Causal Mediation in Natural Experiments

Senan Hogan-Hennessy*
Economics Department, Cornell University†

First draft: 12 February 2025
This version: 25 February 2025

Work in Progress, newest version available here.

Abstract

Natural experiments are a cornerstone of applied economics, providing settings for estimating causal effects with a compelling argument for treatment ignorability. Causal Mediation (CM) methods aim to estimate how much of the treatment effect operates through a proposed mediator, illuminating mechanisms that causal effects operate through. The most popular approach to CM relies on assuming the mediator is conditionally ignorable — in addition to the causal research design for the initial treatment. This assumption conveniently ignores individuals' choice to take or refuse the mediator, by assuming they did so naïvely or the researcher observed everything that could have affected this decision. This paper shows that the conventional approach to CM can lead to biased inference, solving for explicit bias terms when the mediator is not ignorable. Individuals' selection based on expected gains and costs is inconsistent with mediator ignorability in a natural experiment setting, suggesting bias would be present in practice. I consider a control function approach, which overcomes these hurdles under alternative assumptions, using cost of mediator take-up as an instrument. Simulations confirm that this method corrects for persistent bias in conventional CM estimates, though require large sample sizes for correct inference. This approach gives applied researchers an alternative method to estimate CM effects when they can only establish a credible argument for randomisation of 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.

---

*For helpful comments I thank Neil Cholli, Hyewon Kim, Jiwoo Kim, Lukáš Lafférs, Jiwon Lee, Yiqi Liu, Douglas Miller, Zhuan Pei, Brenda Prallon, and Evan Riehl. Some preliminary results previously circulated in an earlier version of the working paper "The Direct and Indirect Effects of Genetics and Education." I thank seminar participants at Cornell University (2025) for helpful discussion. Any comments or suggestions may be sent to me at seh325@cornell.edu, or raised as an issue on the Github project.

†Address: Uris Hall #447, Economics Department, Cornell University NY 14853 USA.

Economists use natural experiments to credibly answer social questions, when an experiment was infeasible. Did Vietnam-era military service lead to income losses? Does access to health insurance lead to employment gains? Do transfer payment lead to measurable long-run economic gains? Quasi-experimental variation gives methods to answer 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 (causal) gain from a transfer payment came from individuals choosing to attend higher education? This paper shows that the conventional approach to estimating CM effects is inappropriate in a natural experiment setting, giving a theoretical framework for how bias operates, and 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 — a sum of selection bias and non-parametric group differences. I then show how this bias operates in an applied regression framework, with bias coming from a correlated error term. If individuals have been choosing whether to partake in a mediator based on expected costs and benefits (i.e., following a rational maximisation process) then sequential ignorability cannot hold. This means the identifying assumption for conventional CM methods are unlikely to hold, and will lead to biased inference in natural experiment settings, if the researcher does not use another design to isolate random variation in the mediator (at the same time).

I consider an alternative control function approach to estimating CM effects. This approach solves the identification problem by a structural assumption for selection into the mediator (monotonicity), and assumes the researcher has a valid instrument for mediator take-up. While these assumptions are strong, they are plausible in many applied settings. Mediator monotonicity is in-line with conventional theories for selection-into-treatment, and is accepted widely in many applications using an instrumental variables research design. The existence of a valid instrument is a stronger assumption, which will not hold in every setting, though is important to avoid further modelling assumptions. The most compelling example is using data on the cost of mediator take-up as a first-stage instrument, if it varies between individuals for exogenous reasons and is strong in explaining compliance. Using an instrument avoids parametric assumptions on unexplained mediator selection, though limits the wider applicability of the method. This approach is not perfect: it is computationally demanding, and requires large sample sizes for semi-parametric estimation steps. Additionally, it provides no harbour for estimating CM effects if the core 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 epidemiology, medicine, and psychology to estimate CM effects in observational studies. Assuming mediator ignorability (also known as selection-on-observables) conveniently ignores individuals' choice to take or refuse the mediator, by assuming they did so naïvely or the researcher observed everything that could have affected this decision. If a researcher is studying single-cell organisms in a laboratory, then it may make sense to study causal mechanisms with this approach; single-cell organisms make simple decisions to take or refuse a treatment or mediator. On the other hand, social science researchers study social settings where humans have make complex decisions based on costs, benefits, and preferences — all of which are not fully known by the researcher. Assuming a mediator is ignorable in such a setting would be naïve at best.

The applied economics literature has not picked up the practice of estimating CM effects by selection-on-observables, partially in an understanding that this assumption would be invalid in most observational settings. Indeed, a new strand of the econometric literature has developed estimators for CM effects under overlapping quasi-experimental research designs (Deuchert, Huber & Schelker 2019, Frölich & Huber 2017), a partial identification approach (Flores & Flores-Lagunes 2009), or testing 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 makes this part explicit, showing exactly how a conventional approach to CM in a natural experiment can fail in practice, and warding the applied economics literature away from picking up this practice.

This paper considers the case when it is not credible to assume the mediator is ignorable (e.g., none of the research designs above apply), leveraging classic labour economic theory for selection-into-treatment to identify direct and indirect effects. A selection-on-observables approach to CM in this setting suffers from bias of the same flavour as classic selection bias (Heckman, Ichimura, Smith & Todd 1998), plus additional bias from group differences. The group differences-bias is a non-parametric version of bad controls bias, which has only previously been studied in a linear setting (Cinelli, Forney & Pearl 2024, Ding & Miratrix 2015).

Throughout, I use the Roy (1951) model as a benchmark for judging the Imai, Keele & Yamamoto (2010) mediator ignorability assumption in a natural experiment setting, and find it unlikely to hold in practice.[1] 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 approach to correct for the bias developed above. This approach assumes mediator monotonicity, to ensure the mediator follows a selection model (Vytlacil 2002), requiring a valid instrument for mediator take-up, to avoid parametric assumptions on unobserved selection (Heckman & Navarro-Lozano 2004, Florens, Heckman, Meghir & Vytlacil 2008). Doing so is as an extension of using instruments to identify CM effects — as noted by Frölich & Huber (2017).[2] Using a control function to estimate CM effects builds on the influential

[1] An alternative method to estimate CM effects is ensuring sequential ignorability holds by a running randomised controlled trial for both treatment and mediator at the same time. This setting has been considered in the literature previously, in theory (Imai, Tingley & Yamamoto 2013) and in practice (Ludwig, Kling & Mullainathan 2011).

[2] Indeed, this paper does not improve on control function methods in any way, instead noting its applicability in this setting. See Frölich & Huber (2017) for the newest development of control function methods with

Imai, Keele & Yamamoto (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 CM, and develops expressions for the 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 achieves identification by a control function approach, in the case that a mediator is monotone in the original treatment and a researcher observes exogenous variation in cost of mediator take-up, giving 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 $i = 1, \dots, n$.[3] The outcomes are a sum of their potential outcomes.[4]

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

Assume Z_i is ignorable.[5]

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

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

instruments, and Imbens (2007) for a general overview of the approach.

[3]Other literatures use different notation. For example, Imai, Keele & Yamamoto (2010) write T_i, M_i, Y_i for the randomised treatment, mediator, and outcome, respectively. I use the Z_i, D_i, Y_i instrumental variables notation, more familiar in empirical economics (Angrist, Imbens & Rubin 1996).

[4]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.

[5]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.

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

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

   It common in the economics literature to assume that Z influences D in at most one direction, $\Pr\left(D_i(1) \geq D_i(0)\right) = 1$ — monotonicity (Imbens & Angrist 1994). I assume monotonicity (and its conditional variant) holds through-out to simplify notation.[6]

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

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

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

$$\text{Average Direct Effect (ADE), } Z \to Y: \quad \mathbb{E}\left[Y_i(1, D_i(Z_i)) - Y_i(0, D_i(Z_i))\right],$$

$$\text{Average Indirect Effect (AIE), } D(Z) \to Y: \quad \mathbb{E}\left[Y_i(Z_i, D_i(1)) - Y_i(Z_i, D_i(0))\right].$$

Estimating the AIE answers the following question: how much of the causal effect $Z \to Y$ goes through the D channel? If a researcher is studying the income effect of a man being randomly drafted into the US military, and is interested in military service as the mediator, the AIE represents how much of the effect comes from military service (Angrist 1990). Estimating the ADE answers the following equation: how much is left over after accounting for the D channel?[7] For the military draft example, how much of the effect of random conscription is a direct effect, other than military service — e.g., from an education/child deferment to dodge

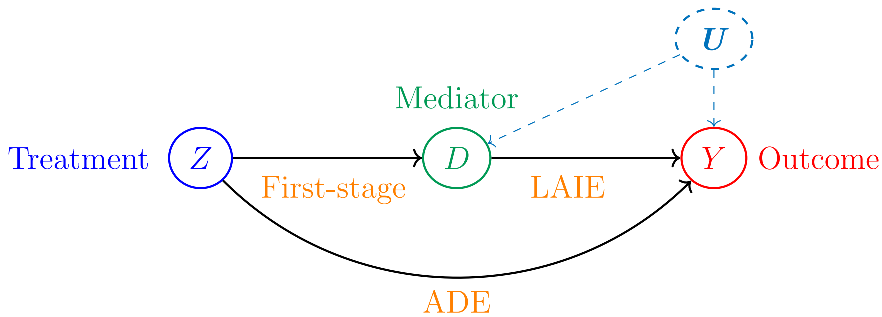
[6] Assuming monotonicity also brings closer to the IV notation, and has other beneficial implications in this setting (see Section 3).

[7] In a non-parametric setting it is not necessary that the ADE and AIE sum to the ATE. See Imai, Keele & Yamamoto (2010) for this point in full.

the draft? An instrumental variables approach is a setting where this direct effect is assumed to be zero for everyone — that the draft had no other effects (i.e., the exclusion restriction). CM is a different framework attempting to explicitly model the direct effect, not assuming the ADE is zero.

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

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 $D \to Y$) local to $D(Z)$ compliers, so that AIE = average first-stage $\times$ LAIE. Unobserved confounder $\boldsymbol{U}$ represents this paper's focus on the case that D is not ignorable, by showing an unobserved confounder. Subsection 2.1 formally defines $\boldsymbol{U}$ in an applied setting.

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 set of control variables $\boldsymbol{X}_i$.

Definition 1. *Sequential Ignorability (Imai, Keele & Yamamoto 2010).*

$$Z_i \perp\!\!\!\perp D_i(z), Y_i(z', d) \;\mid\; \boldsymbol{X}_i, \qquad \text{for } z, z', d = 0, 1 \tag{1}$$

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

Sequential ignorability assumes that the initial treatment Z_i is ignorable conditional on $\boldsymbol{X}_i$. It then also assumes that, after Z_i is assigned, that D_i is ignorable conditional on $\boldsymbol{X}_i, Z_i$. If sequential ignorability, 1(1) and 1(2), holds then the ADE and AIE are identified by two-stage mean differences, after conditioning on $\boldsymbol{X}_i$.[8]

$$\mathbb{E}_{D_i=d',\boldsymbol{X}_i}\left[\underbrace{\mathbb{E}\left[Y_i \mid Z_i=1, D_i=d', \boldsymbol{X}_i\right]-\mathbb{E}\left[Y_i \mid Z_i=0, D_i=d', \boldsymbol{X}_i\right]}_{\text{Second-stage regression, } Y_i \text{ on } Z_i \text{ holding } D_i, \boldsymbol{X}_i \text{ constant}}\right]=\underbrace{\mathbb{E}\left[Y_i(1, D_i(Z_i))-Y_i(0, D_i(Z_i))\right]}_{\text{Average Direct Effect (ADE)}}$$

$$\mathbb{E}_{Z_i=z',\boldsymbol{X}_i}\left[\underbrace{\left(\mathbb{E}\left[D_i \mid Z_i=1, \boldsymbol{X}_i\right]-\mathbb{E}\left[D_i \mid Z_i=0, \boldsymbol{X}_i\right]\right)}_{\text{First-stage regression, } D_i \text{ on } Z_i} \times \underbrace{\left(\mathbb{E}\left[Y_i \mid Z_i=z', D_i=1, \boldsymbol{X}_i\right]-\mathbb{E}\left[Y_i \mid Z_i=z', D_i=0, \boldsymbol{X}_i\right]\right)}_{\text{Second-stage regression, } Y_i \text{ on } D_i \text{ holding } Z_i, \boldsymbol{X}_i \text{ constant}}\right]$$
$$=\underbrace{\mathbb{E}\left[Y_i(Z_i, D_i(1))-Y_i(Z_i, D_i(0))\right]}_{\text{Average Indirect Effect (AIE)}}$$

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, Keele & Yamamoto 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 research may use a natural experiment to study settings where treatment Z_i is ignorable, justifying assumption 1(1). Rarely does research relying on a quasi-experimental research design employ an additional, overlapping identification design for D_i to justify assumption 1(2) as part of the 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

[8] Imai, Keele & Yamamoto (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, Keele & Yamamoto (2010) identification result, and then develops the two-stage regression notation which holds as a consequence of sequential ignorability.

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 $\boldsymbol{X}_i$ notation for brevity. □

For the direct effect: CM estimand = ADE + selection bias + group differences.

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

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

$$
\begin{aligned}
&\mathbb{E}_{Z_i=z'}\left[\Big(\mathbb{E}\left[D_i \mid Z_i=1\right]-\mathbb{E}\left[D_i \mid Z_i=0\right]\Big)\times\Big(\mathbb{E}\left[Y_i \mid Z_i=z', D_i=1\right]-\mathbb{E}\left[Y_i \mid Z_i=z', D_i=0\right]\Big)\right] \\
&= \mathbb{E}\left[Y_i(Z_i, D_i(1))-Y_i(Z_i, D_i(0))\right] \\
&\quad + \Pr\left(D_i(1)=1, D_i(0)=0\right)\Big(\mathbb{E}\left[Y_i(Z_i, 0) \mid D_i=1\right]-\mathbb{E}\left[Y_i(Z_i, 0) \mid D_i=0\right]\Big) \\
&\quad + \Pr\left(D_i(1)=1, D_i(0)=0\right)\times \\
&\quad\left[\begin{array}{l}\Big(1-\Pr\left(D_i=1\right)\Big)\begin{pmatrix}\mathbb{E}\left[Y_i(Z_i, 1)-Y_i(Z_i, 0) \mid D_i=1\right] \\ -\,\mathbb{E}\left[Y_i(Z_i, 1)-Y_i(Z_i, 0) \mid D_i=0\right]\end{pmatrix} \\ -\left(\dfrac{1-\Pr\left(D_i(1)=1, D_i(0)=0\right)}{\Pr\left(D_i(1)=1, D_i(0)=0\right)}\right)\begin{pmatrix}\mathbb{E}\left[Y_i(Z_i, 1)-Y_i(Z_i, 0) \mid D_i(1)=0 \text{ or } D_i(0)=1\right] \\ -\,\mathbb{E}\left[Y_i(Z_i, 1)-Y_i(Z_i, 0)\right]\end{pmatrix}\end{array}\right]
\end{aligned}
$$

The selection bias terms come from systematic differences between groups take or refuse the mediator ($D_i = 0$ versus $D_i = 1$), differences not fully unexplained by $\boldsymbol{X}_i$. These selection bias terms would equal to zero if the mediator was ignorable 1(2), but do not necessarily average to zero if not. The group differences represent the fact that a matching estimator 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).[9] The group differences term is 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 et al. 2024, or M-bias in Ding & Miratrix 2015).[10,11]

2 Causal Mediation (CM) in Applied Settings

In this section, I further develop the issue of selection in CM estimates. First, I show the non-parametric bias terms from above can be written as omitted variables bias in a regression framework. Second, I show how selection bias operates in an applied model for selection into a 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 $Y_i(.,.)$ as a sum of observed and unobserved factors,

[9]The bias terms here mirror those in Heckman et al. (1998), Angrist & Pischke (2009) for a single $D \to Y$ treatment effect, when D_i is not ignorable:

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

[10]The group differences term is longer for the AIE estimate, because the indirect effect is comprised from the effect of D_i local to Z_i compliers; a matching estimator gets the average effect among the $D_i = 1$ group, and the longer term adjusts for differences with the average effect among compliers.

[11]The selection-on-observables approach could, instead, focus on the average effect on treated populations (as do Keele, Tingley & Yamamoto 2015). This runs into a problem of comparisons: CM estimates would give average effects on different treated groups. The CM estimand for the ADE on treated gives the ADE local to the $Z_i = 1$ treated group, and local to the $D_i = 1$ group for the AIE. In this way, these ADE and AIE on treated terms are not comparable to each other, so I focus on the true averages to avoid these misaligned comparisons.

following the notation of Heckman & Vytlacil (2005). For each $z', d' = 0, 1$, put $\mu_{d'}(z'; \boldsymbol{X}) = \mathbb{E}\left[Y_i(z', d') \,|\, \boldsymbol{X}\right]$ and the corresponding error terms, $U_{d',i} = Y_i(z', d') - \mu_{d'}(z'; \boldsymbol{X})$, so we have the following expressions:

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

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

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

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

$$D_i = \phi + \pi Z_i + \varphi(\boldsymbol{X}_i) + \eta_i \tag{3}$$

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

First-stage (3) is identified, with $\phi + \varphi(\boldsymbol{X}_i)$ the intercept, and π the first-stage compliance rate (which may depend on $\boldsymbol{X}_i$). Second-stage (4) is not identified thanks to omitted variables bias. $\alpha + \zeta(\boldsymbol{X}_i)$ is the intercept term, and β, γ, δ are conditional direct and indirect effects — all whose value may depend on $\boldsymbol{X}_i$, see Subsection A.3 for full definitions. $(1 - D_i)\, U_{0,i} + D_i U_{1,i}$ is the possibly correlated error term, which disrupts identification. The ADE and AIE are averages of these coefficients.[12]

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

$$\text{AIE} = \mathbb{E}\left[\pi\left(\beta + \delta Z_i\right)\right].$$

By construction, $\boldsymbol{U}_i \coloneqq (U_{0,i}, U_{1,i})$ is an unobserved confounder. The regression estimates of second-stage (4) give unbiased estimates only if D_i is also conditionally ignorable: $D_i \perp\!\!\!\perp (U_{0,i}, U_{1,i})$.

[12] The AIE, in fact, refers only to treatment gains among $D_i(z)$ compliers, so includes a group differences term, $\pi \times \mathbb{E}\left[D_i U_{1,i} - (1 - D_i)\, U_{0,i} \,|\, \boldsymbol{X}_i, D_i(1) = 1, D_i(0) = 0\right]$. The formula above skips this part, which would equal zero if there are constant treatment effects (for example), to keep with regression notation.

If not, then regression estimates suffer from omitted variables bias from failing to adjust for the unobserved confounder, $\boldsymbol{U}_i$.

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 $Z_i, \boldsymbol{X}_i$ 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 $\mathbb{1}\{.\}$ for the indicator function. The Roy model has i taking the mediator if the benefits exceed the costs,

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

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.[13] 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.

[13] 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).

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

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

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 $\boldsymbol{X}_i$ must be incredibly rich. Together, $\boldsymbol{X}_i$ 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 $\boldsymbol{X}_i$, 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 A Control Function Approach

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 example would yield unbiased estimates. The control function method takes this insight seriously, providing conditions to model the unobserved $(U_{0,i}, U_{1,i})$, and then control for it.[14]

Write K_i for the expected values in predicting the mediator with observed data $Z_i, \boldsymbol{X}_i$.

$$K_i = D_i - \mathbb{E}\left[D_i \,|\, Z_i, \boldsymbol{X}_i\right]$$

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

Definition 3. *Control function assumptions.*

$$\Pr\left(D_i(1) \geq D_i(0) \,|\, \boldsymbol{X}_i\right) = 1 \tag{6}$$

$$D_i \perp\!\!\!\perp Y_i(.,.) \,\Big|\, \boldsymbol{X}_i^{-}, K_i \tag{7}$$

$$\boldsymbol{X}_i^{IV} \text{ satisfies } \frac{\partial}{\partial \boldsymbol{X}_i^{IV}}\left[\mu_1(\boldsymbol{X}_i) - \mu_0(\boldsymbol{X}_i)\right] = 0 < \frac{\partial}{\partial \boldsymbol{X}_i^{IV}} \mathbb{E}\left[D_i(z') \,|\, \boldsymbol{X}_i\right], \text{ for } z' = 0, 1. \tag{8}$$

Assumption 3(6) is the (conditional) monotonicity assumption (Imbens & Angrist 1994), which is untestable but acceptable in many empirical applications. Assumption 3(7) is the control function assumption, assuming that first-stage unobserved heterogeneity explains second-stage selection into D_i. Assumption 3(8) is assuming that 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; strength of the first-stage is testable, and must be justified with data by methods common in the instrumental variables literature.

K_i serves as a control function in this setting.

[14] This section does not improve on the control function approach, instead only noting its utility to solve the identification problem of CM in a natural experiment setting.

Theorem 2. *If 3(6) and 3(8) hold, then the mean potential differences (and thus CM effects) are identified by a control function approach. For each* $z', d' = 0, 1$,

$$\mathbb{E}\left[Y_i \,\middle|\, Z_i = 1, D_i = d', \boldsymbol{X}_i^-, K_i\right] - \mathbb{E}\left[Y_i \,\middle|\, Z_i = 0, D_i = d', \boldsymbol{X}_i^-, K_i\right] = \mathbb{E}\left[Y_i(1, d') - Y_i(0, d') \,\middle|\, \boldsymbol{X}_i^-\right]$$

$$\mathbb{E}\left[Y_i \,\middle|\, Z_i = z', D_i = 1, \boldsymbol{X}_i^-, K_i\right] - \mathbb{E}\left[Y_i \,\middle|\, Z_i = z', D_i = 0, \boldsymbol{X}_i^-, K_i\right] = \mathbb{E}\left[Y_i(z', 1) - Y_i(z', 0) \,\middle|\, \boldsymbol{X}_i^-\right].$$

Proof. Special case of Florens et al. (2008, Theorem 1), Imbens & Newey (2009, Theorem 3). □

Assumption 3(6) guarantees that mediator can be represented by a selection model (Vytlacil 2002), $D_i(.) = \mathbb{1}\left\{\overline{\mu}(.,; \boldsymbol{X}_i) \geq K_i\right\}$ for some function $\overline{\mu}$. Assumption 3(7) connects the first- and second-stage for identification. Assumption 3(8) separately identifies the control function to identify the second-stage model. The approach exploits the fact that the bias terms, coming from correlated the errors in Subsection 2.1, can be estimated in a first-stage regression and included as controls in the second-stage.

If the underlying selection model had been a Roy model, the control function approach captures the unobserved benefits to taking mediator (independent of observed controls), and thus driving take-up of the mediator. In this case, $K_i = U_{C,i} - (U_{1,i} - U_{0,i})$, so the independence result is simple. By incorporating the control function from the first-stage model, the approach adjusts for the unobserved confounding from unobserved gains, $U_{1,i} - U_{0,i}$. By contrast, assuming the mediator was ignorable would have been assuming that there are no unobserved benefits to the mediator take-up, so that there is no bias in the second-stage to account for.

The instrument is key to avoid distributional assumptions on the unobserved errors terms. In the Roy model, the exclusion restriction can be satisfied in one key way: having an instrument for cost of mediator take up μ_C. If the instrument $\boldsymbol{X}_i^{\text{IV}}$ enters the cost function μ_C, and not the benefits function μ, then it satisfies the exclusion restriction. In an applied world, $\boldsymbol{X}_i^{\text{IV}}$ can be data that explain cost differences in taking D_i, unrelated to other demographic information. If a researcher is looking into higher education as a proposed mediator, then data which explains different costs of attending university (unrelated to education gains) can serve this role. This is the logic behind the Card (1993) distance-instrument, and can be extended to a CM setting with education as the mediator.

3.1 Estimation

In practice, the approach relies on estimating the control function K_i, then including this in the second-stage as a control, and accounting for the estimation error for these in the standard errors. These reliances come with major concerns. First, it is imperative that the control function is estimated correctly, so it is necessary to employ a non-parametric approach to estimate the first-stage. Second, the error terms enters the second-stage (4) linearly, but is an unknown function (possibly non-linear) of the control function; thus, the second-stage must be estimated semi-parametrically.[15] Lastly, the standard errors must account for estimation uncertainty in the above two non-parametric steps.

These concerns are worth noting, because non-parametric regression is computationally demanding, and requires large samples for estimator convergence. Furthermore, these are estimated in two steps, so that the concerns are of greater importance. Otherwise, small sample bias properties could even dominate the bias terms identified in Theorem 1.[16] It is beyond the scope of this paper to develop the optimal procedure here, but these concerns are important. For applied research aiming to estimate CM effects, the control function method is only appropriate in extremely large sample sizes, such as applications using administrative sources or biobanks.

With these concerns in mind, I propose the following method to estimate CM effects with a control function approach:

1. Estimate the first-stage, $\mathbb{E}\left[D_i \,\middle|\, Z_i, \boldsymbol{X}_i^{\mathrm{IV}}, \boldsymbol{X}_i^{-}\right]$ with a non-parametric estimator (e.g., a prob-

[15] In practice this can be done by adding a polynomial for the estimated control function into the outcome regression, or a splines approach, etc.

[16] See (Imbens & Newey 2009, Section 6) for a full discussion of the asymptotic theory of a control function estimator.

ability forest, or fully interacted OLS specification).

2. Calculate estimates of the control function:

$$\widehat{K}_i = D_i - \widehat{\mathbb{E}}\left[D_i \middle| Z_i, \boldsymbol{X}_i^{\text{IV}}, \boldsymbol{X}_i^{-}\right].$$

3. Estimate the second-stage with OLS (including an interaction term between Z_i and D_i), and a semi-parametric regressor of the control function.

$$\mathbb{E}\left[Y_i \middle| Z_i, D_i, \boldsymbol{X}_i^{-}, \widehat{K}_{0,i}, \widehat{K}_i\right] = \beta D_i + \gamma Z_i + \delta Z_i D_i + l\left(\widehat{K}_i\right)$$

$l(.)$ is a nuisance function with unknown form, so can be approximated with a semi-parametric spline specification, for example.

4. Calculate the ADE and AIE estimates from the first and second-stages.

$$\widehat{\text{ADE}} = \mathbb{E}\left[\widehat{\mathbb{E}}\left[Y_i \middle| Z_i = 1, D_i, \boldsymbol{X}_i^{-}, \widehat{K}_i\right] - \widehat{\mathbb{E}}\left[Y_i \middle| Z_i = 0, D_i, \boldsymbol{X}_i^{-}, \widehat{K}_i\right]\right]$$

$$\widehat{\text{AIE}} = \mathbb{E}\left[\begin{array}{l}\left(\widehat{\mathbb{E}}\left[D_i \middle| Z_i = 1, \boldsymbol{X}_i^{\text{IV}}, \boldsymbol{X}_i^{-}\right] - \widehat{\mathbb{E}}\left[D_i \middle| Z_i = 1, \boldsymbol{X}_i^{\text{IV}}, \boldsymbol{X}_i^{-}\right]\right) \\ \times\left(\widehat{\mathbb{E}}\left[Y_i \middle| Z_i, D_i = 1, \boldsymbol{X}_i^{-}, \widehat{K}_i\right] - \widehat{\mathbb{E}}\left[Y_i \middle| Z_i, D_i = 0, \boldsymbol{X}_i^{-}, \widehat{K}_i\right]\right)\end{array}\right]$$

5. Bootstrap across the previous steps, to calculate standard errors for the respective ADE and AIE estimates.

3.2 Simulation Evidence

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

$$Z_i \sim \text{Binom}\left(0.5\right), \quad \boldsymbol{X}_i^{-} \sim N(4, 1), \quad \boldsymbol{X}_i^{\text{IV}} \sim \text{Binom}\left(0.5\right),$$

$$\left(U_{0,i}, U_{1,i}\right) \sim \text{BivariateNormal}\left(0, 0, \sigma_0, \sigma_1, \rho\right), \quad U_{C,i} \sim N(0, 0.5).$$

$N = 10,000$ 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 $z', d' = 0, 1$.

$$D_i(z') = \mathbb{1}\left\{Y_i(z', 1) - Y_i(z', 0) \geq C_i\right\},$$
$$\mu_{d'}\left(z'; \boldsymbol{X}_i\right) = \boldsymbol{X}_i^- + \left(z' + d' + z'd'\right), \quad \mu_C\left(z'; \boldsymbol{X}_i\right) = 3z' + \boldsymbol{X}_i^- - \boldsymbol{X}_i^{\mathrm{IV}}.$$

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

$$D_i = \mathbb{1}\left\{-3Z_i - \boldsymbol{X}_i^{\mathrm{IV}} + \boldsymbol{X}_i^- \geq U_{C,i} - \left(U_{1,i} - U_{0,i}\right)\right\},$$
$$Y_i = Z_i + D_i + Z_i D_i + \boldsymbol{X}_i^- + \left(1 - D_i\right) U_{0,i} + D_i U_{1,i}.$$

Z_i has an effect on outcome Y_i, and it operates partially through mediator D_i. Outcome mean $\mu_{D_i}(Z_i; .)$ 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 $\Pr\left(D_i = 1\right) = 0.437$, and 65.29% of the sample are mediator compliers (where $D_i(1) = 1$ and $D_i(0) = 0$). This gives an ATE ($Z \to Y$) value of 2.58, ADE 1.44, and AIE 1.13, respectively.[17]

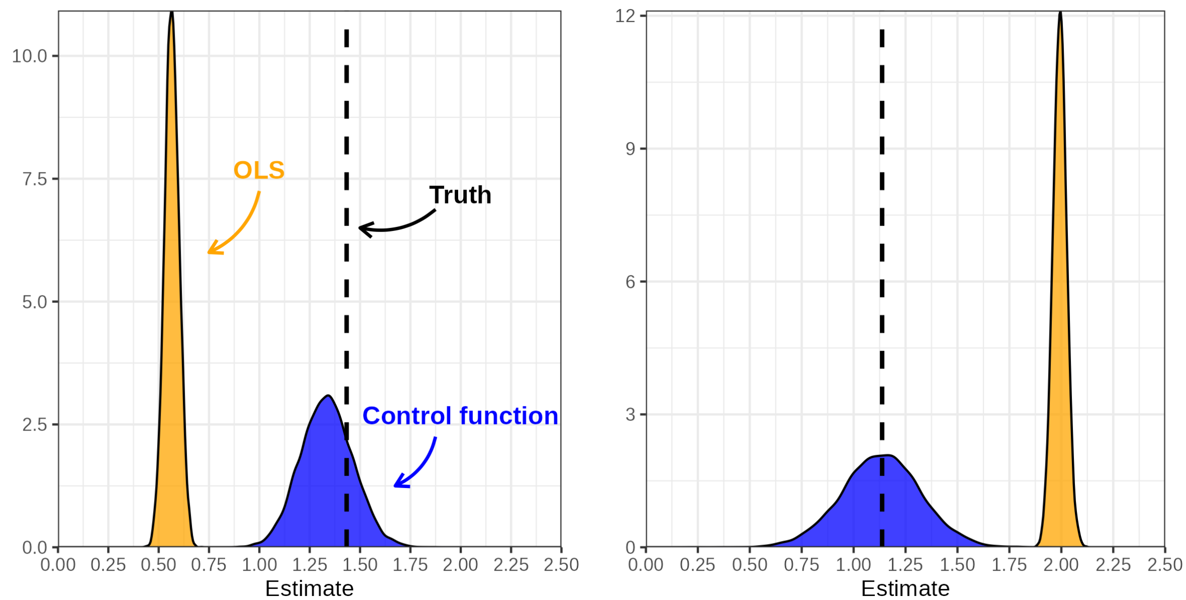
After Z_i is assigned, i chooses to take mediator D_i by considering the costs and benefits — which vary based on Z_i, demographic controls $\boldsymbol{X}_i$, and the (non-degenerate) unobserved error terms $U_{i,0}, U_{1,i}$. As a result, sequential ignorability does not hold; the mediator is not conditionally ignorable. Thus, a standard OLS (selection-on-observables) approach to CM does not give an estimate for how much of the $Z \to Y$ 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. Figure 2 shows the distribution of bootstrapped point estimates in this simulation, showing OLS

[17] Note that ATE = ADE + AIE, in this setting. $\Pr\left(Z_i = 1\right) = 0.5$ ensures this equality, but is not guaranteed in general.

Figure 2: Simulated Distribution of CM Effect Estimates.

(a) ADE. **(b)** AIE.



Note: These figures show the empirical density of point estimates, for 10,000 replications of the data generating process described above. The black dashed line is the true value; orange is the distribution of naïve OLS estimates, and blue the control function approach.

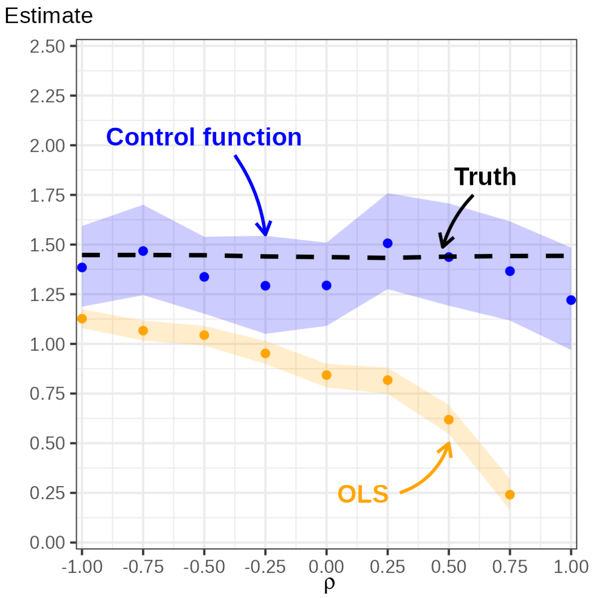
against the control function approach. The OLS approach implicitly assumes that the mediator is ignorable (when it is not), so its point estimates under and over-estimate the true ADE and AIE, respectively. The distance between the OLS estimates and the true values are the underlying bias terms derived 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 control function approach exhibits bias, though the 95% confidence intervals cover the truth.

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 $\sigma_0, \sigma_1 \geq 0$.[18] Figure 3 shows control function estimates against estimates calculated by standard OLS, showing 95% confidence intervals calculated from 1,000 bootstraps. The point estimates of the control function do not exactly equal the true values, as they are estimates from one simulation (not

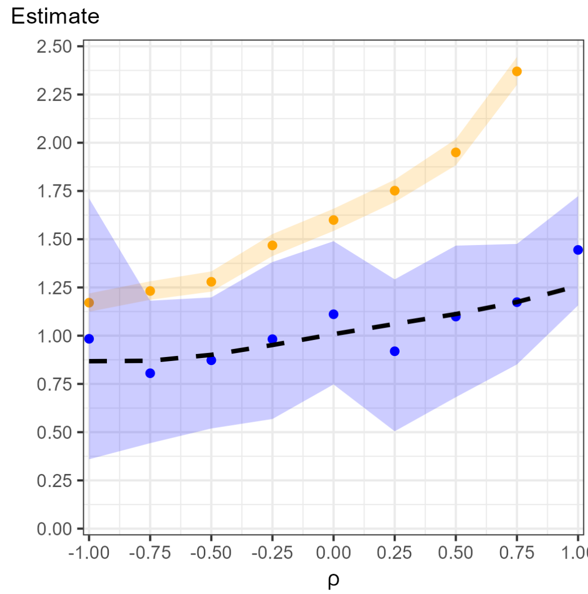
[18] Indeed, this setting has error terms following a bivariate normal distribution, so the canonical Heckman (1974) selection model would produce the most efficient estimates by maximum likelihood. The control function approach avoids this assumption, and bias from breaking it, by relying on an instrument.

Figure 3: Point Estimates of CM Effects, OLS versus Control Function, varying ρ values with $\sigma_0 = 1, \sigma_1 = 2$ fixed.

(a) ADE.



(b) AIE.



Note: These figures show the OLS and control function 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 ρ 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 control function approach. The true AIE values vary with ρ, because $D_i(Z_i)$ compliers have higher average values of $U_{1,i} - U_{0,i}$ with greater ρ values.

averages across many simulations, as in Figure 2). The control function approach improves on OLS estimates by correcting for bias, with confidence regions overlapping the true values.[19,20] This correction did not come for free: the standard errors are significantly greater in a control function 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 control function approach to estimating CM effects in practice.

[19] The code behind this simulation estimates the first-stage with an interacted OLS specification, and splines included for the continuous regressor $\boldsymbol{X}_i^-$. The second-stage is an OLS specification, including the control function with a spline specification.

[20] In the appendix, Figure A1 shows the same simulation while varying σ_1, with fixed $\sigma_0 = 1, \rho = 0.5$. The conclusion is the same as for varying the correlation coefficient, ρ, in Figure 3.

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 select-into-treatment as a way of judging CM methods. It has drawn on the classic literature, and pointed to already-in-use 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 control function as an additional robustness in the growing double robustness literature (Huber 2020, Bia, Huber & Lafférs 2024).

This paper has not lit the way for applied researchers to use CM methods unabashedly, with or without a control function adjustment. The structural assumptions are strong and large sample sizes are needed; if the assumptions are broken, then the control function method does not improve on a naïve selection-on-observables approach to CM estimates. 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 exogenous variation in mediator take-up costs. In these cases, this paper opens the door to identifying mechanisms behind treatment effects in natural experiment settings.

References

Angrist, J. D. (1990), 'Lifetime earnings and the vietnam era draft lottery: evidence from social security administrative records', *The american economic review* pp. 313–336. https://doi.org/10.3386/w3514. 5

Angrist, J. D. (1998), 'Estimating the labor market impact of voluntary military service using social security data on military applicants', *Econometrica* **66**(2), 249–288. https://doi.org/10.2307/2998558. 7

Angrist, J. D. (2022), 'Empirical strategies in economics: Illuminating the path from cause to effect', *Econometrica* **90**(6), 2509–2539. https://doi.org/10.3982/ecta20640. 12

Angrist, J. D., Imbens, G. W. & Rubin, D. B. (1996), 'Identification of causal effects using instrumental variables', *Journal of the American statistical Association* **91**(434), 444–455. https://doi.org/10.2307/2291629. 4

Angrist, J. D. & Pischke, J.-S. (2009), *Mostly harmless econometrics: An empiricist's companion*, Princeton university press. 9, 12

Bia, M., Huber, M. & Lafférs, L. (2024), 'Double machine learning for sample selection models', *Journal of Business & Economic Statistics* **42**(3), 958–969. https://doi.org/10.1080/07350015.2023.2271071. 20

Card, D. (1993), 'Using geographic variation in college proximity to estimate the return to schooling'. 15

Cinelli, C., Forney, A. & Pearl, J. (2024), 'A crash course in good and bad controls', *Sociological Methods & Research* **53**(3), 1071–1104. https://doi.org/10.2139/ssrn.3689437. 3, 9

Deuchert, E., Huber, M. & Schelker, M. (2019), 'Direct and indirect effects based on difference-in-differences with an application to political preferences following the vietnam draft lottery', *Journal of Business & Economic Statistics* **37**(4), 710–720. https://doi.org/10.1080/07350015.2017.1419139. 2

Ding, P. & Miratrix, L. W. (2015), 'To adjust or not to adjust? sensitivity analysis of m-bias and butterfly-bias', *Journal of Causal Inference* **3**(1), 41–57. https://doi.org/10.1515/jci-2013-0021. 3, 9

Florens, J.-P., Heckman, J. J., Meghir, C. & Vytlacil, E. (2008), 'Identification of treatment effects using control functions in models with continuous, endogenous treatment and heterogeneous effects', *Econometrica* **76**(5), 1191–1206. https://doi.org/10.3982/ecta5317. 3, 14

Flores, C. A. & Flores-Lagunes, A. (2009), 'Identification and estimation of causal mechanisms and net effects of a treatment under unconfoundedness', *IZA Discussion paper* . https://doi.org/10.2139/ssrn.1423353. 2

Frölich, M. & Huber, M. (2017), 'Direct and indirect treatment effects–causal chains and mediation analysis with instrumental variables', *Journal of the Royal Statistical Society Series B: Statistical Methodology* **79**(5), 1645–1666. https://doi.org/10.1111/rssb.12232. 2, 3

Heckman, J. (1974), 'Shadow prices, market wages, and labor supply', *Econometrica: journal of the econometric society* pp. 679–694. https://doi.org/10.2307/1913937. 18

Heckman, J., Ichimura, H., Smith, J. & Todd, P. (1998), 'Characterizing selection bias using experimental data', *Econometrica* **66**(5), 1017–1098. https://doi.org/10.2307/2999630. 3, 9

Heckman, J. J. (1979), 'Sample selection bias as a specification error', *Econometrica: Journal of the econometric society* pp. 153–161. https://doi.org/10.2307/1912352. 3

Heckman, J. J. & Honore, B. E. (1990), 'The empirical content of the roy model', *Econometrica: Journal of the Econometric Society* pp. 1121–1149. https://doi.org/10.2307/2938303. 3, 11

Heckman, J. J. & Vytlacil, E. (2005), 'Structural equations, treatment effects, and econometric policy evaluation 1', *Econometrica* **73**(3), 669–738. https://doi.org/10.1111/j.1468-0262.2005.00594.x. 10

Heckman, J. & Navarro-Lozano, S. (2004), 'Using matching, instrumental variables, and control functions to estimate economic choice models', *Review of Economics and statistics* **86**(1), 30–57. https://doi.org/10.3386/w9497. 3

Huber, M. (2020), 'Mediation analysis', *Handbook of labor, human resources and population economics* pp. 1–38. https://doi.org/10.1007/978-3-319-57365-6_162-1. 2, 20

Huber, M., Hsu, Y.-C., Lee, Y.-Y. & Lettry, L. (2020), 'Direct and indirect effects of continuous treatments based on generalized propensity score weighting', *Journal of Applied Econometrics* **35**(7), 814–840. https://doi.org/10.1002/jae.2765. 4

Imai, K., Keele, L., Tingley, D. & Yamamoto, T. (2010), Causal mediation analysis using r, *in* 'Advances in social science research using R', Springer, pp. 129–154. 31

Imai, K., Keele, L. & Yamamoto, T. (2010), 'Identification, inference and sensitivity analysis for causal mediation effects', *Statistical Science* pp. 51–71. https://doi.org/10.1214/10-sts321. 2, 3, 4, 5, 6, 7, 24, 29, 31

Imai, K., Tingley, D. & Yamamoto, T. (2013), 'Experimental designs for identifying causal mechanisms', *Journal of the Royal Statistical Society Series A: Statistics in Society* **176**(1), 5–51. https://doi.org/10.1111/j.1467-985x.2012.01032.x. 3

Imbens, G. & Angrist, J. (1994), 'Identification and estimation of local average treatment effects', *Econometrica* **62**(2), 467–475. https://doi.org/10.2307/2951620. 5, 13

Imbens, G. W. (2007), 'Nonadditive models with endogenous regressors', *Econometric Society Monographs* **43**, 17. https://doi.org/10.1017/ccol0521871549.002. 4

Imbens, G. W. & Newey, W. K. (2009), 'Identification and estimation of triangular simultaneous equations models without additivity', *Econometrica* **77**(5), 1481–1512. https://doi.org/10.3982/ecta7108. 14, 15

Keele, L., Tingley, D. & Yamamoto, T. (2015), 'Identifying mechanisms behind policy interventions via causal mediation analysis', *Journal of Policy Analysis and Management* **34**(4), 937–963. https://doi.org/10.1002/pam.21853. 9

Kwon, S. & Roth, J. (2024), 'Testing mechanisms', *arXiv preprint arXiv:2404.11739* . https://doi.org/10.20944/preprints202411.2377.v1. 2

Ludwig, J., Kling, J. R. & Mullainathan, S. (2011), 'Mechanism experiments and policy evaluations', *Journal of economic Perspectives* **25**(3), 17–38. https://doi.org/10.3386/w17062. 3

R Core Team (2023), *R: A Language and Environment for Statistical Computing*, R Foundation for Statistical Computing, Vienna, Austria. https://www.R-project.org/. 31

Robins, J. (1986), 'A new approach to causal inference in mortality studies with a sustained exposure period—application to control of the healthy worker survivor effect', *Mathematical modelling* **7**(9-12), 1393–1512. https://doi.org/10.1016/0270-0255(86)90088-6. 7

Roy, A. D. (1951), 'Some thoughts on the distribution of earnings', *Oxford economic papers* **3**(2), 135–146. https://doi.org/10.1093/oxfordjournals.oep.a041827. 3, 11

Słoczyński, T. (2022), 'Interpreting ols estimands when treatment effects are heterogeneous: Smaller groups get larger weights', *Review of Economics and Statistics* **104**(3), 501–509. https://doi.org/10.1162/rest_a_01169. 7

Vytlacil, E. (2002), 'Independence, monotonicity, and latent index models: An equivalence result', *Econometrica* **70**(1), 331–341. https://doi.org/10.1111/1468-0262.00277. 3, 14

Wang, W. & Yan, J. (2021), 'Shape-restricted regression splines with r package splines2.', *Journal of Data Science* **19**(3). https://doi.org/10.6339/21-jds1020. 31

Wickham, H., Averick, M., Bryan, J., Chang, W., McGowan, L. D., François, R., Grolemund, G., Hayes, A., Henry, L., Hester, J., Kuhn, M., Pedersen, T. L., Miller, E., Bache, S. M., Müller, K., Ooms, J., Robinson, D., Seidel, D. P., Spinu, V., Takahashi, K., Vaughan, D., Wilke, C., Woo, K. & Yutani, H. (2019), 'Welcome to the tidyverse', *Journal of Open Source Software* **4**(43), 1686. https://doi.org/10.21105/joss.01686. 31

A Appendix

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

A.1 Identification in Causal Mediation

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

$$\mathbb{E}\left[Y_i(1, D_i(z')) - Y_i(0, D_i(z'))\right] = \int \int \Big(\mathbb{E}\left[Y_i \,|\, Z_i = 1, D_i, \boldsymbol{X}_i\right] - \mathbb{E}\left[Y_i \,|\, Z_i = 0, D_i, \boldsymbol{X}_i\right] \Big) dF_{D_i \,|\, Z_i = z', \boldsymbol{X}_i} dF_{\boldsymbol{X}_i},$$

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

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

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

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 cpmpliers conditional on $\boldsymbol{X}_i$ and AIE conditional on $\boldsymbol{X}_i$, LAIE = AIE, (2) identification of the complier score.

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

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, $\Pr\left(D_i(1) \neq D_i(0) \,|\, \boldsymbol{X}_i\right) = \mathbb{E}\left[D_i \,|\, Z_i = 1, \boldsymbol{X}_i\right] - \mathbb{E}\left[D_i \,|\, Z_i = 0, \boldsymbol{X}_i\right]$.

A.2 Bias in Mediation Estimates

Suppose that Z_i is ignorable conditional on $\boldsymbol{X}_i$, 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 $\boldsymbol{X}_i$. For each $d' = 0, 1$.

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

And so,

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

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.

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

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

Collect everything together, as follows.

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

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

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 $\boldsymbol{X}_i$.

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

When D_i is not ignorable, the bias comes from estimating the second term, $\mathbb{E}\left[Y_i(Z_i, 1) - Y_i(Z_i, 0) \,|\, D_i(1) = 1, D_i(0) = 0, \boldsymbol{X}_i\right]$.

For each $z' = 0, 1$.

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

So compose the CM estimand, as follows.

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

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.

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

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

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

Collect everything together, as follows.

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

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

A.3 A Regression Framework for Direct and Indirect Effects

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

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

$U_{0,i}, U_{1,i}$ are error terms with unknown distributions, mean independent of $Z_i, \boldsymbol{X}_i$ by definition — but possibly correlated with D_i.

Z_i is conditionally independent of potential outcomes, so that $U_{0,i}, U_{1,i} \perp\!\!\!\perp Z_i$. Thus, the first-stage regression of $Z \to Y$ has unbiased estimates.

$$\begin{aligned}
D_i &= Z_i D_i(1) + (1 - Z_i) D_i(0) \\
&= D_i(0) + Z_i \left[D_i(1) - D_i(0)\right] \\
&= \underbrace{\mathbb{E}\left[D_i(0) \,|\, \boldsymbol{X}_i\right]}_{\text{Intercept}} + \underbrace{Z_i \mathbb{E}\left[D_i(1) - D_i(0)\right]}_{\text{Regressor}} \\
&\quad + \underbrace{D_i(0) - \mathbb{E}\left[D_i(0) \,|\, \boldsymbol{X}_i\right] + Z_i \Big(D_i(1) - D_i(0) - \mathbb{E}\left[D_i(1) - D_i(0) \,|\, \boldsymbol{X}_i\right]\Big)}_{\text{Mean-zero independent error term, since } Z_i \perp\!\!\!\perp D_i \,|\, \boldsymbol{X}_i} \\
&=: \phi + \pi Z_i + \varphi(\boldsymbol{X}_i) + \eta_i
\end{aligned}$$

$\implies \mathbb{E}\left[D_i \,|\, Z_i, \boldsymbol{X}_i\right] = \phi + \pi Z_i + \varphi(\boldsymbol{X}_i)$, and thus unbiased estimates since $Z_i \perp\!\!\!\perp \phi, \eta_i$.

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

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

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

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

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

With the following definitions:

(a) $\alpha = \mathbb{E}\left[\mu_0(0; \boldsymbol{X}_i)\right]$ and $\zeta(\boldsymbol{X}_i) = \mu_0(0; \boldsymbol{X}_i) - \alpha$ are the intercept terms.

(b) $\beta = \mu_1(0; \boldsymbol{X}_i) - \mu_0(0; \boldsymbol{X}_i)$ is the indirect effect under $Z_i = 0$

(c) $\gamma = \mu_0(1; \boldsymbol{X}_i) - \mu_0(0; \boldsymbol{X}_i)$ is the direct effect under $D_i = 0$.

(d) $\delta = \mu_1(1; \boldsymbol{X}_i) - \mu_0(1; \boldsymbol{X}_i) - \left(\mu_1(0; \boldsymbol{X}_i) - \mu_0(0; \boldsymbol{X}_i)\right)$ 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:

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

Taking the conditional expectation, and collecting for the expressions of the direct and indirect effects:[21]

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

These terms are conventionally estimated in a simultaneous regression (Imai, Keele & Yamamoto 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.

[21]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 $\boldsymbol{X}_i$ only makes sense in the case of heterogeneous treatment effects.

$$\mathbb{E}_{\boldsymbol{X}_i}\left[\mathbb{E}\left[Y_i \mid Z_i = D_i = 0, \boldsymbol{X}_i\right]\right] = \mathbb{E}\left[\alpha\right] + \mathbb{E}\left[U_{0,i} \mid D_i = 0\right]$$

$$\mathbb{E}_{\boldsymbol{X}_i}\left[\mathbb{E}\left[Y_i \mid Z_i = 0, D_i = 1, \boldsymbol{X}_i\right] - \mathbb{E}\left[Y_i \mid Z_i = 0, D_i = 0, \boldsymbol{X}_i\right]\right] = \mathbb{E}\left[\beta\right] + \left(\mathbb{E}\left[U_{1,i} \mid D_i = 1\right] - \mathbb{E}\left[U_{0,i} \mid D_i = 0\right]\right)$$

$$\mathbb{E}_{\boldsymbol{X}_i}\left[\mathbb{E}\left[Y_i \mid Z_i = 1, D_i = 0, \boldsymbol{X}_i\right] - \mathbb{E}\left[Y_i \mid Z_i = 0, D_i = 0, \boldsymbol{X}_i\right]\right] = \mathbb{E}\left[\gamma\right] + \mathbb{E}\left[U_{0,i} \mid D_i = 0\right]$$

$$\mathbb{E}_{\boldsymbol{X}_i}\begin{bmatrix}\mathbb{E}\left[Y_i \mid Z_i = 1, D_i = 1, \boldsymbol{X}_i\right] - \mathbb{E}\left[Y_i \mid Z_i = 1, D_i = 0, \boldsymbol{X}_i\right] \\ -\left(\mathbb{E}\left[Y_i \mid Z_i = 0, D_i = 1, \boldsymbol{X}_i\right] - \mathbb{E}\left[Y_i \mid Z_i = 0, D_i = 0, \boldsymbol{X}_i\right]\right)\end{bmatrix} = \mathbb{E}\left[\delta\right]$$

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 this, 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 $U_{0,i}, U_{1,i}$:

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

In this case, the control set $\boldsymbol{X}_i$ 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 $\boldsymbol{X}_i$), 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 $Z_i, \boldsymbol{X}_i$. For each $z', d' = 0, 1$

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

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.

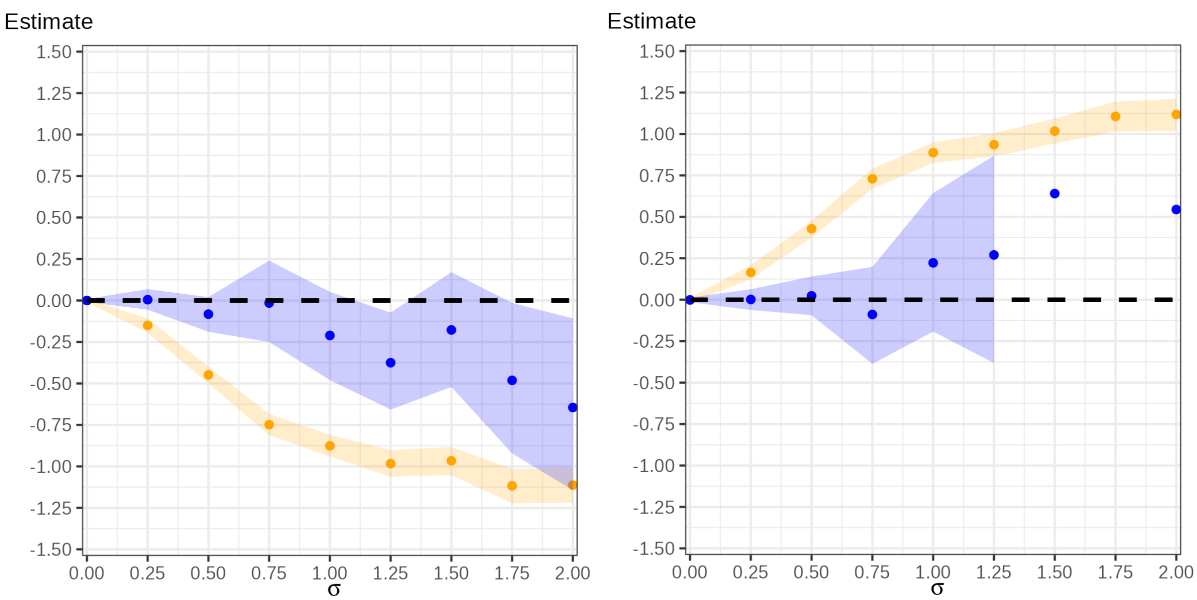
A.5 Control Function Simulation

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, Grolemund, 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* (Imai, Keele, Tingley & Yamamoto 2010) automates the sequential-ignorability estimates of CM effects (Imai, Keele & Yamamoto 2010) in the R language.

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

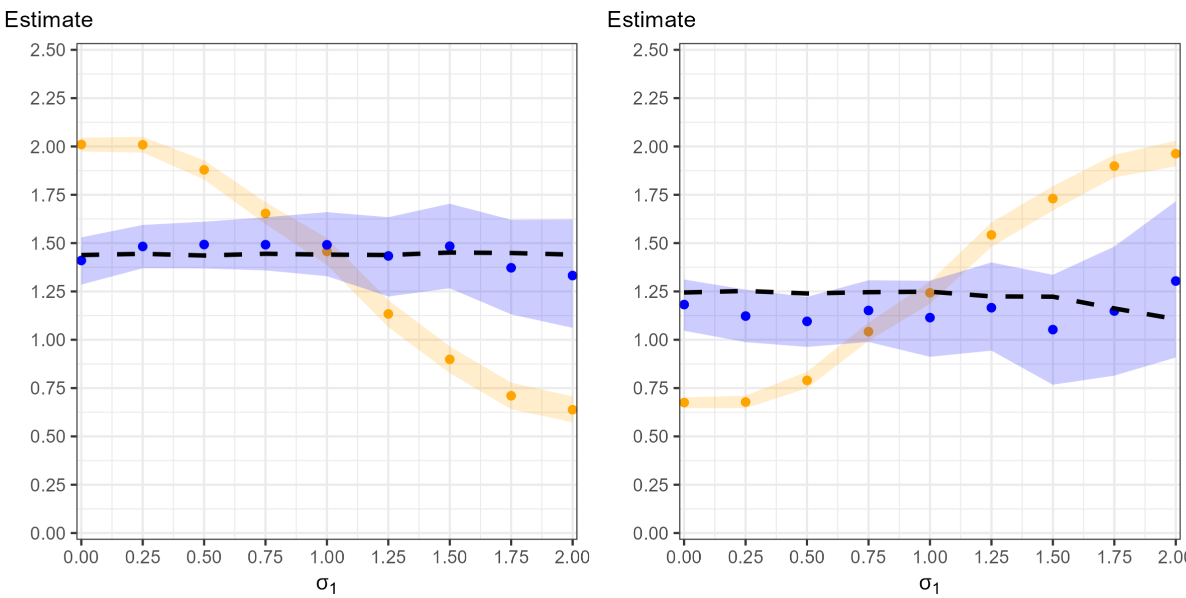
(a) $\widehat{\text{ADE}} - \text{ADE}$. **(b)** $\widehat{\text{AIE}} - \text{AIE}$.



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 σ_1 relative to $\sigma_0 = 1$.

(a) ADE. **(b)** AIE.



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 σ_1 varied across $[0, 2]$. Shaded regions are the 95% confidence intervals; orange are the OLS estimates, blue the control function approach.