

# Causal Mediation in Natural Experiments

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## Abstract

Natural experiments are a cornerstone of applied economics, providing settings for estimating causal effects with a compelling argument for treatment ignorability. Applied researchers often investigate mechanisms behind treatment effects by controlling for a mediator of interest, alluding to Causal Mediation (CM) methods for estimating direct and indirect effects (CM effects). This approach to investigating mechanisms unintentionally assumes the mediator is ignorable — in addition to the causal research design for the initial treatment. Individuals’ choice to take (or refuse) a mediator based on expected gains (and costs) is inconsistent with mediator ignorability, suggesting in-practice estimates of CM effects are biased in natural experiment settings. I solve for explicit bias terms when the mediator is not ignorable, imitating classical selection bias for average causal estimates. I consider an alternative approach to credibly estimate CM effects, when mediator selection is driven by unobserved gains. The approach uses a control function adjustment for unobserved selection into mediator, relying on mediator take-up cost as an instrument. Simulations confirm that this method corrects for selection bias in conventional CM estimates. This approach gives applied researchers an alternative method to estimate CM effects when they can only establish a credible argument for quasi-random assignment for the initial treatment, and not a mediator, as is common in natural experiments.

**Keywords:** Direct/indirect effects, quasi-experiment, selection, control function.

**JEL Codes:** C21, C31.

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Economists use natural experiments to credibly answer social questions, when an experiment was infeasible. For example, does gaining access to health insurance causally benefit health outcomes (Finkelstein, Taubman, Wright, Bernstein, Gruber, Newhouse, Allen, Baicker & Group 2012)? Natural experiments are settings which give answers to these questions, but give no indication of how these effects came about. Causal Mediation (CM) aims to estimate the mechanisms behind causal effects, by estimating how much of the treatment effect operates through a proposed mediator. For example, how much of the health gain from new access to health insurance comes from individuals choosing to visit hospitals more frequently, when previously they would have avoided doing so? This study of mechanism is important in understanding the economic foundation of causal effects in the real world. This paper shows that the conventional approach to estimating CM effects is inappropriate in a natural experiment setting, provides a theoretical framework for how bias operates, and develops an approach to correctly estimate CM effects under alternative assumptions.

This paper starts by answering the following question: what does a selection-on-observables approach to CM actually estimate when a mediator is not ignorable? Estimates for the average direct and indirect effects are contaminated by bias terms — selection bias plus group difference terms. I then show how this bias operates in an applied regression framework, with bias coming from a correlated error term. For example, if individuals had been choosing to seek medical care more often after receiving new health insurance (i.e., following a rational maximisation process), mediator ignorability would require a researcher observe and control for everyone’s underlying health conditions as part of the CM analysis — even if they are not diagnosed. Should a researcher consider running a CM analysis without using another natural experiment to isolate random variation in the mediator (in addition to the one for the original treatment), then this assumption is unlikely to hold true. This means that investigating mechanisms by CM methods will lead to biased inference in natural experiment settings.

I consider an alternative approach to estimating CM effects, adjusting for unobserved selection-into-mediator with a control function. This solves the identification problem with structural assumptions for selection-into-mediator — mediator monotonicity and selection based on costs and benefits — and requires a valid cost instrument for mediator take-up. While these assumptions are strong, they are plausible in many applied settings. Mediator monotonicity aligns with conventional theories for selection-into-treatment, and is accepted widely in many applications using an instrumental variables research design. Selection based on costs and benefits is central to economic theory, and is the dominant concern for judging empirical designs that use quasi-experimental variation in treatment to estimate causal effects. Access to a valid instrument is a strong assumption, though is important to avoid further

modelling assumptions; the most compelling example is using variation in mediator take-up cost as a first-stage instrument. This approach is not perfect in every setting: the structural assumptions are strong, and are tailored to selection-into-mediator concerns pertinent to economic applications. This approach provides no harbour for estimating CM effects when a mediator is not ignorable, if these structural assumptions do not hold true.

The most popular approach to CM assumes that the original treatment, and the subsequent mediator, are both ignorable (Imai, Keele & Yamamoto 2010). This approach arose in the statistics literature, and is widely used in social sciences to estimate CM effects in observational studies. Informal investigations of mechanisms in applied economics are fundamentally CM analyses, and so unintentionally import CM’s identifying assumptions.

Assuming the mediator is ignorable (i.e., satisfies selection-on-observables) conveniently sidesteps the problem of individual choice by assuming that either people made decisions to take (or refuse) potential mediators naïvely — or a researcher has observed everything relevant to this decision. This assumption might be reasonable when studying single-celled organisms in a laboratory — their “decisions” are simple and mechanical. However, social scientists study humans who make complex choices based on costs, benefits, and preferences — many of which remain unobserved by researchers. Assuming a mediator is ignorable in social science contexts is often unrealistic. In practice, the only setting where mediator ignorability becomes credible is when researchers find another natural experiment affecting the mediator — a rare occurrence given how difficult it is to find one source of random variation, let alone two, simultaneously.

The applied economics literature has been hesitant to use explicit CM methods, and began conducting informal mechanism analyses by analysing causal effects when controlling for a proposed mediator (Blackwell, Ma & Opacic 2024). This practice is fundamentally a CM analysis, despite not being named so explicitly, so falls prey to the assumptions of conventional CM analyses. Indeed, a new strand of the econometric literature has developed estimators for CM effects under a variety of strategies to avoid relying on unrealistic assumptions. This includes overlapping quasi-experimental research designs (Deuchert, Huber & Schelker 2019, Frölich & Huber 2017, Heckman & Pinto 2015), partial identification (Flores & Flores-Lagunes 2009), or a hypothesis test of full mediation through observed channels (Kwon & Roth 2024) — see Huber (2020) for an overview. The new literature has arisen in partial acknowledgement that a conventional selection-on-observables approach to CM in an applied setting can lead to biased inference, and needs alternative methods for credible inference.

This paper explicitly shows how conventional approaches to CM in natural experiments lead to biased inference. I develop a formal framework showing exactly how selection bias contaminates CM estimates when mediator choices are driven by unobserved gains — settings

where none of the natural experiment research designs in the previously cited papers apply (i.e., the mediator is not ignorable). This provides a rigorous warning to applied economists against uncritically applying conventional CM methods to investigate mechanisms in natural experiments. Instead, I propose an alternative approach grounded in classic labour economic theory.

I use the [Roy \(1951\)](#) model as a benchmark for judging the [Imai et al. \(2010\)](#) mediator ignorability assumption in a natural experiment setting, and find it unlikely to hold in practice.<sup>1</sup> This motivates a solution to the identification problem inspired by classic labour economic work, which also uses the Roy model as a benchmark ([Heckman 1979](#), [Heckman & Honore 1990](#)). I follow the lead of these papers by using a control function to correct for the selection bias in conventional CM analyses.

The control function approach requires a mediator take-up respond only positively to an initial treatment, exploiting the instrumental variables equivalence result in a mediation setting ([Vytlacil 2002](#)). Second, it assumes that mediator take-up is motivated by mediator costs and benefits so that first-stage errors inform second-stage unobserved confounding ([Florens, Heckman, Meghir & Vytlacil 2008](#)). Last, it requires a valid instrument for mediator take-up, to avoid relying on parametric assumptions on unobserved selection ([Heckman & Navarro-Lozano 2004](#)). While these assumptions are strong, they are plausible in many applied settings.

This approach to identifying CM effects (despite selection-into-mediator) imports insights from an instrumental variables setting ([Kline & Walters 2019](#)), to account for selection and identifying mediator compliers for CM effects. Doing so is related to using instruments to identify CM effects among instrument complier groups — as noted by [Frölich & Huber \(2017\)](#).<sup>2</sup> Using a control function to estimate CM effects builds on the influential [Imai et al. \(2010\)](#) approach, marrying the CM literature with labour economic theory on selection-into-treatment for the first time.

This paper proceeds as follows. [Section 1](#) introduces the formal framework for CM, and develops expressions for bias in CM estimates in natural experiments. [Section 2](#) describes this bias in applied settings with (1) a regression framework, (2) a setting with selection based on costs and benefits. [Section 3](#) shows how a control function can effectively purge

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<sup>1</sup>An alternative method to estimate CM effects is ensuring treatment and mediator ignorability holds by a running randomised controlled trial (or suitable quasi-experiment) for both treatment and mediator, at the same time. This set-up has been considered in the literature previously, in theory ([Imai, Tingley & Yamamoto 2013](#), [Heckman & Pinto 2015](#)) and in practice ([Ludwig, Kling & Mullainathan 2011](#), [Heckman, Pinto & Savelyev 2013](#)).

<sup>2</sup>Indeed, this paper does not improve on selection model/control function methods, instead noting their applicability in this setting. See [Frölich & Huber \(2017\)](#) for the newest development of control function methods with instruments, and [Wooldridge \(2015\)](#) for a general overview of the approach.

this bias from CM estimates, giving supporting simulation evidence. [Section 4](#) concludes.

## 1 Direct and Indirect Effects

Causal mediation decomposes causal effects into two channels, through a mediator (indirect effect) and through all other paths (direct effect). To develop notation for direct and indirect effects, write  $Z_i$  for an exogenous binary treatment,  $D_i$  a binary mediator, and  $Y_i$  an outcome for individuals  $i = 1, \dots, n$ . The outcomes are a sum of their potential outcomes.<sup>3</sup>

$$\begin{aligned} D_i &= Z_i D_i(1) + (1 - Z_i) D_i(0), \\ Y_i &= Z_i Y_i(1, D_i(1)) + (1 - Z_i) Y_i(0, D_i(0)). \end{aligned}$$

Assume  $Z_i$  is ignorable.<sup>4</sup>

$$Z_i \perp\!\!\!\perp D_i(z), Y_i(z', d), \text{ for } z, z', d = 0, 1$$

There are only two average effects which are identified (without additional assumptions).

1. The average first-stage refers to the effect of the treatment on mediator,  $Z \rightarrow D$ :

$$\mathbb{E}[D_i | Z_i = 1] - \mathbb{E}[D_i | Z_i = 0] = \mathbb{E}[D_i(1) - D_i(0)].$$

It is common in the economics literature to assume that  $Z$  influences  $D$  in at most one direction,  $\Pr(D_i(1) \geq D_i(0)) = 1$  — monotonicity ([Imbens & Angrist 1994](#)). I assume mediator monotonicity (and its conditional variant) holds throughout to simplify notation.

2. The Average Treatment Effect (ATE) refers to the effect of the treatment on outcome,  $Z \rightarrow Y$ , and is also known as the average total effect or intent-to-treat effect in social science settings, or reduced-form effect in the instrumental variables literature:

$$\mathbb{E}[Y_i | Z_i = 1] - \mathbb{E}[Y_i | Z_i = 0] = \mathbb{E}[Y_i(1, D_i(1)) - Y_i(0, D_i(0))].$$

$Z$  affects outcome  $Y$  directly, and indirectly via the  $D(Z)$  channel, with no reverse causality. [Figure 1](#) visualises the design, where the direction arrows denote the causal

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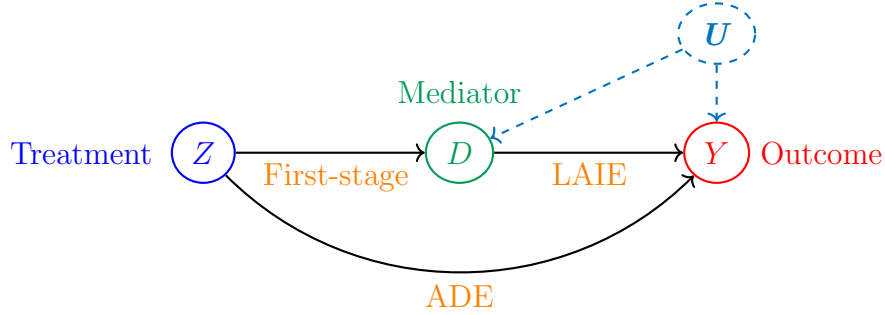
<sup>3</sup>This paper exclusively focuses on the binary case. See [Huber, Hsu, Lee & Lettry \(2020\)](#) for a discussion of CM with continuous treatment and/or mediator, and the assumptions required.

<sup>4</sup>This assumption can hold conditional on covariates. To simplify notation in this section, leave the conditional part unsaid, as it changes no part of the identification framework.

direction. CM aims to decompose the ATE of  $Z \rightarrow Y$  into these two separate pathways:

$$\begin{aligned} \text{Average Direct Effect (ADE), } Z \rightarrow Y : & \mathbb{E} [Y_i(1, D_i(Z_i)) - Y_i(0, D_i(Z_i))] , \\ \text{Average Indirect Effect (AIE), } D(Z) \rightarrow Y : & \mathbb{E} [Y_i(Z_i, D_i(1)) - Y_i(Z_i, D_i(0))] . \end{aligned}$$

**Figure 1:** Structural Causal Model for Causal Mediation.



**Note:** This figure shows the structural causal model behind causal mediation. LAIE refers to the AIE (i.e., effect of the mediator  $D \rightarrow Y$ ) local to  $D(Z)$  compliers, so that  $\text{AIE} = \text{average first-stage} \times \text{LAIE}$ . Unobserved confounder  $U$  represents this paper’s focus on the case that  $D$  is not ignorable, by showing an unobserved confounder. [Subsection 2.1](#) formally defines  $U$  in an applied setting.

Estimating the AIE answers the following question: how much of the causal effect  $Z \rightarrow Y$  goes through the  $D$  channel? If a researcher is studying the health gains of health insurance ([Finkelstein et al. 2012](#)), and wants to study the role of healthcare usage, the AIE represents how much of the effect comes from using the hospital more often. Estimating the ADE answers the following equation: how much is left over after accounting for the  $D$  channel?<sup>5</sup> For the health insurance example, how much of the health insurance effect is a direct effect, other than increased healthcare usage — e.g., long-term effects of lower medical debt, or less worry over health shocks. An instrumental variables approach assumes this direct effect is zero for everyone (the exclusion restriction). CM is a similar, yet distinct, framework attempting to explicitly model the direct effect, and not assuming it is zero.

The ADE and AIE are not separately identified without further assumptions.

## 1.1 Identifying Causal Mediation (CM) Effects

The conventional approach to estimating direct and indirect effects assumes both  $Z_i$  and  $D_i$  are ignorable, conditional on a vector of control variables  $\mathbf{X}_i$ .

<sup>5</sup>In a non-parametric setting it is not necessary that  $\text{ADE} + \text{AIE} = \text{ATE}$ . See [Imai et al. \(2010\)](#) for this point in full.

**Definition 1.** *Sequential Ignorability* (Imai et al. 2010).

$$Z_i \perp\!\!\!\perp D_i(z), Y_i(z', d) \mid \mathbf{X}_i, \quad \text{for } z, z', d = 0, 1 \quad (1)$$

$$D_i \perp\!\!\!\perp Y_i(z', d) \mid \mathbf{X}_i, Z_i = z', \quad \text{for } z', d = 0, 1 \quad (2)$$

Sequential ignorability assumes that the initial treatment  $Z_i$  is ignorable conditional on  $\mathbf{X}_i$ . It then also assumes that, after  $Z_i$  is assigned, that  $D_i$  is ignorable conditional on  $\mathbf{X}, Z$ . In addition, a common support condition for both  $Z_i, D_i$  (across  $\mathbf{X}_i$ ) is necessary. If sequential ignorability, 1(1) and 1(2), holds then the ADE and AIE are identified by two-stage mean differences, after conditioning on  $\mathbf{X}_i$ .<sup>6</sup>

$$\begin{aligned} \mathbb{E}_{D_i, \mathbf{X}_i} \left[ \underbrace{\mathbb{E}[Y_i \mid Z_i = 1, D_i, \mathbf{X}_i] - \mathbb{E}[Y_i \mid Z_i = 0, D_i, \mathbf{X}_i]}_{\text{Second-stage regression, } Y_i \text{ on } Z_i \text{ holding } D_i, \mathbf{X}_i \text{ constant}} \right] &= \underbrace{\mathbb{E}[Y_i(1, D_i(Z_i)) - Y_i(0, D_i(Z_i))]}_{\text{Average Direct Effect (ADE)}} \\ \mathbb{E}_{Z_i, \mathbf{X}_i} \left[ \underbrace{\left( \mathbb{E}[D_i \mid Z_i = 1, \mathbf{X}_i] - \mathbb{E}[D_i \mid Z_i = 0, \mathbf{X}_i] \right)}_{\text{First-stage regression, } D_i \text{ on } Z_i} \times \underbrace{\left( \mathbb{E}[Y_i \mid Z_i, D_i = 1, \mathbf{X}_i] - \mathbb{E}[Y_i \mid Z_i, D_i = 0, \mathbf{X}_i] \right)}_{\text{Second-stage regression, } Y_i \text{ on } D_i \text{ holding } Z_i, \mathbf{X}_i \text{ constant}} \right] \\ &= \underbrace{\mathbb{E}[Y_i(Z_i, D_i(1)) - Y_i(Z_i, D_i(0))]}_{\text{Average Indirect Effect (AIE)}} \end{aligned}$$

I refer to the estimands on the left-hand side as Causal Mediation (CM) estimands. These estimands are typically estimated with linear models, with resulting estimates composed from two-stage Ordinary Least Squares (OLS) estimates (Imai et al. 2010). While this is the most common approach in the applied literature, I do not assume the linear model. Linearity assumptions are unnecessary to my analysis; it suffices to note that heterogeneous treatment effects and non-linear confounding would bias OLS estimates of CM estimands in the same manner that is well documented elsewhere (see e.g., Angrist 1998, Słoczyński 2022). This section focuses on problems that plague CM by selection-on-observables, regardless of estimation method.

## 1.2 Bias in Causal Mediation (CM) Estimates

Applied researchers often use a natural experiment to study settings where treatment  $Z_i$  is ignorable, justifying assumption 1(1). Rarely do they also have access to additional, overlapping natural experiment to isolate random variation  $D_i$  to justify assumption 1(2) as

<sup>6</sup>Imai et al. (2010) show a general identification statement; I show identification in terms of two-stage regression, notation for which is more familiar in economics. This reasoning is in line with G-computation reasoning (Robins 1986); Subsection A.1 states the Imai et al. (2010) identification result, and then develops the two-stage regression notation which holds as a consequence of sequential ignorability.



part of the same analysis. One might consider conventional CM methods in such a setting to learn about the mechanisms behind the causal effect  $Z \rightarrow Y$  under study. This approach leads to biased estimates, and contaminates inference regarding direct and indirect effects.

**Theorem 1.** *Absent an identification strategy for the mediator, causal mediation estimates are at risk of selection bias. Suppose 1(1) holds, but 1(2) does not. Then CM estimands are contaminated by selection bias and group differences.*

*Proof.* See Subsection A.2 for the proof. Below I present the relevant selection bias and group difference terms, omitting the conditional on  $\mathbf{X}_i$  notation for brevity.  $\square$

For the direct effect: CM estimand = ADE + selection bias + group differences.<sup>7</sup>

$$\begin{aligned} & \mathbb{E}_{D_i} \left[ \mathbb{E} [Y_i | Z_i = 1, D_i] - \mathbb{E} [Y_i | Z_i = 0, D_i] \right] \\ &= \mathbb{E} [Y_i(1, D_i(Z_i)) - Y_i(0, D_i(Z_i))] \\ &+ \mathbb{E}_{D_i=d'} \left[ \mathbb{E} [Y_i(0, D_i(Z_i)) | D_i(1) = d'] - \mathbb{E} [Y_i(0, D_i(Z_i)) | D_i(0) = d'] \right] \\ &+ \mathbb{E}_{D_i=d'} \left[ \left( 1 - \Pr(D_i(1) = d') \right) \left( \mathbb{E} [Y_i(1, D_i(Z_i)) - Y_i(0, D_i(Z_i)) | D_i(1) = 1 - d'] \right) \right. \\ &\quad \left. - \mathbb{E} [Y_i(1, D_i(Z_i)) - Y_i(0, D_i(Z_i)) | D_i(1) = d'] \right] \end{aligned}$$

For the indirect effect: CM estimand = AIE + selection bias + group differences.

$$\begin{aligned} & \mathbb{E}_{Z_i} \left[ \left( \mathbb{E} [D_i | Z_i = 1] - \mathbb{E} [D_i | Z_i = 0] \right) \times \left( \mathbb{E} [Y_i | Z_i, D_i = 1] - \mathbb{E} [Y_i | Z_i, D_i = 0] \right) \right] \\ &= \mathbb{E} [Y_i(Z_i, D_i(1)) - Y_i(Z_i, D_i(0))] \\ &+ \Pr(D_i(1) = 1, D_i(0) = 0) \left( \mathbb{E} [Y_i(Z_i, 0) | D_i = 1] - \mathbb{E} [Y_i(Z_i, 0) | D_i = 0] \right) \\ &+ \Pr(D_i(1) = 1, D_i(0) = 0) \times \\ &\quad \left[ \left( 1 - \Pr(D_i = 1) \right) \left( \mathbb{E} [Y_i(Z_i, 1) - Y_i(Z_i, 0) | D_i = 1] \right. \right. \\ &\quad \left. \left. - \mathbb{E} [Y_i(Z_i, 1) - Y_i(Z_i, 0) | D_i = 0] \right) \right. \\ &\quad \left. - \left( \frac{1 - \Pr(D_i(1) = 1, D_i(0) = 0)}{\Pr(D_i(1) = 1, D_i(0) = 0)} \right) \left( \mathbb{E} [Y_i(Z_i, 1) - Y_i(Z_i, 0) | D_i(1) = 0 \text{ or } D_i(0) = 1] \right) \right] \end{aligned}$$

The selection bias terms come from systematic differences between the groups taking or refusing the mediator ( $D_i = 1$  versus  $D_i = 0$ ), differences not fully unexplained by  $\mathbf{X}_i$ . These

<sup>7</sup>The bias terms here mirror those in Heckman, Ichimura, Smith & Todd (1998), Angrist & Pischke (2009) for a single  $D \rightarrow Y$  treatment effect, when  $D_i$  is not ignorable:

$$\mathbb{E} [Y_i | D_i = 1] - \mathbb{E} [Y_i | D_i = 0] = \text{ATE} + \underbrace{\left( \mathbb{E} [Y_i(., 0) | D_i = 1] - \mathbb{E} [Y_i(., 0) | D_i = 0] \right)}_{\text{Selection Bias}} + \underbrace{\Pr(D_i = 0) (\text{ATT} - \text{ATU})}_{\text{Group-differences Bias}}.$$



selection bias terms would equal zero if the mediator had been ignorable 1(2), but do not necessarily average to zero if not.

The group differences represent the fact that a matching approach gives an average effect on the treated group and, when selection-on-observables does not hold, this is systematically different from the average effect (Heckman et al. 1998). These terms are a non-parametric framing of the bias from controlling for intermediate outcomes, previously studied only in a linear setting (i.e., bad controls in Cinelli, Forney & Pearl 2024, or M-bias in Ding & Miratrix 2015).

The AIE group differences term is longer, because the indirect effect is comprised of the effect of  $D_i$  local to  $Z_i$  compliers.

$$\text{AIE} = \mathbb{E} [Y_i(Z_i, D_i(1)) - Y_i(Z_i, D_i(0))] = \mathbb{E} [D_i(1) - D_i(0)] \underbrace{\mathbb{E} [Y_i(Z_i, 1) - Y_i(Z_i, 0) \mid D_i(1) = 1, D_i(0) = 0]}_{\text{Average } D \rightarrow Y \text{ effect among } D(z) \text{ compliers}}$$

This group differences term in the AIE arises because the selection-on-observables approach assumes that this complier average effect is equal to the population average effect, which does not hold true if the mediator is not ignorable.

## 2 Causal Mediation (CM) in Applied Settings

Unobserved confounding is particularly problematic when studying the mechanisms behind treatment effects. For example, in studying health gains from health insurance, we might expect that health gains came about because those with new insurance started visiting their healthcare provider more often, when in past they forewent using healthcare over financial concerns (Finkelstein et al. 2012). Applying conventional CM methods to investigate this expectation would be dismissing unobserved confounders for how often individuals visit healthcare providers, leading to biased results.

The wider population does not have one uniform bill of health; many people are born predisposed to ailments, due to genetic variation or other unrelated factors. Many of these conditions exist for years before being fully diagnosed, and recorded in health record databases. Suffers of severe underlying conditions may visit healthcare providers more often than the rest of the population, to investigate or begin treating the ill-effects. It stands to reason that people with more severe underlying conditions may gain more from more often attending healthcare providers once given health insurance. These underlying causes for responding more to new access to health insurance cannot be controlled for by researchers, as they are not even known to the individuals themselves before being fully diagnosed. This means underlying health conditions are an unobserved confounder, and will bias estimates of the

ADE and AIE in this setting.

In this section, I further develop the issue of selection on unobserved factors in a general CM setting. First, I show the non-parametric bias terms from [Section 1](#) can be written as omitted variables bias in a regression framework. Second, I show how selection bias operates in a basic model for selection-into-mediator based on costs and benefits.

## 2.1 Regression Framework

Inference for CM effects can be written in a regression framework, showing how correlation between the error term and the mediator persistently biases estimates.

Start by writing potential outcomes  $Y_i(\cdot, \cdot)$  as a sum of observed and unobserved factors, following the notation of [Heckman & Vytlacil \(2005\)](#). For each  $z', d' = 0, 1$ , put  $\mu_{d'}(z'; \mathbf{X}) = \mathbb{E}[Y_i(z', d') | \mathbf{X}]$  and the corresponding error terms,  $U_{d',i} = Y_i(z', d') - \mu_{d'}(z'; \mathbf{X})$ , so we have the following expressions:

$$Y_i(Z_i, 0) = \mu_0(Z_i; \mathbf{X}_i) + U_{0,i}, \quad Y_i(Z_i, 1) = \mu_1(Z_i; \mathbf{X}_i) + U_{1,i}.$$

In these terms, the ADE and AIE are represented as follows,

$$\begin{aligned} \text{ADE} &= \mathbb{E} \left[ D_i \left( \mu_1(1; \mathbf{X}_i) - \mu_1(0; \mathbf{X}_i) \right) + (1 - D_i) \left( \mu_0(1; \mathbf{X}_i) - \mu_0(0; \mathbf{X}_i) \right) \right], \\ \text{AIE} &= \mathbb{E} \left[ \left( D_i(1) - D_i(0) \right) \times \left( \mu_1(Z_i; \mathbf{X}_i) - \mu_0(Z_i; \mathbf{X}_i) + U_{1,i} - U_{0,i} \right) \right]. \end{aligned}$$

With this notation, observed data  $Z_i, D_i, Y_i, \mathbf{X}_i$  have the following outcome equations — which characterise direct effects, indirect effects, and selection bias.

$$D_i = \phi + \bar{\pi}Z_i + \zeta(\mathbf{X}_i) + \eta_i \tag{3}$$

$$Y_i = \alpha + \beta D_i + \gamma Z_i + \delta Z_i D_i + \varphi(\mathbf{X}_i) + \underbrace{(1 - D_i)U_{0,i} + D_i U_{1,i}}_{\text{Correlated error term.}} \tag{4}$$

First-stage (3) is identified, with  $\phi + \zeta(\mathbf{X}_i)$  the intercept, and  $\bar{\pi}$  the first-stage compliance rate (which may depend on  $\mathbf{X}_i$ ). Second-stage (4) has the following definitions, and is not identified thanks to omitted variables bias.<sup>8</sup>

- (a)  $\alpha = \mathbb{E}[\mu_0(0; \mathbf{X}_i)]$  and  $\varphi(\mathbf{X}_i) = \mu_0(0; \mathbf{X}_i) - \alpha$  are the intercept terms.
- (b)  $\beta = \mu_1(0; \mathbf{X}_i) - \mu_0(0; \mathbf{X}_i)$  is the AIE local to  $Z_i = 0$ .
- (c)  $\gamma = \mu_0(1; \mathbf{X}_i) - \mu_0(0; \mathbf{X}_i)$  is the ADE local to  $D_i = 0$ .

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<sup>8</sup>See [Subsection A.3](#) for the derivation.

- (d)  $\delta = \mu_1(1; \mathbf{X}_i) - \mu_0(1; \mathbf{X}_i) - (\mu_1(0; \mathbf{X}_i) - \mu_0(0; \mathbf{X}_i))$  is the average interaction effect.
- (e)  $(1 - D_i)U_{0,i} + D_iU_{1,i}$  is the disruptive error term.

The ADE and AIE are averages of these regression coefficients.

$$\text{ADE} = \mathbb{E} [\gamma + \delta D_i],$$

$$\text{AIE} = \mathbb{E} \left[ \bar{\pi} \left( \beta + \delta Z_i + \tilde{U}_i \right) \right], \quad \text{with } \tilde{U}_i = \underbrace{\mathbb{E} [D_i U_{1,i} - (1 - D_i) U_{0,i} \mid \mathbf{X}_i, D_i(1) = 1, D_i(0) = 0]}_{\text{Unobserved complier gains}}.$$

The ADE is a simple sum of the coefficients, while the AIE includes a group differences term because it only refers to  $D(z)$  compliers.

By construction,  $\mathbf{U}_i := (U_{0,i}, U_{1,i})$  is an unobserved confounder. The regression estimates of  $\beta, \gamma, \delta$  in second-stage (4) give unbiased estimates only if  $D_i$  is also conditionally ignorable:  $D_i \perp\!\!\!\perp \mathbf{U}_i$ . If not, then estimates of CM effects suffer from omitted variables bias from failing to adjust for the unobserved confounder,  $\mathbf{U}_i$ .

## 2.2 Selection on Costs and Benefits

CM is at risk of bias because  $D_i \perp\!\!\!\perp (U_{0,i}, U_{1,i})$  is unlikely to hold in applied settings. A separate identification strategy could disrupt the selection-into- $D_i$  based on unobserved factors, and lend credibility to the mediator ignorability assumption. Without it, bias will persist, given how we conventionally think of selection-into-treatment.

Consider a model where individual  $i$  selects into a mediator based on costs and benefits (in terms of outcome  $Y_i$ ), after  $Z_i, \mathbf{X}_i$  have been assigned. In a natural experiment setting, an external factor has disrupted individuals selecting  $Z_i$  by choice (thus  $Z_i$  is ignorable), but it has not disrupted the choice to take mediator (thus  $D_i$  is not ignorable). Write  $C_i$  for individual  $i$ 's costs of taking mediator  $D_i$ , and  $\mathbb{1}\{\cdot\}$  for the indicator function. The Roy model has  $i$  taking the mediator if the benefits exceed the costs,

$$D_i(z') = \mathbb{1} \left\{ \underbrace{Y_i(z', 1) - Y_i(z', 0)}_{\text{Benefits}} \geq \underbrace{C_i}_{\text{Costs}} \right\}, \quad \text{for } z' = 0, 1. \quad (5)$$

The Roy model provides an intuitive framework for analysing selection mechanisms because it captures the fundamental economic principle of decision-making based on costs and benefits in terms of the outcome under study (Roy 1951, Heckman & Honore 1990). If the outcome  $Y_i$  is a measure of income, and the mediator a choice of taking education, then it models an individual choice to attend more education in terms of gaining a higher

income compared to the costs.<sup>9</sup> This makes it particularly useful as a base case for CM, where selection-into-the mediator may be driven by private information (unobserved by the researcher). By using the Roy model as a benchmark, I explore the practical limits of the mediator ignorability assumption.

Decompose the costs into its mean and an error term,  $C_i(Z_i) = \mu_C(Z_i; \mathbf{X}_i) + U_{C,i}$ , to give a representation of Roy selection in terms of observed and unobserved factors,

$$D_i(z') = 1 \left\{ \mu_1(z'; \mathbf{X}_i) - \mu_0(z'; \mathbf{X}_i) - \mu_C(z'; \mathbf{X}_i) \geq U_{C,i} - (U_{1,i} - U_{0,i}) \right\}, \quad \text{for } z' = 0, 1.$$

If selection is Roy style, and the mediator is ignorable, then unobserved benefits play no part in selection. The only driver in differences in selection are differences in costs (and not benefits). If there are any unobserved benefits for selection-into- $D_i$  unobserved to the researcher, then sequential ignorability cannot hold.

**Definition 2.** *Suppose mediator selection follows a Roy model (5), and selection is not fully explained by costs and observed gains. Then sequential ignorability does not hold.*

If there are any unobserved sources of gains, then sequential ignorability does not hold. This is an equivalence statement: selection based on costs and benefits is only consistent with mediator ignorability if the researcher observed every single source of mediator benefits. See [Subsection A.4](#) for the proof.

This means than the vector of control variables  $\mathbf{X}_i$  must be incredibly rich. Together,  $\mathbf{X}_i$  and unobserved cost differences  $U_{C,i}$  must explain selection-into- $D_i$  one hundred percent. In the Roy model framework, however, individuals make decisions about mediator take-up based on gains, which the researcher may not observe fully. These unobservables are unlikely to be fully captured by an observed control set  $\mathbf{X}_i$ , except in very special cases (see e.g., the discussion in [Angrist & Pischke 2009](#), [Angrist 2022](#)). In practice, the only way to believe in the ignorability assumption is to study a setting where the researcher has a causal research design for both treatment  $Z_i$  and mediator  $D_i$ , at the same time. A simple addition of “we assume the mediator satisfies selection-on-observables” will not cut it here, and will lead to biased inference in practice.

### 3 Solving Identification with a Control Function (CF)

If your goal is to estimate CM effects, and you could control for unobserved selection terms  $U_{0,i}, U_{1,i}$ , then you would. This ideal (but infeasible) scenario would yield unbiased estimates

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<sup>9</sup>If the choice is made for a sum of outcomes, then a simple extension to a utility maximisation model maintains this same framework. See [Heckman & Honore \(1990\)](#), [Eisenhauer, Heckman & Vytlacil \(2015\)](#).

for the ADE and AIE. A Control Function (CF) approach takes this insight seriously, providing conditions to model the implied confounding by  $U_{0,i}, U_{1,i}$ , and then controlling for it.

The main problem is that second-stage regression equation (4) is not identified, because  $U_{0,i}, U_{1,i}$  are unobserved.

$$\begin{aligned} \mathbb{E}[Y_i | Z_i, D_i, \mathbf{X}_i] &= \alpha + \beta D_i + \gamma Z_i + \delta Z_i D_i + \varphi(\mathbf{X}_i) \\ &+ (1 - D_i) \mathbb{E}[U_{0,i} | D_i = 0, \mathbf{X}_i] + D_i \mathbb{E}[U_{1,i} | D_i = 1, \mathbf{X}_i] \end{aligned} \quad (6)$$

CF methods were first devised to correct for sample selection problems (Heckman 1974), and were extended to a general selection problem (Heckman 1979). The approach works in the following manner: (1) assume that the variable of interest follows a selection model, where unexplained first-stage selection informs unobserved second-stage confounding; (2) extract information about unobserved confounding from the first-stage; and (3) incorporate this information as control terms in the second-stage equation to adjust for selection-into-mediator. Identification in CF methods typically relies on either distributional assumptions on the unobserved error terms, or an exclusion restriction for instrumental variables in the first-stage, or both. By explicitly accounting for the information contained in the first-stage selection model, CF methods enable consistent estimation of causal effects in the second-stage even when selection is driven by unobserved factors (Florens et al. 2008).

In the example of analysing health gains from health insurance (Finkelstein et al. 2012), a CF approach could be used to address the unobserved confounding from not observing underlying health conditions. It would do so by assuming that unobserved selection-into-more frequent health care usage is informative for underlying health conditions; this assumes people with more severe underlying conditions visit the doctor (weakly) more often than those without. Then it controls for this in the second-stage estimation of how much the effect goes through increased usage of healthcare (i.e., the ADE and AIE).

### 3.1 Reidentifying Causal Mediation (CM) Effects

The following assumptions are sufficient to model the correlated error terms, identifying  $\beta, \gamma, \delta$  in the second-stage regression (6), and thus both the ADE and AIE.

**Assumption CF–1.** Mediator monotonicity.

$$\Pr(D_i(1) \geq D_i(0) | \mathbf{X}_i) = 1.$$

Assumption CF–1 is the monotonicity condition first used in an instrumental variables

context (Imbens & Angrist 1994). Here, it is assuming that people respond to treatment,  $Z_i$ , by consistently taking or refusing the mediator  $D_i$  (always or never-takers), or taking the mediator  $D_i$  if and only if assigned to the treatment  $Z_i = 1$  (mediator compliers). There are no mediator defiers.

The main implication of Assumption CF-1 is that selection-into-mediator can be written as a selection model (Vytlacil 2002).

$$D_i(z') = \mathbb{1} \{ \psi(z'; \mathbf{X}_i) \geq V_i \}, \quad \text{for } z' = 0, 1$$

where  $V_i$  is a latent variable with continuous distribution and cumulative density function (CDF)  $F_V(\cdot)$ , and  $\psi(\cdot)$  collects observed sources of mediator selection.  $V_i$  can be assumed to follow a known distribution; the canonical Heckman selection model (“Heckit”) assumes  $V_i$  is normally distributed.

I focus on the equivalent transformed model of Heckman & Vytlacil (2005),

$$D_i(z') = \mathbb{1} \{ \pi(z'; \mathbf{X}_i) \geq U_i \}, \quad \text{for } z' = 0, 1$$

where  $U_i := F_V(V_i)$  follows a uniform distribution, and  $\pi(z'; \mathbf{X}_i) = F_V(\psi(z'; \mathbf{X}_i)) = \mathbb{E}[D_i | Z_i = z', \mathbf{X}_i]$  is the mediator propensity score.  $U_i$  is the unobserved mediator take-up costs.

Note the maintained assumption that treatment  $Z_i$  is ignorable (conditional on  $\mathbf{X}_i$ ) implies  $Z_i \perp\!\!\!\perp U_i$ . This selection model setup is equivalent to the monotonicity condition, and is importing an equivalence result from the instrumental variables literature to the CM setting.

**Assumption CF-2.** Selection on mediator costs and benefits.

$$\text{Cov}(U_i, U_{0,i}), \text{Cov}(U_i, U_{1,i}) \neq 0.$$

Assumption CF-2 is stating that unobserved selection in mediator take-up ( $U_i$ ) informs second-stage confounding, both when refusing or taking the mediator (i.e.,  $U_{0,i}$  or  $U_{1,i}$ ).

This is a strong assumption, and will not hold in all examples. If people had been deciding to take  $D_i$  by a Roy model, then this assumption holds because  $V_i = U_{C,i} - (U_{1,i} - U_{0,i})$ . Individuals could be making decisions based on other outcomes, but as long as mediator costs and benefits guide at least part of this decision (i.e., bounded away from zero), then this assumption will hold.

For notation purposes, suppose the vector of control variables  $\mathbf{X}_i$  has at least two entries; denote  $\mathbf{X}_i^{\text{IV}}$  as one entry in the vector, and  $\mathbf{X}_i^-$  as the remaining.

**Assumption CF–3.** Mediator take-up cost instrument.

$$\mathbf{X}_i^{\text{IV}} \text{ satisfies } \frac{\partial}{\partial \mathbf{X}_i^{\text{IV}}} [\mu_1(z', \mathbf{X}_i) - \mu_0(z', \mathbf{X}_i)] = 0 < \frac{\partial}{\partial \mathbf{X}_i^{\text{IV}}} \mathbb{E}[D_i(z') | \mathbf{X}_i], \text{ for } z' = 0, 1.$$

Assumption CF–3 is requiring at least one control variable guides selection-into- $D_i$  — an instrumental variable. It assumes an instrument exists, which satisfies an exclusion restriction (i.e., not impacting mediator gains  $\mu_1 - \mu_0$ ), and has a non-zero influence on the mediator (i.e., strong first-stage). The exclusion restriction is untestable, and must be guided by domain-specific knowledge; first-stage strength is testable, and must be justified with data by methods common in the instrumental variables literature.

This assumption identifies the mediator propensity score, avoiding indeterminacy in the second-stage outcome equation. While not technically required for identification, it avoids relying entirely on an assumed distribution for unobserved error terms (and bias from inevitably breaking it). The most compelling example of a mediator instrument is using data on the cost of mediator take-up as a first-stage instrument, if it varies between individuals for unrelated reasons and is strong in explaining mediator take-up.

**Proposition 1.** If assumptions CF–1, CF–2, CF–3 hold, then second-stage regression equation (4) is identified with a CF adjustment.

$$\begin{aligned} \mathbb{E}[Y_i | Z_i, D_i, \mathbf{X}_i] &= \alpha + \beta D_i + \gamma Z_i + \delta Z_i D_i + \varphi(\mathbf{X}_i) \\ &\quad + \rho_0 (1 - D_i) \lambda_0(\pi(Z_i; \mathbf{X}_i)) + \rho_1 D_i \lambda_1(\pi(Z_i; \mathbf{X}_i)), \end{aligned}$$

where  $\lambda_0, \lambda_1$  are the Control Functions (CFs), mediator propensity score  $\pi(z'; \mathbf{X}_i)$  is identified in the first-stage (3), and  $\rho_0, \rho_1$  are linear parameters.

The CFs are functions which measure unobserved mediator gains. Following the notation of Kline & Walters (2019) in an instrumental variables setting, put  $\mu_V = \mathbb{E}[F_V^{-1}(U_i)]$ , to give the following representation for the CFs for  $p' \in (0, 1)$ :

$$\begin{aligned} \lambda_0(p') &= \mathbb{E}[F_V^{-1}(U_i) - \mu_V | p' < U_i], \\ \lambda_1(p') &= \mathbb{E}[F_V^{-1}(U_i) - \mu_V | U_i \leq p'] = -\lambda_0(p') \left( \frac{1 - p'}{p'} \right). \end{aligned}$$

If we are using the canonical Heckman selection model, we assume the error term follows a normal distribution, so that  $\lambda_0, \lambda_1$  are the inverse Mills ratio. Alternatively,  $\lambda_0, \lambda_1$  could be linear functionals (Wooldridge 2015), or have other definitions following on the assumed distribution of the error terms. If we do not know what distribution class the errors follow, then  $\lambda_0, \lambda_1$  can be estimated with semi-parametric methods.



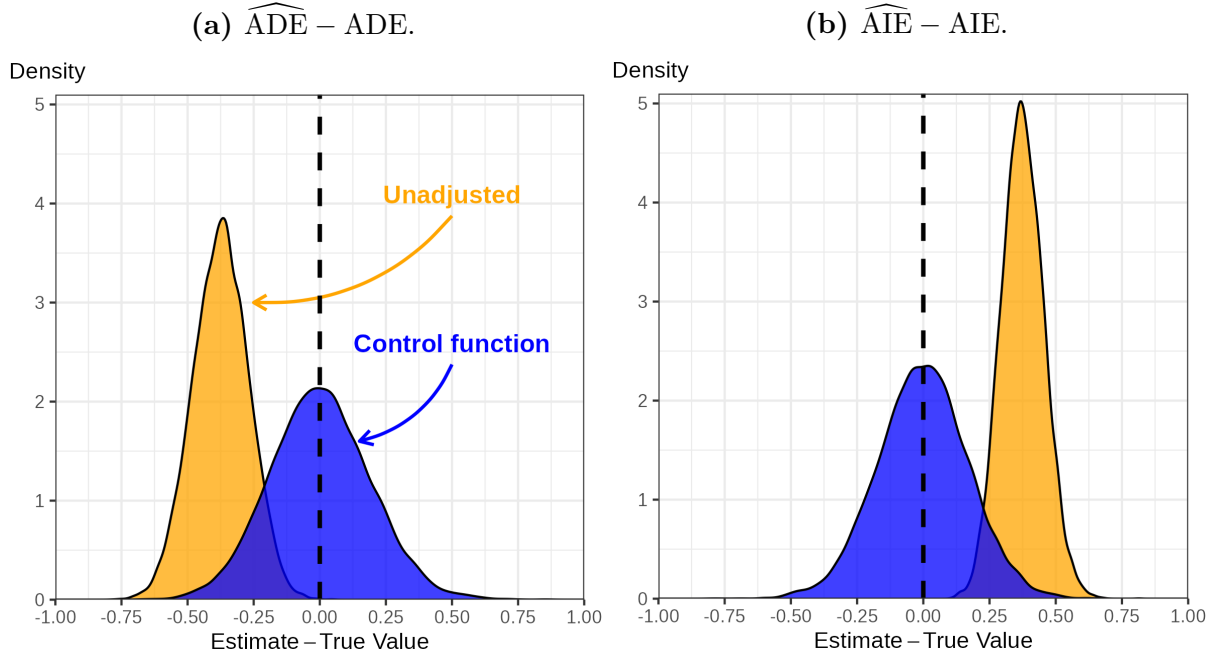
**Theorem CF.** If assumptions [CF-1](#), [CF-2](#), [CF-3](#) hold, the ADE and AIE are identified as a function of the parameters identified in [Proposition 1](#).

$$\begin{aligned} \text{ADE} &= \mathbb{E} [\gamma + \delta D_i], \\ \text{AIE} &= \mathbb{E} \left[ \bar{\pi} \left( \beta + \delta Z_i + \underbrace{(\rho_1 - \rho_0) \Gamma(\pi(0; \mathbf{X}_i), \pi(1; \mathbf{X}_i))}_{\text{Complier adjustment}} \right) \right] \end{aligned}$$

where  $\Gamma(p, p') = \mathbb{E} [F_V^{-1}(U_i) - \mu_V \mid p < U_i \leq p'] = \frac{p' \lambda_1(p') - p \lambda_1(p)}{p' - p}$ , and  $\bar{\pi} = \pi(1; \mathbf{X}_i) - \pi(0; \mathbf{X}_i)$  is the mediator complier score.

This theorem provides a solution to the identification problem for CM effects when facing selection bias; rather than assuming away selection problems, it explicitly models them. The ADE is straightforward to calculate as an average of the direct effect parameters, while the AIE also includes an adjustment for unobserved complier gains to the mediator. Again, this is because the AIE only refers to individuals who were induced by treatment  $Z_i$  into taking mediator  $D_i$  (mediator compliers). The CFs allow us to measure both selection bias and complier differences, purging persistent bias in CM effect analyses.

**Figure 2:** Simulated Distribution of CM Effect Estimates, Relative to True Value.



**Note:** These figures show the empirical density of point estimates, for 10,000 different datasets generated from a Roy model with correlated normally distributed error terms (further described in ??). The black dashed line is the true value; orange is the distribution of conventional CM estimates from two-stage OLS ([Imai et al. 2010](#)), and blue estimates with a two-stage Heckman selection adjustment.

Figure 2 shows how a CF adjustment corrects unadjusted CM estimates. In a simulation with selection based on observed error terms, the CF adjustment pushes conventional CM estimates back to the true value.

### 3.2 Simulation Evidence

The following simulation gives an example to show how this method works in practice. Suppose data observed to the researcher  $Z_i, D_i, Y_i, \mathbf{X}_i$  are drawn from the following data generating processes, for  $i = 1, \dots, N$ .

$$\begin{aligned} Z_i &\sim \text{Binom}(0.5), \quad \mathbf{X}_i^- \sim N(4, 1), \quad \mathbf{X}_i^{\text{IV}} \sim \text{Binom}(0.5), \\ (U_{0,i}, U_{1,i}) &\sim \text{BivariateNormal}(0, 0, \sigma_0, \sigma_1, \rho), \quad U_{C,i} \sim N(0, 0.5). \end{aligned}$$

$N = 10,000$  allows the large sample properties of the approach to operate; indeed, smaller sample sizes may not.

Suppose each  $i$  chooses to take mediator  $D_i$  by a Roy model, with following mean definitions for each  $z', d' = 0, 1$ .

$$\begin{aligned} D_i(z') &= \mathbb{1} \{Y_i(z', 1) - Y_i(z', 0) \geq C_i\}, \\ \mu_{d'}(z'; \mathbf{X}_i) &= \mathbf{X}_i^- + (z' + d' + z'd'), \quad \mu_C(z'; \mathbf{X}_i) = 3z' + \mathbf{X}_i^- - \mathbf{X}_i^{\text{IV}}. \end{aligned}$$

Following Section 2, these data have the following first and second-stage equations:

$$\begin{aligned} D_i &= \mathbb{1} \left\{ -3Z_i - \mathbf{X}_i^{\text{IV}} + \mathbf{X}_i^- \geq U_{C,i} - (U_{1,i} - U_{0,i}) \right\}, \\ Y_i &= Z_i + D_i + Z_i D_i + \mathbf{X}_i^- + (1 - D_i) U_{0,i} + D_i U_{1,i}. \end{aligned}$$

$Z_i$  has an effect on outcome  $Y_i$ , and it operates partially through mediator  $D_i$ . Outcome mean  $\mu_{D_i}(Z_i; \cdot)$  contains an interaction term,  $Z_i D_i$ , so while both  $Z_i$  and  $D_i$  have constant partial effects, the ATE depends on how many  $i$  choose to take the mediator. In this simulation  $\Pr(D_i = 1) = 0.437$ , and 65.29% of the sample are mediator compliers (where  $D_i(1) = 1$  and  $D_i(0) = 0$ ). This gives an ATE ( $Z \rightarrow Y$ ) value of 2.58, ADE 1.44, and AIE 1.13, respectively.<sup>10</sup>

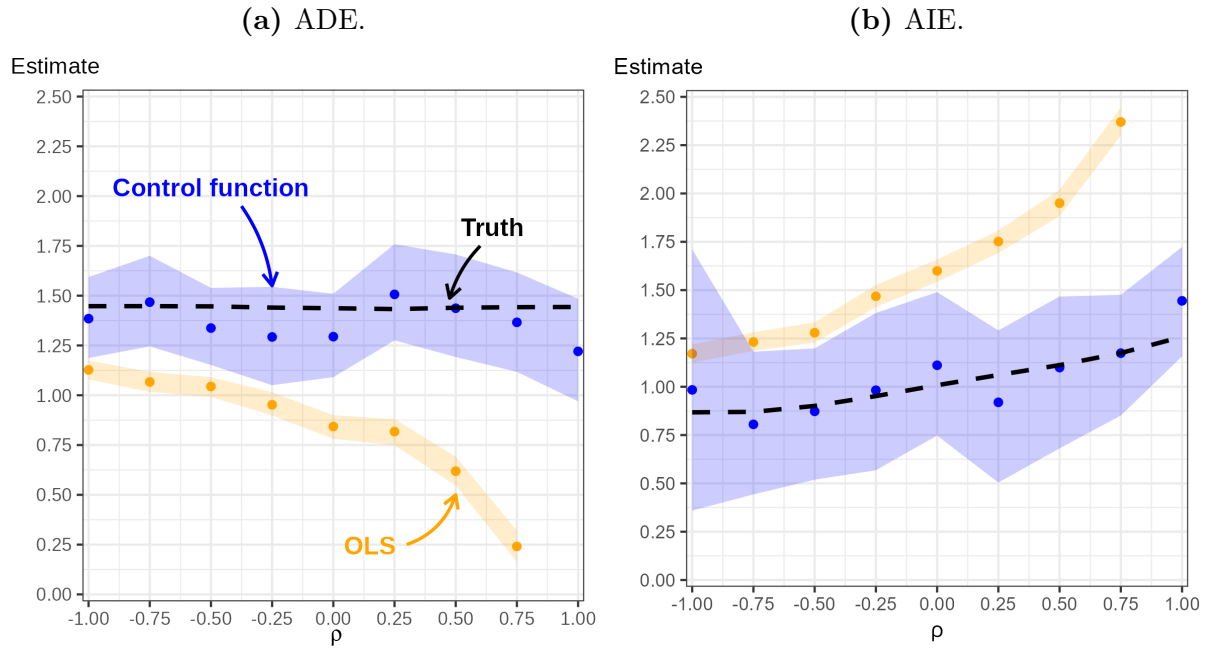
After  $Z_i$  is assigned,  $i$  chooses to take mediator  $D_i$  by considering the costs and benefits — which vary based on  $Z_i$ , demographic controls  $\mathbf{X}_i$ , and the (non-degenerate) unobserved error terms  $U_{i,0}, U_{i,1}$ . As a result, sequential ignorability does not hold; the mediator is not conditionally ignorable. Thus, a standard OLS (selection-on-observables) approach to

<sup>10</sup>Note that  $\text{ATE} = \text{ADE} + \text{AIE}$  in this setting.  $\Pr(Z_i = 1) = 0.5$  ensures this equality, but it is not guaranteed in general.

CM does not give an estimate for how much of the  $Z \rightarrow Y$  ATE goes through mediator  $D$ . Instead, the OLS approach gives biased inference. The bias in OLS estimates comes from the unobserved error terms being related.

The CF estimator here uses the inverse Mills ratio as the CFs (i.e., a Heckman selection model); in-progress work develops a constrained semi-parametric approach to model  $\lambda_0$  (and thus  $\lambda_1$ ) to avoid distributional assumptions. Figure 2 shows the distribution of simulated point estimates in this simulation, showing OLS against the CF approach. The OLS approach implicitly assumes that the mediator is ignorable (when it is not), so its point estimates under and over-estimate the true ADE and AIE, respectively. The distance between the OLS estimates and the true values are the underlying bias terms derived in Theorem 1. In this data generating process, the OLS confidence interval do not overlap the true values for any standard level of significance. The CF approach exhibits bias, though the 95% confidence intervals cover the truth.

**Figure 3:** Point Estimates of CM Effects, OLS versus CF, varying  $\rho$  values with  $\sigma_0 = 1, \sigma_1 = 2$  fixed.



**Note:** These figures show the OLS and CF point estimates of the ADE and AIE, for  $N = 10,000$  sample size. The black dashed line is the true value, points are points estimates from data simulated with a given  $\rho$  value and  $\sigma_0 = 1, \sigma_1 = 2$ , and shaded regions are the 95% confidence intervals from 1,000 bootstraps each. Orange represents OLS estimates, blue the CF approach. The true AIE values vary with  $\rho$ , because  $D_i(Z_i)$  compliers have higher average values of  $U_{1,i} - U_{0,i}$  with greater  $\rho$  values.

The error terms determine the bias in OLS estimates of the ADE and AIE, so the bias

varies for different values of the error-term parameters  $\rho \in [-1, 1]$  and  $\sigma_0, \sigma_1 \geq 0$ .<sup>11</sup> Figure 3 shows CF estimates against estimates calculated by standard OLS, showing 95% confidence intervals calculated from 1,000 bootstraps. The point estimates of the CF do not exactly equal the true values, as they are estimates from one simulation (not averages across many simulations, as in Figure 2). The CF approach improves on OLS estimates by correcting for bias, with confidence regions overlapping the true values.<sup>12,13</sup> This correction did not come for free: the standard errors are significantly greater in a CF approach than OLS. Standard errors on the AIE are larger than those for the ADE, because the AIE estimates are first-stage times second-stage estimates, so standard errors account for uncertainty in both estimates multiplied. In this manner, this simulation shows the pros and cons of using the CF approach to estimating CM effects in practice.

## 4 Summary and Concluding Remarks

This paper has studied a selection-on-observables approach to CM in a natural experiment setting. I have shown the pitfalls of using the most popular methods for estimating direct and indirect effects without a clear case for the mediator being ignorable. Using the Roy model as a benchmark, a mediator is unlikely to be ignorable in natural experiment settings, and the bias terms likely crowd out inference regarding CM effects.

This paper has contributed to the growing CM literature in economics, integrating labour economic theory for selection-into-treatment as a way of judging the credibility of conventional CM analyses. It has drawn on the classic literature, and pointed to already-in-use selection models/control function methods as a compelling way of estimating direct and indirect effects in a natural experiment setting. Further research could build on this approach by suggesting efficiency improvements, adjustments for common statistical irregularities (say, cluster dependence), or integrating the selection model/control function as an additional robustness in the growing double robustness literature (Farbmacher, Huber, Laffers, Langen & Spindler 2022, Bia, Huber & Laffers 2024).

This paper does not provide a blanket endorsement for applied researchers to use CM

<sup>11</sup>Indeed, this setting has error terms following a bivariate normal distribution, so the canonical Heckman (1974) selection model would produce the most efficient estimates by maximum likelihood. The CF approach avoids this assumption, and bias from breaking it, by relying on an instrument.

<sup>12</sup>The code behind this simulation estimates the first-stage with an interacted OLS specification. The second-stage is an OLS specification, including the CFs. The in-progress semi-parametric approach uses a spline specification for  $\lambda_1$  in the  $D_i = 1$  sample, and  $\lambda_0$  in the  $D_i = 0$  sample, composing a moment restricted estimate of  $\lambda_1$  from these.

<sup>13</sup>In the appendix, Figure A1 shows the same simulation while varying  $\sigma_1$ , with fixed  $\sigma_0 = 1, \rho = 0.5$ . The conclusion is the same as for varying the correlation coefficient,  $\rho$ , in Figure 3.

methods. The structural assumptions are strong, and design-based inference requires an instrument for mediator take-up; if the assumptions are broken, then selection-adjusted estimates of CM effects will also be biased, and will not improve on the selection-on-observables approach. And yet, there are likely settings in which the structural assumptions are credible. Mediator monotonicity aligns well with economic theory in many cases, and it is plausible for researchers to study big data settings with external variation in mediator take-up costs. In these cases, this paper opens the door to identifying mechanisms behind treatment effects in natural experiment settings.

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## A Appendix

Any comments or suggestions may be sent to me at [seh325@cornell.edu](mailto:seh325@cornell.edu), or raised as an issue on the Github project.

### A.1 Identification in Causal Mediation

Imai et al. (2010, Theorem 1) states that the ADE and AIE are identified under sequential ignorability, at each level of  $Z_i = 0, 1$ . For  $z' = 0, 1$ :

$$\begin{aligned}\mathbb{E}[Y_i(1, D_i(z')) - Y_i(0, D_i(z'))] &= \int \int \left( \mathbb{E}[Y_i | Z_i = 1, D_i, \mathbf{X}_i] - \mathbb{E}[Y_i | Z_i = 0, D_i, \mathbf{X}_i] \right) dF_{D_i | Z_i=z', \mathbf{X}_i} dF_{\mathbf{X}_i}, \\ \mathbb{E}[Y_i(z', D_i(1)) - Y_i(z', D_i(0))] &= \int \int \mathbb{E}[Y_i | Z_i = z', D_i, \mathbf{X}_i] \left( dF_{D_i | Z_i=1, \mathbf{X}_i} - dF_{D_i | Z_i=0, \mathbf{X}_i} \right) dF_{\mathbf{X}_i}.\end{aligned}$$

I focus on the averages, which are identified by consequence of the above.

$$\begin{aligned}\mathbb{E}[Y_i(1, D_i(Z_i)) - Y_i(0, D_i(Z_i))] &= \mathbb{E}_{Z_i} [\mathbb{E}[Y_i(1, D_i(z')) - Y_i(0, D_i(z')) | Z_i = z']] \\ \mathbb{E}[Y_i(Z_i, D_i(1)) - Y_i(Z_i, D_i(0))] &= \mathbb{E}_{Z_i} [\mathbb{E}[Y_i(z', D_i(1)) - Y_i(z', D_i(0)) | Z_i = z']]\end{aligned}$$

My estimand for the ADE is a simple rearrangement of the above. The estimand for the AIE relies on a different sequence, relying on (1) sequential ignorability, (2) conditional monotonicity. These give (1) identification equivalence of AIE local to compliers conditional on  $\mathbf{X}_i$  and AIE conditional on  $\mathbf{X}_i$ , LAIE = AIE, (2) identification of the complier score.

$$\begin{aligned}\mathbb{E}[Y_i(Z_i, D_i(1)) - Y_i(Z_i, D_i(0)) | \mathbf{X}_i] &= \Pr(D_i(1) = 1, D_i(0) = 0 | \mathbf{X}_i) \mathbb{E}[Y_i(Z_i, 1) - Y_i(Z_i, 0) | D_i(1) = 1, D_i(0) = 0, \mathbf{X}_i] \\ &= \Pr(D_i(1) = 1, D_i(0) = 0 | \mathbf{X}_i) \mathbb{E}[Y_i(Z_i, 1) - Y_i(Z_i, 0) | \mathbf{X}_i] \\ &= \Pr(D_i(1) = 1, D_i(0) = 0 | \mathbf{X}_i) \left( \mathbb{E}[Y_i | Z_i, D_i = 1, \mathbf{X}_i] - \mathbb{E}[Y_i | Z_i, D_i = 0, \mathbf{X}_i] \right) \\ &= \left( \mathbb{E}[D_i | Z_i = 1, \mathbf{X}_i] - \mathbb{E}[D_i | Z_i = 0, \mathbf{X}_i] \right) \left( \mathbb{E}[Y_i | Z_i, D_i = 1, \mathbf{X}_i] - \mathbb{E}[Y_i | Z_i, D_i = 0, \mathbf{X}_i] \right)\end{aligned}$$

Monotonicity is not technically required for the above. Breaking monotonicity would not change the identification in any of the above; it would be the same except replacing the complier score with a complier/defier score,  $\Pr(D_i(1) \neq D_i(0) | \mathbf{X}_i) = \mathbb{E}[D_i | Z_i = 1, \mathbf{X}_i] - \mathbb{E}[D_i | Z_i = 0, \mathbf{X}_i]$ .

### A.2 Bias in Mediation Estimates

Suppose that  $Z_i$  is ignorable conditional on  $\mathbf{X}_i$ , but  $D_i$  is not.

### A.2.1 Bias in Direct Effect Estimates

To show that the conventional approach to mediation gives an estimate for the ADE with selection and group difference-bias, start with the components of the conventional estimands. This proof starts with the relevant expectations, conditional on a specific value of  $\mathbf{X}_i$ . For each  $d' = 0, 1$ .

$$\begin{aligned}\mathbb{E}[Y_i | Z_i = 1, D_i = d', \mathbf{X}_i] &= \mathbb{E}[Y_i(1, D_i(Z_i)) | D_i(1) = d', \mathbf{X}_i], \\ \mathbb{E}[Y_i | Z_i = 0, D_i = d', \mathbf{X}_i] &= \mathbb{E}[Y_i(0, D_i(Z_i)) | D_i(0) = d', \mathbf{X}_i]\end{aligned}$$

And so,

$$\begin{aligned}& \mathbb{E}[Y_i | Z_i = 1, D_i = d', \mathbf{X}_i] - \mathbb{E}[Y_i | Z_i = 0, D_i = d', \mathbf{X}_i] \\ &= \mathbb{E}[Y_i(1, D_i(Z_i)) | D_i(1) = d', \mathbf{X}_i] - \mathbb{E}[Y_i(0, D_i(Z_i)) | D_i(0) = d', \mathbf{X}_i] \\ &= \mathbb{E}[Y_i(1, D_i(Z_i)) - Y_i(0, D_i(Z_i)) | D_i(1) = d', \mathbf{X}_i] \\ & \quad + \mathbb{E}[Y_i(0, D_i(Z_i)) | D_i(1) = d', \mathbf{X}_i] - \mathbb{E}[Y_i(0, D_i(Z_i)) | D_i(0) = d', \mathbf{X}_i].\end{aligned}$$

The final term is a sum of the ADE, conditional on  $D_i(1) = d'$ , and a selection bias term — difference in baseline outcomes between the (partially overlapping) groups for whom  $D_i(1) = d'$  and  $D_i(0) = d'$ .

To reach the final term, note the following.

$$\begin{aligned}& \mathbb{E}[Y_i(1, D_i(Z_i)) - Y_i(0, D_i(Z_i)) | \mathbf{X}_i] \\ &= \mathbb{E}[Y_i(1, D_i(Z_i)) - Y_i(0, D_i(Z_i)) | D_i(1) = d', \mathbf{X}_i] \\ & \quad + \left(1 - \Pr(D_i(1) = d' | \mathbf{X}_i)\right) \left( \mathbb{E}[Y_i(1, D_i(Z_i)) - Y_i(0, D_i(Z_i)) | D_i(1) = d', \mathbf{X}_i] \right. \\ & \quad \left. - \mathbb{E}[Y_i(1, D_i(Z_i)) - Y_i(0, D_i(Z_i)) | D_i(1) = 1 - d', \mathbf{X}_i] \right)\end{aligned}$$

The second term is the difference between the ADE and LADE local to relevant complier groups.

Collect everything together, as follows.

$$\begin{aligned}
& \mathbb{E}[Y_i | Z_i = 1, D_i = d', \mathbf{X}_i] - \mathbb{E}[Y_i | Z_i = 0, D_i = d', \mathbf{X}_i] \\
&= \underbrace{\mathbb{E}[Y_i(1, D_i(Z_i)) - Y_i(0, D_i(Z_i)) | \mathbf{X}_i]}_{\text{ADE, conditional on } \mathbf{X}_i} \\
&+ \underbrace{\mathbb{E}[Y_i(0, D_i(Z_i)) | D_i(1) = d', \mathbf{X}_i] - \mathbb{E}[Y_i(0, D_i(Z_i)) | D_i(0) = d', \mathbf{X}_i]}_{\text{Selection bias}} \\
&+ \underbrace{\left(1 - \Pr(D_i(1) = d' | \mathbf{X}_i)\right) \left( \mathbb{E}[Y_i(1, D_i(Z_i)) - Y_i(0, D_i(Z_i)) | D_i(1) = 1 - d', \mathbf{X}_i] \right.}_{\text{group difference-bias}} \\
&\quad \left. - \mathbb{E}[Y_i(1, D_i(Z_i)) - Y_i(0, D_i(Z_i)) | D_i(1) = d', \mathbf{X}_i] \right)
\end{aligned}$$

The proof is achieved by applying the expectation across  $D_i = d'$ , and  $\mathbf{X}_i$ .

### A.2.2 Bias in Indirect Effect Estimates

To show that the conventional approach to mediation gives an estimate for the AIE with selection and group difference-bias, start with the definition of the ADE — the direct effect among compliers times the size of the complier group.

This proof starts with the relevant expectations, conditional on a specific value of  $\mathbf{X}_i$ .

$$\begin{aligned}
& \mathbb{E}[Y_i(Z_i, D_i(1)) - Y_i(Z_i, D_i(0)) | \mathbf{X}_i] \\
&= \Pr(D_i(1) = 1, D_i(0) = 0 | \mathbf{X}_i) \mathbb{E}[Y_i(Z_i, 1) - Y_i(Z_i, 0) | D_i(1) = 1, D_i(0) = 0, \mathbf{X}_i]
\end{aligned}$$

When  $D_i$  is not ignorable, the bias comes from estimating the second term,

$$\mathbb{E}[Y_i(Z_i, 1) - Y_i(Z_i, 0) | D_i(1) = 1, D_i(0) = 0, \mathbf{X}_i].$$

For each  $z' = 0, 1$ .

$$\begin{aligned}
\mathbb{E}[Y_i | Z_i = z', D_i = 1, \mathbf{X}_i] &= \mathbb{E}[Y_i(z', 1) | D_i = 1, \mathbf{X}_i], \\
\mathbb{E}[Y_i | Z_i = z', D_i = 0, \mathbf{X}_i] &= \mathbb{E}[Y_i(z', 0) | D_i = 0, \mathbf{X}_i]
\end{aligned}$$

So compose the CM estimand, as follows.

$$\begin{aligned}
& \mathbb{E}[Y_i | Z_i = z', D_i = 1, \mathbf{X}_i] - \mathbb{E}[Y_i | Z_i = z', D_i = 0, \mathbf{X}_i] \\
&= \mathbb{E}[Y_i(z', 1) | D_i = 1, \mathbf{X}_i] - \mathbb{E}[Y_i(z', 0) | D_i = 0, \mathbf{X}_i] \\
&= \mathbb{E}[Y_i(z', 1) - Y_i(z', 0) | D_i = 1, \mathbf{X}_i] + \mathbb{E}[Y_i(z', 0) | D_i = 1, \mathbf{X}_i] - \mathbb{E}[Y_i(z', 0) | D_i = 0, \mathbf{X}_i]
\end{aligned}$$

The final term is a sum of the AIE, among the treated group  $D_i = 1$ , and a selection bias term — difference in baseline terms between the groups  $D_i = 1$  and  $D_i = 0$ .

The AIE is the direct effect among compliers times the size of the complier group, so we need to compensate for the difference between the treated group  $D_i = 1$  and complier group  $D_i(1) = 1, D_i(0) = 0$ .

Start with the difference between treated group's average and overall average.

$$\begin{aligned} & \mathbb{E} [Y_i(z', 1) - Y_i(z', 0) \mid D_i = 1, \mathbf{X}_i] \\ &= \mathbb{E} [Y_i(z', 1) - Y_i(z', 0) \mid \mathbf{X}_i] \\ &+ \left(1 - \Pr(D_i = 1 \mid \mathbf{X}_i)\right) \left( \begin{aligned} & \mathbb{E} [Y_i(z', 1) - Y_i(z', 0) \mid D_i = 1, \mathbf{X}_i] \\ & - \mathbb{E} [Y_i(z', 1) - Y_i(z', 0) \mid D_i = 0, \mathbf{X}_i] \end{aligned} \right) \end{aligned}$$

Then the difference between the compliers' average and the overall average.

$$\begin{aligned} & \mathbb{E} [Y_i(z', 1) - Y_i(z', 0) \mid D_i(1) = 1, D_i(0) = 0, \mathbf{X}_i] \\ &= \mathbb{E} [Y_i(z', 1) - Y_i(z', 0) \mid \mathbf{X}_i] \\ &+ \frac{1 - \Pr(D_i(1) = 1, D_i(0) = 0 \mid \mathbf{X}_i)}{\Pr(D_i(1) = 1, D_i(0) = 0 \mid \mathbf{X}_i)} \left( \begin{aligned} & \mathbb{E} [Y_i(z', 1) - Y_i(z', 0) \mid D_i(1) = 0 \text{ or } D_i(0) = 1, \mathbf{X}_i] \\ & - \mathbb{E} [Y_i(z', 1) - Y_i(z', 0) \mid \mathbf{X}_i] \end{aligned} \right) \end{aligned}$$

Collect everything together, as follows.

$$\begin{aligned} & \mathbb{E} [Y_i \mid Z_i = z', D_i = 1, \mathbf{X}_i] - \mathbb{E} [Y_i \mid Z_i = z', D_i = 0, \mathbf{X}_i] \\ &= \underbrace{\mathbb{E} [Y_i(z', 1) - Y_i(z', 0) \mid D_i(1) = 1, D_i(0) = 0, \mathbf{X}_i]}_{\text{AIE among compliers, conditional on } \mathbf{X}_i, Z_i = z'} \\ &+ \underbrace{\mathbb{E} [Y_i(z', 0) \mid D_i = 1, \mathbf{X}_i] - \mathbb{E} [Y_i(z', 0) \mid D_i = 0, \mathbf{X}_i]}_{\text{Selection bias}} \\ &+ \underbrace{\left[ \begin{aligned} & \left(1 - \Pr(D_i = 1 \mid \mathbf{X}_i)\right) \left( \begin{aligned} & \mathbb{E} [Y_i(z', 1) - Y_i(z', 0) \mid D_i = 1, \mathbf{X}_i] \\ & - \mathbb{E} [Y_i(z', 1) - Y_i(z', 0) \mid D_i = 0, \mathbf{X}_i] \end{aligned} \right) \\ & - \frac{1 - \Pr(D_i(1) = 1, D_i(0) = 0 \mid \mathbf{X}_i)}{\Pr(D_i(1) = 1, D_i(0) = 0 \mid \mathbf{X}_i)} \left( \begin{aligned} & \mathbb{E} [Y_i(z', 1) - Y_i(z', 0) \mid D_i(1) = 0 \text{ or } D_i(0) = 1, \mathbf{X}_i] \\ & - \mathbb{E} [Y_i(z', 1) - Y_i(z', 0) \mid \mathbf{X}_i] \end{aligned} \right) \end{aligned} \right]}_{\text{group difference-bias}} \end{aligned}$$

The proof is finally achieved by multiplying by the complier score,  $\Pr(D_i(1) = 1, D_i(0) = 0 \mid \mathbf{X}_i)$   $= \mathbb{E}[D_i \mid Z_i = 1, \mathbf{X}_i] - \mathbb{E}[D_i \mid Z_i = 0, \mathbf{X}_i]$ , then applying the expectation across  $Z_i = z'$ , and  $\mathbf{X}_i$ .

### A.3 A Regression Framework for Direct and Indirect Effects

Put  $\mu_{d'}(z'; \mathbf{X}) = \mathbb{E}[Y_i(z', d') | \mathbf{X}]$  and  $U_{d',i} = Y_i(z', d') - \mu_{d'}(z'; \mathbf{X})$  for each  $z', d' = 0, 1$ , so we have the following expressions:

$$Y_i(Z_i, 0) = \mu_0(Z_i; \mathbf{X}_i) + U_{0,i}, \quad Y_i(Z_i, 1) = \mu_1(Z_i; \mathbf{X}_i) + U_{1,i}.$$

$U_{0,i}, U_{1,i}$  are error terms with unknown distributions, mean independent of  $Z_i, \mathbf{X}_i$  by definition — but possibly correlated with  $D_i$ .  $Z_i$  is conditionally independent of potential outcomes, so that  $U_{0,i}, U_{1,i} \perp\!\!\!\perp Z_i$ .

The first-stage regression of  $Z \rightarrow Y$  has unbiased estimates, since  $Z_i \perp\!\!\!\perp D_i(\cdot) | \mathbf{X}_i$ . Put  $\pi(z'; \mathbf{X}) = \mathbb{E}[D_i(z') | \mathbf{X}]$ , and  $\eta_{z',i} = D_i(z') - \pi(z'; \mathbf{X})$  the first-stage error terms.

$$\begin{aligned} D_i &= Z_i D_i(1) + (1 - Z_i) D_i(0) \\ &= D_i(0) + Z_i [D_i(1) - D_i(0)] \\ &= \underbrace{\pi(0; \mathbf{X}_i)}_{\text{Intercept, } := \phi + \zeta(\mathbf{X}_i)} + \underbrace{Z_i \mathbb{E}[\pi(1; \mathbf{X}_i) - \pi(0; \mathbf{X}_i)]}_{\text{Regressor, } := \bar{\pi} Z_i} + \underbrace{(1 - Z_i) \eta_{0,i} + Z_i \eta_{1,i}}_{\text{Errors, } := \eta_i} \\ \implies \mathbb{E}[D_i | Z_i, \mathbf{X}_i] &= \phi + \bar{\pi} Z_i + \zeta(\mathbf{X}_i). \end{aligned}$$

Since the ignorability assumption gives  $\mathbb{E}[Z_i \eta_{z',i}] = \mathbb{E}[Z_i] \mathbb{E}[\eta_{z',i}] = 0$ , for each  $z' = 0, 1$ .

By the same argument  $Z_i$  is also assumed independent of potential outcomes  $Y_i(\cdot, \cdot)$ , so that  $U_{0,i}, U_{1,i} \perp\!\!\!\perp Z_i$ . Thus, the reduced form regression  $Z \rightarrow Y$  also leads to unbiased estimates for the ATE.

The same cannot be said of the regression that estimates direct and indirect effects, without further assumptions.

$$\begin{aligned} Y_i &= Z_i Y_i(1, D_i(1)) + (1 - Z_i) Y_i(0, D_i(0)) \\ &= Z_i D_i Y_i(1, 1) \\ &\quad + (1 - Z_i) D_i Y_i(0, 1) \\ &\quad + Z_i (1 - D_i) Y_i(1, 0) \\ &\quad + (1 - Z_i) (1 - D_i) Y_i(0, 0) \\ &= Y_i(0, 0) \\ &\quad + Z_i [Y_i(1, 0) - Y_i(0, 0)] \\ &\quad + D_i [Y_i(0, 1) - Y_i(0, 0)] \\ &\quad + Z_i D_i [Y_i(1, 1) - Y_i(1, 0) - (Y_i(0, 1) - Y_i(0, 0))] \end{aligned}$$

And so  $Y_i$  can be written as a regression equation in terms of the observed factors and error

terms.

$$\begin{aligned}
Y_i &= \mu_0(0; \mathbf{X}_i) \\
&\quad + D_i [\mu_1(0; \mathbf{X}_i) - \mu_0(0; \mathbf{X}_i)] \\
&\quad + Z_i [\mu_0(1; \mathbf{X}_i) - \mu_0(0; \mathbf{X}_i)] \\
&\quad + Z_i D_i [\mu_1(1; \mathbf{X}_i) - \mu_0(1; \mathbf{X}_i) - (\mu_1(0; \mathbf{X}_i) - \mu_0(0; \mathbf{X}_i))] \\
&\quad + U_{0,i} + D_i (U_{1,i} - U_{0,i}) \\
&= \alpha + \beta D_i + \gamma Z_i + \delta Z_i D_i + \varphi(\mathbf{X}_i) + (1 - D_i) U_{0,i} + D_i U_{1,i}
\end{aligned}$$

With the following definitions:

- (a)  $\alpha = \mathbb{E} [\mu_0(0; \mathbf{X}_i)]$  and  $\varphi(\mathbf{X}_i) = \mu_0(0; \mathbf{X}_i) - \alpha$  are the intercept terms.
- (b)  $\beta = \mu_1(0; \mathbf{X}_i) - \mu_0(0; \mathbf{X}_i)$  is the indirect effect under  $Z_i = 0$
- (c)  $\gamma = \mu_0(1; \mathbf{X}_i) - \mu_0(0; \mathbf{X}_i)$  is the direct effect under  $D_i = 0$ .
- (d)  $\delta = \mu_1(1; \mathbf{X}_i) - \mu_0(1; \mathbf{X}_i) - (\mu_1(0; \mathbf{X}_i) - \mu_0(0; \mathbf{X}_i))$  is the interaction effect.
- (e)  $(1 - D_i) U_{0,i} + D_i U_{1,i}$  is the remaining error term.

This sequence gives us the resulting regression equation:

$$\begin{aligned}
\mathbb{E} [Y_i | Z_i, D_i, \mathbf{X}_i] &= \alpha + \beta D_i + \gamma Z_i + \delta Z_i D_i + \varphi(\mathbf{X}_i) \\
&\quad + (1 - D_i) \mathbb{E} [U_{0,i} | D_i = 0, \mathbf{X}_i] + D_i \mathbb{E} [U_{1,i} | D_i = 1, \mathbf{X}_i]
\end{aligned}$$

Taking the conditional expectation, and collecting for the expressions of the direct and indirect effects:

$$\begin{aligned}
\mathbb{E} [Y_i(Z_i, D_i(1)) - Y_i(Z_i, D_i(0))] &= \mathbb{E} [\pi (\beta + Z_i \delta)] \\
\mathbb{E} [Y_i(1, D_i(Z_i)) - Y_i(0, D_i(Z_i))] &= \mathbb{E} [\gamma + \delta D_i + \tilde{U}_i]
\end{aligned}$$

These equations have simpler expressions after assuming constant treatment effects in a linear framework; I have avoided this as having compliers, and controlling for observed factors  $\mathbf{X}_i$  only makes sense in the case of heterogeneous treatment effects.

These terms are conventionally estimated in a simultaneous regression (Imai et al. 2010). If sequential ignorability does not hold, then the regression estimates from estimating the mediation equations (without adjusting for the contaminated bias term) suffer from omitted variables bias.

$$\begin{aligned}
& \mathbb{E}_{\mathbf{X}_i} [\mathbb{E} [Y_i | Z_i = D_i = 0, \mathbf{X}_i]] = \mathbb{E} [\alpha] + \mathbb{E} [U_{0,i} | D_i = 0] \\
& \mathbb{E}_{\mathbf{X}_i} [\mathbb{E} [Y_i | Z_i = 0, D_i = 1, \mathbf{X}_i] - \mathbb{E} [Y_i | Z_i = 0, D_i = 0, \mathbf{X}_i]] = \mathbb{E} [\beta] + (\mathbb{E} [U_{1,i} | D_i = 1] - \mathbb{E} [U_{0,i} | D_i = 0]) \\
& \mathbb{E}_{\mathbf{X}_i} [\mathbb{E} [Y_i | Z_i = 1, D_i = 0, \mathbf{X}_i] - \mathbb{E} [Y_i | Z_i = 0, D_i = 0, \mathbf{X}_i]] = \mathbb{E} [\gamma] + \mathbb{E} [U_{0,i} | D_i = 0] \\
& \mathbb{E}_{\mathbf{X}_i} \left[ \mathbb{E} [Y_i | Z_i = 1, D_i = 1, \mathbf{X}_i] - \mathbb{E} [Y_i | Z_i = 1, D_i = 0, \mathbf{X}_i] \right. \\
& \quad \left. - (\mathbb{E} [Y_i | Z_i = 0, D_i = 1, \mathbf{X}_i] - \mathbb{E} [Y_i | Z_i = 0, D_i = 0, \mathbf{X}_i]) \right] = \mathbb{E} [\delta]
\end{aligned}$$

And so the ADE and AIE estimates are contaminated by these bias terms. Additionally, the AIE estimates refers to gains from the mediator among  $D(z)$  compliers (not the entire average), so will be biased when not accounting for  $\tilde{U}_i$ , too.

#### A.4 Roy Model and Sequential Ignorability

Suppose  $Z_i$  is ignorable, and selection into  $D_i$  follows a Roy model, with the definitions in [Section 2](#). If selection into  $D_i$  is degenerate on  $U_{0,i}, U_{1,i}$ :

$$\mathbb{E} [D_i | Z_i, \mathbf{X}_i, U_{1,i} - U_{0,i} = u] = \mathbb{E} [D_i | Z_i, \mathbf{X}_i, U_{1,i} - U_{0,i} = u'], \text{ for all } u, u' \text{ in the range of } U_{1,i} - U_{0,i}.$$

In this case, the control set  $\mathbf{X}_i$  and the costs  $\mu_c, U_{c,i}$  are the only determinants of selection into  $D_i$  — and,  $U_{0,i}, U_{1,i}$  play no role. This could be achieved by either assuming that unobserved gains are degenerate (the researcher had observed everything in  $\mathbf{X}_i$ ), or selection into  $D_i$  had been disrupted in some fashion (e.g., by a natural experiment design for  $D_i$ ).

To motivate a contraposition argument, suppose  $D_i$  is ignorable conditional on  $Z_i, \mathbf{X}_i$ . For each  $z', d' = 0, 1$

$$\begin{aligned}
& D_i \perp\!\!\!\perp Y_i(z', d') \mid \mathbf{X}_i, Z_i = z' \\
& \implies D_i \perp\!\!\!\perp \mu_{d'}(z'; \mathbf{X}_i) + U_{d',i} \mid \mathbf{X}_i, Z_i = z' \\
& \implies D_i \perp\!\!\!\perp U_{d',i} \mid \mathbf{X}_i, Z_i = z' \\
& \implies D_i \perp\!\!\!\perp U_{1,i} - U_{0,i} \mid \mathbf{X}_i, Z_i = z' \\
& \implies \mathbb{E} [D_i | U_{1,i} - U_{0,i} = u', \mathbf{X}_i, Z_i = z'] = \mathbb{E} [D_i | \mathbf{X}_i, Z_i = z'] \\
& \text{for all } u' \text{ in the range of } U_{1,i} - U_{0,i}.
\end{aligned}$$

This final implication is that selection into  $D_i$  is degenerate on  $U_{0,i}, U_{1,i}$ . Thus, a contraposition argument has that if selection into  $D_i$  is non-degenerate on  $U_{0,i}, U_{1,i}$ , then  $D_i$  is not ignorable.

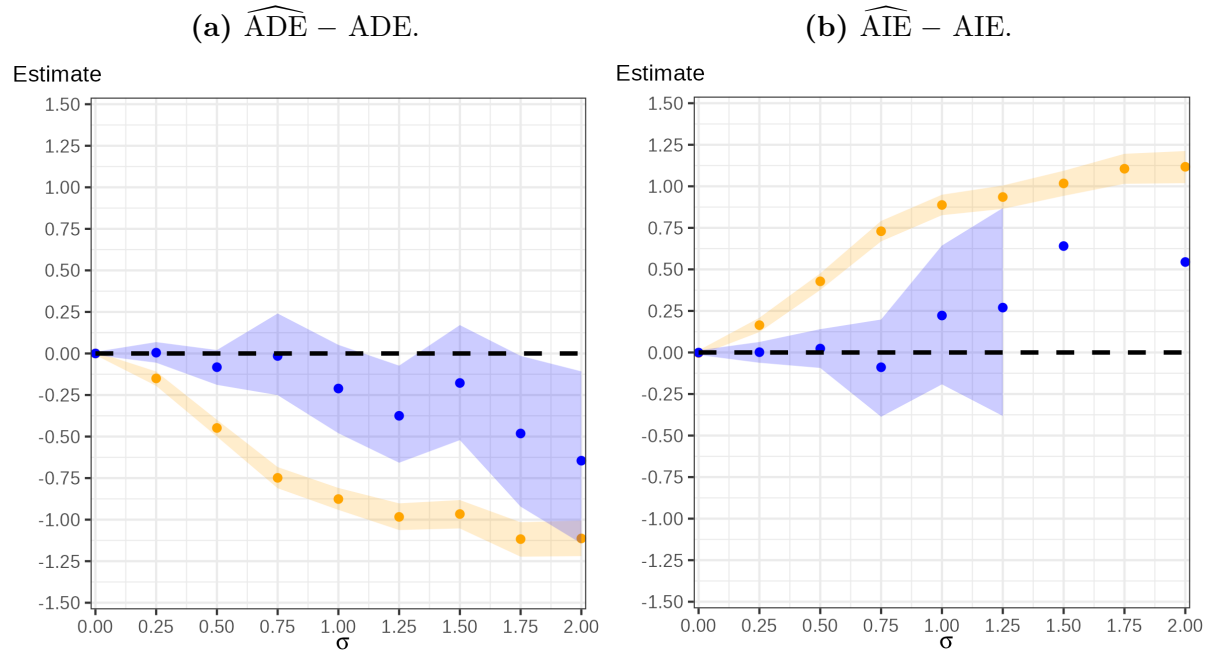


## A.5 Control Function Simulation

A number of statistical packages, for the R language ([R Core Team 2023](#)), made the simulation analysis for this paper possible.

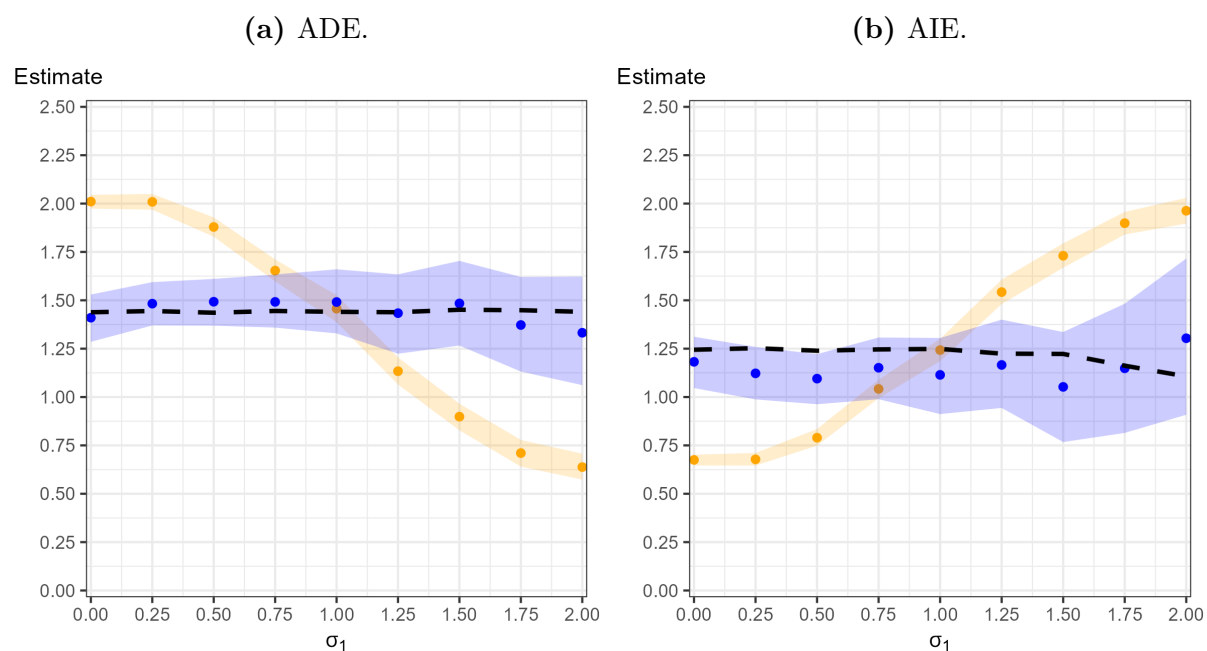
- *Tidyverse* ([Wickham, Averick, Bryan, Chang, McGowan, François, Golemund, Hayes, Henry, Hester, Kuhn, Pedersen, Miller, Bache, Müller, Ooms, Robinson, Seidel, Spinu, Takahashi, Vaughan, Wilke, Woo & Yutani 2019](#)) collected tools for data analysis in the R language.
- *Splines* ([Wang & Yan 2021](#)) allows semi-parametric estimation, using splines, in the R language.
- *Mediate* ([Tingley, Yamamoto, Hirose, Keele & Imai 2014](#)) automates the sequential-ignorability estimates of CM effects ([Imai et al. 2010](#)) in the R language.

**Figure A1:** Point Estimates of CM Effects, OLS and Control Function versus True Value.



**Note:** These figures show the OLS and control function point estimates of the ADE and AIE, for  $N = 10,000$  sample size, minus the true value of the ADE and AIE, respectively.  $y$ -axis value of zero means the point estimate had estimated the ADE, or AIE, exactly. Points are points estimates from data simulated with a given  $\rho = 0.5$  value, varying the  $\sigma_0 = \sigma, \sigma_1 = 2\sigma$  values. Orange represents OLS estimates, blue the control function approach. Shaded regions are the 95% confidence intervals from 1,000 bootstraps each.

**Figure A2:** OLS versus Control Function Estimates of CM Effects, varying  $\sigma_1$  relative to  $\sigma_0 = 1$ .



**Note:** These figures show the OLS and control function estimates of the ADE and AIE, for  $N = 10,000$  sample size. The black dashed line is the true value, points are points estimates from data simulated with a given  $\rho = 0.5, \sigma_0 = 1$  and  $\sigma_1$  varied across  $[0, 2]$ . Shaded regions are the 95% confidence intervals; orange are the OLS estimates, blue the control function approach.