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Working Paper 23840

<http://www.nber.org/papers/w23840>

NATIONAL BUREAU OF ECONOMIC RESEARCH

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September 2017

Basu acknowledges support from NIH research grants RC4CA155809 and R01CA155329. Coe acknowledges support from National Institute of Nursing Research grant NIH 1R01NR13583 (PI: Van Houtven). Chapman acknowledges support from SMT Inc. (PI: Schooley) and the Institute for Healthcare Improvement (PI: Cozad). We thank two anonymous reviewers for their very useful comments. Opinions expressed are ours and do not reflect those of the University of Washington or the NBER. All errors are our own. The views expressed herein are those of the authors and do not necessarily reflect the views of the National Bureau of Economic Research.

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Comparing 2SLS vs 2SRI for Binary Outcomes and Binary Exposures

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NBER Working Paper No. 23840

September 2017

IEL No. C26,110,118

ABSTRACT

This study uses Monte Carlo simulations to examine the ability of the two-stage least-squares (2SLS) estimator and two-stage residual inclusion (2SRI) estimators with varying forms of residuals to estimate the local average and population average treatment effect parameters in models with binary outcome, endogenous binary treatment, and single binary instrument. The rarity of the outcome and the treatment are varied across simulation scenarios. Results show that 2SLS generated consistent estimates of the LATE and biased estimates of the ATE across all scenarios. 2SRI approaches, in general, produce biased estimates of both LATE and ATE under all scenarios. 2SRI using generalized residuals minimizes the bias in ATE estimates. Use of 2SLS and 2SRI is illustrated in an empirical application estimating the effects of long-term care insurance on a variety of binary healthcare utilization outcomes among the near-elderly using the Health and Retirement Study.

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1. INTRODUCTION

Instrumental variables (IV) methods are used to obtain causal estimates of the effects of endogenous variables on outcomes using observational data. These methods mediate potential bias from unmeasured confounders affecting observed treatment through identifying and specifying an instrumental variable, which may represent a “natural experiment” affecting treatment through satisfying two principal assumptions: the instrument is sufficiently correlated with the endogenous variable (strength), and the instrument is uncorrelated with the error term in the outcome equation (validity). IV methods are usually implemented using a two-stage approach where the first-stage estimates an expectation of the endogenous variable conditional on measured confounders and one or more instrumental variables. The second stage model then predicts outcomes as a function of the estimated treatment values from the first-stage, measured confounders, and potentially other control variables.

In what has been popularly dubbed as the two-stage least-squares (2SLS) approach, the first and second stage models are parametrized using ordinary least squares regression, where the model fit is chosen through minimizing the sum of squared residuals from linear models. The 2SLS approach is a special case of the more general two-stage predictor substitution (2SPS) method, which follows the procedure described above but may apply alternative methods for estimating first- and second-stage models. Alternatively, one can obtain the residuals from the first stage regression and then run the second stage regression with the original endogenous variable, observed confounders and the residuals from the first stage as an added covariate. This approach, known as the two-stage residual inclusion (2SRI) approach, is analogous to the 2SLS approach when both first- and second-stage models are linear.

These estimation methods were originally derived in a linear setting with continuous endogenous treatments and continuous outcome measures but are often applied to what may be considered an inherently non-linear setting, such as with binary treatment or outcome measures. However, when treatment (exposure) or outcome is binary and therefore has a conditional expectation that follows a probability scale, a non-linear model featuring a convenient cumulative density function (CDF) is often used to model the conditional mean of the treatment indicator in the first-stage or outcome in the second-stage. Popular approaches include using probit or logit regression models.

However, complications arise when the outcome in the second stage is binary, and analysts consider using CDF-based non-linear models. It is well established that the 2SPS approach produces biased estimates of the population average treatment effect (ATE) in these scenarios (Blundell and Powell 2001; Terza et al. 2008). Under full parametric assumptions of joint-normality, bi-variate probit models can be used to model the two stages simultaneously (Bhattacharya et al. 2006).

Alternatively, it has been suggested that nonlinear 2SRI is the appropriate approach for estimation when first- or second-stage models have a dependent variable that is binary or otherwise suited for non-linear regression; especially when full parametric assumptions, where statistical joint distribution of error terms of the exposure and outcomes are specified, are not wanted (Blundell and Powell 2003, 2004; Terza et al. 2008). Nonlinear 2SRI methods identify the ATE through relying on the concepts that support control function methods (Blundell and Powell 2003, 2004), which were developed in the context of continuous endogenous variables. However, the applicability of nonlinear 2SRI to models with binary endogenous treatments remains contentious.

An important source of further complexity and potential confusion in comparisons of these estimates is that the specific treatment effect parameter identified by the 2SLS or 2SRI approaches may differ and depends on whether treatment effects are heterogeneous across the population and vary across levels of observed or unobserved confounders (aka essential heterogeneity). In such a situation, it is well-established that traditional IV approaches such as 2SLS identify an average treatment effect across only the subgroup of "marginal" individuals whose treatment choices were affected by changes in the specified instrumental variable(s) (Heckman 1997; Heckman et al. 2006; Basu et al. 2007). When the instrumental variable is binary (which is the focus of this paper), this effect is known as the local average treatment effect (LATE) (Imbens and Angrist 1994). Both 2SLS and the analogous strictly linear application of 2SRI will generate consistent estimates of LATE as long as the linear mean model specifications in both stages are correct.¹

Terza et al. (2007, 2008) claimed that nonlinear 2SRI, but not 2SLS or 2SPS, produced consistent estimates of ATE in models with inherently nonlinear dependent variables. However, it is not clear which treatment effect parameter is being estimated under a 2SRI

¹ The LATE effect is non-parametrically identified in a 2SLS setting within any cell defined by levels of all observed covariates X (Imbens and Angrist 1994). However, in a regression setting with many X 's, where a full saturated model is typically not used, the consistency of estimating LATE would rely on the appropriateness of the linear model specification.

approach for a binary treatment. Particularly in applications with binary IVs, the 2SRI approach relies on functional form assumptions for identification (as explained below) that are difficult to test in most applied setting and many analysts, especially economists, have favored the 2SLS approach regardless of whether treatment and outcome are continuous or binary. As such, many questions remain about the best approaches to IV estimation with such data. On the one hand, linear probability models may not provide a good fit to the data, especially when treatment or outcome variables are “rare” or otherwise imbalanced in nature, which in turn may lead to imprecise estimates. On the other hand, probit and logit models may provide a better fit to observed data overall but generate biased estimates depending on the support of the residual distribution (across all X’s).

For example, Chapman and Brooks showed that small changes to the simulation settings of Terza et al. (2007) resulted in different results and conclusions about the properties of 2SLS and 2SRI. They showed that 2SLS produced consistent estimates of LATE across alternative scenarios while 2SRI estimates were not consistent for either ATE or LAIE. However, the evidence produced by Chapman and Brooks is limited in that their scenarios all had treatment and outcome rates near 50%, a setting that may have inadvertently favored the 2SLS method.

Moreover, there is a debate in the health econometrics literature about the right form of the residual to be used in 2SRI approaches. Garrido et al. (2012) compared results from 2SRI models with different versions of residuals when applied to health expenditure data. They found that results varied widely depending on the type of residuals they use in the second stage. They raised the concern that raw residuals may not be the right control function variable. However, there is no theoretical rationale as to why different forms of the residual matter, nor did they do any simulations to show which one is better.

In this paper, we try to provide theoretical and empirical evidence to inform these debates.² We study a simple scenario with a binary outcome, a binary treatment that is made endogenous by a continuous unobserved confounder, binary instrument, and a binary measured confounder. After a theoretical discussion on the expected effects of alternative estimators, we study the properties of 2SLS and alternative 2SRI methods

² There are other forms of estimators that deal with a binary outcome and a binary endogenous treatment model, such as a GMM approaches (McCarthy and Tchernis 2011) and semi-parametric estimators (Abadie 2003; Abrevaya et al. 2009; Chiburis 2010; Shaikh and Vytlacil 2011). However, these estimators are not as popular as the 2SLS and the 2SRI approaches and so we do not cover them in this paper.

across a range of scenarios where the rarity of the treatment and/or the outcomes are varied using extensive Monte-Carlo simulation exercises.

Results show that the 2SLS method with binary IV produced consistent estimates of LATE across the entire range of rarity for either treatment or the outcome. The rarity of either did not affect the coverage probabilities of these estimators. In contrast, the 2SRI approach with any residuals studied was a biased estimator for LATE. In principle, nonlinear 2SRI estimators are designed to estimate the ATE parameter. However, 2SRI estimates of ATE were also generally biased, with the level of bias varying by residual form and outcome rarity. Amongst 2SRI models, those using generalized residuals were most often least biased in estimating ATE, though 2SRI with Anscombe residuals generated less biased estimates in scenarios with very rare outcomes (<5%). Implications of these results are discussed.

Finally, we examined the implications of model choice using an empirical setting that resembles the simulated scenario with endogenous binary treatment, binary outcomes, and binary observable confounders. The alternative instrumental variable methods were applied to evaluate the effect of long-term care insurance on a variety of health care utilization outcomes using tax treatment as an instrument for long-term care insurance holding, as has been validated in the literature (Goda 2011; Konetzka, et al. 2014; Coe, Goda and Van Houtven 2015). The results from applying the alternative estimators are discussed in the context of our simulation results.

]

2. ECONOMETRIC THEORY & METHODS

Consider the binary structural response model

$$y_i = 1\{y_i^* > 0\}, \quad (1)$$

where the latent variable y_i^* follows a linear model of the form

$$y_i^* = \mathbf{x}_i \boldsymbol{\beta} + u_i, \quad (2)$$

where \mathbf{x}_i is a row vector of covariates and u_i is a stochastic disturbance term for individual i . Throughout this section, bold-face is used to represent a vector. If u_i is independent of \mathbf{x}_i , a single index regression model such as:

$$E(y_i | \mathbf{x}_i) = G(\mathbf{x}_i \boldsymbol{\beta}) \quad G(a) = \Pr\{u_i > -a\} \quad (3)$$

can be used to obtain consistent estimates of $\boldsymbol{\beta}$. However, it may often be the case that u_i is not independent of \mathbf{x}_i because some component of \mathbf{x}_i , say d_i , is determined jointly with y_i such that

$$\mathbf{x}_i = (d_i, \mathbf{w}_i), y_i = 1\{d_i\beta_1 + \mathbf{w}_i\boldsymbol{\beta}_2 + u_i > 0\}, \text{ and } d_i \perp u_i, \quad (4)$$

where \perp indicates statistical independence. Let the reduced form of d_i , which we denote to be the *endogenous treatment variable*, be given as

$$\begin{aligned} d_i &= E(d_i | \mathbf{w}_i, \mathbf{z}_i) + v_i \\ &= \lambda(\mathbf{w}_i, \mathbf{z}_i) + v_i \end{aligned} \quad (5)$$

where \mathbf{z}_i = vector of instrumental variables, λ is the true function through which d_i is determined by \mathbf{w}_i and \mathbf{z}_i , v_i is a stochastic disturbance term, and $E(v_i | \mathbf{w}_i, \mathbf{z}_i) = 0$ by construction. It is assumed throughout that expectation of d is a non-trivial function of \mathbf{z} given \mathbf{w} .

For evaluation research, interest generally lies in estimating $\boldsymbol{\beta}$ parameters or, more specifically, the components of $\boldsymbol{\beta}$ that represent the causal effect of an exogenous shift in treatment, d_i , on the response probabilities. The interpretation of those parameters of interest then must be considered. The broadest and perhaps most intuitive treatment effect parameter is the *average treatment effect* (ATE), which represents the mean change in an outcome that would be realized if everyone in a target population changed from not receiving treatment to receiving treatment. The ATE can be written as

$$\begin{aligned} ATE(\mathbf{w}) &= \int_{u \in U | \mathbf{w}} \{E(y_i | \mathbf{w}_i, u_i, d_i = 1) - E(y_i | \mathbf{w}_i, u_i, d_i = 0)\} \cdot dF(u | \mathbf{w}) \\ &= G(\beta_1 + \mathbf{w}_i \boldsymbol{\beta}_w) - G(\mathbf{w}_i \boldsymbol{\beta}_w) \end{aligned} \quad (6)$$

where $ATE(\mathbf{w})$ represents the *conditional* average treatment effect for a sample, which may be distinct in the mix of characteristics \mathbf{w} .

If it is the case that treatment effects are heterogenous across the population and this heterogeneity is related to treatment choice (i.e., essential heterogeneity), then treatment effectiveness will vary over levels of u_i when components of \mathbf{w} are unmeasured by the researcher (i.e., there are unmeasured confounders). As a result, identification of ATE will

require strong assumptions. First, the ATE can be estimated through identification of the function represented by $G(\cdot)$, which is to akin to identifying the full parametric distribution of u_i . In the absence of full parametric assumptions, the ATE can be identified in special cases using instrumental variables methods, where the specified IV(s) fully identify the conditional distribution of $u_i \mid v_i$, which can then be integrated over the distribution of v_i identified in the IV-based first-stage model. More simply put, the specified IV(s) must be considered as potentially influencing treatment choice for all types of individuals in the sample, defined by their levels of observed and unobserved characteristics. These IV assumptions may be particularly difficult to satisfy when a single binary instrument is used, as only two points of support in the distribution of v_i are identified non-parametrically.

More generally, as Imbens and Angrist (1994) have shown, the IV effect estimated using a single binary IV, z_i , is referred to as the *local average treatment effect (LATE)* and is given as:

$$LATE(\mathbf{w}) = (E(y_i \mid \mathbf{w}_i, z_i=1) - E(y_i \mid \mathbf{w}_i, z_i=0)) / (E(d_i \mid \mathbf{w}_i, z_i=1) - E(d_i \mid \mathbf{w}_i, z_i=0)) \quad (/)$$

The LATE reflects the average causal effect of d_i on the probability of y_i among those (marginal) individuals whose treatment statuses would likely change with a change in the level of the instrumental variable (Angrist & Imbens 1994, 1996; Heckman 1997). The LATE parameter is only “locally” interpretable in the context of the instrument specified. Even with very strong instruments that lead all patients in the sample to be marginal, LATE will not often converge to the ATE because, unlike randomization, the instrument may put more weight on some marginal patient than others. Therefore, since it is often difficult to identify the marginal patients directly (i.e., to know for whom the instrument affected choice), it may also be difficult to understand to whom the estimate applies (Heckman 1997; Newhouse and McClellan, 1998). In some cases where a binary IV is related to a specific policy, LATE may be interpretable as the effect of changing d_i among those individuals who would be induced to change their treatment status by the policy (Heckman et al. 2006). Naturally, if the true treatment effect is constant then the true LATE and ATE are the same.

The following discussion focuses on three popular approaches for estimation of mean effects on response probabilities from an instrument-driven exogenous shift in the treatment d_i : the fully parametric bivariate probit (BVP) model, the semi-parametric residual inclusion (2SRI) approach, and the linear two-stage least squares (2SLS) approach. Each of these methods employs different assumptions and attempt to identify different parameters. In fact, Chiburis et al. (2012) have argued that many of the documented

differences in the treatment effect estimates from 2SLS and bi-variate probit models in the literature may be driven by the fact that they are estimating different parameters, to begin with. We now look at these estimators in detail.

□

2.1 Approach 1 (Fully parametric): e.g. Bivariate-Probit

If the joint distribution of the structural error term u_i and the reduced form error term v_i were parametrically specified (e.g., Gaussian), and $\lambda(\mathbf{w}_i, z_i)$ is parametrically specified, then under some normalization of the $\text{Var}(u_i)$ (Blundell and Smith 1986),

$$\begin{aligned} E(y_i \mid d_i, \mathbf{w}_i, v_i) &= \Pr(u_i > -d_i\beta_1 - \mathbf{w}_i\beta_2 \mid v_i) \\ &= \Phi(d_i\beta_1 + \mathbf{w}_i\beta_2 + \rho v_i), \end{aligned} \quad (8)$$

where ρ is the vector of population regression coefficients of u_i on v_i . The parameters β , $\lambda(\cdot)$ and ρ can be estimated using maximum likelihood estimation. When both y_i and d_i are binary, this approach can be implemented using a bivariate probit regression (Heckman 1978). However, bivariate probit models can be sensitive to heteroscedasticity and are usually more robust when treatment probabilities approach 0 or 1 (Chiburis et al. 2012). If the underlying distributions are correctly specified, this method structurally recovers the average treatment effect (ATE) parameter since $u_i \mid v_i$, identified through the IV, is structurally linked to u_i through the parametric assumption.

The sample analog for the population treatment effect parameter identified by this approach is given by:

$$E_{\mathbf{w}}\{E_{\hat{v}}\{\Phi(1 \cdot \hat{\beta}_1 + \mathbf{w}_i\hat{\beta}_2 + \hat{\rho} \cdot \hat{v}_i) - \Phi(0 \cdot \hat{\beta}_1 + \mathbf{w}_i\hat{\beta}_2 + \hat{\rho} \cdot \hat{v}_i)\}\}, \quad (9)$$

where $\hat{\cdot}$ indicates that these quantities have been estimated from the data at hand.

□

2.2 Approach 2 (Semi-parametric): e.g. 2SRI

The semi-parametric approach uses estimates of the reduced form error term, v_i , to control for endogeneity of d_i in the outcomes structural model (Blundell and Powell 2004). The identification of β_1 and the distribution functions of the error term, u_i , is through

distributional exclusion restrictions, the first of which requires that the dependence of u_i on each of d_i , \mathbf{w}_i and \mathbf{z}_i are completely characterized by the reduced form error vector v_i :

$$u_i \mid d_i, \mathbf{w}_i, \mathbf{z}_i \sim u_i \mid d_i, \mathbf{w}_i, v_i$$

$$\sim u_i \mid v_i \quad (10)$$

Under this assumption,

$$E(y_i \mid d_i, \mathbf{w}_i, v_i) = \Pr[u_i \leq -d_i\beta_1 - \mathbf{w}_i\beta_2 \mid d_i, \mathbf{w}_i, v_i]$$

$$= F(d_i\beta_1 + \mathbf{w}_i\beta_2 \mid v_i). \quad (11)$$

where $F(\cdot)$ is the conditional c.d.f. of $-u_i$ given v_i .

The marginal distribution function $G(\cdot)$ with respect to $-u_i$ could be identified using a control function approach such as (Blundell and Powell 2004):

$$G(d_i\beta_1 + \mathbf{w}_i\beta_2) = \int F(d_i\beta_1 + \mathbf{w}_i\beta_2, v_1) H_V, \quad (12)$$

where H_V is the distribution function of v . Consequently, ATE can be identified using (6).

Note that, unlike the fully parametric approach, one can be agnostic about the parametric distribution of u_i and v_i as long as the distributional exclusion criterion is met. However, Blundell and Powell's (2003) identification relies on **a continuous** v_i . Moreover, the identification of ATE relies on the fact that the error term in the outcomes model is additively separable. These conditions allow for a counterfactual to be determined without the need for any additional functional form assumptions given that the β are consistently estimated. However, in non-linear models, such as those in (2), these counterfactuals inherently depend on the functional form assumption of the control function.

For example, in practice, this approach is implemented through "residual inclusion", which follows estimating the error term in the first-stage regression and then including these estimated residuals as a covariate in the second-stage outcomes regression. A recycled predictions approach can then be used to recover the marginal effect of d_i on $E(y_i)$.

However, when implementing this approach for a binary treatment variable, the residuals from the first stage would always be positive for treatment recipients and negative for non-recipients. Hence, in a non-linear outcomes model, the conditional treatment effect, conditional on any level of the *estimated* v_i (say, \hat{v}_i), must be obtained via extrapolation.

Figure 1 illustrates this idea for a group of individuals with the same \mathbf{w}_i , which is kept implicit, but different values of z_i . Suppose the residuals among treatment recipients are 0.1, 0.2, 0.3, 0.4, 0.7 and those among non-recipients are -0.1, -0.2, -0.3, -0.4, -0.7. Conditional on a positive level of the residual v_i+ , $E(y_i | d=1, \hat{v}_i+) = E(y_i^1 | \hat{v}_i+)$ is obtained from the data where y_i^1 is the potential outcome under treatment. However, the counterfactual outcome, i.e. the corresponding potential outcome y_i^0 under no treatment, is not observed in the data as there are no non-recipients that have a positive level of the residual by construction. The counterfactual outcome has to be obtained via extrapolation of the functional specification of $F()$, which in turn determines the estimate for β_1 . Figure 1(a) illustrates this extrapolation. The overall treatment effect is then obtained by averaging the conditional treatment effects obtained over the distribution of \hat{v}_i .

Symmetry in the distribution of \hat{v}_i , to the extent that it can be attained, can facilitate this extrapolation. Most forms of residuals used in non-linear settings attempt to mimic a normal distribution. Alternate forms of residuals, such as standardized, deviance, Anscombe, and generalized (Gourieroux et.al., 1987), may also be used in the residual inclusion approach and have been explored (Garrido et al. 2012). When estimated by a nonlinear approach, such as probit or logit, raw-scale residuals for a binary treatment variable will always lie between 0 and 1 in absolute values. Therefore, each type of residual transformation is likely to spread the support of the residual distribution on the real line. For example, if predicted $\Pr(d|z) = 0.4$ and 0.7 for two observations with $d = 1$, then the raw-scale residuals will be 0.6 and 0.3 respectively, but the standardized residuals ($= (d - \hat{p}(z)) / \sqrt{\hat{p}(z)(1 - \hat{p}(z))}$) will be 1.22 and 0.65 respectively. Consequently, standardized residuals may provide a better fit to the outcomes data and increase the robustness of extrapolations. For example, when the treatment is rare, the raw-scale residuals on either the negative or the positive side are likely to be far away from zero. Transformation can help these residuals to spread out, so as to increase accuracy when estimating the functional form of the outcome conditional on these residuals. A priori, it is difficult to predict what form of residuals from a binary treatment model would best approximate the non-separable error term in the outcomes equation.

It is worth reiterating that a central problem, beyond the issue of non-overlap in support of \hat{v}_i as discussed above, when the instrumental variable is also binary is that only two points on the support of \hat{v}_i are identified for any level of \mathbf{w} . Model fit and extrapolation is based only on those two points in the support for \hat{v}_i .

□

2.3 Approach 3 (Non-parametric): e.g. 2SLS

Distinct from BVP and 2SRI approaches discussed above, which are designed to identify the ATE, a 2SLS approach is designed to estimate the LATE parameter. A 2SLS approach attempts to estimate the LATE from the data non-parametrically by estimating the slope of outcomes and exposure, conditional on the instrument. In the case of a single binary instrument, this slope is based upon the two points of support identified by the two levels of the instrument. That is, it plugs in the sample analogs of the numerator and the denominator in the LATE parameter defined above. However, this process assumes that the mean outcomes and the exposure models are linear in terms of \mathbf{w} .³ When one or both of these linear specifications are violated, 2SLS may be a biased estimator for the outcome probabilities (Horace and Oaxaca 2006). While this could, in turn, induce bias in the estimation of LATE, some have suggested that risk of such bias is minimal in many applied settings and concerns are exaggerated. (Angrist, Fernandez-Val 2001)

The 2SLS approach of linear IV models can be viewed as a special case of control function methods (Teisler 1964), where both first and second stage regressions are linear. However, since 2SLS approaches rely only on mean-independence requirements, and not on the full conditional independence of the distribution as in (8), demands the “correct” specification of the first-stage to provide consistent estimates of the second-stage parameters (Blundell and Powell, 2004). However, this requirement seems to apply mostly for the estimation of ATE; as the LATE value is not necessarily equivalent or determined by the true structural parameters under essential heterogeneity. It is unclear how violation of this requirement affects the estimation of LATE. We expect that for a binary treatment in the first stage, a linear approximation of the conditional mean is likely to be most appropriate when the mean treatment is close to 50%. Chapman and Brooks (2016) demonstrates that this is the case through their simulations.

These discussions establish the rationale for the simulations in this paper. It is conjectured that 2SRI approach applied to binary endogenous variables can produce biased results when extrapolations are not appropriate. Alternative versions of the residuals could improve the performance of 2SRI approaches through mutating the scale of the residual distribution used, which could influence the estimation of the underlying structural functions through the 2SRI approach as was observed in Garrido et al. (2012). Second,

³ There can certainly be a more elaborate model building exercise that can overcome this problem, but such exercises are seldom found in the economics and health economics literature. In any case, such exercises typically lead one away from a simple linear model into the realm of non-linear models.

when the endogenous binary variable becomes rare, the linear model specification in the first-stage could break down, resulting in a biased estimation of second-stage parameters in the 2SLS approach. These biases could then compound biases from misfit of the linear model to rare outcomes in the second-stage.

□

3. SIMULATIONS

We consider the simplest case where we have a binary outcome (y_i), a binary treatment (d_i), three binary controls (w_i) and a binary instrument (z_i). We chose three binary controls so that the residuals from the first stage regression have at least thirty unique values in their support. The central questions we try to answer with these simulations are: Can linear approximation (2SLS) provide consistent estimates of the LAIE for a binary outcome/binary endogenous variable model? What form of residuals are most suited to a correctly specified nonlinear 2SRI (Probit-Probit) approach? How do the results change if outcomes (y_i) and/or treatment (d_i) become rare?

The data generating processes (DGPs) are described below (subscripts i are suppressed for clarity).

3.1 Exposure (treatment) DGP

$$d^* = \alpha_0 + \alpha_1 \cdot w_1 + \alpha_2 \cdot w_2 + \alpha_3 \cdot w_3 + \alpha_7 \cdot z + (\alpha_{11} \cdot w_{11} - \omega), \quad (13)$$

where $(\alpha_1, \alpha_2, \alpha_3) = (0.5, 1, 2)$, $\alpha_{11} = 1$, $\alpha_7 = 1$. Observed variables w_1, w_2, w_3 and z are all binary variables with mean equal to 0.5, generated by dichotomizing standard normal variables around the value of 0. Together, $(\alpha_{11} \cdot w_{11} - \omega)$ represents the empirical error term for the treatment model and consists of the binary unobserved confounder, w_{11} , which is also based on dichotomizing a Normal $(0,1)$, and the continuous model disturbance term, $\omega \sim \text{Normal}(0,1)$. Observed treatment, d , is derived from the index function ($d^* > 0$) and $\Pr(d) \equiv \Phi((\alpha_0 + 2.25)/\sqrt{3.5625})$. We vary the model intercept, α_0 , to take on values of -2, -1.25, -0.3, 0.5, and 1.5 which correspond to $\Pr(d) = 0.55, 0.70, 0.85, 0.93$, and 0.995 respectively.

3.2 Outcomes DGP

$$y^* = \beta_0 + \beta_D \cdot d + \beta_1 \cdot w_1 + \beta_2 \cdot w_2 + \beta_3 \cdot w_3 + (\beta_{11} \cdot w_{11} - \varepsilon) \quad (14)$$

Together $(\beta_{01} \cdot w_{01} - \varepsilon)$ represents the empirical error term, u , from the theoretical outcomes model under Section 2. Across all simulation models, true values of coefficients $(\beta_1, \beta_2, \beta_3)$ were set to (1,1,1), the coefficient for the unmeasured confounder, β_{01} , was set to 2, and coefficient on treatment, β_D , was set to 1. The model disturbance term $\varepsilon \sim \text{Normal}(0,1)$ and $\Pr(y|d) = \Phi((\beta_0 + \beta_D \cdot d + 1.5)/\sqrt{5.75})$. We vary β_0 across simulations to take on values of -2, 0.5, 1.5, and 2.5 which correspond to $\Pr(y) = 0.51, 0.82, 0.93$ and 0.96 respectively.

]

3.3 Target parameters

The primary target parameters were the ATE and the LATE. True values for the ATE and LATE concepts were calculated in each simulation as:

$$\text{ATE} = E(y|d=1) - E(y|d=0) = \Phi((\beta_0 + 2.5)/\sqrt{5.75}) - \Phi((\beta_0 + 1.5)/\sqrt{5.75}) \quad (15)$$

$$\text{LATE} = E_w\{[E(y|z=1, \mathbf{w}) - E(y|z=0, \mathbf{w})] / [E(d|z=1, \mathbf{w}) - E(d|z=0, \mathbf{w})]\} \quad (16)$$

where $\mathbf{w} = (w_1, w_2, w_3, w_{01})$. The true value of the LATE parameter was simulated based on 100 samples of 1 million observations each.

3.4 Simulations

Estimates were generated using Monte-Carlo simulation methods, using 1,000 samples of 50,000 observations each to mitigate finite sample issues and also to align our simulation with our empirical example. For each of the 1,000 simulated samples, 500 bootstrap re-samples were drawn and used to calculate standard error and coverage values. Percent bias was calculated as $(\hat{\Delta}_k - \text{LATE}) \cdot 100 / \text{LATE}$ or $(\hat{\Delta}_k - \text{ATE}) \cdot 100 / \text{ATE}$ averaged over all simulated samples, where $\hat{\Delta}_k$ is the estimated treatment effect for sample k . The coefficient of variation is based on the standard deviation of the mean estimates across the 1,000 Monte-Carlo samples divided by the average of the mean estimates from those samples. Finally, coverage probabilities for LATE and ATE were determined by averaging $I((\hat{\Delta}_k - 1.96 \cdot \widehat{SE}_{E_k}) \leq \text{LATE} \leq (\hat{\Delta}_k + 1.96 \cdot \widehat{SE}_{E_k}))$ and $I((\hat{\Delta}_k - 1.96 \cdot \widehat{SE}_{E_k}) \leq \text{ATE} \leq (\hat{\Delta}_k + 1.96 \cdot \widehat{SE}_{E_k}))$, respectively, across all 1,000 samples, where $I()$ is an indicator function and \widehat{SE}_{E_k} is the sample-specific standard error obtained via bootstrap.

Simulations were repeated using a sample size of 5,000 to magnify any finite sample issues, and those results are presented in the appendix.

3.5 Estimators

We compared the following estimators:

1) IV regression with LPM (2SLS)

2) Probit-Probit 2SRI with

a) raw residuals as $(d_i - \hat{d}_i)$,

b) standardized (Pearson) residuals given by $(d_i - \hat{d}_i) / \sqrt{\{(1 - \hat{d}_i) \hat{d}_i\}}$,

c) deviance residuals, given by $\sqrt{2 \{y_i \log(\frac{d_i}{\hat{d}_i}) + (1 - d_i) \log(\frac{1 - d_i}{1 - \hat{d}_i})\}}$ and

d) Anscombe residuals, $(A(d_i) - A(\hat{d}_i)) / [A'(\hat{d}_i) \sqrt{\{(d - \hat{d}_i) \hat{d}_i\}}]$, where $A(d_i) = (B(d_i, \frac{2}{3}, \frac{2}{3}) - B(\hat{d}_i, \frac{2}{3}, \frac{2}{3})) / \sqrt{\{(1 - \hat{d}_i) \hat{d}_i\}}$ and $B()$ is a Beta Function.

e) Generalized residuals (Gourieroux et al. 1987): $\hat{d}_i \cdot (d - \hat{d}_i) / \{(1 - \hat{d}_i) \hat{d}_i\}$

3) Bi-variate probit regression model, which is the MLE for the DGPs.

3.6 Results

Descriptive statistics for our DGPs are provided in Table 1. As expected, the true mean average treatment effect (ATE) parameter values varied across scenarios varying the intercept in the outcome models, β_0 , but not across scenarios varying the intercept in the treatment models. LATE, however, varies with the intercepts in both the outcome and treatment choice models. As outcomes become rare, following an underlying probit model, both ATE and LATE decrease.

Simulation results are presented in Tables 2 and 3. Table 2 reports percent bias, the coefficient of variation, and coverage probabilities on the LATE. We find that 2SLS always provides consistent estimates of LATE, irrespective of the treatment rarity or outcomes rarity. This indicates that 2SLS can consistently estimate the LATE effect even if the linear probability model misfits the data and produces out of range predictions. Results do not

show any major drop in coverage probabilities for LAIE across simulation design points. Estimates from nonlinear 2SRI and bi-variate probit were generally biased for the LAIE.

Table 3 reports percent bias, the coefficient of variation and coverage probabilities on the AIE. As expected, given the DGPs, bi-variate probit always produced the least biased estimates of the AIE. Also as expected, 2SLS produced biased estimates of AIE, especially as the AIE and LAIE became increasingly distinct in value with rarer treatment and outcome. Results showed that all of the 2SRI estimators produced substantially larger biases (and poor coverage probabilities) than bi-variate probit in estimating AIE. This highlights the difficulty of estimating the AIE through extrapolation using the first-stage residuals. Among the residual inclusion approaches, 2SRI with generalized residual appeared to have the least bias in estimating AIE in most cases. However, the corresponding coverage probabilities were low.

One interesting observation was that, for rare outcomes (such as those below 5%), 2SRI with Anscombe residuals produced the least bias in estimating AIE, with coverage probabilities close to 95% in each case. The coverage probabilities did not deteriorate when treatment also became rare. This may indicate that the Anscombe transformation of the first-stage residuals are helping to approximate better the distribution of $u_i|v_i$ where the outcomes are rare and, therefore, abetting the extrapolation for the counterfactuals.

Results for patterns of bias with 2SLS and 2SRI held similar for the simulations with a sample size of 5000 (Appendix Tables A2 and A3).

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4. EMPIRICAL EXAMPLE

To illustrate the potential impact of the estimation method on empirical results, we use the case of long-term care insurance (LTCI) and its impact on long-term care (LTC) utilization. This issue has been studied by Konetzka, He, Guo and Nyman (2014) and Coe, Goda and Van Houtven (2015). This application is fitting to illustrate the concepts examined in the simulation models, as it is characterized by: 1) a relatively low $E(Y)$ -- few elderly hold long-term care insurance; 2) an empirically strong and widely accepted instrumental variable -- state tax policies that reduce the cost of insurance influence LTCI holding; and 3) multiple outcomes, at varying means $\Pr(Y)$.

4.1 Data

Three main data sources were used, following Coe, Goda and Van Houtven (2015): (1) the Health and Retirement Study (HRS) (including RAND versions) (<http://hrsonline.isr.umich.edu/>); (2) the HRS restricted geographic identifiers (HRS/G), in order to match the individual to the state of residence, and (3) state-level tax subsidy data for the purchase and holding of state-approved LTCI policies (GS Goda, 2011).

Data from ten waves of the HRS (1996-2010), a publicly available, bi-annual survey of the near elderly in the U.S. were used.⁴ Respondents were ages 50 and older when they initially entered the sample, and many respondents are observed long enough to have used some type of long-term care. To increase the relevance of the instrumental variable used for analysis – the state tax subsidy – the sample was limited to individuals who report filing taxes and individuals in the top half of the income distribution in our sample. The sample size consisted of 46,639 individual-wave observations. The *Cross-Wave Geographic Information (State)* file matches respondents to their state of residence, which is then matched to hand-collected data from individual state income tax return forms from 1996-2010 that describe tax subsidy programs for private long-term care insurance.

4.2 Measures and Descriptive Statistics

Five binary outcome measures were created; the measures had varying means to illustrate the bias due to the estimation methods. Each outcome measure is created from HRS data one wave (approximately two years) ahead of the data used to create explanatory measures described below. Descriptive statistics for the data are shown in Table 3.

Informal Helper: Defining informal care in the HRS requires an algorithm based on several variables. The process first identifies whether the person received care for specific IADLS and ADLS and then uses information from relationship codes measured in the helper file to determine whether the care was from a child, a friend or another relative to ensure that the care recipient was not paid. We create 3 variables based on who provided the informal care: 60 percent of the sample receives informal care from any person; 43 percent receive informal care from a child; 16.5 percent receive care from other relatives.

Home Health care: The formal home health care variables are: "Since the previous interview, has any medically-trained person come to your home to help you, yourself?" In 2000, the HRS clarified that medically-trained persons include professional nurses, visiting

⁴ Earlier waves of the survey are omitted because of the lower quality information on the LTCI question (Finkelstein and McGarry, 2006) and state information is not yet available for later waves.

nurse's aides, physical or occupational therapists, chemotherapists, and respiratory oxygen therapists, which may represent an expansion of the definition of home health care. 6.8 percent received home health care.

Nursing home care: The HRS asks: "Since (Previous Wave Interview Month-Year/In the last two years), have you been a patient overnight in a nursing home, convalescent home, or other long-term health care facility?" For individuals who died between waves, nursing home use was measured from data in the HRS exit interviews. 2.3 percent received nursing home care.

LTCL (mean=0.157): Starting in the 1996 wave, respondents were asked to respond yes or no to the following question: "Not including government programs, do you now have any long term care insurance which specifically covers nursing home care for a year or more or any part of personal or medical care in your home?". LTCL status is defined as having LTCL in year t , based on the recorded response to this question; 15.7 percent of individual-waves had long-term care insurance.

State Tax Subsidy (an instrument for LTCL): Following the literature, a binary variable indicating whether a state has a tax subsidy available in a particular year was created to be used as an instrument for LTCL. The state tax subsidy indicated any subsidy, regardless of the form of the subsidy (i.e., credit or a deduction), the fraction of premiums eligible, monetary caps on the value of the subsidy, income limits, or whether the state subsidy was available in addition to the federal subsidy (GS Goda, 2011; Konetzka et al. 2014; Coe, Goda and Van Houtven 2015). The availability of a state tax subsidy varied considerably over time and across states; while only three states had tax incentives for LTCL in 1996, a total of 24 states plus the District of Columbia had adopted a subsidy by 2008. Prior literature has provided evidence that the state tax subsidy is empirically important in whether someone holds a LTCL policy and meets essential criteria for use as an instrumental variable in this context. In the first stage regression, the estimated coefficient on the binary state tax subsidy variable suggested that individuals in states with subsidies are about three percentage points more likely to own LTCL (F-stat: 65.93, $p < 0.001$).

Individual-level control variables. Control variables in the models included binary variables indicating respondent's marital status, sex, number of children, retirement status, education, income, race, ethnicity, health status (fair or poor self-reported health and the presence of any limitations in the activities of daily living (ADLs)), and age fixed effects.

Fixed-effects. All models include the year and state fixed-effects. The year fixed-effects account for time trends in the data while the state fixed-effects account for non-time-varying differences across states. The inclusion of state fixed-effects suggests that the empirical models identify the effect of LTCI coverage on the outcome for individuals whose LTCI coverage was sensitive to within-state differences in the state tax policy.

Analyses included the use of all estimators represented in the simulations models described in the previous section. Each estimator was used to estimate the effect of long-term care insurance on each of the five outcomes described above, using the binary state tax subsidy variable as an instrumental variable. For each estimator, estimates from 500 clustered bootstrap samples were used to compute standard errors for the marginal effect in each case.

□

4.3 Results

The simulation results indicated that 2SLS should produce consistent estimates of LAIEs, regardless of treatment or outcome rarity. Conversely, results suggested 2SRI models were likely to produce bias in estimating average treatment effects on outcomes (ATE or LAIE), with generalized residuals estimator (2SRI-Gres) producing the least bias. For very rare outcome, such as nursing home care and home health care in our empirical application, 2SRI with Anscombe residual (2SRI-ares) may produce estimates close to the unbiased estimates of ATE.

Table 4 provides summary statistics for outcomes and other variables used in the empirical models. The marginal effects and their bootstrapped standard errors are shown in Table 5.

The 2SLS-based consistent LAIE estimates for LTCI were -0.302 (Informal care from any source), -0.329 (Informal care from child), 0.161 (Informal care from relatives), -0.252 (home health care), and 0.087 (Any nursing home care). The interpretation of LAIE always refers to the marginal individuals. For example, in the model predicting informal care from any source, the LAIE estimate suggests that LTCI decreases the use of informal care from any source by 30 percentage points among people who are moved to acquire LTCI due to the subsidy. Sometimes, LAIE can provide treatment effects estimates that are difficult to interpret, and may even be considered nonsensical, even when the IV is policy-driven. For example, assuming that access to LTCI would increase receipt of formal care, which will act

as a substitute for all forms of informal care, the effect of LTCI on Informal care from any source would perhaps not be expected to be smaller than the effect on Informal care from child, yet that is what LAIE suggests. Similarly, it is difficult to envision how the effect from having LTCI, for those who have insurance due to state subsidies, increases informal care from a relative; though this LAIE estimate does not reach statistical significance. One may invoke complicated stories about complementarity between formal care and informal care from relatives and particularities about the generosity of LTCI for those who have it due to state subsidies, to explain these result. Then again, the real world is full such complexities and taking the time to disentangle such nuanced relationships may be considered worthwhile. Note that the LAIEs for different outcomes belong to the same marginal group of patients who are influenced by this specific IV.

Treatment effect estimates produced from the 2SRI models are often quite different from the 2SLS-based LAIE estimates. This was expected. The 2SRI-Gres estimates of ATE for LTCI are -0.268 (Informal care from any source), -0.179 (Informal care from child), -0.111 (Informal care from relatives), -0.077 (home health care) and 0.023 (Any nursing home care). Taken at face value, these estimates did not have the contextual inconsistencies, as it relates to our a priori theory about the relationships under study, which were seen in LAIE estimates. The 2SRI estimates were also quite similar to those produced by the Bi-Probit model, especially when outcomes mean was close to 0.50. It is quite plausible that the underlying distribution of outcomes is well approximated by a normal distribution when the binary outcome mean is close to 0.50, and hence, for these outcomes, the bi-probit model is likely to produce consistent estimates of ATE.⁵ For rarer outcomes, the bi-probit estimates and the 2SRI-gres estimates differ and it is not clear if any of those estimates are unbiased estimates of ATE.

For any nursing home care, which is the rarest outcome, 2SRI-gres (with Anscombe residuals) estimates of ATE are close to being unbiased, according to our simulations. Although this point estimate of 0.038 differs from that of Bi-probit ($\hat{=}$ 0.023), neither reach statistical significance. Hence, it is reasonable to conclude that the overall average effect of LTCI in the entire population does not significantly affect any nursing home care.

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⁵ Note that in contrast to our simulations, where we generate all outcomes under the normal distribution and found the BVP perform better for rare outcomes, here we are suggesting that when the outcomes mean is around 50% its underlying data-generating process is more likely to be normal.

5. CONCLUSIONS

The economics literature is teeming with applications where linear probability models are used for binary outcomes. In case of instrumental variables methods, both the binary treatment (in 1st stage) and the binary outcome (in 2nd stage) are often modeled with linear probability models with two-stage least squares (2SLS) estimators. In contrast, a control function approach may be used with non-linear models (e.g., probit or logit applied to first and/or second stage models) where the estimated residuals from the first stage are used as an additional covariate in the second stage. However, the residual inclusion approach does not identify a treatment effect non-parametrically. Instead, it relies on extrapolation for the counterfactual outcomes conditional of the level of a residual using the functional form used. The proper characterization of these residuals is thought to be important to carry out such extrapolations. This research considered the case where a local average treatment effect (LATE) parameter is non-parametrically identified using a binary instrument in the presence of all binary covariates. Extensive simulations that varied the rarity of both the outcome and treatment were performed to answer questions of whether 2SLS or 2SRI methods with different forms of residuals has the least bias in estimating the LATE or the ATE parameters.

Results show that the 2SLS method with binary IV, applied to a binary endogenous treatment and a binary outcome, produces consistent estimates of LATE across the entire range of rarity for either treatment or the outcome. The rarity of either does not affect the coverage probabilities of these estimators. In contrast, the 2SRI approach with any residuals studied was a biased estimator for LATE. However, in principle, the 2SRI estimators are designed to estimate the ATE parameter. Still, results showed that 2SRI does not appear dependable for producing unbiased estimates of ATE. Rather, there were varying levels of bias associated with 2SRI estimates of ATE. Among the residual forms, 2SRI with generalized residuals appeared to produce the least biased estimates of the ATE. For very rare outcomes (<5%) 2SRI with Anscombe residual generated the least bias in estimating ATE. We conjecture that the symmetric transformation of these residuals may be leading to better extrapolation properties of the 2SRI estimators. However, whether these findings represent a general operating characteristic of 2SRI or are unique to our simulation settings is not known.

Results from this study conform to the simulation results of Chapman and Brooks (2016), who carry out similar simulations to find that 2SLS produced the consistent estimates for the LATE while 2SRI does not reliably estimate either the ATE or LATE.

However, their study did not vary rarity of treatment or outcome from approximately 0.5 or examine alternative forms of 2SRI residuals. The results of this study provide additional evidence showing how 2SLS are consistent estimators of LAIE over a wider range of means for binary outcomes and binary treatments.

We hope that this work will help the applied researcher to cautiously approach and interpret the results generated from IV estimation in models with binary treatment, binary outcome, and binary instrumental variable. Careful interpretation of treatment effects that are identified and being estimated, as well as the potential for bias arising from methodologic decisions, are key factors to consider in conducting these analyses and responsibly reporting the results from them. While estimating the LAIE may be straightforward given a valid instrument, the interpretation of LAIEs is often nuanced and may heighten the potential for unintentionally misleading or erroneous inferences and conclusions. On the other hand, interpreting population mean treatment effect parameters such as the AIE is straight-forward but estimating them is often problematic and potentially infeasible, as doing so demands either richer data or a slew of statistical assumptions that may not be met. Moreover, under settings of essential heterogeneity in treatment effectiveness, the potential usefulness of a population-wide average effect may be limited and more nuanced parameters are required for practical impact. It's important that researchers understand precisely the assumptions underlying identification of alternative treatment effect concepts and the related theory to support an approach for estimating them. We are hopeful that our results and discussions can help untangle these challenges.

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Appendix

Table A1: Simulations results (N=5,000) for Local Average Treatment Effects (LATEs) - %Bias (Coeff. Var.) {Coverage Pr}

E(Y)	Estimators	Pr(D) = 0.55	Pr(D) = 0.70	Pr(D) = 0.85	Pr(D) = 0.93	Pr(D) = 0.995
0.50~0.60	Naïve Probit	170 [.02] {0}	182 [.03] {0}	242 [.03] {0}	381 [.03] {0}	845 [.04] {0}
	2SLS	-1 [.27] {.94}	-2 [.35] {.95}	-4 [.71] {.96}	-11 [2.08] {.96}	-61 [27.76] {.97}
	2SRI	-47 [.59] {.67}	-31 [.5] {.83}	44 [.37] {.86}	208 [.35] {.45}	476 [.85] {.58}
	2SRI - sres	11 [.27] {.92}	32 [.29] {.82}	96 [.33] {.59}	215 [.42] {.52}	428 [.99] {.53}
	2SRI - dres	-103 [-9.25] {.14}	-99 [38.24] {.28}	-47 [1.25] {.82}	131 [.58] {.76}	534 [.75] {.5}
	2SRI - ares	-88 [2.74] {.24}	-81 [1.98] {.41}	-32 [.94] {.86}	123 [.59] {.79}	488 [.81] {.54}
	2SRI - gres	-46 [.56] {.65}	-32 [.49] {.82}	24 [.44] {.91}	155 [.46] {.67}	399 [.98] {.61}
	Bi.Probit	-22 [.31] {.83}	-16 [.34] {.89}	9 [.49] {.93}	54 [1.06] {.87}	297 [1.83] {.47}
0.80 ~0.90	Naïve Probit	233 [.04] {0}	185 [.04] {0}	155 [.04] {0}	160 [.04] {0}	226 [.06] {0}
	2SLS	-3 [.52] {.95}	-1 [.37] {.95}	-1 [.36] {.94}	-2 [.53] {.95}	-7 [1.74] {.96}
	2SRI	-3 [.47] {.95}	-36 [.54] {.75}	-70 [1.01] {.33}	-78 [1.71] {.42}	-44 [1.71] {.79}
	2SRI - sres	74 [.19] {.39}	69 [.17] {.32}	57 [.18] {.41}	61 [.22] {.52}	106 [.34] {.55}
	2SRI - dres	-75 [2.27] {.73}	-95 [7.59] {.26}	-103 [-9.52] {.09}	-94 [5.58] {.22}	-33 [1.26] {.82}
	2SRI - ares	-52 [1.07] {.83}	-68 [1.09] {.49}	-76 [1.15] {.23}	-70 [1.18] {.44}	-18 [1.02] {.84}
	2SRI - gres	-4 [.45] {.96}	-31 [.47] {.8}	-51 [.58] {.5}	-59 [.87] {.51}	-38 [1.35] {.79}
	Bi.Probit	-5 [.4] {.94}	-31 [.4] {.74}	-47 [.45] {.43}	-52 [.62] {.47}	-33 [1.11] {.8}
0.9 ~ 0.95	Naïve Probit	322 [.05] {0}	232 [.05] {0}	165 [.05] {0}	143 [.06] {0}	160 [.08] {0}
	2SLS	-2 [.96] {.93}	0 [.61] {.93}	1 [.46] {.93}	0 [.52] {.93}	-5 [1.15] {.95}
	2SRI	58 [.44] {.82}	-9 [.54] {.92}	-69 [1.18] {.41}	-94 [4.73] {.22}	-83 [3.52] {.53}
	2SRI - sres	134 [.19] {.15}	97 [.19] {.19}	64 [.2] {.43}	43 [.21] {.66}	51 [.29] {.77}
	2SRI - dres	-27 [1.35] {.94}	-77 [2.57] {.69}	-97 [10.3] {.19}	-98 [12.3] {.14}	-77 [2.09] {.51}
	2SRI - ares	0 [.86] {.94}	-45 [.96] {.83}	-66 [.98] {.4}	-72 [1.08] {.34}	-55 [1.13] {.64}
	2SRI - gres	52 [.43] {.81}	-8 [.51] {.91}	-47 [.63] {.57}	-66 [.9] {.34}	-67 [1.47] {.57}
	Bi.Probit	24 [.54] {.92}	-21 [.51] {.88}	-50 [.57] {.45}	-62 [.71] {.29}	-60 [1.09] {.55}
0.95~0.98	Naïve Probit	492 [.07] {0}	322 [.07] {0}	202 [.08] {0}	150 [.09] {0}	130 [.12] {0}
	2SLS	-3 [2] {.94}	-4 [1.1] {.94}	-2 [.66] {.94}	0 [.58] {.95}	-1 [.9] {.95}
	2SRI	158 [.47] {.83}	34 [.53] {.99}	-61 [1.22] {.64}	-101 [-37.55] {.25}	-92 [6.21] {.51}
	2SRI - sres	236 [.29] {.32}	144 [.21] {.17}	84 [.24] {.56}	41 [.26] {.81}	19 [.34] {.92}
	2SRI - dres	56 [1.15] {.95}	-52 [2.02] {.98}	-92 [5.92] {.45}	-98 [15.37] {.19}	-87 [2.92] {.41}
	2SRI - ares	86 [.82] {.95}	-14 [.91] {1}	-55 [.96] {.64}	-70 [.98] {.39}	-65 [1.27] {.53}
	2SRI - gres	148 [.47] {.81}	25 [.52] {.99}	-38 [.7] {.73}	-67 [.89] {.43}	-74 [1.64] {.48}
	Bi.Probit	26 [2.05] {.85}	-7 [.78] {.97}	-50 [.73] {.64}	-68 [.74] {.34}	-70 [1.25] {.46}

2SRI – sres: 2SRI with standardized residuals; 2SRI – dres: 2SRI with deviance residuals; 2SRI – ares: 2SRI with Anscombe residuals;
 2SRI-gres: 2SRI with generalized residuals; Shaded cells highlight estimator with lowest percentage bias.

Table A2: Simulations results (N=5,000) comparing to Average Treatment Effects (ATEs) - %Bias (Coeff. Var.) {Coverage Pr}

E(Y)	Estimators	Pr(D) = 0.55	Pr(D) = 0.70	Pr(D) = 0.85	Pr(D) = 0.93	Pr(D) = 0.995
0.50~0.60	Naïve Probit	248 [.02] {0}	237 [.03] {0}	210 [.03] {0}	187 [.03] {0}	163 [.04] {0}
	2SLS	28 [.27] {.88}	18 [.35] {.91}	-13 [.71] {.94}	-47 [2.08] {.94}	-89 [27.76] {.96}
	2SRI	-32 [.59] {.86}	-17 [.5] {.9}	31 [.37] {.89}	84 [.35] {.66}	61 [.85] {.71}
	2SRI - sres	44 [.27] {.81}	58 [.29] {.68}	78 [.33] {.64}	88 [.42] {.68}	47 [.99] {.67}
	2SRI - dres	-104 [-9.25] {.3}	-99 [38.24] {.39}	-52 [1.25] {.8}	38 [.58] {.85}	77 [.75] {.69}
	2SRI - ares	-85 [2.74] {.42}	-78 [1.98] {.53}	-38 [.94] {.84}	33 [.59] {.86}	64 [.81] {.69}
	2SRI - gres	-31 [.56] {.86}	-18 [.49] {.90}	12 [.44] {.91}	52 [.46] {.81}	39 [.98] {.7}
	Bi.Probit	1 [.31] {.93}	0 [.34] {.93}	-1 [.49] {.93}	-8 [1.06] {.86}	11 [1.83] {.5}
0.80 ~0.90	Naïve Probit	244 [.04] {0}	314 [.04] {0}	407 [.04] {0}	488 [.04] {0}	582 [.06] {0}
	2SLS	0 [.52] {.95}	43 [.37] {.84}	97 [.36] {.71}	121 [.53] {.82}	95 [1.74] {.93}
	2SRI	0 [.47] {.95}	-7 [.54] {.95}	-40 [1.01] {.81}	-49 [1.71] {.77}	17 [1.71] {.9}
	2SRI - sres	79 [.19] {.36}	145 [.17] {.07}	213 [.18] {.02}	262 [.22] {.07}	331 [.34] {.31}
	2SRI - dres	-74 [2.27] {.74}	-93 [7.59] {.53}	-105 [-9.52] {.39}	-87 [5.58] {.59}	40 [1.26] {.89}
	2SRI - ares	-50 [1.07] {.83}	-53 [1.09] {.78}	-51 [1.15] {.75}	-32 [1.18] {.81}	71 [1.02] {.89}
	2SRI - gres	-1 [.45] {.97}	1 [.47] {.94}	-3 [.58] {.92}	-8 [.87] {.88}	29 [1.35] {.88}
	Bi.Probit	-2 [.4] {.94}	0 [.4] {.95}	4 [.45] {.95}	9 [.62] {.91}	41 [1.11] {.9}
0.9 ~ 0.95	Naïve Probit	226 [.05] {0}	327 [.05] {0}	482 [.05] {0}	648 [.06] {0}	883 [.08] {0}
	2SLS	-25 [.96] {.91}	28 [.61] {.91}	121 [.46] {.68}	208 [.52] {.65}	260 [1.15] {.85}
	2SRI	22 [.44] {.9}	18 [.54] {.94}	-32 [1.18] {.84}	-80 [4.73] {.64}	-37 [3.52] {.86}
	2SRI - sres	81 [.19] {.3}	154 [.19] {.05}	260 [.2] {0}	340 [.21] {.02}	472 [.29] {.19}
	2SRI - dres	-44 [1.35] {.93}	-70 [2.57] {.81}	-93 [10.3] {.59}	-93 [12.3] {.57}	-13 [2.09] {.85}
	2SRI - ares	-23 [.86] {.93}	-29 [.96] {.91}	-25 [.98] {.87}	-14 [1.08] {.86}	71 [1.13] {.93}
	2SRI - gres	18 [.43] {.92}	18 [.51] {.94}	17 [.63] {.91}	3 [.9] {.9}	27 [1.47] {.9}
	Bi.Probit	-4 [.54] {.95}	2 [.51] {.94}	10 [.57] {.93}	16 [.71] {.91}	52 [1.09] {.93}
0.95~0.98	Naïve Probit	202 [.07] {0}	326 [.07] {0}	546 [.08] {0}	815 [.09] {0}	1277 [.12] {0}
	2SLS	-50 [2] {.89}	-3 [1.1] {.94}	110 [.66] {.86}	265 [.58] {.7}	491 [.9] {.79}
	2SRI	32 [.47] {.96}	35 [.53] {.99}	-16 [1.22] {.95}	-103 [-37.55] {.71}	-50 [6.21] {.79}
	2SRI - sres	72 [.29] {.79}	146 [.21] {.17}	295 [.24] {.03}	417 [.26] {.03}	612 [.34] {.24}
	2SRI - dres	-20 [1.15] {.96}	-52 [2.02] {.98}	-83 [5.92] {.8}	-94 [15.37] {.71}	-25 [2.92] {.83}
	2SRI - ares	-5 [.82] {.96}	-14 [.91] {1}	-4 [.96] {.96}	10 [.98] {.93}	109 [1.27] {.93}
	2SRI - gres	27 [.47] {.95}	26 [.52] {.99}	32 [.7] {.98}	21 [.89] {.94}	55 [1.64] {.91}
	Bi.Probit	-36 [2.05] {.94}	-6 [.78] {.97}	7 [.73] {.94}	18 [.74] {.93}	78 [1.25] {.93}

2SRI – sres: 2SRI with standardized residuals; 2SRI – dres: 2SRI with deviance residuals; 2SRI – ares: 2SRI with Anscombe residuals;
2SRI-gres: 2SRI with generalized residuals; Shaded cells highlight estimator with lowest percentage bias.

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□

Figure 1: Illustration of residual inclusion approach for binary treatment variable.

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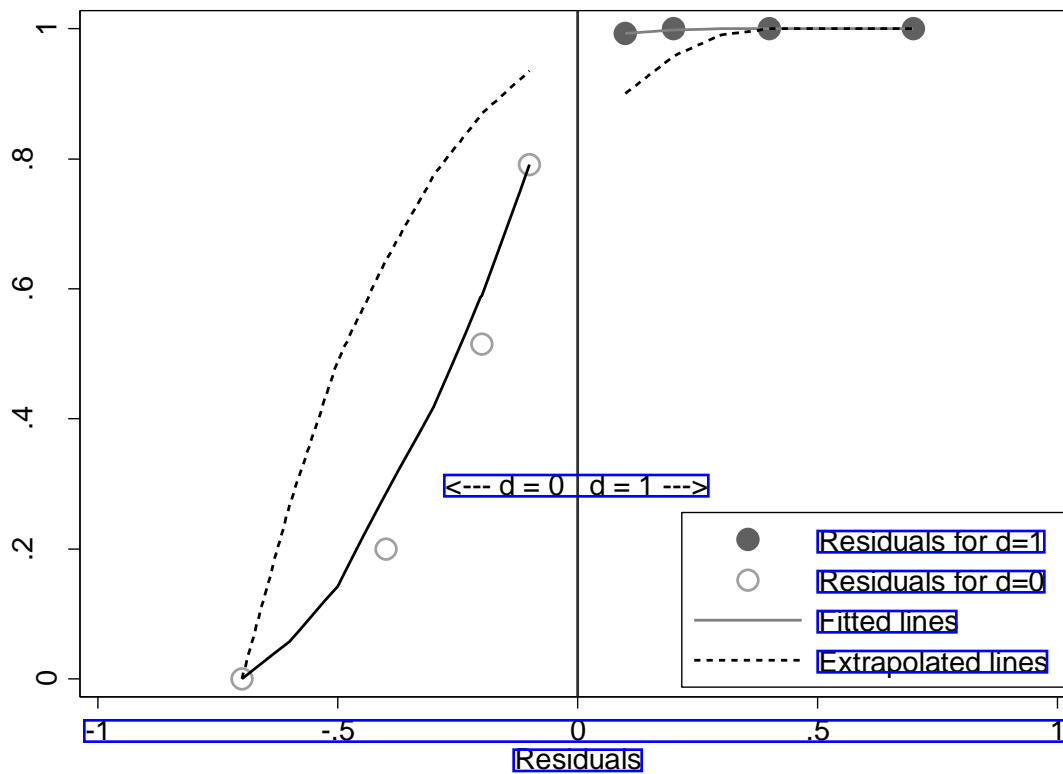


Table 1: Descriptive statistics for alternative data generating processes.

Outcomes DGP (β_0)	Exposure DGP (α_0)				
	-2	-1.25	-0.3	0.5	1.5
-2	$\Pr(D) = 0.55$	$\Pr(D) = 0.70$	$\Pr(D) = 0.85$	$\Pr(D) = 0.93$	$\Pr(D) = 0.995$
	$E(Y) = 0.51$	$E(Y) = 0.54$	$E(Y) = 0.57$	$E(Y) = 0.57$	$E(Y) = 0.58$
	$ATE = 0.165$	$ATE = 0.165$	$ATE = 0.165$	$ATE = 0.165$	$ATE = 0.165$
	$IT = 0.168$	$IT = 0.176$	$IT = 0.176$	$IT = 0.172$	$IT = 0.170$
	$TUT = 0.160$	$TUT = 0.140$	$TUT = 0.101$	$TUT = 0.071$	$TUT = 0.031$
	$LATE = 0.212$	$LATE = 0.198$	$LATE = 0.150$	$LATE = 0.098$	$LATE = 0.046$
0.5	$\Pr(D) = 0.55$	$\Pr(D) = 0.70$	$\Pr(D) = 0.85$	$\Pr(D) = 0.93$	$\Pr(D) = 0.995$
	$E(Y) = 0.82$	$E(Y) = 0.84$	$E(Y) = 0.86$	$E(Y) = 0.87$	$E(Y) = 0.89$
	$ATE = 0.097$	$ATE = 0.097$	$ATE = 0.097$	$ATE = 0.097$	$ATE = 0.097$
	$IT = 0.044$	$IT = 0.060$	$IT = 0.078$	$IT = 0.088$	$IT = 0.93$
	$TUT = 0.162$	$TUT = 0.181$	$TUT = 0.202$	$TUT = 0.201$	$TUT = 0.172$
	$LATE = 0.100$	$LATE = 0.141$	$LATE = 0.192$	$LATE = 0.218$	$LATE = 0.203$
1.5	$\Pr(D) = 0.55$	$\Pr(D) = 0.70$	$\Pr(D) = 0.85$	$\Pr(D) = 0.93$	$\Pr(D) = 0.995$
	$E(Y) = 0.93$	$E(Y) = 0.93$	$E(Y) = 0.93$	$E(Y) = 0.95$	$E(Y) = 0.95$
	$ATE = 0.058$	$ATE = 0.058$	$ATE = 0.058$	$ATE = 0.058$	$ATE = 0.058$
	$IT = 0.017$	$IT = 0.025$	$IT = 0.038$	$IT = 0.047$	$IT = 0.054$
	$TUT = 0.109$	$TUT = 0.133$	$TUT = 0.168$	$TUT = 0.197$	$TUT = 0.217$
	$LATE = 0.045$	$LATE = 0.075$	$LATE = 0.127$	$LATE = 0.178$	$LATE = 0.220$
2.5	$\Pr(D) = 0.55$	$\Pr(D) = 0.70$	$\Pr(D) = 0.85$	$\Pr(D) = 0.93$	$\Pr(D) = 0.995$
	$E(Y) = 0.96$	$E(Y) = 0.96$	$E(Y) = 0.96$	$E(Y) = 0.98$	$E(Y) = 0.98$
	$ATE = 0.029$	$ATE = 0.029$	$ATE = 0.029$	$ATE = 0.029$	$ATE = 0.029$
	$IT = 0.005$	$IT = 0.008$	$IT = 0.014$	$IT = 0.020$	$IT = 0.023$
	$TUT = 0.059$	$TUT = 0.077$	$TUT = 0.110$	$TUT = 0.144$	$TUT = 0.185$
	$LATE = 0.015$	$LATE = 0.029$	$LATE = 0.062$	$LATE = 0.107$	$LATE = 0.175$

IT: Effect on the Treated; TUT: Effect on the Untreated; True values of IT and TUT are provided for information only

Table 2: Simulations results (N=50,000) for Local Average Treatment Effects (LATEs) - %Bias (Coeff. Var.) {Coverage Pr.}

E(Y)	Estimators	Pr(D) = 0.55	Pr(D) = 0.70	Pr(D) = 0.85	Pr(D) = 0.93	Pr(D) = 0.995
0.50~0.60	Naïve Probit	170 [.01]{0}	182 [.01]{0}	242 [.01]{0}	382 [.01]{0}	846 [.01]{0}
	2SLS	-1 [.08]{.96}	-1 [.11]{.96}	-2 [.21]{.95}	-5 [.59]{.94}	-30 [4.64]{.94}
	2SRI	-49 [.19]{0}	-33 [.16]{.17}	42 [.12]{.34}	205 [.12]{0}	774 [.15]{.01}
	2SRI - sres	12 [.08]{.75}	36 [.09]{.17}	109 [.11]{0}	267 [.14]{0}	799 [.2]{.04}
	2SRI - dres	-106 [-1.45]{0}	-102 [-5.19]{0}	-50 [.42]{.36}	126 [.19]{.15}	834 [.12]{0}
	2SRI - ares	-91 [1.07]{0}	-84 [.68]{0}	-34 [.3]{.62}	120 [.19]{.18}	775 [.15]{0}
	2SRI - gres	-48 [.18]{0}	-33 [.15]{.13}	22 [.14]{.73}	150 [.15]{.03}	656 [.22]{.05}
	Bi.Probit	-23 [.1]{.17}	-17 [.1]{.5}	9 [.15]{.92}	63 [.3]{.75}	171 [1.57]{.84}
0.80 ~0.90	Naïve Probit	233 [.01]{0}	185 [.01]{0}	156 [.01]{0}	161 [.01]{0}	228 [.02]{0}
	2SLS	0 [.17]{.91}	0 [.13]{.92}	0 [.12]{.92}	0 [.17]{.93}	-1 [.51]{.93}
	2SRI	-1 [.16]{.92}	-38 [.19]{.09}	-75 [.38]{0}	-86 [.8]{0}	-79 [1.38]{.25}
	2SRI - sres	75 [.06]{0}	71 [.05]{0}	63 [.06]{0}	72 [.08]{0}	134 [.11]{0}
	2SRI - dres	-71 [.69]{.04}	-97 [3.72]{0}	-107 [-1.15]{0}	-101 [-6.45]{0}	-59 [.65]{.38}
	2SRI - ares	-48 [.34]{.15}	-68 [.39]{0}	-79 [.42]{0}	-74 [.42]{0}	-35 [.45]{.67}
	2SRI - gres	-1 [.15]{.92}	-31 [.17]{.17}	-55 [.2]{0}	-65 [.3]{0}	-62 [.69]{.35}
	Bi.Probit	-3 [.13]{.93}	-31 [.14]{.08}	-50 [.15]{0}	-56 [.19]{0}	-51 [.44]{.33}
0.9 ~ 0.95	Naïve Probit	322 [.02]{0}	232 [.02]{0}	166 [.02]{0}	144 [.02]{0}	162 [.02]{0}
	2SLS	-1 [.29]{.94}	-1 [.18]{.95}	-1 [.13]{.95}	-1 [.15]{.94}	-2 [.31]{.96}
	2SRI	61 [.12]{.1}	-12 [.16]{.82}	-76 [.41]{0}	-102 [-3.35]{0}	-108 [-1.19]{0}
	2SRI - sres	134 [.06]{0}	97 [.05]{0}	68 [.06]{0}	51 [.08]{0}	63 [.11]{.02}
	2SRI - dres	-18 [.34]{.9}	-78 [.77]{.01}	-103 [-2.91]{0}	-105 [-1.29]{0}	-96 [2.73]{0}
	2SRI - ares	7 [.23]{.91}	-47 [.28]{.11}	-71 [.32]{0}	-78 [.39]{0}	-68 [.49]{.04}
	2SRI - gres	56 [.12]{.14}	-11 [.15]{.83}	-52 [.19]{0}	-73 [.31]{0}	-84 [.8]{0}
	Bi.Probit	29 [.16]{.66}	-22 [.15]{.48}	-54 [.17]{0}	-67 [.2]{0}	-73 [.38]{0}
0.95~0.98	Naïve Probit	493 [.02]{0}	324 [.02]{0}	203 [.02]{0}	151 [.03]{0}	133 [.04]{0}
	2SLS	-2 [.6]{.95}	-1 [.32]{.96}	-1 [.19]{.97}	-2 [.17]{.97}	-3 [.25]{.96}
	2SRI	174 [.1]{0}	32 [.14]{.62}	-67 [.36]{0}	-108 [-.99]{0}	-111 [-.33]{0}
	2SRI - sres	244 [.06]{0}	142 [.06]{0}	87 [.07]{0}	48 [.09]{.01}	30 [.12]{.4}
	2SRI - dres	88 [.22]{.45}	-43 [.44]{.63}	-95 [2.42]{0}	-104 [-1.66]{0}	-102 [-2.92]{0}
	2SRI - ares	111 [.17]{.16}	-11 [.23]{.94}	-60 [.29]{0}	-76 [.32]{0}	-78 [.49]{0}
	2SRI - gres	164 [.1]{0}	25 [.14]{.72}	-44 [.21]{.05}	-74 [.3]{0}	-89 [.82]{0}
	Bi.Probit	90 [.24]{.48}	-2 [.19]{.96}	-53 [.2]{0}	-73 [.22]{0}	-83 [.4]{0}

2SRI – sres: 2SRI with standardized residuals; 2SRI – dres: 2SRI with deviance residuals; 2SRI – ares: 2SRI with Anscombe residuals;
 2SRI-gres: 2SRI with generalized residuals; Shaded cells highlight estimator with lowest percentage bias.

Table 3: Simulations results (N=50,000) comparing to Average Treatment Effects (ATEs) - %Bias (Coeff. Var.) {Coverage Pr}

E(Y)	Estimators	Pr(D) = 0.55	Pr(D) = 0.70	Pr(D) = 0.85	Pr(D) = 0.93	Pr(D) = 0.995
0.50~0.60	Naïve Probit	248 [.01]{0}	237 [.01]{0}	211 [.01]{0}	187 [.01]{0}	164 [.01]{0}
	2SLS	28 [.08]{.28}	18 [.11]{.69}	-11 [.21]{.92}	-43 [.59]{.78}	-80 [4.64]{.86}
	2SRI	-34 [.19]{.28}	-20 [.16]{.66}	28 [.12]{.55}	82 [.12]{.03}	144 [.15]{.09}
	2SRI - sres	44 [.08]{.05}	63 [.09]{.01}	90 [.11]{0}	119 [.14]{.02}	151 [.2]{.18}
	2SRI - dres	-108 [-1.45]{0}	-103 [-5.19]{0}	-55 [.42]{.19}	35 [.19]{.71}	161 [.12]{.01}
	2SRI - ares	-88 [1.07]{0}	-80 [.68]{0}	-40 [.3]{.42}	31 [.19]{.74}	144 [.15]{.05}
	2SRI - gres	-33 [.18]{.3}	-20 [.15]{.63}	11 [.14]{.88}	49 [.15]{.42}	111 [.22]{.36}
	Bi.Probit	-1 [.1]{.95}	-1 [.1]{.97}	-1 [.15]{.95}	-3 [.3]{.94}	-25 [1.57]{.85}
0.80 ~0.90	Naïve Probit	244 [.01]{0}	314 [.01]{0}	407 [.01]{0}	489 [.01]{0}	587 [.02]{0}
	2SLS	3 [.17]{.9}	45 [.13]{.25}	98 [.12]{.01}	125 [.17]{.1}	107 [.51]{.78}
	2SRI	2 [.16]{.9}	-10 [.19]{.85}	-49 [.38]{.25}	-68 [.8]{.26}	-55 [1.38]{.72}
	2SRI - sres	80 [.06]{0}	149 [.05]{0}	224 [.06]{0}	289 [.08]{0}	390 [.11]{0}
	2SRI - dres	-71 [.69]{.04}	-95 [3.72]{0}	-114 [-1.15]{0}	-103 [-6.45]{.01}	-13 [.65]{.89}
	2SRI - ares	-47 [.34]{.22}	-54 [.39]{.1}	-58 [.42]{.1}	-42 [.42]{.56}	36 [.45]{.88}
	2SRI - gres	2 [.15]{.92}	0 [.17]{.91}	-10 [.2]{.89}	-20 [.3]{.8}	-20 [.69]{.87}
	Bi.Probit	0 [.13]{.94}	0 [.14]{.91}	0 [.15]{.93}	0 [.19]{.94}	2 [.44]{.93}
0.9 ~ 0.95	Naïve Probit	226 [.02]{0}	327 [.02]{0}	484 [.02]{0}	649 [.02]{0}	891 [.02]{0}
	2SLS	-24 [.29]{.79}	27 [.18]{.76}	117 [.13]{.02}	204 [.15]{0}	272 [.31]{.38}
	2SRI	24 [.12]{.6}	13 [.16]{.89}	-48 [.41]{.36}	-107 [-3.35]{.04}	-131 [-1.19]{.19}
	2SRI - sres	81 [.06]{0}	154 [.05]{0}	268 [.06]{0}	365 [.08]{0}	519 [.11]{0}
	2SRI - dres	-37 [.34]{.6}	-72 [.77]{.09}	-107 [-2.91]{0}	-115 [-1.29]{0}	-85 [2.73]{.42}
	2SRI - ares	-18 [.23]{.85}	-31 [.28]{.59}	-37 [.32]{.5}	-32 [.39]{.7}	19 [.49]{.95}
	2SRI - gres	21 [.12]{.67}	14 [.15]{.85}	4 [.19]{.95}	-17 [.31]{.83}	-39 [.8]{.76}
	Bi.Probit	0 [.16]{.92}	0 [.15]{.95}	0 [.17]{.94}	1 [.2]{.95}	1 [.38]{.93}
0.95~0.98	Naïve Probit	203 [.02]{0}	328 [.02]{0}	549 [.02]{0}	819 [.03]{0}	1292 [.04]{0}
	2SLS	-50 [.6]{.62}	0 [.32]{.96}	111 [.19]{.26}	259 [.17]{.02}	482 [.25]{.13}
	2SRI	40 [.1]{.23}	33 [.14]{.60}	-29 [.36]{.78}	-128 [-.99]{.03}	-164 [-.33]{.06}
	2SRI - sres	76 [.06]{0}	144 [.06]{0}	301 [.07]{0}	444 [.09]{0}	679 [.12]{0}
	2SRI - dres	-4 [.22]{.96}	-42 [.44]{.66}	-89 [2.42]{.1}	-114 [-1.66]{.02}	-112 [-2.92]{.21}
	2SRI - ares	8 [.17]{.91}	-10 [.23]{.94}	-15 [.29]{.89}	-12 [.32]{.91}	30 [.49]{.97}
	2SRI - gres	35 [.1]{.32}	26 [.14]{.7}	19 [.21]{.91}	-3 [.3]{.95}	-36 [.82]{.8}
	Bi.Probit	-3 [.24]{.94}	-1 [.19]{.96}	0 [.2]{.96}	0 [.22]{.97}	2 [.4]{.94}

2SRI – sres: 2SRI with standardized residuals; 2SRI – dres: 2SRI with deviance residuals; 2SRI – ares: 2SRI with Anscombe residuals;
 2SRI-gres: 2SRI with generalized residuals; Shaded cells highlight estimator with lowest percentage bias.

Table 4: Descriptive Statistics for HRS dataset

Binary Variables	Mean (sd)
Outcomes	
Informal Care from Any Source	0.60 (0.49)
Informal Care from Child	0.43 (0.50)
Informal Care from other Relative	0.165 (0.37)
Home Health Care	0.068 (0.25)
Any Nursing Home Care	0.023 (0.15)
Treatment	
LTCI coverage	0.157 (0.364)
IV	
Subsidies	0.335 (0.472)
Other covariates	
Marital status==2	0.11 (0.32)
Marital status ==3	0.17 (0.37)
Marital status==4	0.06 (0.24)
Female	0.56 (0.5)
No. of children==1	0.1 (0.3)
No. of children==2	0.31 (0.46)
No. of children==3	0.22 (0.42)
No. of children==4	0.13 (0.34)
No. of children==5	0.15 (0.36)
No. of children==6	0.01 (0.11)
Retired	0.47 (0.5)
Education category ==2	0.35 (0.48)
Education category ==3	0.26 (0.44)
Education category ==4	0.3 (0.46)
Income category==2	0.36 (0.48)
Income category==3	0.64 (0.48)
Race category ==2	0.06 (0.25)
Race category ==3	0.03 (0.18)
Fair/Poor health	0.17 (0.37)
Any ADL	0.1 (0.29)

□

Table 5: Effects of long-term care insurance on different outcomes.

Outcomes→	Informal Care from Any Source	Informal Care from Child	Informal Care from other Relative	Home Health Care	Any Nursing Home Care
Estimators	Pr(Y) = 0.60	Pr(Y) = 0.43	Pr(Y) = 0.165	Pr(Y) = 0.07	Pr(Y) = 0.023
Naïve Probit	-0.037 (0.006)++	-0.032 (0.006)++	-0.015 (0.004)++	-0.005 (0.003)	0.001 (0.002)
2SLS	-0.302 (0.165)+	-0.329 (0.165)++	0.161 (0.114)	-0.252 (0.089)++	0.087 (0.055)
2SRI	-0.319 (0.103)++	-0.238 (0.099)++	-0.091 (0.062)	-0.142 (0.031)++	0.063 (0.097)
2SRI - sres	-0.118 (0.029)++	-0.074 (0.029)++	-0.06 (0.017)++	-0.028 (0.013)++	0.008 (0.012)
2SRI - dres	-0.392 (0.085)++	-0.28 (0.082)++	-0.126 (0.052)++	-0.127 (0.032)++	0.072 (0.102)
2SRI - ares	-0.297 (0.07)++	-0.198 (0.068)++	-0.114 (0.038)++	-0.085 (0.026)++	0.038 (0.055)
2SRI - gres	-0.268 (0.062)++	-0.179 (0.061)++	-0.111 (0.032)++	-0.077 (0.023)++	0.029 (0.041)
Bi.Probit	-0.283 (0.055)++	-0.179 (0.059)++	-0.147 (0.044)++	-0.117 (0.033)++	0.023 (0.028)

Pr(long-term care insurance) in these data = 0.15%. 2SRI - sres: 2SRI with standardized residuals; 2SRI - dres: 2SRI with deviance residuals; 2SRI - ares: 2SRI with Anscombe residuals;

+ p-val ≤ 0.10, ++ p-val ≤ 0.05

□

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