Causal Mediation in Natural Experiments

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Abstract

Natural experiments are a cornerstone of applied economics, providing settings for estimating causal effects with a compelling argument for treatment ignorability. Economists are often interested in understanding the mechanisms through which causal treatment effects operate, and Causal Mediation (CM) methods aid this by estimating how much of the treatment effect operates through a proposed mediator. The most popular CM approach relies on assumptions which are unrealistic in natural experiment settings: assuming the mediator is conditionally ignorable — in addition to the ignorability argument for the initial treatment. This paper shows that this approach leads to biased inference, solving for explicit bias terms when the mediator is not ignorable. Using the case of a Roy model for a mediator, I show that individuals' selection based on expected gains and costs is inconsistent with mediator ignorability without implausible behavioural assumptions, and that bias terms are large in practice. I show a control function approach, which overcomes these hurdles if monotonicity holds, using cost of mediator take-up as an instrument. Simulations confirm that this method corrects for persistent bias in conventional CM estimates, and performs comparably to a selection-on-observables approach when the structural assumptions do not hold. This approach gives applied researchers a practical method to estimate CM effects when they can only establish a credible argument for randomisation of the initial treatment, as is common in natural experiments.

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JEL Codes: D31, D91, I24, J24, Z00.

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Economists use natural experiments to credibly answer social questions, without the trouble of guiding randomisation of what they study. Did Vietnam-era military service lead to income losses? Does access to health insurance lead to employment gains? Do transfer payment lead to measurable long-run economic gains? Quasi-experimental variation gives methods to answer these questions, but give no indication of how these effects came about. Causal Mediation (CM) aims to estimate the mechanisms behind causal treatment effects, by estimating how much of the treatment effect operates through a proposed mediator. For example, how much of the (causal) gain from a transfer payment came from individuals choosing to attend higher education? This paper shows that the conventional approach to estimating CM effects is inappropriate in a natural experiment setting, giving a theoretical framework for how large bias terms are in the real world, and an approach to correctly estimate CM effects under minimal structural assumptions.

This paper starts by answering the following question: what does a selection-on-observables CM approach actually estimate when the mediator is not ignorable? The resulting estimates, the average direct effect and indirect effects respectively, are contaminated by bias terms — a sum of selection bias and non-parametric group differences. I then show how this bias operates in an applied regression framework, with bias coming from a correlated error term, showing that the bias term grows larger with the degree of unexplained selection. If individuals have been choosing whether to partake in a mediator based on expected costs and benefits (i.e., following a rational maximisation process), then assuming the mediator is ignorable gives unlikely implications for choice behaviour. This means the identifying assumption for conventional CM methods are unlikely to hold, and likely lead to biased inference in natural experiment settings.

I consider an alternative control function approach to estimating mediation effects. This approach solves the identification problem by instead placing a structural assumption for selection into the mediator (monotonicity), and assumes the researcher has a valid instrument for mediator take-up. It is realistic that these assumptions may hold in real-world natural experiment settings. Mediator monotonicity is in-line with conventional theories for selection-into-treatment, and is accepted widely in many applications using an instrumental variables research design. The existence of a valid instrument is a stronger assumption, which will not hold in every applied example, though is important to avoid parametric assumptions. The most compelling example is using data on the cost of mediator take-up as a first-stage instrument, if it varies between individuals for exogenous reasons and is strong in explaining compliance. Using an instrument avoids parametric assumptions on unexplained mediator selection, though limits the wider applicability of the method. This approach is not perfect: it provides no harbour for estimating CM effects if these structural assumptions do not hold true, though performs no worse than conventional CM methods in this case.

The most popular approach to CM estimates direct and indirect effects by assuming that a treatment is ignorable, and then assuming that a mediator is ignorable conditional on the treatment assignment (Imai et al. 2010). This approach arose in the statistics literature, and is widely used in epidemiology, medicine, and psychology to estimate mediation effects in observational studies. The applied economics literature has not picked up this practice, partially in an understanding that these assumptions are invalid in most observational settings. Indeed, a new strand of the econometric literature has developed estimators for CM effects under overlapping quasi-experimental research designs (Frölich & Huber 2017, Deuchert et al. 2019), a partial identification approach (Flores & Flores-Lagunes 2009), or testing full mediation through observed channels (Kwon & Roth 2024) — see Huber (2019) for an overview. The new literature has arisen in partial acknowledgement that a conventional selection-on-observables approach to CM in an applied setting can lead to biased inference, and needs alternative methods for credible inference in many cases. This paper makes this part explicit, showing exactly how a conventional approach to CM in a natural experiment can fail in practice.

This paper considers when no credible case can be made for mediator exogeneity, through any of the research designs above, leveraging classic labour economic theory for selection-into-treatment to identify direct and indirect effects. A selection-on-observables approach to CM in this setting suffers from bias of the same flavour as classic selection bias (Heckman et al. 1998), plus additional bias from group differences. Throughout, I use the Roy (1951) model as my bench-mark for judging the Imai et al. (2010) mediator ignorability assumption in a natural experiment setting, and find it unlikely to hold in practice.[1] This motivates a solution to the identification problem inspired by classic labour economic work, which also uses the Roy model as a benchmark (Heckman 1979, Heckman & Honore 1990). I follow the lead of these papers by using a control function approach to correct for the bias developed above.[2] This approach assumes mediator monotonicity, to ensure the mediator follows a selection model (Vytlacil 2002), and a valid instrument for mediator take-up, to avoid parametric assumptions on unobserved selection (Heckman & Navarro-Lozano 2004). This approach builds on the influential Imai et al. (2010) approach to CM, marrying the CM literature with labour economic theory on selection-into-treatment for the first time.

This paper proceeds as follows. Section 1 introduces CM, and develops expressions for the bias in mediation estimates in natural experiments. Section 2 describes this bias in applied settings with (1) a regression framework, (2) a setting with selection based on costs and benefits. Section 3 solves the identification problem with a control function, assuming a mediator follows a selection model and a researcher observes exogenous variation in cost of mediator take-up. Section 4 concludes.

[1]An alternative method to estimate CM effects is ensuring sequential ignorability holds by designing an overlapping randomised experiment for both treatment and mediator, which has been considered in the literature before (Ludwig et al. 2011, Imai et al. 2013).

[2]See Imbens (2007) for an overview of the control function as a solution to a more general case (of which my focus in a special case).

1 Direct and Indirect Effects

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

$$D_i = Z_i D_i(1) + (1 - Z_i) D_i(0),$$
$$Y_i = Z_i Y_i(1, D_i(1)) + (1 - Z_i) Y_i(0, D_i(0)).$$

Assume Z_i is ignorable.[4]

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

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, $Z \to D$.

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

It common in the economics literature to assume that Z influences D in at most one direction, $\Pr\left(D_i(1) \geq D_i(0)\right) = 1$ — monotonicity (Imbens & Angrist 1994). I assume monotonicity (and its conditional variant) holds through-out to simplify notation.[5]

[3] Other literatures use different notation. For example, Imai et al. (2010) write T_i, M_i, Y_i for the randomised treatment, mediator, and outcome, respectively. I use Z_i, D_i, Y_i to stick to the instrumental variables notation Angrist et al. (1996), more familiar in empirical economics (Angrist & Pischke 2009).

[4] 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.

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

2. The reduced-form effect refers to the effect of the treatment on outcome, $Z \to Y$, and is also known as the intent-to-treat effect in experimental settings, or total effect in causal mediation literature.

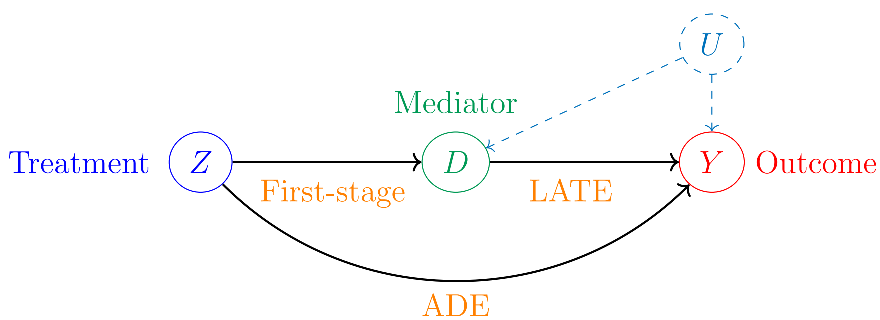
$$\mathbb{E}\left[Y_i \,|\, Z_i = 1\right] - \mathbb{E}\left[Y_i \,|\, Z_i = 0\right] = \mathbb{E}\left[Y_i(1, D_i(1)) - Y_i(0, D_i(0))\right]$$

In this setting, Z_i affects outcome Y_i directly, and indirectly via the $D_i(Z_i)$ channel, with no reverse causality. Figure 1 visualises the design, where the direction arrows denote the causal direction (and no reverse causality). On the other hand, mediation aims to decompose the reduced form effect of $Z \to Y$ into these two separate pathways.

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

These effects are not separately identified without further assumptions.

Figure 1: Structural Causal Model for Causal Mediation.



Note: This figures shows the structural causal model behind causal mediation. LATE refers to the effect $D \to Y$ local to Z compliers, so that AIE = average first-stage × LATE. Unobserved confounder U represents this paper's focus on the case that D_i is not ignorable, by showing an implied unobserved confounder. Subsection 2.1 formally defines U in this set-up.

1.1 Causal Mediation (CM) Estimands

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

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

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

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

Sequential ignorability assumes that the initial treatment Z_i is assigned randomly, conditional on $\boldsymbol{X}_i$. It then also assumes that, after Z_i is assigned, that D_i is assigned randomly conditional $\boldsymbol{X}_i, Z_i$. If sequential ignorability, 1(1) and 1(2), holds then the direct and indirect effects are identified by two-stage mean differences, after conditioning on $\boldsymbol{X}_i$.[6]

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

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

I refer to the estimands on the left-hand side as Causal Mediation (CM) estimands. These estimands are typically estimated with linear models, and CM estimates are composed from OLS estimates (Imai et al. 2010). While this is the most common approach in the applied literature, I do not assume the linear model. Linearity assumptions are unnecessary to my analysis; it suffices to note that heterogeneous treatment effects and non-linear confounding

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

would bias OLS estimates of CM estimands in the same manner that is well documented elsewhere (see e.g., Angrist 1998, Słoczyński 2022). This section focuses on problems that plague CM in practice, regardless of estimation method.

1.2 Bias in Causal Mediation Estimates

Applied research may use a natural experiment to justify the treatment Z_i is ignorable, justifying assumption 1(1). Rarely does research relying on a quasi-experimental research design employ an additional, overlapping identification design for D_i as part of the analysis. One might consider using conventional CM methods to estimate direct and indirect effects, and learn about the mechanisms behind the treatment effect under study.[7] This approach leads to biased estimates, and contaminates inference regarding direct and indirect effects.

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

Proof. See Subsection A.4 for the extended proof. □

Below I present the relevant selection bias and group difference terms, omitting the conditional on $\boldsymbol{X}_i$ notation for brevity.

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

[7] Imai et al. (2013) call attention to the need for a separate research design to isolate causal effects of D_i in randomised controlled trials; Subsection A.3 overviews literature, finding many papers that employ mediation methods with a research design for Z_i, but not for D_i.

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

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

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

The selection bias terms come from systematic differences between the treated and untreated groups, differences not fully unexplained by $\boldsymbol{X}_i$. These selection bias terms would equal to zero if the mediator was ignorable 1(2), but do not necessarily average to zero if not. The group differences represent the fact that a matching estimator gives an average effect on the treated group and, when selection-on-observables does not hold, this is systematically different from the average effect (Heckman et al. 1998).[8,9,10]

2 Causal Mediation in Applied Settings

In this section, I further develop the issue of selection in causal mediation estimates. First, I show the non-parametric bias terms from above can be written as omitted variables bias in a regression framework. Second, I show how selection bias operates in an applied model for selection into a mediator based on costs and benefits.

2.1 Regression Framework

Inference for direct and direct effects can be written in a regression framework, showing how correlation between the error term and the mediator persistently biases estimates. Write $Y_i(Z, D)$ as a sum of observed factors $Z_i, \boldsymbol{X}_i$ and unobserved factors, $U_{1,i}, U_{0,i}$ (following the notation of Heckman & Vytlacil 2005). Put $\mu_D(Z; \boldsymbol{X}_i) = \mathbb{E}\left[Y_i(Z_i, 0) \,|\, \boldsymbol{X}\right]$, to give a representation of the average direct and indirect effects.

$$\mathbb{E}\left[Y_i(Z_i, D_i(1)) - Y_i(Z_i, D_i(0))\right] = \mathbb{E}\Big[\Big(D_i(1) - D_i(0)\Big) \times \Big(\mu_1(Z_i; \boldsymbol{X}_i) - \mu_0(Z_i; \boldsymbol{X}_i)\Big)\Big],$$
$$\mathbb{E}\left[Y_i(1, D_i(Z_i)) - Y_i(0, D_i(Z_i))\right] = \mathbb{E}\Big[\mu_{D_i}(1; \boldsymbol{X}_i) - \mu_{D_i}(0; \boldsymbol{X}_i)\Big].$$

[8]The group differences term is a non-parametric framing of the bias from controlling for intermediate outcomes, previously studied only in a linear setting. These are referred to as bad controls by Cinelli et al. (2024), or M-bias by Ding & Miratrix (2015).

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

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

Then define the error terms.

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

With this notation, observed data Z_i, D_i, Y_i take the following representation, which characterises direct effects, indirect effects, and bias from selection.

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

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

First-stage (3) is identified, with $\phi, \varphi(\boldsymbol{X}_i)$ the intercept, and π the average rate of compliance (which may depend on $\boldsymbol{X}_i$). Second-stage (4) is not identified without further assumptions. $\alpha, \zeta(\boldsymbol{X}_i)$ are the intercept terms, and β, γ, δ are values that comprise mediation effects — all whose values may depend on $\boldsymbol{X}_i$, see Subsection A.6 for full definitions. $U_{0,i} + D_i \left(U_{1,i} - U_{0,i}\right)$ is the possibly correlated error term, which disrupts identification. The average direct and indirect effects are averages of these coefficients.

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

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

By construction, $U_i = U_{1,i} - U_{0,i}$ is an unobserved confounder. The regression estimates of second-stage (4) give unbiased estimates only if D_i is also conditionally ignorable: $D_i \perp\!\!\!\perp U_i$. If not, then regression estimates suffer from omitted variables bias if they do not adjust for the unobserved confounder U_i.

2.2 Selection on Costs and Benefits

The key to noting that CM is at risk of bias is noting that $D_i \perp\!\!\!\perp U_i$ is unlikely to hold in applied settings. Without an identification strategy for D_i, in addition one for Z_i, 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, after $Z_i, \boldsymbol{X}_i$ have been assigned. 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.

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

Paragraph here talking about why the Roy model is useful. (Roy 1951, Heckman & Honore 1990).

Decompose the costs into its mean and unobserved error, as above $C_i(Z_i) = \mu_C(Z_i; \boldsymbol{X}_i) + U_{C,i}$, and collect the mean costs and benefits, $\mu \coloneqq \mu_1 - \mu_0 - \mu_C$. So we can write the first-stage selection equation in full.

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

Theorem: if selection is Roy style, and sequential ignorability holds, then unobserved benefits play no part in selection. The only driver in differences in selection are differences in costs (and not benefits).

$$\mathbb{E}\left[D_i(z') \,|\, U_i = u\right] = \mathbb{E}\left[D_i(z') \,|\, U_i = u'\right]$$

For all u', u in the range of the distribution of U_i. Proof: by contradiction, add to the appendix. This could, for example, hold if $U_{1,i} - U_{0,i}$ is degenerate conditional on $\boldsymbol{X}_i$.

Short paragraph on why this means $\boldsymbol{X}_i$ must be incredibly rich. Write about if D_i is the choice to attend education, then $\boldsymbol{X}_i$ must soak up all gains to education. Or assuming that all variation in D_i comes from unobserved differences in take-up costs. This is unlikely to hold true, absent a separate research design for D_i, limiting the selection to an information restricted version of the Roy model.

If not, then selection bias propagates, including writing here for what the selection bias term is equal to.

2.3 Applied Settings

Three parapgraphs on what goes on in empirical settings. Survey the papers, and speak about it heavily in one paragraph.

table:

name — $Z \to Y$ — design for Z — Primary mediatory — controls — Possible U.

3 Solving Identification with a Control Function

If you could control for U_i, then you would. Laffers et al, for example, tests sequential ignoability.

The IV literature assumes a first-stage monotonicity condition, where randomised Z_i influences mediator D_i in at most one direction.

Definition 2. *First-stage Monotonicity (Imbens & Angrist 1994).*

$$\Pr\left(D_i(1) \geq D_i(0)\right) = 1 \tag{5}$$

Assuming 2(5) in a mediation setting opens mediation to the wide literature on IV and selection models for identification in the presence of selection.

Theorem 2. *Under monotonicity, mediator D_i can be represented by a selection model. Suppose 2(5) holds, then there is a function $\mu(.)$ and random variable U_i such that D_i takes the following form.*

$$D_i(z) = \mathbb{1}\left\{\mu(z) \geq U_i\right\}, \quad \forall z = 0, 1$$

Proof. Special case of the Vytlacil (2002) equivalence result; see Subsection A.5. □

Theorem 2 is a powerful result: it says that at the cost of assuming monotonicity (as is done in the IV literature), then selection into D_i takes a latent index form, and opens up identification in a mediation context to the wide literature on identifying treatment effects in selection models.

4 Summary and Concluding Remarks

This paper studies the returns to higher education, using IV methods from the epidemiology literature and adjustments from the causal mediation literature to tackle violations of the exclusion restriction. First, I derive identification of the average mechanism effect under a selection-on-observables type assumption, and partial identification when unobserved selection confounding. I apply these methods to a sample of retirement age Americans in the years 1990–2021, using genetic information to instrument for higher education, estimating that higher education leads to roughly 40% higher earnings (point estimates), or between 8–44% higher earnings (partial bounds). Additionally, women had significantly higher returns to higher education over this time period.

The methods here provide alternatives to assuming the exclusion restriction in empirical applications of IV models, so can be useful in sensitivity analyses for any application of

IV methods. Mendelian randomisation is a particularly useful application of IV methods, though the exclusion restriction is particularly problematic in practice. The approach allows researchers to use MR to study effects of both health conditions and behaviours with significant selection-into-treatment concerns, such as higher education.

The approach could be used in AB tests, where a firm randomises a treatment and costs of a suspected mediator (if they do not want to also randomise a mediator fully).

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A Appendix

This project used computational tools which are fully open-source. Any comments or suggestions may be sent to me at seh325@cornell.edu, or raised as an issue on the Github project.

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

- *Tidyverse* (Wickham et al. 2019) collected tools for data analysis in the R language.
- *DoubleML* (Bach et al. 2024) implemented doubly robust methods used in the empirical analysis.
- *GRF* (Athey et al. 2019, Tibshirani et al. 2023) compiled forest computational tools for the R language.
- *Stargazer* (Hlavac 2018) provided methods to efficiently convert empirical results into presentable output in LaTeX.

A.1 Identification in Causal Mediation

Imai et al. (2010, Theorem 1) states that the direct and indirect effects 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.

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

My estimand for the average direct effect is a simple rearrangement of the above. The estimand for the average indirect effect relies on a different sequence, relying on (1) sequential ignorability, (2) conditional monotonicity. These give (1) identification of, and equivalence between, LADE conditional on $\boldsymbol{X}_i$ and ADE conditional on $\boldsymbol{X}_i$, (2) identification of the complier score.

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

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

A.2 Continuous Average Causal Responses

Section here relating the approach to the average causal response function (see e.g., Angrist Imbens JASA 1996, Andrew Bacon for DiD 2023).

A.3 Previous Literature

Create a table in this section that surveys previous research which employs mediation methods while having a clear causal design for Z_i, but not D_i.

Paper	Field	Research Design for Z_i	Research Design for D_i	Selection bias?
Paper name 1.				

A.4 Bias in Mediation Estimates

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

A.4.1 Bias in Direct Effect Estimates

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

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

And so

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

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

To reach the final term, note the following.

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

The second term is a difference term between the average and the average for relevant complier groups.

Collect everything together, as follows.

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

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

A.4.2 Bias in Indirect Effect Estimates

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

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

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

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

For each $z' = 0, 1$.

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

So compose the CM estimand, as follows.

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

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

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

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

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

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

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

Collect everything together, as follows.

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

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

A.5 Proof of the Selection Model Representation

Write the proof in here, following Vytlacil (2002) construction in the forward direction. Note that the notation needs updating for no exclusion restriction.

A.6 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 $U_{D,i} = Y_i(Z, D) - \mu_D(Z; \boldsymbol{X})$, so we have the following expressions.

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

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

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

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

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

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

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

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

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

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

With the following definitions:

- $\alpha = \mathbb{E}\left[\mu_0(0; \boldsymbol{X}_i) \right]$ and $\zeta(\boldsymbol{X}_i) = \mu_0(0; \boldsymbol{X}_i) - \alpha$ are the intercept terms.
- $\beta = \mu_1(0; \boldsymbol{X}_i) - \mu_0(0; \boldsymbol{X}_i)$ is the indirect effect under $Z_i = 0$

- $\gamma = \mu_0(1; \boldsymbol{X}_i) - \mu_0(0; \boldsymbol{X}_i)$ is the direct effect under $D_i = 0$.
- $\delta = \mu_1(1; \boldsymbol{X}_i) - \mu_0(1; \boldsymbol{X}_i) - (\mu_1(0; \boldsymbol{X}_i) - \mu_0(0; \boldsymbol{X}_i))$ is the interaction effect.
- $U_{0,i} + D_i \left(U_{1,i} - U_{0,i}\right)$ is the remaining error term.

This sequence gives us the resulting regression equation:

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

Taking the conditional expectation, and collecting for the expressions of the direct and indirect effects:[11]

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

These terms are conventionally estimated in a simultaneous regression (Imai et al. 2010).

If sequential ignorability does not hold, then the regression estimates from estimating the mediation equations (without adjusting for the contaminated bias term) suffer from omitted variables bias.

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

And so the direct and indirect effect estimates are contaminated by these bias terms.

[11]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.