2SLS with Multiple Treatments

Manudeep Bhuller*   Henrik Sigstad†

August 10, 2023

Abstract: We study what two-stage least squares (2SLS) identifies in models with multiple treatments under treatment effect heterogeneity. Two conditions are shown to be necessary and sufficient for the 2SLS to identify positively weighted sums of agent-specific effects of each treatment: average conditional monotonicity and no cross effects. Our identification analysis allows for any number of treatments, any number of continuous or discrete instruments, and the inclusion of covariates. We provide characterizations of choice behavior implied by our identification conditions and discuss how the conditions can be tested empirically.

Keywords: multiple treatments, monotonicity, instrumental variables, two-stage least squares

JEL codes: C36

Acknowledgments: We thank Gaurab Aryal, Brigham Frandsen, and Magne Mogstad for useful comments.

*University of Oslo; CEPR; CESifo; and IZA. E-mail: manudeep.bhuller@econ.uio.no
†BI, Norwegian Business School. E-mail: henrik.sigstad@bi.no
1 Introduction

In many settings—e.g., education, career choices, and migration decisions—estimating the causal effects of a series of different treatments is valuable. For instance, in the context of criminal justice, one might be interested in separately estimating the effects of conviction and incarceration on defendant outcomes (Humphries et al., 2023). Identifying treatment effects in settings with multiple treatments using instruments has, however, proven challenging (Heckman et al., 2008). A common approach in the applied literature is to estimate a “multivariate” two-stage least squares (2SLS) regression with indicators for receiving various treatments as multiple endogenous variables and at least as many instruments.\(^1\) While such an approach is valid under homogenous treatment effects, it does not generally identify meaningful treatment effects under treatment effect heterogeneity.

To fix ideas about the identification problem, consider a case with three mutually exclusive treatments—\(T \in \{0, 1, 2\}\)—and a vector of valid instruments \(Z\). Let \(\beta_{1i}\) and \(\beta_{2i}\) be the causal effects of receiving treatments 1 and 2 relative to receiving treatment 0 for agent \(i\) on an outcome variable. Then, the 2SLS estimate of the causal effect of receiving treatment 1 is, in general, a weighted sum of \(\beta_{1i}\) and \(\beta_{2i}\) across agents, where the weights can be negative. Thus, the estimated effect of treatment 1 can both put negative weight on the effect of treatment 1 for some agents and be contaminated by the effect of treatment 2. In severe cases, the estimated effect of treatment 1 can be negative even though \(\beta_{1i} > 0\) for all agents. The existing literature has, however, not clarified which conditions are necessary for the 2SLS estimate of the effect of treatment 1 to assign proper weights—non-negative weight on \(\beta_{1i}\) and zero weight on \(\beta_{2i}\).\(^2\)

In this paper, we present two necessary and sufficient conditions—besides the standard rank, exclusion, and exogeneity assumptions—for multivariate 2SLS to assign proper weights: average conditional monotonicity and no cross effects. Our results apply in the general case with \(n\) treatments and \(m \geq n\) continuous or discrete instruments. We also provide results for settings where exogeneity holds only conditional on covariates—a common feature in applied work that complicates the analysis of 2SLS already in the binary treatment case (Słoczyński, 2020; Blandhol et al., 2022).\(^3\) Finally,\(^1\)

\(^1\) Examples of studies estimating models with multiple treatments using 2SLS—either as the main specification or as an extension of a baseline specification with a binary treatment—include Persson & Tabellini (2004); Acemoglu & Johnson (2005); Rohlfs (2006); Angrist (2006); Angrist et al. (2009); Autor et al. (2015); Mueller-Smith (2015); Kline & Walters (2016); Jaeger et al. (2018); Bombardini & Li (2020); Bhuller et al. (2020); Norris et al. (2021); Humphries et al. (2023); Angrist et al. (2022).

\(^2\) The requirement that 2SLS assign proper weights is equivalent to 2SLS being weakly causal (Blandhol et al. 2022)—providing estimates with the correct sign—under arbitrary heterogeneous effects. See Section B.1. Note that weak causality is a weak criterion. As argued by Heckman & Vytlacil (2007), the particular weighted sum produced by 2SLS—even when all weights are non-negative—is not necessarily a parameter of policy interest. The methods based on marginal treatment effects outlined in Section 4.2 aim to target more policy relevant treatment parameters (possibly at the cost of lower efficiency).

\(^3\) Blandhol et al. (2022)’s analysis also extend to the estimation of aggregate effects of multiple ordered treatment.
our results allow the researcher to specify any set of relative treatment effects, not just effects relative to an excluded treatment (treatment 0).

For expositional ease, however, we continue with the three-treatment example. When does the 2SLS estimate of receiving treatment 1 put non-negative weight on $\beta_{1i}$ and zero weight on $\beta_{2i}$? To develop intuition about the required conditions, assume we run 2SLS with the following two instruments: the linear projection of an indicator for receiving treatment 1 on the instrument vector $Z$, which we refer to as “instrument 1”, and the linear projection of an indicator for receiving treatment 2 on the instrument vector $Z$, i.e., “instrument 2”. Using instruments 1 and 2—the predicted treatments from the 2SLS first stage—as instruments is numerically equivalent to using the full vector $Z$ as instruments. The first condition—average conditional monotonicity—requires that, conditional on instrument 2, instrument 1 does not, on average, induce agent $i$ out of treatment 1.\(^4\) The second condition—no cross effects—requires that, conditional on instrument 1, instrument 2 does not, on average, induce agent $i$ into or out of treatment 1. The latter condition is necessary to ensure zero weight on $\beta_{2i}$ and is particular to the case with multiple treatments. Notably, both conditions can be tested empirically. For instance, one can regress an indicator for receiving treatment 1 on instrument 1 and 2 on subsamples and test whether the coefficient on instrument 1 is non-negative and whether the coefficient on instrument 2 is zero.

Building upon our general identification results, we consider a prominent special case: the just-identified 2SLS with $n$ mutually exclusive treatments and $n$ mutually exclusive binary instruments. This case is a natural generalization of the canonical case with a binary treatment and a binary instrument to multiple treatments. A typical application is a randomized controlled trial where each agent is randomly assigned to one of $n$ treatments, but compliance is imperfect. In this setting, our two conditions require that each instrument affects exactly one treatment indicator. In particular, there must be a labeling of the instruments such that instrument $k$ moves agents only from the excluded treatment 0 into treatment $k$. This result gives rise to a more powerful test in just-identified models: If 2SLS assigns proper weights, each instrument must affect exactly one treatment indicator in a first-stage IV regression. This test can be applied both in the whole population and in subsamples.

The requirement that each instrument affects exactly one choice margin restricts choice behavior in a particular way. In particular, 2SLS assigns proper weights only when choice behavior can be described by a selection model where the excluded treatment is always the preferred alternative or the next-best alternative. It is thus not sufficient that each instrument influences the utility of only one choice alternative. For instance, an instrument that affects only the utility of receiving treatment 1 could still affect the take-

\(^4\)This condition generalizes Frandsen et al. (2023)’s average monotonicity condition to multiple treatments and is substantially weaker than the Imbens-Angrist monotonicity condition (Imbens & Angrist, 1994).
up of treatment 2 by inducing agents who would otherwise have selected treatment 2 to select treatment 1. Such cross effects are avoided if the excluded treatment is always at least the next-best alternative. To apply 2SLS in this just-identified case, the researcher must argue why the excluded treatment is always the best or the next-best alternative. Our results essentially imply that unless researchers can infer next-best alternatives—as in Kirkeboen et al. (2016) and the following literature—2SLS in just-identified models does not identify a meaningful causal effect under arbitrary heterogeneous effects.

Until now, we have considered unordered treatment effects—treatment effects relative to an excluded treatment. Our results, however, also apply to any other relative treatment effects a researcher might seek to estimate through 2SLS. An important case is ordered treatment effects—the effect of treatment $k$ relative to treatment $k-1$. In the ordered case, the just-identified 2SLS assigns proper weights if and only if there exists a labeling of the instruments such that instrument $k$ moves agents only from treatment $k-1$ to treatment $k$. As in the unordered case, this condition can be tested both in the full population and in subsamples. The condition also imposes a particular restriction on agents’ choice behavior: we show that 2SLS assigns proper weights in just-identified ordered choice models only when agents’ preferences can be described as single-peaked over the treatments. When treatments have a natural ordering—such as years of schooling—the researcher might be able to make a strong theoretical case in favor of such preferences.

We finally present another special case of ordered choice where our conditions are satisfied: a classical threshold-crossing model applicable when treatment assignment depends on a single index crossing multiple thresholds. For instance, treatments can be grades and the latent index the quality of the student’s work, or treatments might be years of prison and the latent index the severity of the committed crime. Suppose the researcher has access to exogenous shocks to these thresholds, for instance through random assignment to judges or graders that agree on ranking but use different cutoffs. Then 2SLS assigns proper weights provided that there is a linear relationship between the predicted treatments—an easily testable condition.

Our paper contributes to a growing literature on the use of instruments to identify causal effects in settings with multiple treatments (Heckman et al., 2008; Kline & Walters, 2016; Kirkeboen et al., 2016; Heckman & Pinto, 2018; Lee & Salanié, 2018; Galindo, 2020; Pinto, 2021) or multiple instruments (Mogstad et al., 2021; Goff, 2020; Mogstad et al., forthcoming). Our main contribution is to provide the exact conditions under

---

5. Angrist & Imbens (1995) showed the conditions under which 2SLS with the multivalued treatment indicator $T$ as the endogenous variable identifies a convex combination of the effect of treatment 2 relative to treatment 1 and the effect of treatment 1 relative to treatment 0. In contrast, we seek to determine the conditions under which 2SLS with two binary treatment indicators—$D_1 = 1 [T \geq 1]$ and $D_2 = 1 [T = 2]$—separately identifies the effect of each of the two treatment margins.

6. More precisely, the conditional expectation of predicted treatment $k$ must be a linear function of predicted treatment $l \neq k$. 
which 2SLS with multiple treatments assigns proper weights under arbitrary treatment
effect heterogeneity. We allow for any number of treatments, any number of continuous
or discrete instruments, and any definition of treatment indicators. We also allow for
covariates. Moreover, we show how the conditions can be tested. By comparison, the
existing literature provides only sufficient conditions in the just-identified case with three
treatments, three instrument values, and no controls (Behaghel et al., 2013; Kirkeboen
et al., 2016).

In the case of just-identified models with unordered treatments, we show that the extended monotonicity condition provided by Behaghel et al. (2013) is not only sufficient
but also necessary for 2SLS to assign proper weights, after a possible permutation of the
instruments. This non-trivial result gives rise to a new test in just-identified models: For
2SLS to assign proper weights each instrument can only affect one treatment. Further-
more, we show that knowledge of agents’ next-best alternatives—as in Kirkeboen et al.
(2016)—is implicitly assumed whenever estimates from just-identified 2SLS models with
multiple treatments are interpreted as a positively weighted sum of individual treatment
effects. We thus show that the assumption that next-best alternatives are observed or
can be inferred is not only sufficient but also essentially necessary for 2SLS to identify
a meaningful causal parameter. 

We also provide new identification results for ordered treatments. First, we show
when 2SLS with multiple ordered treatments identifies separate treatment effects in
a standard threshold-crossing model considered in the ordered choice literature (e.g.,
Carneiro et al. 2003; Cunha et al. 2007; Heckman & Vytlacil 2007). While Heckman &
Vytlacil (2007) show that local IV identifies ordered treatment effects in such a model,
we show that 2SLS can also identify the effect of each treatment transition under an
easily testable linearity condition. We also show how the result of Behaghel et al. (2013)
extends to ordered treatment effects. Finally, we show that for 2SLS to assign proper
weights in just-identified models with ordered treatment effects, it must be possible to
describe agents’ preferences as single-peaked over the treatments.

In contrast to Heckman & Pinto (2018), who provide general identification results
in a setting with multiple treatments and discrete instruments, we focus specifically on
the properties of 2SLS—a standard and well-known estimator common in the applied
literature. Other contributions to the literature on the use of instrumental variables
to separately identify multiple treatments (Lee & Salanié, 2018; Galindo, 2020; Lee &
Salanié, 2023; Mountjoy, 2022; Pinto, 2021) focus on developing new approaches to iden-
tification. We do not necessarily recommend 2SLS over these alternative methods. But

7The only exception being that next-best alternatives need not be observed for always-takers.
8For instance, the method of Heckman & Pinto (2018) identify causal effects under strictly weaker
assumptions than those required for 2SLS to assign proper weights in models in just-identified models
with three treatments (see Section 4.2). Also, as argued by Heckman & Vytlacil (2007), the weighted
average of treatment effects produced by 2SLS in overidentified models is not necessarily an interesting
parameter, even when the weights are non-negative. The alternatives to 2SLS, discussed in Section 4.2,
since 2SLS remain a popular approach among practitioners, we see a need to clarify the exact conditions under which multivariate 2SLS is a valid approach—contributing to a recent body of research assessing the robustness of standard estimators to heterogeneous effects (e.g., de Chaisemartin & d’Haultfoeuille 2020a,b; Callaway & Sant’Anna 2021; Goodman-Bacon 2021; Sun & Abraham 2021; Borusyak et al. 2021; Goldsmith-Pinkham et al. 2022).

In Section 2, we develop the exact conditions for the multivariate 2SLS to assign proper weights to agent-specific causal effects and discuss how these conditions can be tested. In Section 3, we consider two special cases—the just-identified case and a threshold-crossing model. Section 4 discusses implications when the conditions fail and alternatives to 2SLS. Section 5 provides an illustration of our conditions for the random judge IV design. We conclude in Section 6. Proofs and additional results are in the Appendix.

2 Multivariate 2SLS with Heterogeneous Effects

In this section, we develop sufficient and necessary conditions for the multivariate 2SLS to identify a positively weighted sum of individual treatment effects under heterogeneous effects and explain how these conditions can be tested.

2.1 Definitions

Fix a probability space where an outcome corresponds to a randomly drawn agent $i$. Define the following random variables: A multi-valued treatment $T \in \mathcal{T} \equiv \{0, 1, 2\}$, an outcome $Y \in \mathbb{R}$, and a vector-valued instrument $Z \in \mathcal{Z} \subseteq \mathbb{R}^m$ with $m \geq 2$. For expositional ease, we focus on the case with three treatments and no control variables. All our results generalize to an arbitrary number of treatments. In Section see B.3, we discuss how our results extend to the case with control variables.

Define $\mathcal{S}$ as all possible mappings $f: \mathcal{Z} \rightarrow \mathcal{T}$ from instrument values to treatments—all possible ways the instrument can affect treatment. Following Heckman & Pinto (2018), we refer to the elements of $\mathcal{S}$ as response types. The random variable $S \in \mathcal{S}$ describes agents’ potential treatment choices: If agent $i$ has $S = s$, then $s(z)$ for $z \in \mathcal{Z}$ indicates the treatment selected by agent $i$ if $Z$ is set to $z$. The response type of an agent describes how the agent’s choice of treatment reacts to changes in the instrument.

---

9 All random variables thus correspond to a randomly drawn agent. We omit $i$ subscripts.
10 We focus on mutually exclusive treatments throughout. Treatments that are not mutually exclusive can always be made into mutually exclusive treatments. For instance, two not mutually exclusive treatments and an excluded treatment can be thought of as four mutually exclusive treatments: Receiving the excluded treatment, receiving only treatment 1, receiving only treatment 2, and receiving both treatments.
11 See Section B.2.
For example, in the case with a binary treatment and a binary instrument, the possible response types are never-takers \(s(0) = s(1) = 0\), always-takers \(s(0) = s(1) = 1\), compliers \(s(0) = 0, s(1) = 1\), and defiers \(s(0) = 1, s(1) = 0\). Similarly, define \(Y(t)\) for \(t \in T\) as the agent’s potential outcome when \(T\) is set to \(t\).

Using 2SLS, a researcher can aim to estimate two out of three relative treatment effects. We focus on two special cases: the unordered and ordered case. In the unordered case, the researcher seeks to estimate the effects of treatment 1 and 2 relative to treatment 0 by estimating 2SLS with treatment indicators \(D_{\text{unordered}}^1 \equiv 1[T = 1]\) and \(D_{\text{unordered}}^2 \equiv 1[T = 2, 2]^{12}\). In that case, the treatment effects of interest are represented by the random vector \(\beta_{\text{unordered}} \equiv (Y(1) - Y(0), Y(2) - Y(0))^T\). In the ordered case, the researcher is interested in \(\beta_{\text{ordered}} \equiv (Y(1) - Y(0), Y(2) - Y(1))^T\) and uses treatment indicators \(D_{\text{ordered}}^1 \equiv 1[T \geq 1]\) and \(D_{\text{ordered}}^2 \equiv 1[T = 2]\). Let \(\beta \equiv (\beta_1, \beta_2)^T\) denote the treatment effects of interest and \(D \equiv (D_1, D_2)^T\) the corresponding treatment indicators. Unless otherwise specified, our results hold for any definition of \(\beta\). But to ease exposition we focus on the unordered case when interpreting our results. We maintain the following standard IV assumptions throughout:

**Assumption 1.** (Exogeneity and Exclusion). \(\{Y(0), Y(1), Y(2), S\} \perp Z\)

**Assumption 2.** (Rank). \(\text{Cov}(Z, D)\) has full rank.

For a response type \(s \in S\), let \(v_s\) be the induced mapping between instruments and treatment indicators: \(v_s(z) \equiv (s_1(z), s_2(z))^T\) for all \(z \in Z\) with \(s_t(z) \equiv 1[s(z) = t]\). The function \(v_s\) is just another way to summarize the choice behavior of response type \(s\). Define the 2SLS estimand \(\beta_{\text{2SLS}} = (\beta_{1\text{2SLS}}, \beta_{2\text{2SLS}})^T\) by

\[
\beta_{\text{2SLS}} \equiv \text{Var}(P)^{-1} \text{Cov}(P, Y)
\]

where \(P\) is the linear projection of \(D\) on \(Z\)

\[
P = (P_1, P_2)^T \equiv E[D] + \text{Var}(Z)^{-1} \text{Cov}(Z, D)(Z - E[Z])
\]

We refer to \(P\) as the predicted treatments—the best linear prediction of the treatment indicators given the value of the instruments. Similarly, we refer to \(P_k\) for \(k \in \{1, 2\}\) as predicted treatment \(k\). Since 2SLS using predicted treatments as instruments is numerically equivalent to using the original instruments, we can think of \(P\) as our instruments. In particular, \(P\) is a linear transformation of the original instruments \(Z\) such that we get one instrument corresponding to each treatment indicator. We will occasionally use language such as “the effect of \(P_1\) on treatment 2”.

---

\(^{12}\)The more general case of unordered choice—where the researcher seeks to estimate all relative treatment effects—could be analyzed by varying which treatment is considered to be the excluded treatment. In that case, the researcher should discuss and test the conditions in Section 2 for each choice of excluded treatment. Often, however, the researcher will only be interested in some relative treatment effects.
2.2 Identification Results

What does multivariate 2SLS identify under Assumptions 1 and 2 when treatment effects are heterogeneous across agents? The following proposition expresses the 2SLS estimand as a weighted sum of average treatment effects across response types:

**Proposition 1.** Under Assumptions 1 and 2

\[
\beta_{2SLS} = E[w_S \beta^S]
\]

where for \(s \in S\)

\[
w_s \equiv \text{Var}(P)^{-1} \text{Cov}(P, v_s(Z))
\]

\[
\beta^s \equiv (\beta^s_1, \beta^s_2)^T \equiv E[\beta | S = s]
\]

In the case of a binary treatment and a binary instrument, \(\beta_{2SLS}\) is a weighted sum of average treatment effects for compliers and defiers. Proposition 1 is a generalization of this result to the case with three treatments and \(m\) instruments. In this case, \(\beta_{2SLS}\) is a weighted sum of the average treatment effects of all response types present in the population. The vector \(\beta^s\) describes the average treatment effects for agents of response type \(s\). The weight matrix \(w_s\) summarizes how the treatment effects of agents of response type \(s\) contribute to the estimated coefficients. The diagonal elements of \(w_s\) indicate how the average effects of treatment \(k\) of agents with response type \(s\) contribute to the estimated effect of treatment \(k\). Without further restrictions, these weights could be both positive and negative. The off-diagonal elements of \(w_s\) describe how the average effects of treatment \(k\) for response type \(s\) contribute to the estimated effect of treatment \(l\) for \(l \neq k\). Thus, in general, all treatment effects for response type \(s\) might contribute, either positively or negatively, to all estimated treatment effects.

In the canonical case of a binary treatment and a binary instrument, identification is ensured when there are no defiers. The aim of this paper is to generate similar restrictions on the possible response types in the case of multiple treatments. To become familiar with our notation and the implications of Proposition 1, consider the following example.

**Example 1.** Consider the case of three treatments and two mutually exclusive binary instruments: \((Z_1, Z_2) \in \{(0, 0), (1, 0), (0, 1)\}\). Assume we are interested in unordered treatment effects, \(\beta_{\text{unordered}} = (Y(1) - Y(0), Y(2) - Y(0))^T\), with corresponding treatment indicators \(D_1 = 1[T = 1]\) and \(D_2 = 1[T = 2]\). One possible response type

---

13The literature has documented that 2SLS gives a weighted average of individual treatment effects in the case of two mutually exclusive treatment indicators when using a discrete instrument (Kirkeboen et al., 2016) or two continuous instruments (Mountjoy, 2022). Proposition 1 generalizes these results to more than three treatments, other definitions of treatment indicators, and more instruments than treatments. Moreover, the proposition provides a concise and easily interpretable expression for the weights.
in this case is defined by

\[
s(z) = \begin{cases} 
1 & \text{if } z = (0, 0) \\
1 & \text{if } z = (1, 0) \\
2 & \text{if } z = (0, 1) 
\end{cases}
\]

Response type \(s\) thus selects treatment 1 unless \(Z_2\) is turned on. When \(Z_2 = 1\), \(s\) selects treatment 2. Assume the best linear predictors of the treatments indicators are \(P_1 = 0.1 + 0.4Z_1\) and \(P_2 = 0.2 + 0.5Z_2\) and that all instrument values are equally likely. If response type \(s\) were present in the population, the weights on the average treatment effects of response type \(s\) would be:\(^{14}\)

\[
w_s = \begin{bmatrix} 0 & 0 \\ -2 & 2 \end{bmatrix}
\]

The average effect of treatment 2 for response type \(s\) thus contributes positively to the estimated effect of treatment 2 (\(\beta_2^{2SLS}\)) and negatively to the estimated effect of treatment 1 (\(\beta_1^{2SLS}\)). The presence of this response type in the population would be problematic. The response type

\[
s(z) = \begin{cases} 
0 & \text{if } z = (0, 0) \\
0 & \text{if } z = (1, 0) \\
2 & \text{if } z = (0, 1) 
\end{cases}
\]

on the other hand, has weight matrix

\[
w_s = \begin{bmatrix} 0 & 0 \\ 0 & 2 \end{bmatrix}
\]

The effect of treatment 2 for this response type only contributes to the estimated effect of treatment 2. Two-stage least squares thus assigns proper weights on this response type’s average treatment effects. \(\square\)

Under homogeneous effects, or homogenous responses to the instruments, the weight 2SLS assigns on the treatment effects of a particular response type is not a cause of concern. By the following corollary of Proposition 1, the off-diagonal weights are zero on average and the diagonal weights are, on average, positive: \(^{15}\)

\[
w_s = \text{Var}(P)^{-1} \text{Cov}(P, v_s(Z)) = \left(\frac{1}{9} \begin{bmatrix} 0.32 & -0.2 \\ -0.2 & 0.5 \end{bmatrix}\right)^{-1} \frac{1}{9} \begin{bmatrix} 0.4 & -0.4 \\ -1 & 1 \end{bmatrix} = \begin{bmatrix} 0 & 0 \\ -2 & 2 \end{bmatrix}
\]

\(^{14}\)For ease of reading, we present results for \(\beta_1^{2SLS}\) only. By symmetry, analogous results hold for \(\beta_2^{2SLS}\).

\(^{15}\)For ease of reading, we present results for \(\beta_1^{2SLS}\) only. By symmetry, analogous results hold for \(\beta_2^{2SLS}\).
Corollary 1. Under Assumptions 1 and 2, the 2SLS estimand $\beta_{12SLS}^s$ is a weighted sum of $\beta_1^s$ and $\beta_2^s$ where the weights on the first sum to one and the weights on the latter sum to zero.

Thus, if $\beta^s = \beta$ for all $s \in \mathcal{S}$, we get $\beta_{2SLS}^s = \beta$. Also, if the population consists of only one response type $s$, we get $\beta_{2SLS}^s = \beta^s$. But under heterogeneous effects and heterogeneous responses to the instruments the estimated effect of treatment 1 might be contaminated by the effect of treatment 2. This makes interpreting 2SLS estimates hard. Ideally, we would like the diagonal elements of $w_s$ to be non-negative, and the off-diagonal elements of $w_s$ to be zero for all $s$. Only in that case can we interpret the 2SLS estimate of the effect of treatment 1 as a positively weighted average of the effect of treatment 1 under heterogeneous effects. Throughout the rest of the paper, we say that the 2SLS estimate of the effect of treatment 1 assigns proper weights if the following holds:

Definition 1. The 2SLS estimate of the effect of treatment 1 assigns proper weights if for each $s \in \text{supp}(\mathcal{S})$, the 2SLS estimand $\beta_{12SLS}^s$ places non-negative weights on $\beta_1^s$ and zero weights on $\beta_2^s$.

We say that 2SLS assigns proper weights if it assigns proper weights for both treatment effect estimates. Note that we do not require the estimand to assign equal weights to all response types.\footnote{One can allow for always-takers and never-takers.} Requiring 2SLS to assign proper weights is equivalent to requiring 2SLS to be weakly causal (Blandhol et al., 2022) under arbitrary heterogeneous effects, i.e., the 2SLS provides estimates with the correct sign.\footnote{Such a criterion would essentially rule out the use of 2SLS beyond the just-identified case discussed in Section 3.1: As noted by Heckman & Vytlacil (2007), 2SLS assigns higher weights to response types that are more influenced by the instruments, producing a weighted average that is not necessarily of policy interest. See Section 4.2 for alternatives to 2SLS that seek to target more policy relevant treatment effects.} For 2SLS to assign proper weights, the matrix $w_s$ must be a non-negative diagonal matrix for all $s$. When is this the case? To gain intuition, note that the formula determining $w_s$ is the same as the coefficients obtained from hypothetical linear regressions of the potential treatments $\{s_1(Z), s_2(Z)\}$ on the predicted treatment indicators $P$. In other words, the weights on a response type’s average treatment effects in the 2SLS estimand have the same sign as the partial correlations between the response type’s potential treatments and the predicted treatment across values of the instrument.\footnote{See Section B.1.} The following condition thus ensures that $\beta_{12SLS}^s$ assigns non-negative weights on $\beta_1^s$ for all $s$.

\footnote{The partial correlation between $P_1$ and $s_1(Z)$ given $P_2$ is the Pearson correlation coefficient between the residuals from regressing $P_1$ on $P_2$ and the residuals from regressing $s_1(Z)$ on $P_2$. In other words, it is the correlation between $P_1$ and $s_1(Z)$ when linearly controlling for $P_2$. This partial correlation has the same sign as the coefficient on $P_1$ in a linear regression of $s_1(Z)$ on $P_1$ and $P_2$.}
**Assumption 3.** (Average Conditional Monotonicity). For all $s$, the partial correlation between $P_1$ and $s_1(Z)$ given $P_2$ is non-negative.

**Corollary 2.** Under Assumptions 1 and 2, the weight on $\beta_1^s$ in $\beta_1^{2SLS}$ is non-negative for all $s$ if and only if Assumption 3 holds.

Assumption 3 requires that, after controlling linearly for predicted treatment 2, the instrument values at which a given agent selects treatment 1 can not have a lower than average predicted treatment 1. Intuitively, Assumption 3 requires that, controlling linearly for predicted treatment 2, there is a positive correlation between predicted treatment 1 and potential treatment 1 for each agent across values of the instruments. We refer to the condition as *average conditional monotonicity* since it generalizes the average monotonicity condition defined by Frandsen *et al.* (2023) to multiple treatments. In particular, the positive relationship between predicted treatment and potential treatment only needs to hold “on average” across realizations of the instruments. Thus, an agent might be a “defier” for some pairs of instrument values as long as she is a “complier” for sufficiently many other pairs. Informally, we can think of Assumption 3, as requiring the partial effect of $P_1$ on treatment 1 to be, on average, non-negative for all agents. In order to make sure that all the weights on $\beta_2^s$ in $\beta_1^{2SLS}$ are zero we need the following much stronger condition.

**Assumption 4.** (No Cross Effects). For all $s$ the partial correlation between $P_1$ and $s_2(Z)$ given $P_2$ is zero.

**Corollary 3.** Under Assumptions 1 and 2, the weight on $\beta_2^s$ in $\beta_1^{2SLS}$ is zero for all $s$ if and only if Assumption 4 holds.

This condition requires that, linearly controlling for predicted treatment 2, there is no correlation between predicted treatment 1 and potential treatment 2 for each agent. Informally, we can think of Assumption 4 as requiring there to be no partial effect of $P_1$ on treatment 2 for all agents. Intuitively, if $P_1$ has a tendency to push certain agents into or out of treatment 2—even after controlling linearly for $P_2$—the estimated effect of treatment 1 will be contaminated by these agents’ treatment effect of treatment 2. Assumptions 3 and 4 are necessary and sufficient conditions to ensure that 2SLS assigns proper weights. This is our main result:

**Corollary 4.** The 2SLS estimate of the effect of treatment 1 assigns proper weights under Assumptions 1 and 2 if and only if Assumptions 3 and 4 hold.

---

20 With one treatment, Assumption 3 reduces to $E[P_1 | s(Z) = 1] \geq E[P_1] \Leftrightarrow \text{Cov}(P_1, s_1(Z)) \geq 0$ which coincides with the average monotonicity condition in Frandsen *et al.* (2023). Under Assumption 4, Assumption 3 is equivalent to the correlation between $P_1$ and $s_1(Z)$ being non-negative (without having to condition on $P_2$).
Informally, 2SLS assigns proper weights if and only if for all agents and treatments $k$, (i) increasing predicted treatment $k$ tend to weakly increase adoption of treatment $k$ and (ii) conditional on predicted treatment $k$, increases in predicted treatment $l \neq k$ do not tend to push the agent into or out of treatment $k$. In Appendix Section B.5, we show how our conditions relate to the Imbens-Angrist (IA) monotonicity condition (Imbens & Angrist, 1994). In particular, we show that if IA monotonicity holds, then 2SLS assigns proper weights under an easily testable linearity condition.

2.3 Testing the Identification Conditions

Assumptions 3 and 4 can not be directly assessed since we only observe $s(z)$ for the observed instrument values. But the assumptions do have testable implications. We here provide two testable predictions. The first is a generalization of Kitagawa (2015):\(^{21}\)

**Proposition 2.** Maintain Assumptions 1 and 2 and let $B \subset \text{supp}(Y)$. If 2SLS assigns proper weights then

$$\text{Var}(P)^{-1} \text{Cov}(P, 1_{Y \in B}D)$$

is a non-negative diagonal matrix.

For instance, for a binary outcome variable $Y \in \{0, 1\}$, Proposition 2 implies that $\text{Var}(P)^{-1} \text{Cov}(P, YD)$ and $\text{Var}(P)^{-1} \text{Cov}(P, (1 - Y)D)$ are non-negative diagonal matrices. That $\text{Var}(P)^{-1} \text{Cov}(P, YD)$ is non-negative diagonal can be tested by running the following regressions\(^ {22}\)

$$YD_1 = \varphi_1 + \vartheta_{11}P_1 + \vartheta_{12}P_2 + v_1$$
$$YD_2 = \varphi_2 + \vartheta_{21}P_1 + \vartheta_{22}P_2 + v_2$$

and test whether $\vartheta_{11}$ and $\vartheta_{22}$ are non-negative and whether $\vartheta_{12} = \vartheta_{21} = 0$. Intuitively, if no-cross effects is satisfied, $P_2$ can not increase nor decrease the share of agents with $T = 1$ and $Y(1) = 1$, after controlling for $P_1$. For a continuous outcome variable, the implications could be tested using a Kolmogorov–Smirnov test statistic as described in Kitagawa (2015). Alternatively, one can apply the method of Mourifie & Wan (2017)—first transforming the Proposition 2 condition to conditional moment inequalities and then apply Chernozhukov et al. (2013). In that case, the relevant conditional moment conditions are that

$$\text{Var}(P)^{-1} \text{E}[(P - \text{E}[P]) D \mid Y = y]$$

\(^{21}\)We thank the associate editor for suggesting this test.

\(^{22}\)Whether $\text{Var}(P)^{-1} \text{Cov}(P, (1 - Y)D)$ is non-negative diagonal can be tested by using $(1 - Y)D_1$ and $(1 - Y)D_2$ as outcome variables.
is non-negative diagonal for all $y \in \text{supp}(Y)$. Note that this test is a joint test of Assumptions 1, 3, and 4. If the test rejects, it could be due to a violation of exogeneity or the exclusion restriction.

The second test relies on having access to “pre-determined” covariates. In particular, let $X \in \{0, 1\}$ be a random variable such that $Z \perp (X, S)$. Informally, $X$ is a pre-determined variable not influenced by or correlated with the instrument. We then have the following testable prediction.

**Proposition 3.** Maintain Assumptions 1 and 2 and assume $X \in \{0, 1\}$ with $Z \perp (X, S)$. If 2SLS assigns proper weights then

$$\text{Var}(P \mid X = 1)^{-1} \text{Cov}(P, D \mid X = 1)$$

is a diagonal non-negative matrix.

This prediction can be tested by running the following regressions

$$D_1 = \gamma_1 + \eta_{11}P_1 + \eta_{12}P_2 + \varepsilon_1$$
$$D_2 = \gamma_2 + \eta_{21}P_1 + \eta_{22}P_2 + \varepsilon_2$$

for the sub-sample $X = 1$ and testing whether $\eta_{11}$ and $\eta_{22}$ are non-negative and whether $\eta_{12} = \eta_{21} = 0$. The predicted treatments $P$ can be estimated from a linear regression of the treatments on the instruments on the whole sample—a standard first-stage regression. This test thus assesses the relationship between predicted treatments and selected treatments in subsamples. If 2SLS assigns proper weights, we should see a positive relationship between treatment $k$ and predicted treatment $k$ and no statistically significant relationship between treatment $k$ and predicted treatment $l \neq k$ in (pre-determined) subsamples. Note that failing to reject that $\text{Var}(P \mid X = 1)^{-1} \text{Cov}(P, D \mid X = 1)$ is non-negative diagonal across all observable pre-determined $X$ does not prove that 2SLS assigns proper weights, even in large samples: There might always be unobserved

$$\text{Cov}(P, 1_{Y \in B}D) = E[P1_{Y \in B}D] - E[P]E[1_{Y \in B}D]$$
$$= (E[PD \mid Y \in B] - E[P]E[D \mid Y \in B])\Pr[Y \in B]$$
$$= E[PD - E[P]D \mid Y \in B]\Pr[Y \in B]$$

We have already assumed $Z \perp S$ (Assumption 1). The condition $Z \perp (X, S)$ requires, in addition, that $Z$ is independent of the joint distribution of $X$ and $S$. If the instrument $Z$ is truly random, then any variable $X$ that pre-dates the randomization satisfies $Z \perp (X, S)$. For instance, in a randomized control trial, $X$ can be any pre-determined characteristic of the individuals in the experiment.

The one-treatment version of this test is commonly applied in the literature (e.g., Dobbie et al. 2018; Bhuller et al. 2020) and formally justified by Frandsen et al. (2023).

If the test is applied on various subsamples, $p$-values need to be adjusted to account for multiple testing. It is only meaningful to apply the test on subsamples. The condition is always mechanically satisfied in the full sample.
pre-determined characteristics $X$ such that $\text{Var} (P \mid X = 1) ^{-1} \text{Cov} (P, D \mid X = 1)$ is not non-negative diagonal.\footnote{Similarly, in randomized control trials, showing that treatment is uncorrelated with observed pre-determined covariates does not prove that treatment is randomly assigned.}

3 Special Cases

In this section, we first provide general identification results in the just-identified case and derive the implied restrictions on choice behavior under ordered or unordered treatment effects. Then, we provide identification results for a standard threshold-crossing model—an example of an overidentified model with ordered treatment effects.

3.1 The Just-Identified Case

3.1.1 Identification Results

The standard application of 2SLS involves one binary instrument and one binary treatment. In this section, we apply the results in Section 2 to show how this canonical case generalizes to the case with three possible treatments and three possible values of the instrument.\footnote{The results generalize to $n$ possible treatments and $n$ possible values of the instruments. See Section B.2 in the Appendix.} This case is the just-identified case with multiple treatments: the number of distinct values of the instruments equals the number of treatments. In particular, assume we have an instrument, $V \in \{0, 1, 2\}$, from which we create two mutually exclusive binary instruments $Z = (Z_1, Z_2)^T$ with $Z_v \equiv 1 \{V = v\}$.\footnote{There are other ways of creating two instruments from $V$. For instance, one could define $Z_1 = 1 \{V \geq 1\}$ and $Z_2 = 1 \{V = 2\}$. Such a parameterization would give exactly the same 2SLS estimate. We focus on the case of mutually exclusive binary instruments since it allows for an easier interpretation of our results.} We maintain Assumptions 1 and 2. This setting is common in applications. For instance, $Z_v$ might be an inducement to take up treatment $v$, as considered by Behaghel et al. (2013). Under which conditions does the multivariate 2SLS assign proper weights in this setting? It turns out that proper identification is achieved only when each instrument affects only one treatment. For simpler exposition, we represent in this section the response types $s$ as functions $s : \{0, 1, 2\} \rightarrow \{0, 1, 2\}$ where $s(v)$ is the treatment selected by response type $s$ when $V$ is set to $v$. Thus, $s(v)$ is the potential treatment when $Z_v = 1$.

Proposition 4. 2SLS assigns proper weights in the above model if and only if there exists a one-to-one mapping $f : \{0, 1, 2\} \rightarrow \{0, 1, 2\}$ between instruments and treatments, such that for all $k \in \{1, 2\}$ and $s \in \mathcal{S}$ either $s_k(v) = 0$ for all $v \in \{0, 1, 2\}$, $s_k(v) = 1$ for all $v \in \{0, 1, 2\}$, or $s_k(v) = 1 \Leftrightarrow f(v) = k$.

In words, for each agent $i$ and treatment $k$, either $i$ never takes up treatment $k$, always takes up treatment $k$, or takes up treatment $k$ if and only if $f(V) = k$. Each instrument

\footnote{In words, for each agent $i$ and treatment $k$, either $i$ never takes up treatment $k$, always takes up treatment $k$, or takes up treatment $k$ if and only if $f(V) = k$. Each instrument}
Table 1: Allowed Response Types in the Just-Identified Case.

| Response Type          | \((s(0), s(1), s(2))\) |
|------------------------|------------------------|
| Never-taker            | (0, 0, 0)              |
| Always-1-taker         | (1, 1, 1)              |
| Always-2-taker         | (2, 2, 2)              |
| 1-complier             | (0, 1, 0)              |
| 2-complier             | (0, 0, 2)              |
| Full complier          | (0, 1, 2)              |

\(Z_v\) is thus associated with exactly one treatment \(D_{f(v)}\). For ease of exposition, we can thus, without loss of generality, assume that the treatments and instruments are ordered in the same way:

**Assumption 5.** Assume instrument values are labeled such that \(f(k) = k\) for all \(k \in \{0, 1, 2\}\) where \(f\) is the unique mapping defined in Proposition 4.

To see the implications of Proposition 4, consider the response types defined in Table 1. It turns out that, for 2SLS to assign proper weights, the population can not consist of any other response type:

**Corollary 5.** 2SLS with \(D_t = 1[T = t]\) assigns proper weights under Assumption 5 if and only if all agents are either never-takers, always-1-takers, always-2-takers, 1-compliers, 2-compliers, or full compliers.

Behaghel et al. (2013) show that these assumptions are sufficient to ensure that 2SLS assigns proper weights. See also Kline & Walters (2016) for similar conditions in the case of multiple treatments and one instrument.\(^{30}\) Proposition 4 shows that these conditions are not only sufficient but also necessary, after a possible permutation of the instruments. These response types are characterized by instrument \(k\) not affecting treatment \(l \neq k\)—no cross effects. Under the response type restrictions of Table 1, 2SLS identifies the average treatment effect of treatment 1 for the combined population of 1-compliers and full compliers and the average treatment effect of treatment 2 for the combined population of 2-compliers and full compliers.\(^{31}\) These treatment effects coincides with the treatment effects identified by the method of Heckman & Pinto (2018).\(^{32}\)

\(^{30}\)Equation 1 in Kline & Walters (2016), generalized to two instruments, also describes the same response types: \(s(1) \neq s(0) \Rightarrow s(1) = 1\) and \(s(2) \neq s(0) \Rightarrow s(2) = 2\). The requirement that the instruments can cause individuals to switch only from “no treatment” (the excluded treatment) into some treatment is shared by Rose & Shem-Tov (2021b)’s extensive margin compliers only assumption.

\(^{31}\)Thus, in the just-identified case, whenever 2SLS assigns proper weights it also assigns equal weight to all complier types. This result does not generalize to the overidentified case.

\(^{32}\)The methods proposed by Heckman & Pinto (2018) can be applied to identify further parameters of interest. In particular, if we denote always-1-takers by \(s_{A1}\), always-2-takers by \(s_{A2}\), never-takers by \(s_N\), 1-compliers by \(s_{C1}\), 2-compliers by \(s_{C2}\), and full compliers by \(s_F\), their method allows to identify the counterfactuals \(E[Y(1) | S = s_{A1}]\), \(E[Y(2) | S = s_{A2}]\), \(E[Y(1) | S \in \{s_{C1}, s_F\}]\), \(E[Y(0) | S \in \{s_{C1}, s_F\}]\), \(E[Y(0) | S \in \{s_{C2}, s_F\}]\), \(E[Y(1) | S \in \{s_N, s_{C1}\}]\), \(E[Y(2) | S \in \{s_N, s_{C2}\}]\), and \(E[Y(0) | S \in \{s_N, s_{C2}\}]\).
3.1.2 Testing For Proper Weighting in Just-Identified Models

By Proposition 4, there must exist a permutation of the instruments such that instrument \( k \) affects only treatment \( k \). This result gives rise to a more powerful test applicable in just-identified models: Testing whether, possibly after a permutation of the instruments, treatment \( k \) is affected only by instrument \( k \) in first-stage regressions.\(^{33}\) This test can be applied both in the full population and in subsamples. Formally

**Proposition 5.** Assume \( X \in \{0,1\} \) with \( V \perp X \mid S \). If Assumptions 3 and 4 hold then there is a permutation of the instruments \( f : \{0,1,\ldots,n\} \rightarrow \{0,1,\ldots,n\} \) such that

\[
\text{Var} \left( Z^* \mid X = 1 \right)^{-1} \text{Cov} \left( Z^*, D \mid X = 1 \right)
\]

is a diagonal non-negative matrix where \( Z^* = (Z^*_1, Z^*_2, \ldots, Z^*_n)^T \) with \( Z^*_v \equiv 1 \) \([V = f(v)]\).

As in Section 2.3, this prediction can be tested by running a first-stage regression\(^{34}\)

\[
D = \gamma + \eta Z^* + \varepsilon
\]

on the sub-sample \( X = 1 \) and testing whether \( \eta \) is a non-negative diagonal matrix. In other words, for 2SLS to assign proper weights, there must exist a permutation of the instruments such that there is a positive relationship between treatment \( k \) and instrument \( k \) and no statistically significant relationship between treatment \( k \) and instrument \( l \neq k \) in all (pre-determined) subsamples.

3.1.3 Choice-Theoretic Characterization

What does the condition in Proposition 4 imply about choice behavior? To analyze this, we use a random utility model.\(^{35}\) Assume that response type \( s \)'s indirect utility from choosing treatment \( k \) when \( V = v \) is \( I_{ks}(v) \) and that \( s \) selects treatment \( k \) if \( I_{ks}(v) > I_{ls}(v) \) for all \( l \neq k \).\(^{36}\) The implicit assumptions about choice behavior differ

and \( \text{E} \left[ Y(0) \mid S \in \{s_N, s_C\} \right] \). Moreover, their method allows for the characterization of always-1-takers, always-2-takers, and the following combined populations: 1-compliers and full compliers, 2-compliers and full compliers, 1-compliers and never-takers, and 2-compliers and never-takers.\(^{33}\)

The researcher would have a clear hypothesis about which instrument is supposed to affect each treatment, avoiding the need to run the test for all possible permutations.\(^{34}\)

Heinesen et al. (2022) show that the coefficients from a first-stage regression on the full sample can be used to partially identify violations of “irrelevance” and “next-best” assumptions invoked in Kirkeboen et al. (2016). Their Proposition 4 implies that \( \eta \) is non-negative if their monotonicity, irrelevance, and next-best assumptions hold. We show that this test is a valid test not only of their invoked assumptions, but also more generally of whether 2SLS assigns proper weights. Moreover, we show that the test can be applied on subsamples.

In a seminal article, Vytlacil (2002) showed that the Imbens & Angrist (1994) monotonicity condition is equivalent to assuming that agents’ selection into treatment can be described by a random utility model where agents select into treatment when a latent index crosses a threshold. In this section, we seek to provide similar characterizations of the condition in Proposition 4.

Since all agents of the same response type have identical behavior, it is without loss of generality to assume that all agents of a response type have the same indirect utility function. We assume response
according to which treatment effects we seek to estimate. We here consider the cases of ordered and unordered treatment effects.

**Unordered Treatment Effects.** What are the implicit assumptions on choice behavior when \( \beta = (Y(1) - Y(0), Y(2) - Y(0))^T \)? In this case, instrument \( k \) can affect only treatment \( k \). Thus, instrument \( k \) can not impact the utility of treatment \( l \neq k \) in any way that changes choice behavior. Also, for instrument \( k \) to impact treatment \( k \) without impacting treatment \( l \), it must be that instrument \( k \) can change treatment status only from treatment 0 to treatment \( k \) and vice versa. Thus, essentially, Proposition 4 requires that only instrument \( k \) affects the indirect utility of treatment \( k \), and that the excluded treatment always is at least the next-best alternative.\(^{37}\)

**Proposition 6.** 2SLS with \( D_t = 1 \) \([T = t]\) assigns proper weights if and only if there exists \( u_{ks} \in \mathbb{R} \) and \( \mu_{ks} \geq 0 \) such that \( s(v) = \text{arg max}_k I_{ks}(v) \) with\(^{38}\)

\[
I_{0s}(v) = 0
\]

\[
I_{ks}(v) = u_{ks} + \mu_{ks} 1[v = k]
\]

\[
I_{ks}(v) > I_{0s}(v) \Rightarrow I_{ls}(v) < I_{0s}(v)
\]

for all \( s \in \mathcal{S}, v \in \{0, 1, 2\}, k, l \in \{1, 2\} \) with \( l \neq k \).

The excluded treatment is, thus, always “in between” the selected treatment and all other treatments in this selection model. Since next-best alternatives are not observed, there are selection models consistent with 2SLS assigning proper weights where other alternatives than the excluded treatment are occasionally next-best alternatives. But when indirect utilities are given by \( I_{0s} = 0 \) and \( I_{ks} = u_{ks} + \mu_{ks} 1[v = k] \), this can only happen when \( s \) always selects the same treatment, in which case the identity of the next-best alternative is irrelevant.\(^{39}\)

In which settings can a researcher plausibly argue that the excluded treatment is always the best or the next-best alternative? A natural type of setting is when there are three treatments and the excluded treatment is “in the middle”. For example, consider estimating the causal effect of attending “School 1” and “School 2” compared to attending “School 0” on student outcomes. Here, School 0 is the excluded treatment. Assume students are free to choose their preferred school. If School 0 is geographically located

\(^{37}\)Lee & Salanié (2023) consider a similar random utility model. Their Additive Random Utility Model in combination with strict one-to-one targeting and the assumption that all treatments except \( T = 0 \) are targeted gives \( I_{ks}(v) = u_{ks} + \mu_k 1[v = k] \) for constants \( u_{ks} \) and \( \mu_k > 0 \). As shown by Lee & Salanié (2023), these assumptions are generally not sufficient to point-identify local average treatment effects. Our result shows that—in the just-identified case—local average treatment effects can be identified if we additionally assume that all agents have the same next-best alternative.

\(^{38}\)The assumption that \( \text{arg max}_k I_{ks}(z) \) is a singleton implies that preferences are strict.

\(^{39}\)See the proof of Proposition 6.
Figure 1: Example with Excluded Treatment Always the Best or the Next-Best Alternative.

Note: Example of setting where the excluded treatment could plausibly be argued to be the best or the next-best alternative for all agents. Here the treatments are schools, with School 0 being the excluded treatment. Agents are students choosing which school to attend. The open dots indicate locations of schools, and the closed dots indicate the location of two example students. If students care sufficiently about travel distance, School 0 will be either the best or the next-best alternative for all students.

in between School 1 and School 2, as depicted in Figure 1, and students care sufficiently about travel distance, it is plausible that School 0 is the best or the next-best alternative for all students. For instance, student A in Figure 1, who lives between School 1 and School 0, is unlikely to prefer School 2 over School 0. Similarly, student B in Figure 1 is unlikely to have School 1 as her preferred school. If we believe no students have School 0 as their least favorite alternative and we have access to random shocks to the utility of attending Schools 1 and 2 multivariate 2SLS can be safely applied.40

Kirkeboen et al. (2016) and a literature that followed exploit knowledge of next-best alternatives for identification in just-identified models. In the Appendix Section B.7, we show that the conditions invoked in this literature are not only sufficient for 2SLS to assign proper weights but also essentially necessary.41 Thus, to apply 2SLS in just-identified models with arbitrary heterogeneous effects, researchers have to either directly observe next-best alternatives or make assumptions about next-best alternatives based on institutional and theoretical arguments.

Ordered Treatment Effects. What are the implicit assumptions on choice behavior when \( \beta = (Y(1) - Y(0), Y(2) - Y(1))^T \)? In that case, instrument \( k \) can influence only treatment indicator \( D_k \)—whether \( T \geq k \). Thus, instrument \( k \) can not impact relative utilities other than \( I_{si} - I_{li} \) for \( s \geq k \) and \( l < k \) in a way that changes choice behavior. Thus, without loss of generality, we can assume that instrument \( k \) increases the utility of treatments \( t \geq k \) by the same amount while keeping the utility of treatments \( t < k \) constant. But this assumption is not sufficient to prevent instrument \( k \) from influencing treatment indicators other than \( D_k \). In addition, we need that preferences are single-peaked:

---

40Another example where the excluded treatment is always the best or the next-best alternative is when agents are randomly encouraged to take up one treatment and can not select into treatments they are not offered. In that case, one might estimate the causal effects of each treatment in separate 2SLS regressions on the subsamples not receiving each of the treatments. But when control variables are included in the regression, estimating a 2SLS model with multiple treatments can improve precision.

41The Kirkeboen et al. (2016) assumptions can be relaxed by allowing for always-takers.
Figure 2: Single-Peaked Preferences and Ordered Treatment Effects.

(a) Preferences not Single-Peaked.

(b) Single-Peaked Preferences.

Note: Indirect utilities of response type $s$ over treatments 0–2 when instrument 1 is turned off (solid dots) and on (open dots). Figure (a) shows how instrument 1 might affect $D_2 = 1 \ [T \geq 2]$ when preferences are not single-peaked.

Definition 2 (Single-Peaked Preferences). Preferences are single-peaked if for all $s \in \mathcal{S}$, $z \in \mathcal{Z}$, and $k, l, r \in \{1, \ldots, n\}$

$$k > l > r \text{ and } I_{ks} (v) > I_{ls} (v) \Rightarrow I_{ls} (v) \geq I_{rs} (v)$$

$$k < l < r \text{ and } I_{ls} (v) < I_{rs} (v) \Rightarrow I_{ks} (v) \geq I_{ls} (v)$$

To see this, consider Figure 2a. The solid dots indicate the indirect utilities of agent $i$ over treatments 0–2 when instrument 1 is turned off ($V = 0$). In this case, the agent’s preferences has two peaks: one at $k = 0$ and another at $k = 2$. If instrument 1 increases the utilities of treatments $k \geq 1$ by the same amount, the agent might change the selected treatment from $T = 0$ to $T = 2$ as indicated by the open dots. This change impacts both $D_1$ and $D_2$. In Figure 2b, however, where preferences are single-peaked, a homogeneous increase in the utility of treatments $k \geq 1$ can never impact any other treatment indicator than $D_1$. We thus have the following result.

Proposition 7. 2SLS with $D_t = 1 \ [T \geq t]$ assigns proper weights if and only if there exists $u_{ks} \in \mathbb{R}$ and $\mu_{ks} \geq 0$ such that $s (v) = \arg \max_k I_{ks} (v)$ with

$$I_{0s} = 0$$

$$I_{ks} (v) = u_{ks} + \mu_{ks} 1 \ [k \geq v]$$

for all $s \in \mathcal{S}$, $v \in \{0, 1, 2\}$, and $k \in \{1, 2\}$ and the preferences are single-peaked.
3.2 A Threshold-Crossing Model

In several settings, treatments have a clear ordering, and assignment to treatment could be described by a latent index crossing multiple thresholds. For instance, treatments could be grades and the latent index the quality of the student’s work. When estimating returns to education, treatments might be different schools, thresholds the admission criteria, and the latent index the quality of the applicant. In the context of criminal justice, treatments might be conviction and incarceration as in Humphries et al. (2023) or years of prison as in Rose & Shem-Tov (2021a) and the latent index the severity of the crime committed. If the researcher has access to random variation in these thresholds, for instance through random assignment to graders or judges who agree on ranking but use different cutoffs, then the identification of the causal effect of each consecutive treatment by 2SLS could be possible.

We here present a simple and easily testable condition under which 2SLS assigns proper weights in such a threshold-crossing model. Our model is a simplified version of the model considered by Heckman & Vytlacil (2007), Section 7.2.43 We assume agents differ by an unobserved latent index $U$ and that treatment depends on whether $U$ crosses certain thresholds. In particular, assume there are thresholds $g_1(Z) < g_2(Z)$ such that

$$T = \begin{cases} 
0 & \text{if } U < g_1(Z) \\
1 & \text{if } g_1(Z) \leq U < g_2(Z) \\
2 & \text{if } U \geq g_2(Z) 
\end{cases}$$

We are interested in the ordered treatment effects $\beta = (Y(1) - Y(0), Y(2) - Y(1))^T$, using as treatment indicators $D_t = 1[U \geq g_t(Z)]$ for $t \in \{1, 2\}$. Assume the first stage is correctly specified:

**Assumption 6.** *(First Stage Correctly Specified.)* Assume that $E[D_k | Z]$ is a linear function of $Z$ for all $k$.

This assumption is always true, for instance, when $Z$ is a set of mutually exclusive binary instruments. We maintain Assumptions 1 and 2. We then have:45

---

42The model below applies in this setting if applicants agree on the ranking of schools and schools agree on the ranking of candidates.

43Heckman & Vytlacil (2007) focus on identifying marginal treatment effects and discuss what 2SLS with a multivalued treatment identifies in this model, but do not consider 2SLS with multiple treatments. Lee & Salanié (2018) consider a similar model with two thresholds where agents are allowed to differ in two latent dimensions.

44This specification is routinely applied to study ordered choices (Greene & Hensher, 2010).

45A similar result is found in a contemporaneous project by Humphries et al. (2023) in the context of the random judge design. They suggest that the required linearity condition between $P_1$ and $P_2$ can be relaxed by running a 2SLS specification with $D_1$ as a single treatment indicator and $P_1$ as the instrument while flexibly controlling for $P_2$ (and vice versa). They further show that if treatment assignment depends on several unobserved latent indices—instead of just one—2SLS does not, in general, assign proper weights.
Proposition 8. 2SLS assigns proper weights in the above model if $E[P_k \mid P_l]$ is linear in $P_l$ for all $k, l \in \{1, 2\}$.

Thus, when treatment can be described by a single index crossing multiple thresholds, and we have access to random shocks to these thresholds, multivariate 2SLS estimates of the effect of crossing each threshold will be a positively weighted sum of individual treatment effects. The required linearity condition between predicted treatments can easily be tested empirically. In Proposition B.7, we show that 2SLS does not assign proper weights if $E[P_k \mid P_l]$ is non-linear for one $k$ and $l$ and there is a positive density of agents at all values of $U \in [0, 1]$. A linear relationship between predicted treatments is thus close to necessary for 2SLS to assign proper weights in the above model.

4 If the Conditions Fail

If either Assumption 3 or 4 are violated, 2SLS is not weakly causal—provide estimates with the correct sign (Blandhol et al., 2022)—under arbitrary heterogeneous effects. The researcher then has two choices: Either impose assumptions on treatment effect heterogeneity or select an alternative estimator.

4.1 Assumptions on Treatment Effect Heterogeneity

If the researcher is willing to make assumptions restricting the degree of treatment effect heterogeneity, 2SLS could still be weakly causal even when Assumption 3 or 4 are violated. For instance, if treatment effects do not systematically differ across response types, 2SLS is weakly causal without any restrictions on response types. Furthermore, if the amount of “selection on gains” and the violations of Assumptions 3 and 4 are both moderate, 2SLS could still be weakly causal. See Section B.4 for a formal analysis.

4.2 Alternative Estimators

The econometric literature has proposed several ways to identify multiple treatment effects when 2SLS fails. The most general method is provided by Heckman & Pinto (2018). Their method allows the researcher to learn which treatment effects are identified—and how they are identified—for any given restriction on response types. The identification results apply to all settings with discrete-valued instruments—also in cases where 2SLS is not valid. For instance, if we allow for the presence of the response type $(s(0), s(1), s(2)) = (2, 1, 2)$ in addition to the response types in Table 1 in the Section 3.1 model, 2SLS no longer assigns proper weights but the method of Heckman & Pinto (2018) still recovers causal effects. Heckman & Pinto (2018) and Pinto (2021) discusses

---

46See Section B.1.
how to use revealed preference analysis to restrict the possible response types.\textsuperscript{47} In a related approach, \cite{Lee2023} show how assuming that certain instrument values \textit{targets} certain treatments can lead to partial identification of treatment effects.

Another important strand of methods to identify multiple treatment effects relies on continuous instruments and the marginal treatment effects framework brought forward by \cite{Heckman1999, Heckman2005}. In the case of ordered treatment effects, \cite{Heckman2007} show how separate treatment effects can be identified if treatment is determined by a single latent index crossing multiple thresholds and the researcher has access to shocks to each threshold. Using this method, separate treatment effects can be recovered in the Section 3.2 model also when predicted treatments are not linearly related.\textsuperscript{48} Another advantage of the methods based on marginal treatment effects is that they allow for the calculation of treatment effects that are more policy-relevant than the weighted average produced by 2SLS. In the context of unordered treatments, \cite{Heckman2007, Heckman2008} show that analogous assumptions can recover the causal effect of a given treatment versus the next-best alternative.\textsuperscript{49} In their framework, causal effects between two specified treatments can be identified by focusing on instrument realizations for which the probability of taking up all other treatments is zero—if such instrument realizations exist.\textsuperscript{50} \cite{Lee2018} show how the knowledge of the exact threshold rules can be used to identify the effect of one treatment versus another treatment without relying on such “identification at infinity” arguments. \cite{Mountjoy2022} has recently developed a method based on continuous instruments where knowledge of the exact threshold rules is not required either. Instead, identification is achieved by two relatively weak assumptions: \textit{Partial unordered monotonicity} and \textit{comparative compliers}.\textsuperscript{51}

\textsuperscript{47}Pinto (2021) combines revealed preference analysis with a functional form assumption to identify causal effects in an RCT with multiple treatments and non-compliance.

\textsuperscript{48}Intuitively, the causal effect of treatment 1 can be obtained by varying $P_1$ while keeping $P_2$ fixed. The 2SLS requires $E[P_1 | P_2]$ to be linear since instead of keeping $P_2$ fixed it controls \textit{linearly} for $P_2$.

\textsuperscript{49}A 2SLS version of this approach would be to run 2SLS with a single binarized treatment indicator. The recovered treatment effect—the effect of receiving a given treatment compared to a mix of alternative treatments—might sometimes be a parameter of policy interest. \cite{Heckman2018} extend this result to discrete instruments. In particular, they show that the causal effect of a given treatment versus the next-best alternative is identified under \textit{unordered monotonicity}.

\textsuperscript{50}In the Section 5 application, one could identify the causal effect of incarceration versus conviction by studying cases assigned to judges that never acquits—if such judges exist. The 2SLS equivalent of this approach is to run 2SLS on the sample of judges that never acquits.

\textsuperscript{51}Mountjoy (2022) considers a case with three treatments and two continuous instruments. Partial unordered monotonicity requires that instrument $k$ weakly increases up-take of treatment $k$ and weakly reduces up-take of all other treatments for all agents. Partial unordered monotonicity and our Assumptions 3 and 4 do not nest each other. On the one hand, partial unordered monotonicity allows for heterogeneity in how instrument $k$ induces agents out of treatments $l \neq k$ in ways that would violate our no cross effect condition. On the other hand, average conditional monotonicity allows monotonicity to be violated for some instrument pairs in ways that would violate partial unordered monotonicity. A main advantage of partial unordered monotonicity, however, is that it has a clear economic interpretation, which Assumptions 3 and 4 lack. Comparative compliers requires that those induced from treatment $l$ to treatment $k$ by a marginal increase in instrument $k$ are similar to those induced from treatment $k$ to treatment $l$ by a marginal increase in instrument $l$. This condition is satisfied in a broad
A final alternative to 2SLS is to explicitly model the selection into treatment using the method of Heckman (1979) (e.g., Kline & Walters 2016). This approach, however, requires the researcher to make distributional assumptions about unobservables.

5 Illustration: The Effects of Incarceration and Conviction

In this section, we illustrate how our results can be applied in practice. In particular, we consider identifying the effects of conviction and incarceration on defendant recidivism using randomly assigned judges as our instrument. The application of 2SLS to this case has recently been considered in several studies (Bhuller et al., 2020; Humphries et al., 2023; Kamat et al., 2023).

Assume the possible treatments are

\[ T \in \{0, 1, 2\} = \{\text{acquittal, non-incarceration conviction, incarceration}\} \]

We consider the treatment indicators \( D_1 = 1 \{T > 0\} \) (conviction) and \( D_2 = 1 \{T = 2\} \) (incarceration). We thus seek to separately identify the effect of conviction versus acquittal and the effect of incarceration versus conviction. As instruments \( Z \), we use randomly assigned judges.\(^{52}\) The predicted treatments \( P_1 \) and \( P_2 \) then equal the rate at which the randomly assigned judge convicts and incarcerates defendants, respectively.

When does 2SLS assign proper weights in this setting? Applying the Section 3.2 model, we get that 2SLS assigns proper weights if judges agree on how to rank cases but use different cutoffs for conviction and incarceration and there is a linear relationship between the judges’ incarceration and conviction rates.\(^{53}\)

Such a restrictive model of judge behavior is, however, not necessary for 2SLS to

\(^{52}\)Formally, \( Z \) is a vector of binary judge indicators where \( Z_k = 1 \) if the case is assigned judge \( k \).

\(^{53}\)In the notation of Section 3.2, \( U \) would represent the “strength” of the case, \( g_1 (Z) \) the conviction cutoff for the randomly chosen judge, and \( g_2 (Z) \) the incarceration cutoff.
assign proper weights. An example of a response type not allowed by the single-index model but satisfying Assumptions 3 and 4 is given in Figure 3a. Here, there are nine judges with their conviction rate \( (P_1) \) on the \( x \)-axis and their incarceration rate \( (P_2) \) on the \( y \)-axis. After controlling linearly for the conviction rate (the blue line), the judges convicting the defendant do not, on average, have a higher nor lower incarceration rate than the judges acquitting the defendant. Thus, the 2SLS estimate of the effect of incarceration places zero weight on this defendant’s effect of conviction. In fact, Assumption 3 and 4 admit a wide range of response types beyond the Figure 3a response type. But no cross effects (Assumption 4) is also a knife-edge condition that can easily be violated by small deviations from the allowed response types. For instance, the response type in Figure 3b violates no cross effects. Note, however, that if response types do not deviate much from the allowed response types and heterogeneous effects are moderate, the bias would still be small (see Section B.4).

6 Conclusion

Two-stage least squares (2SLS) is a common approach to causal inference. We have presented necessary and sufficient conditions for the 2SLS to identify a properly weighted sum of individual treatment effects when there are multiple treatments and arbitrary treatment effect heterogeneity. The conditions require in just-identified models that each instrument only affects one choice margin. In overidentified models, 2SLS identifies ordered treatment effects in a general threshold-crossing model conditional on an easily verifiable linearity condition. Whether 2SLS with multiple treatments should be used depends on the setting. Justifying its use in the presence of heterogeneous effects requires both running systematic empirical tests of the average conditional monotonicity and no cross effects conditions and a careful discussion of why the conditions are likely to hold.

References

ACEMOGLU, Daron, & JOHNSON, Simon. 2005. Unbundling institutions. *Journal of political Economy*, 113(5), 949–995.

---

54 The single index model has been criticized as an excessively restrictive model of judge behavior by (Humphries et al., 2023; Kamat et al., 2023).

55 Graphically, the sum of the red lines equals the sum of the green lines.

56 The estimate also places a positive weight on the defendant’s effect of incarceration: After controlling linearly for the conviction rate, the judges incarcerating the defendant tend to have an above average incarceration rate. The 2SLS estimate of the effect of conviction versus acquittal also assigns proper weights on this response type’s treatment effects.

57 Even after controlling linearly for the conviction rate, the judges incarcerating the defendant have a lower-than-average conviction rate. Graphically, the sum of the red lines is higher than the sum of the green lines. The 2SLS estimand of the effect of incarceration will thus place a non-zero (negative) weight on the causal effect of convicting this defendant.
Angrist, Joshua, Lang, Daniel, & Oreopoulos, Philip. 2009. Incentives and services for college achievement: Evidence from a randomized trial. *American Economic Journal: Applied Economics*, 1(1), 136–63.

Angrist, Joshua, Hull, Peter, & Walters, Christopher R. 2022. *Methods for Measuring School Effectiveness*. Tech. rept. National Bureau of Economic Research.

Angrist, Joshua D. 2006. Instrumental variables methods in experimental criminological research: what, why and how. *Journal of Experimental Criminology*, 2, 23–44.

Angrist, Joshua D., & Imbens, Guido W. 1995. Two-Stage Least Squares Estimation of Average Causal Effects in Models with Variable Treatment Intensity. *Journal of the American Statistical Association*, 90(430), 431–442.

Angrist, Joshua D, & Pischke, Jörn-Steffen. 2009. *Mostly harmless econometrics: An empiricist’s companion*. Princeton university press.

Autor, David, Maestas, Nicole, Mullen, Kathleen J, Strand, Alexander, et al. 2015. Does delay cause decay? The effect of administrative decision time on the labor force participation and earnings of disability applicants. Tech. rept. National Bureau of Economic Research.

Behagel, Luc, Crepon, Bruno, & Gurgand, Marc. 2013. Robustness of the Encouragement Design in a Two-Treatment Randomized Control Trial. *IZA Discussion Paper 7447*.

Bhuller, Manudeep, Dahl, Gordon B., Løken, Katrine V., & Mogstad, Magne. 2020. Incarceration, Recidivism, and Employment. *Journal of Political Economy*, 128(4), 1269–1324.

Blandhol, Christine, Bonney, John, Mogstad, Magne, & Torgovitsky, Alexander. 2022. When is TSLS Actually LATE? *University of Chicago, Becker Friedman Institute for Economics Working Paper*.

Bombardini, Matilde, & Li, Bingjing. 2020. Trade, pollution and mortality in China. *Journal of International Economics*, 125, 103321.

Borusyak, Kirill, Jaravel, Xavier, & Spiess, Jann. 2021. Revisiting event study designs: Robust and efficient estimation. *arXiv preprint arXiv:2108.12419*.

Callaway, Brantly, & Sant’Anna, Pedro HC. 2021. Difference-in-differences with multiple time periods. *Journal of Econometrics*, 225(2), 200–230.

Carneiro, Pedro, Heckman, James J, & Vytlacil, Edward. 2003. Understanding what instrumental variables estimate: Estimating marginal and average returns
to education. processed, University of Chicago, The American Bar Foundation and Stanford University, July, 19.

Chernozhukov, Victor, Lee, Sokbae, & Rosen, Adam M. 2013. Intersection bounds: Estimation and inference. *Econometrica*, 81(2), 667–737.

Cunha, Flavio, Heckman, James J, & Navarro, Salvador. 2007. The identification and economic content of ordered choice models with stochastic thresholds. *International Economic Review*, 48(4), 1273–1309.

de Chaisemartin, Clément, & d’Haultfoeuille, Xavier. 2020a. Two-way fixed effects estimators with heterogeneous treatment effects. *American Economic Review*, 110(9), 2964–96.

de Chaisemartin, Clément, & d’Haultfoeuille, Xavier. 2020b. Two-way fixed effects regressions with several treatments. *Available at SSRN 3751060*.

Dobbie, Will, Goldin, Jacob, & Yang, Crystal S. 2018. The Effects of Pretrial Detention on Conviction, Future Crime, and Employment: Evidence from Randomly Assigned Judges. *American Economic Review*, 108(2), 201–40.

Frandsen, Brigham, Lefgren, Lars, & Leslie, Emily. 2023. Judging Judge Fixed Effects. *American Economic Review*, 113(1), 253–77.

Galindo, Camila. 2020. Empirical Challenges of Multivalued Treatment Effects.

Goff, Leonard. 2020. A Vector Monotonicity Assumption for Multiple Instruments. *arXiv preprint arXiv:2009.00553*.

Goldsmith-Pinkham, Paul, Hull, Peter, & Kolesár, Michal. 2022. Contamination bias in linear regressions. Tech. rept. National Bureau of Economic Research.

Goodman-Bacon, Andrew. 2021. Difference-in-differences with variation in treatment timing. *Journal of Econometrics*, 225(2), 254–277.

Greene, William H, & Hensher, David A. 2010. *Modeling ordered choices: A primer*. Cambridge University Press.

Heckman, James J. 1979. Sample selection bias as a specification error. *Econometrica: Journal of the econometric society*, 153–161.

Heckman, James J, & Pinto, Rodrigo. 2018. Unordered monotonicity. *Econometrica*, 86(1), 1–35.

Heckman, James J, & Vytlacil, Edward J. 1999. Local Instrumental Variables and Latent Variable Models for Identifying and Bounding Treatment Effects. *Proceedings of the National Academy of Sciences*, 96(8), 4730–4734.
HECKMAN, JAMES J, & VYTLACIL, EDWARD J. 2005. Structural Equations, Treatment Effects, and Econometric Policy Evaluation. *Econometrica*, 73(3), 669–738.

HECKMAN, JAMES J, & VYTLACIL, EDWARD J. 2007. Econometric evaluation of social programs, part II: Using the marginal treatment effect to organize alternative econometric estimators to evaluate social programs, and to forecast their effects in new environments. *Handbook of Econometrics*, 6, 4875–5143.

HECKMAN, JAMES J., URZUA, SERGIO, & VYTLACIL, EDWARD. 2008. Instrumental Variables in Models with Multiple Outcomes: the General Unordered Case. *Annales d’Economie et de Statistique*, 151–174.

HEINESEN, ESKIL, HVID, CHRISTIAN, KIRKEBOEN, LARS JOHANNESSEN, LEUVEN, EDWIN, & MOGSTAD, MAGNE. 2022. *Instrumental Variables with Unordered Treatments: Theory and Evidence from Returns to Fields of Study*. Tech. rept. National Bureau of Economic Research.

HUMPHRIES, JOHN ERIC, OUSS, AURÉLIE, STEVENSON, MEGAN, STAVREVA, KAMELIA, & VAN DIJK, WINNIE. 2023. *Conviction, Incarceration, and Recidivism: Understanding the Revolving Door*. https://johnerichumphries.com/conviction_incarceration_and_recidivism_draft20230619.pdf. Accessed July 5th 2023.

IMBENS, G. W, & ANGRIST, J. D. 1994. Identification and Estimation of Local Average Treatment Effects. *Econometrica*, 62(2), 467–475.

JAEGGER, DAVID A, RUIST, JOAKIM, & STUHLER, JAN. 2018. *Shift-share instruments and the impact of immigration*. Tech. rept. National Bureau of Economic Research.

KAMAT, VISHAL, NORRIS, SAMUEL, & PECENCO, MATTHEW. 2023. *Conviction, Incarceration, and Policy Effects in the Criminal Justice System*. https://www.samuel-norris.com/. Accessed August 10th 2023.

KIRKEBOEN, LARS J., LEUVEN, EDWIN, & MOGSTAD, MAGNE. 2016. Field of Study, Earnings, and Self-Selection. *Quarterly Journal of Economics*, 131(3), 1057–1111.

KITAGAWA, TORU. 2015. A test for instrument validity. *Econometrica*, 83(5), 2043–2063.

KLINE, PATRICK, & WALTERS, CHRISTOPHER R. 2016. Evaluating public programs with close substitutes: The case of Head Start. *Quarterly Journal of Economics*, 131(4), 1795–1848.

LEE, SOKBAE, & SALANIÉ, BERNARD. 2018. Identifying effects of multivalued treatments. *Econometrica*, 86(6), 1939–1963.
LEE, SOKBAE, & SALANIÉ, BERNARD. 2023. Filtered and Unfiltered Treatment Effects with Targeting Instruments. *arXiv preprint arXiv:2007.10432.*

MOGSTAD, MAGNE, TORGOVITSKY, ALEXANDER, & WALTERS, CHRISTOPHER R. 2021. The Causal Interpretation of Two-Stage Least Squares with Multiple Instrumental Variables. *American Economic Review*, 3663–3698.

MOGSTAD, MAGNE, TORGOVITSKY, ALEXANDER, & WALTERS, CHRISTOPHER R. forthcoming. Policy evaluation with multiple instrumental variables. *Journal of Econometrics.*

MOUNTJOY, JACK. 2022. Community colleges and upward mobility. *American Economic Review*, 112(8), 2580–2630.

MOURIFIÉ, ISMAEL, & WAN, YUANYUAN. 2017. Testing local average treatment effect assumptions. *Review of Economics and Statistics*, 99(2), 305–313.

MUELLER-SMITH, MICHAEL. 2015. *The Criminal and Labor Market Impacts of Incarceration.* Tech. rept. Working Paper.

NORRIS, SAMUEL, PECENCO, MATTHEW, & WEAVER, JEFFREY. 2021. The effects of parental and sibling incarceration: Evidence from ohio. *American Economic Review*, 111(9), 2926–63.

PERSSON, TORSTEN, & TABELLINI, GUIDO. 2004. Constitutional rules and fiscal policy outcomes. *American Economic Review*, 94(1), 25–45.

PINTO, RODRIGO. 2021. Beyond intention to treat: Using the incentives in moving to opportunity to identify neighborhood effects. *NBER Working Paper.*

ROHLFS, CHRIS. 2006. The government’s valuation of military life-saving in war: A cost minimization approach. *American Economic Review*, 96(2), 39–44.

ROSE, EVAN K, & SHEM-TOV, YOTAM. 2021a. How does incarceration affect reoffending? estimating the dose-response function. *Journal of Political Economy*, 129(12), 3302–3356.

ROSE, EVAN K, & SHEM-TOV, YOTAM. 2021b. On Recoding Ordered Treatments as Binary Indicators. *arXiv preprint arXiv:2111.12258.*

SŁOCZYŃSKI, TYMON. 2020. When should we (not) interpret linear IV estimands as LATE? *arXiv preprint arXiv:2011.06695.*

SUN, LIYANG, & ABRAHAM, SARAH. 2021. Estimating dynamic treatment effects in event studies with heterogeneous treatment effects. *Journal of Econometrics*, 225(2), 175–199.
VYTLACIL, EDWARD. 2002. Independence, monotonicity, and latent index models: An equivalence result. *Econometrica*, 70(1), 331–341.
A Proofs

In the proofs below we consider the general case with \( n \) treatments: \( T \in \{0, 1, \ldots, n\} \).

Proof. (Proposition 1). We have

\[
\text{Cov} (P, Y) = \mathbb{E} [(P - \mathbb{E} [P]) (Y - \mathbb{E} [Y])]
\]

\[
= \mathbb{E} [(P - \mathbb{E} [P]) Y]
\]

\[
= \mathbb{E} [\mathbb{E} [(P - \mathbb{E} [P]) Y | Z, S]]
\]

\[
= \mathbb{E} [(P - \mathbb{E} [P]) \mathbb{E} [Y | Z, S]]
\]

\[
= \mathbb{E} [(P - \mathbb{E} [P]) \mathbb{E} [Y (0) + v_S (Z) \beta | Z, S]]
\]

\[
= \mathbb{E} [(P - \mathbb{E} [P]) (\mathbb{E} [Y (0) | S] + v_S (Z) \mathbb{E} [\beta | S])] = \mathbb{E} [\text{Cov} (P, v_S (Z) | S) \mathbb{E} [\beta | S]]
\]

The third equality uses the law of iterated expectations. The fourth equality uses that \( P \) is a deterministic function of \( Z \). The sixth and the seventh equality invokes Assumption 1.\(^{58}\) Thus \( \beta_{SLS} \equiv \mathbb{E} [w_S \beta^S] \) where \( w_s \equiv \text{Var} (P)^{-1} \text{Cov} (P, v_s (Z)) \) and \( \beta^s \equiv \mathbb{E} [\beta | S = s] \).

\( \Box \)

Proof. (Corollary 1). We have

\[
\mathbb{E} [\text{Cov} (P, v_S (Z) | S)] = \text{Cov} (P, \mathbb{E} [v_S (Z) | Z])
\]

\[
= \text{Cov} (P, \mathbb{E} [D | Z])
\]

\[
= \text{Var} (P)
\]

The first equality uses the distributive property of covariances of sums. The third equality uses that, by a standard property of linear projections, \( \text{Cov} (Z, D - P) = 0 \). This property implies \( \text{Cov} (Z, P) = \text{Cov} (Z, D) = \text{Cov} (Z, \mathbb{E} [D | Z]) \)\(^{59}\) which gives

\( \Box \)

\(^{58}\)The seventh equality also relies on the law of iterated expectations. In particular

\[
\mathbb{E} [(P - \mathbb{E} [P]) \mathbb{E} [Y (0) | S]] = \mathbb{E} [\mathbb{E} [(P - \mathbb{E} [P]) \mathbb{E} [Y (0) | S] | S]]
\]

\[
= \mathbb{E} [\mathbb{E} [P | S] - \mathbb{E} [P]] \mathbb{E} [Y (0) | S]]
\]

\[
= \mathbb{E} [(\mathbb{E} [P] - \mathbb{E} [P]) \mathbb{E} [Y (0) | S]]
\]

\[
= 0
\]

\(^{59}\)By the law of iterated expectations

\[
\text{Cov} (Z, D) = \mathbb{E} [ZD] - \mathbb{E} [Z] \mathbb{E} [D]
\]

\[
= \mathbb{E} [\mathbb{E} [ZD | Z]] - \mathbb{E} [Z] \mathbb{E} [\mathbb{E} [D | Z]]
\]

\[
= \mathbb{E} [Z \mathbb{E} [D | Z]] - \mathbb{E} [Z] \mathbb{E} [\mathbb{E} [D | Z]]
\]

\[
= \text{Cov} (Z, \mathbb{E} [D | Z])
\]
\[
\text{Cov}(P, E[D | Z]) = \text{Cov}(P, P) = \text{Var}(P) \quad \text{since } P \text{ is a linear function of } Z. \quad \square
\]

**Proof.** (Corollary 2). The partial correlation between \(P_1\) and \(s_1(Z)\) given \(P_2\) is the Pearson correlation coefficient between the residuals from regressing \(P_1\) on \(P_2\) and the residuals from regressing \(s_1(Z)\) on \(P_2\). By the Frisch-Waugh-Lovell theorem, this partial correlation has the same sign as the coefficient on \(P_1\) in a regression of \(s(Z_1)\) on \(P\). \(\square\)

**Proof.** (Corollary 3). The proof is analogous to the proof of Corollary 2. \(\square\)

**Proof.** (Corollary 4). This result follows immediately from Corollaries 2–3. \(\square\)

**Proof.** (Proposition 2.) We have that

\[
\begin{align*}
\text{Cov}(P, 1_{Y \in B} D_1) &= \text{Cov}(P, 1_{Y(1) \in B} D_1) \\
&= \mathbb{E}\left[\text{Cov}(P, 1_{Y(1) \in B} D_1 | S)\right] \\
&= \mathbb{E}\left[\text{Cov}(P, 1_{Y(1) \in B} s_1(Z) | S)\right] \\
&= \mathbb{E}\left[\text{Cov}(P, s_1(Z) | S) \Pr[Y(1) \in B | S]\right]
\end{align*}
\]

where the second equality uses the law of iterated expectations and the fourth equality invokes Assumption 1. Thus

\[
\text{Var}(P)^{-1} \text{Cov}(P, 1_{Y \in B} D) = \mathbb{E}[w_s \mathbb{E}[1_{Y \in B} | S]]
\]

where \(w_s \equiv \text{Var}(P)^{-1} \text{Cov}(P, v_s(Z))\). If 2SLS assigns proper weights then \(w_s\) is non-negative diagonal for all \(s\) by Proposition 1. Since \(\mathbb{E}[1_{Y \in B} | S] \geq 0, \mathbb{E}[w_s \mathbb{E}[1_{Y \in B} | S]]\) is then also a non-negative diagonal matrix. \(\square\)

**Proof.** (Proposition 3). Assume Assumptions 3 and 4 hold. Since \(Z \perp X | S\), we then have \(\text{Cov}(P, v_s(Z) | X = 1) = \text{Cov}(P, v_s(Z))\) for each response type \(s\) present in the subsample \(X = 1\). Furthermore, \(\text{Var}(P | X = 1) = \text{Var}(P)\). Thus

\[
w_s|X \equiv \text{Var}(P | X = 1)^{-1} \text{Cov}(P, v_s(Z) | X = 1) = \text{Var}(P)^{-1} \text{Cov}(P, v_s(Z))
\]

is non-negative diagonal. Noting that \(\text{Cov}(P, v_s(Z) | X = 1) = \text{Cov}(P, D | X = 1, S = s)\), we then have that

\[
\text{Var}(P | X = 1)^{-1} \text{Cov}(P, D | X = 1) = \mathbb{E}\left[\text{Var}(P | X = 1)^{-1} \text{Cov}(P, D | X = 1, S)\right]
\]
To prove Proposition 4, the following lemma is useful.

**Lemma 1.** When $n = m$, 2SLS assigns proper weights if and only if for all $s, k,$ and $l$
\[
\frac{\text{Cov} (Z_k, s_l (Z))}{\text{Cov} (Z_k, D_k)} \geq 0
\]

**Proof.** By Proposition 1, the 2SLS assigns proper weights if and only if the matrix
\[
w_s = \text{Var} (P)^{-1} \text{Cov} (P, v_s (Z))
\]
is non-negative diagonal for all $s$. When $n = m$, we can simplify to
\[
w_s = \text{Cov} (Z, D)^{-1} \text{Cov} (Z, v_s (Z)) \Rightarrow \text{Cov} (Z, D) w_s = \text{Cov} (Z, v_s (Z))
\]
Thus, for all $s, k,$ and $l$, \[
\text{Cov} (Z_k, D_l) w_{s ll} = \text{Cov} (Z_k, s_l (Z)) \text{ where } w_{s ll} \geq 0 \text{ is the } l\text{th diagonal element of } w_s.
\]

**Proof.** (Proposition 4). Consider a treatment $k$. Defining \[
\pi_l \equiv \text{Pr} [V = l],
\]
we have
\[
\text{Cov} (Z_l, s_k (V)) = \left( \text{Pr} [s_k (V) = 1 | V = l] - \text{Pr} [s_k (V) = 1 | V \neq l] \right) \text{Pr} [V = l] \text{Pr} [V \neq l]
\]
\[
= \left( s_k (l) - \sum_r \pi_r s_k (r) \right) \pi_l (1 - \pi_l)
\]
\[
= \left( s_k (l) - \sum_r \pi_r s_k (r) \right) \pi_l
\]
\[
= \left( s_k (l) - \text{E} [s_k (V)] \right) \pi_l
\]

By Lemma 1, for all $l$ and $r$, we must have that
\[
\frac{(s_k (l) - \text{E} [s_k (V)]) \pi_l}{\text{Cov} (Z_l, D_k)} = \frac{(s_k (r) - \text{E} [s_k (V)]) \pi_r}{\text{Cov} (Z_r, D_k)} \geq 0
\]
is constant across $s$. These equations are satisfied for never-$k$-takers—agents with $s_k (l) = 0$ for all $l$—and always-$k$-takers—agents with $s_k (l) = 1$ for all $l$. Beyond never-$k$-takers and always-$k$-takers, there can only be one response type for treatment indicator $k$. To see this, assume response types $s$ and $s'$ are neither never-$k$-takers nor always-$k$-takers. We then must have $s_k (l) = s_k (r) \Rightarrow \text{Cov} (Z_l, D_k) \pi_r = \text{Cov} (Z_r, D_k) \pi_l \Rightarrow$

\[
\text{E} [\text{Cov} (P, D | X = 1, S)] = \text{E} [\text{E} [PD | X = 1, S]] - \text{E} [\text{E} [P | X = 1, S] \text{E} [D | X = 1, S]]
\]
\[
= \text{E} [PD | X = 1] - \text{E} [\text{E} [P | X = 1] \text{E} [D | X = 1, S]]
\]
\[
= \text{E} [PD | X = 1] - \text{E} [P | X = 1] \text{E} [D | X = 1]
\]
\[
= \text{Cov} (P, D | X = 1)
\]
where the second equality uses the law of iterated expectation and that $P \perp S$. 
Thus, either \( s_k (l) = s_k^r (l) \) or \( s_k (l) = 1 - s_k^r (l) \) for all \( l \). The latter case is not possible since \( s_k (l) - \text{E} [s_k (V)] \) and \( s_k^r (l) - \text{E} [s_k^r (V)] \) will then have opposite signs. Thus \( s_k (l) = s_k^r (l) \) for all \( l \). Since, for all treatments \( k \), there must exist at least one response type that is neither an always-\( k \)-taker nor a never-\( k \)-taker for the rank condition (Assumption 2) to hold and \( n = m \), the response type for treatment \( k \) must have \( s_k (l) = 1 \) for exactly one \( l \). This defines a one-to-one mapping \( f : \{0, 1, \ldots, n\} \rightarrow \{0, 1, \ldots, n\} \) such that, for all \( k \), the only non-trivial response type for treatment indicator \( k \) is defined by \( s_k (v) = 1 \iff f (v) = k \).

\[ \text{Proof. (Corollary 5). This result follows directly from Proposition 4.} \]

\[ \text{Proof. (Proposition 5). Assume Assumptions 3 and 4 hold. Let } f \text{ be the mapping defined in Proposition 4. Let } a_k \text{ denote the share of always-} k \text{-takers and } b_k \text{ the share of } k \text{-compliers in the population. Since these are the only possible response types for treatment } k, \text{ predicted treatments based on the instruments } Z^* \text{ equals } P = a + b Z^* \text{ where } a \equiv (a_1, \ldots, a_n)' \text{ and } b \text{ is a diagonal matrix with diagonal elements } b_k. \text{ By Proposition 3, the following matrix must be non-negative diagonal:} \]

\[
\text{Var} (P | X = 1)^{-1} \text{Cov} (P, D | X = 1) = \text{Var} (b Z^* | X = 1)^{-1} \text{Cov} (b Z^*, D | X = 1) = b^{-1} \text{Var} (Z^* | X = 1)^{-1} \text{Cov} (Z^*, D | X = 1)
\]

Since \( b^{-1} \) is a positive diagonal matrix, \( \text{Var} (Z^* | X = 1)^{-1} \text{Cov} (Z^*, D | X = 1) \) must also be a non-negative diagonal matrix.

\[ \text{Proof. (Proposition 6). We first show that indirect utilities of the stated form satisfy the assumptions in Proposition 4 required for 2SLS to assign proper weights. We need to show that (i) a response type never selects } k \text{ unless she selects it at } V = k \text{ and (ii) a response type that selects } k \text{ when } V = l \neq k \text{ always selects } k. \text{ To show (i), assume } s \text{ selects } l \neq k \text{ when } V = k. \text{ Then } I_{ls} (k) = u_{ls} > I_{ks} (k) = u_{ks} + \mu_{ks}. \text{ Since } \mu_{ks} \geq 0, \text{ we must then have } I_{ls} (v) > I_{ks} (v) \text{ for all } v \text{—response type } s \text{ never selects } k. \text{ To show (ii), assume } s \text{ selects } k \text{ when } V = l \neq k. \text{ Then } I_{ks} (l) = u_{ks} > 0 = I_{0s} (l). \text{ Thus } I_{ks} (v) > I_{0s} (v) = 0 \text{ for all } v. \text{ By the last stated property of the indirect utilities in the proposition, we must then have } I_{rs} (v) < I_{0s} (v) = 0 \text{ for all } v \text{ and } r \notin \{0, k\}. \text{ Thus } k \text{ is always selected.}
\]

To prove the other direction of the equivalence, assume that 2SLS assigns proper weights. We want to show that choice behavior can be described by indirect utilities of the form given in the proposition. To do this, define indirect utilities \( I_{ks} (v) = u_{ks} + \mu_{ks} 1 [v = k] \) where

32
By Proposition 4, these cases cover all possible response types when 2SLS assigns proper weights. It is straightforward to verify that \( s(v) = \arg \max_k I_{ks}(v) \) with these values of \( u_{ks} \) and \( \mu_{ks} \). Note that no response type is ever indifferent, so \( \arg \max_k I_{ks}(v) \) is indeed a singleton. The indirect utilities also satisfy \( I_{ks}(v) > I_{0s}(v) \Rightarrow I_{ls}(v) < I_{0s}(v) \) for \( l \notin \{0, k\} \): We only have \( I_{ks}(v) > 0 = I_{0s}(v) \) when \( s \) selects \( k \) at \( V = v \). In that case \( l \) is not selected at \( V = v \) and \( I_{ls}(v) = -1 < 0 = I_{0s}(v) \).

**Proof.** (Proposition 7). We first show that indirect utilities of the stated form satisfy the assumptions in Proposition 4 required for 2SLS to assign proper weights. Under ordered treatment effects, we have \( s_k(v) = 1[s(v) \geq k] \). The required conditions are thus that, for each \( k \), a response type \( s \) either always selects \( k \) or above, never selects \( k \) or above, or selects \( k \) or above if and only if \( V = k \). These conditions together imply that \( s \) either always select the same treatment or selects treatment \( k \) if \( V = k \) and treatment \( k - 1 \) if \( V \neq k \) for some \( k \).

Assume \( s \) selects \( k - 1 \) when \( V = 0 \). Thus \( u_{k-1,s} > u_{ls} \) for all \( l \neq k - 1 \). We need to show that (i) \( s \) always selects \( k - 1 \) when \( V \neq k \) and (ii) \( s \) selects either \( k \) or \( k - 1 \) when \( V = k \). To show (i), consider first the case when \( V = r < k \). Then \( I_{k-1,s}(r) = u_{k-1,s} + \mu_{rs} \) is still the largest indirect utility, so \( k - 1 \) is still selected. Now, consider the case when \( V = r > k \). Then, \( I_{k-1,s}(r) = u_{k-1,s} + u_{ks} > I_{ls}(r) \) and \( I_{k-1,s}(r) = u_{k-1,s} + u_{k-2,s} = I_{k-2,s}(r) \). Informally, there is a “peak” at treatment \( k - 1 \).

By single-peakedness, \( I_{k-1,s}(r) \) must then be the largest indirect utility when \( V = r \). Thus \( s \) always selects \( k - 1 \) when \( V \neq k \). To show (ii), assume that \( r \neq k \) is selected when \( V = k \). Since \( V = k \) only increases the utility of treatment \( k \) and above we must have \( r \geq k - 1 \). If \( r > k \), then \( I_{rs}(k) = u_{rs} + u_{ks} > I_{ks}(k) \Rightarrow u_{rs} > u_{ks} \). But then \( I_{rs}(0) > I_{ks}(0) \) and \( I_{k-1,s}(0) > I_{ks}(0) \), violating single-peakedness at \( V = 0 \).

Thus, we must have \( r = k - 1 \).

To prove the other direction of the equivalence, assume that 2SLS assigns proper weights. We want to show that choice behavior can be described by indirect utilities of the form given in the proposition. To do this, define indirect utilities \( I_{ks}(v) = u_{ks} + \mu_{vs}1[k \geq v] \) where

\[
(u_{ks}, \mu_{ks}) = \begin{cases} 
(1, 0) & \text{if } s \text{ selects } k \text{ when } V = 0 \\
(-1, 0) & \text{if } s \text{ never selects } k \\
(0, 2) & \text{if } s \text{ selects } k \text{ if } V = k \text{ and } k - 1 \text{ otherwise}
\end{cases}
\]

These cases cover all possible response types when 2SLS assigns proper weights: As
argued above, under ordered treatment effects, the conditions in Proposition 4 require that each a response type either always select the same treatment or selects treatment \( k \) if \( V = k \) and treatment \( k - 1 \) if \( V \neq k \) for some \( k \). It is straightforward to verify that 
\[
s(v) = \arg \max_k I_{ks}(v)
\]
with these values of \( u_{ks} \) and \( \mu_{ks} \). Note that no response type is ever indifferent, so \( \arg \max_k I_{ks}(v) \) is indeed a singleton.

We now verify that the indirect utilities given above are always single peaked. First, consider the case when response type \( s \) always selects treatment \( k \). Then \( I_k = 1 \) and \( I_{l} = -1 \) for all \( v \) and \( l \neq k \) and the response type’s preferences have a single peak at \( k \). Second, consider the case when response type \( s \) selects \( k \) if \( V = k \) and \( k - 1 \) otherwise. We have \( I_k = 2 \), \( I_{k-1} = 1 \), \( I_l = 1 \) for \( l < k \), and \( I_{l} = -1 \) for \( l < k - 1 \). Thus preferences have a single peak at treatment \( k \) when \( V = k \). Moreover, for \( l \neq k \) we have \( I_{k-1} = 1 \), \( I_k = 0 \), and \( I_{r} = -1 \) for \( r \notin \{k, k - 1\} \). Thus preferences have a single peak (at treatment \( k - 1 \)) also when \( V = l \neq k \).

\[\square\]

Proof. (Proposition 8). This result follows directly from Proposition B.8.

Proof. (Corollary B.1). This result follows directly from Proposition 4.

Proof. (Proposition B.1). We have

\[
\text{Cov} \left( \tilde{P}, \tilde{Y} \right) = E \left[ \tilde{P} \tilde{Y} \right] - E \left[ \tilde{P} \right] E \left[ \tilde{Y} \right]
\]

\[
= E \left[ \tilde{P} \tilde{Y} \right]
\]

\[
= E \left[ \tilde{P} Y \right]
\]

\[
= E \left[ E \left[ \tilde{P} Y \mid Z, X, S \right] \right]
\]

\[
= E \left[ \tilde{P} E \left[ Y \mid Z, X, S \right] \right]
\]

\[
= E \left[ \tilde{P} \left( E \left[ \alpha \mid X, S \right] + v_S(\tilde{Z}) E \left[ \beta \mid X, S \right] \right) \right]
\]

\[
= E \left[ \tilde{P} v_S(\tilde{Z}) E \left[ \beta \mid X, S \right] \right]
\]

\[
= E \left[ \text{Cov} \left( \tilde{P}, v_S(\tilde{Z}) \mid X, S \right) E \left[ \beta \mid X, S \right] \right]
\]

The second equality uses that \( E \left[ \tilde{P} \right] = 0 \). The third equality uses that

\[
E \left[ \tilde{P} \tilde{Y} \right] = E \left[ \tilde{P} Y \right] - E \left[ \tilde{P} E \left[ Y \mid X \right] \right]
\]
and, using $\mathbb{E}[\tilde{P} \mid X] = 0$ and the law of iterated expectations

$$
\mathbb{E}[\tilde{P} \mathbb{E}[Y \mid X]] = \mathbb{E}[\mathbb{E}[\tilde{P} \mathbb{E}[Y \mid X] \mid X]] = \mathbb{E}[\mathbb{E}[Y \mid X] \mathbb{E}[\tilde{P} \mid X]] = 0
$$

The fourth equality applies the law of iterated expectations. The sixth equality uses

$$
\mathbb{E}[Y \mid Z, X, S] = \mathbb{E}[\alpha + D\beta \mid Z, X, S] = \mathbb{E}[\alpha + v_S(Z) \beta \mid Z, X, S] = \mathbb{E}[\alpha \mid X, S] + v_S(Z) \mathbb{E}[\beta \mid X, S]
$$

where the third equality invokes the conditional independence assumption. The seventh equality uses $\mathbb{E}[\tilde{P} \mid X] = 0$ combined with the law of iterated expectations.\footnote{In particular,}

**Proof.** (Proposition B.2). Assume 2SLS assigns proper weights. Since $Z \perp W \mid S, X$, we have\footnote{Cov \left( \tilde{P}, v_s(Z) \mid W = 1 \right) = \mathbb{E}[\tilde{P}v_s(Z) \mid W = 1] - \mathbb{E}[\tilde{P} \mid W = 1]\mathbb{E}[v_s(Z) \mid W = 1]

$$
\begin{align*}
\mathbb{E}\left[\tilde{P}\mathbb{E}[\alpha \mid X, S]\right] &= \mathbb{E}\left[\mathbb{E}\mathbb{E}[\tilde{P}\mathbb{E}[\alpha \mid X, S] \mid X, S] \mid X, S\right] \\
&= \mathbb{E}\left[E[\alpha \mid X, S]\mathbb{E}[\tilde{P} \mid X, S] \mid X, S\right] \\
&= \mathbb{E}\left[E[\alpha \mid X, S]\mathbb{E}[\tilde{P} \mid X] \mid X, S\right] \\
&= 0
\end{align*}
$$

where the third equality uses Assumption B.1.}

$$
\begin{align*}
\mathbb{E}[\tilde{P}v_s(Z) \mid W = 1] &= \mathbb{E}[\tilde{P}v_s(Z) \mid W = 1, X, S]\mathbb{E}[v_s(Z) \mid X, S] \\
&= \mathbb{E}[\tilde{P}v_s(Z) \mid X, S] \\
&= \mathbb{E}[\tilde{P}v_s(Z)]
\end{align*}
$$

where the second equation uses $Z \perp W \mid S, X$ and the law of iterated expectations. E.g.,
is a non-negative diagonal matrix. Also, note that
\[ \text{Cov} \left( \tilde{P}, v_s(Z) \mid W = 1 \right) = \text{Cov} \left( \tilde{P}, D \mid W = 1, S = s \right) \]

We then have that\(^{63}\)
\[ \text{Var} \left( \tilde{P} \right)^{-1} \text{Cov} \left( \tilde{P}, D \mid W = 1 \right) = \mathbb{E} \left[ \text{Var} \left( \tilde{P} \right)^{-1} \text{Cov} \left( \tilde{P}, D \mid W = 1, S \right) \right] \]

is a non-negative diagonal matrix. The result follows since \( \text{Cov} \left( \tilde{P}, D \mid W = 1 \right) = \text{Cov} \left( \tilde{P}, \tilde{D} \mid W = 1 \right) \).

**Proof.** (Proposition B.3). Under Assumptions B.1–B.4, we have
\[ w_{s,x} = \text{Var} \left( \tilde{P} \right)^{-1} \text{Cov} \left( \tilde{P}, v_s(Z) \mid X = x \right) \]
\[ = a_x \text{Var} \left( \tilde{P} \mid X = x \right)^{-1} \text{Cov} \left( \tilde{P}, v_s(Z) \mid X = x \right) \]
\[ = a_x \text{Var} \left( P_X + \mathbb{E} \left[ D \mid X \right] \mid X = x \right)^{-1} \text{Cov} \left( P_X + \mathbb{E} \left[ D \mid X \right], v_s(Z) \mid X = x \right) \]
\[ = a_x \text{Var} \left( P_X \mid X = x \right)^{-1} \text{Cov} \left( P_X, v_s(Z) \mid X = x \right) \]

The result then follows by an application of Proposition 4 on each subsample \( X = x \). The permutation \( f \) must be the same across values of \( X \) since Assumption B.4 requires that \( \mathbb{E} \left[ D \mid Z, X \right] = \gamma Z + \phi_X \), i.e., the relationship between \( Z \) and \( D \) can not differ across \( X \). \( \square \)

**Proof.** (Proposition B.4). As shown in the proof of Proposition B.3, we have
\[ w_{s,x} = a_x \text{Var} \left( P_X \mid X = x \right)^{-1} \text{Cov} \left( P_X, v_s(Z) \mid X = x \right) \]

\[ \mathbb{E} \left[ \text{Cov} \left( \tilde{P}, D \mid W = 1, S \right) \right] = \mathbb{E} \left[ \mathbb{E} \left[ \tilde{P} D \mid W = 1, S \right] \right] - \mathbb{E} \left[ \mathbb{E} \left[ \tilde{P} \mid W = 1, S \right] \mathbb{E} \left[ D \mid W = 1, S \right] \right] \]
\[ = \mathbb{E} \left[ \tilde{P} D \mid W = 1 \right] - \mathbb{E} \left[ \mathbb{E} \left[ \tilde{P} \mid W = 1 \right] \mathbb{E} \left[ D \mid W = 1, S \right] \right] \]
\[ = \mathbb{E} \left[ \tilde{P} D \mid W = 1 \right] - \mathbb{E} \left[ \tilde{P} \mid W = 1 \right] \mathbb{E} \left[ D \mid W = 1, S \right] \]
\[ = \mathbb{E} \left[ \tilde{P} D \mid W = 1 \right] - \mathbb{E} \left[ \tilde{P} \mid W = 1 \right] \mathbb{E} \left[ D \mid W = 1 \right] \]
\[ = \text{Cov} \left( \tilde{P}, D \mid W = 1 \right) \]

where the second equality uses the law of iterated expectation and
\[ \mathbb{E} \left[ \tilde{P} \mid W = 1, S \right] = \mathbb{E} \left[ \mathbb{E} \left[ \tilde{P} \mid W = 1, S, X \right] \right] \]
\[ = \mathbb{E} \left[ \mathbb{E} \left[ \tilde{P} \mid W = 1, X \right] \right] \]
\[ = \mathbb{E} \left[ \tilde{P} \mid W = 1 \right] \]

since \( \tilde{P} \perp S \mid X \).

36
under Assumptions B.1–B.4. The result then follows by applying Proposition 5 on each subsample \( X = x \). The permutation \( f \) must be the same across values of \( X \) since Assumption B.4 requires that \( \mathbb{E}[D \mid Z, X] = \gamma Z + \phi_X \), i.e., the relationship between \( Z \) and \( D \) can not differ across \( X \).

\[ \square \]

**Proof.** (Proposition B.5). As in the proof of Proposition B.3, Assumptions B.1–B.4 imply

\[
 w_{s,x} = a_x \text{Var} (P_X \mid X = x)^{-1} \text{Cov} (P_X, v_s \mid X = x)
\]

The result then follows by applying Proposition 8 on each subsample \( X = x \).

\[ \square \]

**Proof.** (Proposition B.6). To see that IA is not sufficient for Assumption 4, consider the following example. In the model of Section 3.1 with unordered treatment effects, assume there are only two response types, \( s \) and \( s' \), defined by

\[
 s(v) = \begin{cases} 
 0 & v = 0 \\
 1 & v = 1 \\
 2 & v = 2 
\end{cases} \quad s'(v) = \begin{cases} 
 0 & v = 0 \\
 2 & v = 1 \\
 2 & v = 2 
\end{cases} 
\]

In a population composed of \( s \) and \( s' \), IA monotonicity is trivially satisfied. But the no-cross effect condition (Assumption 4) is not satisfied. Instrument 2 affects both treatment 1 and treatment 2 violating the conditions in Proposition 4. In particular, response type \( s' \) is not among the allowed response types in Corollary 5.

To see that IA monotonicity does not imply Assumption 3, assume \( Z = \{Z_1, Z_2\} \) is composed of two continuous instruments taking values between 0 and \( \bar{z} \). For simplicity, assume only \( Z_1 \) affects treatment 1 and \( P_1 \perp P_2 \). Assume the linear relationship between \( Z_1 \) and \( D_1 \) is increasing but the highest propensity to take up treatment 1 occurs at \( Z_1 = 0 \). In other words, the linear relationship between the instrument and treatment imposed by the first stage is misspecified. Then a response type that selects \( D_1 \) if and only if \( Z_1 = 0 \) satisfies IA monotonicity but violates average conditional monotonicity.

To prove ii), consider the case when Assumption 6 holds and \( P_1 \perp P_2 \). IA monotonicity then requires that the selection of treatment 1 is strictly non-decreasing in \( P_1 \) for all agents. But Assumption 3 is weaker and only requires the selection of treatment 1 to have a non-negative correlation with \( P_1 \) for all agents.

\[ \square \]

**Proof.** (Proposition B.7). When Assumption 6 holds, \( P_k \)—the best linear prediction of \( D_k \) given \( Z \)—equals \( \mathbb{E}[D_k \mid Z] \), the propensity of crossing threshold \( k \) given \( Z \). For each

\[ \text{Cov} (P_1, s_1(Z)) \geq 0 \] which is violated in this case.

\[ \text{Cov} (P_1, s_1(Z)) \geq 0 \]

\[ \text{Cov} (P_1, s_1(Z)) \geq 0 \]
response type $s$ there must then exist a threshold $u_{ks} \in [0, 1]$ such that $P_k \geq u_{ks} \iff s_k(Z) = 1$. By Lemma 1, 2SLS assigns proper weights if and only if

$$\frac{\text{Cov}(P_k, s_l(Z))}{\text{Cov}(P_k, D_l)} = \frac{\text{Cov}(P_l, s_l(Z))}{\text{Cov}(P_l, D_l)} \geq 0$$

for all $l$ and $k$. Since

$$\text{Cov}(P_k, s_l(Z)) = (E[P_k | P_l \geq u_{ls}] - E[P_k | P_l < u_{ls}]) \Pr[s_l(Z) = 1] \Pr[s_l(Z) = 0]$$

this is equivalent to

$$\frac{E[P_k | P_l \geq u_{ls}] - E[P_k | P_l < u_{ls}]}{\text{Cov}(P_k, D_l)} = \frac{E[P_l | P_l \geq u_{ls}] - E[P_l | P_l < u_{ls}]}{\text{Cov}(P_l, D_l)} \geq 0 \quad (A.1)$$

We want to show that this condition is equivalent to

$$E[P_k | P_l] = \gamma_{kl} - \delta_{kl} P_l \quad (A.2)$$

for constants $\gamma_{kl}$ and $\delta_{kl}$. First, assume Equation A.2. We then have\(^{65}\)

$$E[P_k | P_l \geq u_{ls}] - E[P_k | P_l < u_{ls}] = -\delta_{kl} (E[P_l | P_l \geq u_{ls}] - E[P_l | P_l < u_{ls}])$$

and\(^{66}\)

$$\frac{\text{Cov}(P_k, D_l)}{\text{Cov}(P_l, D_l)} = \frac{E[\text{Cov}(P_k, S_l(Z) | S)]}{E[\text{Cov}(P_l, S_l(Z) | S)]} = -\delta_{kl}$$

Thus Equation A.1 holds. To prove the opposite direction of the equivalence, assume Equation A.1 holds. Rearranging Equation A.1 gives

$$E[P_k | P_l \geq u_{ls}] = \gamma - \delta E[P_l | P_l \geq u_{ls}]$$

where $\gamma \equiv E[P_k] + \delta E[P_l]$ and $\delta \equiv -\frac{\text{Cov}(P_k, D_l)}{\text{Cov}(P_l, D_l)}$. Thus, defining $f_{kl}(p_k, p_l)$ as the joint distribution of $P_k$ and $P_l$ and $f_l(p_l)$ as the distribution of $P_l$, we get

$$\int_{p_l \leq u_{ls}} p_k f_{kl}(p_k, p_l) dp_k dp_l = \gamma + \delta \int_{0}^{u_{ls}} f_l(p_l) dp_l \int_{0}^{u_{ls}} f_{kl}(p_k, p_l) dp_k$$

\(^{65}\)Using $E[P_k | P_l \geq u_{ls}] = \gamma_{kl} - \delta_{kl} E[P_l | P_l \geq u_{ls}]$ and $E[P_k | P_l < u_{ls}] = \gamma_{kl} - \delta_{kl} E[P_l | P_l < u_{ls}]$.

\(^{66}\)By Lemma 1, 2SLS assigns proper weights if and only if

$$\text{Cov}(P_k, s_l(Z)) = -\delta_{kl} (E[P_l | P_l \geq u_{ls}] - E[P_l | P_l < u_{ls}]) \Pr[s_l(Z) = 1] \Pr[s_l(Z) = 0]$$

$$= -\delta_{kl} \text{Cov}(P_l, s_l(Z))$$
\[
\int_{p_t \leq u_{ls}} p_k f_{kl}(p_k, p_l) dp_k dp_l = \gamma \int_{0}^{u_{ls}} f_l(p_l) dp_l + \delta \int_{0}^{u_{ls}} p_l f_l(p_l) dp_l
\]

Invoking Assumption B.6, we can differentiate this equation with respect to \(u_{ls}\), giving
\[
E [P_k | P_l = u_{ls}] = \gamma - \delta u_{ls}.
\]
Thus, Equation A.2 is satisfied.

**Proof.** (Proposition B.8). This follows directly from the proof of Proposition B.7: Assumption B.6 is used in the proof only to prove that the linearity condition is necessary.

**Proof.** (Corollary B.2.) Assumption B.7 implies that Assumption B.5 holds for all possible choices for the excluded treatment. We can thus apply Proposition B.8 for each choice for the excluded treatment.

**Proof.** (Proposition B.9). First, assume the conditions in the Proposition are true and fix a pair \(\{s, k\}\) for which neither \(s_k(v) = 0\) for all \(v\) nor \(s_k(v) = 1\) for all \(v\) (\(s\) is neither an always-\(k\)-taker nor a never-\(k\)-taker). We need to show that \(s(k) = k\). By Condition 3, we must have \(s(0) = 0\). If \(s(k) = l\) with \(l \neq k\) we must have, by Condition 2 (irrelevance), that \(s_l(k) = s_l(0) = 0 \Rightarrow s(k) \neq l\), a contradiction. Thus \(s(k) = k\).

The reverse direction of the equivalence is straightforward to verify.

### B Additional Results

#### B.1 Relationship to Weak Causality

Blandhol et al. (2022) call an estimand weakly causal if it has the correct sign when the treatment effect have the same sign across all agents. In our setting, 2SLS is weakly causal if \(\beta_{k}^{2SLS} \geq 0\) \((\leq 0)\) whenever \(\beta_{s}^{*} \geq 0\) \((\leq 0)\) for all \(s \in S\) for \(k \in \{1, 2\}\). In this section, we show that 2SLS assigns proper weights if and only if 2SLS is weakly causal for all possible heterogeneous effects.

First assume 2SLS is weakly causal for all possible heterogeneous effects. Assume the causal effect of treatment 1 for response type \(s\) is \(\beta_{1}^{s} = 1\) and all other causal effects are 0. Weak causality then requires that \(\beta_{1}^{2SLS}\) is non-negative. Since, by Proposition 1, \(\beta_{1}^{2SLS} = E \left[ w_{11}^{S} \beta_{1}^{S} + w_{12}^{S} \beta_{2}^{S} \right] = w_{11}^{*} \Pr [S = s] \) in this case, weak causality implies that \(w_{11}^{*}\) is non-negative. Similarly, weak causality requires that the 2SLS estimate for treatment 2 is zero.\(^{68}\) Since, by Proposition 1, \(\beta_{2}^{2SLS} = E \left[ w_{21}^{S} \beta_{1}^{S} + w_{22}^{S} \beta_{2}^{S} \right] = w_{21}^{*} \Pr [S = s] \), weak causality implies that \(w_{21}^{*}\) is zero. The argument can be repeated for all response types and for the remaining two elements of the weight matrix. Thus, if 2SLS is weakly causal for all possible heterogeneous effects we must have that the

---

\(^{67}\)The proof is a straightforward generalization of Proposition 4 in Blandhol et al. (2022).

\(^{68}\)Weak causality requires that the 2SLS estimate is non-negative (non-positive) if the effect for all response types is non-negative (non-positive). When the treatment effect is zero (both non-negative and non-positive) for all response types, the 2SLS estimate is then required to be zero (both non-negative and non-positive).
weight matrix \( w^s \) is non-negative diagonal for all response types \( s \). The proof that proper weights implies weak causality is trivial.

**B.2 Generalization to More Than Three Treatments**

In this section, we show how our results generalize to the case with more than three treatments. Assume there are \( n + 1 \) treatments: \( T \in \mathcal{T} \equiv \{0, 1, \ldots, n\} \). Otherwise, the notation remains the same. In particular, the treatment effect of treatment \( k \) relative to treatment \( t \) is given by \( Y (k) - Y (l) \). Using 2SLS, we can seek to estimate \( n \) of these treatment effects denoted by the random vector \( \beta \equiv (\delta_1, \delta_2, \ldots, \delta_n)^T \). For instance, we might be interested in unordered treatment effects—the causal effect of each treatment \( T > 0 \) compared to the excluded treatment \( T = 0 \). In that case, we choose \( \delta_t = Y (t) - Y (0) \). Alternatively, we might be interested in estimating ordered treatment effects—the causal effect of treatment \( T = t \) compared to treatment \( T = t - 1 \)—in which case we define \( \delta_t = Y (t) - Y (t - 1) \).

Once we have determined the treatment effects of interest, we define corresponding treatment indicators \( D \equiv \{D_1, \ldots, D_n\} \) where \( D_t = 1 [T \in R_t] \) for a subset \( R_t \subset \{0, \ldots, n\} \). The subsets \( R_t \) are chosen by the researcher to correspond to the treatment effects of interest. In particular, the sets \( R_t \) are selected such that \( Y = \alpha + D\beta \) where \( \alpha \) is a function of \( \{Y (0), Y (1), \ldots, Y (n)\} \). For instance, if \( \delta_t = Y (t) - Y (0) \) we define \( D_t = 1 [T = t] \) (i.e., \( R_t = \{t\} \)) such that \( Y = Y (0) + D\beta \). Alternatively, if \( \delta_t = Y (t) - Y (t - 1) \) we define \( D_t = 1 [T \geq t] \) (e.g., \( R_t = \{t, t + 1, \ldots, n\} \)). In Figure B.1, we show these two special cases and a third generic case as directed graphs where an arrow from \( T = l \) to \( T = k \) indicates that we are interested in estimating the treatment effect \( Y (k) - Y (l) \). By using different treatment indicators one can estimate any subset of \( n \) treatment effects that connects the treatments in an acyclic graph.\(^69\)

For a response type \( s \in \mathcal{S} \), define by \( v_s \) the induced mapping between instruments and treatment indicators: \( v_s (z) \equiv (s_1 (z), \ldots, s_n (z))^T \) for all \( z \in \mathcal{Z} \) with \( s_t (z) \equiv 1 [s (z) \in R_t] \). Proposition 1 generalizes directly to this setting with more than three treatments—the only required modification to the proof is to replace \( Y (0) \) with \( \alpha \). Corollaries 2–3 generalize as follows: The weight on \( \beta_{sk} \) in \( \beta_k^{2SLS} \) is non-negative for all \( s \) if and only if, for all \( s \), the partial correlation between \( P_k \) and \( s_k (Z) \) given \( (P_1, \ldots, P_{k-1}, P_{k+1}, \ldots, P_n)^T \) is non-negative. And the weight on \( \beta_{sl} \) in \( \beta_k^{2SLS} \) is zero for all \( s \) and \( l \neq k \) if and only if, for all \( s \) and \( l \neq k \), the partial correlation between \( P_k \) and \( s_l (Z) \) given \( (P_1, \ldots, P_{k-1}, P_{k+1}, \ldots, P_n)^T \) is zero.

\(^69\) Formally, when \( \delta_t = Y (k) - Y (l) \), one can define \( D_t = 1 [T \in R_t] \) where \( R_t \) is the set of all treatments that are connected to \( l \) through \( k \) in the graph of treatment effects.
Figure B.1: Treatment Effects Corresponding to Different 2SLS Specifications.

(a) Unordered Treatment Effects.

(b) Ordered Treatment Effects.

(c) Generic Example.

Note: Three possible choices for the set of treatment effects of interest $\beta$. An arrow from $T = k$ to $T = l$ means that the researcher is interested in the treatment effect $Y(k) - Y(l)$. In Figure (a), the treatment effects of interest are $\delta_t = Y(t) - Y(0)$. The corresponding treatment indicators are $D_t = 1[T = t]$. In Figure (b), the treatment effects of interest are $\delta_t = Y(t) - Y(t - 1)$ and the corresponding treatment indicators are $D_t = 1[T \geq t]$. In Figure (c),

$$
\beta = \begin{bmatrix}
Y(1) - Y(2) \\
Y(0) - Y(2) \\
Y(0) - Y(3) \\
Y(4) - Y(0)
\end{bmatrix}
$$

and

$$D = \begin{bmatrix}
1[T = 1] \\
1[T \in \{0, 3, 4\}] \\
1[T \in \{0, 1, 2, 4\}] \\
1[T = 4]
\end{bmatrix}.$$ 

B.2.1 The Just-Identified Case

The proof of Proposition 4 is stated in terms of $n$ treatments and the result thus trivially extends to more than three treatments. To interpret Proposition 4 with more than three treatments, introduce the following terminology: For each $k$, call response type $s$ an always-$k$-taker if $s_k(v) = 1$ for all $v$, a never-$k$-taker if $s_k(v) = 0$ for all $v$, and a $k$-complier if $s_k(v) = 1 \iff v = k$. A $k$-complier thus selects treatment $k$ only when instrument $k$ is turned on.
Table B.1: Allowed Response Types in the Just-Identified Case with Four Treatments.

| Response Type   | \((s(0), s(1), s(2), s(3))\) |
|-----------------|--------------------------------|
| Never-taker     | \(0, 0, 0, 0\)                |
| Always-1-taker  | \(1, 1, 1, 1\)                |
| Always-2-taker  | \(2, 2, 2, 2\)                |
| Always-3-taker  | \(3, 3, 3, 3\)                |
| 1-complier      | \(0, 1, 0, 0\)                |
| 2-complier      | \(0, 0, 2, 0\)                |
| 3-complier      | \(0, 0, 0, 3\)                |
| 1+2-complier    | \(0, 1, 2, 0\)                |
| 1+3-complier    | \(0, 1, 0, 3\)                |
| 2+3-complier    | \(0, 0, 2, 3\)                |
| Full complier   | \(0, 1, 2, 3\)                |

Corollary B.1. Under Assumption 5, 2SLS assigns proper weights if and only if, for all \(k \neq 0\), all agents are either always-\(k\)-takers, never-\(k\)-takers, or \(k\)-compliers.

In words, only instrument \(k\) can influence treatment \(k\). For instance, in Table B.1 we show all allowed response types in the case with four treatments. In the case of unordered treatments effects, \(D_t = 1 [T = t]\), treatment 0 must play a special role: Unless response type \(s\) always selects the same treatment, we must have either \(s(k) = k\) or \(s(k) = 0\), for all \(k\). If instrument \(k\) is an inducement to take up treatment \(k\), an agent can thus never select any other treatment \(l \notin \{0, k\}\) when induced to take up treatment \(k\), unless the agent always selects treatment \(l\).

### B.3 Multivariate 2SLS with Covariates

In this section, we show how our results generalize to 2SLS with multiple treatments and control variables. In particular, assume we have access to a vector of control variables \(X\) with finite support such that the instruments \(Z\) are exogenous only conditional on \(X\).

Assumption B.1. (Conditional Exogeneity and Exclusion). \(\{Y(0), \ldots, Y(n), S\} \perp Z \mid X\)

We will consider the case of running 2SLS while flexibly controlling for \(X\) using fully saturated fixed effects. In particular, define the demeaned variables \(\bar{Y} = Y - E[Y \mid X]\), \(\bar{D} = D - E[D \mid X]\), and \(\bar{Z} = Z - E[Z \mid X]\). The 2SLS estimand, controlling for \(X\)-specific fixed effects, is then \(\beta^{2SLS} \equiv \text{Var}(\bar{P})^{-1} \text{Cov}(\bar{P}, \bar{Y})\) where \(\bar{P} \equiv \text{Var}(\bar{Z})^{-1} \text{Cov}(\bar{Z}, \bar{D})\). This estimand is equivalent to the one obtained from 2SLS while controlling for indicator variables for each

---

70 We consider the case with \(n + 1\) treatments \((T \in \{0, \ldots, n\})\) also in this section.

71 Blandhol et al. (2022) show that 2SLS specifications that are not saturated in the controls might fail to assign proper weights even in the case of a binary treatment.
possible value of $X$ in the first and the second stage. Let $\mathcal{S}_x$ be the set of all response types $s$ such that $\Pr[S = s \mid X = x] > 0$. We maintain the following assumption.

**Assumption B.2.** *(Rank).* $\text{Cov} \left( \tilde{Z}, \tilde{D} \right)$ has full rank.

Proposition 1 then generalizes as follows:

**Proposition B.1.** *Under Assumptions B.1 and B.2*

$$\beta^{2\text{SLS}} = \mathbb{E} \left[ w_{S,X} \beta^{S,X} \right]$$

where, for $s \in \mathcal{S}_x$ and $x \in X$

$$w_{s,x} \equiv \text{Var} \left( \tilde{P} \right)^{-1} \text{Cov} \left( \tilde{P}, v_s(Z) \mid X = x \right)$$

$$\beta^{s,x} \equiv (\beta_1^{s,x}, \ldots, \beta_n^{s,x})^T \equiv \mathbb{E} \left[ \beta \mid S = s, X = x \right]$$

Thus, 2SLS with controls assigns proper weights if and only if $w_{s,x}$ is a non-negative diagonal matrix for all $s \in \mathcal{S}_x$ and $x \in X$. This condition is in general hard to interpret. But the condition has a straightforward interpretation under the following assumption:

**Assumption B.3** *(Predicted treatments covary similarly across $X$).*

$$\text{Var} \left( \tilde{P} \mid X = x \right) = a_x \text{Var} \left( \tilde{P} \right) \text{ for all } x \in X \text{ and constants } a_x > 0.$$

Then the condition reduces to Assumption 3 and 4 holding conditional on $X$:\(^{72}\) The 2SLS estimate of the effect of treatment $k$ assigns proper weights if and only if, for all agents, there is a non-negative (zero) partial correlation between predicted treatment $k$ and potential treatment $k$ ($l \neq k$) *conditional on $X$*. For instance, consider a random judge design where random assignment of cases to judges holds only within courts and we are interested in separately estimating the effect of incarceration and conviction on defendant future outcomes. Assume incarceration rates and conviction rates vary in a similar way across courts.\(^{73}\) Then, the 2SLS estimate of the effect of incarceration assigns proper weights if and only if the following holds for each case $i$ among the judges sitting in the court where the case is filed (i.e., conditional on $X$):\(^{74}\)

\(^{72}\)I.e., to $\text{Var} \left( \tilde{P} \mid X = x \right)^{-1} \text{Cov} \left( \tilde{P}, v_s(Z) \mid X = x \right)$ being a non-negative diagonal matrix for all $s \in \mathcal{S}_x$ and $x \in X$. Note that predicted treatment is predicted based on $Z$ and $X$, but not the interaction.

\(^{73}\)In this setting, $\text{Var} \left( \tilde{P} \mid X = x \right) = a_x \text{Var} \left( \tilde{P} \right)$ is equivalent to (i) the correlation between incarceration and conviction rates between judges not varying across courts and (ii) the ratio between the standard deviation of incarceration rates and the standard deviation of conviction rates not varying across courts.

\(^{74}\)Using the assigned judge as the instrument, the predicted treatments are the judge’s incarceration and conviction rates. In applied work, these rates are typically estimated by leave-one-out estimates to avoid small sample bias.
1. There is a non-negative correlation between incarcerating in case \( i \) and the judge’s incarceration rate conditional on the judge’s conviction rate

2. There is no correlation between convicting in case \( i \) and the judge’s incarceration rate conditional on the judge’s conviction rate

Note that 2SLS could assign proper weights even though Assumptions 3 and 4 do not hold conditional on \( X \): It might that the bias from such violations are counteracted by bias coming from predicted treatments covarying differently across values of \( X \). This case, however, seems hard to characterize in an interpretable way. Also note that Assumption B.3 only depends on population moments and can thus be easily tested.

It is useful to contrast 2SLS with multiple treatments and covariates to OLS with multiple treatment and covariates. Goldsmith-Pinkham et al. (2022) show that OLS with controls and multiple treatments is generally affected by “contamination bias”—the estimated effect of one treatment being contaminated by the effects of other treatments. OLS can be seen as a special case of 2SLS with \( Z = D \). Proposition B.1 then implies that there is no contamination bias in OLS whenever \( \text{Var}(\hat{D} | X = x) = a_x \text{Var}(\hat{D}) \) for constants \( a_x > 0 \). Since \( D \) is restricted to be either zero or one, this condition can only hold when \( E[D | X] = E[D] \)—when treatment is unconditionally independent. Goldsmith-Pinkham et al. (2022) show that this is the only case in which OLS does not suffer from contamination bias under arbitrary treatment effect heterogeneity. This result, however, does not generalize to 2SLS. In the case of 2SLS, the predicted treatments \( P \) are not restricted to be either zero or one. One can easily show that it is possible that Assumption B.3 is satisfied without instruments being unconditionally independent. It is thus possible for 2SLS with multiple treatments and covariates to not suffer from contamination bias even though the instruments are independent only when conditioning on the controls.

If Assumptions 3 and 4 hold conditional on \( X \) but Assumption B.3 is violated, 2SLS will suffer from contamination bias. In that case, one can avoid contamination bias by the Angrist & Pischke (2009) “saturate and weight” approach—running 2SLS fully interacted with \( X \). Such an approach, however, requires a large number of observations per value of \( X \) to avoid issues of weak instruments.

---

75In the OLS case, we get \( w_{x,x} = \text{Var}(\hat{D})^{-1} \text{Var}(\hat{D} | X = x) \).

76The condition implies that, for \( x_1, x_2 \in \mathcal{X} \), we must have \( \text{Var}(D | X = x_1) = a_{x_1 x_2} \text{Var}(D | X = x_2) \) for a constant \( a_{x_1 x_2} \). In other words, we must have \( E[D_k | X = x_1](1 - E[D_k | X = x_2]) = a_{x_1 x_2} E[D_k | X = x_2](1 - E[D_k | X = x_2]) \) and \( E[D_k | X = x_1]E[D_l | X = x_2] = a_{x_1 x_2} E[D_k | X = x_2]E[D_l | X = x_2] \) for all \( k \) and \( l \neq k \). This is only possible when \( E[D | X = x_1] = E[D | X = x_2] \) and \( a_{x_1 x_2} = 1 \).

77For instance, consider a random judge design with two courts, \( X \in \{0, 1\} \) with two judges each. In Court 0 (\( X = 0 \)), the two judges have conviction and incarceration rates of \((0.6, 0.3)\) and \((0.7, 0.2)\). In Court 1 (\( X = 1 \)), the two judges have conviction and incarceration rates of \((0.5, 0.4)\) and \((0.8, 0.1)\). Then \( \text{Var}(\hat{P} | X = 1) = 9 \text{Var}(\hat{P} | X = 0) \). Had incarceration and conviction rates been forced to be either zero or one—as in OLS—it would not have been possible to construct such an example.

78Such an approach is or equivalent to running separate 2SLS regressions for each value of \( X \) and then
In the case of a binary treatment, Blandhol et al. (2022) show that 2SLS with flexible controls assigns proper weights under a monotonicity-correctness condition. The requirement that average conditional monotonicity (Assumption 3) needs to hold conditional on each value of \( X \) can be seen as a weakening of monotonicity-correctness.\(^{79}\)

Proposition 3 generalizes to 2SLS with covariates as follows:

**Proposition B.2.** Assume \( W \in \{0,1\} \) with \( Z \perp W \mid S,X \). If 2SLS controlling flexibly for \( X \) assigns proper weights then \( \text{Var} \left( \tilde{P} \right)^{-1} \text{Cov} \left( \tilde{P}, \tilde{D} \mid W = 1 \right) \) is a diagonal non-negative matrix.

Note that this prediction can not be tested by a linear regression in the same way that the prediction in Proposition 3 can. The reason is that the covariance between \( \tilde{P} \) and \( \tilde{D} \) at the sample \( W = 1 \) is related to the variance of \( \tilde{P} \) in a different sample—the full population. Instead, one can test this prediction by, e.g., comparing the sample analog of \( \text{Var} \left( \tilde{P} \right)^{-1} \text{Cov} \left( \tilde{P}, \tilde{D} \mid W = 1 \right) \) to critical values obtained through resampling methods.

Due to the potential presence of contamination bias, Propositions 4, 5, and 8 do not immediately generalize to the case with covariates. The results, however, do hold if one is willing to maintain Assumption B.3 and that the first stage is sufficiently flexible to capture differences in how agents respond to the instruments across \( X \):

**Assumption B.4 (First stage capturing heterogeneity across \( X \)).**

\[
\tilde{P} = P_X - E[D \mid X]
\]

where \( P_X \equiv E[D \mid X] + \text{Var} (Z \mid X)^{-1} \text{Cov} (Z, D \mid X) (Z - E[Z \mid X]) \).

This assumption requires that the predicted treatments estimated on the full sample coincides with predicted treatments estimated separately for each value of \( X \). Assumption B.4 holds trivially when the instruments are flexibly interacted with \( X \). In the just-identified case, Assumption B.4 is equivalent to

\[
E[D \mid Z,X] = \gamma Z + \phi_X
\]

for a matrix of constants \( \gamma \) and vectors of constants \( \phi_x \).\(^{80}\) We then have

**Proposition B.3.** Proposition 4 holds under Assumptions B.1–B.4 when \( X \) is being flexibly controlled for in the 2SLS specification.

aggregate the resulting estimates across \( X \). Goldsmith-Pinkham et al. (2022)’s solution to contamination bias in OLS—running one-treatment-at-a-time regressions—is not valid in the 2SLS case since it would entail running regressions on samples selected based on an endogenous variable (received treatment).

\(^{79}\)Essentially, Assumption 3 only requires monotonicity-correctness to hold on average across instrument values.

\(^{80}\)By definition, \( \tilde{P} = \gamma (Z - E[Z \mid X]) \) for a constant matrix \( \gamma \). Furthermore, when \( Z \) is a set of indicators, we have \( P_X = E[D \mid Z,X] \). The assumption allows the baseline probabilities of receiving the various treatments to vary across \( X \) but requires the first stage coefficients to be homogenous across \( X \).
Again, if the conditions in Proposition 4 hold but Assumption B.3 or Assumption B.4 is violated, 2SLS might suffer from contamination bias. If one has enough observations per value of $X$ this contamination bias can be avoided by running 2SLS fully interacted with $X$.

Proposition 5 generalizes to:

**Proposition B.4.** Maintain Assumptions B.1–B.4. Assume $W \in \{0, 1\}$ with $V \perp W \mid S, X$. If 2SLS assigns proper weights when controlling flexibly for $X$ then there is a permutation of the instruments $f : \{0, 1, \ldots, n\} \to \{0, 1, \ldots, n\}$ such that

$$\text{Var}(Z^* \mid W = 1, X = x)^{-1} \text{Cov}(Z^*, D \mid W = 1, X = x)$$

is a diagonal non-negative matrix where $Z^* = (Z^*_1, Z^*_2, \ldots, Z^*_n)^T$ with $Z^*_v \equiv 1[V = f(v)]$.

Finally, Proposition 8 generalizes as follows:

**Proposition B.5.** Maintain Assumptions B.1–B.4. Then 2SLS assigns proper weights when controlling flexibly for $X$ in the Section 3.2 model if $E[P_k \mid \tilde{P}_l, X = x]$ is linear in $\tilde{P}_l$ for all $k, l$, and $x$.

### B.4 Assumptions About Heterogeneous Effects

Average conditional monotonicity and no cross effects (Assumptions 3 and 4) make sure that 2SLS is weakly causal for arbitrary heterogeneous effects.\(^{81}\) But these assumptions can be relaxed if we are willing to make some assumptions about heterogeneous effects.

To analyze this case, define compliers (defiers) as the response types pushed into (out of) treatment 1 by instrument 1. To simplify language, we here refer to $P_1$ as “instrument 1” and $P_2$ as “instrument 2”.\(^{82}\) Define the following parameters:

---

\(^{81}\)See Section B.1.

\(^{82}\)All statements below are conditional on controlling linearly for the other instrument. E.g., “pushed into treatment 1 by instrument 1”=”pushed into treatment 1 by instrument 1 when controlling linearly for instrument 2”. We also change the notation of $w_S$ to $w^S$ to improve readability.
\[
\begin{align*}
\beta_{1}^{\text{compliers}} & \equiv \frac{\mathbb{E}[w_{1i}^{S}\beta_{1}^{S}|w_{1i}^{S}>0]}{\mathbb{E}[w_{1i}^{S}|w_{1i}^{S}>0]} & \text{weighted average effect of treatment 1 for compliers} \\
\beta_{1}^{\text{defiers}} & \equiv \frac{\mathbb{E}[w_{1i}^{S}\beta_{1}^{S}|w_{1i}^{S}<0]}{\mathbb{E}[w_{1i}^{S}|w_{1i}^{S}<0]} & \text{weighted average effect of treatment 1 for defiers} \\
\beta_{2}^{\text{pushed in}} & \equiv \frac{\mathbb{E}[w_{12}^{S}\beta_{2}^{S}|w_{12}^{S}>0]}{\mathbb{E}[w_{12}^{S}|w_{12}^{S}>0]} & \text{weighted average effect of treatment 2 for response types pushed into treatment 2 by instrument 1} \\
\beta_{2}^{\text{pushed out}} & \equiv \frac{\mathbb{E}[w_{12}^{S}\beta_{2}^{S}|w_{12}^{S}<0]}{\mathbb{E}[w_{12}^{S}|w_{12}^{S}<0]} & \text{weighted average effect of treatment 2 for response types pushed out of treatment 2 by instrument 1} \\
w^{\text{negative}} & \equiv \frac{\mathbb{E}[w_{1i}^{S}|w_{1i}^{S}<0]}{\mathbb{P}[w_{1i}^{S}<0]} & \text{sum of the negative weights on treatment 1 in } \beta_{1}^{2\text{SLS}} \\
w^{\text{cross}} & \equiv \frac{\mathbb{E}[w_{12}^{S}|w_{12}^{S}>0]}{\mathbb{P}[w_{12}^{S}>0]} & \text{sum of the positive weights on treatment 2 in } \beta_{1}^{2\text{SLS}}
\end{align*}
\]

We then obtain the following decomposition:83

\[
\beta_{1}^{2\text{SLS}} = \beta_{1}^{\text{compliers}} - \left(\beta_{1}^{\text{defiers}} - \beta_{1}^{\text{compliers}}\right)w^{\text{negative}} - \left(\beta_{2}^{\text{pushed out}} - \beta_{2}^{\text{pushed in}}\right)w^{\text{cross}}
\]

The 2SLS estimate thus equals the weighted average treatment effect among the compliers and two “bias terms”: The first bias term is non-zero if there are defiers and defiers have a different weighted average treatment effect than the compliers. The second bias term is non-zero if there exist response types pushed into or out of treatment 2 by instrument 1, and the weighted average effect of treatment 2 differ between those pushed into and out of treatment 2. We thus see that if we are willing to assume \(\beta_{1}^{\text{defiers}} = \beta_{1}^{\text{compliers}}\) and \(\beta_{2}^{\text{pushed out}} = \beta_{2}^{\text{pushed in}}\) —“no selection on gains”—2SLS is weakly causal without any restrictions on response types. This is, for instance, accomplished if \(\beta \perp S\) —treatment effects do not systematically differ across response types.

The 2SLS estimate is also weakly causal if we are willing to impose some restrictions on both response types and the amount of selection on gains. For instance, if we maintain \(w^{\text{cross}} = 0\) (no cross effects) then 2SLS is weakly causal if and only if84

\[
\frac{1 + w^{\text{negative}}}{w^{\text{negative}}} \geq \frac{\beta_{1}^{\text{defiers}}}{\beta_{1}^{\text{compliers}}}
\]

Thus, if \(w^{\text{negative}} = 0.1\) (roughly 8% defiers), 2SLS is weakly causal unless the treatment

83This identity follows directly from \(\beta_{1}^{2\text{SLS}} = \mathbb{E}[w_{1i}^{S}\beta_{1}^{S} + w_{12}^{S}\beta_{2}^{S}]\).

84Remember that the 2SLS estimand is weakly causal if \(\beta_{1}^{2\text{SLS}} \geq 0\) whenever \(\beta \geq 0\) (\(\beta \leq 0\)) for all agents. If \(\beta \geq 0\) for all agents, weak causality thus requires \(\beta_{1}^{\text{compliers}} - (\beta_{1}^{\text{defiers}} - \beta_{1}^{\text{compliers}})w^{\text{negative}} \geq 0 \Rightarrow \frac{1 + w^{\text{negative}}}{w^{\text{negative}}} \geq \frac{\beta_{1}^{\text{defiers}}}{\beta_{1}^{\text{compliers}}}\). The same inequality is obtained when \(\beta \leq 0\) for all agents.
effect for defiers is more than 11 times the treatment effect for compliers.\footnote{Since } Similarly, if we maintain \( w_{\text{negative}} \equiv 0 \) (average conditional monotonicity) then 2SLS is weakly causal if and only if\footnote{If } 
\[
\frac{1}{w_{\text{cross}}} \geq \frac{\beta_{2}^{\text{pushed out}} - \beta_{2}^{\text{pushed in}}}{\beta_{1}^{\text{compliers}}}.
\]

Thus, if \( w_{\text{cross}} = 0.1 \) (20% are pushed into or out of treatment 2 by instrument 1), 2SLS is weakly causal unless the difference in the treatment effects between those pushed into and out of treatment 2 is above ten times the treatment effect for compliers.\footnote{Since the weights on compliers sum to one when \( w_{\text{negative}} = 0 \), the magnitude of \( 2w_{\text{cross}} \) can be interpreted as the number of agents pushed into or out of treatment 2 by instrument 1 as a share of the complier population.}

### B.5 Relationship to Imbens-Angrist Monotonicity

In this section, we show how the conditions in Section 2.2 relate to the classical Imbens & Angrist (1994) monotonicity condition. We focus on the following natural generalization of this monotonicity condition to the case of multiple treatments:\footnote{Informally, IA monotonicity is satisfied when Imbens & Angrist (1994) monotonicity holds for each binary treatment indicator.}

**Assumption B.5. (IA Monotonicity.)** IA monotonicity is satisfied if for each treatment \( k \in \{1, \ldots, n\} \) and instrument values \( z \) and \( z' \) either \( s_{k}(z) \geq s_{k}(z') \) for all \( s \in S \) or \( s_{k}(z) \leq s_{k}(z') \) for all \( s \in S \).

This assumption requires Imbens & Angrist (1994) monotonicity to hold for all the treatment indicators \( \{D_{1}, \ldots, D_{k}\} \). It turns out that IA monotonicity is neither sufficient nor necessary for 2SLS to assign proper weights under multiple treatments. On the one hand, IA monotonicity is not sufficient to ensure proper weights since it does not rule out cross effects (Assumption 4). IA monotonicity might also fail to ensure average conditional monotonicity (Assumption 3) if the first stage is incorrectly specified. On the other hand, IA monotonicity is not necessary for 2SLS to assign proper weights since Assumption 3 only requires potential treatment \( k \) to be non-decreasing, on average, in predicted treatment \( k \).\footnote{IA monotonicity requires potential treatment to be \textit{strictly} non-decreasing in the probability of receiving treatment \( k \) conditional on the instruments. When there are many instrument values, as in for instance random judge designs with many judges, IA monotonicity is considerably more demanding than Assumption 3. In the just-identified case with \( n \) treatments and \( n \) mutually exclusive binary instruments, however, Assumptions 3 and 4 do imply IA monotonicity.} Thus, to summarize

\footnote{IA monotonicity requires potential treatment to be \textit{strictly} non-decreasing in the probability of receiving treatment \( k \) conditional on the instruments. When there are many instrument values, as in for instance random judge designs with many judges, IA monotonicity is considerably more demanding than Assumption 3. In the just-identified case with \( n \) treatments and \( n \) mutually exclusive binary instruments, however, Assumptions 3 and 4 do imply IA monotonicity.}
Proposition B.6. i) IA monotonicity does not imply Assumption 4 nor Assumption 3.

ii) Assumptions 3 and 4 do not imply IA monotonicity.

But we can ask under which additional conditions IA monotonicity is sufficient to ensure that Assumptions 3 and 4 hold. To answer this question, consider the ideal case when Assumption 6 holds—the first stage equation is correctly specified. Under Assumption 6, IA monotonicity is equivalent to each agent selecting treatment \( k \) if predicted treatment \( k \) is above a certain threshold. Thus, for each response type \( s \) in the population there exists a value \( u \in [0,1] \) such that \( P_k \geq u \iff s_k(Z) = 1 \). It turns out that, even in this ideal case, IA monotonicity is not sufficient to ensure that Assumptions 3 and 4 hold—an additional linearity assumption is needed. In particular, if all possible thresholds are in use, multivariate 2SLS assigns proper weights under IA monotonicity and Assumption 6 if and only if the predicted treatments are linearly related:

Assumption B.6. (All Thresholds in Use.) For each \( k \in \{1, \ldots, n\} \) and threshold \( u \in [0,1] \) there exists a response type \( s \) in the population with \( P_k \geq u \iff s_k(Z) = 1 \).

Proposition B.7. Under Assumptions 1, 2, 6, B.5, and B.6, 2SLS assigns proper weights if and only if for all \( k, l \in \{1, \ldots, n\} \) \( E[P_k | P_l] = \delta_{kl} (1 - P_l) \) for constants \( \delta_{kl} \).

To see why the predicted treatments need to be linearly related, consider the following example. Assume there are three treatments and that the relationship between \( P_2 \) and \( P_1 \) is given by \( E[P_2 | P_1] = P_1 - P_2 \). The probability of receiving treatment 2 is highest when the probability of receiving treatment 1 is 50% and equal to zero when the probability of receiving treatment 1 is either zero or one. Since we only control linearly for \( P_1 \) in 2SLS, changes in \( P_2 \) will then push agents into or out of treatment 1, violating the no cross effects condition.

The required linearity condition is easy to test and ensures that 2SLS assigns proper weights also when Assumption B.6 does not hold:

Proposition B.8. If Assumptions 1, 2, 6, and B.5 hold and \( E[P_k | P_l] = \gamma_{kl} - \delta_{kl} P_l \) for all \( k, l \in \{1, \ldots, n\} \) and constants \( \gamma_{kl} \) and \( \delta_{kl} \), then 2SLS assigns proper weights.

In Section 3.2, we apply Proposition B.8 to the case when treatment is characterized by a latent index crossing multiple thresholds.

B.6 Relationship to Unordered Monotonicity

In this section, we show how the conditions in Section 2.2 relate to Heckman & Pinto (2018)'s unordered monotonicity condition. Consider the case of unordered treatments \( (D_k = 1[T = k]) \) and define \( s_0(z) \equiv 1[s(z) = 0] \). Unordered monotonicity is then defined by Heckman & Pinto (2018) as:
**Assumption B.7.** *(Unordered Monotonicity.)* Unordered monotonicity is satisfied if for each treatment \( k \in \{0, \ldots, n\} \) and instrument values \( z \) and \( z' \) either \( s_k(z) \geq s_k(z') \) for all \( s \in S \) or \( s_k(z) \leq s_k(z') \) for all \( s \in S \).

Note that unordered monotonicity is a strictly stronger condition than Assumption B.5.\(^{90}\) Assumption B.5 requires IA monotonicity to hold for indicators for each treatment except the excluded treatment. Unordered monotonicity requires IA monotonicity to hold also for an indicator for receiving the excluded treatment (\( k = 0 \)). Define \( P_0 \equiv \Pr[T = 0 \mid Z] \).\(^{91}\) We then get the following corollary of Proposition B.8:

**Corollary B.2.** If Assumptions 1, 2, B.7 and 6 hold and \( \mathbb{E}[P_k \mid P_l] = \gamma_{kl} - \delta_{kl}P_l \) for all \( k, l \in \{0, \ldots, n\} \) and constants \( \gamma_{kl} \) and \( \delta_{kl} \), then 2SLS assigns proper weights for all possible choices for the excluded treatment.

In other words, under unordered monotonicity and a linearity condition, 2SLS can identify all possible relative treatment effects, not just treatment effects relative to an excluded treatment.

### B.7 Knowledge of Next-Best Alternatives

Kirkeboen *et al.* (2016) and a following literature exploit data where agents’ next-best alternative—their treatment choice when \( V = 0 \)—is plausibly observed. In such settings, 2SLS can identify meaningful treatment effects under much weaker conditions than those of Corollary B.1. We show how our results relate to the Kirkeboen *et al.* (2016) approach in this section. Their approach is to first identify all agents with treatment \( k \) as their next-best alternative and then run 2SLS on this subsample using treatment \( k \) as the excluded treatment. By varying \( k \) one can obtain causal estimates of all relative treatment effects as opposed to only treatment effects relative to one excluded treatment. Kirkeboen *et al.* (2016) show that 2SLS assigns proper weights in this setting under two relatively mild conditions: monotonicity and irrelevance.\(^{93}\)

In our notation, their monotonicity condition is \( s_k(k) \geq s_k(0) \) for all \( s \) and \( k \) and their irrelevance condition is \( s_k(k) = s_k(0) \Rightarrow s_l(k) = s_l(0) \) for all \( s, k \) and \( l \). In words, irrelevance requires that if instrument \( k \) does not induce an agent into treatment \( k \), it can not induce her into treatment \( l \neq k \) either. To see how our results relate to the Kirkeboen *et al.* (2016) approach, assume we run 2SLS on the subsample of all agents we believe have treatment 0 as their next-best alternative using treatment 0 as the excluded treatment.\(^{94}\) It turns out that the conditions of Kirkeboen *et al.* (2016)

---

\(^{90}\)For instance, consider the just-identified case with three treatments where all the response types in Table 1 are present. In that case, Assumption B.5 is satisfied but unordered monotonicity is violated: We have \( s_0(2) > s_0(1) \) for “1-compliers” and \( s_0(2) < s_0(1) \) for “2-compliers.”

\(^{91}\)Under Assumption 6, \( P_k \equiv \Pr[T = k \mid Z] \) for \( k \in \{1, \ldots, n\} \).

\(^{92}\)Proposition B.7 has a similar corollary.

\(^{93}\)Heinesen *et al.* (2022) study the properties of 2SLS when irrelevance or the “next-best” assumption is violated and discuss how these conditions can be tested.

\(^{94}\)The corresponding result for subsamples of agents selecting treatment \( k \neq 0 \) is analog.
are almost equivalent to the conditions in Corollary B.1 holding in the subsample, with the only exception that our conditions allow for the presence of always-takers:  

**Proposition B.9.** Maintain Assumption 5 and assume $D_t = 1 [T = t]$. The conditions in Corollary B.1 are then equivalent to the following conditions holding for all response types $s$ in the population:

1. $s_k(k) \geq s_k(0)$ for all $k$ (Monotonicity)
2. $s_k(k) = s_k(0) \Rightarrow s_l(k) = s_l(0)$ for all $k$ and $l$ (Irrelevance)
3. Either $s(0) = 0$ or there is a $k$ such that $s(v) = k$ for all $v$ (Next-best alternative or always-taker).

In words, 2SLS applied on a subsample assigns proper weights if and only if monotonicity and irrelevance holds and the subsample is indeed composed only of (i) agents with the excluded treatment as their next-best alternative and (ii) always-takers. The conditions of Kirkeboen et al. (2016) are thus not only sufficient for 2SLS to assign proper weights, but also close to necessary—they only way they can be relaxed is by allowing for always-takers. Thus, knowing next-best alternatives is necessary for 2SLS to assign proper weights in just-identified models with unordered treatment effects. In other words, when interpreting estimates from a just-identified 2SLS model with multiple treatments as a positively weighted sum of individual treatment effects one implicitly assumes knowledge about agents’ next-best alternatives.

---

*The approach of Kirkeboen et al. (2016) is to run 2SLS on the subsample of agents having the same treatment as their next-best alternative. Trivially, however, there is no problem if some agents with another next-best alternative are included in the sample, as long as they always select this alternative—always-takers do not affect 2SLS estimates.*