1 Regulated report precision

Now, return to the report precision example introduced in chapter 2. Recall regulators set a target report precision as regulation increases report precision and improves the owner's welfare relative to private precision choice. However, regulation also invites transaction design (commonly referred to as earnings management) which produces deviations from regulatory targets. The owner's expected utility including the cost of transaction design $\alpha_d \left(\hat{b} - \sigma_2^2\right)^2$ is

$$EU\left(\sigma_2\right) = \mu - \beta \frac{\sigma_1^2 \bar{\sigma}_2^2}{\sigma_1^2 + \bar{\sigma}_2^2} - \gamma \frac{\sigma_1^4 \left(\sigma_1^2 + \sigma_2^2\right)}{\left(\sigma_1^2 + \bar{\sigma}_2^2\right)^2} - \alpha \left(b - \sigma_2^2\right)^2 - \alpha_d \left(\hat{b} - \sigma_2^2\right)^2$$

Outcomes Y are reflected in exchange values or prices and accordingly reflect only a portion of the owner's expected utility.

$$Y = P\left(\bar{\sigma}_2\right) = \mu + \frac{\sigma_1^2}{\sigma_1^2 + \bar{\sigma}_2^2}\left(s - \mu\right) - \beta \frac{\sigma_1^2 \bar{\sigma}_2^2}{\sigma_1^2 + \bar{\sigma}_2^2}$$

In particular, cost may be hidden from the analysts' view; cost includes the explicit cost of report precision, $\alpha \left(b - \sigma_2^2\right)^2$, cost of any transaction design, $\alpha_d \left(\hat{b} - \sigma_2^2\right)^2$, and the owner's risk premia, $\gamma \frac{\sigma_1^4 \left(\sigma_1^2 + \sigma_2^2\right)}{\left(\sigma_1^2 + \bar{\sigma}_2^2\right)^2}$. Further, outcomes (prices) reflect realized draws from the accounting system s whereas the owner's expected utility is based on anticipated reports and her knowledge of the distribution for (s, EU). The causal effect of treatment (report precision) choice on outcomes is the subject under study and almost surely is endogenous.

1.1 Binary report precision choice

Suppose there are two types of owners, those with low cost parameter α_d^L, and those with high cost parameter α_d^H. An owner makes her report precision choice based on maximizing her expected utility, a portion of which is unobservable. For simplicity, we initially assume report precision is binary and observable to the analyst.

1.1.1 Base case

Focus attention on the treatment effect of report precision on price. To facilitate this exercise, we simulate data by drawing 200 samples of $2,000$ observations for normally distributed reports with mean μ and variance $\sigma_1^2 + \sigma_2^2$ where $\mu = 1,000$, $\sigma_1^2 = 100$, $\beta^L = \beta^H = \beta = 7$, $b = 150$, $\widehat{b} = 128.4$, $\gamma = 2.5$, and $\alpha = 0.02$. The two cost types are characterized by α_d, where α_d^L is independent normally distributed with mean 0.02 and standard deviation 0.005, and α_d^H is independent normally distributed with mean 0.04 and variance 0.01. These draws are not

observed by firm owners until after their report precision choices are made.[1] On the other hand, the analyst observes α_d draws ex post but their mean is unknown.[2] Owners choose report precision $\left\{\sigma_2^L, \sigma_2^H\right\}$ to maximize expected utility given their type $E\left[\alpha_d^L\right]$, or $E\left[\alpha_d^H\right]$, $\left(\sigma_2^L\right)^2 = 133.5$ and $\left(\sigma_2^H\right)^2 = 131.7$.

The treatment effect is driven by cost of transaction design, α_d. For α_d^L, investors' conjecture $\left(\overline{\sigma}_2^L\right)^2 = 133.5$ and the owner's best response is $\left(\sigma_2^L\right)^2 = 133.5$. While for α_d^H, investors' conjecture $\left(\overline{\sigma}_2^H\right)^2 = 131.7$ and the owner's best response is $\left(\sigma_2^H\right)^2 = 131.7$. Hence, the owner's expected utility associated with low variance reports given α_d^L is $(EU_1 \mid D = 1) = 486.8$ while the owner's expected utility associated with high variance reports given α_d^L is lower, $(EU_0 \mid D = 1) = 486.6$. Also, the owner's expected utility associated with high variance reports given α_d^H is $(EU_0 \mid D = 0) = 487.1$ while the owner's expected utility associated with low variance reports given α_d^H is lower, $(EU_1 \mid D = 0) = 486.9$. But observable outcomes are traded values, P, so the observed treatment effect on the treated is

$$\begin{aligned} TT &= \left(P^L \mid D = 1\right) - \left(P^H \mid D = 1\right) = (Y_1 \mid D = 1) - (Y_0 \mid D = 1) \\ &= \left(\mu + \frac{\sigma_1^2}{\sigma_1^2 + \left(\bar{\sigma}_2^L\right)^2}\left(s^L - \mu\right) - \beta\frac{\sigma_1^2\left(\bar{\sigma}_2^L\right)^2}{\sigma_1^2 + \left(\bar{\sigma}_2^L\right)^2}\right) \\ &\quad - \left(\mu + \frac{\sigma_1^2}{\sigma_1^2 + \left(\bar{\sigma}_2^L\right)^2}\left(s^H - \mu\right) - \beta\frac{\sigma_1^2\left(\bar{\sigma}_2^L\right)^2}{\sigma_1^2 + \left(\bar{\sigma}_2^L\right)^2}\right) \end{aligned}$$

Since $E\left[s^L - \mu\right] = E\left[s^H - \mu\right] = 0$, $E\left[TT\right] = ATT = 0$. Also, the observed treatment effect on the untreated is

$$\begin{aligned} TUT &= \left(P^L \mid D = 0\right) - \left(P^H \mid D = 0\right) = (Y_1 \mid D = 0) - (Y_0 \mid D = 0) \\ &= \left(\mu + \frac{\sigma_1^2}{\sigma_1^2 + \left(\bar{\sigma}_2^H\right)^2}\left(s^L - \mu\right) - \beta\frac{\sigma_1^2\left(\bar{\sigma}_2^H\right)^2}{\sigma_1^2 + \left(\bar{\sigma}_2^H\right)^2}\right) \\ &\quad - \left(\mu + \frac{\sigma_1^2}{\sigma_1^2 + \left(\bar{\sigma}_2^H\right)^2}\left(s^H - \mu\right) - \beta\frac{\sigma_1^2\left(\bar{\sigma}_2^H\right)^2}{\sigma_1^2 + \left(\bar{\sigma}_2^H\right)^2}\right) \end{aligned}$$

and $E\left[TUT\right] = ATUT = 0$. Therefore, the average treatment effect is $ATE =$

[1] For the simulation, type is drawn from a Bernoulli distribution with probability 0.5.

[2] Consequently, even if other parameters are observed by the analyst, there is uncertainty associated with selection due to α_d.

0, also. However, the OLS estimand is

$$\begin{aligned} OLS &= E\left[\left(P^L \mid D=1\right)-\left(P^H \mid D=0\right)\right]=E\left[\left(Y_1 \mid D=1\right)-\left(Y_0 \mid D=0\right)\right] \\ &= \left(\mu+\frac{\sigma_1^2}{\sigma_1^2+\left(\bar{\sigma}_2^L\right)^2}E\left[s^L-\mu\right]-\beta\frac{\sigma_1^2\left(\bar{\sigma}_2^L\right)^2}{\sigma_1^2+\left(\bar{\sigma}_2^L\right)^2}\right) \\ &\quad -\left(\mu+\frac{\sigma_1^2}{\sigma_1^2+\left(\bar{\sigma}_2^H\right)^2}E\left[s^H-\mu\right]-\beta\frac{\sigma_1^2\left(\bar{\sigma}_2^H\right)^2}{\sigma_1^2+\left(\bar{\sigma}_2^H\right)^2}\right) \\ &= \beta\frac{\sigma_1^2\left(\bar{\sigma}_2^H\right)^2}{\sigma_1^2+\left(\bar{\sigma}_2^H\right)^2}-\beta\frac{\sigma_1^2\left(\bar{\sigma}_2^L\right)^2}{\sigma_1^2+\left(\bar{\sigma}_2^L\right)^2} \end{aligned}$$

For the present example, the OLS bias is nonstochastic

$$\beta\left(\frac{\sigma_1^2\left(\bar{\sigma}_2^H\right)^2}{\sigma_1^2+\left(\bar{\sigma}_2^H\right)^2}-\frac{\sigma_1^2\left(\bar{\sigma}_2^L\right)^2}{\sigma_1^2+\left(\bar{\sigma}_2^L\right)^2}\right)=-2.33$$

Suppose we employ a naive (unsaturated) regression model, ignoring the OLS bias,

$$Y=\beta_0+\beta_1 s+\beta_2 D$$

or even a saturated regression model that ignores the OLS bias

$$Y=\beta_0+\beta_1 s+\beta_2 Ds+\beta_3 D$$

where

$$D=\begin{array}{ll} 1 & \text{if } EU^L>EU^H \\ 0 & \text{if } EU^L<EU^H \end{array}$$

$$\begin{aligned} EU^j &= \mu-\beta^j\frac{\sigma_1^2\left(\bar{\sigma}_2^j\right)^2}{\sigma_1^2+\left(\bar{\sigma}_2^j\right)^2}-\gamma\frac{\sigma_1^4\left(\sigma_1^2+\left(\sigma_2^j\right)^2\right)}{\left(\sigma_1^2+\left(\bar{\sigma}_2^j\right)^2\right)^2} \\ &\quad -\alpha\left(b-\left(\sigma_2^j\right)^2\right)^2-E\left[\alpha_d^j\right]\left(\hat{b}-\left(\sigma_2^j\right)^2\right)^2, \end{aligned}$$

$$Y=DY^L+\left(1-D\right)Y^H,$$

$$Y^j=\mu+\frac{\sigma_1^2}{\sigma_1^2+\left(\bar{\sigma}_2^j\right)^2}\left(s^j-\mu\right)-\beta^j\frac{\sigma_1^2\left(\bar{\sigma}_2^j\right)^2}{\sigma_1^2+\left(\bar{\sigma}_2^j\right)^2},$$

and

$$\begin{aligned} s &= Ds^L+\left(1-D\right)s^H \\ s^j &\sim N\left(\mu,\sigma_1^2+\left(\sigma_2^j\right)^2\right) \end{aligned}$$

for $j \in \{L, H\}$. Estimation results for the above naive regression are

statistic	β_0	β_1	$\beta_2\,(estATE)$
mean	172.2	0.430	−2.260
median	172.2	0.430	−2.260
std.dev.	0.069	0.0001	0.001
minimum	172.0	0.430	−2.264
maximum	172.4	0.430	−2.257
OLS parameter estimates for $E[Y \mid D, s] = \beta_0 + \beta_1 s + \beta_2 D$			
statistic	*ATT*	*ATUT*	*ATE*
mean	0.024	−0.011	0.006
median	0.036	0.002	0.008
std.dev.	0.267	0.283	0.191
minimum	−0.610	−0.685	−0.402
maximum	0.634	0.649	0.516
Sample statistics for average treatment effects			

Estimation results for the above saturated regression are

statistic	β_0	β_1	β_2
mean	602.1	0.432	−0.003
median	602.1	0.432	0.003
std.dev.	0.148	0.000	0.000
minimum	601.7	0.432	−0.003
maximum	602.6	0.432	−0.003
OLS parameter estimates for $E[Y \mid D, s] = \beta_0 + \beta_1 s + \beta_2 Ds + \beta_3 D$			
statistic	*estATT*	*estATUT*	$\beta_3\,(estATE)$
mean	−2.260	−2.260	−2.260
median	−2.260	−2.260	−2.260
std.dev.	0.001	0.001	0.001
minimum	−2.264	−2.265	−2.264
maximum	−2.255	−2.256	−2.257
Estimated average treatment effects			
statistic	*ATT*	*ATUT*	*ATE*
mean	0.024	−0.011	0.006
median	0.036	0.002	0.008
std.dev.	0.267	0.283	0.191
minimum	−0.610	−0.685	−0.402
maximum	0.634	0.649	0.516
Sample statistics for average treatment effects			

Since this is simulation, we have access to the "missing" data and can provide sample statistics for various treatment effects. Sample statistics for some standard treatment effects, *ATE*, *ATT*, and *ATUT*, are tabulated above (below the

model estimates). As expected, the results indicate substantial *OLS* selection bias in both regressions. Clearly, to effectively estimate any treatment effect, we need to eliminate this *OLS* selection bias from outcome.

It's unusual to encounter nonstochastic selection bias.[3] Normally, nonstochastic bias is easily eliminated as it's captured in the intercept but here the selection bias is perfectly aligned with the treatment effect of interest. Consequently, we decompose the two effects; we separate the selection bias from the treatment effect. Since the components of selection bias are proportional to the coefficients on the reports and these coefficients are consistently estimated when selection bias is nonstochastic, we can utilize the estimates from the coefficients on s^L and s^H. For example, the coefficient on s^L is $\frac{\sigma_1^2}{\sigma_1^2+\left(\bar{\sigma}_2^L\right)^2}$. Say, its estimate is ω_{s^L}. Then, $\left(\bar{\sigma}_2^L\right)^2 = \frac{\sigma_1^2\left(1-\omega_{s^L}\right)}{\omega_{s^L}}$ and $\frac{\sigma_1^2\left(\bar{\sigma}_2^L\right)^2}{\sigma_1^2+\left(\bar{\sigma}_2^L\right)^2} = \omega_{s^L}\frac{\sigma_1^2\left(1-\omega_{s^L}\right)}{\omega_{s^L}} = \sigma_1^2\left(1-\omega_{s^L}\right)$. Hence, the *OLS* selection bias is

$$\begin{aligned} & \beta\left(\frac{\sigma_1^2\left(\bar{\sigma}_2^H\right)^2}{\sigma_1^2+\left(\bar{\sigma}_2^H\right)^2} - \frac{\sigma_1^2\left(\bar{\sigma}_2^L\right)^2}{\sigma_1^2+\left(\bar{\sigma}_2^L\right)^2}\right) \\ = \quad & \beta\sigma_1^2\left(\omega_{s^L}-\omega_{s^H}\right) \end{aligned}$$

Now, we work with outcome $Y' = Y - \beta\sigma_1^2\left(D\omega_{s^L} - (1-D)\,\omega_{s^H}\right)$.

The adjustment can be estimated as follows. Estimate ω_{s^L} and ω_{s^H} from the regression

$$E\left[Y \mid D, s^L, s^H\right] = \omega_0 + \omega_1 D + \omega_{s^L} D s^L + \omega_{s^H}\left(1-D\right)s^H$$

Then, utilize the regression

$$\begin{aligned} & E\left[Y \mid s^L, s^H, \omega_{s^L}, \omega_{s^H}\right] - \omega_0 + \omega_{s^L} D\left(s^L-\mu\right) - \omega_{s^H}\left(1-D\right)\left(s^H-\mu\right) \\ & = \omega_L D\left(1-\omega_{s^L}\right) + \omega_H\left(1-D\right)\left(1-\omega_{s^H}\right) \end{aligned}$$

to recover the weights, $\omega_L = \omega_H = \omega = \beta\sigma_1^2$, on ω_{s^L} and ω_{s^H}. Finally, $Y' = Y - \omega\left(D\omega_{s^L} - (1-D)\,\omega_{s^H}\right)$.

Now, we repeat the saturated regression employing the adjusted outcome Y'.

$$E\left[Y' \mid D, s\right] = \beta_0 + \beta_1\left(s-\mu\right) + \beta_2 D + \beta_3 D_1\left(s-\mu\right)$$

The coefficient on D, β_2,estimates the average treatment effect.

[3] Like the asset revaluation setting, the explanation lies in the lack of common support for identifying counterfactuals. In this base case, cost of transaction design type (L or H) is a perfect predictor of treatment. That is, $\Pr\left(D=1 \mid \text{type}=L\right) = 1$ and $\Pr\left(D=1 \mid \text{type}=H\right) = 0$. In subsequent settings, parameter variation leads to common support and selection bias is resolved via more standard *IV* approaches.

Estimation results for the saturated regression with adjusted outcome are

statistic	β_0	β_1	β_2
mean	1000	0.432	−0.000
median	1000	0.432	0.000
std.dev.	0.148	0.000	0.001
minimum	999.6	0.432	−0.004
maximum	1001	0.432	0.003
OLS parameter estimates for $E[Y' \mid D, s] = \beta_0 + \beta_1 (s - \overline{s}) + \beta_2 D (s - \overline{s}) + \beta_3 D_1$			
statistic	*estATT*	*estATUT*	β_3 (*estATE*)
mean	−0.000	−0.000	−0.000
median	0.000	−0.000	0.000
std.dev.	0.001	0.002	0.001
minimum	−0.004	−0.005	−0.004
maximum	0.005	0.004	0.003
Estimated average treatment effects			
statistic	*ATT*	*ATUT*	*ATE*
mean	0.024	−0.011	0.006
median	0.036	0.002	0.008
std.dev.	0.267	0.283	0.191
minimum	−0.610	−0.685	−0.402
maximum	0.634	0.649	0.516
Sample statistics for average treatment effects			

As there is no residual uncertainty, response is homogeneous and the sample statistics for standard treatment effects, ATE, ATT, and $ATUT$, are of very similar magnitude—certainly within sampling variation. No residual uncertainty (in the adjusted outcome) implies treatment is ignorable.

1.1.2 Heterogeneous response

Now let's explore a more interesting setting. Everything remains as in the base case except there is unobserved (by the analyst) variation in β the parameter controlling the discount associated with uncertainty in the buyer's ability to manage the assets. In particular, β^L, β^H are independent normally distributed with mean 7 and unit variance.[4] These β^L, β^H draws are observed by the owner in conjunction with the known mean for α_d^L, α_d^L when selecting report precision. In this setting, it is as if the owners choose equilibrium inverse-report precision, σ_2^L or σ_2^H, based on the combination of β^L and α_d^L or β^H and α_d^H with greatest expected utility.[5] Therefore, unlike the base case, common support is satisfied,

[4] Independent identically distributed draws of β for L-type and H-type firms ensure the variance-covariance matrix for the unobservables/errors is nonsingular.

[5] Notice the value of β does not impact the value of the welfare maximizing report variance. Therefore, the optimal inverse report precision choices correspond to $\left(\alpha, \gamma, E\left[\alpha_d^j\right]\right)$ as in the base case but the binary choice σ_2^L or σ_2^H does depend on β^j.

i.e., there are no perfect predictors of treatment, $0 < \Pr\left(D = 1 \mid \beta^j, \alpha_d^j\right) < 1$. Plus, the choice equation and price regressions have correlated, stochastic unobservables. In fact, this correlation in the errors[6] creates a classic endogeneity concern addressed by Heckman [1974, 1975, 1978, 1979].

The treatment effect on the treated is

$$\begin{aligned} & E\left[Y_1 - Y_0 \mid D = 1, \beta^H, \beta^L\right] \\ = \; & E\left[\begin{array}{c} \mu + \frac{\sigma_1^2}{\sigma_1^2 + \left(\bar{\sigma}_2^L\right)^2}\left(s^L - \mu\right) - \beta^L \frac{\sigma_1^2\left(\bar{\sigma}_2^L\right)^2}{\sigma_1^2 + \left(\bar{\sigma}_2^L\right)^2} \\ -\left(\mu + \frac{\sigma_1^2}{\sigma_1^2 + \left(\bar{\sigma}_2^L\right)^2}\left(s^H - \mu\right) - \beta^H \frac{\sigma_1^2\left(\bar{\sigma}_2^L\right)^2}{\sigma_1^2 + \left(\bar{\sigma}_2^L\right)^2}\right) \end{array}\right] \\ = \; & \left(\beta^H - \beta^L\right) \frac{\sigma_1^2\left(\bar{\sigma}_2^L\right)^2}{\sigma_1^2 + \left(\bar{\sigma}_2^L\right)^2} \end{aligned}$$

The treatment effect on the untreated is

$$\begin{aligned} & E\left[Y_1 - Y_0 \mid D = 0, \beta^H, \beta^L\right] \\ = \; & E\left[\begin{array}{c} \mu + \frac{\sigma_1^2}{\sigma_1^2 + \left(\bar{\sigma}_2^H\right)^2}\left(s^L - \mu\right) - \beta^L \frac{\sigma_1^2\left(\bar{\sigma}_2^H\right)^2}{\sigma_1^2 + \left(\bar{\sigma}_2^H\right)^2} \\ -\left(\mu + \frac{\sigma_1^2}{\sigma_1^2 + \left(\bar{\sigma}_2^H\right)^2}\left(s^H - \mu\right) - \beta^H \frac{\sigma_1^2\left(\bar{\sigma}_2^H\right)^2}{\sigma_1^2 + \left(\bar{\sigma}_2^H\right)^2}\right) \end{array}\right] \\ = \; & \left(\beta^H - \beta^L\right) \frac{\sigma_1^2\left(\bar{\sigma}_2^H\right)^2}{\sigma_1^2 + \left(\bar{\sigma}_2^H\right)^2} \end{aligned}$$

We first attempt to estimate treatment effects via *OLS*

$$Y = \beta_0 + \beta_1\left(s - \overline{s}\right) + \beta_2\left(s - \overline{s}\right)D + \beta_3 D$$

Following Wooldridge, the coefficient on D, β_3, is the model-based average treatment effect (under strong identification conditions). Throughout the remaining discussion $(s - \overline{s})$ is the regressor of interest (based on our structural model). The model-based average treatment effect on the treated is

$$estATT = \beta_3 + \frac{\sum_i D_i\left(s_i - \overline{s}\right)\beta_2}{\sum_i D_i}$$

and the model-based average treatment effect on the untreated is

$$estATUT = \beta_3 - \frac{\sum_i D_i\left(s_i - \overline{s}\right)\beta_2}{\sum_i \left(1 - D_i\right)}$$

[6] The two regression equations and the choice equation have trivariate normal error structure.

Simulation results, including model-based estimates and sample statistics for standard treatment effects, are tabulated below.

statistic	β_0	β_1	β_2
mean	634.2	0.430	−0.003
median	634.2	0.429	−0.007
std.dev.	1.534	0.098	0.137
minimum	629.3	0.197	−0.458
maximum	637.7	0.744	0.377
OLS parameter estimates for $E[Y \mid s, D] = \beta_0 + \beta_1 (s - \overline{s}) + \beta_2 (s - \overline{s}) D + \beta_3 D$			
statistic	β_3 $(estATE)$	$estATT$	$estATUT$
mean	−2.227	−2.228	−2.225
median	−2.236	−2.257	−2.207
std.dev.	2.208	2.210	2.207
minimum	−6.672	−6.613	−6.729
maximum	3.968	3.971	3.966
OLS average treatment effect estimates			
statistic	ATE	ATT	$ATUT$
mean	0.189	64.30	−64.11
median	0.298	64.19	−64.10
std.dev.	1.810	1.548	1.462
minimum	−4.589	60.47	−67.80
maximum	4.847	68.38	−60.90
Sample statistics for average treatment effects			

Not surprisingly, *OLS* performs poorly. The *OLS* identification condition is ignorable treatment but this is not sustained by the *DGP*. Model-based estimates of ATE not within sampling variation of the average treatment effect. Further, the data are clearly heterogeneous and *OLS* (ignorable treatment) implies homogeneity.

1.1.3 *IV* approaches

Poor instruments Now, we consider various IV approaches for addressing endogeneity. First, we explore various linear IV approaches. The analyst observes D and α_d^L if $D = 1$ or α_d^H if $D = 0$. Suppose the analyst employs $\alpha_d = D\alpha_d^L + (1 - D)\,\alpha_d^H$ as an "instrument." As required, α_d is related to report precision selection, unfortunately α_d is not conditional mean independent, $E\left[y^j \mid s, \alpha_d\right] \neq E\left[y^j \mid s\right]$. To see this, recognize the outcome errors are a function of β^j and while α_d^j and β^j are independent, only α_d and not α_d^j is observed. Since α_d and β^j are related through selection D, α_d is a poor instrument. Two stage least squares instrumental variable estimation (*2SLS-IV*) produces the following results where β_3 is the model estimate for ATE. Sample statistics for the average treatment effects along with the effect estimated by *OLS* are also

tabulated.

statistic	β_0	β_1	β_2	
mean	634.2	0.433	−0.010	
median	634.4	0.439	−0.003	
std.dev.	1.694	0.114	0.180	
minimum	629.3	0.145	−0.455	
maximum	638.2	0.773	0.507	
Poor *2SLS-IV* parameter estimates for $E[Y \mid s, D] = \beta_0 + \beta_1 (s - \overline{s}) + \beta_2 (s - \overline{s}) D + \beta_3 D$				
statistic	$\beta_3\,(estATE)$	$estATT$	$estATUT$	
mean	−2.123	−2.125	−2.121	
median	−2.212	−2.217	−2.206	
std.dev.	2.653	2.650	2.657	
minimum	−7.938	−7.935	−7.941	
maximum	6.425	6.428	6.423	
Average treatment effect estimates				
statistic	ATE	ATT	$ATUT$	OLS
mean	0.189	64.30	−64.11	−2.227
median	0.298	64.19	−64.10	−2.236
std.dev.	1.810	1.548	1.462	2.208
minimum	−4.589	60.47	−67.80	−6.672
maximum	4.847	68.38	−60.90	3.968
Sample statistics for average treatment effects				

These results differ little from the OLS results except the IV model-based interval estimates of the treatment effects are wider as is expected even of a well-specified IV model. This serves as a reminder of how little consolation comes from deriving similar results from two or more poorly-specified models.

Weak instruments Suppose we have a "proper" instrument z_α in the sense that z_α is conditional mean independent. Here, we wish to explore the implications for treatment effect estimation if the instrument is only weakly related to treatment. For purposes of the simulation, we construct the instrument z_α as the residuals from a regression of α_d onto

$$U^L = -\left(\beta^L - E[\beta]\right)\left[D\frac{\sigma_1^2\left(\sigma_2^L\right)^2}{\sigma_1^2+\left(\sigma_2^L\right)^2} + (1-D)\frac{\sigma_1^2\left(\sigma_2^H\right)^2}{\sigma_1^2+\left(\sigma_2^H\right)^2}\right]$$

and

$$U^H = -\left(\beta^H - E[\beta]\right)\left[D\frac{\sigma_1^2\left(\sigma_2^L\right)^2}{\sigma_1^2+\left(\sigma_2^L\right)^2} + (1-D)\frac{\sigma_1^2\left(\sigma_2^H\right)^2}{\sigma_1^2+\left(\sigma_2^H\right)^2}\right]$$

We create a noisy instrument by adding an independent normal random variable ε with mean zero and standard deviation 0.1. This latter perturbation ensures

the instrument is weak. This instrument $z_\alpha + \varepsilon$ is employed to generate model-based estimates of some standard treatment effects via *2SLS-IV*. Results are tabulated below where β_3 is the model estimate for *ATE*.

statistic	β_0	β_1	β_2	
mean	628.5	−0.605	2.060	
median	637.3	0.329	0.259	
std.dev.	141.7	7.678	15.52	
minimum	−856.9	−73.00	−49.60	
maximum	915.5	24.37	153.0	
Weak *2SLS-IV* parameter estimates for $E[Y \mid s, D] = \beta_0 + \beta_1 (s - \overline{s}) + \beta_2 (s - \overline{s}) D + \beta_3 D$				
statistic	$\beta_3\,(estATE)$	$estATT$	$estATUT$	
mean	8.770	8.139	9.420	
median	−6.237	−6.532	−6.673	
std.dev.	276.8	273.2	280.7	
minimum	−573.3	−589.4	−557.7	
maximum	2769	2727	2818	
Average treatment effect estimates				
statistic	ATE	ATT	$ATUT$	OLS
mean	0.189	64.30	−64.11	−2.227
median	0.298	64.19	−64.10	−2.236
std.dev.	1.810	1.548	1.462	2.208
minimum	−4.589	60.47	−67.80	−6.672
maximum	4.847	68.38	−60.90	3.968
Sample statistics for average treatment effects				

The weak *IV* model-estimates are extremely noisy. Weak instruments frequently are suspected to plague empirical work. In a treatment effects setting, this can be a serious nuisance as evidenced here.

A stronger instrument Suppose z_α is available and employed as an instrument. Model-based treatment effect estimates are reported below where β_3 is

the model estimate for ATE.

statistic	β_0	β_1	β_2	
mean	634.3	0.427	0.005	
median	634.2	0.428	0.001	
std.dev.	2.065	0.204	0.376	
minimum	629.2	−0.087	−0.925	
maximum	639.8	1.001	1.005	
Stronger *2SLS-IV* parameter estimates for $E[Y \mid s, D] = \beta_0 + \beta_1 (s - \overline{s}) + \beta_2 (s - \overline{s}) D + \beta_3 D$				
statistic	$\beta_3\,(estATE)$	$estATT$	$estATUT$	
mean	−2.377	−2.402	−2.351	
median	−2.203	−2.118	−2.096	
std.dev.	3.261	3.281	3.248	
minimum	−10.15	−10.15	−10.15	
maximum	6.878	6.951	6.809	
Average treatment effect estimates				
statistic	ATE	ATT	$ATUT$	OLS
mean	0.189	64.30	−64.11	−2.227
median	0.298	64.19	−64.10	−2.236
std.dev.	1.810	1.548	1.462	2.208
minimum	−4.589	60.47	−67.80	−6.672
maximum	4.847	68.38	−60.90	3.968
Sample statistics for average treatment effects				

These results are far less noisy but nonetheless appear rather unsatisfactory. The results, on average, diverge from sample statistics for standard treatment effects and provide little or no evidence of heterogeneity. Why? As Heckman and Vytlacil [2005, 2007] discuss, it is very difficult to identify what treatment effect linear IV estimates and different instruments produce different treatment effects. Perhaps then, it is not surprising that we are unable to connect the IV treatment effect to ATE, ATT, or $ATUT$.

Propensity score as an instrument A popular ignorable treatment approach implies homogeneous response and uses the propensity score as an instrument. Suppose we employ a common approach by estimating the propensity score via a probit regression of D onto instruments z_α and z_σ, where z_α is (as defined above) the residuals of $\alpha_d = D\alpha_d^L + (1 - D)\,\alpha_d^H$ onto U^L and U^H and z_σ is the residuals from a regression of $\sigma_2 = D\sigma_2^L + (1 - D)\,\sigma_2^H$ onto U^L and U^H. Now, use the estimated probabilities $m = \Pr(D = 1 \mid z_\alpha, z_\sigma)$ in place of D to estimate the treatment effects.

$$Y = \beta_0 + \beta_1 (s - \overline{s}) + \beta_2 (s - \overline{s})\, m + \beta_3 m$$

Model-based estimates of the treatment effects are tabulated below with β_3 corresponding to ATE.

statistic	β_0	β_1	β_2	β_3
mean	634.4	0.417	0.024	−2.610
median	634.3	0.401	0.039	−2.526
std.dev.	1.599	0.151	0.256	2.075
minimum	630.9	−0.002	−0.617	−7.711
maximum	638.9	0.853	0.671	2.721
Propensity score parameter estimates for $E[Y \mid s,m] = \beta_0 + \beta_1(s-\overline{s}) + \beta_2(s-\overline{s})m + \beta_3 m$				
statistic	*estATE*	*estATT*	*estATUT*	
mean	−74.64	−949.4	−799.8	
median	7.743	−386.1	412.8	
std.dev.	1422	2400	1503	
minimum	−9827	−20650	57.75	
maximum	7879	−9.815	17090	
Average treatment effect estimates				
statistic	*ATE*	*ATT*	*ATUT*	*OLS*
mean	0.189	64.30	−64.11	−2.227
median	0.298	64.19	−64.10	−2.236
std.dev.	1.810	1.548	1.462	2.208
minimum	−4.589	60.47	−67.80	−6.672
maximum	4.847	68.38	−60.90	3.968
Sample statistics for average treatment effects				

These results again are very unsatisfactory and highly erratic. Poor performance of the propensity score IV for estimating average treatment effects is not surprising as the data are inherently heterogeneous and the propensity score IV identification condition is ignorability of treatment.[7] Next, we explore propensity score matching followed by two IV control function approaches.

[7] Ignorable treatment implies homogeneous response, $ATE = ATT = ATUT$, except for common support variations.

Propensity score matching Propensity score matching estimates of average treatment effects are tabulated below.[8]

statistic	*estATE*	*estATT*	*estATUT*
mean	−2.227	−39.88	35.55
median	−2.243	−39.68	35.40
std.dev.	4.247	5.368	4.869
minimum	−14.00	−52.00	23.87
maximum	12.43	−25.01	46.79

Propensity score matching estimates of average treatment effects

statistic	*ATE*	*ATT*	*ATUT*	*OLS*
mean	0.189	64.30	−64.11	−2.227
median	0.298	64.19	−64.10	−2.236
std.dev.	1.810	1.548	1.462	2.208
minimum	−4.589	60.47	−67.80	−6.672
maximum	4.847	68.38	−60.90	3.968

Sample statistics for average treatment effects

While not as erratic as the previous results, these results are also unsatisfactory. Estimated ATT and $ATUT$ are the opposite sign of one another as expected but reversed of the underlying sample statistics (based on "unobserved" counterfactuals). This is not surprising as ignorability of treatment is the identifying condition for propensity score matching.

Ordinate *IV* control function Let's consider an ordinate control function IV approach. The regression is

$$Y = \beta_0 + \beta_1 (s - \overline{s}) + \beta_2 D (s - \overline{s}) + \beta_3 \phi (Z\theta) + \beta_4 D$$

and is estimated via IV where instruments $\{\iota, (s - \overline{s}), m (s - \overline{s}), \phi (Z\theta), m\}$ are employed and $m = \Pr \left(D = 1 \mid Z = \left[\begin{array}{ccc} \iota & z_\alpha & z_\sigma \end{array} \right]\right)$ is estimated via probit. ATE is estimated via β_4, the coefficient on D. Following the general IV identification of ATT, ATT is estimated as

$$estATT = \beta_4 + \frac{\sum D_i \beta_3 \phi (Z_i \theta)}{\sum D_i}$$

and $ATUT$ is estimated as

$$estATUT = \beta_4 - \frac{\sum D_i \beta_3 \phi (Z_i \theta)}{\sum (1 - D_i)}$$

[8] Propensity scores within 0.02 are matched.

Simulation results along with sample statistics for common average treatment effects are tabulated below.

statistic	β_0	β_1	β_2	β_3
mean	598.6	0.410	0.030	127.6
median	598.5	0.394	0.049	127.1
std.dev.	3.503	0.139	0.237	12.08
minimum	590.0	0.032	−0.595	91.36
maximum	609.5	0.794	0.637	164.7
Ordinate control IV parameter estimates for $E[Y \mid s, D, \phi] = \beta_0 + \beta_1 (s - \overline{s}) + \beta_2 D (s - \overline{s}) + \beta_3 \phi (Z\theta) + \beta_4 D$				
statistic	$\beta_4\,(estATE)$	$estATT$	$estATUT$	
mean	−2.184	33.41	−37.91	
median	−2.130	33.21	−37.83	
std.dev.	1.790	3.831	3.644	
minimum	−6.590	22.27	−48.56	
maximum	2.851	43.63	−26.01	
Average treatment effect estimates				
statistic	ATE	ATT	$ATUT$	OLS
mean	0.189	64.30	−64.11	−2.227
median	0.298	64.19	−64.10	−2.236
std.dev.	1.810	1.548	1.462	2.208
minimum	−4.589	60.47	−67.80	−6.672
maximum	4.847	68.38	−60.90	3.968
Sample statistics for average treatment effects				

The ordinate control function results are clearly the best so far but still underestimate the extent of heterogeneity. Further, an important insight is emerging. If we compare OLS and ATE estimates, we might conclude endogeneity is a minor concern. However, estimates of ATT and $ATUT$ and their support of self-selection clearly demonstrate the false nature of such a conclusion.

Inverse-Mills' *IV* Heckman's control function approach, utilizing inverse-Mills' ratios as the control function for conditional expectations, involves

$$\begin{aligned} Y &= \beta_0 + \beta_1 (1 - D)(s - \overline{s}) + \beta_2 D (s - \overline{s}) \\ &\quad + \beta_3 (1 - D)\lambda^H + \beta_4 D\lambda^L + \beta_5 D \end{aligned}$$

where $\overline{s}$ is the sample average of s, $\lambda^H = -\frac{\phi(Z\theta)}{1-\Phi(Z\theta)}$, $\lambda^L = \frac{\phi(Z\theta)}{\Phi(Z\theta)}$, and θ is the estimated parameter vector from a probit regression of report precision choice D on $Z = \begin{bmatrix} \iota & z_\alpha & z_\sigma \end{bmatrix}$ (ι is a vector of ones). The coefficient on D, β_5, is the model-based estimate of the average treatment effect, ATE. The average treatment effect on the treated is estimated as

$$ATT = \beta_5 + (\beta_2 - \beta_1) E[s - \overline{s}] + (\beta_4 - \beta_3) E\left[\lambda^L\right]$$

While the average treatment effect on the untreated is estimated as

$$ATUT = \beta_5 + (\beta_2 - \beta_1) E[s - \overline{s}] + (\beta_4 - \beta_3) E\left[\lambda^H\right]$$

Simulation results including model-estimated average treatment effects on treated ($estATT$) and untreated ($estATUT$) as well as sample statistics for average treatment effects are tabulated below.

statistic	β_0	β_1	β_2	β_3	β_4
mean	603.2	0.423	0.433	−56.42	56.46
median	603.1	0.416	0.435	−56.72	56.63
std.dev.	1.694	0.085	0.089	2.895	2.939
minimum	598.7	0.241	0.188	−65.40	48.42
maximum	607.8	0.698	0.652	−47.53	65.59
Inverse-Mills' IV parameter estimates for $E[Y \mid s, D, \lambda] = \beta_0 + \beta_1 (1 - D)(s - \overline{s}) + \beta_2 D (s - \overline{s}) + \beta_3 (1 - D)\lambda^H + \beta_4 D\lambda^L + \beta_5 D$					
statistic	β_5 ($estATE$)	$estATT$	$estATUT$		
mean	−2.155	59.65	−64.14		
median	−2.037	59.59	−64.09		
std.dev.	1.451	2.950	3.039		
minimum	−6.861	51.36	−71.19		
maximum	1.380	67.19	−56.10		
Average treatment effect estimates					
statistic	ATE	ATT	$ATUT$	OLS	
mean	0.189	64.30	−64.11	−2.227	
median	0.298	64.19	−64.10	−2.236	
std.dev.	1.810	1.548	1.462	2.208	
minimum	−4.589	60.47	−67.80	−6.672	
maximum	4.847	68.38	−60.90	3.968	
Sample statistics for average treatment effects					

The inverse-Mills' treatment effect estimates correspond nicely with their sample statistics (based on "unobservable" counterfactuals).

1.2 Binary treatment *observed* from continuum of report precision

1.2.1 Heterogeneous response

Now, suppose the analyst only observes high or low report precision but there is considerable variation across firms. In other words, a continuum of report precision choices reflects wide variation in parameters across firms.[9] Specifically, variation in the cost of report precision parameter α and the owner's risk

[9] It is not uncommon for analysts to observe discrete choices even though there is a richer underlying choice set. Any discrete choice serves our purpose here, for simplicity we work with the binary case.

premium parameter γ produces variation in owners' optimal report precision $\frac{1}{\sigma_2}$.

Variation in α_d is again not observed by the owners prior to selecting report precision. However, α_d is observed ex post by the analyst where α_d^L is normally distributed with mean 0.02 and standard deviation 0.005, while α_d^H is normally distributed with mean 0.04 and standard deviation 0.01. There is unobserved (by the analyst) variation in β the parameter controlling the discount associated with uncertainty in the buyer's ability to manage the assets such that β is independent normally distributed with mean 7 and variance 0.2. Independent identically distributed draws of β are taken for L-type and H-type firms so that the variance-covariance matrix for the unobservables/errors is nonsingular. On the contrary, draws for "instruments" α (normally distributed with mean 0.03 and standard deviation 0.005) and γ (normally distributed with mean 5 and standard deviation 1) are not distinguished by type to satisfy IV assumptions. Otherwise, conditional mean independence of the outcome errors and instruments is violated.[10] For greater unobservable variation (that is, variation through the β term), the weaker are the instruments, and the more variable is estimation of the treatment effects. Both the choice equation and price regression have correlated, stochastic unobservables.

OLS model First, we estimate the following regression via OLS

$$E\left[Y \mid s, D\right] = \beta_0 + \beta_1 \left(s - \overline{s}\right) + \beta_2 D \left(s - \overline{s}\right) + \beta_3 D$$

[10] As we discuss later, these conditions are insufficient to establish α and γ as instruments — though weak instruments.

Simulation results are

statistic	β_0	β_1	β_2
mean	634.3	0.423	0.004
median	634.3	0.425	0.009
std.dev.	1.486	0.096	0.144
minimum	630.7	0.151	−0.313
maximum	638.4	0.658	0.520
OLS parameter estimates for $E[Y \mid s, D] = \beta_0 + \beta_1 (s - \overline{s}) + \beta_2 D (s - \overline{s}) + \beta_3 D$			
statistic	β_3 $(estATE)$	$estATT$	$estATUT$
mean	−1.546	−1.544	−1.547
median	−1.453	−1.467	−1.365
std.dev.	2.083	2.090	2.078
minimum	−8.108	−8.127	−8.088
maximum	5.170	5.122	5.216
Average treatment effect estimates			
statistic	ATE	ATT	$ATUT$
mean	0.194	64.60	−64.20
median	0.215	64.55	−64.18
std.dev.	1.699	1.634	1.524
minimum	−4.648	60.68	−68.01
maximum	4.465	68.70	−60.18
Sample statistics for average treatment effects			

In this setting, OLS effectively estimates the average treatment effect, ATE, for a firm/owner drawn at random. This is readily explained by noting the sample statistic estimated by OLS is within sampling variation of the sample statistic for ATE but ATE is indistinguishable from zero. However, if we're interested in response heterogeneity and other treatment effects, OLS, not surprisingly, is sorely lacking. OLS provides inconsistent estimates of treatment effects on the treated and untreated and has almost no diagnostic power for detecting response heterogeneity—notice there is little variation in OLS-estimated ATE, ATT, and $ATUT$.

Propensity score as an instrument Now, we estimate the propensity score via a probit regression of D onto instruments α and γ, and use the estimated probabilities $m = \Pr(D = 1 \mid z_\alpha, z_\sigma)$ in place of D to estimate the treatment effects.

$$Y = \beta_0 + \beta_1 (s - \overline{s}) + \beta_2 (s - \overline{s}) m + \beta_3 m$$

Model-based estimates of the treatment effects as well as sample statistics for average treatment effects are tabulated below.

statistic	β_0	β_1	β_2	β_3
mean	612.2	0.095	0.649	42.80
median	619.9	0.309	0.320	24.43
std.dev.	248.2	4.744	9.561	499.2
minimum	−1693	−29.80	−46.64	−1644
maximum	1441	23.35	60.58	4661
Propensity score parameter estimates for $E[Y \mid s,m] = \beta_0 + \beta_1 (s - \overline{s}) + \beta_2 (s - \overline{s}) m + \beta_3 m$				
statistic	*estATE*	*estATT*	*estATUT*	
mean	−1.558	−1.551	−1.565	
median	−1.517	−1.515	−1.495	
std.dev.	2.086	2.090	2.085	
minimum	−8.351	−8.269	−8.437	
maximum	5.336	5.300	5.370	
Average treatment effect estimates				
statistic	*ATE*	*ATT*	*ATUT*	*OLS*
mean	0.194	64.60	−64.20	−1.546
median	0.215	64.55	−64.18	−1.453
std.dev.	1.699	1.634	1.524	2.083
minimum	−4.648	60.68	−68.01	−8.108
maximum	4.465	68.70	−60.18	5.170
Sample statistics for average treatment effects				

These results again are very unsatisfactory and highly variable. As before, poor performance of the propensity score IV for estimating average treatment effects is not surprising as the data are inherently heterogeneous and the propensity score IV identification condition is ignorability of treatment (conditional mean redundancy).

Propensity score matching Propensity score matching estimates of average treatment effects are tabulated below.[11]

statistic	*estATE*	*estATT*	*estATUT*
mean	−1.522	−1.612	−1.430
median	−1.414	−1.552	−1.446
std.dev.	2.345	2.765	2.409
minimum	−7.850	−8.042	−8.638
maximum	6.924	9.013	4.906

Propensity score matching
estimates of average treatment effects

statistic	*ATE*	*ATT*	*ATUT*	*OLS*
mean	0.194	64.60	−64.20	−1.546
median	0.215	64.55	−64.18	−1.453
std.dev.	1.699	1.634	1.524	2.083
minimum	−4.648	60.68	−68.01	−8.108
maximum	4.465	68.70	−60.18	5.170

Sample statistics for average treatment effects

While not as erratic as the previous results, these results are also unsatisfactory. Estimated ATT and $ATUT$ are nearly identical even though the data are quite heterogeneous. The poor performance is not surprising as ignorability of treatment is the identifying condition for propensity score matching.

Ordinate control *IV* model Now, we consider two IV approaches for addressing endogeneity. The ordinate control function regression is

$$E\left[Y \mid s, D, \phi\right] = \beta_0 + \beta_1 \left(s - \overline{s}\right) + \beta_2 D \left(s - \overline{s}\right) + \beta_3 \phi\left(Z\theta\right) + \beta_4 D$$

and is estimated via IV where instruments $\{\iota, \left(s - \overline{s}\right), m\left(s - \overline{s}\right), \phi\left(Z\theta\right), m\}$ are employed and $m = \Pr\left(D = 1 \mid Z = \begin{bmatrix} \iota & \alpha & \gamma \end{bmatrix}\right)$ is estimated via probit. ATE is estimated via β_4, the coefficient on D. Simulation results are tabulated

[11] Propensity scores within 0.02 are matched.

below.

statistic	β_0	β_1	β_2	β_3
mean	−11633	5.798	−10.68	30971
median	772.7	0.680	−0.497	−390.8
std.dev.	176027	36.08	71.36	441268
minimum	−2435283	−58.78	−663.3	−1006523
maximum	404984	325.7	118.6	6106127
Ordinate control IV parameter estimates for $E[Y \mid s, D, \phi] = \beta_0 + \beta_1 (s - \overline{s}) + \beta_2 D (s - \overline{s}) + \beta_3 \phi (Z\theta) + \beta_4 D$				
statistic	$\beta_4\,(estATE)$	$estATT$	$estATUT$	
mean	−173.7	12181	−12505	
median	−11.21	−168.6	176.3	
std.dev.	1176	176015	175648	
minimum	−11237	−407049	−2431259	
maximum	2598	2435846	390220	
Average treatment effect estimates				
statistic	ATE	ATT	$ATUT$	OLS
mean	0.194	64.60	−64.20	−1.546
median	0.215	64.55	−64.18	−1.453
std.dev.	1.699	1.634	1.524	2.083
minimum	−4.648	60.68	−68.01	−8.108
maximum	4.465	68.70	−60.18	5.170
Sample statistics for average treatment effects				

The ordinate control function results are inconsistent and extremely noisy. Apparently, the instruments, α and γ, are sufficiently weak that the propensity score is a poor instrument. If this conjecture holds, we should see similar poor results in the second IV approach as well.

Inverse-Mills' *IV* model The inverse-Mills' IV control function regression is

$$\begin{aligned} E[Y \mid s, D, \lambda] &= \beta_0 + \beta_1 (1 - D)(s - \overline{s}) + \beta_2 D (s - \overline{s}) \\ &\quad + \beta_3 D \lambda^H + \beta_4 (1 - D) \lambda^L + \beta_5 D \end{aligned}$$

where $\overline{s}$ is the sample average of s, $\lambda^H = -\frac{\phi(Z\theta)}{1-\Phi(Z\theta)}$, $\lambda^L = \frac{\phi(Z\theta)}{\Phi(Z\theta)}$, and θ is the estimated parameters from a probit regression of precision choice D on $Z = \begin{bmatrix} \iota & \alpha & \gamma \end{bmatrix}$ (ι is a vector of ones). The coefficient on D, β_5, is the estimate of the average treatment effect, ATE. The average treatment effect on the treated is estimated as

$$ATT = \beta_5 + (\beta_2 - \beta_1) E[s - \overline{s}] + (\beta_4 - \beta_3) E\left[\lambda^L\right]$$

While the average treatment effect on the untreated is estimated as

$$ATUT = \beta_5 + (\beta_2 - \beta_1) E[s - \overline{s}] + (\beta_4 - \beta_3) E\left[\lambda^H\right]$$

Simulation results including estimated average treatment effects on treated ($estATT$) and untreated ($estATUT$) are

statistic	β_0	β_1	β_2	β_3	β_4
mean	633.7	0.423	0.427	−0.926	−55.41
median	642.2	0.424	0.418	9.178	−11.44
std.dev.	198.6	0.096	0.106	249.9	407.9
minimum	−1141	0.152	0.164	−2228	−3676
maximum	1433	0.651	0.725	1020	1042
Inverse-Mills' IV parameter estimates for $E[Y \mid s, D, \lambda] = \beta_0 + \beta_1 (1-D)(s-\overline{s}) + \beta_2 D(s-\overline{s}) + \beta_3 D\lambda^H + \beta_4 (1-D)\lambda^L + \beta_5 D$					
statistic	β_5 ($estATE$)	$estATT$	$estATUT$		
mean	43.38	−0.061	86.87		
median	23.46	−16.03	17.39		
std.dev.	504.2	399.1	651.0		
minimum	−1646	−1629	−1663		
maximum	12.50	3556	5867		
Average treatment effect estimates					
statistic	ATE	ATT	$ATUT$	OLS	
mean	0.194	64.60	−64.20	−1.546	
median	0.215	64.55	−64.18	−1.453	
std.dev.	1.699	1.634	1.524	2.083	
minimum	−4.648	60.68	−68.01	−8.108	
maximum	4.465	68.70	−60.18	5.170	
Sample statistics for average treatment effects					

While not as variable as ordinate control function model estimates, the inverse-Mills IV estimates are inconsistent and highly variable. We are likely unable to detect endogeneity or diagnose heterogeneity. The explanation for the problem lies with our supposed instruments, α and γ. Conditional mean independence may be violated due to variation in report precision or the instruments may be weak. That is, optimal report precision is influenced by variation in α and γ and variation in report precision is reflected in outcome error variation $U^L = -\left(\beta^L - E[\beta]\right)\left[D\frac{\sigma_1^2\left(\bar{\sigma}_2^L\right)^2}{\sigma_1^2+\left(\bar{\sigma}_2^L\right)^2} + (1-D)\frac{\sigma_1^2\left(\bar{\sigma}_2^H\right)^2}{\sigma_1^2+\left(\bar{\sigma}_2^H\right)^2}\right]$ and $U^H = -\left(\beta^H - E[\beta]\right)\left[D\frac{\sigma_1^2\left(\bar{\sigma}_2^L\right)^2}{\sigma_1^2+\left(\bar{\sigma}_2^L\right)^2} + (1-D)\frac{\sigma_1^2\left(\bar{\sigma}_2^H\right)^2}{\sigma_1^2+\left(\bar{\sigma}_2^H\right)^2}\right]$. To investigate the poor instrument problem we report sample correlation statistics $r(\cdot,\cdot)$ for α and γ determinants of optimal report precision with *unobservable* outcome errors U^L and U^H. We also report sample correlations between potential instruments,

α, γ, w_1, w_2, and treatment D to check for weak instruments.

statistic	$r\left(\alpha, U^L\right)$	$r\left(\alpha, U^H\right)$	$r\left(\gamma, U^L\right)$	$r\left(\gamma, U^H\right)$
mean	−0.001	−0.002	0.003	−0.000
median	−0.001	−0.004	0.003	0.001
std.dev.	0.020	0.024	0.023	0.024
minimum	−0.052	−0.068	−0.079	−0.074
maximum	0.049	0.053	0.078	0.060
statistic	$r\left(\alpha, D\right)$	$r\left(\gamma, D\right)$	$r\left(w_1, D\right)$	$r\left(w_2, D\right)$
mean	−0.000	0.001	−0.365	0.090
median	−0.001	0.003	−0.365	0.091
std.dev.	0.021	0.025	0.011	0.013
minimum	−0.046	−0.062	−0.404	0.049
maximum	0.050	0.075	−0.337	0.122
Sample correlations				

The problem with the supposed instruments, α and γ, is apparently that they're weak and not that they're correlated with U^L and U^H. On the other hand, w_1 and w_2 hold some promise. We experiment with these instruments next.

Stronger instruments To further investigate this explanation, we employ strong instruments, w_1 (the component of α_d independent of U^L and U^H) and w_2 (the component of $\sigma_2^D \equiv D\sigma_2^L + (1-D)\,\sigma_2^H$ independent of U^L and U^H),[12] and reevaluate propensity score as an instrument.[13]

Propensity score as an instrument Now, we use the estimated probabilities $m = \Pr\left(D = 1 \mid z_\alpha, z_\sigma\right)$ from the above propensity score in place of D to estimate the treatment effects.

$$Y = \beta_0 + \beta_1\left(s - \overline{s}\right) + \beta_2\left(s - \overline{s}\right)m + \beta_3 m$$

[12] For purposes of the simulation, these are constructed from the residuals of regressions of α_d and σ_2^D on unobservables U^H and U^L.

[13] A complementary possibility is to search for measures of nonpecuniary satisfaction as instruments. That is, measures which impact report precision choice but are unrelated to outcomes.

Model-based estimates of the treatment effects are tabulated below.

statistic	β_0	β_1	β_2	β_3
mean	637.1	0.419	0.012	−7.275
median	637.1	0.419	−0.007	−7.215
std.dev.	2.077	0.203	0.394	3.455
minimum	631.8	−0.183	−0.820	−16.61
maximum	1441	23.35	60.58	4661
Propensity score parameter estimates for $E[Y \mid s,m] = \beta_0 + \beta_1 (s-\overline{s}) + \beta_2 (s-\overline{s})\, m + \beta_3 m$				
statistic	*estATE*	*estATT*	*estATUT*	
mean	−70.35	−99.53	−41.10	
median	−69.73	−97.19	−41.52	
std.dev.	12.92	21.04	7.367	
minimum	−124.0	−188.0	−58.59	
maximum	5.336	5.300	5.370	
Average treatment effect estimates				
statistic	*ATE*	*ATT*	*ATUT*	*OLS*
mean	0.194	64.60	−64.20	−1.546
median	0.215	64.55	−64.18	−1.453
std.dev.	1.699	1.634	1.524	2.083
minimum	−4.648	60.68	−68.01	−8.108
maximum	4.465	68.70	−60.18	5.170
Sample statistics for average treatment effects				

These results again are very unsatisfactory and highly variable. As before, poor performance of the propensity score IV for estimating average treatment effects is not surprising as the data are inherently heterogeneous and the propensity score IV identification condition is ignorability of treatment (conditional mean independence).

Propensity score matching Propensity score matching estimates of average treatment effects are tabulated below.[14]

statistic	*estATE*	*estATT*	*estATUT*
mean	2.291	−7.833	13.80
median	2.306	−8.152	13.74
std.dev.	2.936	3.312	3.532
minimum	−6.547	−17.00	5.189
maximum	12.38	4.617	24.94

Propensity score matching
estimates of average treatment effects

statistic	*ATE*	*ATT*	*ATUT*	*OLS*
mean	0.194	64.60	−64.20	−1.546
median	0.215	64.55	−64.18	−1.453
std.dev.	1.699	1.634	1.524	2.083
minimum	−4.648	60.68	−68.01	−8.108
maximum	4.465	68.70	−60.18	5.170

Sample statistics for average treatment effects

While not as erratic as the previous results, these results are also unsatisfactory. Estimated ATT and $ATUT$ are opposite their sample statistics. The poor performance is not surprising as ignorability of treatment is the identifying condition for propensity score matching.

Ordinate control function *IV* model The ordinate control function regression is

$$E\left[Y \mid s, D, \phi\right] = \beta_0 + \beta_1 \left(s - \overline{s}\right) + \beta_2 D \left(s - \overline{s}\right) + \beta_3 \phi\left(Z\theta\right) + \beta_4 D$$

and is estimated via IV where instruments $\{\iota, \left(s - \overline{s}\right), m\left(s - \overline{s}\right), \phi\left(Z\theta\right), m\}$ are employed and $m = \Pr\left(D = 1 \mid Z = \left[\begin{array}{cccc} \iota & w_1 & w_2 \end{array}\right]\right)$ is estimated via probit. ATE is estimated via β_4, the coefficient on D. Simulation results are tabulated

[14] Propensity scores within 0.02 are matched.

below.

statistic	β_0	β_1	β_2	β_3
mean	616.0	0.419	0.010	66.21
median	616.5	0.418	−0.006	65.24
std.dev.	7.572	0.202	0.381	24.54
minimum	594.0	−0.168	−0.759	1.528
maximum	635.5	0.885	1.236	147.3
Ordinate control IV parameter estimates for $E[Y \mid s, D, \phi] = \beta_0 + \beta_1 (s - \overline{s}) + \beta_2 D (s - \overline{s}) + \beta_3 \phi (Z\theta) + \beta_4 D$				
statistic	$\beta_4\,(estATE)$	$estATT$	$estATUT$	
mean	−11.91	12.52	−36.35	
median	−11.51	12.31	−36.53	
std.dev.	4.149	7.076	12, 14	
minimum	−24.68	−5.425	−77.47	
maximum	−2.564	32.37	−4.535	
Average treatment effect estimates				
statistic	ATE	ATT	$ATUT$	OLS
mean	0.194	64.60	−64.20	−1.546
median	0.215	64.55	−64.18	−1.453
std.dev.	1.699	1.634	1.524	2.083
minimum	−4.648	60.68	−68.01	−8.108
maximum	4.465	68.70	−60.18	5.170
Sample statistics for average treatment effects				

The ordinate control function results are markedly improved relative to those obtained with poor instruments, α and γ. Model-estimated average treatment effects are biased somewhat toward zero. Nonetheless, the ordinate control IV approach might enable us to detect endogeneity, even though OLS and ATE are within sampling variation of one another, via heterogeneity. The important point illustrated here is that the effectiveness of IV control function approaches depend heavily on strong instruments. It's important to remember proper instruments in large part have to be evaluated ex ante — sample evidence is of limited help due to unobservability of counterfactuals.

Inverse-Mills' *IV* model The inverse-Mills' IV regression is

$$\begin{aligned} E[Y \mid s, D, \lambda] &= \beta_0 + \beta_1 (1 - D)(s - \overline{s}) + \beta_2 D (s - \overline{s}) \\ &\quad + \beta_3 D \lambda^H + \beta_4 (1 - D) \lambda^L + \beta_5 D \end{aligned}$$

where $\overline{s}$ is the sample average of s, $\lambda^H = -\frac{\phi(Z\theta)}{1-\Phi(Z\theta)}$, $\lambda^L = \frac{\phi(Z\theta)}{\Phi(Z\theta)}$, and θ is the estimated parameters from a probit regression of precision choice D on $Z = \begin{bmatrix} \iota & w_1 & w_2 \end{bmatrix}$ (ι is a vector of ones). The coefficient on D, β_5, is the estimate of the average treatment effect, ATE. Simulation results including estimated

average treatment effects on treated ($estATT$) and untreated ($estATUT$) are

statistic	β_0	β_1	β_2	β_3	β_4
mean	611.6	0.423	0.428	−32.03	80.04
median	611.5	0.431	0.422	−32.12	79.84
std.dev.	2.219	0.093	0.099	3.135	6.197
minimum	606.6	0.185	0.204	−41.47	62.39
maximum	617.5	0.635	0.721	−20.70	98.32
Inverse-Mills' IV parameter estimates for $E[Y \mid s, D, \lambda] = \beta_0 + \beta_1 (1-D)(s-\overline{s}) + \beta_2 D(s-\overline{s}) + \beta_3 D\lambda^H + \beta_4 (1-D)\lambda^L + \beta_5 D$					
statistic	$\beta_5\,(estATE)$	$estATT$	$estATUT$		
mean	−35.55	43.77	−114.8		
median	−35.11	43.80	−114.7		
std.dev.	3.868	4.205	8.636		
minimum	−47.33	30.02	−142.0		
maximum	−26.00	57.97	−90.55		
Average treatment effect estimates					
statistic	ATE	ATT	$ATUT$	OLS	
mean	0.194	64.60	−64.20	−1.546	
median	0.215	64.55	−64.18	−1.453	
std.dev.	1.699	1.634	1.524	2.083	
minimum	−4.648	60.68	−68.01	−8.108	
maximum	4.465	68.70	−60.18	5.170	
Sample statistics for average treatment effects					

Similar to the binary precision choice setting, the inverse-Mills' IV estimates are the closest to their respective sample statistics of the models considered. The inverse-Mills' IV estimates are within sampling variation of their estimands (except ATE, which is biased downward). We are able to detect endogeneity or diagnose heterogeneity by examining estimated ATT and $ATUT$. Importantly, this derives from employing strong instruments, w_1 (the component of α_d independent of U^L and U^H) and w_2 (the component of $\sigma_2^D = D\sigma_2^L + (1-D)\sigma_2^H$ independent of U^L and U^H). The next example reexamines treatment effect estimation in a setting where OLS and ATE differ markedly and estimates of ATE may help detect endogeneity.

1.2.2 Simpson's paradox

Suppose a firm's owner receives nonpecuniary and unobservable (to the analyst) satisfaction associated with report precision choice. Is this plausible? Surely! This setting highlights a deep concern when analyzing data — perversely omitted, correlated variables which produce a Simpson's paradox result.

Consider α_d^L is normally distributed with mean 1.0 and standard deviation 0.25, while α_d^H is normally distributed with mean 0.04 and standard deviation

0.01.[15] As with β^j, these differences between L and H-type cost parameters are perceived or observed by the owner; importantly, β^L has standard deviation 2 while β^H has standard deviation 0.2 and each has mean 7. The unpaid cost of transaction design is passed on to the firm and its investors by L-type owners. Investors are aware of this (and price the firm accordingly) but the analyst is not (hence it's unobserved). L-type owners get nonpecuniary satisfaction from transaction design such that their personal cost is only 2% of $\alpha_d^L \left(\widehat{b} - \sigma_2^2\right)^2$, while H-type owners receive no nonpecuniary satisfaction — hence the labels.[16] Other features remain as in the previous setting. Accordingly, expected utility for L-type owners who choose treatment is

$$\begin{aligned} EU^L\left(\sigma_2^L\right) &= \mu - \beta^L \frac{\sigma_1^2\left(\bar{\sigma}_2^L\right)^2}{\sigma_1^2 + \left(\bar{\sigma}_2^L\right)^2} - \gamma \frac{\sigma_1^4\left(\sigma_1^2 + \left(\bar{\sigma}_2^L\right)^2\right)}{\left(\sigma_1^2 + \left(\bar{\sigma}_2^L\right)^2\right)^2} \\ &\quad -\alpha\left(b - \left(\sigma_2^L\right)^2\right)^2 - 0.02\alpha_d^L\left(\hat{b} - \left(\sigma_2^L\right)^2\right)^2 \end{aligned}$$

while expected utility for H-type owners who choose no treatment is

$$\begin{aligned} EU^H\left(\sigma_2^H\right) &= \mu - \beta^H \frac{\sigma_1^2\left(\bar{\sigma}_2^H\right)^2}{\sigma_1^2 + \left(\bar{\sigma}_2^H\right)^2} - \gamma \frac{\sigma_1^4\left(\sigma_1^2 + \left(\bar{\sigma}_2^H\right)^2\right)}{\left(\sigma_1^2 + \left(\bar{\sigma}_2^H\right)^2\right)^2} \\ &\quad -\alpha\left(b - \left(\sigma_2^H\right)^2\right)^2 - \alpha_d^H\left(\hat{b} - \left(\sigma_2^H\right)^2\right)^2 \end{aligned}$$

Also, outcomes or prices for owners who choose treatment include the cost of transaction design and accordingly are

$$Y^L = P\left(\bar{\sigma}_2^L\right) = \mu + \frac{\sigma_1^2}{\sigma_1^2 + \left(\bar{\sigma}_2^L\right)^2}\left(s^L - \mu\right) - \beta^L \frac{\sigma_1^2\left(\bar{\sigma}_2^L\right)^2}{\sigma_1^2 + \left(\bar{\sigma}_2^L\right)^2} - \alpha_d^L\left(\hat{b} - \left(\sigma_2^L\right)^2\right)^2$$

***OLS* Model** An *OLS* regression is

$$E\left[Y \mid s, D\right] = \beta_0 + \beta_1\left(s - \overline{s}\right) + \beta_2 D\left(s - \overline{s}\right) + \beta_3 D$$

[15] The labels seem reversed, but bear with us.

[16] The difference in variability between β^L and β^H creates the spread between ATE and the effect estimated via OLS while nonpecuniary reward creates a shift in their mean outcomes such that OLS is positive and ATE is negative.

Simulation results are

statistic	β_0	β_1	β_2
mean	603.2	0.434	−0.014
median	603.2	0.434	−0.007
std.dev.	0.409	0.023	0.154
minimum	602.2	0.375	−0.446
maximum	604.4	0.497	0.443
OLS parameter estimates for $E[Y \mid s, D] = \beta_0 + \beta_1 (s - \overline{s}) + \beta_2 D (s - \overline{s}) + \beta_3 D$			
statistic	β_3 $(estATE)$	$estATT$	$estATUT$
mean	54.03	54.03	54.04
median	53.89	53.89	53.91
std.dev.	2.477	2.474	2.482
minimum	46.17	46.26	46.08
maximum	62.31	62.25	62.37
Average treatment effect estimates			
statistic	ATE	ATT	$ATUT$
mean	−33.95	57.76	−125.4
median	−34.06	57.78	−125.4
std.dev.	2.482	2.386	2.363
minimum	−42.38	51.15	−131.3
maximum	−26.57	66.49	−118.5
Sample statistics for average treatment effects			

Clearly, OLS produces poor estimates of the average treatment effects.

Ordinate control IV model Now, we consider two IV control function approaches for addressing endogeneity. An ordinate control function regression is

$$E[Y \mid s, D, \phi] = \beta_0 + \beta_1 (s - \overline{s}) + \beta_2 D (s - \overline{s}) + \beta_3 \phi (Z\theta) + \beta_4 D$$

and is estimated via IV where instruments $\{\iota, s, m (s - \overline{s}) m, \phi (Z\theta), m\}$ are employed and $m = \Pr\left(D = 1 \mid Z = \begin{bmatrix} \iota & w_1 & w_2 \end{bmatrix}\right)$ is estimated via probit. ATE

is estimated via β_4, the coefficient on D. Simulation results are tabulated below.

statistic	β_0	β_1	β_2	β_3
mean	561.0	0.441	−0.032	266.3
median	561.5	0.479	−0.041	263.7
std.dev.	9.703	0.293	0.497	31.41
minimum	533.5	−0.442	−1.477	182.6
maximum	585.7	1.305	1.615	361.5
Ordinate control IV parameter estimates for $E[Y \mid s, D, \phi] = \beta_0 + \beta_1 (s - \overline{s}) + \beta_2 D (s - \overline{s}) + \beta_3 \phi (Z\theta) + \beta_4 D$				
statistic	$\beta_4 (estATE)$	$estATT$	$estATUT$	
mean	−48.72	48.45	−145.6	
median	−49.02	47.97	−143.0	
std.dev.	8.190	10.43	16.58	
minimum	−71.88	21.53	−198.0	
maximum	−25.12	84.89	−99.13	
Average treatment effect estimates				
statistic	ATE	ATT	$ATUT$	OLS
mean	−33.95	57.76	−125.4	54.03
median	−34.06	57.78	−125.4	53.89
std.dev.	2.482	2.386	2.363	2.477
minimum	−42.38	51.15	−131.3	46.17
maximum	−26.57	66.49	−118.5	62.31
Sample statistics for average treatment effects				

As expected, the ordinate control function fairs much better than OLS. Estimates of $ATUT$ are biased somewhat away from zero and, as expected, more variable than the sample statistic, but estimates are within sampling variation. Nevertheless, the ordinate control IV model performs better than in previous settings. Next, we compare results with the inverse-Mills' IV approach.

Inverse-Mills' *IV* model The inverse-Mills' IV control function regression is

$$\begin{aligned} E[Y \mid s, D, \lambda] &= \beta_0 + \beta_1 (1 - D)(s - \overline{s}) + \beta_2 D (s - \overline{s}) \\ &\quad + \beta_3 (1 - D) \lambda^H + \beta_4 D \lambda^L + \beta_5 D \end{aligned}$$

where $\overline{s}$ is the sample average of s, $\lambda^H = -\frac{\phi(Z\theta)}{1-\Phi(Z\theta)}$, $\lambda^L = \frac{\phi(Z\theta)}{\Phi(Z\theta)}$, and θ is the estimated parameters from a probit regression of precision choice D on $Z = \begin{bmatrix} \iota & w_1 & w_2 \end{bmatrix}$ (ι is a vector of ones). The coefficient on D, β_5, is the estimate of the average treatment effect, ATE. Simulation results including estimated

average treatment effects on treated ($estATT$) and untreated ($estATUT$) are

statistic	β_0	β_1	β_2	β_3	β_4
mean	603.3	0.434	0.422	0.057	182.8
median	603.2	0.434	0.425	0.016	183.0
std.dev.	0.629	0.023	0.128	0.787	11.75
minimum	601.1	0.375	0.068	−2.359	151.8
maximum	604.9	0.497	0.760	1.854	221.7
Inverse-Mills' IV parameter estimates for $E[Y \mid s, D, \lambda] = \beta_0 + \beta_1 (1 - D)(s - \overline{s}) + \beta_2 D (s - \overline{s}) + \beta_3 (1 - D)\lambda^H + \beta_4 D\lambda^L + \beta_5 D$					
statistic	β_5 ($estATE$)	$estATT$	$estATUT$		
mean	−74.17	53.95	−201.9		
median	−74.46	53.88	−201.3		
std.dev.	8.387	2.551	16.58		
minimum	−99.78	45.64	−256.7		
maximum	−52.65	61.85	−159.1		
Average treatment effect estimates					
statistic	ATE	ATT	$ATUT$	OLS	
mean	−33.95	57.76	−125.4	54.03	
median	−34.06	57.78	−125.4	53.89	
std.dev.	2.482	2.386	2.363	2.477	
minimum	−42.38	51.15	−131.3	46.17	
maximum	−26.57	66.49	−118.5	62.31	
Sample statistics for average treatment effects					

As with the ordinate control function approach, inverse-Mills' estimates of the treatment effects (especially $ATUT$) are somewhat biased away from zero and, as expected, more variable than the sample statistics. However, the model supplies strong evidence of endogeneity (differs markedly from OLS results) and heterogeneous response ($ATE \neq ATT \neq ATUT$). Importantly, mean and median estimates reveal a Simpson's paradox result—OLS estimates a positive average treatment effect while endogeneity of selection produces a negative average treatment effect.[17]

1.2.3 Nonnormality and MTE

Now, we explore the impact of nonnormality on the analysis of report precision treatment effects. In our simulation, α_d is observed by the owner prior to selecting report precision, α_d^L is drawn from an exponential distribution with rate $\frac{1}{0.02}$ (reciprocal of the mean), α_d^H is drawn from an exponential distribution with rate $\frac{1}{0.04}$, α is drawn from an exponential distribution with rate $\frac{1}{0.03}$ and γ

[17] As noted previously, untabulated results using weak instruments (α and γ) reveal extremely erratic estimates of the treatment effects.

is drawn from an exponential distribution with rate $\frac{1}{5}$.[18] This means the unobservable (by the analyst) portion of the choice equation is nonnormal. Setting parameters are summarized in the table below.

α_d^L	$\sim \exp\left(\frac{1}{0.02}\right)$
α_d^H	$\sim \exp\left(\frac{1}{0.04}\right)$
α	$\sim \exp\left(\frac{1}{0.03}\right)$
γ	$\sim \exp\left(\frac{1}{5}\right)$
β^L	$\sim N(7,1)$
β^H	$\sim N(7,1)$
Parameters	

OLS results First, we report *OLS* simulation results and sample statistics for average treatment effects.

statistic	β_0	β_1	β_2
mean	635.0	0.523	−.006
median	635.0	0.526	−0.066
std.dev.	1.672	0.105	0.148
minimum	630.1	0.226	−0.469
maximum	639.6	0.744	0.406
OLS parameter estimates for $E[Y \mid s, D] = \beta_0 + \beta_1 (s - \overline{s}) + \beta_2 D (s - \overline{s}) + \beta_3 D$			
statistic	β_3 (*estATE*)	*estATT*	*estATUT*
mean	4.217	4.244	4.192
median	4.009	4.020	4.034
std.dev.	2.184	2.183	2.187
minimum	−1.905	−1.887	−1.952
maximum	10.25	10.37	10.13
Average treatment effect estimates			
statistic	*ATE*	*ATT*	*ATUT*
mean	−1.053	62.04	−60.43
median	−1.012	62.12	−60.44
std.dev.	1.800	1.678	1.519
minimum	−6.007	58.16	−64.54
maximum	3.787	65.53	−56.94
Sample statistics for average treatment effects			

As expected, *OLS* estimates of average treatment effects are poor, though there is little difference between *ATE* and *OLS*. Further, *OLS* cannot detect outcome heterogeneity.

[18] Probability as logic implies that if we only know the mean and support is nonnegative, then we conclude α_d has an exponential distribution. Similiar reasoning implies knowledge of the variance leads to a Gaussian distribution in the previous setting (see Jaynes [2003]).

Ordinate control *IV* model The ordinate control function regression is

$$E[Y \mid s, D, \phi] = \beta_0 + \beta_1 (s - \overline{s}) + \beta_2 D (s - \overline{s}) + \beta_3 \phi (Z\theta) + \beta_4 D$$

and is estimated via IV where instruments $\{\iota, (s - \overline{s}), m (s - \overline{s}), \phi (Z\theta), m\}$ are employed and $m = \Pr\left(D = 1 \mid Z = \left[\begin{array}{ccc} \iota & w_1 & w_2 \end{array}\right]\right)$ is estimated via probit. ATE is estimated via β_4, the coefficient on D. Simulation results are tabulated below.

statistic	β_0	β_1	β_2	β_3
mean	805.7	−2.879	5.845	54.71
median	765.9	−2.889	5.780	153.3
std.dev.	469.8	1.100	1.918	1373
minimum	−482.7	−5.282	0.104	−3864
maximum	2135	0.537	10.25	3772
Ordinate control IV parameter estimates for $E[Y \mid s, D, \phi] = \beta_0 + \beta_1 (s - \overline{s}) + \beta_2 D (s - \overline{s}) + \beta_3 \phi (Z\theta) + \beta_4 D$				
statistic	$\beta_4(estATE)$	$estATT$	$estATUT$	
mean	−391.4	−369.6	−411.7	
median	−397.9	−336.5	−430.7	
std.dev.	164.5	390.4	671.2	
minimum	−787.4	−1456	−2190	
maximum	130.9	716.0	1554	
Average treatment effect estimates				
statistic	ATE	ATT	$ATUT$	OLS
mean	−1.053	62.04	−60.43	4.217
median	−1.012	62.12	−60.44	4.009
std.dev.	1.800	1.678	1.519	2.184
minimum	−6.007	58.16	−64.54	−1.905
maximum	3.787	65.53	−56.94	10.25
Sample statistics for average treatment effects				

The ordinate control function treatment effect estimates are inconsistent (biased downward), extremely variable, though, on average, the rank ordering of ATT and $ATUT$ is consistent with the sample statistics. In other words, nonnormality renders the utility of a normality-based ordinate control function approach highly suspect.

Inverse-Mills' *IV* model Heckman's inverse-Mills' ratio regression is

$$\begin{aligned} E[Y \mid s, D, \lambda] &= \beta_0 + \beta_1 (1 - D)(s - \overline{s}) + \beta_2 D (s - \overline{s}) \\ &\quad + \beta_3 (1 - D) \lambda^H + \beta_4 D \lambda^L + \beta_5 D \end{aligned}$$

where $\overline{s}$ is the sample average of s, $\lambda^H = -\frac{\phi(Z\theta)}{1-\Phi(Z\theta)}$, $\lambda^L = \frac{\phi(Z\theta)}{\Phi(Z\theta)}$, and θ is the estimated parameters from a probit regression of precision choice D on $Z = \left[\begin{array}{ccc} \iota & w_1 & w_2 \end{array}\right]$ (ι is a vector of ones). The coefficient on D, β_5, is the estimate

of the average treatment effect, ATE. Simulation results including estimated average treatment effects on treated ($estATT$) and untreated ($estATUT$) are

statistic	β_0	β_1	β_2	β_3	β_4
mean	636.7	0.525	0.468	2.074	0.273
median	636.1	0.533	0.467	0.610	−4.938
std.dev.	30.61	0.114	0.114	39.74	41.53
minimum	549.2	0.182	0.108	−113.5	−118.4
maximum	724.4	0.809	0.761	116.0	121.4
Inverse-Mills' IV parameter estimates for $E[Y \mid s, D, \lambda] = \beta_0 + \beta_1 (1 - D)(s - \overline{s}) + \beta_2 D (s - \overline{s}) + \beta_3 (1 - D)\lambda^H + \beta_4 D\lambda^L + \beta_5 D$					
statistic	β_5 ($estATE$)	$estATT$	$estATUT$		
mean	2.168	0.687	3.555		
median	5.056	0.439	12.26		
std.dev.	48.44	63.22	66.16		
minimum	−173.4	−181.4	−192.9		
maximum	117.8	182.6	190.5		
Average treatment effect estimates					
statistic	ATE	ATT	$ATUT$	OLS	
mean	−1.053	62.04	−60.43	4.217	
median	−1.012	62.12	−60.44	4.009	
std.dev.	1.800	1.678	1.519	2.184	
minimum	−6.007	58.16	−64.54	−1.905	
maximum	3.787	65.53	−56.94	10.25	
Sample statistics for average treatment effects					

The inverse-Mills' estimates of the treatment effects are inconsistent and sufficiently variable that we may not detect nonzero treatment effects (though estimated treated effects are not as variable as those estimated by the ordinate control IV model). Further, the inverse-Mills' results suggest greater homogeneity (all treatment effects are negative, on average) which suggests we likely would be unable to identify outcome heterogeneity.

MTE* estimates via *LIV Now, we employ Heckman's MTE approach for estimating the treatment effects via a semi-parametric local instrumental variable estimator (LIV — see discussion in chapter 11). Our semi-parametric approach only allows us to recover estimates from the outcome equations for β_1 and β_2 where the reference regression is

$$E[Y \mid s, D, \tau_1] = \beta_1 (s - \overline{s}) + \beta_2 D (s - \overline{s}) + \tau_1 (p)$$

We employ a semi-parametric model to estimate the selection equation as well as the outcome equation. Estimated parameters and treatment effects based on

bootstrapped semi-parametric weighted MTE are below.[19]

statistic	β_1	β_2	$estATE$	$estATT$	$estATUT$
mean	1.178	−1.390	17.98	14.73	25.79
std.dev.	0.496	1.009	23.54	26.11	38.08
minimum	0.271	−3.517	−27.63	−32.86	−55.07
maximum	2.213	0.439	64.67	69.51	94.19
MTE parameter and average treatment effect estimates via LIV for $E[Y \mid s, D, \tau_1] = \beta_1 (s - \overline{s}) + \beta_2 D (s - \overline{s}) + \tau_1 (p)$					
statistic		OLS	ATE	ATT	$ATUT$
mean		4.217	−1.053	62.04	−60.43
median		4.009	−1.012	62.12	−60.44
std.dev.		2.184	1.800	1.678	1.519
minimum		−1.905	−6.007	58.16	−64.54
maximum		10.25	3.787	65.53	−56.94
Sample statistics for average treatment effects					

While the MTE results may more closely approximate the sample statistics than their parametric counterpart IV estimators, their high variance and apparent bias compromises their utility. Could we reliably detect endogeneity? Perhaps through heterogeneity. However, the ordering of the estimated treatment effects doesn't correspond well with sample statistics for the treatment effects.

Are these results due to non-normality of the unobservable features of the selection equation? Perhaps, but a closer look suggests that our original thinking applied to this subsection is misguided. While expected utility associated with low (or high) inverse report precision equilibrium strategies are distinctly nonnormal, selection involves their relative ranking or, in other words, the unobservable of interest comes from the difference in unobservables. Remarkably, their difference (V_D) is not distinguishable from Gaussian draws (based on descriptive statistics, plots, etc.).

Then, what is the explanation? It is partially explained by the analyst observing binary choice when there is a mulitiplicity of inverse report precision choices. However, we observed this in an earlier case with a lesser impact than demonstrated here. Rather, the feature that stands out is the quality of the instruments. The same instruments are employed in this "nonnormal" case as previously employed but, for some reason, are much weaker instruments in this "nonnormal" setting. Below we report the analogous sample correlations to

[19] Unlike other simulations which are developed within R, these results are produced using Heckman, Urzua, and Vytlacil's MTE program. Reported results employ a probit selection equation. Similar results obtain when either a linear probability or nonparametric regression selection equation are employed.

those reported above for Gaussian draws.

statistic	$r\left(\alpha, U^L\right)$	$r\left(\alpha, U^H\right)$	$r\left(\gamma, U^L\right)$	$r\left(\gamma, U^H\right)$
mean	−0.004	0.000	0.005	−0.007
median	−0.005	−0.001	0.007	−0.006
std.dev.	0.022	0.024	0.023	0.022
minimum	−0.081	−0.056	−0.048	−0.085
maximum	0.054	0.064	0.066	0.039
statistic	$r\left(\alpha, D\right)$	$r\left(\gamma, D\right)$	$r\left(w_1, D\right)$	$r\left(w_2, D\right)$
mean	0.013	−0.046	−0.114	0.025
median	0.013	−0.046	−0.113	0.024
std.dev.	0.022	0.021	0.012	0.014
minimum	−0.042	−0.106	−0.155	−0.011
maximum	0.082	0.017	−0.080	0.063
Sample correlations				

Correlations between the instruments, w_1 and w_2, and treatment, D, are decidedly smaller than above. Further, α and γ offer little help.

To further explore this explanation, we create a third and stronger instrument, w_3, and utilize it along with w_1 in the selection equation. This third instrument is the residuals of a binary variable $\Im\left(EU\left(\sigma_2^L, \overline{\sigma}_2^L\right) > EU\left(\sigma_2^H, \overline{\sigma}_2^L\right)\right)$ regressed onto U^L and U^H. Below we tabulate ordinate control function and inverse Mills' ratio results along with *OLS* statistics for comparison.

statistic	β_0	β_1	β_2	β_3
mean	596.8	0.423	0.024	137.9
median	597.0	0.414	0.025	138.2
std.dev.	4.168	0.140	0.238	14.87
minimum	586.8	−0.012	−0.717	90.56
maximum	609.8	0.829	0.728	179.2
Ordinate control IV parameter estimates for $E\left[Y \mid s, D, \phi\right] = \beta_0 + \beta_1\left(s - \overline{s}\right) + \beta_2 D\left(s - \overline{s}\right) + \beta_3\phi\left(Z\theta\right) + \beta_4 D$				
statistic	$\beta_4(estATE)$	$estATT$	$estATUT$	
mean	−2.494	40.35	−43.77	
median	−2.449	40.07	−43.58	
std.dev.	2.343	−4.371	5.598	
minimum	−8.850	28.50	−58.91	
maximum	4.162	52.40	−26.60	
Average treatment effect estimates				
statistic	ATE	ATT	$ATUT$	OLS
mean	−0.266	64.08	−62.26	0.578
median	−0.203	64.16	−62.30	0.764
std.dev.	1.596	1.448	1.584	2.100
minimum	−5.015	60.32	−66.64	−4.980
maximum	3.746	67.48	−57.38	6.077
Sample statistics for average treatment effects				

These results are a marked improvement of the previous, wildly erratic results. Although the average treatment effects are attenuated a bit toward zero. Inverse-Mills' results are

statistic	β_0	β_1	β_2	β_3	β_4
mean	608.9	0.432	0.435	−48.27	61.66
median	608.9	0.435	0.438	−48.55	61.60
std.dev.	1.730	0.099	0.086	2.743	3.949
minimum	603.8	0.159	0.238	−54.85	51.27
maximum	613.3	0.716	0.652	−40.70	72.70
Inverse-Mills' IV parameter estimates for $E[Y \mid s, D, \lambda] = \beta_0 + \beta_1 (1-D)(s-\overline{s}) + \beta_2 D(s-\overline{s}) + \beta_3 (1-D)\lambda^H + \beta_4 D\lambda^L + \beta_5 D$					
statistic	β_5 ($estATE$)	$estATT$	$estATUT$		
mean	−8.565	57.61	−72.28		
median	−8.353	57.44	−72.28		
std.dev.	2.282	3.294	4.628		
minimum	−15.51	48.44	−85.37		
maximum	−2.814	67.11	−60.39		
Average treatment effect estimates					
statistic	ATE	ATT	$ATUT$	OLS	
mean	−0.266	64.08	−62.26	0.578	
median	−0.203	64.16	−62.30	0.764	
std.dev.	1.596	1.448	1.584	2.100	
minimum	−5.015	60.32	−66.64	−4.980	
maximum	3.746	67.48	−57.38	6.077	
Sample statistics for average treatment effects					

These results correspond quite well with treatment effect sample statistics. The importance of strong instruments cannot be over-estimated.

Now, we report LIV-estimated average treatment effects derived from MTE with this stronger instrument, w_3.

statistic	β_1	β_2	$estATE$	$estATT$	$estATUT$
mean	0.389	0.220	−7.798	9.385	−24.68
std.dev.	0.159	0.268	9.805	14.17	16.38
minimum	0.107	−0.330	−26.85	−17.69	−57.14
maximum	0.729	0.718	11.58	37.87	−26.85
MTE parameter and average treatment effect estimates via LIV for $E[Y \mid s, D, \tau_1] = \beta_1 (s-\overline{s}) + \beta_2 D(s-\overline{s}) + \tau_1(p)$					
statistic	OLS	ATE	ATT	$ATUT$	
mean	3.609	1.593	63.76	−61.75	
median	3.592	1.642	63.91	−61.70	
std.dev.	2.484	1.894	1.546	1.668	
minimum	−3.057	−4.313	59.58	−66.87	
maximum	11.28	5.821	67.12	−58.11	
Sample statistics for average treatment effects					

Again, the results are improved relative to those with the weaker instruments but as before the average treatment effects are attenuated.[20] Average treatment on the untreated along with the average treatment effect correspond best with their sample statistics. Not surprisingly, the results are noisier than the parametric results. For this setting, we conclude that strong instruments are much more important than relaxation of distributional considerations for identifying and estimating various average treatment effects.

1.3 *Observable* continuous report precision choice

Now we consider the setting where the analyst observes a continuum of choices via the investors' conjecture of the owner's report precision $\overline{\tau} = \frac{1}{\sigma_1^2+\overline{\sigma}_2^2}$. Recall the equilibrium strategy is the fixed point where the owner's expected utility maximizing report precision equals investors' conjectured report precision $\tau = \frac{1}{\sigma_1^2+\sigma_2^2}$ or report variance $\sigma^2 = \sigma_1^2 + \sigma_2^2$. Let conjectured report variance be denoted $\overline{\sigma}^2 \equiv \sigma_1^2 + \overline{\sigma}_2^2$.Recall, the owner's expected utility is

$$EU(\sigma_2) = \mu - \beta\frac{\sigma_1^2\bar{\sigma}_2^2}{\sigma_1^2+\bar{\sigma}_2^2} - \gamma\frac{\sigma_1^4\left(\sigma_1^2+\sigma_2^2\right)}{\left(\sigma_1^2+\bar{\sigma}_2^2\right)^2} - \alpha\left(b-\sigma_2^2\right)^2 - \alpha_d\left(\hat{b}-\sigma_2^2\right)^2$$

substitution of $\overline{\sigma}^2$ for $\sigma_1^2 + \overline{\sigma}_2^2$ yields

$$EU(\sigma_2) = \mu - \beta\frac{\sigma_1^2\left(\bar{\sigma}^2-\sigma_1^2\right)}{\bar{\sigma}^2} - \gamma\frac{\sigma_1^4\sigma^2}{\bar{\sigma}^4} - \alpha\left(b-\sigma^2+\sigma_1^2\right)^2 - \alpha_d\left(\hat{b}-\sigma^2+\sigma_1^2\right)^2$$

The first order condition combined with the equilibrium condition is

$$\begin{aligned} \sigma^2 &= \frac{\alpha b + \alpha_d\widehat{b} - \gamma\frac{\sigma_1^4}{2\bar{\sigma}^4}}{\alpha+\alpha_d} \\ s.t.\ \ \sigma^2 &= \overline{\sigma}^2 \end{aligned}$$

As the outcome equation

$$\begin{aligned} Y &= P\left(\bar{\sigma}_2^2\right) = \mu + \frac{\sigma_1^2}{\sigma_1^2+\bar{\sigma}_2^2}(s-\mu) - \beta\frac{\sigma_1^2\bar{\sigma}_2^2}{\sigma_1^2+\bar{\sigma}_2^2} \\ &= P(\overline{\tau}) = \mu + \sigma_1^2(s-\mu)\overline{\tau} - \beta\sigma_1^2\left(1-\sigma_1^2\overline{\tau}\right) \end{aligned}$$

is not directly affected by the owner's report precision choice (but rather by the conjectured report precision), we exploit the equilibrium condition to define an average treatment effect on the treated[21]

$$ATT(\overline{\tau}) = E\left[\frac{\partial Y}{\partial\overline{\tau}} \mid \overline{\tau} = \tau\right] = \beta\sigma_1^4$$

[20] Reported results employ a probit regression for the selection equations (as is the case for the foregoing parametric analyses). Results based on a nonparametric regression for the treatment equation are qualitatively unchanged.

[21] As Heckman [1997] suggests the average treatment effect based on a random draw from the population of firms often doesn't address a well-posed economic question and, perhaps especially, in a continuous treatment setting.

If β differs across firms, as is likely, the outcome equation

$$Y = \left[\mu - \beta\sigma_1^2\right] + \left[\sigma_1^2\right](s-\mu)\,\overline{\tau} + \left[\beta\sigma_1^4\right]\overline{\tau}$$

is a random coefficients model with error $U = (\beta - E[\beta])\,\sigma_1^4$.

First, we report sample statistics based on 200 simulated samples of size $2,000$ for

$$E[Y \mid s, \overline{\tau}] = \beta_0 + \beta_1 (s-\mu)\,\overline{\tau} + \beta_2\overline{\tau}$$

estimated by *OLS*. Then we compare *OLS* results with *2SLS-IV* estimates. The following independent stochastic components are employed in the simulation.

α	$\sim N(0.02, 0.005)$
α_d	$\sim N(0.02, 0.005)$
γ	$\sim N(2.5, 1)$
β	$\sim N(7, 1)$
s	$\sim N(1000, \sigma)$
Stochastic components	

where σ is the optimal report standard deviation. Hence, the *DGP* is

$$Y = 300 + 100\,(s-\mu)\,\overline{\tau} + (70,000 + \varepsilon_\beta)\,\overline{\tau}$$

where $\varepsilon_\beta = \beta - E[\beta] \sim N(0,1)$.

***OLS* estimation** Results for *OLS* along with the sample statistic for *ATT* are tabulated below where β_2 is the estimate of *ATT*.

statistic	β_0	β_1	β_2 (*estATT*)	*ATT*
mean	303.6	100.0	69166.	70002.
median	303.5	101.3	69155.	69997.
std.dev.	81.23	18.37	18938	216.3
minimum	75.30	59.99	20332.	69414.
maximum	511.6	149.6	121790.	70523.
OLS parameter and average treatment effect estimates for $E[Y \mid s, \overline{\tau}] = \beta_0 + \beta_1 (s - \overline{s})\,\overline{\tau} + \beta_2\overline{\tau}$ and sample statistics for average treatment effect on treated				

While the sample interval for β_2 does overlap with the sample interval for *ATT,* the results are somewhat noisy.

***2SLS-IV* estimation** On the other hand, as suggested by Wooldridge [1997, 2003], *2SLS-IV* consistently estimates *ATT* in this random coefficients setting. We employ the residuals from regressions of $(s-\mu)\,\overline{\tau}$ and $\overline{\tau}$ on U as instruments, z_1 and z_2; these are strong instruments.[22] Results for *2SLS-IV*

[22] As widely noted in the literature, weak instruments produce poor results. Here, unreported results based on a weak instrument for $(s-\overline{s})\,\overline{\tau}$ (but maintaining the same instrument for $\overline{\tau}$ as above) are little different from those generated by *OLS* even though we might expect the instrument for $\overline{\tau}$ is most important.

along with the OLS estimate and sample statistic for ATT are below.

statistic	β_0	β_1	$\beta_2\,(estATT)$	OLS	ATT
mean	300.0	100.0	69998.	69166.	70002.
median	300.0	100.0	70001.	69155.	69997.
std.dev.	1.234	0.000	34.47	18938	216.3
minimum	297.0	100.0	69908.	20332.	69414.
maximum	303.4	100.0	70081.	121790.	70523.
2SLS-IV parameter and average treatment effect estimates for $E[Y \mid s, \overline{\tau}] = \beta_0 + \beta_1 (s - \overline{s})\,\overline{\tau} + \beta_2 \overline{\tau}$ and sample statistics for average treatment effect on treated					

Not surprisingly, these results correspond well with the DGP and the sample statistics for ATT and are far less variable than the OLS results.

1.4 Summary

The endogenous selection of report precision examples serve to highlight several key features in the econometric analysis of accounting choice. First, reliable results follow from carefully linking theory and data. For instance, who observes which data is fundamental. When the analysis demands instruments, their identification and collection is critical. Poor instruments (exclusion restriction fails) or weak instruments (weakly associated with selection) can lead to situations where the "cure" is worse than the symptom. IV results can be less reliable than OLS when faced with endogeneity if we employ faulty instruments. Finally, two (or more) poor analyses don't combine to produce one satisfactory analysis.