Causal Mediation in Natural Experiments

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First draft: 12 February 2025
This version: 14 July 2025

Working Paper, 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 randomisation. Applied researchers often investigate mechanisms with a suggestive investigation of mechanisms, giving at best shaky evidence for mechanisms behind causal effects. Causal Mediation (CM) provides an alternative, robust framework for identifying and estimating direct and indirect effects (CM effects) for causal mechanisms. However, conventional CM methods require strong assumptions, which are implausible in natural experiment settings. In particular, they assume the mediator is as-good-as-random, conditional on treatment and covariates. When individuals select into a mediator based on costs and benefits, this assumption fails, undermining causal inference. I develop an alternative approach to credibly estimate CM effects, using control function methods and instrumental variation in take-up costs to avoid unrealistic assumptions. Simulations confirm this approach corrects for bias in conventional CM estimates, providing parametric and semi-parametric methods. I illustrate the approach by analysing the effect of health insurance through healthcare in the Oregon Health Insurance Experiment. This approach gives applied researchers an alternative method to estimate CM effects when an initial treatment is quasi-randomly assigned, but the mediator is not, as is common in natural experiments.

Keywords: Direct/indirect effects, quasi-experiment, selection, control function, MTEs.
JEL Codes: C21, C31.

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

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Economists use natural experiments to credibly answer social questions, when an experiment was infeasible. For example, does health insurance causally improve health outcomes (Finkelstein, Taubman, Wright, Bernstein, Gruber, Newhouse, Allen, Baicker & Group 2012)? Natural experiments are settings which 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, do causal gains from health insurance come mostly from starting to utilise healthcare more often, or are there other direct effects? This study of mechanisms behind causal effects broadens the economic understanding of social settings studied with natural experiments. 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 quasi-randomly assigned? Estimates for the average direct and indirect effects are contaminated by bias terms — selection bias plus group difference terms. For example, if individuals had been choosing to seek medical care more frequently with new health insurance, then underlying health conditions would confound estimates of the direct and indirect effects of health insurance through using more healthcare. This approach only leads to credible causal estimates if the mediator is also quasi-randomly assigned. 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 condition 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 adjustment. This solves the identification problem with structural assumptions for selection-into-mediator — mediator monotonicity and selection based on 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 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 costs as an 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. Indeed, this approach provides no safe harbour for estimating CM effects if these structural assumptions do not hold true.

The conventional 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 mechanisms analyses in applied economics allude to CM methods (despite masquerading under an alternative moniker), and so unintentionally import this identifying assumption.

Assuming the mediator is ignorable (i.e., quasi-randomly assigned or satisfies selection-on-observables) conveniently ignores selection into the mediator by assuming either (1) people naïvely made decisions to take or refuse a mediator, or (2) a researcher controlled for everything relevant to this decision. This assumption might be reasonable when studying single-celled organisms in a laboratory — their "decisions" are simple and mechanical. Social scientists, however, study humans who make complex choices based on costs, benefits, and preferences — which are only partially observed by researchers (at best). 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 for a treatment, let alone another independent source for a mediator, at the same time.

The applied economics literature has been hesitant to use explicit CM methods, and began conducting informal mechanism analyses by 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 just the same. A new strand of the econometric literature has developed estimators for explicit CM analyses 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), functional form restrictions (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 implicit acknowledgement that a conventional selection-on-observables approach to CM in applied settings can lead to biased inference, and needs alternative methods for credible inference.

This paper explicitly shows how a conventional approaches to CM can lead to biased inference in natural experiments. I develop a 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, and find it unlikely to hold in a natural experiment setting.[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 to correct for the selection bias in conventional CM analyses.

The control function approach requires mediator take-up respond only positively to the initial treatment (monotonicity), which implies mediator selection follows a selection model. Second, it assumes that mediator take-up is motivated by mediator benefits. Last, it requires a valid instrument for mediator take-up, to avoid relying on parametric assumptions on unobserved selection. This approach to identifying CM effects (despite selection-into-mediator) imports insights from the instrumental variables literature, connecting the influential Imai et al. (2010) approach to CM with the economics literature on selection-into-treatment and marginal treatment effects (Vytlacil 2002, Heckman & Navarro-Lozano 2004, Heckman & Vytlacil 2005, Florens, Heckman, Meghir & Vytlacil 2008, Kline & Walters 2019).[2] Frölich & Huber (2017) have previously explored identification of CM effects with a control function, in the context of two instruments (one each for treatment and mediator) and a continuous mediator; this paper only considers a binary mediator, with a correspondingly different identification analysis and resulting estimation strategies.

This paper proceeds as follows. Section 1 describes the dominant approach in economics for studying mechanisms behind treatment effects, illustrating with data from the Oregon Health Insurance Experiment. Section 2 introduces the formal framework for CM, and develops expressions for bias in CM estimates in natural experiments. Section 3 describes this bias in applied settings with (1) a regression framework, (2) a setting with selection based

[1] An alternative method to estimate CM effects is ensuring treatment and mediator ignorability holds by a running two randomised controlled trials (or two suitable quasi-experiments) 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).

[2] Indeed, this paper does not invent control function methods, instead noting their applicability in this setting. See Wooldridge (2015), Imbens (2007) for general overviews of the approach.

on costs and benefits. Section 4 shows how a control function can effectively purge this bias from CM estimates. Section 5 demonstrates how to estimate CM effects with this approach, with either parametric or semi-parametric methods, giving supporting simulation evidence. Section 6 returns to the Oregon Health Insurance Experiment, providing credible estimates of the effect of health insurance mediated through increased healthcare usage. Section 7 concludes.

1 Mechanisms Behind the Effect of Health Insurance

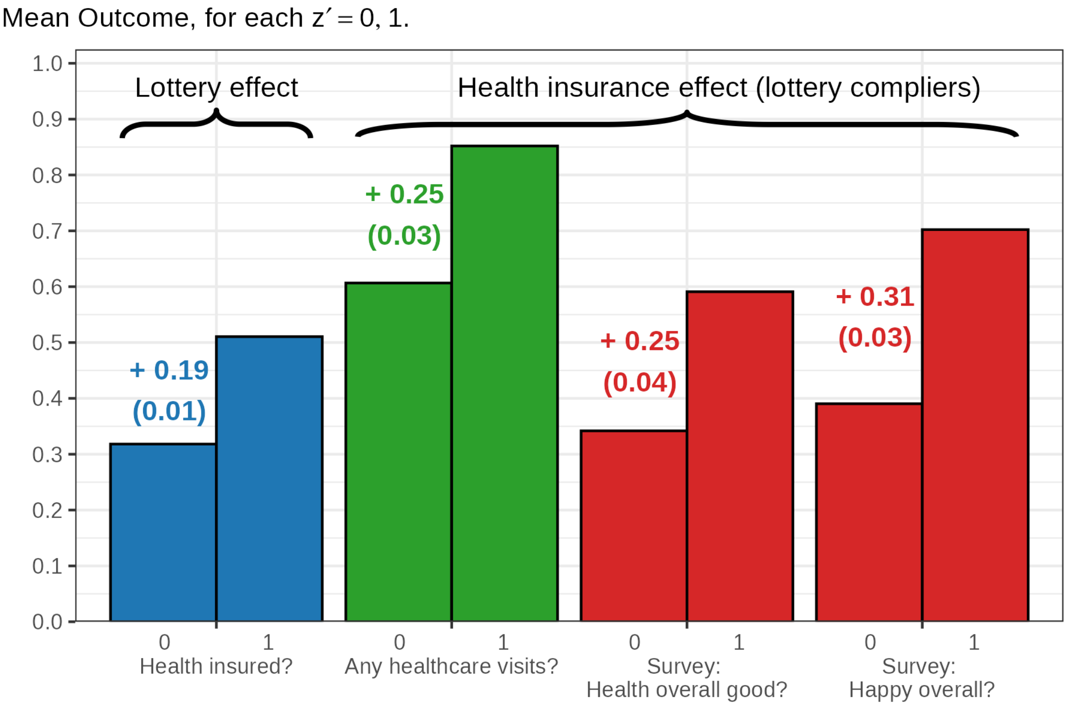
In the United States, healthcare is generally not provided directly by the government. Instead, consumers purchase health insurance to fund healthcare expenses, with the government providing insurance only for elderly individuals (Medicare) and for those with low-incomes (Medicaid). In 2004, the state of Oregon ceased accepting new applications for Medicaid due to budgetary constraints, and did not reopen applications until 2008. When the state resumed enrolment, approximately 90,000 individuals applied, vastly exceeding the programme's capacity. Oregon therefore allocated the opportunity to apply for Medicaid via a lottery system among those on the wait-list. This wait-list lottery significantly increased health insurance coverage among the insurance applicants.

Winning the lottery increased health insurance coverage rate by 19 percentage points (pp). This gave the opportunity to use the lottery as an instrument for having health insurance at all,[3] among the 15,015 people from the wait-list lottery who responded to a survey sent by Finkelstein et al. (2012) one year later. Among the wait-lottery compliers, having health insurance increased the rate of having visited the doctor (either locally or at a hospital) by 25 pp. Furthermore, gaining insurance led to significant improvements in reported well-being: lottery compliers were 25 pp more likely to report being in good overall health, and 31 pp more likely to report being happy overall. These statistics were calculated using the Oregon Health Insurance Experiment replication package (Finkelstein & Baicker 2014), and are summarised in Figure 1.

These results show that health insurance led to large self-reported gains in both health and happiness. The economics, medicine, and health policy literatures have primarily focused on the health benefits — often interpreted as healthcare benefits among lottery compliers, who had previously delayed or avoided care due to cost concerns. However, the original authors also noted other benefits of insurance, including complete elimination of catastrophic out-of-pocket medical debt among the treated. These are plausibly income effects that benefit

[3] This additionally assumes everyone responded was at least more likely to take health insurance if they won the wait-list lottery, and the lottery did not have other direct effects.

Figure 1: Effects of Health Insurance in the Oregon Health Insurance Experiment.



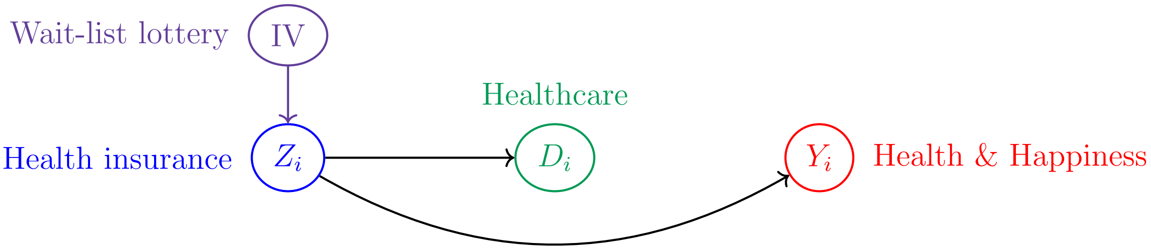
Note: This figure summarises the relevant results of the Oregon Health Insurance Experiment (Finkelstein et al. 2012). The lottery results show that winning the Medicaid wait-list lottery increased health insurance rate by 19 percentage points. The effect of health insurance shows estimates of the average lottery complier health insurance effect on surveyed outcomes. It uses the wait-list lottery as an instrument for having health insurance, and the Abadie (2003) weighting scheme to estimate the average lottery complier levels, $\mathbb{E}\left[Y_i(z', .) \,|\, \text{lottery complier}\right]$ for each $z' = 0, 1$, with standard errors calculated by the bootstrap in brackets.

recipients directly, not only through increased use of healthcare, but also by reducing stress and improving financial security. These plausible direct effects have not been explored in the applied literature.

Accepted practice in applied economics is to investigate mechanisms behind treatment effects with "suggestive evidence of mechanisms." This involves estimating the average causal effect of health insurance on a proposed mediator (healthcare usage) and separately estimating its effect on the final outcomes (self-reported health and happiness). When both estimates are positive, and the mediator precedes the outcome, it is taken as de facto evidence that the mediator transmits the treatment effect. In the case of the Oregon Health Insurance Experiment, this amounts to concluding that increased healthcare usage mediates the positive effects of health insurance on health and happiness. Figure 2 shows this approach in a stylised figure; the approach of suggestive evidence of mechanisms is also prevalent in other social sciences (see Blackwell et al. 2024).

There are two main problems with this approach. First, it provides no evidence for the

Figure 2: Structural Causal Model for Suggestive Evidence of a Mechanism.



Note: This figure shows the structural causal model behind an inform mechanism analysis of the effects of health insurance, where arrows represent causal effects — e.g., $Z_i \rightarrow D_i$ means Z_i affects D_i with no reverse causality. The variables correspond to the causal design in the Oregon Health Insurance Experiment.

effect of healthcare on health outcomes, so does not identify the causal mechanism. It is hiding the assumption that healthcare positively affects health outcomes. While this assumption is not unreasonable, nowhere else in applied economics is a hidden assumption of a positive treatment effect taken at face value — it must be motivated with quantitative evidence. Second, this approach does not quantify the mechanism effects. The proportion of health insurance benefits operating through healthcare could only have small average effects in a 2 year span — or possible very large, even dominant, if very strong. Additionally, the relevant effect is not the average effect of healthcare usage, but the effect for Oregon residents who were induced to use more healthcare because of newly acquired Medicaid insurance. This local effect may differ substantially from a population average, and potentially mislead conclusions about the magnitude or generality of the mechanism. To summarise, this approach is not suggestive evidence of mechanisms, it is assumptive evidence of mechanisms; it compels claims about mechanisms behind treatment effects which are not motivated by causal evidence.

Causal Mediation (CM) offers a compelling alternative framework. Unlike suggestive evidence of mechanisms, CM explicitly defines the estimands of interest (the direct and indirect effects) and lays out clear assumptions under which these quantities are identified. Moreover, it delivers quantitative answers to the key question: how much of a treatment effect operates through a specific mediator mechanism? CM is widely used in fields such as epidemiology and psychology, where researchers regularly decompose treatment effects into component pathways. However, CM methods have not yet been rigorously examined from an economic perspective to assess their applicability in observational causal research, such as natural experiments, which are the dominant source of identification in applied microeconomics.

2 Causal Mediation (CM)

CM decomposes causal effects into two channels, through a mediator (indirect effect) and through all other paths (direct effect). To develop notation, write $Z_i = 0, 1$ for a binary treatment, $D_i = 0, 1$ a binary mediator, and Y_i a continuous outcome.[4] D_i, Y_i are a sum of their potential outcomes,

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

Assume treatment 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.

1. The average first-stage refers to the effect of the treatment on mediator, Z_i on D_i:

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

It is common in the economics literature to assume that Z_i influences D_i in at most one direction, $\Pr\left(D_i(0) \leq D_i(1)\right) = 1$ — 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_i on Y_i, 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:

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

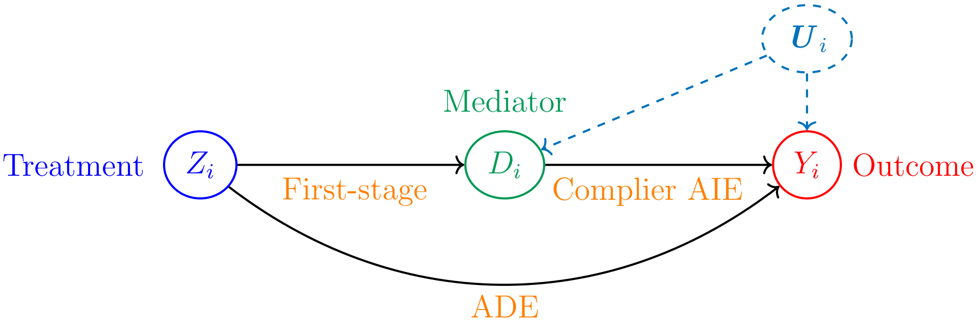
Z_i affects outcome Y_i directly, and indirectly via the $D_i(Z_i)$ channel, with no reverse causality. Figure 3 visualises the design, where the direction arrows denote the causal direction. CM aims to decompose the ATE of Z_i on Y_i into these two separate pathways:

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

[4] This paper exclusively focuses on the binary case. See Huber, Hsu, Lee & Lettry (2020) or Frölich & Huber (2017) 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.

Figure 3: Structural Causal Model for CM.



Note: This figure shows the structural causal model behind CM. The Complier AIE refers to the AIE local to $D_i(Z_i)$ compliers, so that AIE = average first-stage × Complier AIE. $\boldsymbol{U}_i$ represents this paper's focus on the case that D_i is not ignorable by showing an unobserved confounder. Subsection 3.1 defines $\boldsymbol{U}_i$ in an applied setting.

Estimating the AIE answers the following question: how much of the causal effect Z_i on Y_i goes through the D_i channel? When studying the health gains of health insurance (Finkelstein et al. 2012), 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_i channel?[6] For the health insurance example, how much of the health insurance effect is a direct effect, other than increased healthcare usage — e.g., income effects of lower medical debt, or less worry over health shocks. The Instrumental Variables (IV) 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.

2.1 Identification of 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 $\boldsymbol{X}_i$.

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

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

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

[6] In a non-parametric setting it is not necessary that ADE + AIE = ATE. See Imai et al. (2010) for this point in full.

Sequential ignorability assumes that the initial treatment Z_i is ignorable conditional on $\boldsymbol{X}_i$ (as has already been assumed above). It then also assumes that, after Z_i is assigned, that D_i is ignorable conditional on $\boldsymbol{X}, Z_i$ (hereafter, mediator ignorability). If 1(1) and 1(2) hold, then the ADE and AIE are identified by two-stage mean differences conditioning on $\boldsymbol{X}_i$.[7]

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

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

$$= \underbrace{\mathbb{E}\left[Y_i(Z_i, D_i(1)) - Y_i(Z_i, D_i(0))\right]}_{\text{Average Indirect Effect (AIE)}}$$

I refer to the estimands on the left-hand side as CM estimands, which are typically estimated by a composition of 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 for my identification analysis. Linearity assumptions are not necessary for identification, and it suffices to note that heterogeneous treatment effects and non-linear confounding can 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.

2.2 Non-identification of CM Effects

Applied research often uses a natural experiment to study settings where treatment Z_i is ignorable, justifying assumption 1(1). Rarely do they also have access to an additional, overlapping natural experiment to isolate random variation in D_i — to justify mediator ignorability 1(2). One might consider conventional CM methods in such a setting to learn about the mechanisms behind the causal effect Z_i on Y_i, without the problems associated with suggestive evidence of mechanisms. This approach leads to biased estimates, and further contaminates inference regarding direct and indirect effects.

Theorem 1. *Absent an identification strategy for the mediator, CM estimates are at risk of selection bias. If 1(1) holds, and 1(2) does not, then CM estimands are contaminated by*

[7] In addition, a common support condition for both Z_i, D_i (across $\boldsymbol{X}_i$) is necessary. 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. Appendix 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.

selection bias and group differences. Proof: see Appendix A.2.

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.[8]

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

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

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

The selection bias terms come from systematic differences between the groups taking or refusing the mediator ($D_i = 1$ versus $D_i = 0$), differences not fully unexplained by $\boldsymbol{X}_i$. These selection bias terms would equal zero if the mediator had been ignorable 1(2), but do not necessarily average to zero if not. In the Oregon Health Insurance Experiment, the wait-list gave random variation in the treatment (health insurance) but there was not a similar natural experiment for healthcare usage; correspondingly, the selection-on-observables approach to CM has selection bias.

The group differences represent the fact that a matching approach gives an average effect

[8] The bias terms here mirror those in Heckman, Ichimura, Smith & Todd (1998), Angrist & Pischke (2009) for a single D_i on Y_i treatment effect, when D_i is not ignorable:
$$\mathbb{E}\left[Y_i \mid D_i = 1\right] - \mathbb{E}\left[Y_i \mid D_i = 0\right] = \text{ATE} + \underbrace{\Big(\mathbb{E}\left[Y_i(.,0) \mid D_i = 1\right] - \mathbb{E}\left[Y_i(.,0) \mid D_i = 0\right]\Big)}_{\text{Selection Bias}} + \underbrace{\Pr\left(D_i = 0\right)\left(\text{ATT} - \text{ATU}\right)}_{\text{Group-differences Bias}}.$$

on the treated group, which is systematically different from the average effect if selection-on-observables does not hold. 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 $D_i(Z_i)$ compliers.

$$\begin{aligned} \text{AIE} &= \mathbb{E}\left[Y_i(Z_i, D_i(1)) - Y_i(Z_i, D_i(0))\right] \\ &= \mathbb{E}\left[D_i(1) - D_i(0)\right] \underbrace{\mathbb{E}\left[Y_i(Z_i, 1) - Y_i(Z_i, 0) \,|\, D_i(0) = 0, D_i(1) = 1\right]}_{\text{Average } D_i \text{ on } Y_i \text{ effect among } D_i(Z_i) \text{ compliers}} \end{aligned}$$

It is important to acknowledge the mediator compliers here, because the AIE is the treatment effect going through the $D_i(Z_i)$ channel, thus only refers to individuals pushed into mediator D_i by initial treatment Z_i. If we had been using a population average effect for D_i on Y_i, then this is losing focus on the definition of the AIE; it is not about the causal effect D_i on Y_i, it is about the causal effect $D_i(Z_i)$ on Y_i.

The group difference bias term 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. This distinction between average effects and complier average effects in the AIE is skipped over by the "controlled effect" definitions of Pearl (2013).

3 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. 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. These conditions can exist for years before being diagnosed. People with 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 serve 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 researchers cannot hope to observe and control for health conditions that are yet to even be 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 2 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.

3.1 Regression Framework

Inference for CM effects can be written in a regression framework with random coefficients, showing how correlation between unobserved error terms and the mediator disrupts identification.

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

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

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

$$D_i = \theta + \overline{\pi} Z_i + \zeta(\boldsymbol{X}_i) + \eta_i, \tag{3}$$

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

This is not consequence of linearity assumptions; the outcome formulae allow for unconstrained heterogeneous treatment effects, because the coefficients are random. If either Z_i, D_i were continuously distributed, then this representative would not necessarily hold true. First-stage (3) is identified, with $\theta + \zeta(\boldsymbol{X}_i)$ the intercept, and $\overline{\pi}$ the first-stage average compliance rate (conditional on $\boldsymbol{X}_i$). Second-stage (4) has the following definitions, and is not identified thanks to omitted variables bias. See Appendix A.3 for the derivation.

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

(b) $\beta = \mu_1(0; \boldsymbol{X}_i) - \mu_0(0; \boldsymbol{X}_i)$ is the AIE conditional on $Z_i = 0, \boldsymbol{X}_i$.

(c) $\gamma = \mu_0(1; \boldsymbol{X}_i) - \mu_0(0; \boldsymbol{X}_i)$ is the ADE conditional on $D_i = 0, \boldsymbol{X}_i$.

(d) $\delta = \mu_1(1; \boldsymbol{X}_i) - \mu_0(1; \boldsymbol{X}_i) - \big(\mu_1(0; \boldsymbol{X}_i) - \mu_0(0; \boldsymbol{X}_i)\big)$ is the average interaction effect conditional on $\boldsymbol{X}_i$.

(e) $(1 - D_i)\, U_{0,i} + D_i U_{1,i}$ is the disruptive error term.

The ADE and AIE are averages of the random coefficients:

$$\text{ADE} = \mathbb{E}\left[\gamma + \delta D_i\right],$$
$$\text{AIE} = \mathbb{E}\left[\overline{\pi}\big(\beta + \delta Z_i + \widetilde{U}_i\big)\right], \qquad \text{with } \widetilde{U}_i = \underbrace{\mathbb{E}\left[U_{1,i} - U_{0,i} \,|\, \boldsymbol{X}_i, D_i(0) = 0, D_i(1) = 1\right]}_{\text{Unobserved complier gains.}}.$$

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

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

3.2 Selection on Costs and Benefits

CM is at risk of bias because $D_i \perp\!\!\!\perp \boldsymbol{U}_i$ 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). In the Oregon Health Insurance Experiment, the treatment variation comes from the wait-list lottery (for compliers),[9] while healthcare usage was not subject to a similar lottery. 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,

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

[9] Note that health insurance was given by wait-list lottery, so is not independently assigned for everyone. Health insurance was, however, randomly assigned among the population of lottery compliers. See Section 6 for this distinction in further detail.

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). In the Oregon Health Insurance Experiment, this models choice to visit the doctor in terms of health benefits relative to costs.[10] This makes the Roy model useful as a base case for CM, where selection-into-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, $C_i(Z_i) = \mu_C(Z_i; \boldsymbol{X}_i) + U_{C,i}$, to show Roy-selection in terms of unobserved and observed factors,

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

If selection follows a Roy model, and the mediator is ignorable, then unobserved benefits can play no part in selection. The only driver of selection are individuals' differences in costs (and not benefits). If there are any selection-into-D_i benefits unobserved to the researcher, then mediator ignorability cannot hold.

Proposition 1. *Suppose mediator selection follows a Roy model* (5)*, and selection is not fully explained by costs and observed gains. Then mediator 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 Appendix 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 — whether the researcher observes them or not. The unobserved gains are unlikely to be fully captured by an observed control set $\boldsymbol{X}_i$, except in very special cases.

In practice, the only way to believe in the mediator ignorability assumption is to study a setting where the researcher has two causal research designs, one for treatment Z_i and another for mediator D_i, at the same time. An unmotivated note saying "we conduct an informal mechanism analysis by controlling for this variable" or "we assume the mediator satisfies selection-on-observables" does not cut it here, and will lead to biased inference in applied settings.

[10] If the choice is considers over a sum of outcomes, then a simple extension to a utility maximisation model maintains this same framework with expected costs and benefits. See Heckman & Honore (1990), Eisenhauer, Heckman & Vytlacil (2015).

4 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 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, and lead to omitted variables bias.

$$\begin{aligned}\mathbb{E}\left[Y_i \,|\, Z_i, D_i, \boldsymbol{X}_i\right] \;&=\; \alpha + \beta D_i + \gamma Z_i + \delta Z_i D_i + \varphi(\boldsymbol{X}_i) \\ &\quad + \underbrace{(1 - D_i)\, \mathbb{E}\left[U_{0,i} \,|\, D_i = 0, \boldsymbol{X}_i\right] + D_i \mathbb{E}\left[U_{1,i} \,|\, D_i = 1, \boldsymbol{X}_i\right]}_{\text{Unobserved confounding.}} \end{aligned} \tag{6}$$

The CF approach models the contaminating terms in (6), avoiding the bias from omitting them in regression estimates. CF methods were first devised to correct for sample selection problems (Heckman 1974), and were extended to a general selection problem of the same form as Equation (6) (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 an external instrument or distributional assumptions; the identification strategy here focuses exclusively on the case that an instrument is available. 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 addresses the unobserved confounding from underlying health conditions. It does so by assuming that unobserved selection-into-healthcare use is informative for underlying health conditions, assuming people with more severe underlying conditions visit the doctor more often than those without. Then it uses this information in the second-stage estimation of how much the effect goes through increased healthcare usage, estimating the ADE and AIE after controlling for this confounding.

4.1 Re-identification of CM Effects

The following assumptions are sufficient to model the correlated error terms, identifying β, γ, δ in the second-stage regression (4), and thus both the ADE and AIE.

Assumption CF–1. Mediator monotonicity, conditional on $\boldsymbol{X}_i$.

$$\Pr\left(D_i(0) \leq D_i(1) \,|\, \boldsymbol{X}_i\right) = 1.$$

Assumption CF–1 is the monotonicity condition first used in an IV 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-mediators), 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 with ordered threshold crossing values that describe selection-into-D_i (Vytlacil 2002).

$$D_i(z') = \mathbb{1}\left\{V_i \leq \psi\big(z'; \boldsymbol{X}_i\big)\right\}, \quad \text{for } z' = 0, 1$$

where V_i is a latent variable with continuous distribution and conditional cumulative density function $F_V(\,.\,|\boldsymbol{X}_i)$, and $\psi(\,.\,; \boldsymbol{X}_i)$ collects observed sources of mediator selection. V_i could be assumed to follow a known distribution; the canonical Heckman selection model assumes V_i is normally distributed (a "Heckit" model). The identification strategy here applies to the general case that the distribution of V_i is unknown, without parametric restrictions.

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

$$D_i(z') = \mathbb{1}\left\{U_i \leq \pi(z'; \boldsymbol{X}_i)\right\}, \quad \text{for } z' = 0, 1$$

where $U_i := F_V\left(V_i \mid \boldsymbol{X}_i\right)$ follows a uniform distribution, and $\pi(z'; \boldsymbol{X}_i) = F_V\big(\psi(z'; \boldsymbol{X}_i)\big) = \Pr\left(D_i = 1 \,|\, Z_i = z', \boldsymbol{X}_i\right)$ is the mediator propensity score. U_i are the unobserved mediator take-up costs. Note the maintained assumption that treatment Z_i is ignorable conditional on $\boldsymbol{X}_i$ implies $Z_i \perp\!\!\!\perp U_i$ conditional on $\boldsymbol{X}_i$.

This selection model setup is equivalent to the monotonicity condition, and is importing a well-known equivalence result from the IV literature to the CM setting. The main conceptual difference is not assuming Z_i is a valid instrument for identifying the D_i on Y_i effect among compliers; it is using the selection model representation to correct for selection bias. See Appendix A.5 for a validation of the general Vytlacil (2002) equivalence result in a CM setting, with conditioning covariates $\boldsymbol{X}_i$.

Assumption CF–2. Selection on mediator benefits.

$$\mathrm{Cov}\left(U_i,\, U_{0,i}\right),\ \mathrm{Cov}\left(U_i,\, U_{1,i}\right) \neq 0.$$

Assumption CF–2 is stating that unobserved selection in mediator take-up (U_i) informs second-stage confounding, when refusing or taking the mediator ($U_{0,i}$ and $U_{1,i}$). If there is unobserved confounding in Y_i, then it can be measured in D_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 $V_i = U_{C,i} - \big(U_{1,i} - U_{0,i}\big)$. Individuals could be making decisions based on other outcomes, but as long as mediator 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 $\boldsymbol{X}_i$ has at least two entries; denote $\boldsymbol{X}_i^{\mathrm{IV}}$ as one entry in the vector, and $\boldsymbol{X}_i^{-}$ as the remaining.

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

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

Assumption CF–3 is requiring at least one control variable guides selection-into-D_i — an IV. 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 IV first-stage). The exclusion restriction is untestable, and must be guided by domain-specific knowledge; IV first-stage strength is testable, and must be justified with data by methods common in the IV literature.

This assumption identifies the mediator propensity score separately from the direct and indirect effects, 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 this assumption). The most compelling example of a mediator IV is using data on the cost of mediator take-up as a first-stage IV, if it varies between individuals for unrelated reasons and is strong in explaining mediator take-up.

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

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

where λ_0, λ_1 are the Control Functions (CFs), ρ_0, ρ_1 are linear parameters, and mediator propensity score $\pi(z'; \boldsymbol{X}_i)$ is separately identified in the first-stage (3)*. Proof: see Appendix A.6.*

Again, this set-up required no linearity assumptions, and treatment effects vary, because Z_i, D_i are categorical and $\beta, \gamma, \delta, \varphi(\boldsymbol{X}_i)$ vary with $\boldsymbol{X}_i$. The CFs are functions which measure unobserved mediator gains, for those with unobserved mediator costs above or below a propensity score value. Following the IV notation of Kline & Walters (2019), put $\mu_V = \mathbb{E}\left[F_V^{-1}\left(U_i \mid \boldsymbol{X}_i\right)\right]$, to give the following representation for the CFs:

$$\begin{aligned}
\lambda_0\big(p'\big) &= \mathbb{E}\left[F_V^{-1}\left(U_i \mid \boldsymbol{X}_i\right) - \mu_V \,\middle|\, p' < U_i\right], \\
\lambda_1\big(p'\big) &= \mathbb{E}\left[F_V^{-1}\left(U_i \mid \boldsymbol{X}_i\right) - \mu_V \,\middle|\, U_i \leq p'\right] = -\lambda_0\big(p'\big)\left(\frac{1-p'}{p'}\right), \text{ for } p' \in (0,1).
\end{aligned}$$

If we are using the canonical Heckman selection model, we assume the error term follows a normal distribution, so that λ_0, λ_1 are the inverse Mills ratio. Alternatively, λ_0, λ_1 could have other definitions following the assumed distribution of the error terms (see e.g, Wooldridge 2015). If we do not know what distribution class the errors follow, then λ_0, λ_1 can be estimated separately with semi-parametric methods to avoid relying on parametric assumptions.

Theorem CF. If assumptions CF–1, CF–2, CF–3 hold, the ADE and AIE are identified as a function of the parameters in Proposition 2.

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

where $\Gamma\left(p, p'\right) = \mathbb{E}\left[F_V^{-1}\left(U_i \mid \boldsymbol{X}_i\right) - \mu_V \,\middle|\, p < U_i \leq p'\right] = \frac{p'\lambda_1(p') - p\lambda_1(p)}{p' - p}$ is the average unobserved net gains for those with unobserved costs between $p < p'$,[11] and $\overline{\pi} = \pi(1; \boldsymbol{X}_i) - \pi(0; \boldsymbol{X}_i)$ is the mediator complier score. Proof: see Appendix A.7.

This theorem provides a solution to the identification problem for CM effects when facing selection; 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

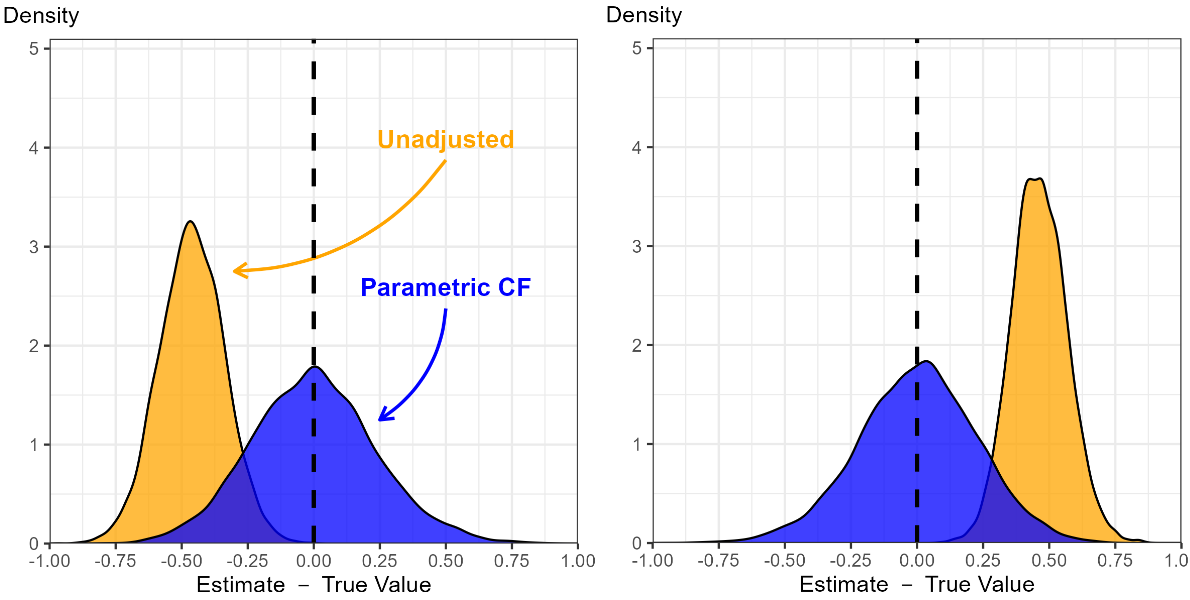
[11] The complier adjustment term was first written in this manner by Kline & Walters (2019) for an IV setting.

mediator D_i (mediator compliers). The CFs allow us to measure both selection bias and complier differences, and thus purge persistent bias in identifying CM effects.

This identification strategy is essentially a Marginal Treatment Effect approach (MTE, Björklund & Moffitt 1987, Heckman & Vytlacil 2005) applied to a CM setting. Just as the local IV approach uses variation in instruments to identify MTEs across the distribution of unobserved treatment take-up costs, this CF approach identifies CM effects across the distribution of unobserved mediator take-up costs. This connection to MTEs provides a conceptual bridge between the literature on structural IV for causal effects and CM.

Figure 4: The CF Adjustment Addresses Persistent Bias in Conventional CM Estimates.

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



Note: These figures show the empirical density of point estimates minus the true average effect, for 10,000 different datasets generated from a Roy model with normally distributed error terms (with both correlation and heteroscedasticity, further described in Subsection 5.3). 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.

The ideal instrument $\boldsymbol{X}_i^{\text{IV}}$ for identification is continuous, and varies $\pi(z'; \boldsymbol{X}_i)$ between 0 and 1 for every possible value of $z', \boldsymbol{X}_i^-$ (identification at infinity). In practice, it is unlikely to find IV(s) that satisfy this condition. In this case, the Brinch, Mogstad & Wiswall (2017) restricted approach can be used — even with a categorical instrument and no control variables. This amounts to assuming a limited specification for the respective CFs, limiting the number of parameters used to approximate λ_0, λ_1 to the number of discrete values that $\pi(z'; \boldsymbol{X}_i)$ takes minus one. E.g., if there are no control variables and $\boldsymbol{X}_i^{\text{IV}}$ is binary, then λ_0, λ_1 can only be identified up to 3 parameters each.[12] Ultimately, this changes little to the identification strategy, and little to the estimation.

In a simulation with Roy selection-into-mediator based on unobserved error terms, the CF adjustment pushes conventional CM estimates back to the true value. Figure 4 shows how a CF adjustment corrects unadjusted CM effect estimates.

5 Control Function (CF) Estimation of CM Effects

A conventional approach to estimating CM effects involves a two-stage approach to estimating the ADE and the AIE: the first-stage (Z_i on D_i), and the second-stage (Z_i, D_i on Y_i). A CF approach is a simple and intuitive addition to this approach: including the CF terms λ_0, λ_1 in the second-stage regression to address selection-into-mediator.

This section presents two practical estimation strategies. First, I demonstrate how to estimate CM effects with an assumed distribution of error terms, focusing on the Heckman selection model as the leading case. Second, I consider a more flexible semi-parametric approach that avoids distributional assumptions — at the cost of semi-parametrically estimating the corresponding CFs. While both methods effectively address the selection bias issues detailed in previous sections, they differ in their implementation complexity, efficiency, and underlying assumptions.

5.1 Parametric CF

A parametric CF solves the identification problem by assuming a distribution for the unobserved error terms in the first-stage selection model, and modelling selection based on this distribution. The Heckman selection model is the most pertinent example, assuming the normal distribution for unobserved errors (Heckman 1979). A parametric CF using other distributions works in exactly the same manner, replacing the relevant density functions for an alternative distribution as needed. As such, this section focuses exclusively on the Heckman selection model. This estimation approach is equivalent to that in the parametric selection model approach to MTEs, in Björklund & Moffitt (1987).

The Heckman selection model assumes unobserved errors V_i follow a normal distribution,

[12] The value of 3 comes from the cases that $Z_i, \boldsymbol{X}_i^{\mathrm{IV}}$ each could take 2 values, so $\pi(z'; \boldsymbol{X}_i)$ has 4 possible values, giving the semi-parametric identification (and estimation) of each CF only 3 degrees of freedom to work with. If the CFs are instead assumed to have a known distribution (i.e., parametric CF), then those concerns do not matter.

so estimates the first-stage using a probit model.

$$\Pr\left(D_i = 1 \,|\, Z_i, \boldsymbol{X}_i\right) = \Phi\big(\theta + \overline{\pi} Z_i + \boldsymbol{\zeta}' \boldsymbol{X}_i\big),$$

where $\Phi(.)$ is the cumulative density function for the standard normal distribution, and $\theta, \overline{\pi}, \boldsymbol{\zeta}$ are parameters estimated with maximum likelihood. In the parametric case, an excluded instrument ($\boldsymbol{X}_i^{\mathrm{IV}}$) is not technically necessary in the first-stage equation — though not including one exposes the method to indeterminacy if the errors are not normally distributed. Thus, it is best practice to use this method with access to an instrument.

From this probit first-stage, construct the inverse Mills ratio terms to serve as the CFs. These terms capture the correlation between unobserved factors influencing both mediator selection and outcomes, when the errors are normally distributed.

$$\lambda_0(p') = \frac{\phi(-\Phi^{-1}(p'))}{\Phi(-\Phi^{-1}(p'))}, \quad \lambda_1(p') = \frac{\phi(\Phi^{-1}(p'))}{\Phi(\Phi^{-1}(p'))}, \quad \text{for } p' \in (0,1)$$

where $\phi(.)$ is the probability density function for the standard normal distribution.

Lastly, the second-stage is estimated with OLS, including the CFs with plug in estimates of the mediator propensity score, and $\boldsymbol{\varphi}'$ a linear approximation of nuisance function $\varphi(.)$.

$$\begin{aligned} \mathbb{E}\left[Y_i \,|\, Z_i, D_i, \boldsymbol{X}_i\right] \;&=\; \alpha + \beta D_i + \gamma Z_i + \delta Z_i D_i + \boldsymbol{\varphi}' \boldsymbol{X}_i^{-} \\ &\qquad + \rho_0 (1 - D_i) \lambda_0\big(\widehat{\pi}(Z_i; \boldsymbol{X}_i)\big) + \rho_1 D_i \lambda_1\big(\widehat{\pi}(Z_i; \boldsymbol{X}_i)\big) + \varepsilon_i, \end{aligned}$$

where $\widehat{\pi}\big(z'; \boldsymbol{X}_i\big)$ are the predictions from the probit first-stage.

The resulting ADE and AIE estimates are composed from sample estimates of the terms in Theorem CF,

$$\widehat{\text{ADE}} = \widehat{\gamma} + \widehat{\delta}\,\overline{D}, \quad \widehat{\text{AIE}} = \widehat{\overline{\pi}}\,\Big(\widehat{\beta} + \widehat{\delta}\,\overline{Z} + \big(\widehat{\rho}_1 - \widehat{\rho}_0\big) \frac{1}{N} \sum_{i=1}^{N} \Gamma\big(\widehat{\pi}(0; \boldsymbol{X}_i), \widehat{\pi}(1; \boldsymbol{X}_i)\big)\Big)$$

where $\overline{D} = \frac{1}{N} \sum_{i=1}^{N} D_i$, $\overline{Z} = \frac{1}{N} \sum_{i=1}^{N} Z_i$, $\widehat{\overline{\pi}}$ is the estimate of the mean compliance rate, and $\frac{1}{N} \sum_{i=1}^{N} \Gamma(.,.)$ is the average of the complier adjustment term as a function of λ_1 with $\widehat{\pi}\big(0; \boldsymbol{X}_i\big), \widehat{\pi}\big(1; \boldsymbol{X}_i\big)$ values plugged in.

The standard errors for estimates can be computed using the delta method. Specifically, accounting for both the sampling variability in the first-stage estimates of the mediator propensity score as well as the second-stage sampling variability. This approach yields $\sqrt{n}$-consistent estimates when the underlying error terms follow a bivariate normal distribution — i.e., when $\pi(Z_i; \boldsymbol{X}_i)$ is correctly modelled by the probit first-stage. Errors can also be estimated by the bootstrap, by including estimation of both the first and second-stage within each bootstrap iteration.

In practice, a parametric CF approach is simple to implement using standard statistical packages. The key advantage is computational simplicity and efficiency, particularly in moderate-sized samples. However, this comes at the cost of strong distributional assumptions. For example, if the error terms deviate substantially from joint normality, the estimates may be biased.[13]

5.2 Semi-parametric CF

For settings where researchers are not comfortable specifying a specific distribution for the error terms, a semi-parametric CF will nonetheless consistently estimate CM effects. This method maintains the same identification strategy but avoids assuming a specific error distribution. This estimation approach is similar to the modern semi-parametric approach to estimating MTEs, for example seen in Brinch et al. (2017), Heckman & Vytlacil (2007).

The semi-parametric approach begins with flexible estimation of the first-stage, estimating the mediator propensity score,

$$\Pr\left(D_i = 1 \,|\, Z_i, \boldsymbol{X}_i\right) = \pi\left(Z_i; \boldsymbol{X}_i\right),$$

where $\boldsymbol{X}_i$ must include the instrument(s) $\boldsymbol{X}_i^{\text{IV}}$. This can be estimated using flexible methods, as long as the first-stage is estimated $\sqrt{n}$-consistently.[14] An attractive option is the Klein & Spady (1993) semi-parametric binary response model, which avoids relying on an assumed distribution of first-stage errors though requires a linear specification. If it is important to avoid a linear specification, then a probability forest avoids linearity assumptions (Athey, Tibshirani & Wager 2019) — though is best used for cases with many columns in the $\boldsymbol{X}_i$ variables.

The second-stage is estimated with semi-parametric methods. Consider the subsamples of mediator refusers and takers separately,

$$\mathbb{E}\left[Y_i \,|\, Z_i, D_i = 0, \boldsymbol{X}_i\right] = \alpha + \gamma Z_i + \varphi\big(\boldsymbol{X}_i^-\big) + \rho_0 \lambda_0\big(\pi(Z_i; \boldsymbol{X}_i)\big),$$
$$\mathbb{E}\left[Y_i \,|\, Z_i, D_i = 1, \boldsymbol{X}_i\right] = (\alpha + \beta) + (\gamma + \delta) Z_i + \varphi\big(\boldsymbol{X}_i^-\big) + \rho_1 \lambda_1\big(\pi(Z_i; \boldsymbol{X}_i)\big).$$

The separated subsamples can be estimated, each individually, with semi-parametric methods. The linear parameters (including a linear approximation $\boldsymbol{\varphi}'$ of nuisance function $\varphi(.)$)[15] can

[13] While this concern is immaterial in an IV setting estimating the LATE (Kline & Walters 2019), it is pertinent in this setting as the CF extrapolates from IV compliers to mediator compliers.

[14] If an estimate of the first-stage that is not $\sqrt{n}$-consistent is used (e.g., a modern machine learning estimator), then the resulting second-stage estimate will not be $\sqrt{n}$-consistent.

[15] Appropriate interactions between Z_i, D_i and $\boldsymbol{X}_i$ can also flexibly control for $\boldsymbol{X}_i$, again avoiding linearity

be estimated with OLS, while $\rho_0 \lambda_0$ and $\rho_1 \lambda_1$ take a flexible semi-parametric specification with first-stage estimates $\widehat{\pi}(Z_i; \boldsymbol{X}_i)$ plugged in. An attractive option is a series estimator, such as a spline specification, as this estimates the function without assuming a functional form but maintains $\sqrt{n}$-consistency.

The ADE is estimated by this approach as follows. Take $\widehat{\gamma}$, the $D_i = 0$ subsample estimate of γ, and $(\widehat{\gamma + \delta})$, the $D_i = 1$ subsample estimate of $(\gamma + \delta)$, to give

$$\widehat{\text{ADE}}^{\text{CF}} = (1 - \overline{D})\, \widehat{\gamma} + \overline{D}\, (\widehat{\gamma + \delta}).$$

The AIE is less simple, for two reasons that differ from the parametric CF setting. First, the intercepts for each subsample, α and $(\alpha + \beta)$, are not separately identified from the CFs if the λ_0, λ_1 functions are flexibly estimated. Second, a semi-parametric specification for the CFs mean ρ_0 and λ_0 are no longer separately identified from each other (and same for ρ_1, λ_1). As such, it is not possible to directly use $\widehat{\lambda}_0, \widehat{\lambda}_1$ in estimating the complier adjustment term (as is done in the parametric case).

These problems can be avoided by estimating the AIE using its relation to the ATE. Write $\widehat{\text{ATE}}$ for the point-estimate of the ATE, and $\widehat{\delta} = (\widehat{\gamma + \delta}) - \widehat{\gamma}$ for the point estimate of δ, to give the following estimator,

$$\widehat{\text{AIE}}^{\text{CF}} = \widehat{\text{ATE}} - (1 - \overline{Z}) \left(\frac{1}{N} \sum_{i=1}^{N} \widehat{\gamma} + \widehat{\delta}\, \widehat{\pi}(1; \boldsymbol{X}_i) \right) - \overline{Z} \left(\frac{1}{N} \sum_{i=1}^{N} \widehat{\gamma} + \widehat{\delta}\, \widehat{\pi}(0; \boldsymbol{X}_i) \right),$$

where $\frac{1}{N} \sum_{i=1}^{N} \widehat{\gamma} + \widehat{\delta}\, \widehat{\pi}(0; \boldsymbol{X}_i)$ estimates the ADE conditional on $Z_i = 0$, $\mathbb{E}\left[\gamma + \delta D_i(0)\right]$, and $\frac{1}{N} \sum_{i=1}^{N} \widehat{\gamma} + \widehat{\delta}\, \widehat{\pi}(1; \boldsymbol{X}_i)$ estimates the ADE conditional on $Z_i = 1$, $\mathbb{E}\left[\gamma + \delta D_i(1)\right]$. Appendix A.8 describes the reasoning for this estimator of the AIE, relative to estimates of the ATE and ADE, in further detail.

This semi-parametric approach achieves valid estimation of the CM effects, without specifying the distribution behind unobserved error terms, and achieves desirable properties as long as the first-stage correctly estimates the mediator propensity score, and the structural assumptions hold true. The standard errors for estimates can again be computed using the delta method, or estimated by the bootstrap — again, across both first and second-stages within each bootstrap iteration. Note that relying on propensity score estimation requires assumptions that can be found wanting in real-world settings; a common support condition for the mediator is required, and a semi-/non-parametric first-stage may become cumbersome if there are many control variables or many rows of data.

assumptions.

5.3 Simulation Evidence

The following simulation gives an example to show how these methods work 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 $i = 1, \dots, N$, with $N = 1,000$ for this simulation.

$$Z_i \sim \text{Binom}\,(0.5)\,, \quad \boldsymbol{X}_i^- \sim N(4,1), \quad \boldsymbol{X}_i^{\text{IV}} \sim \text{Uniform}\,(-1,1)\,, \quad (U_{0,i}, U_{1,i}, U_{C,i}) \sim N\left(\boldsymbol{0}, \boldsymbol{\Sigma}\right)$$

$\boldsymbol{\Sigma}$ is the matrix of parameters which controls the level of confounding from unobserved costs and benefits.[16]

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

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

Following Subsection 3.1, these data have the following first and second-stage equations:

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

Treatment Z_i has a causal effect on outcome Y_i, and it operates partially through mediator D_i. Outcome mean $\mu_{D_i}\left(Z_i; \boldsymbol{X}_i\right)$ contains an interaction term, $Z_i D_i$, so while Z_i, D_i have constant partial effects, the ATE depends on how many i choose to take the mediator so there is treatment effect heterogeneity.

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 conventional approach to CM does not give an estimate for how much of the ATE goes through mediator D, but is contaminated by selection bias thanks to the unobserved error terms.

I simulate this data generating process 10,000 times, using $\boldsymbol{\Sigma} = \left(\begin{smallmatrix} 1 & 0.75 & 0 \\ 0.75 & 2.25 & 0 \\ 0 & 0 & 0.25 \end{smallmatrix}\right)$,[17] and estimate CM effects with conventional CM methods (two-stage OLS) and the introduced CF

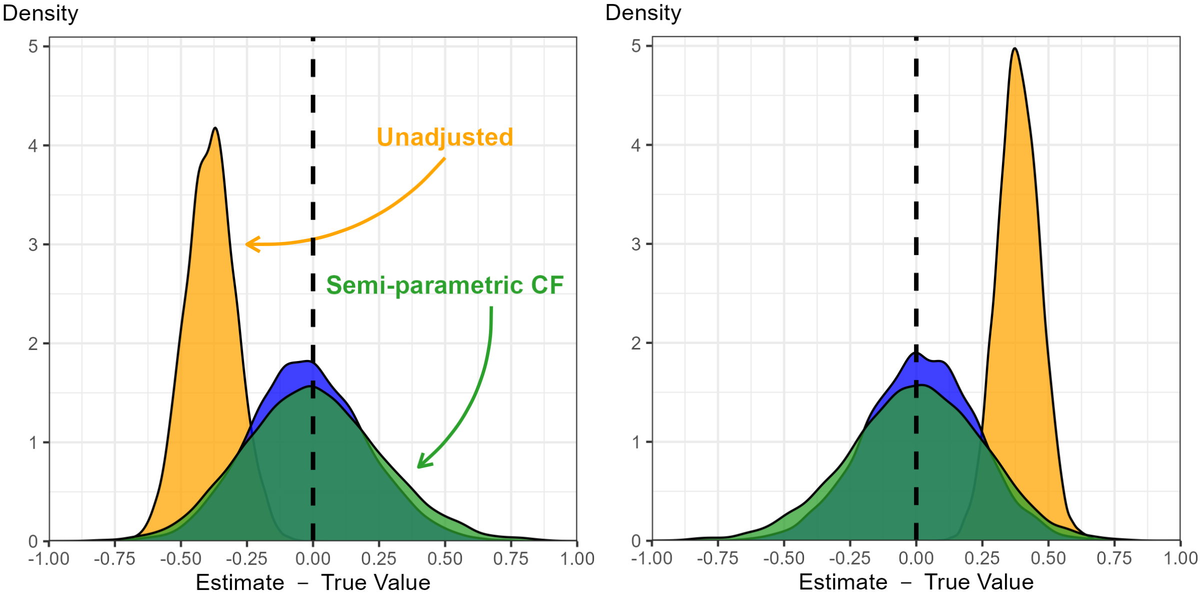
[16] The correlation and relative standard deviations for $U_{0,i}, U_{1,i}$ affect how large selection bias in conventional CM estimates; correlation for these with unobserved costs $U_{C,i}$ does not particularly matter, though increased variance in unobserved costs makes estimates less precise for both OLS and CF methods.

[17] This choice of parameters has Var $\left(U_{0,i}\right) = 1$, Var $\left(U_{1,i}\right) = 2.25$, Corr$\left(U_{0,i}, U_{1,i}\right) = 0.5$ so that unobserved errors meaningfully confound conventional CM methods, with notable heteroscedasticity. Unobserved costs are uncorrelated with $U_{0,i}, U_{1,i}$ (although non-zero correlation would not meaningfully change the results), and Var $\left(U_{C,i}\right) = 0.25$ maintains uncertainty in unobserved costs.

methods. In this simulation $\Pr(D_i = 1) = 0.379$, and 65.77% of the sample are mediator compliers (for whom $D_i(0) = 0$ and $D_i(1) = 1$). This gives an ATE value of 2.60, ADE 1.38, and AIE 1.22, respectively.[18]

Figure 5: Simulated Distribution of CM Effect Estimates, Semi-parametric versus OLS, Relative to True Value.

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



Note: These figures show the empirical density of point estimates minus the true average effect, for 10,000 different datasets generated from a Roy model with correlated uniformly distributed error terms. 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 green estimates with a two-stage semi-parametric CF.

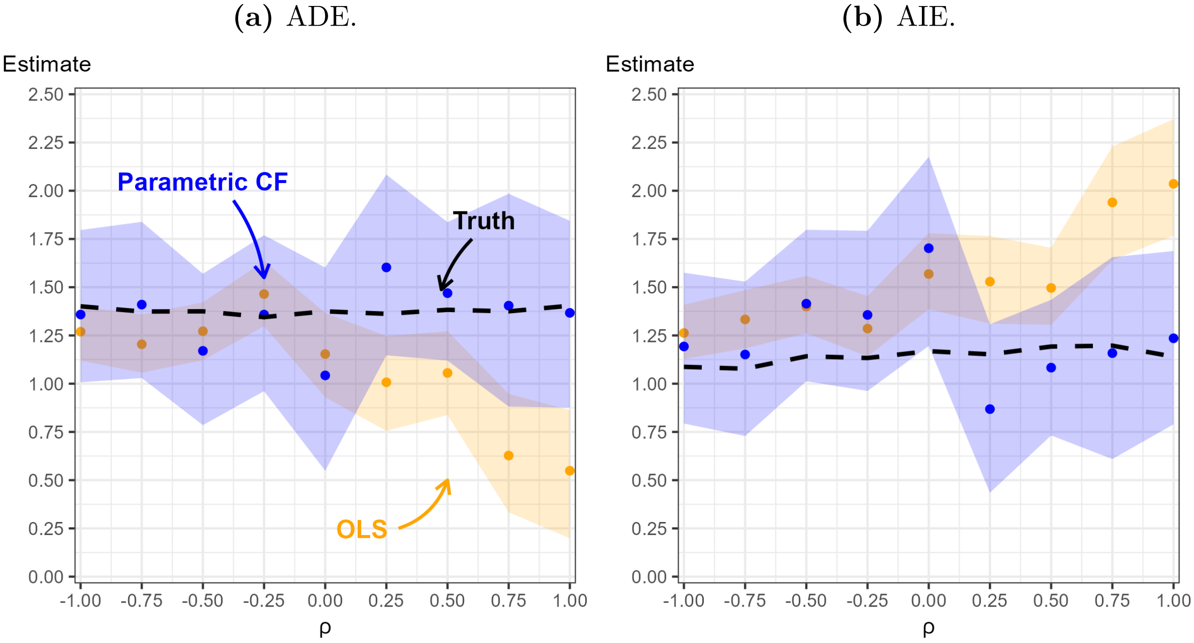
Figure 4 shows how these estimates perform, with a parametric CF approach, relative to the true value. The OLS estimates' distribution do not overlap the true values for any standard level of significance; the distance between the OLS estimates and the true values are the underlying bias terms derived in Theorem 1. The parametric CF approach perfectly reproduces the true values, as the probit first-stage correctly models the normally distributed error terms. The semi-parametric approach (not shown in Figure 4) performs similarly, with a wider distribution; this is to be expected comparing a correctly specified parametric model with a semi-parametric one.

The parametric CF may not be appropriate in setting with non-normal error terms. I simulated the same data again, but transform $U_{0,i}, U_{1,i}$ to be correlated uniform errors (with the same standard deviations as previously). Figure 5 shows the resulting distribution of

[18] Note that ATE = ADE + AIE in this setting. $\Pr(Z_i = 1) = 0.5$ ensures this equality, but it is not guaranteed in general. See Appendix A.8.

point-estimates, relative to the truth, for the parametric and semi-parametric approaches. The parametric CF is slightly off target, showing persistent bias from incorrectly specifying the error term distribution. The semi-parametric approach is centred exactly around the truth, with a slightly high variance (as is expected).

Figure 6: CF Adjusted Estimates Work with Different Error Term Parameters.



Note: These figures show the OLS and CF point estimates of the ADE and AIE, for $N = 1,000$ sample size, varying $\text{Corr}\big(U_{0,i}, U_{1,i}\big)$ values with $\text{Var}\left(U_{0,i}\right) = 1, \text{Var}\left(U_{1,i}\right) = 1.5$ fixed. The black dashed line is the true value, coloured points are points estimates for the respective data generated, and shaded regions are the 95% confidence intervals from 1,000 bootstraps each. Orange represents OLS estimates, blue the CF approach.

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 $\text{Corr}\big(U_{0,i}, U_{1,i}\big) \in [-1, 1]$ and $\text{Var}\left(U_{0,i}\right), \text{Var}\left(U_{1,i}\right) \geq 0$. The true AIE values vary, because $D_i(Z_i)$ compliers have higher average values of $U_{1,i} - U_{0,i}$ as $\text{Corr}\big(U_{0,i}, U_{1,i}\big)$ increases. Figure 6 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 generated datasets, as in Figure 5). The CF approach improves on OLS estimates by correcting for bias, with confidence regions overlapping the true values.[19] This correction did not come for free: the standard errors are significantly greater in a CF approach than OLS. In this manner, this simulation shows the pros and cons of using the CF approach to estimating CM effects in

[19] In the appendix, Figure A1 shows the same simulation while varying $\text{Var}\left(U_{1,i}\right)$, with fixed $\text{Var}\left(U_{0,i}\right) = 1, \text{Corr}\big(U_{0,i}, U_{1,i}\big) = 0.5$. The conclusion is the same as for varying the correlation coefficient, ρ, in Figure 6.

practice.

6 CM in the Oregon Health Insurance Experiment

Apply the parametric + semi-parametric CM to the Oregon data.

7 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 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 to the growing double robustness literature on CM (Farbmacher, Huber, Lafférs, Langen & Spindler 2022, Bia, Huber & Lafférs 2024).

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

This section is for supplementary information, and validation of presented propositions and theorems. It is not meant for publication.

Any comments or suggestions may be sent to me at seh325@cornell.edu, or raised as an issue on the Github project, https://github.com/shoganhennessy/mediation-natural-experiment.

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 $z' = 0, 1$:

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

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

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.

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

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

A.2 Bias in Causal Mediation (CM) Estimands

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

A.2.1 Bias in the Average Direct Effect (ADE)

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 $\boldsymbol{X}_i$ and $d' \in \{0,1\}$.

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

And so,

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

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.

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

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 the Average Indirect Effect (AIE)

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 $\boldsymbol{X}_i$.

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

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

Let $z' \in \{0, 1\}$. Again, note the mean outcomes in terms of average potential outcomes,

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

Compose the selection bias term, 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 potential outcomes between the groups for whom $D_i = 1$ 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 $D_i(0) = 0, D_i(1) = 1$.

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

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

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

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

Collect everything together, as follows.

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

The proof is finally achieved by multiplying by the complier score, $\Pr\left(D_i(0) = 0, D_i(1) = 1 \,|\, \boldsymbol{X}_i\right) = \mathbb{E}\left[D_i \,|\, Z_i = 1, \boldsymbol{X}_i\right] - \mathbb{E}\left[D_i \,|\, Z_i = 0, \boldsymbol{X}_i\right]$, 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:

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

$U_{0,i}, U_{1,i}$ 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 $U_{0,i}, U_{1,i} \perp\!\!\!\perp Z_i$.

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

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

Since the ignorability assumption gives $\mathbb{E}\left[Z_i \eta_{z',i} \,|\, \boldsymbol{X}_i\right] = \mathbb{E}\left[Z_i \,|\, \boldsymbol{X}_i\right] \mathbb{E}\left[\eta_{z',i} \,|\, \boldsymbol{X}_i\right] = 0$, for each $z' = 0, 1$. By the same argument 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 for the ATE.

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

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

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

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

With the following definitions:

(a) $\alpha = \mathbb{E}\left[\mu_0(0; \boldsymbol{X}_i)\right]$ and $\varphi(\boldsymbol{X}_i) = \mu_0(0; \boldsymbol{X}_i) - \alpha$ 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) $\delta = \mu_1(1; \boldsymbol{X}_i) - \mu_0(1; \boldsymbol{X}_i) - (\mu_1(0; \boldsymbol{X}_i) - \mu_0(0; \boldsymbol{X}_i))$ 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:

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

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

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

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 $\boldsymbol{X}_i$ 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.

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

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

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

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

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 $\widetilde{U}_i$, too.

A.4 Roy Model and Sequential Ignorability

Proof of Proposition 1.

Suppose Z_i is ignorable, and selection-into-D_i follows a Roy model, with the definitions in Section 3. If selection-into-D_i is degenerate on $U_{0,i}, U_{1,i}$:

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

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 $z', d' = 0, 1$

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

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 Monotonicity $\implies$ Selection Model, in a CM Setting.

Proof that (conditional) monotonicity implies a selection model representation in a CM setting. This proof is an applied example of the Vytlacil (2002) equivalence result, now including conditioning covariates $\boldsymbol{X}_i$, and is presented merely as a validation exercise.

Assume condition monotonicity CF–1 holds, for any treatment values $z < z'$ and any covariate value $\boldsymbol{X}_i = \boldsymbol{x}$.

$$\Pr\left(D_i(z') \geq D_i(z) \,|\, \boldsymbol{x}\right) = 1.$$

For each value of $\boldsymbol{X}_i = \boldsymbol{x}$ and any treatment values $z < z'$, we first define:

- $\mathcal{A} = \{i : D_i(z) = D_i(z') = 1\}$, always-mediators
- $\mathcal{N} = \{i : D_i(z) = D_i(z') = 0\}$, never-mediators
- $\mathcal{C} = \{i : D_i(z) = 0, D_i(z') = 1\}$, mediator-compliers.

For any mediator complier $i \in \mathcal{C}$, partition the set as follows.

- $\mathcal{Z}_1(i) = \{z' : D_i(z') = 1\}$, treatment values where i takes the mediator
- $\mathcal{Z}_0(i) = \{z' : D_i(z') = 0\}$, treatment values where i doesn't take the mediator.

Note that having binary $Z_i = 0, 1$ reduces this to the simple case of $\mathcal{Z}_0(i) = \{0\}$, and $\mathcal{Z}_1(i) = \{1\}$. The equivalence result holds for continuous values of Z_i, so continue with the more general $\mathcal{Z}_0(i), \mathcal{Z}_1(i)$ notation.

By monotonicity, we have

$$\sup_{z' \in \mathcal{Z}_0(i)} \pi(z'; \boldsymbol{x}) \leq \inf_{z' \in \mathcal{Z}_1(i)} \pi(z'; \boldsymbol{x}), \quad \text{for any } i \in \mathcal{C}$$

where $\pi(z'; \boldsymbol{x}) = \Pr\left(D_i = 1 \,|\, Z_i = z', \boldsymbol{X}_i = \boldsymbol{x}\right)$ is the mediator propensity score. A simple proof by contradiction verifies this statement (Vytlacil 2002, Lemma 1).

Now we construct V_i as follows:

$$V_i = \begin{cases} 1, & \text{if } i \in \mathcal{N} \\ 0, & \text{if } i \in \mathcal{A} \\ \inf_{z' \in \mathcal{Z}_1(i)} \pi(z'; \boldsymbol{x}), & \text{if } i \in \mathcal{C}. \end{cases}$$

Define $\psi(z'; \boldsymbol{x}) = \pi(z'; \boldsymbol{x})$. Then we can represent $D_i(z')$ as a selection model,

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

We can verify this works:

- For $i \in \mathcal{A}$: $V_i = 0$ and $\psi(z'; \boldsymbol{x}) \geq 0$ for all z', so $D_i(z') = 1$
- For $i \in \mathcal{N}$: $V_i = 1$ and $\psi(z'; \boldsymbol{x}) \leq 1$ for all z', with $\psi(z'; \boldsymbol{x}) < 1$ for $z' \in \mathcal{Z}_0(i)$, so $D_i(z') = 0$ for $z' \in \mathcal{Z}_0(i)$
- For $i \in \mathcal{C}$: $V_i = \inf_{z' \in \mathcal{Z}_1(i)} \pi(z'; \boldsymbol{x})$
  - When $z' \in \mathcal{Z}_1(i)$: $\psi(z'; \boldsymbol{x}) \geq \inf_{z'' \in \mathcal{Z}_1(i)} \pi(z''; \boldsymbol{x}) = V_i$, so $D_i(z') = 1$
  - When $z' \in \mathcal{Z}_0(i)$: $\psi(z'; \boldsymbol{x}) < \inf_{z'' \in \mathcal{Z}_1(i)} \pi(z''; \boldsymbol{x}) = V_i$, so $D_i(z') = 0$.

Therefore, the construction $D_i(z') = \mathbb{1}\{\psi(z'; \boldsymbol{X}_i) \geq V_i\}$ is a valid representation of the selection process under monotonicity.

This selection model can be transformed to one with a uniform distribution, to get the general selection model of Heckman & Vytlacil (2005). Let $F_V\big(.\,\big|\,\boldsymbol{X}_i\big)$ be the conditional cumulative density function of V_i given $\boldsymbol{X}_i$. Define

$$U_i = F_V\left(V_i \mid \boldsymbol{X}_i\right)$$
$$\pi(z'; \boldsymbol{X}_i) = F_V\left(\psi(z'; \boldsymbol{X}_i) \mid \boldsymbol{X}_i\right) = \Pr\left(D_i = 1 \,|\, Z_i = z', \boldsymbol{X}_i\right)$$

We can then equivalently represent the mediator choice as the transformed selection model

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

where $U_i \,|\, \boldsymbol{X}_i \sim \text{Uniform}(0,1)$ by the probability integral transformation.

A.6 Control Function (CF) Identification of the Second-stage

Proof of Proposition 2. This proof relies heavily on the notation and reasoning of Kline & Walters (2019) for an IV setting.

By Assumption CF–1 (mediator monotonicity), selection-into-mediator can be represented as a threshold-crossing selection model.

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

where $U_i = F_V\left(V_i \mid \boldsymbol{X}_i\right)$ follows a uniform distribution on $[0, 1]$, and $\pi(z'; \boldsymbol{X}_i) = \mathbb{E}\left[D_i \,|\, Z_i = z', \boldsymbol{X}_i\right]$ is the mediator propensity score.

The threshold crossing selection model represents individuals who refuse the mediator as follows:

$$D_i = 0 \implies \pi(Z_i; \boldsymbol{X}_i) < U_i$$

Our objective is to determine $\mathbb{E}\left[U_{0,i} \,|\, D_i = 0, Z_i, \boldsymbol{X}_i\right]$, which can then be written as

$$\mathbb{E}\left[U_{0,i} \,|\, \pi(Z_i; \boldsymbol{X}_i) < U_i, Z_i, \boldsymbol{X}_i\right].$$

Since Z_i is ignorable, we have:

$$\mathbb{E}\left[U_{0,i} \,|\, \pi(Z_i; \boldsymbol{X}_i) < U_i, Z_i, \boldsymbol{X}_i\right] = \mathbb{E}\left[U_{0,i} \,|\, \pi(Z_i; \boldsymbol{X}_i) < U_i\right]$$

Assumption CF–2 has $\text{Cov}(U_i, U_{0,i}) \neq 0$. This non-zero covariance implies statistical dependence between the selection error and outcome error. This dependence allows us to represent $U_{0,i}$ using a linear projection. We use $F_V^{-1}\left(U_i \mid \boldsymbol{X}_i\right)$ rather than U_i directly in the projection to allow for flexibility in how the selection error affects outcomes. The linear projection can be written as follows

$$U_{0,i} = \rho_0 \big(F_V^{-1}\left(U_i \mid \boldsymbol{X}_i\right) - \mu_V\big) + \varepsilon_{0,i},$$

where

- $\mu_V = \mathbb{E}\left[F_V^{-1}\left(U_i \mid \boldsymbol{X}_i\right)\right]$ is the mean of $F_V^{-1}\left(U_i \mid \boldsymbol{X}_i\right)$
- $\rho_0 = \frac{\text{Cov}\left(U_{0,i}, F_V^{-1}(U_i \mid \boldsymbol{X}_i)\right)}{\text{Var}\left(F_V^{-1}(U_i \mid \boldsymbol{X}_i)\right)}$ is the projection coefficient
- $\varepsilon_{0,i}$ is a residual with $\mathbb{E}\left[\varepsilon_{0,i} \,\middle|\, F_V^{-1}\left(U_i \mid \boldsymbol{X}_i\right)\right] = 0$.

The coefficient ρ_0 is the slope in the best linear predictor of $U_{0,i}$ given $F_V^{-1}\left(U_i \mid \boldsymbol{X}_i\right)$, and is chosen to ensure that the residual $\varepsilon_{0,i}$ is uncorrelated with $F_V^{-1}\left(U_i \mid \boldsymbol{X}_i\right)$. This property is crucial for the identification strategy, as it isolates the component of U_i that is related to selection-into-D_i.

The non-zero covariance condition in CF–2 ensures $\rho_0 \neq 0$, so is relevant. Since U_i and $F_V^{-1}\left(U_i \mid \boldsymbol{X}_i\right)$ are related by a monotonic transformation (the inverse cumulative density function), the covariance $\text{Cov}(U_i, U_{0,i}) \neq 0$ implies $\text{Cov}(F_V^{-1}\left(U_i \mid \boldsymbol{X}_i\right), U_{0,i}) \neq 0$.

Given the linear projection of $U_{0,i}$ onto $F_V^{-1}\left(U_i \mid \boldsymbol{X}_i\right)$, we can compute the conditional expectation:

$$\mathbb{E}\left[U_{0,i} \,|\, \pi(Z_i; \boldsymbol{X}_i) < U_i\right] = \mathbb{E}\left[\rho_0 \big(F_V^{-1}\left(U_i \mid \boldsymbol{X}_i\right) - \mu_V\big) + \varepsilon_{0,i} \,\middle|\, \pi(Z_i; \boldsymbol{X}_i) < U_i\right]$$

Since $\mathbb{E}\left[\varepsilon_{0,i} \,\middle|\, F_V^{-1}\left(U_i \mid \boldsymbol{X}_i\right)\right] = 0$ by construction, and U_i is a function of $F_V^{-1}\left(U_i \mid \boldsymbol{X}_i\right)$, we have

$$\mathbb{E}\left[\varepsilon_{0,i} \,|\, \pi(Z_i; \boldsymbol{X}_i) < U_i\right] = 0.$$

Therefore:

$$\mathbb{E}\left[U_{0,i} \,|\, \pi(Z_i; \boldsymbol{X}_i) < U_i\right] = \rho_0 \mathbb{E}\left[F_V^{-1}\left(U_i \mid \boldsymbol{X}_i\right) - \mu_V \,\middle|\, \pi(Z_i; \boldsymbol{X}_i) < U_i\right].$$

This gives us the control function representation:

$$\mathbb{E}\left[U_{0,i} \,|\, D_i = 0, Z_i, \boldsymbol{X}_i\right] = \rho_0 \lambda_0\big(\pi(Z_i; \boldsymbol{X}_i)\big)$$

where $\lambda_0\left(p'\right) = \mathbb{E}\left[F_V^{-1}\left(U_i \mid \boldsymbol{X}_i\right) - \mu_V \,\middle|\, p' < U_i\right]$. The control function $\lambda_0\left(p'\right)$ captures the expected value of the transformed selection term, conditional on being above the threshold $p' \in (0,1)$.

The same sequence of steps for mediator takers, $D_i = 1$, gives the other CF:

$$\mathbb{E}\left[U_{1,i} \,|\, D_i = 1, Z_i, \boldsymbol{X}_i\right] = \rho_1 \lambda_1\big(\pi(Z_i; \boldsymbol{X}_i)\big),$$

where $\lambda_1\left(p'\right) = \mathbb{E}\left[F_V^{-1}\left(U_i \mid \boldsymbol{X}_i\right) - \mu_V \,\middle|\, U_i \leq p'\right]$ for $p' \in (0,1)$, and $\rho_1 = \frac{\mathrm{Cov}\left(U_{1,i}, F_V^{-1}(U_i|\boldsymbol{X}_i)\right)}{\mathrm{Var}\left(F_V^{-1}(U_i|\boldsymbol{X}_i)\right)}$ is the corresponding projection coefficient.

The relationship between $\lambda_0(p')$ and $\lambda_1(p')$ can be derived as:

$$\lambda_1\left(p'\right) = -\lambda_0\left(p'\right)\left(\frac{1-p'}{p'}\right), \text{ for } p' \in (0,1).$$

This relationship ensures consistency in the CF approach across the $D_i = 0$ and $D_i = 1$ groups (Kline & Walters 2019).

Assumption CF–3 (mediator take-up cost instrument $\boldsymbol{X}_i^{\mathrm{IV}}$) ensures identification of the propensity score function $\pi(z'; \boldsymbol{X}_i)$ in the first stage by providing valid instrumental variation. This variation allows us to identify the propensity score, and consequently the control functions λ_0 and λ_1.

Combining all elements, the conditional expectation of Y_i given $Z_i, D_i, \boldsymbol{X}_i$ is

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

Substitute the CFs,

$$\begin{aligned}&(1 - D_i)\mathbb{E}\left[U_{0,i} \,|\, Z_i, D_i = 0, \boldsymbol{X}_i\right] + D_i \mathbb{E}\left[U_{1,i} \,|\, Z_i, D_i = 1, \boldsymbol{X}_i\right] \\ &= \; (1 - D_i)\rho_0 \lambda_0\big(\pi(Z_i; \boldsymbol{X}_i)\big) + D_i \rho_1 \lambda_1\big(\pi(Z_i; \boldsymbol{X}_i)\big).\end{aligned}$$

This gives the final result,

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

All parameters — $\alpha, \beta, \gamma, \delta, \varphi(.), \rho_0, \rho_1$ — are identified once we control for selection bias through the CFs λ_0, λ_1, with $\pi(z'; \boldsymbol{X}_i)$ identified separately in the first-stage. λ_0, λ_1 can be assumed to be certain functions (say, the inverse Mills ratio in Heckman 1979), or treated as non-parametric parameters to be estimated — at cost of the constant and ρ_0, ρ_1 no longer being separately identified from λ_0, λ_1, see Appendix A.8.

A.7 Control Function (CF) Identification of the ADE and AIE

Proof of Theorem CF.

Assume CF–1, CF–2, CF–3 hold. Then Proposition 2 has $\alpha, \beta, \gamma, \delta, \varphi(.), \rho_0, \rho_1$ identified in a regression. The following composes the ADE and AIE from these parameters.

For the ADE,

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

Identification is similar for the AIE, but also involves the complier adjustment term.

$$\begin{aligned}(\rho_1 - \rho_0)\,\Gamma\big(\pi(0; \boldsymbol{X}_i),\, \pi(1; \boldsymbol{X}_i)\big) &= (\rho_1 - \rho_0)\,\frac{\pi(1; \boldsymbol{X}_i)\lambda_1(\pi(1; \boldsymbol{X}_i)) - \pi(0; \boldsymbol{X}_i)\lambda_1(\pi(0; \boldsymbol{X}_i))}{\pi(1; \boldsymbol{X}_i) - \pi(0; \boldsymbol{X}_i)} \\ &= (\rho_1 - \rho_0)\,\mathbb{E}\left[F_V^{-1}(U_i|\boldsymbol{X}_i) - \mu_V \,\middle|\, \pi(0; \boldsymbol{X}_i) < U_i \leq \pi(1; \boldsymbol{X}_i), \boldsymbol{X}_i\right] \\ &= (\rho_1 - \rho_0)\,\mathbb{E}\left[F_V^{-1}(U_i|\boldsymbol{X}_i) - \mu_V \,\middle|\, D_i(0) = 0, D_i(1) = 1, \boldsymbol{X}_i\right] \\ &= \mathbb{E}\left[\rho_1\big(F_V^{-1}(U_i|\boldsymbol{X}_i) - \mu_V\big) \,\middle|\, D_i(0) = 0, D_i(1) = 1, \boldsymbol{X}_i\right] \\ &\quad - \mathbb{E}\left[\rho_0\big(F_V^{-1}(U_i|\boldsymbol{X}_i) - \mu_V\big) \,\middle|\, D_i(0) = 0, D_i(1) = 1, \boldsymbol{X}_i\right] \\ &= \mathbb{E}\left[U_{1,i} - U_{0,i} \,|\, D_i(0) = 0, D_i(1) = 1, \boldsymbol{X}_i\right].\end{aligned}$$

This complier adjustment was first presented for an IV setting by Kline & Walters (2019).

Collecting for the AIE,

$$
\begin{aligned}
&\mathbb{E}\left[\overline{\pi}\left(\beta + \delta Z_i + (\rho_1 - \rho_0)\Gamma\big(\pi(0; \boldsymbol{X}_i),\, \pi(1; \boldsymbol{X}_i)\big)\right)\right] \\
&= \mathbb{E}\left[\overline{\pi}\left(\Big(\mu_1(0; \boldsymbol{X}_i) - \mu_0(0; \boldsymbol{X}_i)\Big) + Z_i\Big(\mu_1(1; \boldsymbol{X}_i) - \mu_0(1; \boldsymbol{X}_i) - \big(\mu_1(0; \boldsymbol{X}_i) - \mu_0(0; \boldsymbol{X}_i)\big)\Big)\right)\right] \\
&\quad + \mathbb{E}\Big[\overline{\pi}\, \mathbb{E}\left[U_{1,i} - U_{0,i} \,|\, D_i(0) = 0, D_i(1) = 1, \boldsymbol{X}_i\right]\Big] \\
&= \mathbb{E}\left[\overline{\pi}\left(Z_i\Big(\mu_1(1; \boldsymbol{X}_i) - \mu_0(1; \boldsymbol{X}_i)\Big) + (1 - Z_i)\Big(\mu_1(0; \boldsymbol{X}_i) - \mu_0(0; \boldsymbol{X}_i)\Big)\right)\right] \\
&\quad + \mathbb{E}\Big[\overline{\pi}\, \mathbb{E}\left[U_{1,i} - U_{0,i} \,|\, D_i(0) = 0, D_i(1) = 1, \boldsymbol{X}_i\right]\Big] \\
&= \mathbb{E}\left[\overline{\pi}\left(\mu_1(Z_i, \boldsymbol{X}_i) - \mu_0(Z_i, \boldsymbol{X}_i) + \mathbb{E}\left[U_{1,i} - U_{0,i} \,|\, D_i(0) = 0, D_i(1) = 1, \boldsymbol{X}_i\right]\right)\right] \\
&= \mathbb{E}\left[\overline{\pi}\, \mathbb{E}\left[\mu_1(Z_i, \boldsymbol{X}_i) - \mu_0(Z_i, \boldsymbol{X}_i) + U_{1,i} - U_{0,i} \,|\, D_i(0) = 0, D_i(1) = 1, \boldsymbol{X}_i\right]\right] \\
&= \mathbb{E}\left[\mathbb{E}\left[D_i(1) - D_i(0) \,|\, \boldsymbol{X}_i\right]\ \mathbb{E}\left[Y_i(Z_i, 1) - Y_i(Z_i, 0) \,|\, D_i(0) = 0, D_i(1) = 1, \boldsymbol{X}_i\right]\right] \\
&= \mathbb{E}\left[\mathbb{E}\left[Y_i(Z_i, D_i(1)) - Y_i(Z_i, D_i(0)) \,|\, \boldsymbol{X}_i\right]\right] \\
&= \mathbb{E}\left[Y_i(Z_i, D_i(1)) - Y_i(Z_i, D_i(0))\right] \\
&= \text{AIE}.
\end{aligned}
$$

A.8 Semi-parametric Estimation of the AIE

It is difficult to directly use the CFs to compose estimates of the complier adjustment term, because various intercepts lose identification, but also because trusting semi-parametric estimates at individual points across the $\widehat{\lambda}_0(p'), \widehat{\lambda}_1(p')$ functions would increase variation more than is necessary.

This can be avoided by noting the relation between the ATE and the conditional ADE and conditional AIE. The following showing how to identify the AIE via relation to the ATE and conditional ADE, and omits the conditional on $\boldsymbol{X}_i$ for brevity.

A simple algebraic rearrangement has the following (as first noted in Imai et al. 2010, Section 3.1),

$$
\begin{aligned}
\text{ATE} &= \mathbb{E}\left[Y_i(1, D_i(1)) - Y_i(1, D_i(1))\right] \\
&= \mathbb{E}\left[Y_i(1, D_i(1)) - Y_i(0, D_i(1))\right] + \mathbb{E}\left[Y_i(0, D_i(1)) - Y_i(0, D_i(0))\right] \\
&= \underbrace{\mathbb{E}\left[Y_i(1, D_i(Z_i)) - Y_i(0, D_i(Z_i)) \,|\, Z_i = 1\right]}_{\text{ADE conditional on } Z_i=1} + \underbrace{\mathbb{E}\left[Y_i(Z_i, D_i(1)) - Y_i(Z_i, D_i(0)) \,|\, Z_i = 0\right]}_{\text{AIE conditional on } Z_i=0}.
\end{aligned}
$$

A similar re-arrangement also has the following,

$$\text{ATE} = \underbrace{\mathbb{E}\left[Y_i(Z_i, D_i(1)) - Y_i(Z_i, D_i(0)) \,|\, Z_i = 1\right]}_{\text{AIE conditional on } Z_i=1} + \underbrace{\mathbb{E}\left[Y_i(1, D_i(Z_i)) - Y_i(0, D_i(Z_i)) \,|\, Z_i = 0\right]}_{\text{ADE conditional on } Z_i=0}.$$

Reverting to the regression notation, to show how the ADE conditional on Z_i is identified:

$$\begin{aligned} \text{ADE} &= \mathbb{E}\left[Y_i(1, D_i(Z_i)) - Y_i(0, D_i(Z_i))\right] \\ &= \mathbb{E}\left[\gamma + \delta D_i(Z_i)\right]. \\ \implies \text{ADE conditional on } Z_i = 0 &= \mathbb{E}\left[\gamma + \delta D_i(Z_i) \,|\, Z_i = 0\right] \\ &= \mathbb{E}\left[\gamma + \delta D_i(0)\right]. \\ \text{ADE conditional on } Z_i = 1 &= \mathbb{E}\left[\gamma + \delta D_i(Z_i) \,|\, Z_i = 1\right] \\ &= \mathbb{E}\left[\gamma + \delta D_i(1)\right]. \end{aligned}$$

Finally achieve identification of the AIE via the ATE and conditional ADE, as follows,

$$\begin{aligned} \text{AIE} = &\Pr\left(Z_i = 0\right) \underbrace{\mathbb{E}\left[Y_i(Z_i, D_i(1)) - Y_i(Z_i, D_i(0)) \,|\, Z_i = 0\right]}_{\text{AIE conditional on } Z_i=0} \\ &+ \Pr\left(Z_i = 1\right) \underbrace{\mathbb{E}\left[Y_i(Z_i, D_i(1)) - Y_i(Z_i, D_i(0)) \,|\, Z_i = 1\right]}_{\text{AIE conditional on } Z_i=1} \\ = &\Pr\left(Z_i = 0\right) \Big[\text{ATE} - \left(\text{ADE conditional on } Z_i = 1\right)\Big] \\ &+ \Pr\left(Z_i = 1\right) \Big[\text{ATE} - \left(\text{ADE conditional on } Z_i = 0\right)\Big] \\ = &\,\text{ATE} - \Pr\left(Z_i = 0\right) \mathbb{E}\left[\gamma + \delta D_i(1)\right] - \Pr\left(Z_i = 1\right) \mathbb{E}\left[\gamma + \delta D_i(0)\right]. \end{aligned}$$

The semi-parametric AIE estimate then uses this representation, avoiding directly interacting with the estimated CFs, by plugging in estimates $\widehat{\Pr}(Z_i = 1) = \overline{Z}$, $\widehat{\text{ATE}}$, and the estimates from each side of the $D_i = 0, 1$ separated samples $\widehat{\gamma}, \widehat{\delta}$.

$$\widehat{\text{AIE}}^{\text{CF}} = \widehat{\text{ATE}} - (1 - \overline{Z}) \left(\widehat{\gamma} + \frac{1}{N} \sum_{i=1}^{N} \widehat{\delta}\, \widehat{\pi}(1; \boldsymbol{X}_i) \right) - \overline{Z} \left(\widehat{\gamma} + \frac{1}{N} \sum_{i=1}^{N} \widehat{\delta}\, \widehat{\pi}(0; \boldsymbol{X}_i) \right),$$

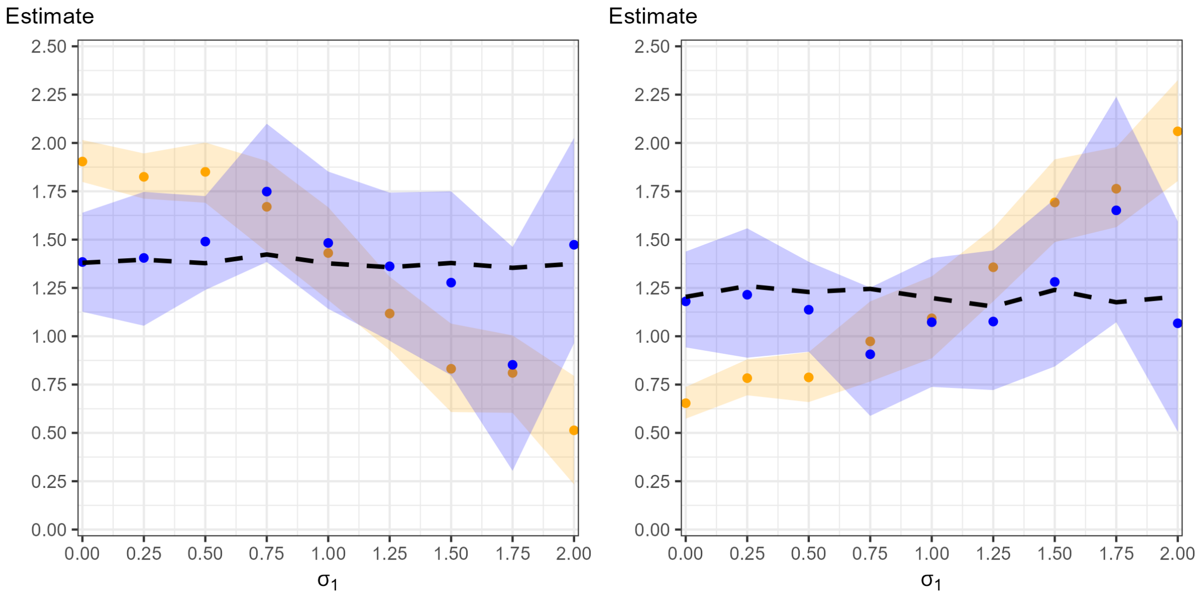
where $\frac{1}{N}\sum_{i=1}^{N} \widehat{\delta}\, \widehat{\pi}(0; \boldsymbol{X}_i)$ estimates $\mathbb{E}\left[\delta D_i(0)\right]$, and $\frac{1}{N}\sum_{i=1}^{N} \widehat{\delta}\, \widehat{\pi}(0; \boldsymbol{X}_i)$ estimates $\mathbb{E}\left[\delta D_i(1)\right]$. Everything involved is a standard point estimate, so their composition will converge to a normal distribution, too. Standard error computation can be achieved by a bootstrap procedure.

A.9 Implementation and Further Simulation Evidence

A number of statistical packages, for the R language (R Core Team 2025), 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.
- *Mgcv* (Wood, N., Pya & S”afken 2016) 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: OLS versus CF Estimates of CM Effects, varying $\text{Var}\left(U_{1,i}\right)$ relative to $\text{Var}\left(U_{0,i}\right) = 1$.



(a) ADE.

(b) AIE.

Note: These figures show the OLS and control function estimates of the ADE and AIE, for $N = 1,000$ sample size. The black dashed line is the true value, points are points estimates from data simulated with a given $\text{Corr}\big(U_{0,i}, U_{1,i}\big) = 0.5$, $\text{Var}\left(U_{0,i}\right) = 1$, and $\text{Var}\left(U_{1,i}\right)^{\frac{1}{2}}$ varied across $[0, 2]$. Shaded regions are the 95% confidence intervals; orange are the OLS estimates, blue the control function approach.