On Estimating Multiple Treatment Effects with Regression*

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Abstract
We study the causal interpretation of regressions on multiple dependent treatments and flexible controls. Such regressions are often used to analyze randomized control trials with multiple intervention arms, and to estimate institutional quality (e.g. teacher value-added) with observational data. We show that, unlike with a single binary treatment, these regressions do not generally estimate convex averages of causal effects—even when the treatments are conditionally randomly assigned and the controls fully address omitted variables bias. We discuss different solutions to this issue, and propose as a solution a new class of efficient estimators of weighted average treatment effects.

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1 Introduction

Consider a linear regression of an outcome $Y_i$ on a vector of treatment indicators $X_i$ and a vector of controls $W_i$. The treatments are assumed to be as-good-as-randomly assigned to the observations $i$, conditional on the controls, and are not independent of each other. For example, $X_i$ may indicate the assignment to mutually exclusive interventions in an randomized control trial (RCT), with the randomization protocol varying across some experimental strata indicators in $W_i$. Or, in an education value-added model (VAM), $X_i$ may indicate the matching of students $i$ to different teachers or schools, with $W_i$ including measures of student demographics, lagged achievement, or other controls which give a credible selection-on-observables assumption. Such regressions are widely used across different fields of economics to estimate the potentially heterogeneous effects of $X_i$ on $Y_i$.\footnote{Prominent examples of the RCT setting include Project STAR (Krueger, 1999) and the RAND Health Insurance Experiment (Manning et al., 1987). Prominent examples of the VAM setting include studies of teachers (Kane & Staiger, 2008; Chetty et al., 2014), schools (Angrist et al., 2017; Mountjoy & Hickman, 2020), and healthcare institutions (Hull, 2018a; Abaluck et al., 2020; Geruso et al., 2020).}

This paper shows that such regressions generally fail to identify convex weighted averages of heterogeneous treatment effects, and discusses solutions to this problem. The general lack of a causal interpretation in multiple treatment regression models may be surprising given the influential result of Angrist (1998), who shows that regressions on a single binary treatment $D_i$ and controls $W_i$ capture a convex weighted average of effects whenever (i) $D_i$ is conditionally as-good-as-randomly assigned and (ii) the conditional probability of assignment is linear in $W_i$ (such as when $W_i$ consists of a set of group indicators). We show that this result does not generalize for multiple treatment regressions, even when generalizations of (i) and (ii) hold. Despite a set of binary treatments being completely randomly assigned within groups, as in a stratified multi-armed RCT, a regression on treatment and strata indicators does not generally yield causally interpretable regression coefficients.

We derive a general characterization of this problem, for regressions with potentially non-binary treatments and controls, under the assumptions of linear (but potentially heterogeneous) causal effects, conditionally ignorable treatment assignment, and sufficiently flexible parameterizations of the controls to avoid omitted variables bias. We focus on the example of multiple unordered treatment indicators, as in the RCT and VAM examples, where the assumption of linear causal effects holds trivially. We show that the regression coefficient on each treatment identifies a weighted average of its causal effects, plus a bias term that is generally non-zero. The own-effect weights are always convex in the unordered treatment indicator case. The bias term is given by a linear combination of the causal effects of other treatments, with weights that sum to zero. Thus, except when effects are constant or uncorrelated with the bias weights, each treatment effect estimate will be contaminated with the effects of other...
treatments. Since the weights on the other treatment effects sum to zero, some are necessarily negative, further complicating the interpretation of the regression coefficients.

This issue echoes—but is fundamentally distinct from—recent concerns about negative weights in difference-in-difference or “event study” regressions (Goodman-Bacon, 2018; Sun & Abraham, 2020; de Chaisemartin & D'Haultfoeuille, 2020; Callaway & Sant’Anna, 2020; Borusyak et al., 2021). One difference is that it arises even under complete (conditional) random assignment of the treatments, and is thus not a consequence of the weaker “parallel trends” assumption that this literature focuses on. The problem we study is more closely related to issues with multiple-treatment panel data regressions previously studied by Hull (2018b) and de Chaisemartin and D’Haultfoeuille (2021), though again we establish it in a general setting of conditional random assignment and non-binary regressors. Our characterization also relates to concerns in interpreting multiple-treatment linear instrumental variables (IV) estimates with heterogeneous treatment effects (Behaghel et al., 2013; Kirkeboen et al., 2016; Hull, 2018c; Kline & Walters, 2019). We discuss how this characterization also has implications for the interpretation of single-treatment IV estimates, with multiple conditionally as-good-as-randomly assigned instruments.²

We then discuss different solutions to this problem, along with their trade-offs. One conceptually simple solution is to adapt approaches to estimating the average treatment effect (ATE) of a binary treatment under unconfoundedness (see, e.g. Imbens & Wooldridge, 2009, for a review) to the case with multiple treatments (e.g. Cattaneo, 2010; Graham et al., 2018; Chernozhukov et al., 2021). For example, one could run an expanded regression that includes interactions between the treatments and (de-meaned) controls. While such ATE estimators achieve the semi-parametric efficiency bound under strong overlap of the covariate distribution for units in each treatment arm, they can be imprecise or infeasible under limited overlap. This observation motivates an alternative solution: to instead estimate a convex weighted average of treatment effects while avoiding the bias of multiple-treatment regressions. We derive the weights that are “easiest” to estimate in that they achieve the semi-parametric efficiency bound. We show that these weights coincide with the implicit linear regression weights when the treatment is binary, formalizing a virtue of regression adjustment in this case. Future drafts will illustrate these solutions across different empirical settings.

We structure the rest of the paper as follows. Section 2 first illustrates the issue in a simple example with a treatment that takes on three values and one binary control. Section 3 then characterizes the general problem in regressions with multiple treatments and controls. Section 4 discusses the alternative solutions and their respective robustness and efficiency properties. Section 5 concludes. All proofs and extensions are given in Appendix A.

²Prominent examples of such IV settings are found with “judge” or “examiner” instruments; see, e.g., Kling (2006), Maestas et al. (2013), Dobbie and Song (2015), and Arnold et al. (2020).
2 Motivating example

We build intuition for the problem in two simple examples. We first consider a regression on a single binary treatment and a single binary control, and show how it identifies a convex average of heterogeneous treatment effects. We then show how this result fails to generalize when we introduce an additional treatment arm. For concreteness, we base these examples on a stylized version of the Project STAR experiment (Krueger, 1999), which studied the effects of two educational interventions on student achievement: a reduction in class size and an introduction of full-time teaching aides.

2.1 Convex weights with a single treatment

Consider the regression of an outcome $Y_i$ on a single treatment indicator $D_i \in \{0, 1\}$, a single binary control $W_i \in \{0, 1\}$, and a constant:

$$Y_i = \alpha + \beta D_i + \gamma W_i + U_i.$$  

(1)

By definition, $U_i$ is a mean-zero regression residual that is uncorrelated with $D_i$ and $W_i$. Krueger (1999), for example, primarily studied the effect of small class size $D_i$ on the average test scores $Y_i$ in a population of middle school students indexed by $i$. Project STAR randomized students to classes within schools with at least three classes per grade. The number of students assigned to each intervention thus varied both by the number of students in a school and the relative classroom size. To account for this non-random treatment variation, Krueger (1999) followed earlier analyses of Project STAR in estimating regressions with school (and sometimes school-by-period) fixed effects as controls. Such specifications are often found in stratified RCTs with varying treatment assignment rates across a set of pre-treatment strata. If we imagine two such strata, demarcated by a binary indicator $W_i$, then eq. (1) corresponds to a stylized two-school version of a Project STAR regression.

We wish to interpret the regression coefficient $\beta$ in terms of the causal effects of $D_i$ on $Y_i$. For this we use standard potential outcomes notation, letting $Y_i(d)$ denote the test scores of student $i$ when $D_i = d$. The potentially heterogeneous causal effects are then $\tau_i = Y_i(1) - Y_i(0)$, and we can write realized achievement as $Y_i = Y_i(0) + \tau_i D_i$. To formalize the random assignment of treatment within schools, we assume that $D_i$ is conditionally independent of potential outcomes given the control $W_i$:

$$(Y_i(0), Y_i(1)) \perp D_i \mid W_i.$$  

We also write $p(W_i) = \Pr(D_i = 1 \mid W_i) = E[D_i \mid W_i]$ as the conditional treatment assignment
probability, or propensity score. Since $W_i$ binary, this is a linear function: $p(W_i) = \mu + \pi W_i$ for $\mu = E[D_i \mid W_i = 0]$ and $\pi = E[D_i \mid W_i = 1] - E[D_i \mid W_i = 0]$.

Angrist (1998) showed that regression coefficients like $\beta$ identify a weighted average of within-strata ATEs, with particular convex weights. Formally, in our stylized Project STAR regression, this result shows

$$\beta = \phi \tau(0) + (1 - \phi) \tau(1), \quad \phi = \frac{\text{var}(D_i \mid W_i = 0) \Pr(W_i = 0)}{\sum_{w=0}^{1} \text{var}(D_i \mid W_i = w) \Pr(W_i = w)},$$

where $\tau(w) = E[Y_i(1) - Y_i(0) \mid W_i = w]$ is the ATE in school $w \in \{0, 1\}$ and, since variances are non-negative, $\phi \in [0, 1]$ gives a convex weighting scheme. Thus, in our example the treatment coefficient $\beta$ identifies a proper weighted average of small classroom effects $\tau(w)$ across the two schools.

Equation (2) can be derived from a simple application of the Frisch-Waugh-Lovell Theorem. The theorem shows that the multivariate regression coefficient $\beta$ can be represented as a bivariate regression coefficient, using residualized regressors:

$$\beta = \frac{E[\tilde{D}_i Y_i]}{E[\tilde{D}_i^2]} = \frac{E[\tilde{D}_i (Y_i(0) + \tau_i D_i)]}{E[\tilde{D}_i^2]}$$

where $\tilde{D}_i$ denotes the population residuals from regressing $D_i$ on $W_i$ and a constant. We substitute the potential outcomes model of $Y_i$ in the second equality of eq. (3). Here, since $E[D_i \mid W_i]$ is linear, $\tilde{D}_i = D_i - E[D_i \mid W_i]$. Thus, $E[\tilde{D}_i \mid W_i] = E[D_i \mid W_i] - E[D_i \mid W_i] = 0$ and it follows under conditional random assignment (eq. (1)) that

$$E[\tilde{D}_i Y_i(0)] = E[E[\tilde{D}_i Y_i(0) \mid W_i]] = E[E[\tilde{D}_i \mid W_i] E[Y_i(0) \mid W_i]] = 0,$$

so that

$$\beta = \frac{E[\tilde{D}_i D_i \tau_i]}{E[\tilde{D}_i^2]}.$$

The first equality in eq. (4) follows from the law of iterated expectations, the second equality follows by conditional random assignment, and the third equality uses $E[\tilde{D}_i \mid W_i] = 0$. Moreover, similar steps show that

$$E[\tilde{D}_i \tau_i D_i] = E[E[\tilde{D}_i \tau_i D_i \mid W_i]] = E[E[\tilde{D}_i D_i \mid W_i] E[\tau_i \mid W_i]] = E[\text{var}(D_i \mid W_i) \tau(W_i)],$$

where $\text{var}(D_i \mid W_i) = E[\tilde{D}_i^2 \mid W_i]$ gives the conditional variance of the small-class treatment.
within schools. Since $E[\text{var}(D_i \mid W_i)] = E[E[\hat{D}_i^2 \mid W_i]] = E[\hat{D}_i^2]$, it follows that we can write

$$
\beta = \frac{E[\text{var}(D_i \mid W_i) \tau(W_i)]}{E[\text{var}(D_i \mid W_i)]} = \phi \tau(0) + (1 - \phi) \tau(1),
$$

proving the representation of $\beta$ in eq. (2).

Key to this derivation is that the auxiliary regression of the treatment $D_i$ on the other regressors $W_i$ identifies the conditional expectation $E[D_i \mid W_i]$. By the Frisch-Waugh-Lovell theorem, treatment coefficients like $\beta$ can always be represented as in eq. (3) even without this property. We next show, however, that the remaining steps in the derivation of eq. (6) fail when an additional treatment arm is included. This failure can be attributed to the fact that the auxiliary regression no longer identifies a conditional expectation, leading to an additional bias term in the expression for the regression coefficient.

### 2.2 Bias with multiple treatments

In reality, as noted above, Project STAR randomized two mutually exclusive interventions within schools: a reduction in class size ($D_i = 1$) and the introduction of full-time teaching aides ($D_i = 2$). We incorporate this extension of our stylized example by now considering a regression of student achievement $Y_i$ on a $2 \times 1$ vector of treatment indicators $X_i = [X_{i1}, X_{i2}]'$, where the first treatment $X_{i1} = 1\{D_i = 1\}$ indicates assignment to a small class and the second treatment $X_{i2} = 1\{D_i = 2\}$ indicates assignment to a class with a full-time aide. We continue to include the school indicator control $W_i$ and a constant, yielding the regression

$$
Y_i = \alpha + \beta_1 X_{i1} + \beta_2 X_{i2} + \gamma W_i + U_i,
$$

where again the residual $U_i$ is uncorrelated with the regressors by definition. As before, we analyze this regression by assuming $X_i$ is conditionally random within schools and thus conditionally independent of the potential achievement outcomes $Y_i(d)$:

$$(Y_i(0), Y_i(1), Y_i(2)) \perp X_i \mid W_i,$$

where we now write $Y_i = Y_i(0) + \tau_{11}X_{i1} + \tau_{12}X_{i2}$ to account for the second treatment, with $\tau_{11} = Y_i(1) - Y_i(0)$ and $\tau_{12} = Y_i(2) - Y_i(0)$ denoting the potentially heterogeneous effects of a class size reduction and introduction of a teaching aide, respectively.

Like before, we can use the Frisch-Waugh-Lovell theorem to analyze the coefficient on $X_{i1}$:

$$
\beta_1 = \frac{E[X_{i1}^2 Y_i]}{E[X_{i1}^2]} = \frac{E[X_{i1}(Y_i(0) + \tau_{11}X_{i1} + \tau_{12}X_{i2})]}{E[X_{i1}^2]},
$$

(8)
where $\tilde{X}_{i1}$ again denotes a population residual, but now from regressing $X_{i1}$ on $W_i$, a constant, and $X_{i2}$. Unlike before, this auxiliary regression does not identify the conditional expectation $E[X_{i1} | W_i, X_{i2}]$ because $X_{i1}$ and $X_{i2}$ are dependent (in fact, mutually exclusive). When $X_{i2} = 1$, for example, $X_{i1}$ must be zero, so the conditional expectation is generally non-linear in $W_i$ and $X_{i2}$ and not identified by the linear regression on $W_i$ and $X_{i2}$.

Because $\tilde{X}_{i1}$ does not coincide with a conditionally de-meaned $X_{i1}$, we can not generally reduce eq. (8) to an expression involving only the effects of the first treatment arm, $\tau_{i1}$. It turns out that we nevertheless still have $E[\tilde{X}_{i1}Y_i(0)] = 0$, as in eq. (5), as the auxiliary regression residuals are still uncorrelated with any pre-randomization characteristic like $Y_i(0)$. \footnote{To see this, note that in the auxiliary regression $X_{i1} = \mu_0 + \mu_1 X_{i2} + \mu_2 W_i + \tilde{X}_{i1}$ we can partial out $W_i$ and the constant from both sides to write $\tilde{X}_{i1} = \mu_1 X_{i2} + \tilde{X}_{i1}$. Thus $\tilde{X}_{i1} = \tilde{X}_{i1} - \mu_1 X_{i2}$ is a linear combination of residuals which, per eq. (5), are both uncorrelated with $Y_i(0)$. It follows that $E[\tilde{X}_{i1}Y_i(0)] = 0$.} In this sense, the regression does not suffer from omitted variables bias. However, we do not generally have $E[\tilde{X}_{i1}\tau_{i2}X_{i2}] = 0$, and the constant from both sides to write $\tilde{X}_{i1} = \mu_1 X_{i2} + \tilde{X}_{i1}$. Thus $\tilde{X}_{i1} = \tilde{X}_{i1} - \mu_1 X_{i2}$ is a linear combination of residuals which, per eq. (5), are both uncorrelated with $Y_i(0)$. It follows that $E[\tilde{X}_{i1}Y_i(0)] = 0$.\footnote{To see this, note that in the auxiliary regression $X_{i1} = \mu_0 + \mu_1 X_{i2} + \mu_2 W_i + \tilde{X}_{i1}$ we can partial out $W_i$ and the constant from both sides to write $\tilde{X}_{i1} = \mu_1 X_{i2} + \tilde{X}_{i1}$. Thus $\tilde{X}_{i1} = \tilde{X}_{i1} - \mu_1 X_{i2}$ is a linear combination of residuals which, per eq. (5), are both uncorrelated with $Y_i(0)$. It follows that $E[\tilde{X}_{i1}Y_i(0)] = 0$.}

\begin{equation}
\beta_1 = \frac{E[\lambda_1(W_i)\tau_{i1}(W_i)]}{E[\lambda_1(W_i)]} + \frac{E[\lambda_2(W_i)\tau_{i2}(W_i)]}{E[\lambda_1(W_i)]}
\end{equation}

as a generalization of the previous eq. (6). Here $\lambda_1(W_i) = E[\tilde{X}_{i1}X_{i1} | W_i]$ can be shown to be non-negative, similar to the var$(D_i | W_i)$ term in the previous expression for $\beta$. If not for the second term in eq. (9), $\beta_1$ would thus also identify a convex average of the conditional ATEs $\tau_{i1}(W_i) = E[Y_i(1) - Y_i(0) | W_i]$. But precisely because $\tilde{X}_{i1}$ does not equal $X_{i1} - E[X_{i1} | W_i, X_{i2}]$, this second term is generally present: i.e. $\lambda_2(W_i) = E[\tilde{X}_{i1}X_{i2} | W_i]$ is generally non-zero, complicating the interpretation of $\beta_1$ by including the conditional effects of the other treatment, $\tau_{i2}(W_i) = E[Y_i(2) - Y_i(0) | W_i]$.

Intuitively, the second bias term in eq. (9) arises because the residualized small class treatment $\tilde{X}_{i1}$ is not conditionally independent of the second full-time aide treatment $X_{i2}$ within schools, despite being uncorrelated with $X_{i2}$ by construction. This can be seen by viewing $\tilde{X}_{i1}$ as the product of an equivalent two-step residualization. First, both $X_{i1}$ and $X_{i2}$ are de-meaned by their respective within-school propensity: $\tilde{X}_{i1} = X_{i1} - E[X_{i1} | W_i] = X_{i1} - p_1(W_i)$ and $\tilde{X}_{i2} = X_{i2} - E[X_{i2} | W_i] = X_{i2} - p_2(W_i)$ where $p_j(W_i) = E[X_{i2} | W_i]$ gives the propensity score for treatment $j$. Second, a bivariate regression of $\tilde{X}_{i1}$ on $\tilde{X}_{i2}$ is used to generate the residuals of interest $\tilde{X}_{i1}$. When the propensity scores vary across the schools (i.e. $p_j(0) \neq p_j(1)$), the line of best fit between $\tilde{X}_{i1}$ and $\tilde{X}_{i2}$ averages across this relationship and not does isolate the conditional (within-school) variation in $X_{i1}$. Consequently, the remaining variation in $\tilde{X}_{i1}$ will tend to predict $X_{i2}$ within schools, causing $\lambda_2(W_i)$ to be non-zero.

A simple numerical example helps make this problem concrete. Suppose the first school
(indicated by \(W_i = 0\)) assigned only 5 percent of the students to the small classroom treatment, with 45 percent of the students assigned to a classroom with a full-time aide and the rest assigned to the control group. In the second school (indicated by \(W_i = 1\)), there was a substantially larger push for students to be placed into treatment groups, such that 45 percent of students were assigned to a small classroom, 45 percent were assigned to a classroom with a full-time aide, and only 10 percent were assigned to the control group. Thus, \(p_1(0) = 0.05\), \(p_2(0) = 0.45\), and \(p_1(1) = p_2(1) = 0.45\). Suppose that the schools have the same number of students, such that \(\Pr(W_i = 1) = 0.5\). It then follows from the above formulas that \(\lambda_2(0) = 0.09\) and \(\lambda_2(1) = -0.09\), while \(E[\lambda_1(W_i)] \approx 0.096\).

The previous intuition makes clear why \(\lambda_2(w) \neq 0\) in this example. Consider the treatments after they are demeaned for their respective strata: \(\tilde{X}_{i1}\) and \(\tilde{X}_{i12}\). Within each strata, the correlation between the two treatments is very different: in the first school, there is almost no correlation, while in the second school there is a much higher negative correlation. The overall regression of \(\tilde{X}_{i1}\) on \(\tilde{X}_{i2}\) will average over these two correlations, leading to a misspecified residual \(\tilde{X}_{i1}\) that will tend to be correlated with \(X_{i2}\) within schools. We illustrate this averaging in Figure 1, by plotting the different potential pairs of the two demeaned treatments \((\tilde{X}_{i1}, \tilde{X}_{i2})\), with the two school strata in different colors and shapes. The figure shows how within the first school the value of the demeaned class aide treatment is only weakly predictive of the small classroom treatment, while it is highly predictive in the second school. The overall regression line in black averages over these different relationships, leading to residuals that are generally correlated with the value of the other class aide treatment.

To illustrate the potential magnitude of bias in this example, suppose that classroom reductions have no effect on student achievement (such that \(\tau_1(W_i) = 0\)) but that the effect of a teaching aide varies across schools (such that \(\tau_2(0) \neq \tau_2(1)\)). In the first school the aide is highly effective, \(\tau_2(0) = 1\), but in the second school, the aide has no effect: \(\tau_2(1) = 0\). Equation (9) then shows that the regression coefficient on the first treatment identifies

\[
\beta_1 = \frac{E[\lambda_1(W_i) \cdot 0]}{E[\lambda_1(W_i)]} + \frac{E[\lambda_2(W_i) \cdot (0 + W_i)]}{E[\lambda_1(W_i)]} \\
\approx 0 + \frac{(-0.09 \times 0 \times 1/2) + (0.09 \times 1 \times 1/2)}{0.096} \approx 0.47.
\]

Thus, in this example, a researcher would conclude small classrooms have a sizeable positive effect on student achievement (equal to around half of the true teaching aide effect in school 0), despite the true small-classroom effect being zero for all students. The positive treatment coefficient is due to the regression incorrectly attributing part of the positive teaching aide effect in the first school to the small classroom intervention, and can be made arbitrarily large or small depending on the heterogeneity in these effects across schools.
To build further intuition for the second bias term in eq. (9), it is useful to consider two cases where the bias term is zero. First, suppose the average effects of the teaching aide treatment are constant across the two schools: $\tau_2(0) = \tau_2(1) \equiv \tau_2$. Since regression residuals are by construction uncorrelated with the included regressors, $E[\tilde{X}_{i1} X_{i2}] = E[\lambda_2(W_i)] = 0$. Thus, the weights on the second treatment effects average to zero (as illustrated in the above numerical example), eliminating bias under constant effects: $E[\lambda_2(W_i) \tau_2(W_i)] = E[\tilde{X}_{i1} X_{i2}] \tau_2 = 0$. More generally, the bias given by the second term of eq. (9) will be small when the variation in average teacher’s aide treatment effects across schools $\tau_2(W_i)$ is small or only weakly correlated with the variation in the $\lambda_2(W_i)$ weights across schools.

Second, consider the case where $X_{i1}$ and $X_{i2}$ are conditionally uncorrelated conditional on $W_i$, such as when the small classroom and teacher aid interventions are independently assigned within schools (violating the previously assumed mutual exclusivity of these treatments). In this case the conditional expectation $E[X_{i1} \mid W_i, X_{i2}] = E[X_{i1} \mid W_i]$ will be linear (since $X_{i1}$ and $X_{i2}$ are unrelated given $W_i$), and will thus be identified by the auxiliary regression of $X_{i1}$ on $W_i$ and $X_{i2}$ (and a constant). Consequently, the $\tilde{X}_{i1}$ residuals will again coincide with $X_{i1} - E[X_{i1} \mid W_i]$. The coefficient on $X_{i1}$ in eq. (7) can therefore be shown to be equivalent to the previous eq. (2), identifying the same convex average of $\tau_1(w)$. This case highlights the
importance of dependency across treatments for bias to arise.

Before proceeding to a general characterization of this bias, we note that the above intuition about the non-linear conditional expectation \( E[X_{i1} \mid W_i, X_{i2}] \) also suggests a simple solution to the problem. By including interactions of \( W_i \) and \( X_{i2} \) in eq. (7), the auxiliary regression of \( X_{i1} \) on the other regressors will be saturated and thus capture the inherent nonlinearity in \( E[X_{i1} \mid W_i, X_{i2}] \). We show below how such interacted regressions can solve the bias problem, though care must be taken in how the interactions are constructed. In particular, we show how a particular interacted regression specification gives an efficient estimator of (unweighted) ATEs that is immune to the bias of the simpler specification. We then propose a new class of estimators which—as with the Angrist (1998) result for single treatments—identify a convex average of conditional ATEs. We show how these estimators may yield more precise estimates (while still being free from bias), and may also be more practical in some scenarios.

3 General problem

We now derive a general statement of the bias problem, in regressions of an outcome \( Y_i \) on a \( K \)-dimensional treatment vector \( X_i \) and controls. We focus on the case where \( X_i \) is a vector of mutually exclusive treatment indicators \( X_{ik} = 1 \{ D_i = k \} \) for different values of an underlying treatment \( D_i \in \{0, \ldots, K\} \) (with the \( 1 \{ D_i = 0 \} \) indicator omitted). This case covers the motivating RCT and VAM settings as well as the previous numerical example. In Section A.1, we derive a more general result, allowing \( X_i \) to correspond to combinations of non-mutually exclusive treatments (either discrete or continuous).

We consider estimation of the effects of \( X_i \) on \( Y_i \) by a partially linear model,

\[
Y_i = X_i' \beta + g(W_i) + U_i, \tag{10}
\]

where \( \beta \) and \( g(\cdot) \) are defined as the minimizers of expected squared residuals \( E[U_i^2] \):

\[
(\beta, g(\cdot)) = \arg\min_{\beta \in \mathbb{R}^K, \tilde{g}(\cdot) \in \mathcal{G}} E[(Y_i - X_i' \beta - \tilde{g}(W_i))^2] \tag{11}
\]

for some set of functions \( \mathcal{G} \). This setup nests linear covariate adjustment as the special case of \( \mathcal{G} = \{ \delta_0 + \delta' \delta : [\delta_0, \delta']' \in \mathbb{R}^{1+\dim(W_i)} \} \), in which case eq. (10) gives a linear regression of \( Y_i \) on \( X_i, W_i \), and a constant as in the motivating examples. The setup also allows for more flexible covariate adjustments, such as by specifying \( \mathcal{G} \) to be a large class of “nonparametric” functions as in Robinson (1988). In either case, we assume that \( \mathcal{G} \) is a rich enough set of
functions to satisfy
\[ p_k(w) \equiv E[X_{ik} \mid W_i = w] \in G. \]
and is closed under linear combinations. Let \( p(w) \) be the vector of (generalized) propensity scores (Imbens, 2000), with elements \( p_k(w) \). Without this condition, the specification (10) will not fully account for potential confounding due to \( W_i \), such that \( \beta \) may exhibit omitted variables bias (a fundamentally distinct problem than the kind of bias we are interested in here). With a linear specification for \( g(\cdot) \), eq. (12) requires the propensity scores to be linear in \( W_i \) (cf. eq. (30) in Angrist & Krueger, 1999). This would hold, for example, when \( W_i \) is a vector of group indicators (e.g. treatment randomization strata).

To characterize the \( \beta \) coefficient, we first reparameterize the problem in eq. (11). Let \( \tilde{X}_i = X_i - p(W_i) \) be the conditionally de-meaned treatment vector, let \( h(W_i) = p(W_i)\beta + g(W_i) \), and note that
\[ E[(Y_i - \tilde{X}_i'\beta - g(W_i))^2] = E[(Y_i - \tilde{X}_i'\beta - h(W_i))^2] = E[(Y_i - \tilde{X}_i'\beta)^2] + E[(Y_i - h(W_i))^2] - E[Y_i^2]. \]
Since \( g(\cdot) \in G \) implies \( h(\cdot) \in G \) when eq. (12) holds, this shows that solving eq. (11) is equivalent to minimizing \( E[(Y_i - \tilde{X}_i'\beta)^2] + E[(Y_i - \tilde{h}(W_i))^2] \) over \( \tilde{\beta} \in \mathbb{R}^K \) and \( \tilde{h}(\cdot) \in G \). Thus,
\[ \beta = E[\tilde{X}_i\tilde{X}_i'^{-1}]^{-1}E[\tilde{X}_iY_i], \]
and \( g(W_i) = \text{argmin}_{\tilde{h}(\cdot) \in G} E[(Y_i - \tilde{h}(W_i))^2] - p(W_i)'\beta \). When \( G \) is sufficiently flexible, such that it contains \( E[Y_i \mid W_i = w] \), it also follows that \( g(W_i) = E[Y_i \mid W_i] - p(W_i)'\beta \).

### 3.1 Causal interpretation

We now consider the interpretation of \( \beta \) in terms of causal effects. Let \( Y_i(k) \) denote the potential outcomes under different values of the treatment \( D_i \), with observed outcomes given by \( Y_i = Y_i(D_i) \). We assume that treatment is as good as randomly assigned given the covariates \( W_i \),
\[ (Y_i(0), \ldots, Y_i(K)) \perp X_i \mid W_i. \]
In the more general characterization given in Section A.1 we relax this assumption to mean-independence of potential outcomes and treatment conditional on the controls. Here we write the observed outcome as \( Y_i = Y_i(0) + X_i'\tau_i \), where \( \tau_i \) now gives a vector of treatment effects with elements \( \tau_{ik} = Y_i(k) - Y_i(0) \). Let \( \tau(w) \) denote a vector of conditional ATEs, with elements \( \tau_k(w) = E[\tau_{ik} \mid W_i = w] \), and let \( \tau = E[\tau(W_i)] \) give the vector of (unconditional) ATEs.

When \( \tau(w) = \tau \), such that average causal effects don’t vary across the controls, it follows from eq. (13) and the law of iterated expectations that
\[ \beta = E[\tilde{X}_i\tilde{X}_i'^{-1}]^{-1}E[\tilde{X}_iE[Y_i \mid X_i, W_i]] = E[\tilde{X}_i\tilde{X}_i'^{-1}]^{-1}E[\tilde{X}_i\tilde{X}_i'^{-1}] = \tau, \]
where we also use the facts that \( E[\tilde{X}_i E[Y_i(0) \mid W_i]] = 0 \) and \( E[\tilde{X}_i X'_i] = E[\tilde{X}_i \tilde{X}'_i] \) by the definition of \( \tilde{X}_i = X_i - E[X_i \mid W_i] \). Thus, \( \beta \) identifies constant treatment effects \( \tau \) so long as the propensity scores are spanned by the controls (i.e. eq. (12) holds), even when the outcome model is misspecified in the sense of \( E[Y_i(0) \mid W_i = W] \not\in \mathcal{G} \). This has been previously observed in, for instance, Robins et al. (1992).

When \( X_i \) is a scalar treatment indicator and the conditional treatment effects are not constant, \( \beta \) identifies a convex weighted average of the conditional treatment effects \( \tau(W_i) \). This follows again by iterated expectations, since with a single treatment

\[
\beta = \frac{E[\tilde{X}_i X_i \tau(W_i)]}{E[X^2_i]} = \frac{E[\text{var}(X_i \mid W_i) \tau(W_i)]}{E[\text{var}(X_i \mid W_i)]},
\]

(14)

This generalizes the Angrist (1998) result to a general control specification. Versions of this extension appear in, for instance, Angrist and Krueger (1999) and Angrist and Pischke (2009, Chapter 3.3). The result provides a rationale for estimating the effect of a scalar treatment as a convex average of heterogeneous treatment effects. Moreover, as we will show in Section 4, under some conditions the weights will ensure the sample analog of \( \beta \) achieves a semi-parametric efficiency bound (conditional on the controls) within a particular class of weighted-average treatment effect estimators. This result makes the partially linear specification (10) especially appealing with a single binary treatment.

The next proposition shows, however, that the interpretation of \( \beta \) as a convex average of heterogeneous effects does not generalize with multiple unordered treatments:

**Proposition 1.** Consider the partially linear model in eq. (10). Suppose that conditions (12) and (13) hold. For \( v(W_i) = \text{var}(X_i \mid W_i) \), let \( v_{-k}(W) \) denote the submatrix with the \( k \)th row and \( k \)th column removed, and let \( v_k(W) \) denote the \( k \)th column. Finally, let \( e_k \) denote the \( k \)th unit vector, and for any \( K \)-dimensional vector a let \( a_{-k} \) denote the subvector with the \( k \)th entry removed. Then

\[
\beta_k = \frac{E[\lambda_{kk}(W_i) \tau_k(W_i)]}{E[\lambda_{kk}(W_i)]} + \sum_{\ell \neq k} \frac{E[\lambda_{k\ell}(W_i) \tau_\ell(W_i)]}{E[\lambda_{kk}(W_i)]},
\]

(15)

where

\[
\lambda_{kk}(W_i) = v(W_i)_{kk} - E[v_k(W_i)(v_k(W_i))^t]^{-1} v_k(W_i)_{-k}
\]

\[
\lambda_{k\ell}(W_i) = v(W_i)_{k\ell} - E[v_k(W_i)(v_k(W_i))^t]^{-1} v_{-k}(W_i)(e_\ell)_{-k}, \quad \ell \neq k.
\]

Furthermore, \( \lambda_{kk}(W_i) \geq 0 \) and \( E[\lambda_{k\ell}(W_i)] = 0 \) for \( \ell \neq k \).
Proposition 1 shows that the coefficient on $X_{ik}$ in eq. (10) is a sum of two terms. The first term is a weighted average of conditional ATEs $\tau_k(W_i)$, with non-negative weights. The second term is a combination of treatment effects of other treatments $\tau_\ell(W_i)$, with weights that average to zero: $E[\lambda_{k\ell}(W_i)] = 0$. Because the weights on $\tau_\ell(W_i)$ are zero on average, unless they are identically zero they must be negative for some values in the support of $W_i$.

Each treatment coefficient $\beta_k$ thus generally does not have a causal interpretation as a convex combination of treatment effects. Two exceptions are when $\lambda_{k\ell}(W_i) = 0$ almost surely for all $\ell \neq k$, and when the conditional effects of these other treatments are homogeneous such that $\tau_\ell(W_i) = \tau_\ell^c$. In the second case $E[\lambda_{k\ell}(W_i)\tau_\ell(W_i)] = \tau_\ell E[\lambda_{k\ell}(W_i)] = 0$, so there is no bias term. To see when the first case holds, note that we can equivalently write $\lambda_{k\ell}(W_i) = E[\tilde{X}_{ik}\tilde{X}_{i\ell} \mid W_i]$, where $\tilde{X}_{ik}$ is the residual from regressing $\tilde{X}_{ik}$ on $\tilde{X}_{i,-k}$. Since $E[\tilde{X}_{ik}\tilde{X}_{i\ell}] = 0$ by this construction, it follows that $\lambda_{k\ell}(W_i) = 0$ if $E[\tilde{X}_{ik} \mid \tilde{X}_{i,-k}, W_i] = E[\tilde{X}_{ik} \mid \tilde{X}_{i,-k}]$; that is, when adding $W_i$ to the auxiliary regression of $\tilde{X}_{ik}$ on $\tilde{X}_{i,-k}$ has no effect. This holds trivially when the treatments are (unconditionally) randomly assigned, with equal propensity scores. It also holds when the conditional expectation of $X_{ik}$ given $X_{i,-k}$ and $W_i$ is partially linear, i.e. $E[X_{ik} \mid X_{i,-k}, W_i] = X_{i,-k}^\alpha + g_k(W)$ for some vector $\alpha$ and $g_k \in G$. However, linearity of $E[X_{ik} \mid X_{i,-k}, W_i]$ in $X_{i,-k}$ is impossible in the case of mutually exclusive treatments, as noted in Section 2. For the general case in Section A.1 linearity may hold, such as when the treatments are mutually independent conditional on $W_i$.

Since the weights in eq. (15) are functions of the conditional treatment variance $v(W_i)$, they are identified and can be used to further characterize each $\beta_k$ coefficient. For example, the bias term can be bounded by the identified $\lambda_{k\ell}(W_i)/E[\lambda_{kk}(W_i)]$ and pre-specified bounds on the heterogeneity in conditional ATEs $\tau_\ell(W_i)$.

### 3.2 Discussion

It is useful to contrast Proposition 1 with other recent characterizations of regression coefficients in terms of heterogeneous causal effects. We first note that the bias term in eq. (15) is conceptually distinct from other biases found in generalized difference-in-difference, or “event study” regressions (Goodman-Bacon, 2018; de Chaisemartin & D’Haultfoeuille, 2020; Sun & Abraham, 2020; Callaway & Sant’Anna, 2020; Borusyak et al., 2021), which estimate the effect of a single binary treatment over time. One difference is that the eq. (15) bias is not a result of a weaker “parallel trends” identifying assumptions used in this literature. Under this assumption, regression may fail to identify convex averages of heterogeneous effects by mixing causal and non-causal comparisons of outcome trends among different groups of treated and untreated observations at different times. Such a problem does not arise under our stronger

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4A third weaker case is when the weights are uncorrelated with the conditional ATEs.
assumption of conditional random treatment assignment (eq. (13)), which makes the treatments fully independent of all functions of potential outcomes (both levels and trends). The bias term in Proposition 1 is more closely related to issues with multiple-treatment panel data (or “mover”) regressions previously studied by Hull (2018b) and de Chaisemartin and D’Haultfoeuille (2021), though it is again derived in a more general setting.

The characterization in Proposition 1 relates to concerns in interpreting multiple-treatment instrumental variables (IV) estimates with heterogeneous treatment effects (Behaghel et al., 2013; Kirkeboen et al., 2016; Hull, 2018c; Kline & Walters, 2019). The IV connection comes from viewing eq. (10) as the second stage of an IV model estimated by a control function approach; in the linear IV case, for example, \( g(W_i) \) can be interpreted as giving the residuals from a first-stage regression of \( X_i \) on a vector of valid instruments \( Z_i \). In the single-treatment case, the resulting \( \beta \) coefficient has an interpretation of a weighted average of conditional local average treatment effects under the appropriate first-stage monotonicity condition (Imbens & Angrist, 1994). But as in Proposition 1 this interpretation fails to generalize when \( X_i \) includes multiple mutually-exclusive treatment indicators: each \( \beta_k \) then combines the local effects of treatment \( k \) with a non-convex average of the effects of other treatments.

Finally, we note that Proposition 1 has implications for the monotonicity condition needed to interpret the single-treatment IV estimand with multiple instruments. The first stage of such IV regressions will tend to have the form of eq. (10), where now \( Y_i \) is interpreted as the treatment and \( X_i \) gives the instrument vector. Proposition 1 then shows that the first-stage coefficients on the instruments \( \beta_k \) will not generally be convex weighted average of the true first-stage effects \( \tau_{ik} \). Because of this non-convexity, the regression specification may fail to satisfy the necessary monotonicity condition even when the true effects are always positive. In other words, the cross-instrument contamination of causal effects may cause monotonicity violations, even when specifications with individual instruments would be appropriate. This adds to growing concerns about monotonicity failures in multiple-instrument designs (Mueller-Smith, 2015; Frandsen et al., 2019; Norris, 2019; Mogstad et al., 2020).

The monotonicity concern may be especially important in “examiner” (or “judge”) designs, which exploit the assignment of observations to multiple decision-makers. Many studies leverage such variation by computing average examiner decision rates, often with a leave-one-out correction, and use this “leniency” measure as a single instrument with linear controls. Such IV estimators can be thought of as implementing versions of a jackknife IV estimator (Angrist et al., 1999), based on first stage that uses examiner indicators as instruments, similar to eq. (10). Proposition 1 thus raises a new concern with these IV analyses when the controls (such as location or time fixed effects) are needed to ensure as-good-as-random assignment.\(^5\)

\(^5\)As we discuss in Section 4, one solution to this problem is to interact the examiner instruments with the controls, which would amount to computing “leniency” separately within (say) location and time groups. This
4 Solutions

We next discuss solutions to the issues that Proposition 1 raises with partially linear regression-based estimates of multiple treatment effects. To motivate these solutions, we first discuss potential estimation approaches in the case of a single conditionally unconfounded binary treatment $D_i$, with potentially non-binary controls $W_i$.

Many approaches are available for efficiently estimating the (unweighted) ATE of a binary treatment. For example, one could compare the mean outcomes of the treated and control group, weighting the observations by the inverse of the estimated propensity score; one could also use regression methods similar to eq. (10) but with interactions between the treatment and controls included (see Imbens & Wooldridge, 2009, for a review). Under strong overlap (i.e. when the propensity score is bounded away from zero or one) the two approaches are asymptotically equivalent and achieve the semiparametric efficiency bound, such that it is impossible to construct other regular estimators with smaller asymptotic variance.

In practice, however, overlap can be limited. Such ATE estimators may then yield large standard errors, or be poorly behaved in small samples. This problem arises because it is hard to estimate the counterfactual outcomes for observations with $p(W_i)$ close to zero or one, and it motivates focusing on other weighted averages of conditional ATEs that downweight these counterfactuals rather than focusing on the unweighted ATE (e.g. Crump et al., 2009). In fact, with a binary treatment, $X_i = D_i$, the partially linear regression in eq. (10) does exactly this: per Angrist (1998), such regressions use weights that are proportional to the conditional variance of treatment $\text{var}(D_i \mid W_i) = p(W_i)(1 - p(W_i))$, which tend to zero as $p(W_i)$ tends to zero or one. Consequently, such regressions may yield small standard errors even in cases when it is not possible to precisely estimate the unweighted ATE. We formalize this virtue of the Angrist (1998) weights below by showing that the partially linear regression estimator achieves a smaller asymptotic variance, conditional on covariates, than any other consistent estimator of some weighted average of conditional ATEs.

The various unconditional ATE estimation approaches readily extend to the multiple treatment case, as we next discuss. However, overlap concerns tend to play a greater role with multiple treatments, because some propensity scores necessarily become closer to zero or one as treatment arms are added. As Proposition 1 shows, the partially linear regression in eq. (10) no longer provides a solution in this case since it no longer estimates a convex average of treatment effects. We therefore propose a new class of estimators in Section 4.2, similar in spirit to Crump et al. (2009), that efficiently estimate a weighted average of multiple treatment effects while avoiding the bias of the partially linear regression.

may greatly increase the effective number of instruments, heightening concerns of many-instrument bias in finite samples as well as the importance of appropriate leave-one-out corrections (e.g., Kolesár, 2013).
4.1 Estimating average treatment effects

A number of existing approaches to estimating the ATE of a binary treatment extend naturally to multiple treatments, including matching, inverse propensity score weighting, regressions with interactions, or doubly robust combinations of these methods (see, among others, Cattaneo, 2010, Graham et al., 2018, and Chernozhukov et al., 2021).

Rather than reviewing these approaches, we briefly outline a simple implementation of one of these methods which follows the intuition given at the end of Section 2. Namely, one may estimate the ATE vector \(\tau\) by expanding the partially linear model in eq. (10) to include treatment interaction terms. This generalizes the implementation in the binary treatment case discussed in Imbens and Wooldridge (2009, Section 5.3). Consider the expanded model

\[
Y_i = X_i'\beta + q_0(W_i) + \sum_{k=1}^K X_{ik}(q_k(W_i) - E[q_k(W_i)]) + U_i, \tag{16}
\]

where \(q_k(\cdot) \in \mathcal{G}, \ k = 0, \ldots, K\) and we continue to define \(\beta\) and these functions as minimizers of \(E[U_i^2]\). When \(\mathcal{G}\) consists of linear functions, eq. (16) specifies a linear regression of \(Y_i\) on \(X_i, W_i\), a constant, and the interactions between each treatment indicator \(X_{ik}\) and the demeaned control vector \(W_i - E[W_i]\). Define \(\mu_k(w) = E[Y_i(k) \mid W_i = w]\) for \(k = 0, \ldots, K\), so that \(\tau_k(w) = \mu_k(w) - \mu_0(w)\). When the unconfoundedness condition (13) holds and \(\mathcal{G}\) is sufficiently rich so that \(\mu_k \in \mathcal{G}\) for \(k = 0, \ldots, K\), then \(\beta = \tau\) and \(q_k(w) = \tau_k(w)\), so that this interacted regression identifies both the ATE vector and the conditional ATEs.

Intuitively, the added interactions in (16) ensure that each treatment coefficients \(\beta_k\) is determined only by the outcomes in treatment arms with \(D_i = 0\) and \(D_i = k\) only, avoiding the other-treatment contamination bias in Proposition 1. Demeaning the \(q_k(W_i)\) in the interactions ensures they are appropriately centered, so one can interpret the coefficients on the uninteracted \(X_{ik}\) as ATEs.

Estimation of eq. (16) is conceptually straightforward by least squares, with sample averages replacing expectations. Furthermore, it can be shown that the resulting estimator achieves the semiparametric efficiency bound under strong overlap. Nonetheless, the estimator may be imprecise under limited overlap. It may moreover be cumbersome to implement with nonlinear \(q_k(\cdot)\) and many treatments \(K\).\(^6\) We thus turn to the problem of estimating weighted averages of conditional ATEs, which are can be more precisely estimated, and may be more practical to implement.

\(^6\)As discussed in Section 3.2, this approach will also address issues with monotonicity concerns in the case of many instruments and two-stage least squares such as “examiner” or “judge” IV designs. But, this may substantially increase the number of instruments as well—heightening concerns of many-instrument bias.
4.2 Efficient weighted averages of treatment effects

Suppose that we wish to estimate a weighted average of conditional potential outcome contrasts:

$$\sum_{k=0}^{K} c_k \mu_k(W_i)$$

where $\mu_k(W_i) = E[Y_i(k) \mid W_i]$ and $c$ is a $(K + 1)$-dimensional vector. For example, to estimate the effect of treatment $k$ relative to the baseline we set $c_k = 1$, $c_0 = -1$ and all other entries of $c$ to zero, so that $\sum_{k=0}^{K} c_k \mu_k(W_i) = \tau_k(W_i)$. We discuss the case where a researcher is equally interested in all possible treatment effects below. We proceed in three steps. First, we establish an efficiency benchmark—a semiparametric efficiency bound—for estimating a given weighted average of such contrasts under idealized conditions. Second, we determine which weighted average is “easiest” to estimate, in the sense of having the smallest efficiency bound. Finally, we give a feasible estimator that achieves this bound.

The following proposition establishes the first step:

**Proposition 2.** Suppose eq. (13) holds in an i.i.d. sample of size $N$, with known propensity scores $p_k(W_i)$. Let $\sigma_k^2(W_i) = \text{var}(Y_i(k) \mid W_i)$. Consider the problem of estimating the weighted average of contrasts

$$\beta_\lambda = \frac{1}{\sum_{i=1}^{N} \lambda(W_i)} \sum_{i=1}^{N} \lambda(W_i) \sum_{k=0}^{K} c_k \mu_k(W_i),$$

where the weighting function $\lambda(\cdot)$ and contrast vector $c$ are both known. Then, conditional on $W_1, \ldots, W_N$, the semiparametric efficiency bound is almost surely given by

$$\mathcal{V}_\lambda(c) = E \left[ \sum_{k=0}^{K} \frac{\lambda(W_i)^2 c_k^2 \sigma_k^2(W_i)}{p_k(W_i)} \right] / E[\lambda(W_i)]^2. \quad (17)$$

The efficiency bound $\mathcal{V}_\lambda(c)$ establishes a lower bound on the asymptotic variance of any regular estimator of $\beta_\lambda$, under the idealized situation of known propensity scores.\(^7\)

To establish the second step, we simply minimize eq. (17) over $\lambda(\cdot)$ subject to the constraint that the weights average to one: $E[\lambda(W_i)] = 1$. Simple algebra shows that the variance-minimizing weighting scheme is given is given by

$$\lambda^*(W_i) = \left( \sum_{k=0}^{K} \frac{c_k^2 \sigma_k^2(W_i)}{p_k(W_i)} \right)^{-1},$$

This is the “easiest” to estimate weighting, in the sense of having the smallest asymptotic

\(^7\)With a binary treatment and $c_1 = -c_0 = 1$, this result gives a semiparametric efficiency bound for estimating the average conditional ATE, $\frac{1}{N} \sum_i E[\tau_1(W_i)]$, namely $E[\sigma_1^2(W_i)/p_1(W_i) + \sigma_0^2(W_i)/p_0(W_i)]$. The efficiency bound in Hahn (1998, Theorem 2) for estimating the unconditional ATE has an additional term, $\text{var}(\tau_1(W_i))$, reflecting the variation due to the controls that we condition on here.
variance:
\[ \nu_{\lambda'}(c) = E \left[ \left( \sum_{k=0}^{K} \frac{c^2_k \sigma^2_k(W_i)}{p_k(W_i)} \right)^{-1} \right]^{-1}, \]

the harmonic mean of the conditional variances \( \sum_{k=0}^{K} \frac{c^2_k \sigma^2_k(W_i)}{p_k(W_i)} \). In contrast, the efficiency bound for the unweighted contrast (i.e., the unconditional ATE) is given by the arithmetic mean \( E \left( \sum_{k=0}^{K} \frac{c^2_k \sigma^2_k(W_i)}{p_k(W_i)} \right) \), which can be considerably bigger when the propensity scores are not bounded far from zero or one.

A special case of Proposition 2 formalizes a justification for using regression to estimate the effects of a single treatment. Suppose we are interested in a weighted average of conditional treatment effects \( \tau_k(W_i) \) for some particular \( k \geq 1 \). That is, \( c_k = 1, c_0 = -1, \) and \( c_j = 0 \) for \( j \not\in \{0, k\} \). Suppose further that the conditional variance is homoskedastic: \( \sigma^2_k(W_i) = \sigma^2 \). It follows that the efficient weights and the resulting minimal asymptotic variance are given by

\[ \lambda^*(w) = \frac{p_0(w)p_k(w)}{p_0(w) + p_k(w)} \left( E \left[ \frac{p_0(W_i)p_k(W_i)}{p_0(W_i) + p_k(W_i)} \right] \right)^{-1}, \quad \nu_{\lambda'}(c) = \sigma^2 E \left[ \frac{p_0(w)p_k(w)}{p_0(w) + p_k(w)} \right], \tag{18} \]

where \( p_0(w) = Pr(D_i = 0 \mid W_i) \). Per eq. (14), this is precisely the weighting of conditional ATEs which the partially linear model \((10)\) identifies when it is fit only on observations with \( D_i \in \{0, k\} \), since the propensity score in the subsample is \( Pr(D_i = k \mid W_i, D_i \in \{0, k\}) = p_k(W_i)/(p_0(W_i) + p_k(W_i)) \), making \( p_0(W_i)p_k(W_i)/(p_0(W_i) + p_k(W_i)) \) the conditional variance of the treatment \( k \). Moreover, with a linear specification for \( g(\cdot) \), it follows by standard arguments that regressing \( Y_i \) onto \( X_{ik} \) and \( W_i \) in the subsample with \( D_i \in \{0, k\} \) achieves the variance bound in eq. (18), provided the subsample propensity score \( p_k(W_i)/(p_0(W_i) + p_k(W_i)) \) is linear in \( W_i \) (such as when \( W_i \) consists of group indicators).

Therefore, if we are interested a single treatment contrast, we can simply run a linear regression with an additive covariate adjustment in the subsample with \( D_i \in \{0, k\} \). Such a regression is convenient and simple to implement, and does not require knowledge of the propensity score nor its explicit estimation. The estimand \( \beta \) is robust in the sense that it is interpretable as a weighted average of conditional treatment effects \( \tau_k(W_i) \), so long as the linearity condition on the propensity score holds. Moreover, the linear regression estimator is locally most efficient: under homoskedasticity, it achieves the lowest possible asymptotic variance among all regular estimators that are consistent for some weighted average of treatment effects, even if these competing estimators use the knowledge of the propensity score.

While the robustness property of such regressions is well-established, by Angrist (1998) and subsequent extensions, to our knowledge the efficiency property is novel. It can be thought to formalize a common motivation for using regression to estimate the effects of a single treatment instead of more involved unconditional ATE estimators: when treatment effect heterogeneity
is minimal or only weakly correlated with the $\lambda^*(W_i)$ weights, the regression’s weighted-average effect will be close to the ATE while being more precisely estimated. Proposition 2 shows that the regression’s precision is optimal, in a precise sense.

We now discuss how the efficiency bound in Proposition 2 can be attained for a full set of contrasts across multiple treatments. Suppose we are interested in reporting $K$ coefficients $\beta_\lambda$ that equally consider all $K(K+1)$ potential contrasts (that is, $\mu_j(W_i) - \mu_k(W_i)$, for all $j \neq k, j, k = 0, \ldots, K$). A natural approach is to choose the weighting $\lambda^*$ that minimizes the average variance, averaged equally across all contrasts:

$$\int V_\lambda(c)dF(c) = \frac{2}{K+1} E \left[ \frac{2}{K} \sum_{k=0}^{K} \sigma_k^2(W_i) \right],$$

where $F(\cdot)$ gives the uniform distribution over the possible (now random) contrasts $c$, so that $c_j = 1$ with probability $1/(K + 1)$ and $-1$ with probability $1/(K + 1)$. This is equivalent to setting $c_k^2 = 2/(K + 1)$, and it follows that under homoskedasticity

$$\lambda^*(W_i) = \frac{\left( \sum_{k=0}^{K} p_k(W_i) \right)^{-1}}{E \left[ \sum_{k=0}^{K} p_k(W_i) \right]^{-1}}, \quad V_\lambda^*(c) = \sigma^2 E \left[ \left( \sum_{k=0}^{K} p_k(W_i)^{-1} \right)^{-1} \right].$$

These weights generalize the intuition behind the single binary treatment, placing higher weight on covariate strata where the treatments are evenly distributed, and putting less weight on the strata where there is an overlap problem. Unlike the previous $\lambda^*(W_i)$ these weights are the same for every treatment contrast $c$, making $\beta_{\lambda^*,k} - \beta_{\lambda^*,\ell}$ a convex weighted average of relative causal effects between every treatment $k$ and $\ell$. An estimator $\hat{\beta}_{\lambda^*}$ that implements these weights can be obtained by estimating $\lambda^*(W_i)$ using estimates of the propensity scores and $\mu_k(W_i) = E[Y_i \mid D_i = k, W_i]$ (which are both straightforward to estimate when, for example, $W_i$ is a vector of group indicators).

5 Conclusion

Regressions with multiple dependent treatments and controls are common across a variety of empirical applications. Prominent examples include experiments with multiple treatment arms and value-added models in education, healthcare, and other settings. This paper shows that such regressions generally fail to estimate a convex weighted average of heterogeneous effects. We provide intuition for why the influential result of Angrist (1998) fails to generalize to multiple dependent treatments. We then highlight potential solutions and propose a new class of estimators that efficiently weights conditional average treatment effects while avoiding
the bias of regression. Future versions of this paper will illustrate the problem and solutions in different empirical examples.

Appendix A  Proofs

A.1 Proof of Proposition 1

We prove a generalization of the main-text proposition which allows for a general vector of (potentially non-binary) treatments $X_i$. We continue to consider the partially linear model, eq. (10), and to assume the set of control functions $G$ is rich enough to satisfy eq. (12). But we relax the baseline assumption of conditional random assignment to a weaker mean-independence condition. Letting $Y_i(x)$ denote potential outcomes when $X_i = x$, we assume:

$$E[Y_i | X_i = x, W_i = w] = E[Y_i(x) | W_i = w]$$

We also assume counterfactual outcomes are linear in $x$, conditional on $W_i$:

$$E[Y_i(x) | W_i = w] = E[Y_i(0) | W_i = w] + x'\tau(w),$$

for some function $\tau(\cdot)$. The latter condition holds trivially in the main-text discussion of mutually exclusive binary treatments. More generally, $\tau_k(w)$ corresponds to the conditional average effect of increasing $X_{ik}$ by one unit among observations with $W_i = w$. We continue to define $\tau = E[\tau(W_i)]$ as the average vector of per-unit effects.

We now prove that under these assumptions $\beta_k$ is given by the expression in eq. (15). We further prove that $E[\lambda_{k\ell}(W_i)] = 0$ for $\ell \neq k$ in general, and that $\lambda_{kk}(W_i) \geq 0$ in the case of mutually exclusive treatment indicators. First note that by iterated expectations,

$$\beta = E[\tilde{X}_i \tilde{X}_i']^{-1} E[\tilde{X}_i E[Y_i(0) | W_i] + X'_i \tau(W_i)] = E[v(W)]^{-1} E[v(W_i)\tau(W_i)].$$

By the block matrix inverse formula,

$$\beta_k = \frac{E[v_k(W_i)k \tau_k(W_i)]}{E[v_k(W_i)k] - E[v_k(W_i)'_k] E[v_k(W_i)]^{-1} E[v_k(W_i) - k]}$$

$$- \frac{E[v_k(W_i)'_k] E[v_k(W_i)]^{-1} E[v_k(W_i) - k] \tau_k(W_i) + v_k(W_i) - k \tau_k(W_i)]}{E[v_k(W_i)k] - E[v_k(W_i)'_k] E[v_k(W_i)]^{-1} E[v_k(W_i) - k]}$$

$$= \frac{E[\lambda_{kk}(W_i) \tau_k(W_i)]}{E[\lambda_{kk}(W_i)]} + \sum_{\ell \neq k} \frac{E[\lambda_{k\ell} \tau(W_i)]}{E[\lambda_{kk}(W_i)]}. $$
To conclude the proof, note first that
\[ E[\lambda_{kk}(W_i)] = E[v(W_i)_{kk}] - E[v_k(W_i)_{-k}](e_{-k})_{-k} = 0. \]

Second, note that when \( X_i \) is a vector of mutually exclusive treatment indicators,
\[
\lambda_{kk}(W_i) = p_k(W_i)(1 - p_k(W_i)) - E[p_k(W_i)p_{-k}(W_i)']E[v_{-k}(W_i)]^{-1}p_{-k}(W_i)p_k(W_i)
\]
\[
= p_k(W_i)p_0(W_i) + E[p_0(W_i)p_{-k}(W_i)']E[v_{-k}(W_i)]^{-1}p_{-k}(W_i)p_k(W_i)
\]
for \( p_0(W_i) = 1 - \sum_{k=1}^K p_k(W_i) \). The second line uses \( v_{-k}(W_i) = p_{-k}(W_i) - p_{-k}(W_i)p_{-k}(W_i)' = (p_k(W_i) + p_0(W_i))p_{-k}(W_i) \), with \( i \) denoting a \((K-1)\)-dimensional vector of ones. Note that the eigenvalues of \( E[v_{-k}(W_i)] \) are positive because it is a covariance matrix. Furthermore, since the off-diagonal elements of \( E[v(W_i)] \) are negative, the off-diagonal elements of \( E[v_{-k}(W_i)] \) are also negative. It therefore follows that \( E[v_{-k}(W_i)] \) is an \( M \)-matrix (Berman & Plemmons, 1994, property \( D_{16} \), p. 135). Hence, all elements of \( E[v_{-k}(W_i)]^{-1} \) are positive (Berman & Plemmons, 1994, property \( N_{38} \), p. 137). Thus, both summands in the above expression for \( \lambda_{kk}(W_i) \) are positive, so that \( \lambda_{kk}(W_i) \geq 0 \) in the main-text case.

### A.2 Proof of Proposition 2

The parameter of interest \( \beta_\lambda \) depends on the realizations of the covariates. We therefore derive the semiparametric efficiency bound conditional on the covariates; i.e. that eq. (17) is almost surely the variance bound for estimators that are regular conditional on the covariates. Relative to the related results in Hahn (1998) and Hirano et al. (2003), we need account for the fact that once we condition on the covariates, the data are no longer i.i.d.

To that end, we use the notion of semiparametric efficiency based on the convolution theorem of van der Vaart and Wellner (1989, Theorem 2.1) (see also van der Vaart & Wellner, 1996, Chapter 3.11). Let us briefly recall the result for convenience. Consider a model \( \mathcal{P} = \{P_{n,\theta}: \theta \in \Theta\} \) parametrized by (a possibly infinite-dimensional) parameter \( \theta \). Let \( \hat{\mathcal{P}} \) denote a tangent space, a linear subspace of a Hilbert space with an inner product \( \langle \cdot, \cdot \rangle \). We say that the model is locally asymptotically normal (LAN) at \( \theta \) relative to a tangent space \( \hat{\mathcal{P}} \) if for each \( g \in \hat{\mathcal{P}} \), there exists a sequence \( \theta_n(g) \) such that the likelihood ratios are asymptotically quadratic, \( dP_{n,\theta_n(g)}/dP_{n,\theta} = \Delta_{n,\theta} - \langle g, g \rangle / 2 + o_{P_{n,\theta}}(1) \), where \( \Delta_{n,\theta} \) converges under \( P_{n,\theta} \) to a Gaussian process with covariance kernel \( \langle g_1, g_2 \rangle \). We say that \( \beta_n(P_n) \) is differentiable if for each \( g, \sqrt{n}(\beta_n(P_{n,\theta_n(g)}) - \beta_n(P_{n,\theta})) \rightarrow \langle \psi, g \rangle \) for some \( \psi \) that lies in the completion of \( \hat{\mathcal{P}} \). Then the semiparametric efficiency bound is given by \( \langle \psi, \psi \rangle \) (in the sense that the asymptotic distribution of any regular estimator is given by the convolution of a \( Z \sim \mathcal{N}(0, \langle \psi, \psi \rangle) \) and some other random variable \( U \), with \( Z \) and \( U \) independent).
To apply this result in our setting, we proceed in three steps. First, we define the tangent space. Next, we verify that the model is LAN. Finally, we verify differentiability and find the efficient influence function \( \psi \).

**Step 1** By the conditional independence assumption, we can write the density of the vector \((Y_{0i}, \ldots, Y_{Ki}, D_i)\) (with respect to some \(\sigma\)-finite measure) conditional on the covariates as

\[
f(y_0, \ldots, y_K, d \mid W_i) \cdot \prod_{k=0}^{K} p_k(W_i)^{1 \{d=k\}},
\]

where \(f\) denotes the conditional density of the potential outcomes, conditional on the covariates. Since the propensity scores are known, the model is parametrized by \(\theta = f\). The density of the observed data \((Y_i, D_i)\) is given by

\[
\prod_{k=0}^{K} (f_k(y \mid W_i)p_k(W_i))^{1 \{d=k\}},
\]

where \(f_k(y \mid W_i) = \int f(y_k, y_{-k} \mid W_i) dy_{-k}\). Consider one-dimensional submodels of the form \(f_k(y \mid W_i; t) = f_k(y \mid W_i)(1 + ts_k(y \mid W_i))\), where \(s_k(\cdot)\) is bounded and satisfies \(E[s_k(Y_i(k) \mid W_i) \mid W_i] = 0\) almost surely for some constant \(K\) (by boundedness of \(g_k(\cdot)\), \(f_k(Y_i \mid W_i; t) \geq 0\) and hence there is a well-defined density for \(t\) small enough). The joint log-likelihood is given by

\[
\sum_{i=1}^{N} \sum_{k=0}^{K} \mathbb{1}\{D_i = k\} (\log f_k(Y_i \mid W_i; t) + \log p_k(W_i)).
\]

The score at \(t = 0\) is given by \(\sum_{i=1}^{N} s_{Ni}\), where \(s_{Ni} = s(Y_i, D_i \mid W_i)\), with \(s(Y_i, D_i \mid W_i) = \sum_{k=0}^{K} \mathbb{1}\{D_i = k\} s_k(Y_i \mid W_i)\). This suggests defining the tangent space to consist of all functions \(s(y, d \mid w) = \sum_{k=0}^{K} \mathbb{1}\{d = k\} s_k(y \mid W_i = w)\), such that \(s_k\) satisfies the conditions above. Let \(\langle s_1, s_2 \rangle = E[s_1(Y_i, D_i \mid W_i), s_2(Y_i, D_i \mid W_i)]\).

**Step 2** We verify that the conditions of Theorem 3.1 in McNeney and Wellner (2000) hold almost surely once we condition on the covariates, with \(\theta_N(s) = f(\cdot \mid \cdot \cdot 1/\sqrt{N})\). Let \(\alpha_{Ni} = \prod_{k=0}^{K} (f_k(Y_i \mid W_i; 1/\sqrt{N})/f_k(y_i \mid W_i))^{1\{D_i=k\}} = \prod_{k=0}^{K} (1 + s_k(Y_i \mid W_i)/\sqrt{N})^{1\{D_i=k\}}\) denote the likelihood ratio. Since this is bounded by the boundedness of \(s_k(\cdot)\), condition (3.7) holds. Also, since \((1+ts_k)^{1/2}\) is continuously differentiable for \(t\) small enough, with derivative \(s_k/2\sqrt{1+ts_k}\), it follows from Lemma 7.6 in van der Vaart (1998) that \(N^{-1} \sum_{i=1}^{N} E[\sqrt{N}(\alpha_{Ni}^{1/2} - s_{Ni}) - 2 | W_i = w]^2 \to 0\), so that condition (3.8) holds. Since \(s_k(\cdot)\) is bounded, condition (3.9) also holds. Also, \(\frac{1}{\sqrt{N}} \sum_{i=1}^{N} E[s_{Ni}^2 | W_i] = \frac{1}{\sqrt{N}} \sum_{i=1}^{N} E[s(Y_i, D_i \mid W_i)^2 | W_i]\), which converges to \(E[s(Y, D_i \mid W_i)^2]\) almost surely by the law of large numbers. Since \(s_{Ni}\) are independent across \(i\), this implies conditions (3.10) and (3.11). Since the scores \(\Delta_{N,i} = \frac{1}{\sqrt{N}} \sum_{i=1}^{N} (s_{Ni} - E[s_{Ni} | W_i])\) are exactly linear in \(s\), condition (3.12) also holds. It follows that the model is LAN almost surely.
Step 3  The parameter of interest $\beta_\lambda$ may be written as $\beta_n(f) = \sum_i \int \lambda(W_i) y \sum_k f_k(y \mid W_i) dy / \sum_i \lambda(W_i)$. Hence,

$$
\sqrt{N}(\beta_N(f(\cdot \mid \cdot ; 1/\sqrt{N})) - \beta_N(f)) = (N^{-1} \sum_i \lambda(W_i))^{-1} \sum_i \int \left[ \lambda(W_i) y \sum_k c_k(f_k(y \mid W_i; 1/\sqrt{N}) - f_k(y \mid W_i)) dy \right]
$$

which converges to $E[\lambda(W_i) \sum_k c_k s_k(Y_i(k) \mid W_i)] / E[\lambda(W_i)]$ almost surely by the law of large numbers. We can write this as $E[\psi g]$, where

$$
\psi(Y_i, D_i, W_i) = \sum_k \mathbb{1}\{D_i = k\} \lambda(W_i) c_k \frac{(Y_i - \mu_k(W_i))}{p_k(W_i) E[\lambda(W_i)]}.
$$

Observe that $\psi$ is in the model tangent space, with the summands playing the role of $s_k(y \mid w)$ (more precisely, since the outcomes are unbounded, it lies in the completion of the tangent space). Hence, the semiparametric efficiency bound is given by $E[\psi^2]$.

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