Using secondary outcomes and covariates to sharpen inference in instrumental variable settings

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Abstract: We derive nonparametric bounds for intention-to-treat effects on a primary outcome for the subpopulations defined by the values of the potential treatment status under each value of the instrument, without requiring the exclusion restriction assumption of the instrument. We exploit restrictions on the joint distribution of the primary outcome and auxiliary variables - secondary outcomes and covariates - implied by the randomization of the instrument. We show how the identifying power of the bounds provides useful tools to detect violations of the exclusion restriction. We also argue that the bounds should be used as a preliminary analysis, before the application of instrumental variable methods, to assess the plausibility of the exclusion restriction assumption and to detect the magnitude of its violation. We also show how the setup we consider offers new identifying assumptions of intention-to-treat effects without the exclusion restriction. The identifying power of the bounds is illustrated in three real data examples of a social job training experiment and two medical randomized encouragement studies.
Keywords: Nonparametric bounds, ITT effects, noncompliance, instrumental variables, multiple outcomes, covariates, violation of exclusion restrictions.

1 Introduction

Instrumental variable (IV) methods are widely used to infer the causal effect of a treatment on an outcome. Randomized experiments with noncompliance are closely related to the econometric IV setting, where treatment assignment plays the parallel role of an instrument. The setup we consider is one with heterogenous effects, as developed in Imbens and Angrist (1994) and Angrist et al. (1996). The approach to adjust for noncompliance applied in Angrist et al. (1996) can also be viewed as a special case of *principal stratification* (Frangakis and Rubin, 2002), where the latent subpopulations of compliers, never-takers, always-takers and defiers are the principal strata with respect to the post-assignment compliance behavior.

Given the setup, it can be shown that causal effects of assignment among compliers (subjects who would take the treatment if offered to take it and would not take it if not offered) are identifiable under the assumptions that a) treatment assignment (i.e., the instrument) is randomized, b) the assignment has a non null effect on treatment receipt (relevance of the instrument) c) there are no subjects who would take the treatment if randomized not to take it, but would not take it if assigned to take it (no-defier or monotonicity assumption), and d) under the crucial exclusion restriction assumption of a null effect of assignment on the outcome for those whose treatment status is not affected by assignment, i.e., the noncompliers, usually distinguished in never-takers and always-takers. These assumptions, which essentially define the validity of an instrument, allow one to uniquely disentangle the observed distribution of the outcome, which is a mixture of distributions associated with the latent groups of compliers, never-takers, and always-takers (Imbens and Rubin, 1997a). The effect on compliers is usually interpreted as the causal effect of receipt of the treatment, under the additional assumption that the effect of assignment for compliers is solely due to the actual treatment receipt. With a valid instrument, sharp bounds on the average treatment effect have also been derived (e.g., Manski, 1990, 1994; Balke and Pearl, 1997).

Depending on the empirical setting, some of these substantive assumptions may be questionable. Here we focus on the violation of the exclusion restriction (ER). Typically, ERs appear plausible in blind or double-blind placebo-controlled experiments, but may be questionable in open-label experiments, in randomized encouragement studies (Hirano et al., 2000), and in observational studies with instrumental variables. Open-label experiments are the norm in the social sciences, where subjects, as well as experimenters, cannot be blinded the treatment received because they actively participate to the treatment, e.g., training program (Zhang et al. 2009), they are offered (e.g., Duflo et al., 2008), and in general assignment can affect the outcome through channels other than the treatment (e.g., Mattei and Mealli, 2007; Chetty et al., 2010). The exclusion restriction of an instrument on certain outcomes is also usually subject to large debates in observational studies (e.g., Angrist, 1990; Angrist and Kruger, 1991; Hoogerheide et al., 2007).

IV literature with violation of ERs has followed different directions. One strand of the literature develops sensitivity analysis of IV estimates in linear models under local violations of the ER, using prior information on a parameter summarizing the extent of the violation (see Conley et al., 2008a,b, and, similarly, Nevo and Rosen, 2008 and Kraay, 2010). In these papers, and in the structural equation literature in general, there is usually no distinction between the effect for the entire population and the effect for compliers, and IV estimates are interpreted as estimates of the effect of the treatment, under the assumption that the effect is constant. Related to this literature is also Small (2007), who provides a sensitivity analysis for structural slope coefficients using overidentifying restrictions.

Alternatively, Manski and Pepper (2000) study partial identification of the average treatment effect, when the usual ER does not hold and it is replaced by a weaker monotone instrumental variable assumption. A similar approach is followed by Flores and Flores-Lagunes (2010), who derive bounds on the local average treatment effect (LATE), i.e., the effect of the treatment for compliers, without assuming the ER, but investigating different sets of assumptions imposing weak-inequality restrictions on the mean potential outcomes.

We take an approach that is closer in spirit to Hirano et al. (2000), in the sense that we focus on partial identification of intention-to-treat (ITT) effects, i.e., of the effects of the instrument/assignment on the subpopulations of compliers, never-takers and always-takers. Hirano et al. (2000) conduct a Bayesian analysis and assess sensitivity to various violations of the ER, by estimating ITT effects for the subpopulations of compliers, never-takers and always-takers in a randomized encouragement study concerning the effects of inoculation for influenza. They found that positive estimates of the overall ITT effect need not be due to the treatment itself, but rather to the encouragement to take the treatment (i.e., they found a *direct* effect of the instrument on the outcome *not through* the treatment).

Unlike Hirano et al. (2000), but also unlike Manski and Pepper (2000) and Flores and Flores-Lagunes (2010), we do not use any additional assumption, neither in the form of prior information and distributional assumptions, nor in the form of alternative weak monotonicity assumptions, but rather use the additional information provided by the joint distribution of the outcome of interest with secondary outcomes and covariates. Sharp bounds on ITT effects are derived, and specifically on the distribution of potential outcomes by compliance type and instrument values; ITT effects are in fact defined as general contrasts of features of the distribution of potential outcomes under different values of the instrument.

When the ER is violated, and the analysis is not augmented with additional assumptions, inference on ITT effects for principal strata can be imprecise even in large samples (Imbens and Rubin, 1997b). In the literature, bounds on these effects have not been explicitly considered, but can be easily derived borrowing bounds derived for so-called principal strata *direct* and *partially mediated* effects (Zhang and Rubin, 2003; Imai, 2008; Lee, 2009; Mattei and Mealli, 2011)[1].

We will show how to sharpen bounds on ITT effects using covariates and secondary outcomes for which the ER is more plausible. Specifically, we exploit restrictions on the joint distribution of those variables and the primary outcome implied by the randomization of the instrument and by the ER on the secondary outcome.

Our use of secondary outcomes and covariates is novel. In fact, on one hand, in the presence of multiple outcomes, analysis is usually conducted separately for one outcome at a time, and the joint analysis of two (or more) outcomes is not pursued, unless analyzing their association is the goal. Issues of adjustments for multiple comparisons are also raised when analyzing multiple outcomes.

However, the exclusion restriction assumption may sometimes be more plausible for secondary outcomes (rather than for primary outcomes) for which the study was not specifically designed, and we will show how this information helps sharpening inference on the primary outcome. Covariates, on the other hand, are usually conditioned on in order to improve the precision of causal estimates, by improving the prediction of the compliance status and the missing potential outcomes (Hirano et al., 2000). They are also used to make identifying assumptions more plausible if stated conditional on them (Manski, 1990; Abadie, 2003; Frolich, 2006; Hong and Nekipelov, 2007). We, instead, do not condition on their values, but rather use the restrictions on their joint distribution with the primary outcome provided by the randomization of the instrument. Randomization of the instrument

[1]A related literature on partial identification of direct effects derives bounds for alternative definition of direct effects, such as *natural* and *controlled* direct effects (Cai et al., 2008; Sjolander, 2009; Imai et al., 2010). These effects involve a priori conterfactual quantities, that we explicitly avoid using here, so that bounds derived in those papers are not relevant to us.

in fact implies that the distribution of covariates is the same for the two assignment/instrument values within the latent subpopulations. We show how this piece of information helps reducing the uncertainty about ITT effects on the outcome of primary interest.

There are three main benefits of the approach we propose.

First, ITT effects only involve outcomes that can potentially be observed. This has clear advantages. For example, the LATE estimand defined in Flores and Flores-Lagunes (2010) involves an *a priori counterfactual* quantity, namely the outcome for compliers when they are assigned to take the treatment and do not take it. Because compliers *do* take the treatment if assigned to take it, data contain no information on this outcome. In this respect, we make a clear distinction of what can be learnt from the data regarding potentially observable quantities, and what can be extrapolated on a priori counterfactuals using additional assumptions.

Second, we argue that there is much to be learnt from sharp bounds on ITT effects. On one hand, ITT effects for noncompliers provide information on the extent of the violation of ERs, and we will show that the sign of the violation is sometimes identified, and even separately for never-takers and always-takers. By sharpening information on the magnitude of the violation, our bounds provide information on the appropriateness of using methods which allow for locally misspecified instruments (Hausman and Hahn, 2005; Conley et al., 2008a,b; Nevo and Rosen, 2008). On the other hand, ITT effects for compliers provide information on the possible extent of the effect of the treatment, particularly when compared with ITT effects for noncompliers. For example, assuming that the effects of the treatment and of assignment are additive and that the effect of assignment for noncompliers is the same as the effect of assignment for compliers, then the effect of treatment for compliers can also be bounded.

Third, our setup provides guidelines on which auxiliary variables should be collected and jointly analyzed. Specifically, we show that bounds collapse in two limiting cases for the relationship of the auxiliary variable with the compliance status and the primary outcome. Bounds collapse when a) a covariate or a secondary outcome is a perfect predictor of the compliance behavior, and when b) a covariate or a secondary outcome is perfectly dependent of the primary outcome conditional on the compliance status. Thus, the stronger the association of an auxiliary variable with the compliance status and/or the primary outcome the narrower the bounds. We also show how our setup provides alternative identifying assumptions, in the form of latent independences, and estimators of ITTs effects under this assumption.

The setup we consider is one with a randomly assigned binary instrument, a binary treatment, and binary outcomes and covariates. This should not be viewed as a limit of our framework. Our results can in fact be used to point-wise bound the cumulative distribution function of a continuous outcome Y for different levels of the outcome, i.e., to derive bounds on the probabilities of the events $Y \leq y$ for each $y \in \mathcal{Y}$, where $\mathcal{Y}$ is the support of the outcome variable Y. The use of our approach to assess violations of the exclusion restriction on continuous variables will be discussed in the paper.

In what follows, we first introduce our framework and notation (Section 2). We then review and derive, in Section 3, partial identification results of ITT effects on a single outcome, with and without exclusion restriction assumptions. In Section 4 sharper bounds on the ITT effects on the primary outcomes are derived. In Section 5 two limiting cases are analyzed, under which bounds collapse. Section 6 introduces an identifying condition for ITT effects and proposes an estimator under this assumption. Section 7 shows the identifying power of our bounds in three illustrative real data examples of a social job training experiment and two medical randomized encouragement studies, where the ER for the randomized assignment may be questionable. Some concluding remarks are offered in Section 8.

2 Framework and Notation

Let introduce the potential outcome notation. Throughout the paper we will make the stability assumption that there is neither interference between units nor different versions of the treatment (SUTVA; Rubin, 1978). Under SUTVA, let Z_i be a binary instrument representing treatment assignment for unit i ($Z_i = 0$ if unit i is assigned to the control group, $Z_i = 1$ if unit i is assigned to the treatment group). We denote by $D_i(z)$ the binary treatment receipt for unit i (1 = treatment, 0 = control) when assigned treatment z. $D_i(Z_i)$ denotes the actual treatment received. The two potential indicators $D_i(0)$ and $D_i(1)$ describe the compliance status and define four subpopulations: compliers (c), for whom $D_i(z) = z$ for $z = 0, 1$; never-takers (n), for whom $D_i(z) = 0$ for $z = 0, 1$; always-takers (a), for whom $D_i(z) = 1$ for $z = 0, 1$; and defiers (d), for whom $D_i(z) = 1 - z$ for $z = 0, 1$ (Angrist et al., 1996). Because only one of the two potential indicators of treatment receipt is observed, these four subpopulations are latent, in the sense that in general it is not possible to identify the specific subpopulation a unit i belongs to. We denote as G_i the subpopulation membership, which takes on values in $\{c, n, a, d\}$. We define four potential outcomes for a bivariate binary outcome,$\mathbf{Y}_i(z, d) = [Y_{i1}(z, d), Y_{i2}(z, d)]'$, for all possible combinations of treatment assignment and treatment received ($z = 0, 1$; $d = 0, 1$). In our setting, the first outcome will be considered as the outcome of primary interest and the second outcome as an auxiliary variable, which can be either a secondary outcome, Y_2, or a covariate, X. In the latter case we will consider the joint distribution of $[Y_{i1}(z, d), X)]'$. Note that, given the compliance status, only two of the four potential outcomes are potentially observed, namely, $\mathbf{Y}_i(z, D_i(z))$, $z = 0, 1$, the other two potential outcomes being *a priori counterfactuals*. In order to avoid the use of such counterfactuals, we let the binary outcome variables depend only on treatment assignment: $\mathbf{Y}_i(z)$.

In what follows we will maintain the following assumptions:

Assumption 1 *Randomly assigned instrument. Z_i is randomly assigned, implying that*

$$Z_i \perp\!\!\!\perp D_i(1), D_i(0), \mathbf{Y}_i(1), \mathbf{Y}_i(0), X.$$

Assumption 2 *Nonzero effect of Z on D.* $E(D_i(1) - D_i(0) \neq 0$.

Assumption 3 *Monotonicity of compliance.* $D_i(1) \geq D_i(0)$, $\forall i$, *which rules out the presence of defiers.*

Assumption 3 implies that the population is only composed of compliers (c), never-takers (n) and always-takers (a); we denote as π_c, π_n, and π_a the proportions of c, n and a in the target population, respectively. Assumptions 2 and 3 imply that $\pi_c \neq 0$.

We introduce the following notation for the joint distribution of potential outcomes:

$$P[\mathbf{Y}_i(z) = (y_1, y_2)|G_i = g, Z_i = z] = P[\mathbf{Y}_i(z) = y_1y_2|G_i = g, Z_i = z] = P_{gz}^{(y_1y_2)} \qquad (1)$$

for $y_1y_2 = \{00, 01, 10, 11\}$, $z = \{0, 1\}$, $g = \{c, n, a\}$, and for the corresponding marginal distributions:

$$P[Y_{i1}(z) = y_1|G_i = g, Z_i = z] = P_{gz}^{(y_1\cdot)}, \qquad (2)$$

$$P[Y_{i2}(z) = y_2|G_i = g, Z_i = z] = P_{gz}^{(\cdot y_2)}. \qquad (3)$$

For the secondary outcome we will maintain the following stochastic exclusion restriction assumption for always-takers and never-takers:

Assumption 4 *Stochastic exclusion restriction.* $P_{n1}^{(\cdot 1)} = P_{n0}^{(\cdot 1)}$ *and* $P_{a1}^{(\cdot 1)} = P_{a0}^{(\cdot 1)}$.

When the auxiliary variable is a covariate, note that, due to random assignment (Assumption 1), $Z_i \perp\!\!\!\perp X|D_i(1), D_i(0)$. This implies that $P[X_i = 1|Z_i = 0, G_i = g] = P[X_i = 1|Z_i = 1, G_i = g]\ \forall g$, and this equality can be interpreted as a form of stochastic exclusion restriction which holds by design, i.e., by the randomization of the instrument, for covariates within all three latent subpopulations.

We focus on identifying intention-to-treat (ITT) effects on the first outcome, Y_1, for the subgroups of compliers, never-takers and always-takers, which are defined as:

$$P_{g1}^{(1\cdot)} - P_{g0}^{(1\cdot)} = E[Y_{i1} - Y_{i0}|G = g] = P_{g1}^{(1\cdot)} - P_{g0}^{(1\cdot)} \quad g = c, n, a. \qquad (4)$$

ITT effects for always-takers and never-takers reflect the effect of the assignment/instrument only and can thus highlight possible violations of the exclusion restriction on the primary outcome. Differently, the ITT effect for compliers includes both the effect of assignment and the effect of treatment, and so provides information on their joint magnitude.

The data we can observe are Z_i, X_i, $D_i^{obs} = D_i(Z_i)$ and $\mathbf{Y}_i^{obs} = \mathbf{Y}_i(Z_i)$, so that the distributions that are asymptotically revealed by the sampling process are the following:

$$P[\mathbf{Y}_i^{obs} = y_1y_2|Z_i = z, D_i^{obs} = d],$$

$$P[Y_{i1}^{obs} = y_1|Z_i = z, D_i^{obs} = d],$$

$$P[Y_{i2}^{obs} = y_2|Z_i = z, D_i^{obs} = d],$$

$$P[X_i = x|Z_i = z, D_i^{obs} = d],$$

$$P[D_i^{obs} = d|Z_i = z]$$

for $y_1 = \{0, 1\}$, $y_2 = \{0, 1\}$, $z = \{0, 1\}$, $d = \{0, 1\}$, $x = \{0, 1\}$. We assume these distributions are known or can be consistently estimated, thereby not taking account of specific statistical problems related to inference in finite samples.

Due to Assumption 3, the strata proportions π_c, π_a, and π_n can be point identified as

$$\pi_a = P[D_i^{obs} = 1|Z_i = 0]$$

$$\pi_n = P[D_i^{obs} = 0|Z_i = 1]$$

$$\pi_c = 1 - \pi_a - \pi_n.$$

3 Identification results for single binary outcomes

We first derive identification results for the primary outcome Y_1 without imposing any form of exclusion restriction. As far as the ITT effect for never-takers and always-takers, the bounds that we derive coincide with the bounds on principal direct effects in Zhang and Rubin (2003), Imai (2008), Lee (2009), for a binary treatment and a binary intermediate outcome (see also Richardson et al., 2011). The proof of the following proposition is sketched in the Appendix.

Proposition 1 *Under Assumptions 1, 2 and 3, $P_{c0}^{(1\cdot)}$ and $P_{c1}^{(1\cdot)}$, $P_{n0}^{(1\cdot)}$ and $P_{a1}^{(1\cdot)}$ can be bounded. Detailed expressions are reported in the Appendix. For example, bounds for $P_{c0}^{(1\cdot)}$ are:*

$$L_{P_{c0}^{(1\cdot)}} = \max\left(\frac{P[Y_{i1}^{obs}=1|Z_i=0,D_i^{obs}=0](\pi_c+\pi_n)}{\pi_c} - \frac{\pi_n}{\pi_c}, 0\right) \le P_{c0}^{(1\cdot)} \le \min\left(\frac{P[Y_{i1}^{obs}=1|Z_i=0,D_i^{obs}=0](\pi_c+\pi_n)}{\pi_c}, 1\right) = U_{P_{c0}^{(1\cdot)}} \quad (5)$$

Corollary 1 *Under Assumptions 1, 2 and 3, ITT effects can be bounded as:*

$$L_{P_{c1}^{(1\cdot)}} - U_{P_{c0}^{(1\cdot)}} \le P_{c1}^{(1\cdot)} - P_{c0}^{(1\cdot)} \le U_{P_{c1}^{(1\cdot)}} - L_{P_{c0}^{(1\cdot)}}, \quad (6)$$

$$P[Y_{i1}^{obs}=1|Z_i=1,D_i^{obs}=0] - U_{P_{n0}^{(1\cdot)}} \le P_{n1}^{(1\cdot)} - P_{n0}^{(1\cdot)} \le P[Y_{i1}^{obs}=1|Z_i=1,D_i^{obs}=0] - L_{P_{n0}^{(1\cdot)}}, \quad (7)$$

$$L_{P_{a1}^{(1\cdot)}} - P[Y_{i1}^{obs}=1|Z_i=0,D_i^{obs}=1] \le P_{a1}^{(1\cdot)} - P_{a0}^{(1\cdot)} \le U_{P_{a1}^{(1\cdot)}} - P[Y_{i1}^{obs}=1|Z_i=0,D_i^{obs}=1]. \quad (8)$$

Let now focus on the secondary outcome, Y_2, and resume the identification results in the presence of stochastic exclusion restrictions for never-takers and always-takers (Assumption 4). The proof of the following proposition is sketched in the Appendix.

Proposition 2 *Under Assumptions 1, 2, 3 and 4, $P_{c0}^{(\cdot 1)}$, $P_{c1}^{(\cdot 1)}$, $P_{n0}^{(\cdot 1)}$ and $P_{a1}^{(\cdot 1)}$ can be identified as:*

$$P_{n1}^{(\cdot 1)} = P_{n0}^{(\cdot 1)} = P[Y_{i2}^{obs}=1|Z_i=1,D_i^{obs}=0], \quad (9)$$

$$P_{a0}^{(\cdot 1)} = P_{a1}^{(\cdot 1)} = P[Y_{i2}^{obs}=1|Z_i=0,D_i^{obs}=1]. \quad (10)$$

$$P_{c0}^{(\cdot 1)} = \frac{P[Y_{i2}^{obs}=1|Z_i=0,D_i^{obs}=0](\pi_c+\pi_n) - \pi_n P[Y_{i2}^{obs}=1|Z_i=1,D_i^{obs}=0]}{\pi_c}, \quad (11)$$

$$P_{c1}^{(\cdot 1)} = \frac{P[Y_{i2}^{obs}=1|Z_i=1,D_i^{obs}=1](\pi_c+\pi_a) - \pi_a P[Y_{i2}^{obs}=1|Z_i=0,D_i^{obs}=1]}{\pi_c}. \quad (12)$$

The same framework can be used to derive identification results for the distribution of a binary covariate, X, within subpopulations. In this case the stochastic exclusion restriction holds by design, i.e., by the randomization of the instrument, within all three latent subpopulations, so that the distribution of X within subpopulations can be identified using analogous results:

$$P[X_i=1|Z_i=1,G_i=a] = P[X_i=1|Z_i=0,G_i=a] = P[X_i=1|Z_i=0,D_i^{obs}=1] \quad (13)$$

$$P[X_i=1|Z_i=0,G_i=n] = P[X_i=1|Z_i=1,G_i=n] = P[X_i=1|Z_i=1,D_i^{obs}=0] \quad (14)$$

$$P[X_i=1|Z_i=1,G_i=c] = P[X_i=1|Z_i=0,G_i=c] = \frac{P[X_i=1|Z_i=0,D_i^{obs}=0](\pi_c+\pi_n) - \pi_n P[X_i=1|Z_i=1,D_i^{obs}=0]}{\pi_c}, \quad (15)$$

where the distribution of X for compliers is also equal to $\frac{P[X_i=1|Z_i=1,D_i^{obs}=1](\pi_c+\pi_a)-\pi_a P[X_i=1|Z_i=0,D_i^{obs}=1]}{\pi_c}$.

4 Bivariate binary outcome and partial exclusion restriction

Let now consider the bivariate case, with two binary outcomes. Bounds on the quantities of interest, e.g., $P_{c1}^{(1\cdot)}$, can be obtained by summing the bounds of the joint probabilities, e.g., $P_{c1}^{(11)}$ and $P_{c1}^{(10)}$. It can be easily shown that, without imposing the exclusion restriction on any of the two outcomes, the same bounds in (5)-(8) are obtained. The secondary outcome does not help sharpening the bounds if no exclusion restriction is imposed on it. This is a different result from the parametric case, where the joint modelling of two outcomes usually improves inference (both from a frequentist and a Bayesian perspective) in terms of increased precision and reduced bias, even if no exclusion restriction on the second is imposed (Frumento *et al.*, 2011a; Mattei *et al.*, 2011).

Assume now that the stochastic exclusion restriction for a and n (Assumption 4) holds. Assumption 4 can be also expressed as follows:

$$P_{a0}^{(\cdot 1)} = P_{a0}^{(11)} + P_{a0}^{(01)} = P_{a1}^{(\cdot 1)} = P_{a1}^{(11)} + P_{a1}^{(01)}, \tag{16}$$

$$P_{n0}^{(\cdot 1)} = P_{n0}^{(11)} + P_{n0}^{(01)} = P_{n1}^{(\cdot 1)} = P_{n1}^{(11)} + P_{n1}^{(01)}.$$

Using the following relationships between joint and marginal probabilities:

$$P_{n0}^{(11)} \leq P_{n0}^{(\cdot 1)}, P_{a1}^{(11)} \leq P_{a1}^{(\cdot 1)}, P_{c0}^{(11)} \leq P_{c0}^{(\cdot 1)}, P_{c1}^{(11)} \leq P_{c1}^{(\cdot 1)}, \tag{17}$$

$$P_{n0}^{(10)} \leq P_{n0}^{(\cdot 0)}, P_{a1}^{(10)} \leq P_{a1}^{(\cdot 0)}, P_{c0}^{(10)} \leq P_{c0}^{(\cdot 0)}, P_{c1}^{(10)} \leq P_{c1}^{(\cdot 0)}, \tag{18}$$

together with (16) leads to sharpen the bounds, as shown in the proof of the following proposition (see Appendix).

Proposition 3 *Under Assumptions 1, 2, 3 and 4,* $P_{c0}^{(11)}$, $P_{c0}^{(10)}$, $P_{c1}^{(11)}$, $P_{c1}^{(10)}$, $P_{n0}^{(11)}$, $P_{n0}^{(10)}$, $P_{a1}^{(11)}$ *and* $P_{a1}^{(10)}$ *can be bounded. Detailed expressions are reported in the Appendix. For example, bound for* $P_{c0}^{(11)}$ *and* $P_{c0}^{(10)}$ *are:*

$$P_{c0}^{(11)} \geq \max\left(\frac{P[\mathbf{Y}_i^{obs} = 11 | Z_i = 0, D_i^{obs} = 0](\pi_c + \pi_n)}{\pi_c} - \frac{\pi_n}{\pi_c} P[Y_{i2}^{obs} = 1 | Z_i = 1, D_i^{obs} = 0], 0\right) = L_{P_{c0}^{(11)}} \tag{19}$$

$$P_{c0}^{(11)} \leq \min\left(\frac{P[\mathbf{Y}_i^{obs} = 11 | Z_i = 0, D_i^{obs} = 0](\pi_c + \pi_n)}{\pi_c}, \frac{P[Y_{i2}^{obs} = 1 | Z_i = 0, D_i^{obs} = 0](\pi_c + \pi_n) - \pi_n P[Y_{i2}^{obs} = 1 | Z_i = 1, D_i^{obs} = 0]}{\pi_c}\right) = U_{P_{c0}^{(11)}}$$

$$P_{c0}^{(10)} \geq \max\left(\frac{P[\mathbf{Y}_i^{obs} = 10 | Z_i = 0, D_i^{obs} = 0](\pi_c + \pi_n)}{\pi_c} - \frac{\pi_n}{\pi_c} P[Y_{i2}^{obs} = 0 | Z_i = 1, D_i^{obs} = 0], 0\right) = L_{P_{c0}^{(10)}} \tag{20}$$

$$P_{c0}^{(10)} \leq \min\left(\frac{P[\mathbf{Y}_i^{obs} = 10 | Z_i = 0, D_i^{obs} = 0](\pi_c + \pi_n)}{\pi_c}, \frac{P[Y_{i2}^{obs} = 0 | Z_i = 0, D_i^{obs} = 0](\pi_c + \pi_n) - \pi_n P[Y_{i2}^{obs} = 0 | Z_i = 1, D_i^{obs} = 0]}{\pi_c}\right) = U_{P_{c0}^{(10)}}$$

Corollary 2 *Under Assumptions 1, 2, 3 and 4, bounds for* $P_{c0}^{(1\cdot)}$, $P_{c1}^{(1\cdot)}$, $P_{n0}^{(1\cdot)}$ *and* $P_{a1}^{(1\cdot)}$, *can be obtained as follows:*

$$L_{P_{c0}^{(11)}} + L_{P_{c0}^{(10)}} \leq P_{c0}^{(1\cdot)} \leq U_{P_{c0}^{(11)}} + U_{P_{c0}^{(10)}}, \tag{21}$$

$$L_{P_{c1}^{(11)}} + L_{P_{c1}^{(10)}} \leq P_{c1}^{(1\cdot)} \leq U_{P_{c1}^{(11)}} + U_{P_{c1}^{(10)}}.$$

$$L_{P_{n0}^{(11)}} + L_{P_{n0}^{(10)}} \leq P_{n0}^{(1\cdot)} \leq U_{P_{n0}^{(11)}} + U_{P_{n0}^{(10)}},$$

$$L_{P_{a1}^{(11)}} + L_{P_{a1}^{(10)}} \leq P_{a1}^{(1\cdot)} \leq U_{P_{a1}^{(11)}} + U_{P_{a1}^{(10)}}.$$

In order to interpret the bounds, take as an example the sum $L_{P_{c0}^{(11)}} + L_{P_{c0}^{(10)}}$; it would correspond to the lower bound obtained in Section 3 if both $L_{P_{c0}^{(11)}}$ and $L_{P_{c0}^{(10)}}$ were greater than zero. The lower bound in (21) becomes strictly greater than the lower bound in (5) if at least one of the two terms is equal to zero. For example, suppose that

$$\frac{P[\mathbf{Y}_i^{obs} = 11|Z_i = 0, D_i^{obs} = 0](\pi_c + \pi_n)}{\pi_c} - \frac{\pi_n}{\pi_c} P[Y_{i2}^{obs} = 1|Z_i = 1, D_i^{obs} = 0] \tag{22}$$

in (19) is > 0 and

$$\frac{P[\mathbf{Y}_i^{obs} = 10|Z_i = 0, D_i^{obs} = 0](\pi_c + \pi_n)}{\pi_c} - \frac{\pi_n}{\pi_c} P[Y_{i2}^{obs} = 0|Z_i = 1, D_i^{obs} = 0] \tag{23}$$

in (20) is < 0; in this case the *new* bound is equal to (19) and it is greater than (5), which is implicitly obtained by adding a negative quantity, (23), to (22). The same is true for the lower bounds of $P_{c1}^{(1\cdot)}$, $P_{n0}^{(1\cdot)}$ and $P_{a1}^{(1\cdot)}$. As for the upper bounds, if both $U_{P_{c0}^{(11)}}$ and $U_{P_{c0}^{(10)}}$ were equal to $U_{P_{c0}^{(11)}} = \frac{P[\mathbf{Y}_i^{obs}=11|Z_i=0,D_i^{obs}=0](\pi_c+\pi_n)}{\pi_c}$ and $U_{P_{c0}^{(10)}} = \frac{P[\mathbf{Y}_i^{obs}=10|Z_i=0,D_i^{obs}=0](\pi_c+\pi_n)}{\pi_c}$, then their sum in (21) would be exactly equal to the upper bound in (5). On the contrary, if either $U_{P_{c0}^{(11)}}$ or $U_{P_{c0}^{(10)}}$ is different from the above quantities, then a strictly smaller upper bound for $P_{c0}^{(1\cdot)}$ is obtained. A similar argument holds for the upper bounds of $P_{c1}^{(1\cdot)}$, $P_{n0}^{(1\cdot)}$ and $P_{a1}^{(1\cdot)}$.

Corollary 3 *Under Assumptions 1, 2, 3 and 4, ITT effects can be bounded as:*

$$L^*_{P_{c1}^{(1\cdot)}} - U^*_{P_{c0}^{(1\cdot)}} \leq P_{c1}^{(1\cdot)} - P_{c0}^{(1\cdot)} \leq U^*_{P_{c1}^{(1\cdot)}} - L^*_{P_{c0}^{(1\cdot)}}, \tag{24}$$

$$P[Y_{1i}^{obs} = 1|Z_i = 1, D_i^{obs} = 0] - U^*_{P_{n0}^{(1\cdot)}} \leq P_{n1}^{(1\cdot)} - P_{n0}^{(1\cdot)} \leq P[Y_{1i}^{obs} = 1|Z_i = 1, D_i^{obs} = 0] - L^*_{P_{n0}^{(1\cdot)}},$$

$$L^*_{P_{a1}^{(1\cdot)}} - P[Y_{1i}^{obs} = 1|Z_i = 0, D_i^{obs} = 1] \leq P_{a1}^{(1\cdot)} - P_{a0}^{(1\cdot)} \leq U^*_{P_{a1}^{(1\cdot)}} - P[Y_{1i}^{obs} = 1|Z_i = 0, D_i^{obs} = 1],$$

where

$$L^*_{P_{c0}^{(1\cdot)}} = L_{P_{c0}^{(11)}} + L_{P_{c0}^{(10)}}, \quad U^*_{P_{c0}^{(1\cdot)}} = U_{P_{c0}^{(11)}} + U_{P_{c0}^{(10)}},$$

$$L^*_{P_{c1}^{(1\cdot)}} = L_{P_{c1}^{(11)}} + L_{P_{c1}^{(10)}}, \quad U^*_{P_{c1}^{(1\cdot)}} = U_{P_{c1}^{(11)}} + U_{P_{c1}^{(10)}}.$$

$$L^*_{P_{n0}^{(1\cdot)}} = L_{P_{n0}^{(11)}} + L_{P_{n0}^{(10)}}, \quad U^*_{P_{n0}^{(1\cdot)}} = U_{P_{n0}^{(11)}} + U_{P_{n0}^{(10)}},$$

$$L^*_{P_{a1}^{(1\cdot)}} = L_{P_{a1}^{(11)}} + L_{P_{a1}^{(10)}}, \quad U^*_{P_{a1}^{(1\cdot)}} = U_{P_{a1}^{(11)}} + U_{P_{a1}^{(10)}}.$$

When using the joint distribution of the primary outcome and a covariate, $[Y_{i1}(z, d), X)]'$, under Assumptions 1, 2 and 3 bounds for the quantities in Proposition 2, Lemma 1 and Lemma 2 are obtained simply substituting Y_{i2}^{obs} with X_i in all expressions.

5 Additional restrictions on the auxiliary variable

We have shown that assuming the exclusion restriction for a and n for the secondary outcome or using a covariate helps sharpening the bounds. Now we investigate if additional characteristics of the distribution of the auxiliary variables may sharpen the bounds to an even larger extent. The intuition is that, on one hand, the auxiliary variable should help identification the stronger its association is with the compliance status. On the other hand, we expect to sharpen inference also the stronger its association is with the primary outcome.

To support these intuitions, we now consider two limiting cases. The first one is when Y_2 is perfectly associated with the compliance behavior and, specifically, when $Y_2 = \mathcal{I}(G = c)$, where $\mathcal{I}$ represents the indicator function. This implies the following equalities:

$$P_{n1}^{(\cdot 1)} = P_{n0}^{(\cdot 1)} = 0, \quad P_{a1}^{(\cdot 1)} = P_{a0}^{(\cdot 1)} = 0, \quad P_{c1}^{(\cdot 1)} = P_{c0}^{(\cdot 1)} = 1, \tag{25}$$

$$P_{n1}^{(11)} = P_{n0}^{(11)} = 0, \quad P_{a1}^{(11)} = P_{a0}^{(11)} = 0, \quad P_{c1}^{(10)} = P_{c0}^{(10)} = 0. \tag{26}$$

Under these restrictions, bound in Proposition 3 collapse.

Corollary 4 *Under Assumptions 1, 2, 3, 4 and if* $Y_2 = \mathcal{I}(G = c)$*, bounds for* $P_{c0}^{(1\cdot)}$*,* $P_{c1}^{(1\cdot)}$*,* $P_{n0}^{(1\cdot)}$ *and* $P_{a1}^{(1\cdot)}$*, and so bounds for ITT effects, collapse as follows:*

$$P_{c0}^{(1\cdot)} = P_{c0}^{(11)} = \frac{P[\mathbf{Y}_i^{obs} = 11 | Z_i = 0, D_i^{obs} = 0](\pi_c + \pi_n)}{\pi_c}, \tag{27}$$

$$P_{c1}^{(1\cdot)} = P_{c1}^{(11)} = \frac{P[\mathbf{Y}_i^{obs} = 11 | Z_i = 1, D_i^{obs} = 1](\pi_c + \pi_a)}{\pi_c}, \tag{28}$$

$$P_{n0}^{(1\cdot)} = P_{n0}^{(10)} = \frac{P[\mathbf{Y}_i^{obs} = 10 | Z_i = 0, D_i^{obs} = 0](\pi_c + \pi_n)}{\pi_n}, \tag{29}$$

$$P_{a1}^{(1\cdot)} = P_{a1}^{(10)} = \frac{P[\mathbf{Y}_i^{obs} = 10 | Z_i = 1, D_i^{obs} = 1](\pi_c + \pi_a)}{\pi_a}. \tag{30}$$

Bounds collapse if the secondary outcome predicts with no uncertainty the compliance status; this is also true if we use a covariate such that $X = \mathcal{I}(G = c)$. Table 1 shows an example of this limiting case.

TABLE 1- Perfect prediction of compliance status

The second limiting case is when the secondary outcome is perfectly dependent on the primary outcome conditional on the compliance status. Specifically, suppose that

$$P_{n0}^{(11)} = P_{n0}^{(\cdot 1)} = P_{n0}^{(1\cdot)}, \quad P_{c0}^{(11)} = P_{c0}^{(\cdot 1)} = P_{c0}^{(1\cdot)} \tag{31}$$

and

$$P_{a1}^{(11)} = P_{a1}^{(\cdot 1)} = P_{a1}^{(1\cdot)}, \quad P_{c1}^{(11)} = P_{c1}^{(\cdot 1)} = P_{c1}^{(1\cdot)} \tag{32}$$

Corollary 5 *Under Assumptions 1, 2, 3, 4 and if (31) and (32) hold, bounds for* $P_{c0}^{(1\cdot)}$*,* $P_{c1}^{(1\cdot)}$*,* $P_{n0}^{(1\cdot)}$ *and* $P_{a1}^{(1\cdot)}$*, and so bounds for* ITT *effects, collapse as follows:*

$$P_{c0}^{(1\cdot)} = P_{c0}^{(11)} = \frac{P[\mathbf{Y}_i^{obs} = 11|Z_i = 0, D_i^{obs} = 0](\pi_c + \pi_n)}{\pi_c} - \frac{\pi_n}{\pi_c} P[Y_{i2}^{obs} = 1|Z_i = 1, D_i^{obs} = 0], \tag{33}$$

$$P_{c1}^{(1\cdot)} = P_{c1}^{(11)} = \frac{P[\mathbf{Y}_i^{obs} = 11|Z_i = 1, D_i^{obs} = 1](\pi_c + \pi_a)}{\pi_c} - \frac{\pi_a}{\pi_c} P[Y_{i2}^{obs} = 1|Z_i = 0, D_i^{obs} = 1], \tag{34}$$

$$P_{n0}^{(1\cdot)} = P_{n0}^{(11)} = \frac{P[\mathbf{Y}_i^{obs} = 11|Z_i = 0, D_i^{obs} = 0](\pi_c + \pi_n)}{\pi_n} - \frac{\pi_c}{\pi_n} P[Y_{i2}^{obs} = 1|Z_i = 1, D_i^{obs} = 0], \tag{35}$$

$$P_{a1}^{(1\cdot)} = P_{a1}^{(11)} = \frac{P[\mathbf{Y}_i^{obs} = 11|Z_i = 1, D_i^{obs} = 1](\pi_c + \pi_a)}{\pi_a} - \frac{\pi_c}{\pi_a} P[Y_{i2}^{obs} = 1|Z_i = 0, D_i^{obs} = 1]. \tag{36}$$

Note that perfect dependence in the sense of (31) and (32) does not imply the exclusion restriction to hold also for the primary outcome, because $P_{n0}^{(1\cdot)}$ may differ from $P_{n1}^{(1\cdot)}$, and $P_{a0}^{(1\cdot)}$ from $P_{a1}^{(1\cdot)}$. Table 2 shows an example where, given the same marginal distributions, different degrees of association between the two outcomes lead to sharper or less sharp bounds.

TABLE 2 - Association with the primary outcome

6 Latent independence as an identifying assumption

Our setup may also suggest alternative identifying assumptions of ITT effects. We can show that ITT effects can be point-identified if we assume that the two outcomes are independent conditional on the compliance status. The identification assumption is a form of *latent independence*, in the sense that independence holds only conditional on a latent variable. This is formalized as follows:

$$P_{gz}^{(11)} = P_{gz}^{(1\cdot)} P_{gz}^{(\cdot 1)}, \quad P_{gz}^{(10)} = P_{gz}^{(1\cdot)} P_{gz}^{(\cdot 0)}, \tag{37}$$

$$P_{gz}^{(01)} = P_{gz}^{(0\cdot)} P_{gz}^{(\cdot 1)}, \quad P_{gz}^{(00)} = P_{gz}^{(0\cdot)} P_{gz}^{(\cdot 0)}, \tag{38}$$

for $g = \{c, n, a\}$ and $z = \{0, 1\}$. The following proposition is proved in the Appendix.

Proposition 4 *Under Assumptions 1, 2, 3, 4, and (37) and (38), the following quantities can be point-identified:*

$$P_{c0}^{(1\cdot)} = \frac{\pi_n + \pi_c}{\pi_c} \cdot \left\{ \frac{P[\mathbf{Y}_i^{obs} = 10|Z_i = 0, D_i^{obs} = 0)]P_{n0}^{(\cdot 1)} - P[\mathbf{Y}_i^{obs} = 11|Z_i = 0, D_i^{obs} = 0](1 - P_{n0}^{(\cdot 1)})}{P_{n0}^{(\cdot 1)} - P_{c0}^{(\cdot 1)}} \right\} \tag{39}$$

$$P_{c1}^{(1\cdot)} = \frac{\pi_a + \pi_c}{\pi_c} \cdot \left\{ \frac{P[\mathbf{Y}_i^{obs} = 10|Z_i = 1, D_i^{obs} = 1)]P_{a1}^{(\cdot 1)} - P[\mathbf{Y}_i^{obs} = 11|Z_i = 1, D_i^{obs} = 1](1 - P_{a1}^{(\cdot 1)})}{P_{a1}^{(\cdot 1)} - P_{c1}^{(\cdot 1)}} \right\}. \tag{40}$$

$$P_{n0}^{(1\cdot)} = \frac{\pi_n + \pi_c}{\pi_n} \cdot \left\{ \frac{P[\mathbf{Y}_i^{obs} = 11|Z_i = 0, D_i^{obs} = 0](1 - P_{c0}^{(\cdot 1)}) - P[\mathbf{Y}_i^{obs} = 10|Z_i = 0, D_i^{obs} = 0)]P_{c0}^{(\cdot 1)}}{P_{n0}^{(\cdot 1)} - P_{c0}^{(\cdot 1)}} \right\} \tag{41}$$

$$P_{a1}^{(1\cdot)} = \frac{\pi_a + \pi_c}{\pi_a} \cdot \left\{ \frac{P[\mathbf{Y}_i^{obs} = 11|Z_i = 1, D_i^{obs} = 1](1 - P_{c1}^{(\cdot 1)}) - P[\mathbf{Y}_i^{obs} = 10|Z_i = 1, D_i^{obs} = 1)]P_{c1}^{(\cdot 1)}}{P_{a1}^{(\cdot 1)} - P_{c1}^{(\cdot 1)}} \right\}. \tag{42}$$

Simple estimators of $P_{c0}^{(1\cdot)}$, $P_{c1}^{(1\cdot)}$, $P_{n0}^{(1\cdot)}$ and $P_{a1}^{(1\cdot)}$ are obtained by substituting the observable distributions with their sample counterparts. Table 3 shows an example where the assumption of latent independence holds. The example shows that bounds do not collapse in this case but, rather, remain wide; however if latent independence is assumed, results in Proposition 4 can be used to correctly point-identify ITT effects.

TABLE 3 - Latent Independence

7 Empirical examples

We now show the identifying power of our bounds in three empirical randomized studies with non-compliance that use the random assignment to take the treatment as an instrument, and where the ER for the random assignment has been questioned. In what follows, we abstract from providing confidence sets which take account of the sampling variability of bounds estimates, although those could be derived following, for example, Manski and Imbens (2004).

The first example is the randomized experiment on Breast Self-Examination (BSE) conducted between January 1988 and December 1990 at the Oncologic Center of the Faenza Health District in Italy, previously analyzed in Mealli et al. (2004) and Mattei and Mealli (2007). In the study two methods of teaching BSE (D), consisting of either mail information about BSE (standard treatment) or the attendance of a course (new treatment) involving theoretical and practical sessions, were compared with the aim of assessing whether teaching programs could increase BSE practice (Y_1) and improve examination skills. The study was affected by two sources of bias: only 55% of women assigned to receive the new treatment complied with their assignment and 35% of the women did not respond to the post-test questionnaire. Here, we neglect nonresponse and consider only complete observations. Mealli et al. (2004) conducted a likelihood analysis under the ER for never-takers, while Mattei and Mealli (2007) used a Bayesian model and found some evidence of a negative effect of assignment on BSE practice for never-takers, suggesting the violation of the ER. In Table 4, we report the observed distributions of the primary outcome and two binary covariates: X_1=knowledge of breast pathophysiology and X_2= prior BSE practice. The two covariates show a different degree of association with Y_1, so that bounds are expected to be sharper when derived using the joint distribution of Y_1 and X_1 ($\rho = -0.37$) than when using the joint distribution of Y_1 and X_2 ($\rho = 0.16$). Indeed, bounds on ITT_n using X_1 as auxiliary variable are sharper and point to a violation of the exclusion restriction for never-takers because they only cover negative values. Bounds on ITT_c do not identify the sign of the effect for compliers. Note that they consistently do not cover the value of ITT_c estimated under the ER for never-takers.

TABLE 4 - Breast Self-Examination trial

The second example is the study on influenza vaccinations, previously analyzed by McDonald et al. (1992) and by Hirano et al. (2000). In this study, physicians were randomly selected to receive a letter encouraging them to inoculate patients at risk for flu (Z). The treatment of interest is the actual flu shot (D), and the outcome is an indicator for flu-related hospital visits (Y_1). A standard ITT analysis suggests a moderate effect of assignment. The analysis of Hirano et al. (2000) suggests that there is little evidence that this ITT effect is actually due to the taking of the vaccine. In fact, under a plausible Bayesian model, they find that the subpopulation of the patients who would receive the vaccine regardless of whether their physician received a letter appears to benefit as much from the letter (i.e., from assignment) as the subpopulation of patients who would only receive the vaccine if their physician received the encouragement letter. Their analysis suggest a strong violation of the ER for always-takers. We reanalyse this study using the 2893 individuals observed in 1980, and report the observed distributions of Y_1 and a set of 8 binary covariates in Table 5: X_1=chronic obstructive pulmonary disease (COPD), X_2=age above median age, X_3=liver disease, X_4=sex, X_5=renal disease, X_6=heart disease, X_7=diabetes, X_8=race. Consistently with the results in Hirano et al. (2000), the bounds on ITT_a, even without using auxiliary variables, only cover negative values, highlighting a reduction of hospitalization for the always-vaccinated individuals receiving the letter.

All the covariates show a weak association with the primary outcome, so that we do not expect major improvements of the bounds on ITT_c, ITT_n, and ITT_a. Bounds reported in Table 5 are derived as intersection of the bounds obtained with each single covariate, and lead to bounds with slightly smaller width.

TABLE 5 - Influenza Vaccine encouragement study

The third example is the National Job Corps (JC) Study, a randomized experiment performed in the mid-1990s to evaluate the effects of participation in JC (D), a large job training program for economically disadvantaged youths aged 16 to 24 years. A random sample of eligible applicants was randomly assigned into treatment and control groups (Z), with the second group being denied access to JC for three years. Both groups were tracked at baseline, soon and at 12, 30 and 48 months after randomization. Previous works have concentrated of global ITT effects, i.e., effects of being assigned to enroll in Job Corps (e.g., Lee, 2009; Zhang et al., 2009). However, non-compliance was present, as only 73% of those assigned to the treatment group actually enrolled in JC. When estimating the effect on compliers, the ER for never-takers was always maintained (e.g., Frumento et al., 2011b). However being denied enrollment in JC, as opposed to deciding not to accept the offer to enroll, may, in principle, affect the labor market behavior of never-takers, especially in the short-term.

The data include a rich set of both covariates and outcomes. To limit exposition, here we concentrate only on short-term (12-months after randomization) effects on employment (Y_1) and use a binary indicator of earnings above the overall median earnings (Y_2) as a secondary outcome, although other auxiliary outcomes and covariates could also be used. Data and results are reported in Table 6. We found no evidence of violation of the ER for never-takers, because the bounds on ITT_n are narrower but still cover 0. On the other hand, bounds for ITT_c are narrower and point to a negative effect on employment for compliers of at least 4% points, confirming lock-in effects of those participating in the program (van Ours, 2004; Lechner and Wunsch, 2007; Frumento et al., 2011b).

TABLE 6 - Job Corps study

8 Concluding remarks

We used restrictions, implied by the randomization of the instrument, on the joint distribution of a primary outcome and auxiliary variables (secondary outcomes or covariates) to derive nonparametric bounds for intention-to-treat effects on a primary outcome on the subpopulations defined by compliance behavior, without requiring the exclusion restriction of the instrument.

Our use of secondary outcomes and covariates is novel in that we do not condition on their values. Our bounds proved to be useful tools to detect violations of the exclusion restriction on the primary outcome, separately for always-takers and never-takers, and should be used as a preliminary analysis, before the application of instrumental variable methods, to assess the plausibility of the exclusion restriction assumption and to detect the magnitude of its violation. This was shown in three real data examples of a social job training experiment and two medical randomized encouragement studies.

The setup we propose provides guidelines for collecting auxiliary variables in instrumental variable settings: in general, covariates and secondary outcomes sharpen inference the stronger their association is with the compliance status and/or the primary outcome.

The approach we followed was nonparametric. Note however that in the binary outcome case the parametric/nonparametric distinction is redundant. In this regard, the moment equalities that we used to derive bounds coincide with first order condition for deriving MLE, so that the derived bounds identify also the maximum likelihood regions. In finite sample, one can use confidence intervals or the shape of the likelihood function to account for sampling variability.

As for extensions to continuous outcomes, our results suggest the following lines of analysis: a) Bounds can be computed at different cutoff points in order to detect some deviations from the exclusion restriction in the outcome distributions; b) the analysis in a) could be extended to the whole support of the outcome variable in order to bound its cdf, and, from this, bound average effects. Even if conceptually feasible, this analysis appears rather complicated to be conducted analytically; c) alternatively, one can pursue parametric approaches using the joint modelling of primary outcomes with secondary outcomes or covariates to improve inference. In fact, our results suggest that jointly modelling the primary outcome with one (or more) secondary outcome or covariate improves also parametric inference, both in a frequentist and a Bayesian perspective.

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Appendix

Proof of Proposition 1 Under Assumptions 1, 2 and 3, the four observable distributions are equal to:

$$P[Y_{i1}^{obs}=1|Z_i=0,D_i^{obs}=1]=P_{a0}^{(1\cdot)},$$

$$P[Y_{i1}^{obs}=1|Z_i=1,D_i^{obs}=0]=P_{n1}^{(1\cdot)},$$

$$P[Y_{i1}^{obs}=1|Z_i=0,D_i^{obs}=0]=\frac{\pi_c P_{c0}^{(1\cdot)}+\pi_n P_{n0}^{(1\cdot)}}{\pi_c+\pi_n}, \tag{43}$$

$$P[Y_{i1}^{obs}=1|Z_i=1,D_i^{obs}=1]=\frac{\pi_c P_{c1}^{(1\cdot)}+\pi_a P_{a1}^{(1\cdot)}}{\pi_c+\pi_a}.$$

Given that $0\leq P_{c0}^{(1\cdot)}, P_{c1}^{(1\cdot)}, P_{n0}^{(1\cdot)}, P_{a1}^{(1\cdot)}\leq 1$, worst case bounds are derived. For example, the lower (upper) bound for $P_{c0}^{(1\cdot)}$ is obtained as the maximum (minimum) of 0 (1) and using (43) when $P_{n0}^{(1\cdot)}=1$ ($P_{n0}^{(1\cdot)}=0$):

$$L_{P_{c0}^{(1\cdot)}}=\max\left(\frac{P[Y_{i1}^{obs}=1|Z_i=0,D_i^{obs}=0](\pi_c+\pi_n)}{\pi_c}-\frac{\pi_n}{\pi_c},0\right)\leq P_{c0}^{(1\cdot)}\leq\min\left(\frac{P[Y_{i1}^{obs}=1|Z_i=0,D_i^{obs}=0](\pi_c+\pi_n)}{\pi_c},1\right)=U_{P_{c0}^{(1\cdot)}}$$

Analogously, the following bounds are derived:

$$L_{P_{c1}^{(1\cdot)}}=\max\left(\frac{P[Y_{i1}^{obs}=1|Z_i=1,D_i^{obs}=1](\pi_c+\pi_a)}{\pi_c}-\frac{\pi_a}{\pi_c},0\right)\leq P_{c1}^{(1\cdot)}\leq\min\left(\frac{P[Y_{i1}^{obs}=1|Z_i=1,D_i^{obs}=1](\pi_c+\pi_a)}{\pi_c},1\right)=U_{P_{c1}^{(1\cdot)}}$$

$$L_{P_{n0}^{(1\cdot)}}=\max\left(\frac{P[Y_{i1}^{obs}=1|Z_i=0,D_i^{obs}=0](\pi_c+\pi_n)}{\pi_n}-\frac{\pi_c}{\pi_n},0\right)\leq P_{n0}^{(1\cdot)}\leq\min\left(\frac{P[Y_{i1}^{obs}=1|Z_i=0,D_i^{obs}=0](\pi_c+\pi_n)}{\pi_n},1\right)=U_{P_{n0}^{(1\cdot)}}$$

$$L_{P_{a1}^{(1\cdot)}}=\max\left(\frac{P[Y_{i1}^{obs}=1|Z_i=1,D_i^{obs}=1](\pi_c+\pi_a)}{\pi_a}-\frac{\pi_c}{\pi_a},0\right)\leq P_{a1}^{(1\cdot)}\leq\min\left(\frac{P[Y_{i1}^{obs}=1|Z_i=1,D_i^{obs}=1](\pi_c+\pi_a)}{\pi_a},1\right)=U_{P_{a1}^{(1\cdot)}}$$

Proof of Proposition 2 Under Assumptions 1, 2, 3 and 4, the four observable distributions are equal to:

$$P[Y_{i2}^{obs}=1|Z_i=0,D_i^{obs}=1]=P_{a0}^{(\cdot 1)},$$

$$P[Y_{i2}^{obs}=1|Z_i=1,D_i^{obs}=0]=P_{n1}^{(\cdot 1)},$$

$$P[Y_{i2}^{obs}=1|Z_i=0,D_i^{obs}=0]=\frac{\pi_c P_{c0}^{(\cdot 1)}+\pi_n P_{n0}^{(\cdot 1)}}{\pi_c+\pi_n}=\frac{\pi_c P_{c0}^{(\cdot 1)}+\pi_n P_{n1}^{(\cdot 1)}}{\pi_c+\pi_n}, \tag{44}$$

$$P[Y_{i2}^{obs}=1|Z_i=1,D_i^{obs}=1]=\frac{\pi_c P_{c1}^{(\cdot 1)}+\pi_a P_{a1}^{(\cdot 1)}}{\pi_c+\pi_a}=\frac{\pi_c P_{c1}^{(\cdot 1)}+\pi_a P_{a0}^{(\cdot 1)}}{\pi_c+\pi_a}, \tag{45}$$

where the second equalities in (44) and in (45) are due to the exclusion restrictions, so that the system can be univocally solved also in $P_{c0}^{(\cdot 1)}$ and $P_{c1}^{(\cdot 1)}$ as

$$P_{c0}^{(\cdot 1)}=\frac{P[Y_{i2}^{obs}=1|Z_i=0,D_i^{obs}=0](\pi_c+\pi_n)-\pi_n P[Y_{i2}^{obs}=1|Z_i=1,D_i^{obs}=0]}{\pi_c},$$

$$P_{c1}^{(\cdot 1)}=\frac{P[Y_{i2}^{obs}=1|Z_i=1,D_i^{obs}=1](\pi_c+\pi_a)-\pi_a P[Y_{i2}^{obs}=1|Z_i=0,D_i^{obs}=1]}{\pi_c}.$$

Proof of Proposition 3 In order to bound $P_{c0}^{(11)}$, $P_{c0}^{(10)}$, $P_{c1}^{(11)}$, $P_{c1}^{(10)}$, $P_{n0}^{(11)}$, $P_{n0}^{(10)}$, $P_{a1}^{(11)}$ and $P_{a1}^{(10)}$, the relevant observable joint distributions are equal to the following:

$$P[\mathbf{Y}_i^{obs}=11|Z_i=0,D_i^{obs}=0]=\frac{\pi_c P_{c0}^{(11)}+\pi_n P_{n0}^{(11)}}{\pi_c+\pi_n}, \tag{46}$$

$$P[\mathbf{Y}_i^{obs}=11|Z_i=1,D_i^{obs}=1]=\frac{\pi_c P_{c1}^{(11)}+\pi_a P_{a1}^{(11)}}{\pi_c+\pi_a},$$

$$P[\mathbf{Y}_i^{obs}=10|Z_i=0,D_i^{obs}=0]=\frac{\pi_c P_{c0}^{(10)}+\pi_n P_{n0}^{(10)}}{\pi_c+\pi_n},$$

$$P[\mathbf{Y}_i^{obs}=10|Z_i=1,D_i^{obs}=1]=\frac{\pi_c P_{c1}^{(10)}+\pi_a P_{a1}^{(10)}}{\pi_c+\pi_a}.$$

Also, the following inequalities follow from the usual relationship between joint and marginal distributions:

$$P_{n0}^{(11)}\leq P_{n0}^{(\cdot 1)}=P[Y_{i2}^{obs}=1|Z_i=1,D_i^{obs}=0], \tag{47}$$

$$P_{c0}^{(11)}\leq P_{c0}^{(\cdot 1)}=$$

$$=\frac{P[Y_{i2}^{obs}=1|Z_i=0,D_i^{obs}=0](\pi_c+\pi_n)-\pi_n P[Y_{i2}^{obs}=1|Z_i=1,D_i^{obs}=0]}{\pi_c},$$

$$P_{a1}^{(11)}\leq P_{a1}^{(\cdot 1)}=P[Y_{i2}^{obs}=1|Z_i=0,D_i^{obs}=1],$$

$$P_{c1}^{(11)}\leq P_{c1}^{(\cdot 1)}=$$

$$\frac{P[Y_{i2}^{obs}=1|Z_i=1,D_i^{obs}=1](\pi_c+\pi_a)-\pi_a P[Y_{i2}^{obs}=1|Z_i=0,D_i^{obs}=1]}{\pi_c},$$

where the equalities follow from results in Proposition 2. Under these restrictions bounds are obtained by using the equalities in (46) and substituting the maximum and minimum values of relevant quantities in (47) as follows:

$$P_{c0}^{(11)}\geq\max\left(\frac{P[\mathbf{Y}_i^{obs}=11|Z_i=0,D_i^{obs}=0](\pi_c+\pi_n)}{\pi_c}-\frac{\pi_n}{\pi_c}P[Y_{i2}^{obs}=1|Z_i=1,D_i^{obs}=0],0\right)=L_{P_{c0}^{(11)}}$$

$$P_{c0}^{(11)}\leq\min\left(\frac{P[\mathbf{Y}_i^{obs}=11|Z_i=0,D_i^{obs}=0](\pi_c+\pi_n)}{\pi_c},\frac{P[Y_{i2}^{obs}=1|Z_i=0,D_i^{obs}=0](\pi_c+\pi_n)-\pi_n P[Y_{i2}^{obs}=1|Z_i=1,D_i^{obs}=0]}{\pi_c}\right)=U_{P_{c0}^{(11)}}$$

$$P_{c0}^{(10)}\geq\max\left(\frac{P[\mathbf{Y}_i^{obs}=10|Z_i=0,D_i^{obs}=0](\pi_c+\pi_n)}{\pi_c}-\frac{\pi_n}{\pi_c}P[Y_{i2}^{obs}=0|Z_i=1,D_i^{obs}=0],0\right)=L_{P_{c0}^{(10)}}$$

$$P_{c0}^{(10)}\leq\min\left(\frac{P[\mathbf{Y}_i^{obs}=10|Z_i=0,D_i^{obs}=0](\pi_c+\pi_n)}{\pi_c},\frac{P[Y_{i2}^{obs}=0|Z_i=0,D_i^{obs}=0](\pi_c+\pi_n)-\pi_n P[Y_{i2}^{obs}=0|Z_i=1,D_i^{obs}=0]}{\pi_c}\right)=U_{P_{c0}^{(10)}}$$

$$P_{c1}^{(11)}\geq\max\left(\frac{P[\mathbf{Y}_i^{obs}=11|Z_i=1,D_i^{obs}=1](\pi_c+\pi_a)}{\pi_c}-\frac{\pi_a}{\pi_c}P[Y_{i2}^{obs}=1|Z_i=0,D_i^{obs}=1],0\right)=L_{P_{c1}^{(11)}}$$

$$P_{c1}^{(11)}\leq\min\left(\frac{P[\mathbf{Y}_i^{obs}=11|Z_i=1,D_i^{obs}=1](\pi_c+\pi_a)}{\pi_c},\frac{P[Y_{i2}^{obs}=1|Z_i=1,D_i^{obs}=1](\pi_c+\pi_a)-\pi_a P[Y_{i2}^{obs}=1|Z_i=0,D_i^{obs}=1]}{\pi_c}\right)=U_{P_{c1}^{(11)}}$$

$$P_{c1}^{(10)}\geq\max\left(\frac{P[\mathbf{Y}_i^{obs}=10|Z_i=1,D_i^{obs}=1](\pi_c+\pi_a)}{\pi_c}-\frac{\pi_a}{\pi_c}P[Y_{i2}^{obs}=0|Z_i=0,D_i^{obs}=1],0\right)=L_{P_{c1}^{(10)}}$$

$$P_{c1}^{(10)}\leq\min\left(\frac{P[\mathbf{Y}_i^{obs}=10|Z_i=1,D_i^{obs}=1](\pi_c+\pi_a)}{\pi_c},\frac{P[Y_{i2}^{obs}=0|Z_i=1,D_i^{obs}=1](\pi_c+\pi_a)-\pi_a P[Y_{i2}^{obs}=0|Z_i=0,D_i^{obs}=1]}{\pi_c}\right)=U_{P_{c1}^{(10)}}.$$

$$P_{n0}^{(11)}\geq\max\left(\frac{P[\mathbf{Y}_i^{obs}=11|Z_i=0,D_i^{obs}=0](\pi_c+\pi_n)}{\pi_n}-\frac{\pi_c}{\pi_n}P[Y_{i2}^{obs}=1|Z_i=1,D_i^{obs}=0],0\right)=L_{P_{n0}^{(11)}}$$

$$P_{n0}^{(11)}\leq\min\left(\frac{P[\mathbf{Y}_i^{obs}=11|Z_i=0,D_i^{obs}=0](\pi_c+\pi_n)}{\pi_n},\frac{P[Y_{i2}^{obs}=1|Z_i=0,D_i^{obs}=0](\pi_c+\pi_n)-\pi_c P[Y_{i2}^{obs}=1|Z_i=1,D_i^{obs}=0]}{\pi_n}\right)=U_{P_{n0}^{(11)}}.$$

$$P_{n0}^{(10)}\geq\max\left(\frac{P[\mathbf{Y}_i^{obs}=10|Z_i=0,D_i^{obs}=0](\pi_c+\pi_n)}{\pi_n}-\frac{\pi_c}{\pi_n}P[Y_{i2}^{obs}=0|Z_i=1,D_i^{obs}=0],0\right)=L_{P_{n0}^{(10)}}$$

$$P_{n0}^{(10)}\leq\min\left(\frac{P[\mathbf{Y}_i^{obs}=10|Z_i=0,D_i^{obs}=0](\pi_c+\pi_n)}{\pi_n},\frac{P[Y_{i2}^{obs}=0|Z_i=0,D_i^{obs}=0](\pi_c+\pi_n)-\pi_c P[Y_{i2}^{obs}=0|Z_i=1,D_i^{obs}=0]}{\pi_n}\right)=U_{P_{n0}^{(10)}}$$

$$P_{a1}^{(11)}\geq\max\left(\frac{P[\mathbf{Y}_i^{obs}=11|Z_i=1,D_i^{obs}=1](\pi_c+\pi_a)}{\pi_a}-\frac{\pi_c}{\pi_a}P[Y_{i2}^{obs}=1|Z_i=0,D_i^{obs}=1],0\right)=L_{P_{a1}^{(11)}}$$

$$P_{a1}^{(11)} \leq \min\left(\frac{P[\mathbf{Y}_i^{obs}=11|Z_i=1,D_i^{obs}=1](\pi_c+\pi_a)}{\pi_a}, \frac{P[Y_{i2}^{obs}=1|Z_i=1,D_i^{obs}=1](\pi_c+\pi_a)-\pi_c P[Y_{i2}^{obs}=1|Z_i=0,D_i^{obs}=1]}{\pi_a}\right)=U_{P_{a1}^{(11)}}$$

$$P_{a1}^{(10)} \geq \max\left(\frac{P[\mathbf{Y}_i^{obs}=10|Z_i=1,D_i^{obs}=1](\pi_c+\pi_a)}{\pi_a} - \frac{\pi_c}{\pi_a}P[Y_{i2}^{obs}=0|Z_i=0,D_i^{obs}=1], 0\right)=L_{P_{a1}^{(10)}}$$

$$P_{a1}^{(10)} \leq \min\left(\frac{P[\mathbf{Y}_i^{obs}=10|Z_i=1,D_i^{obs}=1](\pi_c+\pi_a)}{\pi_a}, \frac{P[Y_{i2}^{obs}=0|Z_i=1,D_i^{obs}=1](\pi_c+\pi_a)-\pi_a P[Y_{i2}^{obs}=0|Z_i=0,D_i^{obs}=1]}{\pi_a}\right)=U_{P_{a1}^{(10)}}.$$

Proof of Proposition 4 Substituting (40) and (41) in the equalities in (46), we have the following system of four equations:

$$P[\mathbf{Y}_i^{obs}=11|Z_i=0,D_i^{obs}=0](\pi_c+\pi_n)=\pi_c P_{c0}^{(1\cdot)}P_{c0}^{(\cdot 1)}+\pi_n P_{n0}^{(1\cdot)}P_{n0}^{(\cdot 1)}, \tag{48}$$

$$P[\mathbf{Y}_i^{obs}=11|Z_i=1,D_i^{obs}=1](\pi_c+\pi_a)=\pi_c P_{c1}^{(1\cdot)}P_{c1}^{(\cdot 1)}+\pi_a P_{a1}^{(1\cdot)}P_{a1}^{(\cdot 1)},$$

$$P[\mathbf{Y}_i^{obs}=10|Z_i=0,D_i^{obs}=0)](\pi_n+\pi_c)=\pi_c P_{c0}^{(1\cdot)}(1-P_{c0}^{(\cdot 1)})+\pi_n P_{n0}^{(1\cdot)}(1-P_{n0}^{(\cdot 1)})=,$$

$$P[\mathbf{Y}_i^{obs}=10|Z_i=1,D_i^{obs}=1](\pi_c+\pi_a)=\pi_c P_{c1}^{(1\cdot)}(1-P_{c1}^{(\cdot 1)})+\pi_a P_{a1}^{(1\cdot)}(1-P_{a1}^{(\cdot 1)}).$$

Now, $P_{c0}^{(\cdot 1)}$, $P_{n0}^{(\cdot 1)}$, $P_{c1}^{(\cdot 1)}$ and $P_{a1}^{(\cdot 1)}$ are identified (see Proposition 2) so that the linear system (48) has only four unknowns $P_{c0}^{(1\cdot)}$, $P_{c1}^{(1\cdot)}$, $P_{n0}^{(1\cdot)}$, and $P_{a1}^{(1\cdot)}$, and can be solved, giving results in (42)-(45).

Tables

Table 1: Perfect prediction of compliance status: bounds collapse

Underlying true marginal distribution

	Compliers		Never-Takers	
	Y_1	Y_2	Y_1	Y_2
π	0.6		0.4	
$Z=0$	0.6	1	0.4	0
$Z=1$	0.4	1	0.2	0
True ITT	**-0.2**	0	**-0.2**	0

Implied observed distribution

Z	D	Y_1	Y_2
0	0	0.52	0.6
1	0	0.2	0
1	1	0.4	1

Underlying joint true distributions under Z = 0

Compliers ($\rho=0$)

Y_1 \ Y_2	1	0	
1	0.6	0	0.6
0	0.4	0	0.4
	1	0	1

Never-Takers ($\rho=0$)

Y_1 \ Y_2	1	0	
1	0	0.4	0.4
0	0	0.6	0.6
	0	1	1

Implied observed distribution under Z = 0

Y_1 \ Y_2	1	0	
1	0.36	0.16	0.52
0	0.24	0.24	0.48
	0.6	0.6	1

ITT_c estimated under ER	-0.33
Bounds on ITT_c without secondary outcome	$(-0.46; 0.2)$
Bounds on ITT_n without secondary outcome	$(-0.8; 0.2)$
Bounds on ITT_c with secondary outcome	$(\mathbf{-0.2}; \mathbf{-0.2})$
Bounds on ITT_n with secondary outcome	$(\mathbf{-0.2}; \mathbf{-0.2})$

Table 2: Association with the primary outcome

Underlying true marginal distribution

	Compliers		Never-Takers	
	Y_1	Y_2	Y_1	Y_2
π	0.6		0.4	
$Z = 0$	0.6	0.6	0.5	0.5
$Z = 1$	0.4	0.4	0.2	0.5
True ITT	**-0.2**	−0.2	**-0.3**	0

Implied observed distribution

Z	D	Y_1	Y_2
0	0	0.56	0.56
1	0	0.2	0.5
1	1	0.4	0.4

(a) Strong dependence: bounds identify sign of ITTc and ITTn

Underlying joint true distributions under $Z = 0$

Compliers ($\rho = 0.8$)

Y_1 \ Y_2	1	0	
1	0.55	0.05	0.6
0	0.05	0.35	0.4
	0.6	0.4	1

Never-Takers ($\rho = 0.4$)

Y_1 \ Y_2	1	0	
1	0.35	0.15	0.5
0	0.15	0.35	0.5
	0.5	0.5	1

Implied observed distribution under $Z = 0$

Y_1 \ Y_2	1	0	
1	0.47	0.09	0.56
0	0.09	0.35	0.44
	0.56	0.44	1

ITT_c estimated under ER	−0.4
Bounds on ITT_c without secondary outcome	$(-0.53; 0.13)$
Bounds on ITT_n without secondary outcome	$(-0.8; 0, 2)$
Bounds on ITT_c with secondary outcome	$(\mathbf{-0.35}; \mathbf{-0.05})$
Bounds on ITT_n with secondary outcome	$(\mathbf{-0.53}; \mathbf{-0.08})$

(b) Perfect dependence under Z=0: bounds collapse

Underlying joint true distributions under $Z = 0$

Compliers ($\rho = 1$)

Y_1 \ Y_2	1	0	
1	0.6	0	0.6
0	0	0.4	0.4
	0.6	0.4	1

Never-Takers ($\rho = 1$)

Y_1 \ Y_2	1	0	
1	0.5	0	0.5
0	0	0.5	0.5
	0.5	0.5	1

Implied observed distribution under $Z = 0$

Y_1 \ Y_2	1	0	
1	0.56	0	0.56
0	0	0.44	0.44
	0.56	0.44	1

Bounds on ITT_c with secondary outcome	$(\mathbf{-0.2}; \mathbf{-0.2})$
Bounds on ITT_n with secondary outcome	$(\mathbf{-0.3}; \mathbf{-0.3})$

Table 3: Latent independence

Underlying true marginal distribution as in Table 2

	Compliers		Never-Takers	
	Y_1	Y_2	Y_1	Y_2
π	0.6		0.4	
$Z = 0$	0.6	0.6	0.5	0.5
$Z = 1$	0.4	0.4	0.2	0.5
True ITT	**-0.2**	−0.2	**-0.3**	0

Implied observed distribution

Z	D	Y_1	Y_2
0	0	0.56	0.56
1	0	0.2	0.5
1	1	0.4	0.4

Latent Independence: bounds do not collapse but ITT identified

Underlying joint true distributions under $Z = 0$

Compliers ($\rho = 0$)

Y_1	Y_2: 1	Y_2: 0	
1	0.36	0.24	0.6
0	0.24	0.16	0.4
	0.6	0.4	1

Never-Takers ($\rho = 0$)

Y_1	Y_2: 1	Y_2: 0	
1	0.25	0.25	0.5
0	0.25	0.25	0.5
	0.5	0.5	1

Implied observed distribution under $Z = 0$

Y_1	Y_2: 1	Y_2: 0	
1	0.316	0.244	0.56
0	0.244	0.196	0.44
	0.56	0.44	1

ITT_c under LI assumptions	**−0.2**
ITT_n under LI assumptions	**−0.3**
Bounds on ITT_c with secondary outcome	(**−0.527**; **0.133**)
Bounds on ITT_n with secondary outcome	(**−0.8**; **0.19**)

Table 4: Breast Self-Examination trial

Observed Marginal distributions

Z	D	Y_1	X_1	X_2
0	0	0.795	0.573	0.640
1	0	0.474	0.474	0.680
1	1	0.897	0.697	0.660
π_c	0.71			
π_n	0.29			

Y_1= BSE practice

X_1=knowledge of breast pathophysiology, X_2=prior BSE practice

Observed joint distributions under $Z = 0$

($\rho = -0.37$)

Y_1	X_1: 1	X_1: 0	
1	0.38	0.415	0.795
0	0.193	0.012	0.205
	0.573	0.427	1

($\rho = 0.16$)

Y_1	X_2: 1	X_2: 0	
1	0.54	0.255	0.795
0	0.100	0.105	0.205
	0.64	0.36	1

ITT_c estimated under ER	−0.034
Bounds on ITT_c without auxiliary variables	(−0.103; 0.186)
Bounds on ITT_n without auxiliary variables	(−0.526; 0, 181)
Bounds on ITT_c with auxiliary covariate X_1	(**−0.024**; **0.186**)
Bounds on ITT_n with auxiliary covariate X_1	(**−0.526**; **−0.01**)
Bounds on ITT_c with auxiliary covariate X_2	(**−0.086**; **0.186**)
Bounds on ITT_n with auxiliary covariate X_2	(**−0.526**; **0.139**)

Table 5: Influenza Vaccine encouragement study

Observed Marginal distributions

Z	D	Y_1	X_1	X_2	X_3	X_4	X_5	X_6	X_7	X_8
0	0	0.088	0.27	0.47	0.004	0.66	0.01	0.56	0.28	0.66
1	0	0.083	0.26	0.46	0.003	0.70	0.02	0.57	0.27	0.66
1	1	0.068	0.32	0.53	0.002	0.64	0.01	0.60	0.28	0.67
0	1	0.112	0.39	0.51	0.004	0.63	0.02	0.59	0.30	0.60
						$\rho_{Y_1 X_j}$				
1	1		0.04	−0.08	−0.01	−0.10	0.05	−0.31	0.03	0.01
0	0		0.05	−0.28	−0.02	−0.002	0.06	−0.33	0.04	0.01
π_c	0.69									
π_n	0.12									
π_a	0.19									

Y_1= flue-related hospital visits, X_1=COPD, X_2=age above median age, X_3=liver disease
X_4=sex, X_5=renal disease, X_6=heart disease, X_7=diabetes, X_8=race

ITT_c estimated under ER	-0.120
Bounds on ITT_c without auxiliary variables	$(-0.606; 0.176)$
Bounds on ITT_n without auxiliary variables	$(-0.020; 0.083)$
Bounds on ITT_a without auxiliary variables	$(-0.112; -0.002)$
$\bigcap$ of bounds on ITT_c with auxiliary covariates	$(\mathbf{-0.515}; \mathbf{0.176})$
$\bigcap$ of bounds on ITT_n with auxiliary covariates	$(\mathbf{-0.020}; \mathbf{0.068})$
$\bigcap$ of bounds on ITT_a with auxiliary covariates	$(\mathbf{-0.111}; \mathbf{-0.002})$

Table 6: Job Corps study

Observed Marginal distributions

Z	D	Y_1	Y_2
0	0	0.44	0.40
1	0	0.45	0.41
1	1	0.35	0.32
π_c	0.72		
π_n	0.28		
$\rho_{Y_1 Y_2}$	0.91		

Y_1= employment, Y_2= earnings over the overall median earnings

ITT_c estimated under ER	-0.08
Bounds on ITT_c without auxiliary variables	$(-0.262; 0.126)$
Bounds on ITT_n without auxiliary variables	$(-0.548; 0.452)$
Bounds on ITT_c with auxiliary outcome Y_2	$(\mathbf{-0.103}; \mathbf{-0.043})$
Bounds on ITT_n with secondary outcome Y_2	$(\mathbf{-0.114}; \mathbf{0.041})$