MISCLASSIFICATION IN DIFFERENCE-IN-DIFFERENCES MODELS

AUGUSTINE DENTEH AND DÉSIRÉ KÉDAGNI

Tulane University and UNC-Chapel Hill

Abstract. The difference-in-differences (DID) design is one of the most popular methods used in empirical economics research. However, there is almost no work examining what the DID method identifies in the presence of a misclassified treatment variable. This paper studies the identification of treatment effects in DID designs when the treatment is misclassified. Misclassification arises in various ways, including when the timing of a policy intervention is ambiguous or when researchers need to infer treatment from auxiliary data. We show that the DID estimand is biased and recovers a weighted average of the average treatment effects on the treated (ATT) in two subpopulations—the correctly classified and misclassified groups. In some cases, the DID estimand may yield the wrong sign and is otherwise attenuated. We provide bounds on the ATT when the researcher has access to information on the extent of misclassification in the data. We demonstrate our theoretical results using simulations and provide two empirical applications to guide researchers in performing sensitivity analysis using our proposed methods.

Keywords: Difference-in-differences, average treatment effect on the treated, misclassification.

JEL subject classification: C14, C31, C35, C36.

Date: The present version is as of August 2, 2022. This draft is preliminary. We thank Santiago Acerenza, Kyunghoon Ban, and Pierre Nguimkeu for helpful discussions and comments. All errors are ours. Augustine Denteh: 6823 St. Charles Avenue, 310 Tilton Hall, New Orleans, LA 70118, USA. Email address: adenteh@tulane.edu; Désiré Kédagni (Corresponding author): 102 Gardner Hall, CB 3305 University of North Carolina Chapel Hill, NC 27599, USA. Email address: dkedagni@unc.edu.
1. Introduction

The difference-in-differences (DID) method is a popular quasi-experimental technique used to identify causal parameters when data is available on the pre- and post-treatment periods. As of 2018, Currie, Kleven, and Zwiers (2020) reports that 25 percent of all National Bureau of Economic Research working papers in applied microeconomics and 15 percent of papers in “top five” economics journals mention DID. Recently, the DID method has drawn significant attention from methodological researchers working to clarify several identification and estimation issues (see reviews in Roth et al., 2022; de Chaisemartin and D’Haultfœuille, 2022). However, when the treatment variable is observed with errors (in which case we say the treatment is misclassified), the DID estimand may not have a clear causal interpretation even when the identifying parallel trends (PT) assumption holds.

Misclassification can arise in DID designs from many sources. One common scenario occurs when there is ambiguity regarding the exact timing of a reform’s passage or implementation. For instance, when a significant amount of time elapses between a legislation’s proposed date and its eventual enactment, the researcher may opt to use the former to define the timing of treatment (Kresch, 2020). Researchers might also lack information about the actual implementation of a policy when there is a lag between the passage and its effective implementation or when they use the lag to define the post-intervention period with the aim of allowing sufficient time for the policy’s impact to kick in (Murray et al., 2016; Bindler and Hjalmarsson, 2018). Even when researchers know the true treatment date, data unavailability might compel them to define overlapping pre- and post-intervention periods that introduce misclassification (Buchmueller, DiNardo, and Valletta, 2011).

In other cases, researchers may need to estimate or infer the treatment variable from auxiliary data, thereby potentially introducing misclassification (Fortson, 2009; de Chaisemartin and D’Haultfœuille, 2018; Cortes, 2013). In their attempt to perform DID estimation with binary treatments, researchers sometimes use a threshold approach to classify treated and control units based on a mismeasured continuous treatment variable. This is often the case when units are classified as treated when an estimated index or rate exceeds a specified cutoff (Miller, 2012; Kessler and Bruce, 2022). The resulting binary treatment variable in such instances is necessarily misclassified. Even when the underlying continuous variable is correctly measured, researchers may need to dichotomize it to define a binary treatment to capture the intensity of exposure to some national policy (Draca, Machin, and Van Reenen, 2011; Galasso and Luo, 2022). Some studies resort to using a proxy variable (e.g., residence in specific geographical areas) to classify units because the treatment variable is not
observed for either the pre- or post-intervention period (Groen and Polivka, 2008). Most of the above studies admit the misclassification problems confronting them, but there is a conspicuous lack of methodological work addressing it.

In this paper, we study the identification of the average treatment effect on the treated (ATT) in the DID framework when the treatment is subject to misclassification. We characterize the resulting bias and propose a partial identification approach that researchers can use to investigate the sensitivity of their DID estimates. Specifically, we make three contributions to the literature. First, this paper appears to be the first to study the identification of causal effects in the DID setting when the treatment is misclassified. We show that under the standard PT assumption, the DID estimand recovers a weighted average of the ATT for the correctly classified and misclassified subpopulations with a non-positive weight for the misclassified units. Furthermore, the weights do not necessarily sum up to one. In general, the direction of the bias is unknown, implying that the DID estimand may induce a sign-reversal phenomenon, where its sign could differ from the true causal effect.

Second, we establish a linear relationship between the ATT and the DID estimand where the coefficients are unidentified. This relationship allows us to discuss conditions under which various types of biases may occur. Using this relationship, we then provide a sufficient condition for the existence of a fixed point where the DID estimand still identifies the ATT even in the presence of misclassification.

Third, we show that under additional assumptions, the DID method identifies the sign of the ATT but remains biased in magnitude. In particular, if the misclassification error is nondifferential and a monotonicity condition holds, then the DID estimand only suffers from attenuation bias—it produces smaller estimates in magnitude. When the extent of the misclassification is bounded by a sensitivity parameter, we derive bounds on the ATT under the aforementioned assumptions. The choice of the sensitivity parameter is specific to each application and we suggest that researchers use institutional or contextual knowledge and the structure of their data to inform their choice.

We illustrate our theoretical results through simulation and empirical exercises. In the simulations, we consider various designs—differential vs nondifferential misclassification, and symmetric vs asymmetric misclassification. The simulation results display sign reversal in the DID estimates when the measurement error is differential regardless of whether the misclassification is symmetric or not. In the case of nondifferential misclassification, we find that the attenuation bias can be substantial depending on the design.
We conclude the paper by revisiting two empirical studies. The first is an analysis of federalism in the Brazilian water and sanitation sector (Kresch, 2020). The second is an investigation of the impact of a change in punishment severity due to abolishing capital punishment in England between 1772 and 1871 (Bindler and Hjalmarsson, 2018). We discuss the possibilities that the treatment is misclassified in these studies and present our sensitivity bounding analysis based on reasonable choices of the sensitivity parameter.

We situate our paper at the intersection of two strands of literature on difference-in-differences designs and measurement error. Our work is directly related to the longstanding literature on the identification of causal parameters when a binary treatment variable is misclassified (Aigner, 1973; Frazis and Loewenstein, 2003; Mahajan, 2006; Lewbel, 2007; Battistin and Sianesi, 2011; Kreider et al., 2012; Battistin, De Nadai, and Sianesi, 2014; Bollinger and van Hasselt, 2017; Chalak, 2017; Ura, 2018; DiTraglia and Garcia-Jimeno, 2019; Nguimkeu, Denteh, and Tchernis, 2019; Jiang and Ding, 2020; Tommasi and Zhang, 2020; Kasahara and Shimotsu, 2021; Acerenza, Ban, and Kédagni, 2021; Possebom, 2021). Although these studies and the additional papers cited therein cover many quasi-experimental designs, including instrumental variable models, they do not consider misclassification in the DID framework. Our paper also connects with the recent literature exploring a related but different problem of missing data in DID analysis. These studies provide point and partial identification results in the DID framework when the treatment variable is missing for either the pre- or post-treatment period (Botosaru and Gutierrez, 2018; Fan and Manzanares, 2017).

The remainder of the paper is organized as follows. Section 2 presents the model, the assumptions, and the main results. Section 3 presents the simulations results, while Section 4 shows the two empirical illustrations. Section 5 concludes. Proofs of the main results are relegated to the appendix.

2. Analytical Framework

Our framework is the canonical DID design comprising two groups and two periods. Consider the following model:

\[
\begin{align*}
Y_t & = [Y_1(1)D^* + Y_1(0)(1 - D^*)] \mathbb{1}\{t = 1\} + Y_0(0)\mathbb{1}\{t = 0\}, \\
D & = D^*(1 - \varepsilon) + (1 - D^*)\varepsilon
\end{align*}
\]

(2.1)

where the vector \((Y_0, Y_1, D)\) represents the observed data, while the vector \((Y_0(0), Y_1(0), Y_1(1), D^*, \varepsilon)\) is latent. In this model, the variable \(Y_0(0)\) is the potential outcome in the baseline period 0.
when no individual/unit is treated. The variables $Y_1(0)$ and $Y_1(1)$ are the potential outcomes that would have been observed in the post-intervention period 1 had the treatment been externally set to 0 and 1, respectively. The variables $Y_0, Y_1 \in \mathcal{Y}$ are the observed outcomes in the baseline and the post-intervention periods, respectively. The variable $D^* \in \{0, 1\}$ is the true treatment occurring between periods 0 and 1, while $D$ is a potentially misclassified version of $D^*$. The latent variable $\varepsilon$ is the indicator for misclassification. When $\varepsilon = 0$, there is no misclassification, but the observed treatment is misclassified whenever $\varepsilon = 1$.

As is customary in the DID literature, model (2.1) assumes away any anticipatory effects of the treatment, so that $Y_0(1) = Y_0(0)$. We also assume $0 < \mathbb{P}(D^* = 1) < 1$, and $0 < \mathbb{P}(D = 1) < 1$, implying that a fraction of the population is treated whether or not the observed treatment is misclassified. In this paper, we are interested in identifying the ATT defined as

$$ATT = \mathbb{E}[Y_1(1) - Y_1(0)|D^* = 1].$$

(2.2)

We state the assumptions needed to identify the ATT and present our findings.

**Assumption 1 (Parallel Trends).**

$$\mathbb{E}[Y_1(0) - Y_0(0)|D = 1] = \mathbb{E}[Y_1(0) - Y_0(0)|D = 0].$$

Assumption 1 is the standard parallel trends assumption commonly used in the literature on difference-in-differences. It states that in the absence of treatment, the control and treatment groups would have followed the same trend on average. It is equivalent to

$$\mathbb{E}[Y_1(0)|D = 1] - \mathbb{E}[Y_1(0)|D = 0] = \mathbb{E}[Y_0|D = 1] - \mathbb{E}[Y_0|D = 0].$$

In the observed model, the standard DID estimand can be defined as

$$\theta_{DID} = \theta^1_{OLS} - \theta^0_{OLS},$$

where $\theta^0_{OLS}$ and $\theta^1_{OLS}$ are the ordinary least squares (OLS) estimands at periods 0 and 1, respectively given by

$$\theta^1_{OLS} = \mathbb{E}[Y_1|D = 1] - \mathbb{E}[Y_1|D = 0],$$

$$\theta^0_{OLS} = \mathbb{E}[Y_0|D = 1] - \mathbb{E}[Y_0|D = 0].$$

Note that Assumption 1 is stated in terms of the observed treatment variable (instead of the true, unobserved treatment). This is because when the researcher suspects that the
treatment variable is misclassified, they may naturally make Assumption 1 to proceed with the DID identification strategy for several reasons. They might do so because they want to ignore the problem, (incorrectly) assert that misclassification has minimal consequences or for convenience due to lack of a viable solution (i.e., alternative estimation method). As a result, we study the causal interpretation of the DID estimand under Assumption 1. However, before we present the results, we provide sufficient conditions on $(D^*, \varepsilon, Y_1(0), Y_0(0))$ for Assumption 1 to hold.

**Assumption 2.**

1. **Parallel trends with the true treatment $D^*$**
   \[
   \mathbb{E} [Y_1(0) - Y_0(0)|D^* = 1] = \mathbb{E} [Y_1(0) - Y_0(0)|D^* = 0].
   \]

2. **Parallel trends for correctly classified and misclassified groups within each true treatment arm**
   \[
   \mathbb{E} [Y_1(0) - Y_0(0)|D^* = d^*, \varepsilon = 1] = \mathbb{E} [Y_1(0) - Y_0(0)|D^* = d^*, \varepsilon = 0] \quad \text{for each } d^* \in \{0, 1\}.
   \]

The first part of Assumption 2 is the standard parallel trends assumption stated in terms of the true treatment variable. The second part states that conditional on the true treatment variable, the average outcomes for the correctly classified and misclassified groups would have followed the same trend. This parallel trends assumption permits different average outcome trends over time across the misclassification groups (defined by $\varepsilon$) in each treatment arm. The following lemma shows that these two assumptions imply Assumption 1.

**Lemma 1.** In model (2.1), Assumption 2 implies Assumption 1.

### 2.1. The DID Estimand under Arbitrary Misclassification

This section provides our main results for the consequences of misclassification in the DID framework. We first allow misclassification to be arbitrary (potentially differential) with no structure imposed on it. The following proposition provides an expression of the DID estimand when the misclassified treatment variable is used for identification.

**Proposition 1.** Suppose that model (2.1) along with Assumption 1 holds. Then, the DID estimand using the misclassified treatment variable can be decomposed as:

\[
\theta_{DID} = \mathbb{E} [Y_1(1) - Y_1(0)|D^* = 1, \varepsilon = 0] \mathbb{P}(\varepsilon = 0|D = 1) \\
- \mathbb{E} [Y_1(1) - Y_1(0)|D^* = 1, \varepsilon = 1] \mathbb{P}(\varepsilon = 1|D = 0).
\] (2.3)
Proposition 1 shows that the DID estimand does not recover the true ATT in equation (2.2), but rather a weighted average of the ATT for two subpopulations—the correctly classified and misclassified treated groups. Importantly, the weights are positive for the correctly observed units and non-positive for the misclassified observations, and do not necessarily sum up to one. It follows that the DID estimand could yield an opposite sign for the treatment effect in some circumstances. Proposition 1 shows that the standard DID estimand could be negative even if the true ATT is positive under misclassification and vice versa. Indeed, we note from equation (2.2) that

\[
ATT = \mathbb{E}[Y_1(1) - Y_1(0)|D^* = 1, \varepsilon = 1] \mathbb{P}(\varepsilon = 1|D^* = 1) + \mathbb{E}[Y_1(1) - Y_1(0)|D^* = 1, \varepsilon = 0] \mathbb{P}(\varepsilon = 0|D^* = 1).
\]

(2.4)

If the quantities \( \mathbb{E}[Y_1(1) - Y_1(0)|D^* = 1, \varepsilon = 0] \) and \( \mathbb{E}[Y_1(1) - Y_1(0)|D^* = 1, \varepsilon = 1] \) are positive, then from equation (2.4), we know that the ATT is positive. It is then straightforward to check that Proposition 1 implies the DID estimand could be negative in this scenario. In other words, the DID estimand using a misclassified treatment fails to identify an interesting causal parameter and could potentially yield misleading conclusions. Our pessimistic results suggest that the consequences of misclassification in DID analysis are more severe than previously understood. This finding is contrary to observations in some previous empirical papers where the researchers concerned about possible misclassification suggest that their estimates are potentially only attenuated (e.g., Miller, 2012; Draca, Machin, and Van Reenen, 2011; Galasso and Luo, 2022).

To elaborate on the nature of the resulting bias due to misclassification, we combine equations (2.3) and (2.4) to obtain the following relationship between the \( ATT \) and \( \theta_{DID} \).

**Proposition 2.** Suppose that model (2.1) along with Assumption 1 holds. Then, we have:

\[
ATT = \frac{\mathbb{P}(D = 1)}{\mathbb{P}(D^* = 1)} \theta_{DID} + \frac{\mathbb{E}[(Y_1(1) - Y_1(0))\varepsilon|D^* = 1]}{\mathbb{P}(D = 0)}.
\]



\[1\]For instance, in reference to a binary treatment variable created from a potentially mismeasured continuous variable representing the 2005 county-level uninsurance rate in Massachusetts (Uninsured2005c), Miller (2012) observes that “One advantage of using a binary indicator, rather than a continuous measure, is that it is not reliant on the assumption of a linear relationship between insurance coverage and emergency room usage and is more robust to measurement error in the variable Uninsured2005c.” Also, Galasso and Luo (2022) remarks that “Second, because of the threshold approach that we use to define the treatment and control groups, the control subclasses also include implant patents. In principle, this will cause attenuation bias and lead to an underestimation of the impact of the increase in liability.”
Proposition 2 shows that the ATT is linearly related to the DID estimand under misclassification, albeit the coefficients are not identified from the observed data. Nonetheless, it is instructive to use this relationship to study the ensuing bias.

Let the slope coefficient be denoted by \( A = \frac{P(D=1)}{P(D^*=1)} \). Also, denote the numerator and denominator of the intercept term by \( B = \mathbb{E}[(Y_1(1) - Y_1(0))\varepsilon | D^* = 1] \), and \( C = P(D = 0) \), respectively. Furthermore, suppose that the probability of having a false positive is less than that of having a false negative, i.e., \( P(D^* = 0, \varepsilon = 1) < P(D^* = 1, \varepsilon = 1) \). This implies \( A < 1 \). Under these assumptions, Figure 1 provides a graphical illustration of the bias in the DID estimand under misclassification based on Proposition 2. The blue line is drawn assuming that \( B > 0 \) and the red line represents the 45-degree line. In this case, the sign of the treatment effect is not identified for certain positive values. That is, when the ATT is positive, the DID takes on the wrong—negative—sign whenever \( 0 < ATT < \frac{B}{C} \).

The sign-reversal region is indicated by the gray shaded area in Figure 1. Further, when the ATT lies between \( \frac{B}{C} \) and \( \frac{B}{C(1-A)} \), the DID estimand is biased downwards (attenuation bias). The DID estimand yields an expansion bias whenever \( ATT > \frac{B}{C(1-A)} > 0 \).

Figure 1. Graphical illustration of the DID estimand under misclassification.

---

\(^2\)For example, \( B > 0 \) could occur when the individual treatment effect (or gain), \( Y_1(1) - Y_1(0) \), is positively correlated with the likelihood of misclassification.
To summarize, when \( \theta_{DID} < \frac{B}{C(1-A)} \), the DID estimand is biased downward and it is biased upwards when \( \theta_{DID} > \frac{B}{C(1-A)} \). When \( \theta_{DID} = \frac{B}{C(1-A)} \), the DID estimand is equal to the ATT, even in the presence of misclassification. This latter scenario corresponds to the fixed point in Figure 1. In Corollary 5, we provide a sufficient condition under which the fixed point result may occur.

We conclude this section with two special cases when we continue to allow arbitrary forms of misclassification. In the first case, we examine one-sided misclassification and assume that only false positives are present. Unidirectional measurement error has been studied in previous works (e.g., Nguimkeu, Denteh, and Tchernis, 2019). An example of one-sided misclassification in DID settings is one discussed in an analysis of the impact of Hurricane Katrina on labor market outcomes of evacuees (Groen and Polivka, 2008). In that study, the treated group was correctly measured in the post-treatment period as those who evacuated due to the storm. However, before the storm, the treated units are subject to misclassification (false positives) because they were determined based on their residence in affected areas. False positives arise because some of those who lived in affected areas before the storm may not have evacuated in the aftermath of the storm, hence not truly treated.

Corollary 1. Suppose that Assumption 1 holds, and there are no false negatives, i.e., \( \mathbb{P}(D^* = 1, \varepsilon = 1) = 0 \). Then,

\[
\theta_{DID} = \mathbb{P}(\varepsilon = 0 | D = 1) \text{ATT (attenuation bias)}
\]

Corollary 1 shows that even when misclassification is arbitrary, its consequence for DID estimation is less severe when the misclassification is one-sided. In the next section, we provide alternative conditions under which attenuation bias can occur.

In the second special case, we consider what happens when misclassification arises following a Roy selection mechanism such that units are misclassified when their treatment effect exceeds some threshold.

Assumption 3 (Roy selection misclassification). \( \varepsilon = 1 \{Y_1(1) - Y_1(0) > q\} \)

Corollary 2. Suppose that Assumptions 1 and 3 hold. Then, the following inequality holds:

\[
\text{ATT} \geq \frac{\mathbb{P}(D = 1)}{\mathbb{P}(D^* = 1)} \theta_{DID} + q \frac{\mathbb{P}(\varepsilon = 1 | D^* = 1)}{\mathbb{P}(D = 0)}.
\]
Corollary 2 implies that if \( q \) is positive and the DID estimand \( \theta_{DID} \) is also positive, the sign of the ATT is identified as positive. But, it is also possible that \( \theta_{DID} \) be negative while the \( ATT \) is positive. When the threshold \( q \) is “sufficiently” large, we can ensure that the \( ATT \) is positive regardless of the value of \( \theta_{DID} \). The proof of this corollary is straightforward from Proposition 2 and Assumption 3.

### 2.2. The DID Estimand under Nondifferential Misclassification.

We now show how the DID estimand fares under additional assumptions on the nature of misclassification. The literature on measurement error often invokes additional assumptions when the researcher believes that the nature of misclassification is uncorrelated with potential outcomes. We show that if the measurement error is nondifferential and a monotonicity condition holds, then the DID estimand yields an attenuation bias.

**Assumption 4** (Nondifferential misclassification). \( \varepsilon \perp \parallel (Y_1(1), Y_1(0)) \mid D^* \)

Assumption 4 states that conditional on the true (unobserved) treatment, misclassification is independent of the potential outcomes. This type of measurement error is likely in some empirical contexts, especially when the misclassification stems from uncertainty regarding the timing of treatment (e.g., when the researcher uses a reform’s proposed date instead of its date of passage).

**Assumption 5** (Monotonicity condition). \( P(\varepsilon = 1 \mid D = 1) + P(\varepsilon = 1 \mid D = 0) < 1. \)

Assumption 5 is equivalent to the well-known monotonicity condition in Hausman, Abrevaya, and Scott-Morton (1998), which states that the sum of false positive and false negative rates of misclassification may not exceed one, i.e., \( P(\varepsilon = 1 \mid D^* = 1) + P(\varepsilon = 1 \mid D^* = 0) < 1. \) In the appendix, we prove the equivalence between Assumption 5 and the monotonicity condition in Hausman, Abrevaya, and Scott-Morton (1998). Assumption 5 is a minimal requirement to ensure that the misclassification problem is not too severe to render the project infeasible.

Under Assumption 4, we have \( E[Y_1(1) - Y_1(0) \mid D^*, \varepsilon] = E[Y_1(1) - Y_1(0) \mid D^*] \). Hence, the following corollary holds.

**Corollary 3.** Under Assumptions 1, 4 and 5, we have:

\[
\theta_{DID} = \text{ATT} \left( 1 - P(\varepsilon = 1 \mid D = 1) - P(\varepsilon = 1 \mid D = 0) \right),
\]

(2.5)

---

3See Bound, Brown, and Mathiowetz (2001) for additional discussions on various types of measurement errors.
where $1 - \mathbb{P}(\varepsilon = 1|D = 1) - \mathbb{P}(\varepsilon = 1|D = 0) \in (0, 1]$.

Corollary 3 shows that the standard DID method under Assumptions 1, 4 and 5 produces smaller effects in magnitude when the treatment is misclassified (attenuation bias).

We next show that when the researcher knows or can estimate the extent of misclassification, we can bound the ATT by scaling the DID estimate accordingly. We, therefore, introduce the following assumption and derive the bounds in the subsequent corollary.

**Assumption 6** (Known upper bound on the extent of misclassification).

\[
\mathbb{P}(\varepsilon = 1|D = 1) + \mathbb{P}(\varepsilon = 1|D = 0) \leq \lambda < 1.
\]

**Corollary 4.** Under Assumptions 1, 4, 5, and 6, we have:

\[
\min\left\{ \frac{\theta_{DID}}{1 - \lambda}, \frac{\theta_{DID}^*}{1 - \lambda} \right\} \leq ATT \leq \max\left\{ \frac{\theta_{DID}}{1 - \lambda}, \frac{\theta_{DID}^*}{1 - \lambda} \right\}. \tag{2.6}
\]

Corollary 4 provides bounds on the ATT based on the DID estimates and $\lambda$, which plays the role of a sensitivity parameter. Larger values of $\lambda$ imply wider bounds for the ATT, while smaller values of $\lambda$ yield tighter bounds. In the special case where $\lambda = 0$ (no misclassification), the bounds collapse to a point, and the ATT is point-identified as the standard DID estimand.

As mentioned earlier, we now provide a sufficient condition that yields a fixed point result under nondifferential misclassification. Here, the DID method is theoretically unaffected by the presence of misclassification.

**Corollary 5.** Suppose that Assumptions 1 and 4 hold. Suppose also that $\mathbb{P}(D^* = 1) \neq \mathbb{P}(D = 1)$. Whenever $1 - \mathbb{P}(\varepsilon = 1|D = 1) - \mathbb{P}(\varepsilon = 1|D = 0) = \frac{\mathbb{P}(\varepsilon = 1, D^* = 1)}{\mathbb{P}(D = 0, \mathbb{P}(D^* = 1, \varepsilon = 1) - \mathbb{P}(D^* = 0, \varepsilon = 1))}$, then $ATT = \theta_{DID}$, and the presence of misclassification in the treatment variable will not induce bias in the DID estimand.

Corollary 5 provides a sufficient condition under which there exists a fixed point in the relationship between the $ATT$ and the DID estimand. At the fixed point, the DID estimand is robust to any misclassification in the treatment variable.
In this section, we present Monte Carlo simulations to illustrate the consequences of misclassification in DID designs and investigate the usefulness of our proposed bounds. Our simulations cover several data generation processes (DGPs) corresponding to various types (differential and nondifferential) and degrees of misclassification.

**Simulation design.** All our simulation designs have the same basic structure but differ in terms of the nature of misclassification. We draw potential outcomes based on the two-period model in (2.1) as follows:

\[
\begin{align*}
Y_0(0) &\sim D^*U_{[-6,0]} + (1 - D^*)U_{[0,2]} + u_0, \\
Y_1(1) &\sim D^*U_{[-3,3]} + (1 - D^*)U_{[3,5]} + u_1, \\
Y_1(0) &= Y_0(0),
\end{align*}
\]

where \(Y_0(0)\) and \(Y_1(d), d \in \{0, 1\}\), are the pre-treatment and post-intervention period potential outcomes, respectively; \(U_{[a,b]}\) are continuous uniform random variables on the interval \([a, b]\); \(u_j, j \in \{0, 1\}\), are standard normal random variables; and \(D^*\) is the true, unobserved treatment. To focus on the misclassification problem, all our designs maintain the parallel trends assumption by setting \(Y_1(0) = Y_0(0)\).

We generate the true treatment indicators as Bernoulli random variables with a success probability of 0.5. The researcher observes a possibly misclassified treatment variable depending on the misclassification dummy, \(\varepsilon\). When misclassification is differential, we generate the misclassification dummy as \(\varepsilon = \mathbb{1}\{Y_1(1) - Y_1(0) > q\}\), where \(q\) denotes an appropriately chosen quantile of the distribution of \(Y_1(1) - Y_1(0)\) to obtain the desired level of misclassification. For nondifferential misclassification, \(\varepsilon = D^*\text{Bernoulli}(p) + (1 - D^*)(1 - \text{Bernoulli}(p))\), where the success probability, \(p\), determines the degree of misclassification.

In both cases (differential misclassification or otherwise), we consider various types of misclassification. We examine the scenario where the misclassification is symmetric or asymmetric across the true treatment states. We refer to misclassification as symmetric when we have equal rates of false negatives and false positives; the errors are asymmetric when those error rates are different. We also study the special case where the misclassification is one-sided, with only false positives or false negatives being present.

The observed data is given by \(\{Y_0, Y_1, D\}\), where the outcomes (pre- and post-treatment) and treatment variable are respectively governed by the following observation mechanism.
For outcomes, \( Y_0 = Y_0(0) \) and \( Y_1 = Y_1(1)D^* + Y_1(0)(1 - D^*) \), and for treatment, \( D = D^*(1 - \varepsilon) + (1 - D^*)\varepsilon \).

**Simulation results.** Tables 1 and 2 present the results of the Monte Carlo study for differential and nondifferential misclassification, respectively. We generate samples of size 10,000 and aggregate the results across 10,000 iterations. The true treatment effect—the ATT—equals 3 in all the experiments.

Under differential misclassification of treatment, Table 1 shows that the DID estimator is severely biased and sometimes yields a sign opposite of the true treatment effect. This general finding aligns with our main theoretical result in Proposition 1. Even when the parallel trends assumption holds, we cannot generally sign the ensuing bias in DID estimation when misclassification is allowed to be differential.

The DID estimates have the wrong (negative) signs in all instances (Panels A through C) except for the case where only false positives are present. We find that the sign switching of the DID estimate occurs with at least a 20% unconditional misclassification rate in Panels B and C but with at least 30% in Panel A. All else equal, one likely reason why the sign reversal occurs at higher levels of misclassification in Panel A is that the errors are symmetric and the biases (from false negatives and false positives) might cancel out more evenly, leading to a less severe overall bias. The sign-reversal result we obtain in the DID context when the parallel trends assumption holds bears resemblance to the consequences of measurement error in linear treatment effect models under strict exogeneity. For instance, Nguimkeu, Denteh, and Tchernis (2019) shows that, even when treatment is exogenous, the sign of the OLS estimator is not generally identified with endogenous (differential) misreporting.

In the case where the misclassification is solely false positives (Panel D), we find that the DID estimates are attenuated but take on the correct (positive) sign of the true ATT. This finding is consistent with Proposition 1 by observing that the second term on the right-hand side of equation (2.3) is zero when there are no false negatives. The simulation results illustrate the severe consequences of misclassification, but do not exhaust the set of possible outcomes thereof. As shown in Proposition 2 and the discussion immediately following it, the resulting bias in the DID estimator can take any form—attenuation bias, expansion bias, or sign reversal—depending on various parameters in specific contexts and data generating processes.
We now turn to the results for nondifferential misclassification reported in Table 2. The results show that the DID estimator continues to be severely biased downwards but maintains the correct sign. Several key results emerge illustrating our theoretical results. In all cases, the DID estimates exhibit an attenuation bias as shown in Corollary 3. The attenuation bias persists for all levels and types of misclassification. The simulation results indicate that the attenuation bias is more severe for the symmetric case in Panel A at higher levels of misclassification. When expressed as a fraction of the true ATT, the DID estimate ranges from 90 percent for 5% misclassification to 2 percent with 50% misclassification. In the remaining panels where the misclassification is either asymmetric or one-sided, the DID estimates range from 92 percent to about 51 percent of the true ATT.

The proposed bounds in Corollary 4 are reported in Column 5 of Table 2 with $\lambda = 0.4$. We chose this threshold because it covers three possibilities that could result from the researcher’s attempt to estimate the extent of misclassification—overestimation, correct estimation, and underestimation. Recall that $\lambda$ is the sensitivity parameter bounding the extent of misclassification in the data, defined in Assumption 6 as the sum of misclassification probabilities, i.e., $\lambda = P(\varepsilon = 1|D = 1) + P(\varepsilon = 1|D = 0)$. For the symmetric case in Panel A, setting $\lambda = 0.4$ means the researcher overestimates the misclassification rate (the 5% and 10% experiments). The same value of $\lambda$ implies that the researcher correctly estimates the degree of misclassification in the 20% experiment but underestimates the misclassification rate in the remaining cases (i.e., the 30%, 40%, and 50% experiments). Whenever the researcher overstates the true extent of misclassification, we obtain informative bounds that include the true ATT, and those bounds tighten as $\lambda$ gets closer to the true extent of misclassification.

When the researcher understates the sum of misclassification probabilities, the bounds are tight but do not include the true ATT. In Panel A of Table 2, the bounds do not include the true ATT for the 30% to 50% experiments. When $\lambda$ correctly estimates the sum of misclassification probabilities (the 20% experiment in Panel A), we find that the upper limit of the estimated bounds collapses to the true ATT (see Corollary 4). We obtain the same pattern of results in the remaining panels of Table 2.

Finally, note that the proposed bounds depend on correctly estimating the sum of the conditional misclassification probabilities and not the unconditional (overall) misclassification rate. For instance, in Panel A, while the unconditional misclassification rate is 30%, the true $\lambda$ is 0.6. By setting $\lambda = 0.4$, the researcher underestimates the extent of misclassification, leading to bounds that do not contain the true ATT. In summary, the simulations illustrate
Table 1. Simulation Results for Differential Misclassification of Treatment

| Overall error rate | False negative | False positive | DID estimates |
|--------------------|----------------|----------------|---------------|
|                    | (1)            | (2)            | (3)           | (4)           |
| Panel A: Symmetric errors |
| 5%                 | 0.050          | 0.050          | 3.000         | 2.134         |
| 10%                | 0.100          | 0.100          | 3.000         | 1.419         |
| 20%                | 0.200          | 0.200          | 3.000         | 0.209         |
| 30%                | 0.300          | 0.300          | 3.000         | -0.789        |
| 40%                | 0.400          | 0.400          | 3.000         | -1.617        |
| 50%                | 0.500          | 0.500          | 3.000         | -2.290        |
| Panel B: Asymmetric errors |
| 5%                 | 0.083          | 0.011          | 3.000         | 1.807         |
| 10%                | 0.155          | 0.024          | 3.000         | 0.937         |
| 20%                | 0.273          | 0.059          | 3.000         | -0.384        |
| 30%                | 0.365          | 0.115          | 3.000         | -1.403        |
| 40%                | 0.439          | 0.222          | 3.000         | -2.242        |
| 50%                | 0.486          | 0.396          | 3.000         | -2.688        |
| Panel C: False negatives only |
| 5%                 | 0.084          | 0              | 3.000         | 1.829         |
| 10%                | 0.179          | 0              | 2.999         | 0.698         |
| 20%                | 0.281          | 0              | 3.000         | -0.443        |
| 30%                | 0.393          | 0              | 3.000         | -1.846        |
| 40%                | 0.456          | 0              | 3.000         | -3.030        |
| 50%                | 0.495          | 0              | 3.000         | -5.023        |
| Panel D: False positives only |
| 5%                 | 0              | 0.075          | 3.001         | 2.775         |
| 10%                | 0              | 0.199          | 3.000         | 2.404         |
| 20%                | 0              | 0.304          | 3.001         | 2.090         |
| 30%                | 0              | 0.386          | 3.000         | 1.843         |
| 40%                | 0              | 0.448          | 2.999         | 1.656         |
| 50%                | 0              | 0.498          | 3.000         | 1.506         |

Notes. This table presents simulation results for the case of differential misclassification of treatment within the differences-in-differences framework. Columns 1 and 2 report false negative and false positive rates conditional on true treatment status. The DID estimates using the true (unobserved) and misclassified (observed) treatment variables are reported in Columns 3 and 4, respectively. The panels correspond to various types of misclassification discussed in the Monte Carlo design setup section.

our theoretical results showing that the consequences of misclassification in DID designs can be severe, sometimes going beyond an attenuation bias to producing the incorrect sign of the treatment effect.

4. EMPIRICAL ILLUSTRATION

In this section, we illustrate our theoretical results using two empirical applications highlighting how misclassification may arise in DID designs in different policy environments.
### Table 2. Simulation Results for Nondifferential Misclassification of Treatment

| Overall error rate | False negative (1) | False positive (2) | DID estimates True treatment (3) | Observed treatment (4) | ATT bounds ($\lambda = 0.4$) (5) |
|--------------------|-------------------|--------------------|---------------------------------|------------------------|----------------------------------|
| **Panel A: Symmetric errors** |                   |                    |                                 |                        |                                  |
| 5%                 | 0.050             | 0.050              | 2.999                           | 2.700                  | (2.700 , 4.499)                 |
| 10%                | 0.100             | 0.100              | 3.000                           | 2.400                  | (2.400 , 4.000)                 |
| 20%                | 0.200             | 0.200              | 3.000                           | 1.800                  | (1.800 , 3.001)                 |
| 30%                | 0.300             | 0.300              | 3.000                           | 1.200                  | (1.200 , 2.001)                 |
| 40%                | 0.400             | 0.400              | 3.000                           | 0.600                  | (0.600 , 1.000)                 |
| 50%                | 0.490             | 0.490              | 3.001                           | 0.060                  | (0.060 , 0.100)                 |
| **Panel B: Asymmetric errors** |                   |                    |                                 |                        |                                  |
| 5%                 | 0.011             | 0.069              | 3.000                           | 2.760                  | (2.760 , 4.599)                 |
| 10%                | 0.170             | 0.028              | 3.000                           | 2.408                  | (2.408 , 4.013)                 |
| 20%                | 0.288             | 0.019              | 2.999                           | 2.077                  | (2.077 , 3.461)                 |
| 30%                | 0.376             | 0.015              | 3.000                           | 1.826                  | (1.826 , 3.044)                 |
| 40%                | 0.445             | 0.012              | 3.000                           | 1.629                  | (1.629 , 2.715)                 |
| 50%                | 0.487             | 0.010              | 3.000                           | 1.511                  | (1.511 , 2.518)                 |
| **Panel C: False negatives only** |                   |                    |                                 |                        |                                  |
| 5%                 | 0.091             | 0                  | 3.000                           | 2.727                  | (2.727 , 4.545)                 |
| 10%                | 0.167             | 0                  | 3.000                           | 2.501                  | (2.501 , 4.168)                 |
| 20%                | 0.286             | 0                  | 3.000                           | 2.143                  | (2.143 , 3.572)                 |
| 30%                | 0.375             | 0                  | 2.999                           | 1.875                  | (1.875 , 3.124)                 |
| 40%                | 0.445             | 0                  | 3.000                           | 1.667                  | (1.667 , 2.778)                 |
| 50%                | 0.487             | 0                  | 3.000                           | 1.535                  | (1.535 , 2.558)                 |
| **Panel D: False positives only** |                   |                    |                                 |                        |                                  |
| 5%                 | 0                 | 0.091              | 3.001                           | 2.728                  | (2.728 , 4.547)                 |
| 10%                | 0                 | 0.167              | 3.000                           | 2.500                  | (2.500 , 4.167)                 |
| 20%                | 0                 | 0.286              | 3.000                           | 2.143                  | (2.143 , 3.571)                 |
| 30%                | 0                 | 0.375              | 3.000                           | 1.875                  | (1.875 , 3.124)                 |
| 40%                | 0                 | 0.444              | 3.000                           | 1.667                  | (1.667 , 2.778)                 |
| 50%                | 0                 | 0.487              | 3.000                           | 1.538                  | (1.538 , 2.563)                 |

**Notes.** This table presents simulation results for the case of nondifferential misclassification of treatment within the difference-in-differences framework. Columns 1 and 2 report false negative and false positive rates conditional on true treatment status. The DID estimates using the true (unobserved) and misclassified (observed) treatment variables are reported in Columns 3 and 4, respectively. The bounds on the ATT in Column 5 are point-estimate bounds based on Corollary 4. The panels correspond to various types of misclassification discussed in the Monte Carlo design setup section.

Our objective is to demonstrate the usefulness of our proposed methods and provide guidance on how researchers may use them. The first application considers the effect of a 2007 legal reform in Brazil that legislated municipal governments (as opposed to state governments) as the ultimate authority to provide public services in the water and sanitation sector (Kresch, 2020). The second empirical illustration revisits how punishment severity affects jury decision-making using the historical English reforms that abolished capital punishment from 1772 to 1871 (Bindler and Hjalmarsson, 2018).
4.1. **Federalism in the Brazilian water and sanitation sector.** For several years in Brazil, there was a shared responsibility in the provision of public services where some municipalities self-provided services and others contracted with state companies to do so. This status quo created uncertainty and the risk of state governments overtaking municipally run companies, which could lead to suboptimal investment and provision of services. In January 2007, the Brazilian Congress passed a law (National Water Law 11.447) that gave municipalities the ultimate authority to provide services, effectively eliminating any takeover risks. Kresch (2020) finds that this legal reform led to a significant increase in the total system investment in service provision that consequently increased access and decreased child mortality.

To implement the DID design, Kresch (2020) compares self-run municipalities (treatment group) to state-run municipalities (control group) following the plausibly exogenous elimination of takeover risk induced by the legal reform.\(^4\) In their primary empirical specification, Kresch (2020) used the date of the proposed legislation (2005) instead of the official date on which the legislation was ratified or passed (2007) to demarcate the pre- and post-intervention periods. Kresch argues that since the popular President Lula da Silva supported the proposed legislation, “the bill’s passage was likely,” which potentially changed investment decisions beginning from the proposed date (Kresch, 2020). While this explanation is reasonable, we view this decision as introducing misclassification in the DID framework.\(^5\) In an online appendix, Kresch (2020) performs a robustness check utilizing 2007 to mark the intervention period and concludes that they find similar results. Unlike most empirical scenarios, Kresch (2020) has access to the true treatment and can, therefore, directly judge the sensitivity of their findings to alternative specifications (i.e., separately using the dates of proposal and passage of the legislation). Without observing the true treatment or timing of treatment in practice, it is not feasible to conduct sensitivity analysis by examining the estimates under the two regimes. This means our proposed bounds could still be helpful to researchers in most cases.

Table 3 presents our findings on the effect of the legal reform on investment decisions. Unlike the original study, we do not include covariates in our estimation because doing so is outside the scope of this paper and is not crucial for the parallel trends assumption to

---

\(^4\)As in the original study, we maintain that the parallel trends assumption plausibly holds given that the decision to be a self-run or state-run municipality was made in the 1970s, with no companies switching their status during the study period.

\(^5\)One alternative interpretation is that using the proposed date identifies *anticipatory effects* of the reform rather than introducing misclassification. However, we think this alternate view moves the goalpost and changes the interpretation of the policy-relevant parameter identified by the DID estimand.
Columns 1 and 2 present the DID estimates using 2005 and 2007 to mark the post-intervention periods, respectively. Both sets of DID estimates show that the legal reform eliminating take-over risk led to a statistically significant increase in overall investments and all types of investments except for government grants and investment in water. While the results across the two legislation dates are qualitatively the same, the estimates using the earlier proposed date are slightly smaller.

We provide a complementary approach to conducting sensitivity analysis by providing bounds on the treatment effect based on the researcher’s determination of the extent of misclassification induced by the uncertainty in the timing of the post-intervention period. These bounds are based on the assumption of nondifferential misclassification using our results in Corollary 4 and help the researcher assess how sensitive the DID estimates are to misclassification. How should the researcher estimate the extent of misclassification, $\lambda$? The answer to this question is context-dependent and should be done on a case-by-case basis. We suggest that researchers use domain knowledge of the relevant policy and information from their data to come up with their best estimate of the degree of potential misclassification.

In this context, the study period spans 11 years (2001-2012), with the post-intervention period being seven years based on the 2005 proposed date and five years when using the official passage date of 2007, respectively. Thus, we can estimate that $\lambda = 0.29$ (i.e., $2/7$).

Column 3 of the table presents the estimated bounds on the ATT. Since the DID estimates represent the lower bounds on the treatment effects, the upper limits of the bounds are informative about the extent to which the DID estimates are biased downwards. Across all types of investments, the upper limits of the bounds on the ATT are roughly 40% higher than the DID estimate (based on the legislation proposal date) except for investments in water. In all but one instance, the bounds include the DID estimate using the date of legislation passage. This finding is reassuring given that the researcher does not usually observe both of those dates in other contexts and cannot check the robustness of their findings to using both dates as in Kresch (2020). Thus, our proposed bounds are informative and provide a complementary way to assess the sensitivity of DID estimates under misclassification.

Our results replicate the estimates in Table 3 of the original study, albeit without covariates. Some of the covariates used in the original study include municipality characteristics such as population size, municipality finance measures, and temperature and rainfall variables.

However, we cannot reject the null hypothesis that both sets of estimates are equal at the 5% level of significance.

Alternatively, one can report alternative bounds using various rates of misclassification based on reasonable beliefs regarding the extent of misclassification.
Table 3. The Impact of Water and Sanitation Legal Reform on Investment

| Dependent variables | Proposed date (2005) | Passage date (2007) | ATT bounds (λ = 0.29) |
|---------------------|----------------------|---------------------|-----------------------|
| Panel A: Overall investment | | | |
| Total Investment | 3,226.26** | 3,287.98* | (3,226.26, 4,516.77) |
| | (1,423.35) | (1,695.24) | |
| Panel B: Sources of investments | | | |
| Self-financing | 1,835.43*** | 1,930.41*** | (1,835.43, 2,569.60) |
| | (481.52) | (520.57) | |
| Loans and Debt | 2,121.43 ** | 2,273.94** | (2,121.43, 2,970.00) |
| | (897.80) | (1,003.09) | |
| Government grants | 36.17 | 40.20 | (36.17, 50.64) |
| | (223.99) | (296.62) | |
| Panel C: Destination of investments | | | |
| Investment in Water | 869.50 | 787.78 | (869.50, 1,217.29) |
| | (536.52) | (689.35) | |
| Investment in Sewer | 1,894.92 *** | 2,025.37* | (1,894.92, 2,652.89) |
| | (948.32) | (1,178.14) | |
| Other Investments | 450.52*** | 501.72*** | (450.52, 630.72) |
| | (146.63) | (175.80) | |
| Observations | 14,460 | 14,460 | |

Notes. This table presents difference-in-differences estimates of the impact of the 2007 legal reform in the Brazilian water and sanitation sector on investment decisions based on the analysis sample in Kresch (2020). The sample comprises yearly municipality-level data on the total system investments (grouped according to the origin of funds) from 2001 to 2012. All analyses are conducted without covariates. Investment amounts are measured in thousand reals. The DID estimates in column (1) use the legislation’s proposed date as in Kresch’s original analysis (2005) to demarcate the pre- and post-periods while those in column (2) are based on the legislation’s passage date (2007). The bounds on the ATT in Column (3) are point-estimate bounds based on Corollary 4 accounting for the misclassification resulting from the choice of the post-treatment period.

4.2. Punishment severity and jury decisions. Bindler and Hjalmarsson (2018) use two natural experiments in English history—the abolition of capital punishment and the temporary halt of transportation during the American Revolution—to investigate the effect of a large change in punishment severity on jury decision making. We use the capital punishment experiment in our empirical illustration because those data contain treatment and control groups permitting a DID analysis. The basic idea is to exploit a series of offense-specific laws in the middle of the 19th that abolished capital in England to study the impact of sharp reduction in punishment severity on a jury’s sentencing decisions. The data are extracted from more than 200,000 criminal cases tried at the Old Bailey Criminal Court in London between 1772 and 1871. In addition to sentencing outcomes (verdicts), the
data includes basic information on each case including the alleged offense, the demographic information on the defendant (name, age, and gender), the session dates, and the names of the judges and juries.

To implement the DID research design, Bindler and Hjalmarsson (2018) compare changes in sentencing outcomes for cases in which capital punishment was abolished for the alleged offense (“treated” offenses) to those cases where the alleged offense was never eligible for capital punishment or always capital eligible (“control” offenses). The authors find that the drastic reduction in punishment severity following the abolishing of capital punishment led to a significant increase in the chance of any conviction and conviction of the original charge. Their results also indicate a decrease in the recommendation for a mercy following the abolishing of capital punishment since this was no longer necessary for juries wanting to spare a defendant of death, especially in violent and fraud cases.

A key empirical challenge in the original analysis is the inability to unambiguously identify treated offense categories and the timing of the treatment years because of the complicated nature of the historical laws spanning almost 200 years. The authors resort to using the observed discontinuities in the share of death sentences to identify treated and control offenses. Specifically, an offense is coded as a “treated” offense when the share of death sentences drops to zero, with the treatment period starting from the year this discontinuity was observed. Otherwise, if no such discontinuity occurs (i.e., always or never capital eligible), then the offense was assigned to the “control” group. Thus, misclassification may arise due to the lag between the passage and implementation of the offense-specific law abolishing capital punishment, especially since the authors cannot code treatment based on the exact month of the law. Admittedly, the authors note that “our coding of the reform as the first year with zero death sentences therefore likely assigns some “treated” cases to the pre-reform period. We believe this to be of minimal concern, however, since the results are completely robust (and available upon request) to excluding from the analysis the reform year, the first pre-reform year, and the first post-reform year” (Bindler and Hjalmarsson, 2018). Their final treatment and control groups comprised sixteen and nine offense categories, respectively.

In our replication, we use the same control group offenses but only include five offense categories in our treatment group. We do so because capital punishment was abolished in different years for many of the treated offense categories over the study period, leading to a staggered treatment adoption DID setup that is outside the scope of this paper (Goodman-Bacon, 2021; Roth et al., 2022; de Chaisemartin and D’Haultfœuille, 2022). However, the
authors determined that the abolishing of capital punishment occurred in the same year (1832) for the five offense categories we use to define treatment. The five offense categories are animal theft, coining offenses, forgery, sodomy, and theft from place. Appendix Figure A.1 displays the share of death sentences for our treated offenses over time with the solid (red) line denoting the year capital punishment was abolished. All the offenses appear to have witnessed a sharp discontinuous drop in the share of death sentences by 1832 although there are some non-zero death sentences for sodomy post-1832.

Table 4 presents our results for the impact of abolishing capital punishment on jury decision making. Similar to our first application, we perform all analyses without covariates and maintain the parallel trends assumption based on the justification and evidence provided in the original study. The table reports the standard DID estimates (with no accounting for misclassification) and two sets of our proposed bounds accounting for potential misclassification of treatment based on two estimates of \( \lambda \). The first set of bounds (Column 2) estimate \( \lambda \) as the proportion of cases in the years of potential misclassification—pre-reform (1831), reform (1832), and post-reform (1833)—to the number of cases in the entire study period 1803-1871, leading to \( \lambda = 0.07 \). The second set of bounds (Column 3) estimates \( \lambda \) using the same numerator as before, but dividing by the number of cases in the post-intervention period spanning 1831-1871; this yields \( \lambda = 0.125 \).

Although we define our treatment group differently to include only offenses treated in the same year of 1832, the DID estimates in Column 1 are qualitatively the same as those in the original paper (using all the treated offenses) except in one instance. The DID estimates show that abolishing capital punishment led to a 6.5 percentage increase in the chance of conviction of any charge (original or lesser charge) but this is not statistically significant.\(^9\) However, we find a statistically significant increase in the probability of any conviction of the original charge by 16.5 percentage points. For the next two outcomes (Panel B), the sample is restricted to those who were convicted of their alleged offenses (as in the original study). We find that the abolishing of capital punishment reduces the probability of lesser offense conditional on being convicted by 15.4 percentage points. Finally, abolishing capital punishment reduces the chance of recommendation for mercy conditional on being convicted by 4.9 percentage points.

\(^9\)Since Bindler and Hjalmarsson (2018) find that their overall results for the probability of any charge are driven by violent offenses, the fact that we do not find a statistically significant effect is not surprising given our definition of treatment includes mostly non-violent offenses.
Our two sets of bounds show how sensitive the DID estimates are to the potential misclassification of treatment. The results in Column 2 show that the DID estimates are biased downwards by about 7 percent in all cases. For a higher rate of misclassification in Column 3, our bounds are slightly wider, with an implied downward bias of about 12.5 percent. Taken together, our proposed bounds can help researchers gauge the robustness of their findings to a potentially misclassified treatment in DID settings with no access to the true treatment (e.g., Bindler and Hjalmarsson (2018)).

Table 4. The Impact of Abolition Capital Punishment on Jury Decisions

| Dependent variables                     | DID estimates | ATT bounds     |
|-----------------------------------------|---------------|----------------|
|                                         |               | \(\lambda = 0.07\) | \(\lambda = 0.125\) |
|                                          | (1)           | (2)            | (3)            |
| Guilty of any offense by jury verdict (0/1) | 0.0655        | (0.0655, 0.0704) | (0.0655, 0.0749) |
|                                          | (0.0670)      |                |                |
| Guilty of original charge by jury verdict (0/1) | 0.1656***     | (0.1656, 0.1781) | (0.1656, 0.1893) |
|                                          | (0.0406)      |                |                |
| Guilty of lesser offense conditional on guilty by jury verdict (0/1), broad definition | -0.1537*      | (-0.1653, -0.1537) | (-0.1757, -0.1537) |
|                                          | (0.0855)      |                |                |
| Recommended for mercy conditional on guilty by jury verdict (0/1) | -0.0495**     | (-0.0532, -0.0495) | (-0.0566, -0.0495) |
|                                          | (0.0166)      |                |                |

Notes. This table presents difference-in-differences estimates of the effect of changes in punishment severity due to abolishing offense-specific capital punishment on jury decision-making Bindler and Hjalmarsson (2018). The sample comprises defendant-case observations tried in the Old Bailey Criminal Court in London between 1772 and 1871. All analyses are conducted without covariates. Column 1 reports the baseline DID estimates. The bounds on the ATT in Columns (2) and (3) are point-estimate bounds based on Corollary 4 that accounts for misclassification. The full sample (Panel A) and the sample of convicted cases comprise 80,925 and 60,214 observations, respectively.

5. Conclusion

The difference-in-differences design continues to be one of the most widely used quasi-experimental designs in economics and related disciplines. Under a parallel trends assumption, this method identifies the average treated effect on the treated. Part of the DID’s appeal is its simplicity and ease of use. The canonical DID design provides a nonparametric estimate of the ATT by comparing the differences in outcomes across two groups and periods. The DID method continues to attract a great deal of attention from methodological researchers along several dimensions. One aspect of the DID design that has received little to no attention is the misclassification of treatment status. When empirical researchers
encounter misclassification in their DID analysis, they often assert that its consequences are likely minimal, resulting in an attenuation bias.

This paper investigates the identification of the DID estimand under arbitrary misclassification. We show that the consequences of misclassification are more severe than previously articulated in the literature, with the sign of the DID estimand possibly being different from the true treatment effect. In particular, the DID estimand using a possibly misclassified treatment variable identifies a weighted average of the ATT for the correctly classified and misclassified subgroups, with the weights being negative for the latter.

Under nondifferential misclassification, we find that the DID estimand is attenuated but recovers the correct sign. We propose bounds on the ATT under reasonable assumptions on the extent of misclassification. Researchers may use these bounds for sensitivity analysis when they suspect misclassification of the treatment variable in their DID framework.

We provide simulation evidence to demonstrate our theoretical results and conclude with two empirical applications that provide guidance on how to estimate the sensitivity parameter used for our bounding exercise. This paper contributes to the literature by providing new insights on the implications of measurement error in DID designs. Future work can consider extensions of our work to staggered DID designs and consider the inclusion of covariates.
References

Acerenza, S., K. Ban, and D. Kédagni. 2021. “Marginal Treatment Effects with Misclassified Treatment.” arXiv preprint arXiv:2105.00358 URL https://arxiv.org/abs/2105.00358.

Aigner, Dennis J. 1973. “Regression with a binary independent variable subject to errors of observation.” Journal of Econometrics 1 (1):49–59.

Battistin, E., M. De Nadai, and B. Sianesi. 2014. “Misreported schooling, multiple measures and returns to educational qualifications.” Journal of Econometrics 181:136–150.

Battistin, E. and B. Sianesi. 2011. “Misclassified Treatment. Status and Treatment Effects: An application to Returns to Education in the United Kingdom.” The Review of Economics and Statistics 93 (2):495–509.

Bindler, Anna and Randi Hjalmarsson. 2018. “How Punishment Severity Affects Jury Verdicts: Evidence from Two Natural Experiments.” American Economic Journal: Economic Policy 10 (4):36–78.

Bollinger, Christopher R and Martijn van Hasselt. 2017. “Bayesian moment-based inference in a regression model with misclassification error.” Journal of Econometrics 200 (2):282–294.

Botosaru, Irene and Federico H Gutierrez. 2018. “Difference-in-differences when the treatment status is observed in only one period.” Journal of Applied Econometrics 33 (1):73–90.

Bound, John, Charles Brown, and Nancy Mathiowetz. 2001. “Measurement error in survey data.” In Handbook of econometrics, vol. 5. Elsevier, 3705–3843.

Buchmueller, Thomas C, John DiNardo, and Robert G Valletta. 2011. “The effect of an employer health insurance mandate on health insurance coverage and the demand for labor: Evidence from Hawaii.” American Economic Journal: Economic Policy 3 (4):25–51.

Chalak, K. 2017. “Instrumental Variables Methods with Heterogeneity and Mismeasured Instruments.” Econometric Theory 33:69—104.

Cortes, Kalena E. 2013. “Achieving the DREAM: The effect of IRCA on immigrant youth postsecondary educational access.” American Economic Review 103 (3):428–32.

Currie, Janet, Henrik Kleven, and Èsmée Zwiers. 2020. “Technology and big data are changing economics: Mining text to track methods.” In AEA Papers and Proceedings, vol. 110. 42–48.
de Chaisemartin, C. and X. D'Haultfoeuille. 2018. “Fuzzy Differences-in-Differences.” Review of Economic Studies 85:999–1028.
de Chaisemartin, Clément and Xavier D'Haultfoeuille. 2022. “Two-way fixed effects and differences-in-differences with heterogeneous treatment effects: a survey.” The Econometrics Journal URL https://doi.org/10.1093/ectj/utac017. Utac017.
DiTraglia, Francis J. and Camilo Garcia-Jimeno. 2019. “Identifying the effect of a misclassified, binary, endogenous regressor.” Journal of Econometrics 209:376–390.
Draca, Mirko, Stephen Machin, and John Van Reenen. 2011. “Minimum wages and firm profitability.” American economic journal: applied economics 3 (1):129–51.
Fan, Yanqin and Carlos A Manzanoa. 2017. “Partial identification of average treatment effects on the treated through difference-in-differences.” Econometric Reviews 36 (6-9):1057–1080.
Fortson, Jane G. 2009. “HIV/AIDS and fertility.” American Economic Journal: Applied Economics 1 (3):170–94.
Frazis, Harley and Mark A Loewenstein. 2003. “Estimating linear regressions with mismeasured, possibly endogenous, binary explanatory variables.” Journal of Econometrics 117 (1):151–178.
Galasso, Alberto and Hong Luo. 2022. “When does product liability risk chill innovation? Evidence from medical implants.” American Economic Journal: Economic Policy 14 (2):366–401.
Goodman-Bacon, Andrew. 2021. “Difference-in-differences with variation in treatment timing.” Journal of Econometrics 225 (2):254–277.
Groen, Jeffrey A and Anne E Polivka. 2008. “The effect of Hurricane Katrina on the labor market outcomes of evacuees.” American Economic Review 98 (2):43–48.
Hausman, J. A., J. Abrevaya, and F. M. Scott-Morton. 1998. “Misclassification of the dependent variable in a discrete-response setting.” Journal of Econometrics 87:239–269.
Jiang, Z. and P. Ding. 2020. “Measurement errors in the binary instrumental variable model.” Biometrika 107 (1):238–245.
Kasahara, H. and K. Shimotsu. 2021. “Identification of Regression Models with a Misclassified and Endogenous Binary Regressor.” Econometric Theory (forthcoming).
Kessler, L. M. and D. Bruce. 2022. “Housing Market and Migration Responses to the Limit on the State and Local Tax Deduction.” Working paper.
Kreider, B., J. V. Pepper, C. Gundersen, and D. Jolliffe. 2012. “Identifying the effects of SNAP (food stamps) on child health outcomes when participation is endogenous and misreported.” Journal of American Statistical Association 107:958–975.
Kresch, Evan Plous. 2020. “The buck stops where? federalism, uncertainty, and investment in the brazilian water and sanitation sector.” American Economic Journal: Economic Policy 12 (3):374–401.

Lewbel, Arthur. 2007. “Estimation of Average Treatment Effects with Misclassification.” Econometrica 75 (2):537–551.

Mahajan, Aprajit. 2006. “Identification and Estimation of Regression Models with Misclassification.” Econometrica 74 (3):631–665.

Miller, S. 2012. “The effect of insurance on emergency room visits: An analysis of the 2006 Massachusetts health reform.” Journal of Public Economics 96 (11–12):893–908.

Murray, Fiona, Philippe Aghion, Mathias Dewatripont, Julian Kolev, and Scott Stern. 2016. “Of mice and academics: Examining the effect of openness on innovation.” American Economic Journal: Economic Policy 8 (1):212–52.

Nguimkeu, Pierre, Augustine Denteh, and Rusty Tchernis. 2019. “On the estimation of treatment effects with endogenous misreporting.” Journal of Econometrics 208 (2):487–506.

Possebom, V. 2021. “Crime and Mismeasured Punishment: Marginal Treatment Effect with Misclassification.” Working Paper.

Roth, Jonathan, Pedro HC Sant’Anna, Alyssa Bilinski, and John Poe. 2022. “What’s Trending in Difference-in-Differences? A Synthesis of the Recent Econometrics Literature.” Journal of Econometrics, forthcoming.

Tommasi, D. and L. Zhang. 2020. “Bounding Program Benefits When Participation Is Misreported.” Discussion Working Paper series, IZA DP No. 13430.

Ura, Takuya. 2018. “Heterogeneous Treatment Effects with Mismeasured Endogenous Treatment.” Quantitative Economics 9 (3):1335–1370.
Figure A.1. Identifying the timing of the abolition of capital punishment (treated offenses)

Notes. The figure displays the proportion of convicted cases that resulted in a death sentence based on data Bindler and Hjalmarsson (2018). The treated offenses are animal theft (Panel a), coining offenses (Panel b), forgery (Panel c), sodomy (Panel d), and theft from place (Panel e). The red vertical line denotes the year of treatment (1832), which is the first year in which the capital sentences drop to zero.
We have
\[
\mathbb{E}[Y_t(0)|D = 1] - \mathbb{E}[Y_t(0)|D = 0] = \mathbb{E}[Y_t(0)|D = 1, D^* = 1]\mathbb{P}(D^* = 1|D = 1) \\
+ \mathbb{E}[Y_t(0)|D = 1, D^* = 0]\mathbb{P}(D^* = 0|D = 1) \\
- \mathbb{E}[Y_t(0)|D = 0, D^* = 1]\mathbb{P}(D^* = 1|D = 0) \\
- \mathbb{E}[Y_t(0)|D = 0, D^* = 0]\mathbb{P}(D^* = 0|D = 0),
\]
where the first equality holds from the law of iterated expectations, and the second holds from the definition of the model. We can then take the difference of the above quantity over time as follows:
\[
(\mathbb{E}[Y_1(0)|D = 1] - \mathbb{E}[Y_1(0)|D = 0]) - (\mathbb{E}[Y_0(0)|D = 1] - \mathbb{E}[Y_0(0)|D = 0]) \\
= \mathbb{E}[Y_1(0) - Y_0(0)|\varepsilon = 0, D^* = 1]\mathbb{P}(D^* = 1|D = 1) \\
+ \mathbb{E}[Y_1(0) - Y_0(0)|\varepsilon = 1, D^* = 0]\mathbb{P}(D^* = 0|D = 1) \\
- \mathbb{E}[Y_1(0) - Y_0(0)|\varepsilon = 1, D^* = 1]\mathbb{P}(D^* = 1|D = 0) \\
- \mathbb{E}[Y_1(0) - Y_0(0)|\varepsilon = 0, D^* = 0]\mathbb{P}(D^* = 0|D = 0),
\]
\[
= \mathbb{E}[Y_1(0) - Y_0(0)|D^* = 1]\mathbb{P}(D^* = 1|D = 1) \\
+ \mathbb{E}[Y_1(0) - Y_0(0)|D^* = 0]\mathbb{P}(D^* = 0|D = 1) \\
- \mathbb{E}[Y_1(0) - Y_0(0)|D^* = 1]\mathbb{P}(D^* = 1|D = 0) \\
- \mathbb{E}[Y_1(0) - Y_0(0)|D^* = 0]\mathbb{P}(D^* = 0|D = 0),
\]
\[
= \mathbb{E}[Y_1(0) - Y_0(0)|D^* = 1](\mathbb{P}(D^* = 1|D = 1) - \mathbb{P}(D^* = 1|D = 0)) \\
+ \mathbb{E}[Y_1(0) - Y_0(0)|D^* = 0](\mathbb{P}(D^* = 0|D = 1) - \mathbb{P}(D^* = 0|D = 0)),
\]
\[
= \{\mathbb{E}[Y_1(0) - Y_0(0)|D^* = 1] - \mathbb{E}[Y_1(0) - Y_0(0)|D^* = 0]\}(\mathbb{P}(D^* = 1|D = 1) - \mathbb{P}(D^* = 1|D = 0)),
\]
\[
= 0.
\]
where the second equality holds from Assumption 2.(2), the fourth follows from the fact that \( \mathbb{P}(D^* = 0|D = 1) - \mathbb{P}(D^* = 0|D = 0) = -(P(D^* = 1|D = 1) - \mathbb{P}(D^* = 1|D = 0)) \), and the last holds from Assumption 2.(1).

**Appendix C. Proof of Proposition 1**

We have

\[
\theta_{OLS}^1 = \mathbb{E}[Y_1|D = 1] - \mathbb{E}[Y_1|D = 0],
\]

\[
= \mathbb{E}[Y_1(1)D^* + Y_1(0)(1 - D^*)|D = 1] - \mathbb{E}[Y_1(1)D^* + Y_1(0)(1 - D^*)|D = 0],
\]

\[
= \mathbb{E}[(Y_1(1) - Y_1(0))D^* + Y_1(0)|D = 1] - \mathbb{E}[(Y_1(1) - Y_1(0))D^* + Y_1(0)|D = 0],
\]

\[
= \mathbb{E}[(Y_1(1) - Y_1(0))D^*|D = 1] - \mathbb{E}[(Y_1(1) - Y_1(0))D^*|D = 0]
\]

\[
+ \mathbb{E}[Y_1(0)|D = 1] - \mathbb{E}[Y_1(0)|D = 0],
\]

where the second equality holds from the definition of model (2.1), and the last from the law of iterated expectations. Now, under Assumption 1 we have

\[
\mathbb{E}[Y_1(0)|D = 1] - \mathbb{E}[Y_1(0)|D = 0] = \mathbb{E}[Y_0|D = 1] - \mathbb{E}[Y_0|D = 0].
\]

Therefore,

\[
\theta_{OLS}^1 = \mathbb{E}[(Y_1(1) - Y_1(0))|D^* = 1, D = 1]P(D^* = 1|D = 1)
\]

\[
- \mathbb{E}[(Y_1(1) - Y_1(0))|D^* = 1, D = 0]P(D^* = 1|D = 0)
\]

\[
+ \theta_{OLS}^0,
\]

which implies

\[
\theta_{OLS}^1 - \theta_{OLS}^0 = \mathbb{E}[(Y_1(1) - Y_1(0))|D^* = 1, D = 1]P(D^* = 1|D = 1)
\]

\[
- \mathbb{E}[(Y_1(1) - Y_1(0))|D^* = 1, D = 0]P(D^* = 1|D = 0),
\]

that is,

\[
\theta_{DID} = \mathbb{E}[(Y_1(1) - Y_1(0))|D^* = 1, \varepsilon = 0]P(D^* = 1|D = 1)
\]

\[
- \mathbb{E}[(Y_1(1) - Y_1(0))|D^* = 1, \varepsilon = 1]P(D^* = 1|D = 0),
\]
since \( \{D^* = 1, D = 1\} = \{D^* = 1, \varepsilon = 0\} \) and \( \{D^* = 1, D = 0\} = \{D^* = 1, \varepsilon = 1\} \) from the definition of model (2.1).

**Appendix D. Proof of Proposition 2**

Equation (2.3) implies

\[
\mathbb{E}[Y_1(1) - Y_1(0)|D^* = 1, \varepsilon = 0] = \frac{\theta_{DID}}{\Pr(\varepsilon = 0|D = 1)} + \frac{\mathbb{E}[Y_1(1) - Y_1(0)|D^* = 1, \varepsilon = 1]\Pr(\varepsilon = 1|D = 0)}{\Pr(\varepsilon = 0|D = 1)}.
\]

By plugging this expression in Equation (2.4), we obtain

\[
ATT = \mathbb{E}[Y_1(1) - Y_1(0)|D^* = 1, \varepsilon = 1]\Pr(\varepsilon = 1|D = 1)
+ \frac{\theta_{DID}\Pr(\varepsilon = 0|D^* = 1)}{\Pr(\varepsilon = 0|D = 1)} + \frac{\mathbb{E}[Y_1(1) - Y_1(0)|D^* = 1, \varepsilon = 1]\Pr(\varepsilon = 1|D = 0)\Pr(\varepsilon = 0|D^* = 1)}{\Pr(\varepsilon = 0|D = 1)}
\]

\[
= \frac{\theta_{DID}\Pr(D = 1)}{\Pr(D^* = 1)} + \frac{\mathbb{E}[Y_1(1) - Y_1(0)|D^* = 1, \varepsilon = 1]\Pr(\varepsilon = 1|D^* = 1) + \Pr(\varepsilon = 1|D = 0)\Pr(D = 1)}{\Pr(D^* = 1)}
\]

\[
= \frac{\mathbb{E}[Y_1(1) - Y_1(0)|D^* = 1, \varepsilon = 1]\Pr(\varepsilon = 1|D^* = 1) + \Pr(\varepsilon = 1|D = 0)\Pr(D = 1)}{\Pr(D^* = 1)}
\]

\[
= \frac{\mathbb{E}[Y_1(1) - Y_1(0)|\varepsilon = 1, D^* = 1]\Pr(\varepsilon = 1|D = 1)}{\Pr(D = 0)}.
\]

**Appendix E. Proof of Corollary 1**

From Proposition 2, we have

\[
ATT = \frac{\Pr(D = 1)}{\Pr(D^* = 1)}\theta_{DID} + \frac{\mathbb{E}[(Y_1(1) - Y_1(0))\varepsilon|D^* = 1]}{\Pr(D = 0)}.
\]

When there are no false negatives, we have \( \Pr(\varepsilon = 1, D^* = 1) = 0 \). Therefore,

\[
ATT = \frac{\Pr(D = 1)}{\Pr(D^* = 1, \varepsilon = 0)}\theta_{DID},
\]

\[
= \frac{\Pr(D = 1)}{\Pr(D = 1, \varepsilon = 0)}\theta_{DID},
\]

which implies

\[
\theta_{DID} = \frac{\Pr(D = 1, \varepsilon = 0)}{\Pr(D = 1)}ATT = \Pr(\varepsilon = 0|D = 1)ATT.
\]
We have
\[
P(\varepsilon = 1|D = 1) = \frac{P(\varepsilon = 1, D = 1)}{P(D = 1)} = \frac{P(\varepsilon = 1, D^* = 0)}{P(D = 1)},
\]

\[
= \frac{P(\varepsilon = 1, D^* = 0)}{P(\varepsilon = 1, D = 1) + P(\varepsilon = 0, D = 1)},
\]

\[
= \frac{P(\varepsilon = 1|D^* = 0)P(D^* = 0)}{P(\varepsilon = 1, D^* = 0) + P(\varepsilon = 0, D^* = 1)},
\]

where the first equality holds from Bayes' rule, the second holds from the definition of the model, the third holds from the law of total probability, and the fourth holds from Bayes' rule and the definition of the model. Similarly, we have

\[
P(\varepsilon = 1|D = 1) = \frac{P(\varepsilon = 1|D^* = 1)P(D^* = 1)}{P(\varepsilon = 1, D^* = 1) + P(\varepsilon = 0, D^* = 0)}.
\]

Denote \( p = P(D^* = 1) \), \( \alpha_0 = P(\varepsilon = 1|D^* = 0) \), and \( \alpha_1 = P(\varepsilon = 1|D = 1) \).

Then,
\[
P(\varepsilon = 1|D = 1) + P(\varepsilon = 1|D = 0) < 1 \iff \frac{\alpha_0(1 - p)}{\alpha_0(1 - p) + (1 - \alpha_1)p} + \frac{\alpha_1p}{\alpha_1p + (1 - \alpha_0)(1 - p)} < 1,
\]

\[
\iff \alpha_0(1 - p)\alpha_1p + \alpha_0(1 - p)(1 - \alpha_0)(1 - p)
\]

\[
+ \alpha_1p\alpha_0(1 - p) + \alpha_1p(1 - \alpha_1)p
\]

\[
< \alpha_0(1 - p)\alpha_1p + \alpha_0(1 - p)(1 - \alpha_0)(1 - p)
\]

\[
+ (1 - \alpha_1)p\alpha_0(1 - p) + (1 - \alpha_1)p(1 - \alpha_0)(1 - p),
\]

\[
\iff \alpha_1\alpha_0 < (1 - \alpha_1)(1 - \alpha_0),
\]

\[
\iff \alpha_0 + \alpha_1 < 1.
\]
Appendix G. Proof of Corollary 3

From Proposition 1, we have
\[
\theta_{DID} = \mathbb{E}[Y_1(1) - Y_1(0)|D^* = 1, \varepsilon = 0] \mathbb{P}(\varepsilon = 0|D = 1) \\
- \mathbb{E}[Y_1(1) - Y_1(0)|D^* = 1, \varepsilon = 1] \mathbb{P}(\varepsilon = 1|D = 0),
\]
\[
= \mathbb{E}[Y_1(1) - Y_1(0)|D^* = 1] \mathbb{P}(\varepsilon = 0|D = 1) \\
- \mathbb{E}[Y_1(1) - Y_1(0)|D^* = 1] \mathbb{P}(\varepsilon = 1|D = 0),
\]
\[
= \mathbb{E}[Y_1(1) - Y_1(0)|D^* = 1] (\mathbb{P}(\varepsilon = 0|D = 1) - \mathbb{P}(\varepsilon = 1|D = 0)),
\]
\[
= \mathbb{E}[Y_1(1) - Y_1(0)|D^* = 1] (1 - \mathbb{P}(\varepsilon = 1|D = 1) - \mathbb{P}(\varepsilon = 1|D = 0)),
\]
where the second equality holds under Assumption 4.

Appendix H. Proof of Corollary 4

From Corollary 3, we have:
\[
ATT = \frac{\theta_{DID}}{1 - \mathbb{P}(\varepsilon = 1|D = 1) - \mathbb{P}(\varepsilon = 1|D = 0)}.
\]
Therefore, the proof is straightforward from this equation and Assumption 6.

Appendix I. Proof of Corollary 5

Suppose \( \mathbb{P}(D^* = 1) \neq \mathbb{P}(D = 1) \). Then under Assumptions 1 and 4, we have from Propositions 1 and 2:
\[
\theta_{DID} = \mathbb{E}[Y_1(1) - Y_1(0)|D^* = 1, \varepsilon = 0] \mathbb{P}(\varepsilon = 0|D = 1) \\
- \mathbb{E}[Y_1(1) - Y_1(0)|D^* = 1, \varepsilon = 1] \mathbb{P}(\varepsilon = 1|D = 0),
\]
\[
= \mathbb{E}[Y_1(1) - Y_1(0)|D^* = 1] (\mathbb{P}(\varepsilon = 0|D = 1) - \mathbb{P}(\varepsilon = 1|D = 0))
\]
and
\[
ATT = \frac{\mathbb{P}(D = 1)}{\mathbb{P}(D^* = 1)} \theta_{DID} + \frac{\mathbb{E}[(Y_1(1) - Y_1(0))\varepsilon|D^* = 1]}{\mathbb{P}(D = 0)}.
\]
From the last equality, it follows that \( ATT = \theta_{DID} \) if \( \theta_{DID} = \frac{\mathbb{E}[(Y_1(1) - Y_1(0))\varepsilon|D^* = 1]}{\mathbb{P}(D = 0)(1 - \mathbb{P}(D = 1))} \). On the other hand, \( \mathbb{E}[(Y_1(1) - Y_1(0))\varepsilon|D^* = 1] = \mathbb{E}[(Y_1(1) - Y_1(0))|D^* = 1]\mathbb{P}(\varepsilon = 1|D^* = 1) \).
Therefore, we have $ATT = \theta_{DID}$ if

$$\mathbb{E}[Y_1(1) - Y_1(0)|D^* = 1] (1 - \mathbb{P}(\varepsilon = 1|D = 1) - \mathbb{P}(\varepsilon = 1|D = 0))$$

$$= \frac{\mathbb{E}[(Y_1(1) - Y_1(0))|D^* = 1]\mathbb{P}(\varepsilon = 1|D^* = 1)}{\mathbb{P}(D = 0) \left(1 - \frac{\mathbb{P}(D = 1)}{\mathbb{P}(D^* = 1)} \right)}.$$

A sufficient condition for the above equalities to hold is:

$$1 - \mathbb{P}(\varepsilon = 1|D = 1) - \mathbb{P}(\varepsilon = 1|D = 0) = \frac{\mathbb{P}(\varepsilon = 1|D^* = 1)}{\mathbb{P}(D = 0) \left(1 - \frac{\mathbb{P}(D = 1)}{\mathbb{P}(D^* = 1)} \right)},$$

which is equivalent to

$$1 - \mathbb{P}(\varepsilon = 1|D = 1) - \mathbb{P}(\varepsilon = 1|D = 0) = \frac{\mathbb{P}(\varepsilon = 1, D^* = 1)}{\mathbb{P}(D = 0) \left(\mathbb{P}(D^* = 1, \varepsilon = 1) - \mathbb{P}(D^* = 0, \varepsilon = 1)\right)},$$

since

$$\mathbb{P}(D^* = 1) = \mathbb{P}(D^* = 1, D = 1) + \mathbb{P}(D^* = 1, \varepsilon = 1),$$

and

$$\mathbb{P}(D = 1) = \mathbb{P}(D^* = 1, D = 1) + \mathbb{P}(D^* = 0, \varepsilon = 1).$$