. The orange lines are the estimated MTE functions (Equation (H.10)). The dark blue lines are the point-estimates of the LIV estimand (Equations (3) and (H.5)). The light blue lines are the estimated upper and lower bounds of the set $\Theta_1$ (Proposition 2 and Equations (H.8) and (H.9)).

Focusing on the estimated MTE function (orange lines), I find that it is negatively sloped. A decreasing MTE function imply that people who would be punished by most judges are more likely to recidivate than people who would be punished only by stricter judges. However, when I account for sampling uncertainty by computing bootstrapped 90%-confidence bands (Appendix J.1), I can draw a horizontal line through all four of them. This result suggest that unobserved heterogeneity is possibly unimportant when discussing the effect of alternative sentences on recidivism. Moreover, all four confidence bands contain, at least partially, the zero function. This finding indicates that effect of alternative sentences on recidivism is likely small.

Figure 5 also illustrates the danger of ignoring misclassification of the treatment variable. By comparing the estimated MTE function (orange lines) and the estimated misclassified LIV estimand (dark blue lines), I can estimate the bias that is generated by the misclassified treatment variable. Note that this bias is always negative in Ribeirão Preto, but it may be negative, zero or positive in the other three cities. This result highlights the complexity of the misclassification bias when estimating entire functions since its sign may change and it may move your estimates away from zero. Consequently, even an expert may have difficulties theorizing about its direction.

Moreover, Figure 5 highlights the possibly large magnitude of the misclassification bias. Although small in absolute terms, the bias function can be relatively sizeable, representing a share of the true MTE function as large as 2.4% in Ribeirão Preto, 4.4% in Guarulhos, 6.9% in Campinas and 11.3% in Santos. Since it is a priori unknown whether any given empirical application resembles the small bias context of Ribeirão Preto or the large bias context of Santos, this result illustrate the usefulness of adopting methods that account for misclassification bias when the treatment variable may be misclassified.\textsuperscript{27}

\textsuperscript{27}See Appendix J.2 for a discussion about the misclassification bias in the intercept and slope coefficients.
When the target parameter is the MTE function, one of these methods is proposed in Sections 2 and 3 and illustrated by the light blue lines in Figure 5. Note that the identified set safely contain the estimated MTE function (orange lines), exemplifying that the proposed method works in a specific real-world example.

![Figure 5: Bounds around the MTE function $\theta(\cdot, \cdot)$ — Proposition 2](image)

Notes: The orange lines are the estimated MTE functions $\theta(\cdot, \cdot)$ based on the LIV estimator that uses the correctly classified treatment variable $D$ (Equation (H.10)). The dark blue lines are the point-estimates of the LIV estimand that uses the misclassified treatment variable $T$ (Equations (3) and (H.5)). The light blue lines are the estimated upper and lower bounds of the set $\Theta_1$ (Proposition 2 and Equations (H.8) and (H.9)) based on a constant $c = 1.1$.

Although successful in my empirical example, the proposed partial identification method may have worked only because my unique dataset allows me to estimate the correctly measured described in Section 4.
propensity score $P_D$ and find a constant $c$ that conservatively satisfies Assumption 6. Since most real-world applications do not have access to the correctly measured treatment variable $D$, this approach to choose $c$ is frequently unfeasible.

For this reason, I propose two alternative ways to approximate the constant $c$ using data. The first one is described in Section 2 and consists simply in choosing different values of $c$ to understand the impact of allowing for a more intense misclassification problem. The second way to approximate the constant $c$ is described in Section 3 and consists of choosing a $c$ that is associated with the most intense misclassification problem that is not falsified by the data. It consists in adapting the test proposed by Frandsen et al. (2019). The largest estimated value of the LIV estimand is equal to 0.58. To make it equal to the largest possible effect, it has to be multiplied by 1.72. Consequently, $c = 1.72$ is the largest value of $c$ that does not falsify the model in Section 2.

Figure 6 shows the estimated bounds based on $c = 1.72$ in purple.28 As expected, they are much wider than the ones based on $c = 1.1$ (light blue line). However, they are still informative. For example, note that all purple bounds do not contain the zero effect function.

Figures 5 and 6 also show the sharp bounds of set $\Theta_2$ in Proposition 3 after adding Assumption G.1. In this case, sets $\Theta_1$ and $\Theta_2$ coincide because the estimated LIV estimator does not change signs in the analyzed districts.

For this reason, I proceed to a discussion about the bounds around typical treatment effect parameters (Corollary 3). Figure 7 shows, in dark blue, the bounds around the average treatment effect (ATE), the average treatment effect on the treated (ATT) and the average treatment effect on the untreated (ATU) that are described in Appendix G. It also shows, in orange, the estimated parameters based on the correctly measured MTE function $\theta(\cdot, \cdot)$. Even though the estimated bounds are narrow, they contain the estimated treatment effect parameters, exemplifying that the proposed method works in a specific real-world example.

Furthermore, the effect of alternative sentences on recidivism seems to be small. While the

---

28Note that any $c \in (1.1, 1.72)$ generates bounds that are between the light blue bounds and the purple bounds in Figure 6. In particular, if I imposed $c = 1.19$ as suggested by the semiparametric estimation of the propensity score functions (Appendix J.4), I would find bounds that are between the light blue bounds and the purple bounds.
Figure 6: Bounds around the MTE function $\theta(\cdot, \cdot)$ — Sensitivity Analysis

Notes: The orange lines are the estimated MTE functions $\theta(\cdot, \cdot)$ based on the LIV estimator that uses the correctly classified treatment variable $D$ (Equation (H.10)). The dark blue lines are the point-estimates of the LIV estimand that uses the misclassified treatment variable $T$ (Equations (3) and (H.5)). The light blue lines are the estimated upper and lower bounds of the set $\Theta_1$ (Proposition 2 and Equations (H.8) and (H.9)) based on a constant $c = 1.1$. The purple lines are the estimated upper and lower bounds of the set $\Theta_1$ (Proposition 2 and Equations (H.8) and (H.9)) based on a constant $c = 1.72$.

estimated bounds are far away from zero in Ribeirão Preto, they are close to zero in the other three court districts. Moreover, when accounting for sampling uncertainty by computing bootstrapped 90%-confidence intervals (light blue lines), I find that ten of the treatment effect parameters are statistically zero, while two of them are only marginally significant at the 10%-significance level.

A small effect of alternative sentences on recidivism is also supported by standard two-stage least squares (2SLS) regressions (Appendix J.3). When I regress recidivism on the
Figure 7: Bounds around Common Treatment Effect Parameters — Appendix G

Notes: The orange points are the estimated treatment effect parameters based on the correctly measured MTE function \( \theta(\cdot, \cdot) \). The dark blue lines are the estimated bounds based on the LIV estimator that uses the misclassified treatment variable \( T \) and a constant \( c = 1.1 \) (Appendix G). The light blue lines are bootstrapped 90%-confidence bands (1,000 repetitions) around the estimated bounds.

cases’ final rulings using the trial judge’s punishment rate as an instrument (Column (4) in Table J.1), I find a point-estimate equal to .054 and marginally significant at the 10%-significance level. When I use a full set of trial judge’s dummies as my instrumental variables (Column (3) in Table J.1), I find a even smaller point-estimate (.048) based on a weak set of instruments. Since 2SLS estimands may assign negative weights to interpretable treatment effect parameters (Heckman et al., 2006; Sloczynski, 2021; Blandhol et al., 2022) and the instruments in Column (3) are weak, the significance of those results should be considered cautiously in light of the null effects that were found when analyzing the MTE function.

Table J.1 also illustrates the danger of ignoring misclassification bias in a 2SLS. When I use the possibly misclassified trial judge’s ruling as my treatment variable, I find point-estimates equal to 0.053 and .049 depending on the chosen instruments. Consequently, the misclassification bias may be equal to 1.3% or 2.2% of the estimated 2SLS estimand depending on the chosen instruments.

Finally, I also report the results based on a standard analysis of the function \( \mathbb{E}[Y_1 - Y_0|U = u, X = x] \) in Appendix J.5. To estimate this function, I use the semi-parametric estimator described by Cornelissen et al. (2016, Appendix B.1) that uses the LIV estimand (Equation (4)) proposed by Heckman et al. (2006). In Figure J.5, the orange line is the estimated MTE function based on the correctly measured propensity score \( P_D \), while the dark blue line is the estimated LIV
estimand $\tilde{f}$ based on the mismeasured propensity score $P_T$. The dotted lines follow the same color scheme and show bootstrapped 90%-confidence intervals.

Similarly to the linear analysis (Figure J.1), these results suggest that the unobserved heterogeneity is possibly unimportant when discussing the effect of alternative sentences on recidivism. Furthermore and similar to the previous analyses, all eight confidence bands contain the zero function, indicating that the effect of alternative sentences is likely small.

6 Conclusion

In this paper, I address a widespread empirical challenge: policy evaluation with a misclassified treatment variable. To do so, I focus on the connection between the LIV estimand based on the mismeasured propensity score and the MTE function.

I start by showing that, differently from the standard context with a correctly measured treatment variable, the LIV estimand does not identify the MTE function. In particular, the LIV estimand’s bias is hard to predict because it may be positive or negative depending on the instrument’s value.

To avoid this measurement error problem, I propose a novel partial identification strategy to identify the MTE function with a misclassified treatment. This method explores restrictions on the relationship between the instrument, the misclassified treatment and the correctly measured treatment, allowing for dependence between the instrument and the potential misclassified treatment variables and the misreporting decision.

To illustrate the usefulness of this new method, I analyze whether alternative sentences affect recidivism in the State of São Paulo, Brazil. I approach this question in three ways: (i) ignoring misclassification and using the standard LIV estimand based on trial judge’s rulings, (ii) accounting for misclassification using the proposed partial identification strategy, and (iii) using Appeals Court data to correctly estimate the MTE function based on each case’s final ruling.

When comparing the estimates based on each case’s final ruling and based on trial judge’s rulings, I find that the misclassification bias is empirically relevant. This bias reaches more
than 10% of the correctly measured treatment effect parameter depending on the observable covariates.

When comparing the estimates based on each case’s final ruling and the results based on my partial identification strategy, I find that the estimated bounds contain the correctly estimated MTE function. Consequently, the proposed method works appropriately in a real-world example.

When I focus on each case’s correctly classified final sentence, I find that the effect of alternative sentences on recidivism is likely small even though the point-estimates present a large amount of observable geographic heterogeneity. In contrast, the unobservable heterogeneity captured by the MTE function does not seem to be relevant in this empirical application.

This result contrasts with recent findings in the empirical literature for Finland (Huttunen et al., 2020), Milwaukee (Giles, 2021) and North Carolina (Klaassen, 2021). While they find that alternative sentences increase recidivism in Finland and Milwaukee and decrease it in North Carolina, my estimated treatment effect parameters are small and rarely statistically different from zero. Given the amount of observable geographic heterogeneity in my estimated results, the difference between the recent literature and my findings may be due to different contexts in Finland, Milwaukee, North Carolina and São Paulo. A deeper understanding of the mechanisms behind these differences deserves further investigation in future work. Given that those effects may depend on the underlying crime level in each location, it is also important to investigate the effect of alternative sentences in regions with higher crime rates, such as other states in Brazil or other countries in Latin America.

References

Abramitzky, R., L. Boustan, and K. Eriksson (2019). To the New World and Back Again: Return Migrants in the Age of Mass Migration. *ILR Review* 72(2), 300–322. (Cited on page 32.)

Acerenza, S. (2021, October). Partial Identification of Marginal Treatment Effects with Discrete Instruments and Misreported Treatment. Available at arxiv.org/abs/2110.06285. (Cited on page 7.)
Acerenza, S., K. Ban, and D. Kédagni (2022). Marginal Treatment Effects with Misclassified Treatment. (Cited on pages 3, 7, 8, 14, 16, 31, and 71.)

Agan, A. Y., J. L. Doleac, and A. Harvey (2021, March). Misdemeanor Prosecution. NBER Working Paper 28600. Available at https://www.nber.org/papers/w28600. (Cited on pages 6 and 10.)

Aizer, A. and J. J. Doyle (2015). Juvenile Incarceration, Human Capital, and Future Crime: Evidence from Randomly Assigned Judges. The Quarterly Journal of Economics 130(2), pp. 759–803. (Cited on page 6.)

Arellano-Bover, J. (2020, May). Displacement, Diversity, and Mobility: Career Impacts of Japanese American Internment. IZA DP No. 12554. Available at http://ftp.iza.org/dp12554.pdf. (Cited on pages 3, 10, and 62.)

Arteaga, C. (2019, September). The Cost of Bad Parents: Evidence from the Effects of Parental Incarceration on Children’s Education. Available at: http://www.carolinaarteaga.com/. (Cited on page 6.)

Bernstein, S., R. Diamond, T. McQuade, and B. Pousada (2019, July). The Contribution of High-Skilled Immigrants to Innovation in the United States. Available at https://web.stanford.edu/~diamondr/BDMP_2019_0709.pdf. (Cited on page 3.)

Bhuller, M., G. B. Dahl, K. V. Loken, and M. Mogstad (2019). Incarceration, Recidivism, and Employment. Journal of Polical Economy. Forthcoming. (Cited on pages 6 and 10.)

Black, D., S. Sanders, and L. Taylor (2003). Measurement of Higher Education in the Census and Current Population Survey. Journal of the American Statistical Association 98(463), pp. 545–554. (Cited on pages 3 and 56.)

Black, S. E., J. T. Denning, and J. Rothstein (2020, March). Winners and Losers? The Effect of Gaining and Losing Access to Selective Colleges on Education and Labor Market Outcomes. NBER Working Paper 26821. Available at https://www.nber.org/papers/w26821. (Cited on pages 3, 10, and 62.)

Black, S. E., P. J. Devereux, and K. G. Salvanes (2005). The More the Merrier? The Effect of Family Size and Birth Order on Children’s Education. The Quarterly Journal of Economics 120(2), pp. 669–700. (Cited on page 11.)

Blandhol, C., J. Bonney, M. Mogstad, and A. Torgovitsky (2022, January). When is TSLS Actually LATE? NBER Working Paper n. 29709. Available at nber.org/papers/w29709. (Cited on page 41.)

Bollinger, C. (1996). Bounding mean regressions when a binary regressor is mismeasured. Journal of Econometrics 73, pp. 387–399. (Cited on page 3.)

Bound, J., C. Brown, and N. Mathiowetz (2001). Measurement Error in Survey Data. In J. Heckman and E. Leamer (Eds.), Handbook of Econometrics, Volume 5, Chapter Chapter 59, pp. pp. 3705–3843. Elsevier. (Cited on page 8.)

44
Brinch, C. N., M. Mogstad, and M. Wiswall (2017). Beyond LATE with a Discrete Instrument. *Journal of Political Economy* 125(4), pp. 985–1039. (Cited on page 11.)

Bruckmeier, K., R. T. Riphahn, and J. Wiemers (2021). Misreporting of Program Take-up in Survey Data and its Consequences for Measuring Non-take-up: New Evidence from Linked Administrative and Survey Data. *Empirical Economics* 61, pp. 1567–1616. (Cited on page 3.)

Calonico, S., M. D. Cattaneo, and M. H. Farrell (2019). nprobust: Nonparametric Kernel-Based Estimation and Robust Bias-Corrected Inference. *Journal of Statistical Software* 91(8), pp. 1–33. (Cited on pages 30, 31, 34, 35, and 93.)

Calvi, R., A. Lewbel, and D. Tommasi (2021, April). Women’s Empowerment and Family Health: Estimating LATE with Mismeasured Treatment. *Journal of Business and Economic Statistics*, pp. 1–17. Available at https://www2.bc.edu/arthur-lewbel/MR_LATE_APR2019.pdf. (Cited on pages 3, 7, 9, 10, 19, and 66.)

Canay, I. A. and A. M. Shaikh (2017). Practical and Theoretical Advances for Inference in Partially Identified Models. In B. Honore, A. Pakes, M. Piazzesi, and L. Samuelson (Eds.), *Advances in Economics and Econometrics*, Volume 11th World Congress of Econometric Society Monographs, pp. pp. 271–306. (Cited on page 24.)

Card, D. (1992, October). The Effect of Unions on the Distribution of Wages: Redistribution or Relabelling. NBER Working Paper No. 4195. (Cited on page 3.)

Card, D. (2001). Estimating the Return to Schooling: Progress on Some Persistent Econometric Problems. *Econometrica* 69(5), pp. 1127–1160. (Cited on page 3.)

Carneiro, P., J. J. Heckman, and E. Vytlacil (2011). Estimating Marginal Returns to Education. *The American Economic Review* 101(6), pp. 2754–2781. (Cited on pages 10 and 56.)

Carneiro, P., M. Lokshin, and N. Umapathi (2017). Average and Marginal Returns to Upper Secondary Schooling in Indonesia. *Journal of Applied Econometrics* 32(1), pp. 16–36. (Cited on page 10.)

Celhay, P. A., B. D. Meyer, and N. Mittag (2022, January). What Leads to Measurement Errors? Evidence from Reports of Program Participation in Three Surveys. NBER Working Paper n. 29652. Available at nber.org/papers/w29652. (Cited on page 3.)

Chalak, K. (2017). Instrumental Variables Methods with Heterogeneity and Mismeasured Instruments. *Econometric Theory* 33(1), pp. 69–104. (Cited on pages 18 and 77.)

Cinelli, C. and C. Hazlett (2019). Making Sense of Sensitivity: Extending Omitted Variable Bias. *Journal of the Royal Statistical Society B* 82(1), pp. 39–67. (Cited on pages 14 and 35.)

Clark, D. and P. Martorell (2014). The Signalling Value of a High School Diploma. *Journal of Political Economy* 122(2), pp. 282–318. (Cited on page 58.)

Cornelissen, T., C. Dustmann, A. Raute, and U. Schonberg (2016). From LATE to MTE: Alternative Methods for the Evaluation of Policy Interventions. *Labour Economics* 41, pp. 47–60. (Cited on pages 41, 73, 75, 94, and 95.)

45
Cornelissen, T., C. Dustmann, A. Raute, and U. Schonberg (2018). Who Benefits from Universal Child Care? Estimating Marginal Returns to Early Child Care Attendance. *Journal of Political Economy* 126(6), pp. 2356–2409. (Cited on pages 10, 73, and 75.)

Dobbie, W., H. Gronqvist, S. Niknami, M. Palme, and M. Priks (2018, January). The Intergenerational Effects of Parental Incarceration. NBER Working Paper 24186. (Cited on page 6.)

Dupas, P. (2014). Short-Run Subsidies and Long-Run Adoption of New Health Products: Evidence From a Field Experiment. *Econometrica* 82(1), pp. 197–228. (Cited on page 10.)

Frandsen, B. R., L. J. Lefgren, and E. C. Leslie (2019, February). Judging Judge Fixed Effects. NBER Working Paper 25528. (Cited on pages 4, 19, 21, and 39.)

Giles, T. (2021, October). The (Non)Economics of Criminal Fines and Fees. Available at https://drive.google.com/file/d/1jyXjQBXX3A9bs9Rf0yTF13U6Li_M8sxb/view. (Cited on pages 8 and 43.)

Green, D. P. and D. Winik (2010). Using Random Judge Assignments to Estimate the Effects of Incarceration and Probation on Recidivism among Drug Offenders. *Criminology* 48(2), pp. 357–387. (Cited on pages 3 and 6.)

Haider, S. and M. Stephens (2020, September). Correcting for Misclassified Binary Regressors Using Instrumental Variables. NBER Working Paper n. 27797. Available at nber.org/papers/w27797. (Cited on pages 8 and 12.)

Heckman, J. and E. Vytlacil (1999). Local Instrumental Variables and Latent Variable Models for Identifying and Bounding Treatment Effects. *Proceedings of the National Academy of Sciences of the United States of America* 96, 4730–4734. (Cited on pages 3, 9, and 64.)

Heckman, J. and E. Vytlacil (2001). Policy-Relevant Treatment Effects. *American Economic Review: Papers and Proceedings* 91(2), pp. 107–111. (Cited on pages 24 and 70.)

Heckman, J. J., S. Urzua, and E. Vytlacil (2006). Understanding Instrumental Variables in Models with Essential Heterogeneity. *The Review of Economics and Statistics* 88(3), pp. 389–432. (Cited on pages 4, 10, 11, 18, 19, 41, 55, and 94.)

Heckman, J. J. and E. Vytlacil (2005). Structural Equations, Treatment Effects and Econometric Policy Evaluation. *Econometrica* 73(3), pp. 669–738. (Cited on pages 4, 17, 19, 54, and 70.)

Hernandez, M. and S. Pudney (2007). Measurement Error in Models of Welfare Participation. *Journal of Public Economics* 91, pp. 328–341. (Cited on pages 3 and 16.)

Hernandez, M., S. Pudney, and R. Hancock (2007). The Welfare Cost of Means-Testing: Pensioner Participation in Income Support. *Journal of Applied Econometrics* 22, pp. 581–598. (Cited on page 3.)

Hu, Y. (2017). The Econometrics of Unobservables: Applications of Measurement Error Models in Empirical Industrial Organization and Labor Economics. *Journal of Econometrics* 200(2), 154–168. (Cited on page 7.)
Humphries, J. E., N. Mader, D. Tannenbaum, and W. van Dijk (2019, July). Does Eviction Cause Poverty? Quasi-experimental Evidence from Cook County, IL. Available at: https://drive.google.com/file/d/1jD-7ogS7Ak7X7DgwjCkrBcgq_NqotxSp/view. (Cited on pages 57 and 58.)

Huttunen, K., M. Kaila, and E. Nix (2020, June). The Punishment Ladder: Estimating the Impact of Different Punishments on Defendant Outcomes. Available at https://drive.google.com/file/d/1DhEoGSDLG8FsOMdkfBq1yUSNjHx8rwh/view?usp=sharing. (Cited on pages 8 and 43.)

Imbens, G. W. and J. D. Angrist (1994). Identification and Estimation of Local Average Treatment Effects. *Econometrica* 62(2), pp. 467–475. (Cited on pages 7, 10, 64, and 65.)

Kane, T. J., C. E. Rouse, and D. Staiger (1999, July). Estimating Returns to Schooling when Schooling is Misreported. NBER Working Paper 7235. (Cited on page 3.)

Klaassen, F. D. (2021, November). Crime and (Monetary) Punishment. Available at https://diazkla.github.io/felipediaz.com/jmp_monetary_sanctions_felipe_diaz.pdf. (Cited on pages 8 and 43.)

Kline, P. and A. Santos (2013). Sensitivity to Missing Data Assumptions: Theory and an Evaluation of the U.S. Wage Structure. *Quantitative Economics* 4(2), pp. 231–267. (Cited on page 25.)

Kling, J. R. (2006, June). Incarceration Length, Employment, and Earnings. *American Economic Review* 96(3), 863–876. (Cited on page 6.)

Lofeffer, C. E. (2013). Does Imprisonment Alter the Life Course? Evidence on Crime and Employment from a Natural Experiment. *Criminology* 51(1), pp. 137–166. (Cited on page 6.)

Long, S. and L. H. Ervin (2000). Using Heteroscedasticity Consistent Standard Errors in the Linear Regression Model. *The American Statistician* 54(3), pp. 217–224. (Cited on page 34.)

Manski, C. F. (2011, August). Policy Analysis with Incredible Certitude. *The Economic Journal* 121(554), pp. F261–F289. (Cited on pages 4 and 11.)

Masten, M. A. and A. Poirier (2018). Identification of Treatment Effects under Conditional Partial Independence. *Econometrica* 86(1), pp. 317–351. (Cited on page 25.)

Masten, M. A. and A. Poirier (2019). Inference on Breakdown Frontier. *Quantitative Economics*. (Cited on page 25.)

Meireles, F. (2021). *genderBR: Predict Gender from Brazilian First Names*. R package version 1.1.2. (Cited on page 33.)

Meyer, B., N. Mittag, and R. Goerge (2018, October). Errors in Survey Reporting and Imputation and their Effects on Estimates of Food Stamp Program Participation. NBER Working Paper 25143. (Cited on page 3.)

Meyer, B. D. and N. Mittag (2019a). Misreporting of Government Transfers: How Important Are Survey Design and Geography? *Southern Economic Journal* 86(1), pp. 230–253. (Cited on page 3.)
Meyer, B. D. and N. Mittag (2019b). Using Linked Survey and Administrative Data to Better Measure Income: Implications for Poverty, Program Effectiveness, and Holes in the Safety Net. *American Economic Journal: Applied Economics* 11(2), pp. 176–204. (Cited on page 3.)

Meyer, B. D., W. K. C. Mok, and J. X. Sullivan (2015). Household Surveys in Crisis. *Journal of Economic Perspectives* 29(4), pp. 199–226. (Cited on page 3.)

Millimet, D. L. (2011). The Elephant in the Corner: A Cautionary Tale about Measurement Error in Treatment Effects Models. *Advances in Econometrics* 27A, pp. 1–39. (Cited on page 3.)

Mogstad, M., A. Santos, and A. Torgovitsky (2018). Using Instrumental Variables for Inference about Policy Relevant Treatment Effects. *Econometrica* 86(5), pp. 1589–1619. (Cited on pages 11 and 70.)

Norris, S., M. Pecenco, and J. Weaver (2021). The Effects of Parental and Sibling Incarceration: Evidence from Ohio. *American Economic Review Forthcoming*. (Cited on page 10.)

Robinson, P. M. (1988). Root-N-Consistent Semiparametric Regression. *Econometrica* 56(4), pp. 931–954. (Cited on pages 34, 35, 93, and 94.)

Schennach, S. M. (2016). Recent Advances in the Measurement Error Literature. *Annual Review of Economics* 8, 341–377. (Cited on page 7.)

Scott, D. W. (1992). *Multivariate Density Estimation: Theory, Practice, and Visualization*. Wiley Series in Probability and Statistics. New York: John Wiley and Sons, Inc. (Cited on page 30.)

Sloczynski, T. (2021, June). When Should We (Not) Interpret Linear IV Estimands as LATE? Available at [https://arxiv.org/abs/2011.06695](https://arxiv.org/abs/2011.06695). (Cited on page 41.)

Tamer, E. (2010). Partial Identification in Econometrics. *The Annual Review of Economics* 2, pp. 167–195. (Cited on pages 4 and 12.)

Tommasi, D. and L. Zhang (2020, June). Bounding Program Benefits When Participation Is Misreported. IZA DP No. 13430. Available at [http://ftp.iza.org/dp13430.pdf](http://ftp.iza.org/dp13430.pdf). (Cited on pages 7, 19, and 59.)

Ura, T. (2018). Heterogeneous Treatment Effects with Mismeasured Endogenous Treatment. *Quantitative Economics* 9, pp. 1335–1370. (Cited on pages 3, 7, 8, 9, 10, 64, and 66.)

Vytlacil, E. (2002). Independence, Monotonicity and Latent Index Models: An Equivalence Result. *Econometrica* 70(1), pp. 331–341. (Cited on pages 10 and 64.)

Waters, N. L., A. Gallegos, J. Green, and M. Rozsi (2015). Criminal Appeals in State Courts. Bureau of Justice Statistics, U.S. Department of Justice, Bulletin NCJ 248874. Available at [https://bjs.ojp.gov/content/pub/pdf/casc.pdf](https://bjs.ojp.gov/content/pub/pdf/casc.pdf). (Cited on page 79.)

Winkler, W. E. (1990). String Comparator Metrics and Enhanced Decision Rules in the Fellegi-Sunter Model of Record Linkage. In *Proceedings of the Section on Survey Research Methods*, pp. 354–369. American Statistical Association. (Cited on pages 31 and 90.)
Yanagi, T. (2019). Inference on Local Average Treatment Effects for Misclassified Treatment. *Econometric Reviews* 38(8), pp. 938–960. (Cited on page 7.)
Supporting Information
(Online Appendix)

A  Proofs of the main results

A.1  Proof of Proposition 1

First, I prove Equation (5).\textsuperscript{29} Fix \( z \in Z \) arbitrarily. Note that

\[
\mathbb{E}[Y|Z = z] = \mathbb{E}[Y_1 \cdot D + Y_0 \cdot (1 - D)| Z = z]
\]

by Equation (1)

\[
= \mathbb{E}[Y_0|Z = z] + \mathbb{E}[(Y_1 - Y_0) \cdot D| Z = z]
\]

by Assumption 1

\[
= \mathbb{E}[Y_0] + \mathbb{E}[(Y_1 - Y_0) \cdot 1 \{U \leq P_D(z)}| Z = z]
\]

by Equation (2)

\[
= \mathbb{E}[Y_0] + \mathbb{E}[(Y_1 - Y_0) \cdot 1 \{U \leq P_D(z)}]
\]

by Assumption 1

\[
= \mathbb{E}[Y_0] + \mathbb{E}[(Y_1 - Y_0)] \cdot \mathbb{E}[Y_1 - Y_0|U] \quad \text{by the LIE}
\]

\[
= \mathbb{E}[Y_0] + \int_0^{P_D(z)} \mathbb{E}[Y_1 - Y_0|U = u] \, du,
\]

implying, by the Leibniz Integral Rule, that

\[
\frac{d \mathbb{E}[Y|Z = z]}{dz} = \frac{dP_D(z)}{dz} \cdot \mathbb{E}[Y_1 - Y_0|U = P_D(z)].
\]

Since \( \frac{d \mathbb{E}[T|Z = z]}{dz} = \frac{dP_T(z)}{dz} \) by definition, Equation (5) holds.

Now, I prove Equation (6). Fix \( p \) in the support of \( P_T(Z) \) arbitrarily. Note that

\[
\mathbb{E}[Y|P_T(Z) = p] = \mathbb{E}[Y|Z = P_T^{-1}(p)] \quad \text{because } P_T \text{ is invertible}
\]

\[
= \mathbb{E}[Y_0] + \int_0^{P_D(P_T^{-1}(p))} \mathbb{E}[Y_1 - Y_0|U = u] \, du,
\]

\textsuperscript{29}In this entire appendix, LIE stands for Law of Iterated Expectations.
implying, by the Leibniz Integral Rule, the Chain Rule and the Inverse Function Theorem, that Equation (6) holds.

A.2 Proof of Proposition 2

I, first, show that $\theta \in \Theta_1$. Fix $z \in \mathcal{Z}$ arbitrarily. Note that

$$|\theta(z)| = \left| \frac{dP_T(z)/dz}{dP_D(z)/dz} \cdot f(z) \right|$$

according to Equation (5)

$$= \left| \frac{dP_T(z)/dz}{dP_D(z)/dz} \right| \cdot |f(z)|$$

$$\leq \left[ \frac{1}{c} \cdot |f(z)|, c \cdot |f(z)| \right]$$

by Assumptions 2 and 6.

As a consequence, I have that $\theta \in \Theta_1$.

For the second part of Proposition 2, observe that, for any $\tilde{\theta} \in \Theta_1$,

$$\left\| \theta - \tilde{\theta} \right\|_{\infty} = \sup_{z \in \mathcal{Z}} \left\{ |\theta(z) - \tilde{\theta}(z)| \right\}$$

by definition of the norm $\|\cdot\|_{\infty}$

$$\leq \sup_{z \in \mathcal{Z}} \left\{ c \cdot f(z) - \frac{1}{c} \cdot f(z) \right\}$$

by definition of $\Theta_1$

$$= \sup_{z \in \mathcal{Z}} \left\{ c - \frac{1}{c} \cdot |f(z)| \right\}$$

$$= \left| c - \frac{1}{c} \right| \cdot \sup_{z \in \mathcal{Z}} \{|f(z)|\}$$

$$= \left| c^2 - 1 \right| \cdot \|f\|_{\infty}$$

by definition of the norm $\|\cdot\|_{\infty}$

and, for any $d \in \mathbb{R}_{++}$,

$$\left\| \theta - \tilde{\theta} \right\|_{d} = \left( \int |\theta(z) - \tilde{\theta}(z)|^d \, dz \right)^{1/d}$$

by definition of the norm $\|\cdot\|_{d}$

$$\leq \left( \int c \cdot f(z) - \frac{1}{c} \cdot f(z) \right|^d \, dz \right)^{1/d}$$

by definition of $\Theta_1$

$$= \left( \int c - \frac{1}{c} \right|^d \cdot |f(z)|^d \, dz \right)^{1/d}$$

$$= \left| c - \frac{1}{c} \right| \cdot \left( \int |f(z)|^d \, dz \right)^{1/d}$$
A.3 Proof of Proposition 3

The proof is by construction. For each \( \tilde{\theta} \in \Theta_2 \), I define the density of the candidate random variables \( \left( \tilde{U}, \tilde{Y}_0, \tilde{Y}_1, \tilde{D} \right) \) to ensure that they satisfy the five restrictions of Proposition 3.

Fix \( \tilde{\theta} \in \Theta_2 \) arbitrarily. By definition, there exist \( (a, \tilde{b}) \in A \) such that \( \tilde{P}_D := Z \rightarrow [0, 1] \) is invertible, \( P_D(z) = \int_{-\infty}^{z} a(\tilde{z}) \cdot \frac{dP_T(\tilde{z})}{dz} \, d\tilde{z} + \tilde{b} \) and \( \tilde{\theta}(z) = \frac{1}{a(z)} \cdot f(z) \) for any \( z \in Z \). I break the construction of the candidate random variables \( \left( \tilde{U}, \tilde{Y}_0, \tilde{Y}_1, \tilde{D} \right) \) in six steps.

Step 1. Note that, by definition, Assumption 7 holds for \( P_D \). Observe also that \( \frac{dP_D(z)}{dz} = a(z) \cdot \frac{dP_T(z)}{dz} \) for any \( z \in Z \), implying that Assumption 2 holds.

Step 2. Define \( \tilde{U} \sim \text{Uniform} [0, 1] \) and \( \tilde{D} := 1 \left\{ \tilde{U} \leq P_D(Z) \right\} \), ensuring that Assumption 3 and Equation (7) hold.

Step 3. Since \( f_{Z|\tilde{U},\tilde{Y}_0,\tilde{Y}_1} = f_Z \cdot f_{\tilde{U}|Z} \cdot \mathbb{P} \left[ \tilde{Y}_0 = \cdot, \tilde{Y}_1 = \cdot | \tilde{U}, Z \right] \), I define the joint density function of \( (Z, \tilde{U}, \tilde{Y}_0, \tilde{Y}_1) \) through its components \( f_Z, f_{\tilde{U}|Z} \) and \( \mathbb{P} \left[ \tilde{Y}_0 = \cdot, \tilde{Y}_1 = \cdot | \tilde{U}, Z \right] \). Fix \( (z, u, y_0, y_1) \in \mathbb{R}^2 \times \{0, 1\}^2 \) arbitrarily.

(a) Note that \( f_Z \) is identified. Consequently, \( f_Z \) is defined according to the data.

(b) Define \( f_{\tilde{U}|Z} (u | z) = f_{\tilde{U}} (u) \) and \( \mathbb{P} \left[ \tilde{Y}_0 = y_0, \tilde{Y}_1 = y_1 | \tilde{U} = u, Z = z \right] = \mathbb{P} \left[ \tilde{Y}_0 = y_0, \tilde{Y}_1 = y_1 | \tilde{U} = u \right] \), ensuring that Assumption 1 holds. Consequently, I only have to define \( f_{\tilde{U}} (u) \) and
\[
\mathbb{P} \left[ \tilde{Y}_0 = y_0, \tilde{Y}_1 = y_1 | \tilde{U} = u \right].
\]

(c) Note that \( f_{\tilde{U}} \) is defined in Step 2.

(d) I impose \( \mathbb{P} \left[ \tilde{Y}_0 = y_0, \tilde{Y}_1 = \tilde{y}_1 | \tilde{U} = u \right] = \mathbb{P} \left[ \tilde{Y}_0 = y_0 | \tilde{U} = u \right] \cdot \mathbb{P} \left[ \tilde{Y}_1 = \tilde{y}_1 | \tilde{U} = u \right] \) for simplicity. Define \( \mathbb{P} \left[ \tilde{Y}_0 = 0 | \tilde{U} = u \right] = 1 - \mathbb{P} \left[ \tilde{Y}_0 = 1 | \tilde{U} = u \right] \) and \( \mathbb{P} \left[ \tilde{Y}_1 = 1 | \tilde{U} = u \right] = 1 - \mathbb{P} \left[ \tilde{Y}_1 = 0 | \tilde{U} = u \right] \). Consequently, I only have to define \( \mathbb{P} \left[ \tilde{Y}_0 = 1 | \tilde{U} = u \right] \) and
\[
\mathbb{P} \left[ \tilde{Y}_1 = 1 | \tilde{U} = u \right].
\]
(e) Define the set \( \mathcal{U} := \{ \tilde{u} \in \mathbb{R} | \tilde{u} = P_D(z) \text{ for some } z \in \mathcal{Z} \} \subseteq [0,1] \).

(f) If \( u \notin \mathcal{U} \), \( \mathbb{P} \left[ \tilde{Y}_0 = 1 \big| \tilde{U} = u \right] = 0 \) and \( \mathbb{P} \left[ \tilde{Y}_1 = 1 \big| \tilde{U} = u \right] = 0 \).

(g) From now on, suppose that \( u \in \mathcal{U} \). Define

\[
\mathbb{P} \left[ \tilde{Y}_0 = 1 \big| \tilde{U} = u \right] = -\tilde{\theta} \left( P_D^{-1}(u) \right) \cdot 1 \left\{ \tilde{\theta} \left( P_D^{-1}(u) \right) < 0 \right\}
\]

and

\[
\mathbb{P} \left[ \tilde{Y}_1 = 1 \big| \tilde{U} = u \right] = \tilde{\theta} \left( P_D^{-1}(u) \right) \cdot 1 \left\{ \tilde{\theta} \left( P_D^{-1}(u) \right) \geq 0 \right\}.
\]

Step 4. Note that \( \tilde{Y}_0 \) and \( \tilde{Y}_1 \) satisfy Assumption 4 and Equation (10).

Step 5. Observe that, for any \( z \in \mathcal{Z} \),

\[
\mathbb{E} \left[ \tilde{Y}_1 - \tilde{Y}_0 \big| U = P_D(z) \right] = \mathbb{P} \left[ \tilde{Y}_1 = 1 \big| \tilde{U} = P_D(z) \right] - \mathbb{P} \left[ \tilde{Y}_0 = 1 \big| \tilde{U} = P_D(z) \right]
\]

\[
= \tilde{\theta} \left( P_D^{-1}(P_D(z)) \right) \cdot 1 \left\{ \tilde{\theta} \left( P_D^{-1}(P_D(z)) \right) \geq 0 \right\}
\]

\[
- \left( -\tilde{\theta} \left( P_D^{-1}(P_D(z)) \right) \cdot 1 \left\{ \tilde{\theta} \left( P_D^{-1}(P_D(z)) \right) < 0 \right\} \right)
\]

\[
= \tilde{\theta}(z),
\]

ensuring that Equation (8) holds.

Step 6. Note that, for any \( z \in \mathcal{Z} \),

\[
\frac{d\mathbb{E} \left[ \tilde{Y} \big| Z = z \right]}{dz} = \frac{dP_D(z)}{dz} \cdot \tilde{\theta}(z)
\]

\[
= a(z) \cdot \frac{dP_T(z)}{dz} \cdot \tilde{\theta}(z) \quad \text{by Step 1}
\]

\[
= a(z) \cdot \frac{dP_T(z)}{dz} \cdot \frac{1}{a(z)} \cdot f(z) \quad \text{by definition}
\]

\[
= \frac{d\mathbb{E} \left[ Y \big| Z = z \right]}{dz} \quad \text{by Equation (3),}
\]

implying that Equation (9) holds.
B Incorrect Rejection of the Generalized Roy Model

Without measurement error, Heckman and Vytlacil (2005, Theorem 1) derive testable implications of Equations (1) and (2) and Assumptions 1-4. For example, they show that \( \mathbb{E}[D \cdot g(Y)|P_D(Z) = p] \) is an increasing function of \( p \), where \( g: \mathbb{R} \rightarrow \mathbb{R}_+ \). However, when using the mismeasured treatment variable and the mismeasured propensity score, this testable restriction of the model may not hold when naively using the mismeasured treatment variable and propensity score in place of the correctly measured ones. To show this result, I find a joint distribution for \((T, D, Y_0, Y_1, U, Z)\) such that Equations (1) and (2) and Assumptions 1-4 hold and \( \mathbb{E}[T \cdot g(Y)|P_T(Z) = p] \) is not an increasing function of \( p \).

Let \( Z \sim \text{Uniform}[0, 1] \), \( U \sim \text{Uniform}[0, 1] \), \( Y_0 \sim \text{Bernouille}(1/5) \) and \( Y_1 \sim \text{Bernouille}(4/5) \) be mutually independent random variables. Define \( g: \mathbb{R} \rightarrow \mathbb{R}_+ \) such that \( g(y) = y \) for any \( y \in \mathbb{R} \), \( D := 1 \{ U \leq Z \} \) and \( T := 1 \{ U \leq 1 - 2 \cdot Z \} \). Note that \( P_D(z) = z \) and \( P_T(z) = 1 - 2 \cdot z \) for any \( z \in [0, 1/2] \). Observe that

\[
\mathbb{E}[T \cdot Y|P_T(Z) = 1/2] - \mathbb{E}[T \cdot Y|P_T(Z) = 1/3] = 1/20 - 1/15 = 1/60,
\]

implying that \( \mathbb{E}[T \cdot Y|P_T(Z) = p] \) is not an increasing function of \( p \).
C Failure of Index Sufficiency

Without measurement error, Heckman et al. (2006) shows that the propensity score satisfy index sufficiency, i.e., \(E[Y|Z = z] = E[Y|PD(Z) = p]\) for any \(z \in Z\) and \(p \in [0, 1]\) such that \(p = PD(z)\). However, the mismeasured propensity score may not satisfy this property. To prove this result, I find a joint distribution for \((T, D, Y_0, Y_1, U, Z)\) such that Equations (1) and (2) and Assumptions 1-4 hold and \(E[Y|Z = z] \neq E[Y|PT(Z) = p]\) for some \(z \in Z\) and \(p \in [0, 1]\) such that \(p = PT(z)\).

Let \(Z \sim \text{Uniform}[0, 1]\), \(U \sim \text{Uniform}[0, 1]\), \(Y_0 \sim N(0, 1)\) and \(Y_1 \sim N(1, 1)\) be mutually independent random variables. Define \(D := 1\{U \leq Z\}\) and \(T := 1\{U \leq 4 \cdot Z^2 - 4 \cdot Z + 1\}\). Note that \(PD(z) = z\) and \(PT(z) = 4 \cdot z^2 - 4 \cdot z + 1\) for any \(z \in [0, 1]\). Observe that \(PT(0) = 1\), \(PT(1) = 1\),

\[
E[Y|Z = 1] = E[Y|PD(Z) = 1] = E[Y_1] = 1
\]

and

\[
E[Y|PT(Z) = 1] = E[Y|Z \in \{0, 1\}]
= E[Y|Z = 0] \cdot P[Z = 0|Z \in \{0, 1\}] + E[Y|Z = 1] \cdot P[Z = 1|Z \in \{0, 1\}]
= \frac{E[Y_0]}{2} + \frac{E[Y_1]}{2}
= \frac{1}{2},
\]

implying that the mismeasured propensity score does not satisfy index sufficiency.
D Simple Sufficient Conditions for Assumptions 5-7

In this appendix, I state simple sufficient conditions that, when combined with Equations (1)-(2) and Assumptions 1-4, ensure that Assumptions 5-7 hold. Moreover, these conditions are stated with specific empirical contexts to illustrate the broad applicability of my theoretical framework to many applied problems. At the end, I impose restrictions on the model’s primitives that imply Assumptions 5-7.

D.1 Assumption 5: Repeated Decision Making

Similarly to Example 1, I analyze another problem when agents with similar behavior have to take the same decision at two points in time and new information is acquired between these two time periods.

Suppose that an entrepreneur wants to borrow from the bank to implement a project and talks to her account manager every time she needs a loan. In this context, $U_T$ is information about the project’s profitability when the entrepreneur talks to her account manager for the first time, $U$ is information about the project’s profitability when she talks to her account manager for the second time, $Z$ is the account’s manager willingness to lend, $T$ is whether the entrepreneur got a loan the first time and $D$ is whether the entrepreneur got a loan at some point in time. Assumption 5 holds in this context under the same assumptions explained in Example 1. Moreover, Assumption 6 may hold depending on the distribution of $U_T$.

D.2 Assumption 6: Random Miscoding

Black et al. (2003) documents that schooling is measured with error in the American Census and in the Current Population Survey (CPS). Assume that, similarly to Carneiro et al. (2011), I want to identify the marginal treatment effect of college attendance ($D$) on wages. To account for measurement error in the treatment variable, I assume that all errors are due to individual inattention when filling the survey questionnaire, i.e., the individual checked the wrong box by mistake. Formally, I assume that the individual’s answer to the
question “Have you ever attended college?” ($T$) is given by

$$T = 1 \{V \leq 0.95\} \cdot D + 1 \{V > 0.95\} \cdot (1 - D),$$

where $V \sim Uniform[0,1]$ captures the individual attention and $V \perp (Z, D)$. Note that, in this case, 5% of the individuals check the wrong box due to inattention. Observe also that, for any $z \in Z$,

$$P_T(z) := \mathbb{P}[T = 1|Z = z]$$

$$= \mathbb{P}[T = 1|Z = z, D = 1] \cdot \mathbb{P}[D = 1|Z = z] + \mathbb{P}[T = 1|Z = z, D = 0] \cdot \mathbb{P}[D = 0|Z = z]$$

$$= 0.95 \cdot P_D(z) + 0.05 \cdot (1 - P_D(z))$$

$$= 0.05 + 0.9 \cdot P_D(z),$$

implying that $\frac{dP_D(z)/dz}{dP_T(z)/dz} = \frac{10}{9}$. Consequently, Assumption 6 holds for $c = \frac{10}{9}$, implying that Assumption 5 holds too.

### D.3 Assumption 7: Dynamic Setting

In Example 3, I show that Assumption 7 holds if $T = 1 \{V \leq 0.95\} \cdot D, V \sim Uniform[0,1], V \perp (Z, D)$ and $P_T$ is invertible. In this subsection, I provide two dynamic examples that fit into the same framework.

#### D.3.1 Eviction Order and Execution

Humphries et al. (2019, Appendix B) discuss the effect of an eviction order when, after the judge’s decision, the sheriff may or may not execute the order. In this case, there are three counterfactual outcomes: $Y_0$ is the outcome of interest when there is no eviction order; $Y_1$ is the outcome of interest when there is a eviction order, but the sheriff does not execute the order; and $Y_2$ is the outcome of interest when there is a eviction order and the sheriff executes the order. Let $D$ denote the eviction order and $T$ denote the execution of the eviction order. Consequently, the observed outcome variable is given by $Y = Y_0 \cdot (1 - D) + Y_1 \cdot D \cdot (1 - T) + \ldots$
For simplicity, I assume that the sheriff’s execution does not matter to the individual, i.e., \( Y_1 = Y_2 \). Consequently, the treatment variable of interest is \( D \), as defined by Humphries et al. (2019). However, I decide to erroneously use \( T \) as my treatment variable. As Figure D.1a makes clear, the measurement error in this problem is similar to the one described in Example 3, i.e., \( D = 0 \) is always correctly measured, but \( D = 1 \) may be mismeasured. As a consequence, Assumption 7 holds in this dynamic setting under the same assumptions described in Example 3.

![Eviction Order and Execution](image)

(a) Eviction Order and Execution

![Returns to High School Education](image)

(b) Returns to High School Education

Figure D.1: Examples: Dynamic Setting

### D.3.2 Returns to High School Education

Assume that I want to compare the wages of three types of individuals: \( Y_0 \) is the counterfactual wage of an agent with 11 years of education; \( Y_1 \) is the counterfactual wage of an agent with 12 years of education and no high school diploma; and \( Y_2 \) is the counterfactual wage of an agent with 12 years of education and a high school diploma. Let \( D \) denote whether the person has 12 (\( D = 1 \)) or 11 years of education and \( T \) denote whether the person has a high school diploma (\( T = 1 \)) or not. Consequently, the observed outcome variable is given by

\[
Y = Y_0 \cdot (1 - D) + Y_1 \cdot D \cdot (1 - T) + Y_2 \cdot D \cdot T.
\]

Following the empirical evidence gathered by Clark and Martorell (2014), I assume that having a high school diploma has no effect, \( Y_1 = Y_2 \). Consequently, the treatment variable of interest is \( D \). However, I decide to erroneously use \( T \) as my treatment variable. As Figure
D.1b makes clear, the measurement error in this problem is similar to the one described in Example 3, i.e., $D = 0$ is always correctly measured, but $D = 1$ may be mismeasured. As a consequence, Assumption 7 holds in this dynamic setting under the same assumptions described in Example 3.

### D.4 Assumptions 5-7: Restrictions on the Model’s Primitives

In this subsection, I adapt to the MTE framework two assumptions that were used by Tommasi and Zhang (2020) to analyze the LATE parameter. Proposition D.1 shows that these two extra assumptions are sufficient conditions for Assumption 5. I also propose two extra assumptions that are sufficient conditions for Assumptions 6 and 7.

**Assumption D.1** The instrumental variable is independent from the measurement error conditional on the latent heterogeneity, i.e., $Z \perp T | U, D$.

**Assumption D.2** The misclassified treatment variable is an informative proxy about the true treatment conditional on the latent heterogeneity, i.e., $\mathbb{P}[T = 1 | U = u, D = 1] > \mathbb{P}[T = 1 | U = u, D = 0]$ for any $u \in [0, 1]$.

**Assumption D.3** There is a known lower bound on the amount of information contained in the misclassified treatment variable, i.e., there exists a known constant $c \in [1, +\infty)$ such that $\mathbb{P}[T = 1 | U = u, D = 1] - \mathbb{P}[T = 1 | U = u, D = 0] \geq \frac{1}{c}$ for any $u \in [0, 1]$.

**Assumption D.4** Function $P_D : Z \rightarrow [0, 1]$ is invertible.

**Proposition D.1** If Assumptions 1-4, D.1 and D.2 hold, then Assumption 5 holds. Moreover, if Assumption D.3 holds too, then Assumption 6 holds. Additionally, if Assumption D.4 holds too, then Assumption 7 holds.

**Proof.** Fix $z \in Z$ arbitrarily. Note that

$$P_T(z) = \mathbb{P}[T = 1 | Z = z]$$

by definition.
\[
\int \mathbb{P}[T = 1 | Z = z, U = u] dF_{U|Z}(u | z) \text{ by the LIE}
\]

\[
= \int_0^1 \mathbb{P}[T = 1 | Z = z, U = u] \, du \text{ by Assumptions 1 and 3}
\]

\[
= \int_0^{P_D(z)} \mathbb{P}[T = 1 | Z = z, U = u, D = 1] \, du
\]

\[
+ \int_{P_D(z)}^1 \mathbb{P}[T = 1 | Z = z, U = u, D = 0] \, du \text{ by the LIE}
\]

\[
= \int_0^{P_D(z)} \mathbb{P}[T = 1 | U = u, D = 1] \, du
\]

\[
+ \int_{P_D(z)}^1 \mathbb{P}[T = 1 | U = u, D = 0] \, du \text{ by Assumption D.1},
\]

implying, by the Leibniz Integral Rule, that

\[
\frac{dP_T(z)}{dz} = \frac{dP_D(z)}{dz} \cdot (\mathbb{P}[T = 1 | U = P_D(z), D = 1] - \mathbb{P}[T = 1 | U = P_D(z), D = 0]). \quad (D.1)
\]

Since \((\mathbb{P}[T = 1 | U = P_D(z), D = 1] - \mathbb{P}[T = 1 | U = P_D(z), D = 0]) > 0\) by Assumption D.2, I have that Assumption 5 holds.

Moreover, if Assumption D.3 holds, then

\[
\frac{dP_D(z)/dz}{dP_T(z)/dz} \in [1, c],
\]

implying that Assumption 6 holds.

Additionally, the Fundamental Theorem of Calculus and Equation (D.1) imply that there exists \(b \in \mathbb{R}\) such that

\[
P_D(z) = \int_{-\infty}^z \frac{dP_D(\tilde{z})}{dz} d\tilde{z} + b
\]

\[
= \int_{-\infty}^z (\mathbb{P}[T = 1 | U = P_D(\tilde{z}), D = 1] - \mathbb{P}[T = 1 | U = P_D(\tilde{z}), D = 0])^{-1} \cdot \frac{dP_T(\tilde{z})}{dz} d\tilde{z} + b.
\]

Consequently, if Assumption D.4 holds too, then Assumption 7 holds. ■
E Extensions

In this appendix, I explore two extensions to the model explained in the main text. In the first subsection, I discuss the case when I observe more than one instrumental variable. In the second subsection, I explain the case when I observe more than one misclassified treatment variable. For brevity, I focus on extending Proposition 2 in both cases.

E.1 Extension: More than One Instrumental Variable

To analyze the case with more than one instrumental variable, I assume that I observe \( M \in \mathbb{N} \) instruments: \( Z_1, \ldots, Z_M \). Consequently, \( Z \) is now a random vector and \( Z \subset \mathbb{R}^M \) denotes its support. I also modify Assumptions 2 and 6 to account for more than one instrumental variable.

Assumption E.1 All partial derivatives of functions \( P_D \) and \( P_T \) are always different from zero, i.e., \( \frac{\partial P_D(z)}{\partial z_m} \neq 0 \) and \( \frac{\partial P_T(z)}{\partial z_m} \neq 0 \) for every \( z \in Z \) and every \( m \in \{1, \ldots, M\} \).

Assumption E.2 The ratios between the partial derivatives of \( P_D \) and \( P_T \) are bounded, i.e., for each \( m \in \{1, \ldots, M\} \), there exists a known \( c_m \in [1, +\infty) \) such that \( \frac{\partial P_D(z)}{\partial z_m} / \partial P_T(z) / \partial z_m \in \left[ \frac{1}{c_m}, c_m \right] \)
for any value \( z \in Z \).

Moreover, I define one LIV estimand \( f_m \) for each instrumental variable \( Z_m \). For any \( m \in \{1, \ldots, M\} \), \( f_m : Z \to \mathbb{R} \) is a function such that, for any \( z \in Z \),

\[
 f_m(z) = \frac{\partial \mathbb{E}[Y|Z = z]}{\partial z_m} / \frac{\partial \mathbb{E}[T|Z = z]}{\partial z_m}.
\]

I, now, extend Proposition 2 to the case with more than one instrumental variable.

Corollary E.1 Suppose Assumptions 1, E.1, 3, 4 and E.2 hold. For each \( m \in \{1, \ldots, M\} \), I have that \( \theta \in \Theta_1^m \), where

\[
 \Theta_1^m := \left\{ \dot{\theta} : Z \to \mathbb{R} \left| \text{For any } z \in Z, |\dot{\theta}(z)| \in \left[ \frac{1}{c_m} \cdot |f_m(z)|, c_m \cdot |f_m(z)| \right] \right. \right\}.
\]
Moreover, for any \( m \in \{1, \ldots, M\} \), \( \tilde{\theta} \in \Theta^m_1 \) and \( d \in (0, +\infty) \), \[ \|\theta - \tilde{\theta}\|_d \leq \left( \frac{c^2 - 1}{c_m} \right) \cdot \|f_m\|_d, \] where \( \|\cdot\|_d \) is the \( L_d \)-norm if \( d < \infty \) and \( \|\cdot\|_d \) is the sup-norm if \( d = \infty \).

Consequently, I have that \[ \theta \in \cap_{m=1}^M \Theta^m_1 \] and \[ \|\theta - \tilde{\theta}\|_d \leq \min_{m \in \{1, \ldots, M\}} \left\{ \left( \frac{c^2 - 1}{c_m} \right) \cdot \|f_m\|_d \right\} \] for any \( \tilde{\theta} \in \cap_{m=1}^M \Theta^m_1 \) and \( d \in (0, +\infty) \).

Finally, note that Corollary E.1 can be used as test of the identifying assumptions in the extended model. If \( \cap_{m=1}^M \Theta^m_1 = \emptyset \), then at least one of Assumptions 1, E.1, 3, 4 and E.2 is invalid.

### E.2 Extension: More than One Misclassified Treatment Variable

Analyzing the case with more than one misclassified treatment variable is potentially interesting to many empirical applications. For instance, many studies use some prediction method to infer the treatment status of each observation (Black et al., 2020; Arellano-Bover, 2020). In those cases, the authors may define one treatment variable for each plausible prediction method and each treatment variable is potentially misclassified due to prediction errors. Even the empirical application in Section 5 could benefit from this extension if I were interested in analyzing the impact of being punished according to the trial judge’s ruling and misclassification was due to the prediction error of different algorithms being used to convert textual trial judge’s rulings to binary treatment variables.

To analyze the case with more than one misclassified treatment variable, I assume that I observe \( Q \in \mathbb{N} \) misclassified treatment variables: \( T_1, \ldots, T_Q \). I also modify Assumptions 2 and 6 to account for more than one misclassified treatment variable.

**Assumption E.3** The derivatives of function \( P_D \) and of the mismeasured propensity scores are always different from zero, i.e., \( \frac{dP_D(z)}{dz} \neq 0 \) and \( \frac{dP^q_T(z)}{dz} \neq 0 \) for every \( z \in \mathcal{Z} \) and
Assumption E.4 The ratios between the derivatives of $P_D$ and $P^q_T$ are bounded, i.e., for each $q \in \{1, \ldots, Q\}$, there exists a known $c_q \in [1, +\infty)$ such that $\frac{dP_D(z)/dz}{dP^q_T(z)/dz} \in \left[\frac{1}{c_q}, c_q\right]$ for any value $z \in Z$.

Moreover, I define one LIV estimand $f_q$ for each misclassified treatment variable $T_q$. For any $q \in \{1, \ldots, Q\}$, $f_q: Z \rightarrow \mathbb{R}$ is a function such that, for any $z \in Z$,

$$f_q(z) = \frac{d\mathbb{E}[Y|Z = z]/dz}{d\mathbb{E}[T_q|Z = z]/dz}.$$

I, now, extend Proposition 2 to the case with more than one misclassified treatment variable.

Corollary E.2 Suppose Assumptions 1, E.3, 3, 4 and E.4 hold. For each $q \in \{1, \ldots, Q\}$, I have that $\theta \in \Theta^q_1$, where

$$\Theta^q_1 := \left\{ \hat{\theta}: Z \rightarrow \mathbb{R} \mid \text{For any } z \in Z, \left| \hat{\theta}(z) \right| \in \left[\frac{1}{c_q} \cdot |f_q(z)|, c_q \cdot |f_q(z)|\right] \right\}.$$

Moreover, for any $q \in \{1, \ldots, Q\}$, $\hat{\theta} \in \Theta^q_1$ and $d \in (0, +\infty)$, $\left\| \theta - \tilde{\theta} \right\|_d \leq \left( \frac{c_q^2 - 1}{c_q} \right) \cdot |f_q|_d$, where $|\cdot|_d$ is the $L_d$-norm if $d < \infty$ and $|\cdot|_d$ is the sup-norm if $d = \infty$.

Consequently, I have that

$$\theta \in \cap_{q=1}^Q \Theta^q_1$$

and

$$\left\| \theta - \tilde{\theta} \right\|_d \leq \min_{q \in \{1, \ldots, Q\}} \left\{ \left( \frac{c_q^2 - 1}{c_q} \right) \cdot |f_q|_d \right\}$$

for any $\tilde{\theta} \in \cap_{q=1}^Q \Theta^q_1$ and $d \in (0, +\infty]$.

Finally, note that Corollary E.2 can be used as test of the identifying assumptions in the extended model. If $\cap_{q=1}^Q \Theta^q_1 = \emptyset$, then at least one of Assumptions 1, E.3, 3, 4 and E.4 is invalid.
F Partial Identification of the LATE Parameter

In this Appendix, I adapt my framework in Section 2 and my results in Section 3 to the binary instrument case, complementing the work developed by Ura (2018, Subsection 3.4) with a sensitivity analysis tool. To analyze the Local Average Treatment Effect (LATE) when the treatment variable is mismeasured, I start with the standard generalized selection model (Heckman and Vytlacil, 1999), which is equivalent to the model in Imbens and Angrist (1994) according to Vytlacil (2002).

\[ Y = Y_1 \cdot D + Y_0 \cdot (1 - D) \]  
\[ D = 1 \{ U \leq P_D (Z) \} \]  \hspace{1cm} (F.1, F.2)

where \( Z \) is an observable binary instrumental variable with support given by \( Z \subset \{0, 1\} \), \( P_D: Z \rightarrow \mathbb{R} \) is a unknown function, \( U \) is a latent heterogeneity variable and \( D \) is the correctly measured treatment. The variable \( Y \) is the realized outcome variable, while \( Y_0 \) and \( Y_1 \) are the potential outcomes when the agent is untreated and treated, respectively.

I augment this model with the possibly mismeasured treatment status indicator, \( T \). The mismeasured treatment is relevant because the researcher observes only the vector \((Y, T, Z)\), while \( Y_1, Y_0, D \) and \( U \) are latent variables.

I use the following assumptions in order to derive my partial identification results.

**Assumption F.1** \( Z \perp \perp (Y_0, Y_1, U) \)

**Assumption F.2** *The minimal required rank assumption is*

\[ \Delta T := P[T = 1|Z = 1] - P[T = 1|Z = 0] \neq 0 \text{ and} \]
\[ \Delta D := P[D = 1|Z = 1] - P[D = 1|Z = 0] \neq 0. \]
However, for ease of notation, I impose

\[ \Delta T := P \{T = 1 | Z = 1\} - P \{T = 1 | Z = 0\} \neq 0 \text{ and } \]
\[ \Delta D := P \{D = 1 | Z = 1\} - P \{D = 1 | Z = 0\} > 0. \]

**Assumption F.3** The distribution of the latent heterogeneity variable \( U \) is absolutely continuous with respect to the Lebesgue measure.

**Assumption F.4** \( \mathbb{E} \{|Y_0|\} < \infty \) and \( \mathbb{E} \{|Y_1|\} < \infty \)

**Assumption F.5** \( \text{sign}(\Delta T) > 0 \)

**Assumption F.6** \( \frac{\Delta D}{\Delta T} \in \left[ \frac{1}{c} , c \right] \) for a known \( c \in [1, +\infty) \)

**Assumption F.7** For a known \( c \in \left[ 0, \frac{1}{\overline{p}} \right] \), there exists an unknown \( \alpha \in \left[ \frac{1}{c} , c \right] \) such that \( P_D(z) = \alpha \cdot P_T(z) \), where \( \overline{p} := \max \{P_T(0), P_T(1)\} = P_T(1) \).

My goal is to derive partial identification results for the Local Average Treatment Effect (LATE) given by \( \beta := \mathbb{E}[Y_1 - Y_0 | U \in [P_D(0), P_D(1)]] \). To achieve this goal, I, first, analyze the consequences of a mismeasured treatment variable on the Wald estimand (Imbens and Angrist, 1994) and, then, derive increasingly strong identification results based on Assumptions F.5-F.7.

If the researcher ignores that the treatment variable is mismeasured, she can compute the Wald estimand using the mismeasured treatment variable \( T \) as if it was the actual treatment variable. The next proposition analyzes which object is identified by this naive approach.

**Proposition F.1 (Wald Estimand)** Under Assumptions F.1-F.4, the Wald estimand satisfies

\[ \frac{\Delta Y}{\Delta T} = \frac{\Delta D}{\Delta T} \cdot \beta, \]  

where \( \Delta Y := \mathbb{E}[Y | Z = 1] - \mathbb{E}[Y | Z = 0] \).

**Proof.** The proof of this proposition follows directly from Imbens and Angrist (1994). ☐
Proposition F.1 shows that, when measurement error is ignored, the Wald estimand does not identify the LATE parameter. This result is discussed in depth by Ura (2018) and Calvi et al. (2021).

Now, adding Assumptions F.5-F.7, I derive increasingly strong identification results for the LATE parameter.

**Corollary F.1 (Identifying the sign of the LATE parameter)** If Assumptions F.1-F.4 and F.5 hold, then \( \text{sign}(\beta) = \text{sign}\left( \frac{\Delta Y}{\Delta T} \right) \).

**Proof.** Under Assumption F.5, I have that \( \frac{\Delta D}{\Delta T} > 0 \) in Equation (F.3).

**Corollary F.2 (Bounds for the LATE parameter)** If Assumptions F.1-F.4 and F.6 hold, then

\[
\beta \in B := \begin{cases} 
\left[ \frac{1}{c} \cdot \frac{\Delta Y}{\Delta T}, \frac{\Delta Y}{\Delta T} \right] & \text{if } \frac{\Delta Y}{\Delta T} > 0 \\
\{0\} & \text{if } \frac{\Delta Y}{\Delta T} = 0 \\
\left[ \frac{\Delta Y}{\Delta T}, \frac{1}{c} \cdot \frac{\Delta Y}{\Delta T} \right] & \text{if } \frac{\Delta Y}{\Delta T} < 0 
\end{cases}
\]

**Proof.** The proof uses Equation (F.3) and the bounds imposed on the ratio \( \frac{\Delta D}{\Delta T} \) by Assumption F.6 to bound the LATE parameter.

**Proposition F.2 (Sharp Identified Set for the LATE Parameter)** Suppose Assumptions F.1-F.4 and F.7 hold. In addition, suppose that the outcome variable \( Y \) is binary and that \( B \subseteq [-1, 1] \). Then, \( B \) is the sharp identified set for \( \beta \) in the sense that, for any \( \tilde{\beta} \in B \), there exist candidate random variables \( \left( \tilde{U}, \tilde{Y}_0, \tilde{Y}_1, \tilde{D} \right) \) and a function \( P_{\tilde{D}} : Z \rightarrow [0, 1] \) such that

1. \( \tilde{D} \) is monotonic with respect to \( Z \) and its index is given by \( P_{\tilde{D}} \), i.e.,

\[
\tilde{D} = 1 \left\{ \tilde{U} \leq P_{\tilde{D}}(Z) \right\} ; \quad (F.4)
\]
2. \( (Z, \tilde{U}, \tilde{Y}_0, \tilde{Y}_1) \) and \( P_D \) achieve the candidate target parameter, i.e.,

\[
\tilde{\beta} = \mathbb{E} \left[ \tilde{Y}_1 - \tilde{Y}_0 \middle| \tilde{U} \in [P_D(0), P_D(1)] \right]; \quad (F.5)
\]

3. \( (Z, \tilde{U}, \tilde{Y}_0, \tilde{Y}_1) \) and \( P_D \) satisfy the data restriction given by

\[
\Delta Y = \Delta \tilde{Y}, \quad (F.6)
\]

where \( \Delta \tilde{Y} := \mathbb{P} \left[ \tilde{Y} = 1 \middle| Z = 1 \right] - \mathbb{P} \left[ \tilde{Y} = 1 \middle| Z = 0 \right] \) and \( \tilde{Y} = \tilde{Y}_1 \cdot \tilde{D} + \tilde{Y}_0 \left( 1 - \tilde{D} \right) \).

4. \( \tilde{Y}_0 \) and \( \tilde{Y}_1 \) satisfy the support condition given by

\[
\tilde{\mathcal{Y}}_0 \subseteq \mathcal{Y}_0 = \{0, 1\} \text{ and } \tilde{\mathcal{Y}}_1 \subseteq \mathcal{Y}_1 = \{0, 1\}, \quad (F.7)
\]

where \( \tilde{\mathcal{Y}}_0, \mathcal{Y}_0 \) and \( \tilde{\mathcal{Y}}_1, \mathcal{Y}_1 \) are the support of \( \tilde{Y}_0, Y_0, \tilde{Y}_1 \) and \( Y_1 \), respectively;

5. \( (Z, \tilde{U}, \tilde{Y}_0, \tilde{Y}_1, \tilde{D}) \) satisfy Assumptions F.1-F.4 and Assumption F.7.

**Proof.** The proof is by construction. For each \( \tilde{\beta} \in B \), I define the density of the candidate random variables \( (\tilde{U}, \tilde{Y}_0, \tilde{Y}_1, \tilde{D}) \) to ensure that they satisfy the five restrictions of Proposition F.2.

Fix \( \tilde{\beta} \in B \) arbitrarily. By definition, there exists \( a \in \left[ \frac{1}{c}, c \right] \) such that \( \tilde{\beta} = \frac{1}{a} \cdot \frac{\Delta Y}{\Delta T} \), where \( c \in \left[ 1, \frac{1}{P} \right] \). I break the construction of the candidate random variables \( (\tilde{U}, \tilde{Y}_0, \tilde{Y}_1, \tilde{D}) \) in six steps.

---

Step 1. Define \( P_D: Z \to [0, 1] \) such that \( P_D(z) = a \cdot P_T(z) \) for any \( z \in Z \), ensuring that Assumptions F.2 and F.7 hold.

Step 2. Define \( \tilde{U} \sim \text{Uniform} [0, 1] \) and \( \tilde{D} := 1 \left\{ \tilde{U} \leq P_D(Z) \right\} \), ensuring that Assumption F.3 and Equation (F.4) hold.

Step 3. Since \( f_{Z, \tilde{U}, \tilde{Y}_0, \tilde{Y}_1} = \mathbb{P} [Z = \cdot] \cdot f_{\tilde{U}} [Z] \cdot \mathbb{P} \left[ \tilde{Y}_0 = \cdot, \tilde{Y}_1 = \cdot \middle| \tilde{U}, Z \right] \), I define the joint density function of \( (Z, \tilde{U}, \tilde{Y}_0, \tilde{Y}_1) \) through its components \( \mathbb{P} [Z = \cdot], f_{\tilde{U}} [Z] \) and \( \mathbb{P} \left[ \tilde{Y}_0 = \cdot, \tilde{Y}_1 = \cdot \middle| \tilde{U}, Z \right] \).
Fix \((z, u, y_{0}, y_{1}) \in \{0, 1\} \times \mathbb{R} \times \{0, 1\}\)² arbitrarily.

(a) Note that \(\mathbb{P}[Z = \cdot] \) is identified. Consequently, \(\mathbb{P}[Z = \cdot] \) is defined according to the data.

(b) Define \(f_{\tilde{U} \mid Z}(u \mid z) = f_{\tilde{U}}(u)\) and \(\mathbb{P}[\tilde{Y}_{0} = y_{0}, \tilde{Y}_{1} = y_{1} \mid \tilde{U} = u, Z = z] = \mathbb{P}[\tilde{Y}_{0} = y_{0}, \tilde{Y}_{1} = y_{1} \mid \tilde{U} = u]\), ensuring that Assumption F.1 holds. Consequently, I only have to define \(f_{\tilde{U}}(u)\)
and \(\mathbb{P}[\tilde{Y}_{0} = y_{0}, \tilde{Y}_{1} = y_{1} \mid \tilde{U} = u]\).

(c) Note that \(f_{\tilde{U}}\) is defined in Step 2.

(d) I impose \(\mathbb{P}[\tilde{Y}_{0} = y_{0}, \tilde{Y}_{1} = y_{1} \mid \tilde{U} = u] = \mathbb{P}[\tilde{Y}_{0} = y_{0} \mid \tilde{U} = u] \cdot \mathbb{P}[\tilde{Y}_{1} = y_{1} \mid \tilde{U} = u]\) for simplicity. Define \(\mathbb{P}[\tilde{Y}_{0} = 0 \mid \tilde{U} = u] = 1 - \mathbb{P}[\tilde{Y}_{0} = 1 \mid \tilde{U} = u]\) and \(\mathbb{P}[\tilde{Y}_{1} = 0 \mid \tilde{U} = u] = 1 - \mathbb{P}[\tilde{Y}_{1} = 1 \mid \tilde{U} = u].\) Consequently, I only have to define \(\mathbb{P}[\tilde{Y}_{0} = 1 \mid \tilde{U} = u]\) and \(\mathbb{P}[\tilde{Y}_{1} = 1 \mid \tilde{U} = u]\).

(e) Define the set \(\mathcal{U} := [P_{\tilde{D}}(0), P_{\tilde{D}}(1)] \subseteq [0, 1].\)

(f) If \(u \notin \mathcal{U}, \mathbb{P}[\tilde{Y}_{0} = 1 \mid \tilde{U} = u] = 0\) and \(\mathbb{P}[\tilde{Y}_{1} = 1 \mid \tilde{U} = u] = 0.\)

(g) From now on, suppose that \(u \in \mathcal{U}.\) Define
\[
\mathbb{P}[\tilde{Y}_{0} = 1 \mid \tilde{U} = u] = -\tilde{\beta} \cdot 1 \{\tilde{\beta} < 0\}
\]
and
\[
\mathbb{P}[\tilde{Y}_{1} = 1 \mid \tilde{U} = u] = \tilde{\beta} \cdot 1 \{\tilde{\beta} > 0\}.
\]

Step 4. Note that \(\tilde{Y}_{0}\) and \(\tilde{Y}_{1}\) satisfy Assumption F.4 and Equation (F.7).

Step 5. Observe that \(\mathbb{E}[\tilde{Y}_{1} - \tilde{Y}_{0} \mid \tilde{U} \in [P_{\tilde{D}}(0), P_{\tilde{D}}(1)]] = \tilde{\beta},\) ensuring that Equation (F.5) holds.

Step 6. Define \(\Delta \tilde{D} := \mathbb{P}[\tilde{D} = 1 \mid Z = 1] - \mathbb{P}[\tilde{D} = 1 \mid Z = 0].\) Note that
\[
\Delta \tilde{Y} = \Delta \tilde{D} \cdot \tilde{\beta}
= a \cdot \Delta T \cdot \tilde{\beta}
= a \cdot \Delta T \cdot \frac{1}{a} \cdot \frac{\Delta Y}{\Delta T}
\]
68
\[ = \Delta Y, \]

implying that Equation (F.6) holds.
G  Details on Corollary 3

In this appendix, I provide explicit formulas for the weighting functions $\omega$ associated with the Average Treatment Effect (ATE), the Average Treatment Effect on the Treated (ATT), the Average Treatment Effect on the Untreated (ATU) and any Policy Relevant Treatment Effect (PRTE, Heckman and Vytlacil, 2001). I also derive numerically easy-to-compute bounds around these treatment effect parameters by imposing that the mismeasured propensity score function is a re-scaled version of the true propensity score.

According to Heckman and Vytlacil (2005, Tables IA and IB), we can write the $ATE := \mathbb{E}[Y_1 - Y_0]$, the $ATT := \mathbb{E}[Y_1 - Y_0|D = 1]$, the $ATU := \mathbb{E}[Y_1 - Y_0|D = 0]$ and any PRTE using the following weighting functions:

$$
\omega_{ATE}(u, P_D) = 1
$$
$$
\omega_{ATT}(u, P_D) = \frac{1 - F_{P_D}(u)}{\mathbb{E}[P_D(Z)]}
$$
$$
\omega_{ATU}(u, P_D) = \frac{F_{P_D}(u)}{1 - \mathbb{E}[P_D(Z)]}
$$
$$
\omega_{PRTE}(u, P_D) = \frac{F_{P^*}(u) - F_{P_D}(u)}{\mathbb{E}[P_D(Z)] - \mathbb{E}[P^*]},
$$

where $u \in [0, 1]$ and $P^*$ is the probability of receiving the treatment under an alternative policy regime.

To derive easy-to-compute bounds around these treatment effect parameters, I strengthen Assumption 7 in the following way:

**Assumption G.1** The function $P_T$ is invertible and, for a known $c \in \left[1, \frac{1}{\mathbb{P}}\right]$, there exists an unknown $\alpha \in \left[\frac{1}{c}, c\right]$ such that $P_D(z) = \alpha \cdot P_T(z)$, where $\mathbb{P} := \sup_{z \in \mathbb{Z}} P_T(z)$.

Note that this assumption imposes that the mismeasured propensity score function is a re-scaled version of the true propensity score. Mathematically, it implies that $\mathcal{A} = \{g \in \mathcal{G} \text{ and } g \text{ is constant.}\}$

---

30 For other treatment effect parameters that can be defined as weighted integrals of the MTE function, see Heckman and Vytlacil (2005) and Mogstad et al. (2018).
{0}. Consequently, under Assumption G.1, the bounds in Corollary 2 become

$$\Theta_2 := \left\{ \tilde{\theta} : Z \to \mathbb{R} \mid \text{For some } a \in \left[ \frac{1}{c}, c \right], \tilde{\theta}(z) = \frac{1}{a} \cdot f(z) \text{ for any } z \in Z. \right\}.$$ 

Note that this result is a more restrictive version of the results derived by Acerenza et al. (2022, Section 4). Moreover, the bounds in Corollary 3 simplify to

$$\inf_{a \in \left[ \frac{1}{c}, c \right]} \int_0^1 \frac{1}{a} \cdot f(P_T^{-1}(u/a)) \, du \leq ATE \leq \sup_{a \in \left[ \frac{1}{c}, c \right]} \int_0^1 \frac{1}{a} \cdot f(P_T^{-1}(u/a)) \, du;$$

$$\inf_{a \in \left[ \frac{1}{c}, c \right]} \int_0^1 \frac{1}{a} \cdot f(P_T^{-1}(u/a)) \cdot \left( \frac{1 - F_{P_T}(u/a)}{a \cdot \mathbb{E}[P_T(Z)]} \right) \, du \leq ATT \leq \sup_{a \in \left[ \frac{1}{c}, c \right]} \int_0^1 \frac{1}{a} \cdot f(P_T^{-1}(u/a)) \cdot \left( \frac{1 - F_{P_T}(u/a)}{a \cdot \mathbb{E}[P_T(Z)]} \right) \, du;$$

$$\inf_{a \in \left[ \frac{1}{c}, c \right]} \int_0^1 \frac{1}{a} \cdot f(P_T^{-1}(u/a)) \cdot \left( \frac{F_{P_T}(u/a)}{1 - a \cdot \mathbb{E}[P_T(Z)]} \right) \, du \leq ATU \leq \sup_{a \in \left[ \frac{1}{c}, c \right]} \int_0^1 \frac{1}{a} \cdot f(P_T^{-1}(u/a)) \cdot \left( \frac{F_{P_T}(u/a)}{1 - a \cdot \mathbb{E}[P_T(Z)]} \right) \, du;$$

$$\inf_{a \in \left[ \frac{1}{c}, c \right]} \int_0^1 \frac{1}{a} \cdot f(P_T^{-1}(u/a)) \cdot \left( \frac{F_{P^*(u)} - F_{P_T}(u/a)}{a \cdot \mathbb{E}[P_T(Z)] - \mathbb{E}[P^*]} \right) \, du \leq PRTE \leq \sup_{a \in \left[ \frac{1}{c}, c \right]} \int_0^1 \frac{1}{a} \cdot f(P_T^{-1}(u/a)) \cdot \left( \frac{F_{P^*(u)} - F_{P_T}(u/a)}{a \cdot \mathbb{E}[P_T(Z)] - \mathbb{E}[P^*]} \right) \, du.$$
H Estimation Details

In this Appendix, my main goal is to estimate the MTE function’s sign (Corollary 1) and its bounds (Proposition 2).

To accomplish this task in my empirical application, I need to take covariates into account because trial judges are only randomly allocated to criminal cases after conditioning on the court district. Consequently, the propensity score becomes

$$P_D(z, x) = P[D = 1 | Z = z, X = x],$$

and the target parameter (MTE function) is now given by

$$\theta(z, x) = E[Y_1 - Y_0 | U = P_D(z, x), X = x]$$

for any value $z$ of the instrument and any value $x$ of the covariates, where the covariates encompass a full set of court district dummies.\(^{31}\)

To estimate the MTE function’s sign and bounds, I need to estimate the LIV estimand

$$f(z, x) = \frac{dE[Y | Z = z, X = x]/dz}{dE[T | Z = z, X = x]/dz}.$$ 

This object is also useful to understand the impact of ignoring misclassification of the treatment variable in my empirical application (Proposition 1).

Moreover, I am able to observe the correctly classified treatment variable $D$ (final sentence in each case) in my empirical application. Consequently, I can point identify the MTE function $\theta(\cdot, \cdot)$ and use it as a benchmark for the partial identification strategy described in Section 3. Estimating my target parameter is my second goal in this appendix. To do so, I need to estimate the correctly measured LIV estimand

$$f^*(z, x) = \frac{dE[Y | Z = z, X = x]/dz}{dE[D | Z = z, X = x]/dz},$$

because $f^*(z, x) = \theta(z, x)$ as a direct consequence of Proposition 1.

\(^{31}\)When the model includes covariates, Assumption 7 imposes that, for any value $x$ of the covariates, $P_D(\cdot, x)$ is invertible as function of its first argument. In a separable model ($P_D(z, x) = g(z) + h(x)$) such as the ones used in this appendix, Assumption 7 imposes that $g$ is invertible.
I explain how to estimate the LIV estimand \( f(\cdot, \cdot) \) and the correctly measured LIV estimand \( f^*(\cdot, \cdot) \) in Subsections H.1 and H.3, respectively. In both subsections, I focus on parametric estimators (Cornelissen et al., 2016, 2018) for brevity. In Subsection H.2, I also explain how to use the estimator of \( f(\cdot, \cdot) \) to estimate the sign of \( \theta(\cdot, \cdot) \) (Corollary 1) and the bounds \( \Theta_1 \) (Proposition 2). In Subsection H.4, I implement a Monte Carlo exercise to illustrate the bias of the LIV estimand \( f(\cdot, \cdot) \) and the finite sample behavior of the estimators described in Subsection H.1 and H.3.

### H.1 Estimation with the Misclassified Treatment Variable

In this subsection, I observe an independent and identically distributed sample \((Y_i, Z_i, X_i, T_i)_{i=1}^N\), where \( N \) is the sample size, index \( i \) denotes a case-defendant pair, \( Y_i \) indicates that defendant \( i \) recidivated within two years of her case’s final decision, \( Z_i \) is the punishment rate of the trial judge who analyzed case \( i \), \( X_i \) is the court district where case \( i \) was analyzed, \( T_i \) is the trial judge’s decision in case \( i \).

Our goal is to estimate the LIV estimand

\[
 f(z, x) = \frac{d\mathbb{E}[Y|Z=z, X=x]}{dz} / \frac{d\mathbb{E}[T|Z=z, X=x]}{dz}
\]

for any value \( z \) of the instrument and any value \( x \) of the covariates.

To estimate the denominator, I use a polynomial model

\[
 \mathbb{E}[T|Z=z, X=x] = \gamma_x + \gamma_1 \cdot z + \ldots + \gamma_L \cdot z^L
\]

with court district fixed effects, where \( L \in \mathbb{N} \). This model’s coefficients can be estimated by ordinary least squares (OLS) and I denote its estimators by \( \hat{\gamma}_x, \hat{\gamma}_1, \ldots, \hat{\gamma}_L \), implying that an estimator for \( \frac{d\mathbb{E}[T|Z=z, X=x]}{dz} \) is given by \( \hat{\gamma}_1 + \ldots + L \cdot \hat{\gamma}_L \cdot z^{L-1} \).

To estimate the numerator, I need to model the propensity score \( P[D_i = 1|Z_i = z, X_i = x] \) and the function \( \mathbb{E}[Y_{1,i} - Y_{0,i}|U_i = u, X_i = x] \). I use polynomial models with court district fixed effects for both objects:

\[
P_D(z, x) = P[D_i = 1|Z_i = z, X_i = x] = P[U \leq \gamma_x^* + \gamma_1^* \cdot z + \ldots + \gamma_L^* \cdot z^L | Z_i = z, X_i = x]
\]
\[ \theta(z, x) = \mathbb{E}[Y_{1,i} - Y_{0,i}] = \beta_x + \beta_1 \cdot u + \ldots + \beta_K \cdot u^K, \quad (H.2) \]

where \( L^* \in \mathbb{N} \) and \( K \in \mathbb{N} \). Based on Equations (H.1) and (H.2), I have that

\[
\theta(z, x) = \mathbb{E}[Y_{1,i} - Y_{0,i}] = P_D(z, x), X_i = x
\]

\[
= \beta_x + \beta_1 \cdot \left( \gamma_x^* + \gamma_1^* \cdot z + \ldots + \gamma_{L^*}^* \cdot z^{L^*} \right) + \ldots + \beta_K \cdot \left( \gamma_x^* + \gamma_1^* \cdot z + \ldots + \gamma_{L^*}^* \cdot z^{L^*} \right)^K
\]

and that

\[
\mathbb{E}[Y_i | Z_i = z, X_i = x] = \mathbb{E}[Y_{0,i} | X_i = x] + \int_0^{P_D(z, x)} \mathbb{E}[Y_{1,i} - Y_{0,i}] = \beta_x + \beta_1 \cdot u + \ldots + \beta_K \cdot u^K \text{ d} u
\]

\[
= \alpha_x + \int_0^{P_D(z, x)} \left( \beta_x + \beta_1 \cdot u + \ldots + \beta_K \cdot u^K \right) \text{ d} u
\]

\[
= \alpha_x + \beta_x \cdot P_D(z, x) + \frac{\beta_1}{2} \cdot [P_D(z, x)]^2 + \ldots + \frac{\beta_K}{K + 1} \cdot [P_D(z, x)]^{K+1}
\]

\[
= \alpha_x + \beta_x \cdot \left[ \gamma_x^* + \gamma_1^* \cdot z + \ldots + \gamma_{L^*}^* \cdot z^{L^*} \right]
\]

\[
+ \frac{\beta_1}{2} \cdot \left[ \gamma_x^* + \gamma_1^* \cdot z + \ldots + \gamma_{L^*}^* \cdot z^{L^*} \right]^2
\]

\[
+ \ldots + \frac{\beta_K}{K + 1} \cdot \left[ \gamma_x^* + \gamma_1^* \cdot z + \ldots + \gamma_{L^*}^* \cdot z^{L^*} \right]^{K+1}
\]

\[
= \delta_x + \delta_{1,x} \cdot z + \ldots + \delta_{L^*,(K+1) - 1,x} \cdot z^{L^*,(K+1) - 1}
\]

\[
+ \delta_{L^*,(K+1)} \cdot z^{L^*,(K+1)}
\]

(\text{H.4})

where \( \alpha_x := \mathbb{E}[Y_{0,i} | X_i = x] \), \( \delta_x, \delta_{1,x}, \ldots, \delta_{L^*,(K+1) - 1,x} \) and \( \delta_{L^*,(K+1)} \) are known functions of \( \gamma_x^*, \gamma_1^*, \ldots, \gamma_{L^*}^*, \beta_x, \beta_1, \ldots, \beta_K \). The coefficients in Equation (\text{H.4}) can be estimated by OLS and I denote its estimators by \( \hat{\delta}_x, \hat{\delta}_{1,x}, \ldots, \hat{\delta}_{L^*,(K+1) - 1,x}, \hat{\delta}_{L^*,(K+1)} \), implying that an estimator for \( d\mathbb{E}[Y|Z = z, X = x]/dz \) is given by \( \hat{\delta}_{1,x} + \ldots + \left( L^* \cdot (K + 1) - 1 \right) \cdot \hat{\delta}_{L^*,(K+1) - 1,x} \cdot z^{L^*,(K+1) - 2} + \left( L^* \cdot (K + 1) \right) \cdot \hat{\delta}_{L^*,(K+1)} \cdot z^{L^*,(K+1) - 1}. \)
Consequently, an estimator for the LIV estimand is given by

\[
\hat{f}(z, x) = \frac{\hat{\delta}_{1,x} + \ldots + (L^* \cdot (K + 1) - 1) \cdot \hat{\delta}_{L^*(K+1)-1,x} \cdot z^{L^*(K+1)-2} + (L^* \cdot (K + 1)) \cdot \hat{\delta}_{L^*(K+1) \cdot z^{L^*(K+1)-1}}}{\hat{\gamma}_1 + \ldots + L \cdot \hat{\gamma}_L \cdot z^{L-1}}.
\]

In my empirical application, I impose that \( L^* = L = K = 1 \), implying that \( \delta_x = \alpha_x + \beta_x \cdot \gamma_x + \frac{\beta_1}{2} \cdot \gamma_x^* \), \( \delta_{1,x} = \beta_x \cdot \gamma_1^* + \beta_1 \cdot \gamma_x \cdot \gamma_1^* \), \( \delta_2 = \frac{\beta_1}{2} \cdot (\gamma_1^*)^2 \) and

\[
\hat{f}(z, x) = \frac{\hat{\delta}_{1,x} + z \cdot \hat{\delta}_2}{\hat{\gamma}_1}.
\] (H.5)

Setting \( L^* = L = K = 1 \) imposes that the functions \( \mathbb{E}[Y_{1,i} - Y_{0,i} | U_i = u, X_i = x] \) and \( P_D(z, x) \) are linear. This linearity assumption is not only common in the empirical literature (Cornelissen et al., 2018), but also seems to be valid in my empirical application. In Appendix J.5, I estimate the function \( \mathbb{E}[Y_{1,i} - Y_{0,i} | U_i = u, X_i = x] \) semi-parametrically (Cornelissen et al., 2016, Appendix B.1) and find that this function is approximately constant. Moreover, my empirical results in Subsection 5.2 and in Appendix J.4 suggest that the propensity score function \( P_D(z, x) \) is linear.

Furthermore, setting \( L^* = L = K = 1 \) imposes that the true MTE function \( \theta(\cdot, \cdot) \) is given by

\[
\theta(z, x) = \beta_x + \beta_1 \cdot (\gamma_x^* + \gamma_1^* \cdot z)
\]

\[
= \beta_x + \beta_1 \cdot \gamma_x^* + \beta_1 \cdot \gamma_1^* \cdot z
\] (H.6)

according to Equation (H.3). This equation highlights the main advantage of the linear approach. Beyond its simplicity when compared against the semi-parametric approach in Appendix J.5, the linearity assumption is also useful to capture the heterogeneous treatment effects in a straightforward way. The entire observed heterogeneity is captured by the intercept \( \beta_x + \beta_1 \cdot \gamma_x^* \), that depends only on the value \( x \) of the covariates. Consequently, I can use a simple histogram to understand the distribution of treatment effects across different covariate values. Furthermore, the entire unobserved heterogeneity is captured by the slope coefficient \( \beta_1 \cdot \gamma_1^* \).
This single parameter indicates whether there is positive or negative selection-into-treatment.

**H.2 Estimating the Sign of \( \theta (\cdot, \cdot) \) and the Bounds of Set \( \Theta_1 \)**

In this subsection, I use the estimator \( \hat{f} (\cdot, \cdot) \) to estimate the sign of \( \theta (\cdot, \cdot) \) (Corollary 1) and the upper and lower bounds of the set \( \Theta_1 \) (Proposition 2).

To estimate the sign of \( \theta (\cdot, \cdot) \), I first define the function \( s (z,x) := \text{sign}(\theta (z,x)) \) for any value \( z \) of the instrument and any value \( x \) of the covariates. Based on Corollary 1, an estimator for \( s (z,x) \) is given by

\[
\hat{s} (z,x) := \text{sign} \left( \hat{f} (z,x) \right). \tag{H.7}
\]

To estimate the upper and lower bounds of set \( \Theta_1 \), I first define its upper bound

\[
\theta_U (z,x) := \sup_{\hat{\theta} \in \Theta_1} \hat{\theta} (z,x)
\]

and its lower bound

\[
\theta_L (z,x) := \inf_{\hat{\theta} \in \Theta_1} \hat{\theta} (z,x),
\]

where

\[
\Theta_1 := \left\{ \tilde{\theta} (\cdot, \cdot) \mid \begin{aligned}
&\text{For any any value } z \text{ of the instrument and any value } x \text{ of the covariates } X, \\
&|\tilde{\theta} (z,x)| \in \left[ 1 - c \cdot |f (z,x)|, c \cdot |f (z,x)| \right].
\end{aligned} \right\}
\]

Based on Proposition 2, estimators for \( \theta_U (z,x) \) and \( \theta_L (z,x) \) are given by

\[
\hat{\theta}_U (z,x) := \begin{cases} 
   c \cdot \hat{f} (z,x) & \text{if } \hat{f} (z,x) \geq 0 \\
   \frac{1}{c} \cdot \hat{f} (z,x) & \text{if } \hat{f} (z,x) < 0
\end{cases} \tag{H.8}
\]

and

\[
\hat{\theta}_L (z,x) := \begin{cases} 
   \frac{1}{c} \cdot \hat{f} (z,x) & \text{if } \hat{f} (z,x) \geq 0 \\
   c \cdot \hat{f} (z,x) & \text{if } \hat{f} (z,x) < 0
\end{cases} \tag{H.9}
\]

respectively.
For brevity, I do not write explicit formulas for the upper and lower bounds of $\Theta_2$ in Corollary 2. They are analogous to $\hat{\theta}_U(z, x)$ and $\hat{\theta}_L(z, x)$, where I replace the true LIV estimand $f(\cdot, \cdot)$ by its estimator $\hat{f}(\cdot, \cdot)$. Estimators for the bounds in Corollary 3 can be written in a similar way.

**H.3 Estimation with the Correctly Classified Treatment Variable**

In this subsection, we observe an independent and identically distributed sample $(Y_i, Z_i, X_i, D_i)_{i=1}^{N}$, where $N$ is the sample size, index $i$ denotes a case-defendant pair, $Y_i$ indicates that defendant $i$ recidivated within two years of her case’s final decision, $Z_i$ is the punishment rate of the trial judge who analyzed case $i$, $X_i$ is the court district where case $i$ was analyzed, $D_i$ is the final decision in case $i$.

Our goal is to estimate the correctly measured LIV estimand $f^*(z, x) = \frac{dE[Y|Z=z, X=x]/dz}{dE[D|Z=z, X=x]/dz}$ for any value $z$ of the instrument and any value $x$ of the covariates. The only difference between this subsection and Subsection H.1 is that, now, I can directly estimate the propensity score (Equation (H.1)) using OLS and I denote its estimators by $\hat{\gamma}_{1}^*, \hat{\gamma}_{2}^*, \ldots, \hat{\gamma}_{L}^*$. Imposing that $L^* = L = K = 1$, an estimator for the correctly measured LIV estimand is given by

$$\hat{f}^*(z, x) = \frac{\hat{\delta}_{1,x} + 2 \cdot \hat{\delta}_{2} \cdot z}{\hat{\gamma}_{1}^*}.$$ (H.10)

Moreover, this object is also an estimator for the true MTE function (Equation (H.6)) as proven by Chalak (2017) in a nonparametric context.

**H.4 Monte Carlo Simulation**

The data generating process (DGP) of this Monte Carlo Simulation is based on the discussion in Appendix D.2 and, consequently, satisfies Assumptions 1-4 and 6. The Monte Carlo’s DGP is given by the following system of equations:

$$D = 1 \{ U \leq \gamma_{2}^* \cdot 1 \{ X = x \} + \gamma_{1}^* \cdot Z \}$$

$$Y_0 = \alpha_x \cdot 1 \{ X = x \} + \epsilon_0$$
\[ Y_1 = \beta_x \cdot \mathbb{1}\{X = x\} + \beta_1 \cdot U + \epsilon_1 \]

\[ T = \mathbb{1}\{V \leq r\} \cdot D + \mathbb{1}\{V > r\} \cdot (1 - D), \]

where \( U \sim Unif(0, 1) \), \( X = 0 \) with probability \( p_0 \), \( X = 1 \) with probability \( p_1 \), \( X = 2 \) with probability \( p_2 = 1 - p_0 - p_1 \), \( Z \sim Unif(0, 1) \), \( \epsilon_0 \sim N(0, \sigma^2) \), \( \epsilon_1 \sim N(0, \sigma^2) \), \( V \sim Unif(0, 1) \), and \( U, X, Z, \epsilon_0, \epsilon_1 \) and \( V \) are mutually independent.

Note that this DGP satisfies the linearity assumptions in Subsections H.1 and H.3 as shown by the following equations:

\[ P_D(z, x) = \gamma_x^* + \gamma_1^* \cdot z \]

\[ E[Y_1 - Y_0 | U = u, X = x] = \beta_x - \alpha_x + \beta_1 \cdot u \]

\[ \theta(z, x) = \beta_x - \alpha_x + \beta_1 \cdot \gamma_x^* + \beta_1 \cdot \gamma_1^* \cdot z \]

\[ E[T | Z = z, X = x] = 1 - r + (2r - 1) \cdot \gamma_x^* + (2r - 1) \cdot \gamma_1^* \cdot z \]

\[ f(z, x) = \frac{\theta(z, x)}{2r - 1}, \]

implying that the misclassification bias of the LIV estimand \( f(\cdot, \cdot) \) is given by

\[ \text{bias}(z, x) = f(z, x) - \theta(z, x) = \theta(z, x) \cdot \left[ \frac{2 - 2r}{2r - 1} \right]. \quad (H.11) \]

Observe that, for \( r \in (0.5, 1] \), the misclassification bias is a decreasing function of \( r \) and has the same sign of \( \theta(z, x) \). As a consequence of the last property, note that the misclassification bias moves the point-estimates away from zero. Observe also that, if the misclassification rate is too large (\( r \in [0, 0.5) \)), Assumption 5 does not hold anymore, implying that misclassification bias is so intense that the LIV estimand \( f \) does not capture the correct sign of the true MTE function \( \theta \).

I choose the following set of parameters for this Monte Carlo Simulation: \( \gamma_{x=0}^* = 0.1, \gamma_{x=1}^* = 0.2, \gamma_{x=2}^* = 0.3, \gamma_1^* = 0.6, \alpha_{x=0} = \alpha_{x=1} = \alpha_{x=2} = 0, \beta_{x=0} = 0, \beta_{x=1} = 0.5, \beta_{x=2} = 1, \beta_1 = -1, p_0 = p_1 = 1/3 \) and \( \sigma^2 = 0.5 \). Moreover, I set \( r = 0.95 \) so that the amount of
misclassification in this Monte Carlo simulation is similar to the share of cases whose trial
judges’ sentences are reversed in my empirical application.\textsuperscript{32} Moreover, sample size $N$ is equal
to 3,000 and the number of Monte Carlo repetitions $M$ is equal to 10,000. Finally, in each
Monte Carlo repetition, I estimate the bounds from Proposition 2 using four values of $c$: $10/9$,
$12/9$, $14/9$, $16/9$. Note that, in this Monte Carlo’s DGP, the smallest constant $c$ that satisfies
Assumption 6 is equal to $1/(2 \cdot r - 1) = 10/9$.

This example illustrates the complexity of the misclassification bias and its possibly large
magnitude. Given the chosen parameters, the misclassification bias is always negative for
$x = 0$, always positive for $x = 2$, and changes its sign at $z = 0.5$ for $x = 1$, illustrating its
complexity when estimating entire functions. Furthermore, the bias represents 11.1\% of the
true MTE effect, illustrating that it can be large even when the misclassification rate is as
small as 5\%. If the misclassification rate was as large as the share of reversed criminal cases in
State Courts in the U.S. (12\%, Waters et al., 2015), the misclassification bias would represent
31.6\% of the true MTE effect.

Using this Monte Carlo exercise, I can compute the bias of the correctly measured LIV
estimator $\hat{f}^* (\cdot, \cdot)$ and of the mismeasured LIV estimator $\hat{f} (\cdot, \cdot)$. Subfigure H.1a shows that
the bias of $\hat{f}^* (\cdot, \cdot)$ is very small, reaching values that are no larger than 0.004 in magnitude.
This result is expected because the parametric model is correctly specified in this simulation.
Moreover, Subfigure H.1b shows that the bias of $\hat{f} (\cdot, \cdot)$ is much larger than the bias of $\hat{f}^* (\cdot, \cdot)$,
reaching values as large as 0.075 in magnitude.

Using this Monte Carlo exercise, I can also compute the coverage rate of the estimated
bounds $\hat{\theta}_U (\cdot, \cdot)$ and $\hat{\theta}_L (\cdot, \cdot)$. Formally, I can estimate $P [\theta (z, x) \in \left[ \hat{\theta}_L (z, x), \hat{\theta}_U (z, x) \right]]$ for the
data generating process described above and each value $z$ of the instrument and each value $x$
of the covariates. Figure H.2 shows the coverage rate for each covariate value $x$, separated in
different subfigures and denoted by different line types, and each possible value of $c$, denoted
by different line colors.

First, note that, when $c = 10/9$ (orange line), the coverage rate is small regardless of the
covariate value $x$. This result is expected because $\hat{\theta}_L (z, x) \overset{p}{\rightarrow} \theta (z, x)$ when $c = 10/9$ and the

\textsuperscript{32}See Subection 5.1.
estimated bounds do not account for sampling uncertainty. If I constructed bootstrapped confidence bands around the estimated bounds, the coverage rate would be equal to the nominal confidence level.

Moreover, when \( c \) increases to \( \frac{12}{9} \) (dark blue line), \( \frac{14}{9} \) (light blue line) and \( \frac{16}{9} \) (purple line), the coverage rate increases monotonically. This result is also expected because the true MTE function \( \theta \) is in the interior of \( \Theta_1 \) when \( c \) increases, reducing the importance of sampling uncertainty.

Second, note that the coverage rate is smaller on the left side of Subfigure H.2a, on the middle of Subfigure H.2b and on the right side of Subfigure H.2c. This decrease in the coverage rate is caused by a MTE function \( \theta(z, x) \) that is close to zero in those regions. When the true MTE function \( \theta \) is close to zero, the LIV estimator \( \hat{f} \) will incorrectly estimate the sign of \( \theta \) due to sampling uncertainty. As a consequence of this mistake, the estimated bounds will not cover the true MTE function \( \theta \) by construction.

In the extreme case when the true MTE function \( \theta \) is exactly zero as it happens when \( z = 0.5 \) and \( x = 1 \) (Subfigure H.2b), the coverage rate is exactly zero. This negative result is expected because the LIV estimator \( \hat{f} \) is different from zero with probability one due to
Sampling uncertainty. Consequently, the estimated bounds will not cover zero by construction with probability one.

For a similar reason, the coverage rate is higher away from the left corner of Subfigure H.2a, away from the center of Subfigure H.2b and away from the right corner of Subfigure H.2c. In those regions, the true MTE function $\theta$ is far away from zero and the LIV estimator $\hat{f}$ is more likely to estimate the sign of $\theta$ correctly. As a consequence, the estimated bounds have a higher probability of covering the true MTE function when $\theta$ is far away from zero.

Despite this last mechanism, the coverage rate decreases when $z$ is very large in Subfigure H.2a and when $z$ is very small in Subfigure H.2c. Even though the true MTE function achieves its largest magnitude in those regions, the coverage rate is slightly smaller there than it is in the center of these two subfigures. The reason for this phenomenon is that the residual variance conditional on $Z$ is larger in those regions, increasing sampling uncertainty and decreasing the coverage rate of the estimated bounds. If I constructed bootstrapped confidence bands around the estimated bounds, this problem would disappear because the higher residual variance would generate wider confidence bands.
I Constructing the Dataset

In this appendix, I provide a detailed explanation on how I constructed the dataset used in my empirical application. I will explain the specific crime types that are included in my sample, the classification algorithms that were used to define which defendants were punished and the fuzzy matching algorithm that I used to define which defendants recidivate.

The final dataset was created from four initial datasets.

1. CPOPG ("Consulta de Processos de Primeiro Grau"): It contains information about all criminal cases in the Justice Court System in the State of São Paulo (TJ-SP) between 2010 and 2019. Its variables are described below.

   (a) id: An unique case identifier that can link cases across all datasets from TJ-SP.

   (b) status: The case’s status define whether the trial judge has achieved her final decision in the trial or not, i.e., whether the case is open or not.

   (c) subject: It denotes each case’s crime type.

   (d) class: The case’s class defines whether the case’s objective is to analyze whether a defendant is guilty or not. For example, some criminal cases aim to start a police investigation or to arrest a person before the trial judge’s sentence.

   (e) assignment: This variable contains information about the case’s starting date and whether the case was randomly assigned to a judge within the case’s court district or whether the case was assigned to a judge that was already analyzing a connected case.

   (f) trialjudge: This variable contains the trial judge’s full name.

   (g) parties: This variable contains a list with the names of all the parties involved in the case, including defendants, prosecutors, defense attorneys and public defenders.

2. CJPG ("Consulta de Julgados de Primeiro Grau"): It contains information about the last decision made by a trial judge in all criminal cases in TJ-SP between 2010 and 2019. Its variables are described below.
(a) id: An unique case identifier that can link cases across all datasets from TJ-SP.

(b) date: It denotes the date of the last decision made by the case's trial judge.

(c) courtdistrict: It provide the court district’s name.

(d) sentence: It provides the full text of the trial judge’s final decision.

3. CPOSG (“Consulta de Processos de Segundo Grau”): It contains information about all appealing criminal cases in TJ-SP between 2010 and 2019. Its variables are described below.

(a) id: An unique case identifier that can link cases across all datasets from TJ-SP.

(b) parties: This variable contains a list with the names of all the parties involved in the case, including defendants, prosecutors, defense attorneys and public defenders.

(c) composition: It contains the name of the three Appeals judges who analyzed the appealing case, including their positions within the case (judge-rapporteur, revising judge, voting judge).

(d) decision: It contains the Appeals Court’s final ruling.

(e) date: It denotes the date of the last decision made by the Appeals Court.

4. Public Defenders’ names: This dataset is a list with full names of all public defenders in the State of São Paulo from 2011 to 2019. It was constructed directly by the Public Defender’s Office after a FOIA request. The Brazilian FOIA is known as “Lei de Acesso à Informação” and is regulated by Law n. 12.527/11.

Starting from the CPOPG dataset, I implement the following steps.

1. I only keep cases that are currently in the Appeals Court (status equal to “Em grau de recurso” or “Em grau de recurso | (Tramitação prioritária)”), closed (status equal to “Extinto”, “Extinto | (Tramitação prioritária)” or “Arquivado”) or whose status is empty. Those cases are already associated with a trial judge’s sentence.

2. I only keep cases whose crime types are associated with sentences that must be less than four years of incarceration. In particular, I keep cases whose subject is equal to
“Atentado Violento ao Pudor” (sexual assault), “Decorrente de Violência Doméstica” (domestic violence), “Violência Doméstica Contra a Mulher” (domestic violence against a woman), “Contravenções Penais” (misdemeanors), “Furto” (theft), “Furto (art. 155)” (theft), “Furto Privilegiado” (qualified theft), “Furto de coisa comum” (theft of a common good), “Desacato” (contempt), “Recepção” (receiving stolen goods), “Ameaça” (threat), “Violação de direito autoral” (copyright violation), “Crimes contra a Propriedade Intelectual” (crimes against intellectual property), “Posse de Drogas para Consumo Pessoal” (drug consumption), “Apropriação indébita” (undue appropriation), “Apropriação indébita (art. 168, caput)” (undue appropriation), “Quadrilha ou Bando” (criminal conspiracy), “Desobediência” (disobedience), “Resistência” (resistance), “Fato Atípico” (atypical fact), “Crimes de Abuso de Autoridade” (abuse of authority), “Crime Culposo” (crime without criminal intent), “Dano Qualificado” (qualified harm), “Violação de domicílio” (trespassing), “Favorecimento real” (illegal favoring), “Comunicação falsa de crime ou de contravenção” (false criminal communication), “Destruição / Subtração / Ocultação de Cadáver” (destruction, subtraction or concealment of a corpse), “Difamação” (libel) and “Injúria” (insult).

3. I only keep cases that aim to analyze whether a defendant is guilty or not. In particular, I drop all the cases whose `class` is equal to “Execução da Pena” (sentence execution), “Habeas Corpus Criminal” (habeas corpus), “Execução Provisória” (temporary sentence execution), “Inquérito Policial” (police investigation), “Procedimento Investigatório Criminal (PIC-MP)” (police investigation), “Auto de Prisão em Flagrante” (pre-trial arrest), “Pedido de Prisão Preventiva” (pre-trial arrest), “Medidas Protetivas de urgência (Lei Maria da Penha) Criminal” (urgent protective acts), “Relatório de Investigações” (police report), “Ação Penal de Competência do Júri” (jury action), “Mandado de Segurança Criminal” (judicial mandate), “Pedido de Busca e Apreensão Criminal” (judicial mandate), “Representação Criminal/Notícia de Crime” (crime notification) and “Termo Circunstanciado” (report).

4. I only keep cases that were randomly assigned to trial judges. In particular, I keep cases
whose assignment contain the word “Livre”.

5. I only keep cases whose starting date is after January 1st, 2010.

After these steps, my dataset contains 98,552 cases. I, then, merged it with the CJPG dataset using cases’ id codes. Since some cases do not have id codes, my dataset now contains 98,422 cases.

After this step, I randomly select 35 cases per year (2010-2019) for manual classification. I manually classify them into five categories: “defendant died during the trial”, “defendant is guilty”, “defendant accepted a non-prosecution agreement” (“transação penal” in Portuguese), “case was dismissed” (“processo suspenso” in Portuguese) and “defendant was acquitted”. Since some sentences are missing or incomplete, I am able to manually classify only 325 sentences.

Now, I use those 325 manually classified cases to train a classification algorithm. To do so, I divide them into a training sample (216 cases) and a validation sample (109 sentences).

First, I design an algorithm to identify which defendants died during the trial. To do so, I check whether the sentence contains any reference to the first paragraph of Article 107 from the Brazilian Criminal Code. This specific part of the Brazilian Criminal Code states that a dead defendant cannot be punished in any way. In both my samples, I find that this simple algorithm perfectly classifies cases into the category “defendant died during the trial”.

Second, I design an algorithm to identify which cases were dismissed. To do so, I check whether the sentence contains any reference to Article 89 in Law n. 9099/95. This specific law article defines the criteria for dismissing a case. In both my samples, I find that this simple algorithm correctly classifies 98% of the cases into the category “case was dismissed”. The few cases that were misclassified are cases that were initially dismissed, but reopened because the defendant committed a second crime.

Third, I design an algorithm to identify which defendants accepted a non-prosecution agreement. To do so, I check whether the sentence contains any of the following expressions: “cumprimento da transação penal” (non-prosecution agreement was fulfilled), “Homologo a proposta” (I accept the proposition), “homologo a transação” (I accept the non-prosecution
agreement), “HOMOLOGO O ACORDO” (I accept the agreement), “proposta transaçao” (A
non-prosecution agreement was proposed), “transaçao penal” (non-prosecution agreement),
“Acolho a proposta” (I accept the proposal) and “aceitacao da proposta” (proposal accep-
tance). Those expressions were selected because, when manually classifying the sentences, I
noticed that they were strong signals of a defendant who accepted a non-prosecution agree-
ment. In both my samples, I find that this simple algorithm correctly classified almost all the
cases into the category “defendant accepted a non-prosecution agreement”, making only three
mistakes. In the misclassified sentences, the judge mentioned that the prosecutor proposed a
non-prosecution agreement, but the defendant missed the agreement’s session.

Finally, I design an algorithm to classify the remaining cases into two categories: “de-
fendant is guilty” and “defendant was acquitted”. To do so, I define a bag of words that
were found to be strong signals of acquittal and guilt when I manually classified the cases
in my samples. This bag of words contains the following expressions: “absolv” (all words
related to acquittal contain this expression in Portuguese), “art. 107, inciso IV” (the fourth
paragraph of Article 107 from the Brazilian Criminal Code defines that a defendant cannot be
punished if he or she is not guilty) and related expressions, “extinta a punibilidade” (it means
that the defendant cannot be punished) and related expressions, “improcedente” (unfounded),
“prescriçao” (statute of limitations), “conden” (all words related to punishment contain this
equation in Portuguese), “pena” (sentence), “procedente” (well-founded), “cumprimento da
pena” (sentence is fulfilled) and related expressions, “dosimetria” (dosimetry of the penalties)
and related expressions, and “rol dos culpados” (book of the guilty). I, then, count how many
times each one of those expressions appear in each sentence and I normalize those counts to
be between 0 and 1.

Using the normalized counts, I train six algorithms using my training sample: k-Nearest
Neighbors, Random Forest, L2-Regularized Logistic Regression, L1-Regularized Logistic Re-
gression, Naive Bayes and xgboost. I, then, validate those algorithm using my validation sam-
ple and find that the k-Nearest Neighbors algorithm correctly classifies 95.3% of the cases, the
Random Forest algorithm correctly classifies 96.5% of the cases, the L2-Regularized Logistic
Regression algorithm correctly classifies 97.7% of the cases, the L1-Regularized Logistic Regression algorithm correctly classifies 98.8% of the cases, the Naive Bayes algorithm correctly classifies 84.9% of the cases and the xgboost algorithm correctly classifies 91.9% of the cases. Given these success rates, I use the L1-Regularized Logistic Regression algorithm to define the treatment variable in my full sample.

Having designed the above algorithms, I use them to define the misclassified treatment variable $T$ in the full sample. First, I find which defendants died during their trials and drop them from my sample. I, then, use the second and third algorithm to define which cases were dismissed and which cases are associated with a non-prosecution agreement. Moreover, I use the trained L1-regularized Logistic Regression algorithm to classify the remaining cases into the categories “defendant is guilty” and “defendant was acquitted”. Finally, I combine the categories “defendant was acquitted” and “case was dismissed” into the untreated group (“not punished”, $T = 0$) and the categories “defendant accepted a non-prosecution agreement” and “defendant is guilty” into the treated group (“punished”, $T = 1$). At the end, my dataset contains 96,225 cases.

Now, I merge my current dataset with the CPOSG dataset using each case’s id code. When merging these datasets, I create an indicator variable that denotes which cases went to the Appeals Court, i.e., which cases were matched. I, then, randomly select 50 cases per year for manual classification (2010-2019) and divide them into three categories: “cases that went to the Appeals Court, but were immediately returned due to bureaucratic errors”, “cases whose trial judge’s sentences were affirmed” and “cases whose trial judge’s sentences were reversed”.

Now, I use those 500 manually classified cases to train a classification algorithm. To do so, I divide them into a training sample (300 cases) and a validation sample (200 sentences).

First, I design an algorithm to identify which cases went to the Appeals Court, but were immediately returned. To do so, I simply check whether Appeals Court’s decision is empty.

Finally, I design an algorithm to classify the non-empty cases into two categories: “cases whose trial judge’s sentences were affirmed” and “cases whose trial judge’s sentences were reversed”.
reversed”. To do so, I define a bag of words that were found to be strong signals of sentence reversal when I manually classified the cases in my sample. This bag of words contains the following expressions: “absolv” (all words related to acquittal contain this expression in Portuguese), “art. 107, inciso IV” (the fourth paragraph of Article 107 from the Brazilian Criminal Code defines that a defendant cannot be punished if he or she is not guilty) and related expressions, “extinta a punibilidade” (it means that the defendant cannot be punished) and related expressions, “prescrição” (statute of limitations), “negaram provimento” (it means that the Appeals Court affirmed the trial judge’s sentence) and related expressions, “deram provimento” (it means that the Appeals Court reversed the trial judge’s sentence) and related expressions, and “parcial provimento” (it means that the Appeals Court reduced the penalty established by the trial judge) and related words. I, then, count how many times each one of those expressions appear in each sentence and I normalize those counts to be between 0 and 1.

Using the normalized counts, I train six algorithms using my training sample: k-Nearest Neighbors, Random Forest, L2-Regularized Logistic Regression, L1-Regularized Logistic Regression, Naive Bayes and xgboost. I, then, validate those algorithm using my validation sample and find that the k-Nearest Neighbors algorithm correctly classifies 97.8% of the cases, the Random Forest algorithm correctly classifies 97.8% of the cases, the L2-Regularized Logistic Regression algorithm correctly classifies 97.2% of the cases, the L1-Regularized Logistic Regression algorithm correctly classifies 96.2% of the cases, the Naive Bayes algorithm correctly classifies 95.6% of the cases and the xgboost algorithm correctly classifies 96.1% of the cases. Given the success rates in this dataset and in the dataset that focus on trial judges’ sentences, I use the L1-Regularized Logistic Regression to define the treatment variable in my full sample.

Having designed the above algorithms, I use them to define the correctly classified treatment variable $D$ in the full sample. First, I set $D = T$ if a case did not go to the Appeals Court or if a case went to the Appeals Court, but was immediately returned. Second, I use the trained L1-Regularized Logistic Regression algorithm to classify the remaining cases into
the categories “reversed trial judge’s sentence” and “affirmed trial judge’s sentence”. I, then, set \( D = T \) if the trial judge’s sentence was affirmed and \( D = 1 - T \) if the trial judge’s sentence was reversed. Moreover, I also drop the cases whose dates (starting date, trial judge’s sentence date and Appeal Court’s decision date) are not appropriately ordered. At the end, my dataset contains 95,119 cases.

Now, my goal is to find the defendants’ names in each case. To do so, I use the variable \texttt{parties} from the CPOPG dataset and search for names listed as “réu”, “ré”, “indiciado”, “indiciada”, “denunciado”, “denunciada”, “coré”, “coréu”, “investigado”, “infrator”, “acusado”, “autordofato”, “autoradofato”, “averiguada”, “averiguado”, “infrator”, “querelado”, “querelada”, “representado”, “reqdo” and “reqda”. Moreover, I use the variable \texttt{parties} from the CPOSG dataset and search for names listed as “apelante”, “recorrente”, “requerente”, “apelado”, “corréu”, “recorrido”, “apelada”, “réu”, “corré” and “querelado”. Furthermore, I analyze the full sentences from the CJPG dataset and search for names listed as “Réu:”, “Ré:”, “RÉ:”, “Ré”, “Ré”, “Autor do Fato:”, “Autor da Fato:”, “Autor(a) do Fato:”, “Indiciado:”, “Indiciada:”, “Sentenciado:”, “Sentenciada:”, “Sentenciado(a):”, “Querelado:”, “Querelada:”, “Averiguado:”, “Averiguada:”, “Sujeito Passivo:”, “Denunciada:”, “Denunciado:”, “Requerido:” and “Requerida:”. Finally, I delete names that are not a person’s name — such as district attorney offices, public defender offices and “unknown author” — or names that are listed in the Public Defenders dataset. My sample now contains 103,423 case-defendant pairs.

Furthermore, I repeat the steps in the last paragraph to find defendants’ names in a dataset that contains all cases from the CPOPG dataset, including cases that are still open and cases with severe crimes. To build this dataset, I followed the steps described above, but did not subset my sample based on the variables \texttt{status} and \texttt{subject}. Moreover, when subsetting my sample based on the variable \texttt{class}, I only dropped the cases whose \texttt{class} was equal to “Execução da Pena”, “Habeas Corpus Criminal”, “Execução Provisória”, “Pedido de Busca e Apreensão Criminal” and “Termo Circunstanciado”. At the end, this dataset

\[33\] All these expression are associated with the word “defendant” in Portuguese.

\[34\] All these expression are associated with the word “defendant” or “appealing party” in Portuguese.

\[35\] All these expression are associated with the word “defendant” in Portuguese.
contains 1,027,120 case-defendants pairs.

Now, I use these two datasets to define my outcome variable \( Y \) (=“recidivism within 2 years of the final sentence”). A defendant \( i \) in a case \( j \) in the smaller dataset recidivated \( (Y_{ij} = 1) \) if and only if defendant \( i \)’s full name appears in a case \( \tilde{j} \) in the larger dataset and if case \( \tilde{j} \)’s starting date is within 2 years after case \( j \)’s final sentence’s date. To match defendants’ names across cases, I use the Jaro–Winkler similarity metric (Winkler, 1990) and I define a match if the similarity between full names in two different cases is greater than or equal to 0.95.

At the end, I delete the case-defendant pairs whose cases started in 2018 and 2019 because their outcome variable is not properly defined due to right-censoring. Consequently, my dataset contains 51,731 case-defendants pairs. This dataset is used in my empirical analysis as described in Subsection 5.1.

J Additional Empirical Results

J.1 Bounds around the MTE function \( \theta(\cdot, \cdot) \) — Confidence Bands

J.2 Misclassification Bias: Intercept and Slope Coefficients

In this appendix, I discuss the bias that is introduced in a linear model by a misclassified treatment variable. In the main text, I estimated the MTE function using a linear model whose intercept varies according to the court district and the slope is common across court districts. Here, I discuss how the intercepts and the slope change when I use the correctly classified treatment variable \( D \) or the misclassified treatment variable \( T \).

Figure J.2 shows the histogram of the estimated intercepts. I show the distribution of intercepts based on the correctly classified treatment variable \( (\hat{\delta}_{1, \cdot}/\hat{\gamma}_{1} \) in Equation (H.10)) on the left and the distribution of intercepts based on the misclassified treatment variable \( (\hat{\delta}_{1, \cdot}/\hat{\gamma}_{1} \) in Equation (H.5)) on the right. Note that these distributions differ slightly, as summarized by a correlation equal to .79 between the two types of intercepts.

Figure J.3 shows the estimated slopes when using the correctly classified treatment variable
Notes: The orange lines are the estimated MTE functions $\theta(\cdot, \cdot)$ based on the LIV estimator $\hat{f}^*$ that uses the correctly classified treatment variable $D$ (Equation (H.10)). The dark blue lines are the point-estimates of the LIV estimator $\hat{f}$ that uses the misclassified treatment variable $T$ (Equation (H.5)). The light blue solid lines are the estimated upper and lower bounds $\hat{\theta}_U$ and $\hat{\theta}_L$ based on a constant $c = 1.1$ (Equations (H.8) and (H.9)). The light blue dotted lines are bootstrapped 90%-confidence bands (1,000 repetitions) around the estimated bounds.

$(2 \cdot \hat{\delta}_1 / \gamma_1^2$ in Equation (H.10)) and when using the misclassified treatment variable $(2 \cdot \hat{\delta}_2 / \gamma_1$ in Equation (H.5)). According to the bootstrapped 90%-confidence intervals written in the text boxes, these slopes are imprecisely estimated and are not statistically significant. This result suggests that the unobserved heterogeneity is possibly unimportant when discussing the effect of alternative sentences on recidivism.

Figure J.3 also shows the misclassification bias in the slope coefficient (third column). Even though the bias is imprecisely estimated, its estimate is relatively large, representing
(a) MTE Function \( \theta(\cdot, \cdot) \) — Estimated Intercepts based on \( \hat{f}^* \)

(b) LIV Estimator \( \hat{f} \) — Estimated Intercepts using the misclassified treatment \( T \)

Figure J.2: LIV Estimators \( \hat{f}^* \) and \( \hat{f} \): Estimated Intercepts

Notes: Subfigure J.2a shows the histogram of the estimated intercepts of the MTE functions \( \theta(\cdot, \cdot) \) for each court district in the sample (Equation (H.10)). Subfigure J.2b shows the histogram of the estimated intercepts of the LIV estimands \( \hat{f}(\cdot, \cdot) \) for each court district in the sample (Equation (H.5)). I only report the intercepts associated to functions whose image is contained in the unit interval. I report the intercepts whose functions are always negative in orange, the intercepts whose functions are always positive in blue and the intercepts whose functions change signs in white. The text boxes describe the share of intercepts that are reported in the subfigures.

Figure J.3: LIV Estimators \( \hat{f}^* \) and \( \hat{f} \): Estimated Slopes

Notes: The first two blue dots report the estimated slope coefficients of the LIV estimators \( \hat{f}^* \) and \( \hat{f} \) (Equations (H.10) and (H.5)). Their bootstrapped 90%-confidence intervals (1,000 repetitions) are reported in the text boxes. The third blue dot is the estimated difference between the two slope coefficients. The blue line is the bootstrapped 90%-confidence interval (1,000 repetitions) around the estimated difference.

10.1% of the estimated slope based on the correctly classified treatment variable (first column). This result highlights that misclassification bias can be severe in a real-world application.
J.3 Results based on 2SLS Estimation

In this appendix, I evaluate the effect of alternative sentences on recidivism using standard 2SLS squares with fixed effects. The outcome variable denotes whether the defendant recidivate within 2 years after the case’s final ruling. The treatment variable is either whether the defendant was punished according to the trial judge’s ruling or whether the defendant was punished according to the final ruling. The instrument is either a full set of trial judges' dummies ("Judge FE") or the leave-one-out punishment rate.

All regressions in this appendix control for a full set of court district fixed effects. Differently from the main text, standard errors are clusterized at the court district level.

Table J.1 shows the results of the four regressions described above. Subsection 5.2 discuss their results.

Table J.1: Effect of Alternative Sentences on Recidivism: 2SLS Estimation

| Overall | Trial Judge’s Ruling | Final Ruling |
|---------|-----------------------|--------------|
|         | Judge FE | Punishment Rate | Judge FE | Punishment Rate |
| 2SLS Estimate | .049** | .053* | .048** | .054* |
| Cluster S.E. | (.020) | (.031) | (.021) | (.032) |
| District FE | ✓ | ✓ | ✓ | ✓ |
| 1st Stage’s F-Stat | 1.76 | 246 | .568 | 275 |

Note: The table reports the estimated effect of alternative sentences on recidivism based on standard two-stage least squares regressions. The treatment variable is described by the first heading in the columns, while the instrumental variables are described by the second heading in the columns. The first stage F-statistic is the test statistic of a $\chi^2$-test whose null hypothesis is that all the coefficients associated with the instrumental variables are equal to zero in the first stage regression. Robust standard errors clustered at the court district level are reported in parenthesis. Significance levels are indicated by *$p \leq 0.10$, **$p \leq 0.05$ and ***$p \leq 0.01$.

J.4 Semiparametric Estimation of the Propensity Score Functions

In this appendix, I estimate the functions $P_D$ and $P_T$ semiparametrically. To do so, I use the semi-parametric estimator proposed by Robinson (1988) combined with the local linear estimator proposed by Calonico et al. (2019).

Figure J.4 presents the results: the solid lines represent the estimated correctly measured propensity score functions while dashed lines represent the mismeasured propensity score func-
tions. Each court district (Campinas, Guarulhos, Ribeirão Preto and Santos) are represented by a different color.

![Graph showing the relationship between Punishment Rate and Propensity Score across different court districts.](image)

Figure J.4: Semiparametric Estimates of the Propensity Score Functions

Notes: The solid lines are the semiparametrically estimated $P_D$ functions using the semi-parametric estimator proposed by Robinson (1988). The dashed lines are the semiparametrically estimated $P_T$ functions using the semi-parametric estimator proposed by Robinson (1988). Colors indicate the court district of associated to each curve.

### J.5 Standard Approach to MTE Estimation

In this appendix, I estimate the function $\mathbb{E}[Y_1 - Y_0 | U = u, X = x]$ defined by Heckman et al. (2006). To do so, I use the semi-parametric estimator proposed by Robinson (1988) and described by Cornelissen et al. (2016, Appendix B.1). Figure J.5 present the results: while it shows the results based on the correctly measured propensity score $P_D$ in orange, it shows the results based on the mismeasured propensity score $P_T$ in dark blue. Subsection 5.2 discuss the results.
Figure J.5: Standard MTE Functions ($\mathbb{E}[Y_1 - Y_0 | U = u, X = x]$) — Semi-parametric Estimation

Notes: The orange solid lines are the estimated MTE functions using the semi-parametric estimator described by Cornelissen et al. (2016, Appendix B.1) and the correctly classified treatment variable $D$. The dark blue solid lines are the estimated MTE functions using the semi-parametric estimator described by Cornelissen et al. (2016, Appendix B.1) and the misclassified treatment variable $T$. The dotted lines are bootstrapped 90%-confidence bands (100 repetitions) around the estimated MTE functions and follow the same color scheme as the solid lines.