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`ivtreatreg`: a new STATA routine for estimating binary treatment models with heterogeneous response to treatment under observable and unobservable selection

Giovanni Cerulli

National Research Council of Italy
Ceris-CNR
Institute for Economic Research on Firms and Growth
Via dei Taurini, 19 - 00185 Roma
Phone: +39.06.4993.7885
E-mail: g.cerulli@ceris.cnr.it

Abstract. This paper presents a new user-written STATA command called `ivtreatreg` for the estimation of *five* different (binary) treatment models *with* and *without* idiosyncratic (or heterogeneous) average treatment effect. Depending on the model specified by the user, `ivtreatreg` provides consistent estimation of *average treatment effects* both under the hypothesis of "selection on observables" and "selection on unobservables" by using Ordinary Least Squares (OLS) regression in the first case, and Intrumental-Variables (IV) and Selection-model (*à la* Heckman) in the second one. Conditional on a pre-specified subset of exogenous variables **x** – thought of as driving the heterogeneous response to treatment – `ivtreatreg` calculates for each model the Average Treatment Effect (ATE), the Average Treatment Effect on Treated (ATET) and the Average Treatment Effect on Non-Treated (ATENT), as well as the estimates of these parameters conditional on the observable factors **x**, i.e., ATE(**x**), ATET(**x**) and ATENT(**x**). The five models estimated by `ivtreatreg` are: Cf-ols (Control-function regression estimated by OLS), Direct-2sls (IV regression estimated by direct two-stage least squares), Probit-2sls (IV regression estimated by Probit and two-stage least squares), Probit-ols (IV two-step regression estimated by Probit and ordinary least squares), and Heckit (Heckman two-step selection model). An extensive treatment of the conditions under which previous methods provide consistent estimation of ATE, ATET and ATENT can be found, for instance, in Wooldgrige (2002, Chapter 18). The value added of this new STATA command is that it allows for a generalization of the regression approach typically employed in standard program evaluation, by assuming *heterogeneous response to treatment*.

0. Introduction

It is nowadays common practice, especially at policymaking level, to perform ex-post evaluation of economic and social programs via evidence-based statistical analysis. This effort in mainly devoted to measure "causal effects" of an intervention on the part of an external authority (generally, local or national Government) on a set of subjects (individuals, firms, etc.) undergoing the program. But also in an environment not characterized by a formal policy intervention, rethinking usual causal relations in a counterfactual stance is becoming an imperative of the modern micro-econometric practice. In this regard, several new user-written STATA commands to accomplish the task of enlarging the set of statistical tools to perform counterfactual causal analysis have been recently realized.

This paper develops on this wake by presenting a new user-written STATA routine called `ivtreatreg` for the estimation of *five* different (binary) treatment models *with* and *without* idiosyncratic (or heterogeneous) average treatment effect. To our knowledge no previous STATA commands addressed this objective. Depending on the model specified by the user, `ivtreatreg` provides consistent estimation of *average treatment effects* both under the hypothesis of "selection on observables" and "selection on unobservables" by using Ordinary Least Squares (OLS) regression in the first case, and Intrumental-Variables (IV) and Selection-model (*à la* Heckman) in the second one. Conditional on a pre-specified subset of exogenous variables **x** – thought of as driving the heterogeneous response to treatment – `ivtreatreg` calculates for each model the Average Treatment Effect (ATE), the Average Treatment Effect on Treated (ATET) and the Average Treatment Effect on Non-Treated (ATENT), as well as the estimates of these parameters conditional on the observable factors **x**, i.e., ATE(**x**), ATET(**x**) and ATENT(**x**). The five models estimated by `ivtreatreg` are: Cf-ols (Control-function regression estimated by OLS), Direct-2sls (IV regression estimated by direct two-stage least squares), Probit-2sls (IV regression estimated by Probit and two-stage least squares), Probit-ols (IV two-step regression estimated by Probit and ordinary least squares), and Heckit (Heckman two-step selection model). An extensive treatment of the conditions under which previous methods provide consistent estimation of ATE, ATET and ATENT can be found, for instance, in Wooldgrige (2002, Chapter 18). The value added of this new STATA command is that it allows for a generalization of the regression approach typically employed in standard program evaluation, by assuming *heterogeneous response to treatment*.

Section 1, 2 and 3 put forward a brief account of definitions and statistical background needed to present in section 4 the five treatment models estimated by `ivtreatreg`. Section 5 presents and discusses the "help" of this routine, while section 6 ends the paper by providing a didactic application of `ivtreatreg` on real data for studying the relation between education and fertility on a set of women living in a developing country.

1. Treatment effect: definition and statistical set-up

From a statistical point of view, our background is that of an analyst interested in the estimation of the so-called "treatment effect" of a given policy program in a "non-experimental" set-up, where the treatment variable *w* (taking value 1 for treated and 0 for untreated units) is expected to affect a specific target variable *y* (that can have a variety of forms: binary, count, continuous, etc.). In this context, we define the unit *i*'s Treatment Effect (TE) as:

$$TE_i = y_{1i} - y_{0i}$$

where y_{1i} is the outcome of unit *i* when it is treated, and y_{0i} is the outcome of unit *i* when it is not treated. Identifying TE_i is not possible: in fact, as this quantity refers to *the same individual at the same time*, it goes without saying that the analyst can observe just *one* of the two quantities feeding into TE_i (i.e. y_{1i} or y_{0i}) but never both. For instance, it might be the case that we can observe the investment behavior of a supported company, but we cannot know what the investment of this company would have been if this firm had not been supported, and vice versa. The analyst faces a fundamental *missing observation problem* (Holland, 1986) that needs to be overcome to recover reliably the causal effect (Rubin, 1974; 1977). What on the contrary is *observable* to the analyst is the actual status of unit *i*, that is:

$$y_i = y_{0i} + w_i (y_{1i} - y_{0i})$$

This relation, called *Potential Outcome Model*, links together the treatment binary indicator, the observable and non observable outcomes. For identification purposes, the treatment evaluation literature suggests to see at a specific effect called the Average Treatment Effect (ATE) of a given policy intervention, defined (in the population) as[1]:

$$\text{Average Treatment Effect} = \text{ATE} = E(y_1 - y_0)$$

Nevertheless, a policymaker might be interested also in knowing what is the effect on the subset of units actually treated. In this case, the parameter of interest is the so called Average Treatment Effect on Treated (ATET), defined as:

$$\text{Average Treatment Effect on Treated} = \text{ATET} = E(y_1 - y_0 \mid w=1)$$

Similarly, it is also possible to define the Average Treatment Effect on Non Treated (ATENT) that is the average treatment effect calculated within the subsample of untreated units:

$$\text{Average Treatment Effect on Non Treated} = \text{ATENT} = E(y_1 - y_0 \mid w=0)$$

[1] For the sake of simplicity we avoid to write the subscript referring to unit *i* when we refer to the population parameters.

The combined knowledge of ATE, ATET and ATENT can provide relevant information on how the causal relation between w and y actually behaves. Furthermore an interesting relation links these parameters, as it can be proved that:

$$\text{ATE} = \text{ATET}\ P(w=1) + \text{ATENT}\ P(w=0)$$

where $P(w=1)$ is the probability of being treated, and $P(w=0)$ of being untreated. But another important ingredient is needed to go on with the analysis of program evaluation. Indeed, for each individual, besides the observation on y and w, analysts (normally) have access also to a certain number of observable covariates that can be collected in a row vector $\mathbf{x}$. Usually, the $\mathbf{x}$-variables represent various individual characteristics such as: age, gender, income, etc.. The knowledge of $\mathbf{x}$-variables, as we will see, is of primary usefulness in the estimation phase of previous parameters, as they represent essential *observable confounding conditioning factors*. It is then worth stressing that, under the knowledge of $\mathbf{x}$, we can also define the previous parameters "conditional on $\mathbf{x}$", as:

$$\text{ATE}(\mathbf{x}) = \text{E}(y_1 - y_0 \mid \mathbf{x})$$
$$\text{ATET}(\mathbf{x}) = \text{E}(y_1 - y_0 \mid w=1, \mathbf{x})$$
$$\text{ATENT}(\mathbf{x}) = \text{E}(y_1 - y_0 \mid w=0, \mathbf{x})$$

These quantities are, by definition, no more single values as before but functions of $\mathbf{x}$. It means that they can also be seen as "individual specific average treatment effects" as each individual owns a different and specific value of $\mathbf{x}$. Furthermore, it comes from the Law of Iterated Expectations that:

$$\text{ATE} = \text{E}_{\mathbf{x}}\{\text{ATE}(\mathbf{x})\}$$
$$\text{ATET} = \text{E}_{\mathbf{x}}\{\text{ATET}(\mathbf{x})\}$$
$$\text{ATENT} = \text{E}_{\mathbf{x}}\{\text{ATENT}(\mathbf{x})\}$$

The aim of the econometrician involved into program evaluation is to recover *consistent* (and, when possible *efficient*) estimators of the previous parameters from observational data, that is from an i.i.d. sample of observed variables for each individual i:

$$\{y_i, w_i, \mathbf{x}_i\} \text{ with } i = 1, \ldots, N$$

Observe that, according to this set-up, we exclude the possibility that the treatment of one unit affects the outcome of another unit. In the literature this is called SUTVA (or *stable unit treatment value assumption*), and we will assume the validity of this hypothesis throughout this paper.

2. Random and non-random assignment

If the sample were drawn at random (*random assignment to program*), it can be showed that ATE=ATET=ATENT and, more importantly, it is possible to estimate ATE as the difference between the sample mean of treated and the sample mean of untreated units: this is the well-known "difference-in-mean estimator" of classical statistics. Indeed, under random assignment, the so-called Independence Assumption (IA), stating that "$(y_1; y_0)$ are independent of w", does hold and the "difference-in-mean" estimator is *consistent*, *efficient* and *asymptotically normal*.

When the sample of treated and untreated units, as it is often the case, is *not randomly drawn*, but it depends on either individual *observable* as well as *unobservable* to analyst characteristics, the difference-in-mean estimator is no longer a consistent estimation strategy. In this case, in fact, it occurs that "$(y_1; y_0)$ are dependent on w" so that a *selection bias* arises and it can be also proved that ATE≠ATET≠ATENT.

What determines *selection bias* in program evaluation settings are basically to mechanisms: (i) the *self-selection* of individuals on the one hand, and (ii) the *selection* procedure from an external actor, on the other hand. Under "selection on observables" the knowledge of **x** may be sufficient to identify previous causal parameters. Self-selection regards the choice of the individuals to apply for a specific program. This entails a cost-benefit calculus, as applying for a policy program can be costly to some extent. This choice may not be assumed to be done at random, as firms are *endogenously* involved in this decision. The selection mechanism is more intuitively following a non random assignment, as a public agency is generally characterized by the pursuit of various objectives, such as *direct* (on the target-variable) and *indirect* (welfare) objectives. For instance , in order to maximize the final effect of an investment supporting program, the agency could apply the principle of "picking-the-winner", that is choosing to support those units having an already high propensity to succeed. This is a sufficient condition to make the sample of beneficiaries far from being randomly built.

3. Selection on observables and selection on unobservables

3.1 Selection on observables

On the part of the evaluator, the factors affecting the non random assignment of beneficiaries could have an *observable* or an *unobservable* nature. In the first case the analyst knows with precision what are the elements driving the self-selection of individuals and the selection of the agency. In this case the knowledge of **x**, the structural variables that are supposed to drive the non-random assignment to treatment, are sufficient to identify, as we will see later, the actual effect of the policy in question. Nevertheless, when other factors driving the non random assignment are impossible or difficult to observe, then the only knowledge of the vector **x** is not sufficient to identify the effect of the policy.

These two situations faced by the evaluator are known in the literature as the case of "selection on observable" and "selection on unobservables": they ask for different methodologies to identify the actual effect of policy programs, and the greatest effort of past and current econometric

literature has been that of dealing with these two situations and provide suitable solutions in both cases.

Under selection on observables the knowledge of **x**, the factors driving the non-random assignment, may be sufficient to identify the causal parameters defined above. Of course, since the missing observation problem still holds, we need to rely on an assumption (or hypothesis) able to overcome that problem. Rosenbaum and Rubin (1983), introduced the so-called *Conditional Independence Assumption* (CIA), stating that - conditional on the knowledge of **x** - y_1 and y_0 are independent of *w*, formally:

$$(y_0, y_1) \perp w \mid \mathbf{x}$$

This assumption means that, once the knowledge of the factors affecting the sample selection are taken into account by the analyst, then the condition of randomization is restored. This assumption can be restricted to the so-called *Conditional Mean Independence* (CMI), stating that:

$$\mathrm{E}(y_1 \mid \mathbf{x}, w) = \mathrm{E}(y_1 \mid \mathbf{x}) \quad \text{and} \quad \mathrm{E}(y_0 \mid \mathbf{x}, w) = \mathrm{E}(y_0 \mid \mathbf{x})$$

that restricts the independence only on the mean. The CMI is the basis for (consistent) estimation of ATE, ATET and ATENT by parametric and non parametric methods. Within the parametric approaches the regression analysis is the most known and applied, while within the non-parametric ones the Matching methods and Reweighting are the most popular. But also the Sharp Regression Discontinuity Design brings to consistent estimation under CMI.

3.2 Selection on unobservables

When the selection into program is governed not only by observable-to-analyst factors, but also by unobservable variables, the CMI is not sufficient to identify causal parameters. Other assumptions are needed. Two classes of models are particularly suitable in this case: Selection model (*à la* Heckman) also known as Heckit Model and the Instrumental Variables (IV) approach[2]. Before going on, it is worth to distinguish between "genuine unobservables" and "contingent unobservables": the first type refers to factors that are intrinsically unknowable to the analyst as, for instance, some individual specific characteristics such as personal ability, propensity to bear risk, etc.; the second type refers to factors that, in principle, would be knowable, but that the available set of information prevents to employ. In many policy contexts the presence of contingent

[2] Furthermore, also the Fuzzy Regression Discontinuity Design can deal with selection on unobservables, as it can be proved that it is a particular kind of IV estimator. Finally, also the Difference-In-Differences (DID) estimator is able to treat unobservable selection, but it needs the availability of longitudinal data.

unobservables could be very problematic, as many (potentially observable) elements driving the selection into program could be overlooked, thus leaving the selection bias still present[3].

4. Estimation methods

The new STATA routine `ivtreaterg` implements the estimation of five models, where three of them are particular IV estimators. These methods are called: *cf-ols* (Control-function regression estimated by OLS), *direct-2sls* (IV regression estimated by direct two-stage least squares), *probit-2sls* (IV regression estimated by Probit and two-stage least squares), *probit-ols* (IV two-step regression estimated by Probit and ordinary least squares), and *heckit* (Heckman two-step selection model). Each of these can be estimated either by assuming *homogenous* or *heterogeneous* response to treatment (for a total of ten models). Before presenting how `ivtreaterg` actually works, the identification conditions, procedures and formulas of each model are briefly set out.

4.1 Control-function regression

To estimate Rosenbaum and Rubin (1983), introduced the so-called *Conditional Independence Assumption* (CIA), stating that - conditional on the knowledge of x - y_1 and y_0 are independent of w. This assumption means that, once the knowledge of the factors affecting the sample selection are taken into account, the condition of randomization is restored. This assumption can be restricted to the so-called *Conditional Mean Independence* (CMI), stating that:

$$E(y_1 \mid \mathbf{x}, w) = E(y_1 \mid \mathbf{x}) \quad \text{and} \quad E(y_0 \mid \mathbf{x}, w) = E(y_0 \mid \mathbf{x})$$

that restricts the independence only on the mean. Suppose to modeling the potential outcomes as follows:

(a) $y_0 = \mu_0 + v_0$, $E(v_0) = 0$, $\mu_0 = parameter$
(b) $y_1 = \mu_1 + v_1$, $E(v_1) = 0$, $\mu_1 = parameter$
(c) $y = y_0 + w\,(y_1 - y_0)$
(d) *CMI holds*

By substituting (a) and (b) into (c) we get:

$$y = \mu_0 + w\,(\mu_1 - \mu_0) + v_0 + w\,(v_1 - v_0)$$

By assuming $E(v_0 \mid \mathbf{x}) = g_0(\mathbf{x}) = \mathbf{x}\boldsymbol{\beta}_0$ and $E(v_1 \mid \mathbf{x}) = g_1(\mathbf{x}) = \mathbf{x}\boldsymbol{\beta}_1$ we can distinguish two case:

[3] In the case of firm R&D and fixed investment support, many studies have a lot of information about firm characteristics, but very little about R&D projects' quality. As selection is led by both these aspects, these studies run the risk to be severely biased (Cerulli, 2010).

Case 1. Homogenous reaction function of y_0 and y_1 to $\mathbf{x}$: $E(v_1|\,\mathbf{x}) = E(v_0|\,\mathbf{x})$
In Case 1 we can show that:

(1) $E(y|\,w, \mathbf{x}) = \mu_0 + w\,\text{ATE} + \mathbf{x}\boldsymbol{\beta}$
(2) $\text{ATE} = \text{ATE}(\mathbf{x}) = \text{ATET} = \text{ATET}(\mathbf{x}) = \text{ATENT} = \text{ATENT}(\mathbf{x}) = \mu_1 - \mu_0$

Thus, no heterogeneous average treatment effect (over $\mathbf{x}$) does exist.
Case 2. Heterogeneous reaction function of y_0 and y_1 to $\mathbf{x}$: $E(v_1|\,\mathbf{x}) \neq E(v_0|\,\mathbf{x})$
In this second case it can be showed that:

(1) $E(y|\,w, \mathbf{x}) = \mu_0 + w\,\text{ATE} + \mathbf{x}\boldsymbol{\beta}_0 + w\,(\mathbf{x} - \boldsymbol{\mu}_\mathbf{x})\boldsymbol{\beta}$
(2) $\text{ATE} \neq \text{ATET} \neq \text{ATENT}$

where an estimator for $\boldsymbol{\mu}_\mathbf{x}=E(\mathbf{x})$ can be the simple sample mean of $\mathbf{x}$. In this case, heterogeneous average treatment effects (over $\mathbf{x}$) exists and the population causal parameters take on the following form:

$$\text{ATE} = (\mu_1 - \mu_0) + \boldsymbol{\mu}_\mathbf{x}\boldsymbol{\beta}$$
$$\text{ATE}(\mathbf{x}) = \text{ATE} + (\mathbf{x} - \boldsymbol{\mu}_\mathbf{x})\boldsymbol{\beta}$$
$$\text{ATET} = \text{ATE} + E_\mathbf{x}\{\mathbf{x} - \boldsymbol{\mu}_\mathbf{x} \mid w=1\}\boldsymbol{\beta}$$
$$\text{ATET}(\mathbf{x}) = [\text{ATE} + (\mathbf{x} - \boldsymbol{\mu}_\mathbf{x})\boldsymbol{\beta}|\, w=1]$$
$$\text{ATENT} = \text{ATE} + E_\mathbf{x}\{\mathbf{x} - \boldsymbol{\mu}_\mathbf{x} \mid w=0\}\boldsymbol{\beta}$$
$$\text{ATENT}(\mathbf{x}) = [\text{ATE} + (\mathbf{x} - \boldsymbol{\mu}_\mathbf{x})\boldsymbol{\beta}|\, w=0]$$

whose sample equivalents are:

$$\hat{\text{ATE}} = \hat{\alpha}$$
$$\hat{\text{ATE}}(\mathbf{x}) = \hat{\alpha} + (\mathbf{x} - \bar{\mathbf{x}})\hat{\boldsymbol{\beta}}$$
$$\hat{\text{ATET}} = \hat{\alpha} + \frac{1}{\sum_{i=1}^{N} w_i}\sum_{i=1}^{N} w(\mathbf{x} - \bar{\mathbf{x}})\hat{\boldsymbol{\beta}}$$
$$\hat{\text{ATET}}(\mathbf{x}) = \left[\hat{\alpha} + (\mathbf{x} - \bar{\mathbf{x}})\hat{\boldsymbol{\beta}}\right]_{(w=1)}$$
$$\hat{\text{ATENT}} = \hat{\alpha} + \frac{1}{\sum_{i=1}^{N}(1 - w_i)}\sum_{i=1}^{N}(1 - w_i)(\mathbf{x}_i - \bar{\mathbf{x}})\hat{\boldsymbol{\beta}}$$
$$\hat{\text{ATENT}}(\mathbf{x}_i) = \left[\hat{\alpha} + (\mathbf{x}_i - \bar{\mathbf{x}})\hat{\boldsymbol{\beta}}\right]_{(w=0)}$$

Operationalizing regression in Case 2 is fairly straightforward:

1. estimate $y_i = \mu_0 + w_i \alpha + \mathbf{x}_i\boldsymbol{\beta}_0 + w_i (\mathbf{x}_i - \boldsymbol{\mu}_\mathbf{x})\boldsymbol{\beta} + error_i$ by OLS, thus getting consistent estimates of μ_0 , α, $\boldsymbol{\beta}_0$ and $\boldsymbol{\beta}$;
2. plug these estimated parameters into the sample formulas and recover all the causal effects.
3. Obtain standard errors for ATET and ATENT via *bootstrapping*.

4.2 Instrumental variables

When the CMI hypothesis does not hold, Control-function regression brings to *biased* estimates of ATE, ATET and ATENT. This happens when the selection-into-treatment is due not only to observable, but also "unobeservable-to-analyst" factors. In this case, *w* becomes endogenous, that is correlated with the regression error term. Instrumental-variables estimation (hereafter, IV) solves this problem by restoring costincency also under the hypothesis of *selection on unobservables*. Nevertheless, the application of IV requires the availability of at least one variable *z*, called "instrumental variable", assumed to have the following two properties:

(1) *z* is (directly) correlated with treatment *w*
(2) *z* is (directly) uncorrelated with outcome *y*.

This means that the selection into program depends on the same factors affecting the outcome *plus z* that does not affect directly the outcome (but only indirectly via its effect on *w*). This is the basic *exclusion restriction* under which IV is able to identify casual parameters.
Now, consider again the *switching random coefficient model*:

$$y = \mu_0 + w (\mu_1 - \mu_0) + v_0 + w (v_1 - v_0)$$

when CMI does not hold we have that $E(v_1| w, \mathbf{x}) \neq E(v_1| \mathbf{x})$ and $E(v_0| w, \mathbf{x}) \neq E(v_0| \mathbf{x})$. As in the case of control-function, we can distinguish these two cases.

Case 1. $v_1 = v_0$ (homogenous case)
In this case $v_1 = v_0$ so that $y = \mu_0 + w (\mu_1 - \mu_0) + v_0$ implying that ATE=ATET=ATENT= $\mu_1 - \mu_0$.
Suppose to have access to a variable *z* (instrumental variable) having these two properties:

(1) $E(v_0| \mathbf{x}, z) = E(v_0| \mathbf{x})$ <=> *z* is uncorrelated with v_0
(2) $E(w| \mathbf{x}, z) \neq E(w| \mathbf{x})$ <=> *z* is correlated with *w*

Taking (1), we assume that: $E(v_0| \mathbf{x}, z) = E(v_0| \mathbf{x}) = g(\mathbf{x}) = \mathbf{x}\boldsymbol{\beta}$ meaning means that $E(v_0| \mathbf{x}, z) \neq 0$. After simple manipulations, we get a regression model having a error term with zero unconditional mean of this type:

$$y = \mu_0 + w \text{ ATE} + \mathbf{x}\boldsymbol{\beta} + u_0$$

that is a regression model in which (**x**, *z*) are uncorrelated with the error term u_0 (i.e., (**x**, *z*) are exogenous) but the error term u_0 is correlated with *w*. These conditions bring to the following *Structural System of (two) Equations*:

$$\begin{cases} \text{(a)} \quad y_i = \mu_0 + w_i \text{ATE} + \mathbf{x}_i\boldsymbol{\beta} + u_{0i} \\ \text{(b)} \quad w_i^* = \eta + \mathbf{q}_i\boldsymbol{\delta} + \varepsilon_i \\ \text{(c)} \quad w_i = \begin{cases} 1 \quad if \quad w_i^* \geq 0 \\ 0 \quad if \quad w_i^* < 0 \end{cases} \\ \text{(d)} \quad \mathbf{q}_i = (\mathbf{x}_i, z_i) \end{cases}$$

where ATE cannot be consistently estimated by OLS because conditions Cov(u_{0i} ; ε_i) ≠ 0 i.e., *w* is *endogenous* in equation (a). Equation (a) is known as the *outcome equation*, equation (b) and (c) is known as the *selection equation* and relation (d) is the *exclusion restriction*. How can we estimate consistently ATE in System (11)? We may rely on three (consistent, but differently efficient) methods:

1. Direct Two-Stage-Least-Squares (2SLS)
2. Probit-2SLS
3. Probit-OLS

Direct Two-Stage-Least-Squares (Direct-2SLS)
By using direct-2SLS the analyst does not consider at all the binary nature of *w*. It follows two steps:

1. run an OLS regression of *w* on **x** and *z* of the type: $w_i = \eta + \mathbf{x}_i\boldsymbol{\delta}_{\mathbf{x}} + z_i\delta_z + error_i$, thus getting the "predicted values" of w_i, that we indicate with $w_{fv,i}$;
2. run a second OLS of *y* on **x** and $w_{fv,i}$. The coefficient of $w_{fv,i}$ is a consistent estimation of ATE.

Probit-2SLS
In this case, the analyst exploits suitably the *binary* nature of *w*: first he applies a Probit of *w* on **x** and *z*, getting the "predicted probability of *w*", and then he uses these probabilities by applying a 2SLS with predicted probabilities as instrument for *w*.
Probit-2SLS is generally *more efficient* than Direct-2SLS. Among all the possible instruments for *w*, the optimal one is the *orthogonal projection* of *w* in the vector space generated by (**x**, *z*). Why and which is this projection? E(*w* | **x**, *z*) is the *orthogonal projection* of *w* in the vector space generated by (**x**, *z*). Among all the projections, the orthogonal one produces the "smallest error". But we know that E(*w* | **x**, *z*) = P(*w*=1 | **x**, *z*) = *Probit selection equation*. It means that the "probabilities of getting treated" (i.e., the propensity scores) estimated from the Selection Equation

is the *best instrument* for w (because it generates the smallest projection error). Operationally, Probit-2SLS follows these three steps:

1. apply a Probit of w on $\mathbf{x}$ and z, getting p_w, i.e., the "predicted probability of w";
2. run OLS of w on (1, $\mathbf{x}$, p_w), thus getting the fitted values $w_{2fv,i}$;
3. run a second OLS of y on (1, $\mathbf{x}$, $w_{2fv,i}$).

The coefficient of $w_{2fv,i}$ is the *most efficient* estimator of ATE in the class of linear instruments for w. Furthermore, this procedure *does not* require for consistency that the process generating w is correctly specified.

Probit-OLS

This method exploits the previous relation $E(w \mid \mathbf{x}, z) = P(w=1 \mid \mathbf{x}, z)$. By tacking the expectation of y conditional on ($\mathbf{x}$, z), we get:

$$E(y \mid \mathbf{x}, z) = \mu_0 + \text{ATE} \cdot E(w \mid \mathbf{x}, z) + \mathbf{x}\boldsymbol{\beta}$$

Since we saw that $E(u_0 \mid \mathbf{x}, z) = 0$. By plug-in (12) into the previous equation we have:

$$E(y \mid \mathbf{x}, z) = \mu_0 + \text{ATE} \cdot P(w=1 \mid \mathbf{x}, z) + \mathbf{x}\boldsymbol{\beta}$$

This relation suggests to estimate consistently ATE with a simple OLS regression of y on (1, p_w, $\mathbf{x}$). This model, however, is less efficient than Probit-2SLS and requires for consistency that the Probit is "correctly" specified. Standard errors have to be corrected for the presence of a "generated regressor" and "heteroscedasticity".

From a technical point of view, in order to identify (μ_0, ATE, $\boldsymbol{\beta}$) in equation (1), it not necessary to introduce z in the selection equation (2). It is sufficient that the selection equation (2) contains just $\mathbf{x}$. Indeed, since G($\mathbf{x}$, $\boldsymbol{\delta}$) is a *non-linear function* of $\mathbf{x}$, then it is not perfectly collinear with $\mathbf{x}$. Therefore, G($\mathbf{x}$, $\boldsymbol{\delta}$) can be used as instrument besides $\mathbf{x}$, as it does not produce problems of collinearity (as it occurs, conversely, if G is a *linear probability model*). Nevertheless, since $\mathbf{x}$ and G($\mathbf{x}$, $\boldsymbol{\delta}$) are strongly correlated and are used jointly as instruments, it can be proved that the IV estimator gets larger variances, thereby becoming more imprecise.

Case 2. $v_1 \neq v_0$

Consider now the case in which: $v_1 \neq v_0$ so that: $y = \mu_0 + w\,(\mu_1 - \mu_0) + v_0 + w\,(v_1 - v_0)$. As in the case of Control-Function, it implies that ATE ≠ ATET ≠ ATENT. We are in the case of observable heterogeneity and ATE($\mathbf{x}$), ATET($\mathbf{x}$) and ATENT($\mathbf{x}$) can be defined and estimated. Suppose that v_1 and v_0 are independent on z: it means that z is assumed to be endogenous in this model, that is:

$E(v_0 | \mathbf{x}, z) = E(v_0 | \mathbf{x}) = g_0(\mathbf{x})$
$E(v_1 | \mathbf{x}, z) = E(v_1 | \mathbf{x}) = g_1(\mathbf{x})$

It is equivalent to write:

$v_0 = g_0(\mathbf{x}) + e_0 \quad \text{with} \quad E(e_0 | \mathbf{x}, z) = 0$
$v_1 = g_1(\mathbf{x}) + e_1 \quad \text{with} \quad E(e_1 | \mathbf{x}, z) = 0$

By substituting these expressions for v_0 and v_1 into the previous *switching regression* for y, we get:

$$y = \mu_0 + \alpha w + g_0(\mathbf{x}) + w[g_1(\mathbf{x}) - g_0(\mathbf{x})] + e_0 + w(e_1 - e_0)$$

Now, by assuming in the previous equation: $g_0(\mathbf{x}) = \mathbf{x}\boldsymbol{\beta}_0$, $g_1(\mathbf{x}) = \mathbf{x}\boldsymbol{\beta}_1$, $\varepsilon = e_0 + w(e_1 - e_0)$ and by applying the same procedure of case 1, we finally get:

$$y = \mu_0 + \text{ATE}w + \mathbf{x}\boldsymbol{\beta}_0 + w\,(\mathbf{x} - \boldsymbol{\mu}_\mathbf{x})\boldsymbol{\beta} + \varepsilon$$

In this model we have two endogenous variables: w and $w(\mathbf{x} - \boldsymbol{\mu}_\mathbf{x})$. Intuitively, if $q = q(\mathbf{x}, z)$ is an instrument for w, then a suitable instrument for $w(\mathbf{x} - \boldsymbol{\mu}_\mathbf{x})$ is: $q \cdot (\mathbf{x} - \boldsymbol{\mu}_\mathbf{x})$. Nevertheless, before applying IV, we have to distinguish *twosub-cases*:

Case 2.1: $e_1 = e_0$ (only *observable heterogeneity*)
Case 2.2: $e_1 \neq e_0$ (both *observable* and *unobservable heterogeneity*)

In what follows we examine the two cases separately.

Case 2.1: $e_1 = e_0$ (only observable heterogeneity)
In this case we have $\varepsilon = e_0$. By remembering that $E(e_0 | \mathbf{x}, z) = 0$ we can conclude that:

$$y = \mu_0 + \alpha w + \mathbf{x}\boldsymbol{\beta}_0 + w\,(\mathbf{x} - \boldsymbol{\mu}_\mathbf{x})\boldsymbol{\beta} + e_0 \ , \ \text{with} \ E(e_0 | \mathbf{x}, z, w) = E(e_0 | w)$$

meaning that what is remaining is just the *endogeneity* due to w. Therefore, the following procedure provides *consistent* estimation:

1. apply a Probit of w on $\mathbf{x}$ and z, getting p_w, i.e., the "predicted probability of w";
2. estimate the following equation: $y_i = \mu_0 + \alpha w_i + \mathbf{x}_i\boldsymbol{\beta}_0 + w_i\,(\mathbf{x}_i - \boldsymbol{\mu}_\mathbf{x})\boldsymbol{\beta} + error_i$ using as instruments: $1, p_w, \mathbf{x}_i, p_w\,(\mathbf{x}_i - \boldsymbol{\mu}_\mathbf{x})$.

This procedure provides *consistent* and *efficient* estimations. Moreover, various functions and interactions of (**x**, *z*) can be used to generate *additional instruments*, in order to get *over-identification*, and thus test the (joint) exogeneity of instruments.

Case 2.2: $e_1 \neq e_0$ *(both observable and unobservable heterogeneity)*
In this case, as seen, the full (and more general) model is:

$$y = \mu_0 + \alpha w + g_0(\mathbf{x}) + w[g_1(\mathbf{x}) - g_0(\mathbf{x})] + e_0 + w(e_1 - e_0)$$

and we have to find a condition to restore consistent estimation. A possible condition could be: $E[w(e_1 - e_0) \mid \mathbf{x}, z] = E[w(e_1 - e_0)]$. Given this condition, and by applying previous procedures, we arrive to the following parametric equation for *y*:

$$y = \mu_0 + \alpha w + \mathbf{x}\boldsymbol{\beta}_0 + w\,(\mathbf{x} - \boldsymbol{\mu}_\mathbf{x})\boldsymbol{\beta} + e_0 + w(e_1 - e_0)$$

By defining:

$$r = w(e_1 - e_0) - E[w(e_1 - e_0)]$$

and by adding and subtracting $E[w(e_1 - e_0)]$ in the previous equation for *y*, we get:

$$y = \eta + \alpha w + \mathbf{x}\boldsymbol{\beta}_0 + w\,(\mathbf{x} - \boldsymbol{\mu}_\mathbf{x})\boldsymbol{\beta} + e_0 + r$$

where $\eta = \mu_0 + E[w(e_1 - e_0)]$. It is immediate to see that $E(e_0 + r \mid \mathbf{x}, z) = 0$. It means that any function of (**x**, *z*) can be used as instrument in the *y*-equation. It brings to apply the IV procedure identical to that for Case 2.1, that is, estimate:

$$y_i = \eta + \alpha w_i + \mathbf{x}_i\boldsymbol{\beta}_0 + w_i\,(\mathbf{x}_i - \boldsymbol{\mu}_\mathbf{x})\boldsymbol{\beta} + error_i$$

using as instruments: 1, p_w, $\mathbf{x}_i$, $p_w\,(\mathbf{x}_i - \boldsymbol{\mu}_\mathbf{x})$. This IV estimator is *consistent*, but *not efficient*. To get an efficient estimation it needs to introduce additional hypotheses. There are recent contributions, using more or less parametric approaches, to restore efficiency. In what follow we focus on the Heckit model with *unobservable heterogeneity*. It is a strong parametric model, but it may be useful sometimes to get efficient estimation. We will treat this model in the part on "Selection Models".

Problems with IV
The main drawback of IV approaches regards the availability of good instruments. To be good an instrument has to be:

1. exogenous for the outcome *y*
2. sufficiently well correlated with *w*

If one of these two conditions is not met, the correctness of IV estimation is questionable. Usually, it is fairly difficult to find a variable that explains the selection-into-program having, at the same time, no relation with the outcome. When such a variable is available, anyways, its *exogeneity* is not easily testable. Indeed, testing instruments' exogeneity requires to rely on an *over-identified* setting, that is, to get access to more than one instrument for *w* (at least two). Observe that, in this case, the analyst can test only the *joint exogeneity* of all the instruments used and not that of each single instrument. In the case of *just-identified* settings (only one instrument for *w*), testing instrument's exogeneity is not possible, and analysts normally have to discuss very carefully the suitability of the instrument adopted.

4.3 Selection model

From the IV-estimation section, in the Case 2.2, we had that:

$$y = \mu_0 + \alpha w + g_0(\mathbf{x}) + w[g_1(\mathbf{x}) - g_0(\mathbf{x})] + e_0 + w(e_1 - e_0)$$

and after some manipulations:

$$y = \mu_0 + \alpha w + \mathbf{x}\boldsymbol{\beta}_0 + w\,(\mathbf{x} - \boldsymbol{\mu}_\mathbf{x})\boldsymbol{\beta} + e_0 + w(e_1 - e_0)$$

This model, as said, presents both observable and unobservable heterogeneity, and a consistent estimation in this case requires strong hypotheses (see the IV section). Nevertheless, we ca use a generalized Heckit model to estimate consistently and efficiently such a model. The prize is that of relying on some distributional hypotheses.
The model is made of these assumptions:

1. $y = \mu_0 + \alpha w + \mathbf{x}\boldsymbol{\beta}_0 + w\,(\mathbf{x} - \boldsymbol{\mu}_\mathbf{x})\boldsymbol{\beta} + u$
2. $E(e_1 \mid \mathbf{x}, z) = E(e_0 \mid \mathbf{x}, z) = 0$
3. $w = 1[\theta_0 + \boldsymbol{\theta}_1\mathbf{x} + \theta_2 z + a \geq 0]$
4. $E(a \mid \mathbf{x}, z) = 0$
5. $(a, e_0, e_1) \sim {}^3N$
6. $a \sim N(0,1) \Rightarrow \sigma_a = 1$
7. $u = e_0 + w(e_1 - e_0)$

Given these starting conditions, we can directly calculate to what is equal $E(y \mid \mathbf{x}, z, w)$. To that end, write the *y*-equation as $y = A + u$, with $A = \mu_0 + \alpha w + \mathbf{x}\boldsymbol{\beta}_0 + w\,(\mathbf{x} - \boldsymbol{\mu}_\mathbf{x})\boldsymbol{\beta}$ and $u = e_0 + w(e_1 - e_0)$.
It can be proved that:

$$\mathrm{E}(y \mid \mathbf{x}, z, w) = \mu_0 + \alpha w + \mathbf{x}\boldsymbol{\beta}_0 + w\,(\mathbf{x} - \boldsymbol{\mu}_{\mathbf{x}})\boldsymbol{\beta} + \rho_1 w \frac{\phi(\mathbf{q}\boldsymbol{\theta})}{\Phi(\mathbf{q}\boldsymbol{\theta})} + \rho_0 (1-w) \frac{\phi(\mathbf{q}\boldsymbol{\theta})}{1-\Phi(\mathbf{q}\boldsymbol{\theta})}$$

where $\rho_1 = \sigma_{e_1}\sigma_{a,e_1}$ and $\rho_0 = \sigma_{e_0}\sigma_{a,e_0}$. For the estimation of this equation a *two-step* procedure can be performed:

1. run a Probit regression of w_i on $(1, \mathbf{x}_i, z_i)$ and gets: $(\hat{\phi}_i, \hat{\Phi}_i)$;
2. run an OLS of y_i on $\left[1, w_i, \mathbf{x}_i, w_i(\mathbf{x}_i - \boldsymbol{\mu}_{\mathbf{x}})_i, w_i \frac{\hat{\phi}_i}{\hat{\Phi}_i}, (1-w_i)\frac{\hat{\phi}_i}{1-\hat{\Phi}_i}\right]$

The previous two-step procedure produces *consistent* and *efficient* estimations. Given estimations, we can also test the hypothesis:

$$\mathrm{H}_0: \ \rho_1 = \rho_0 = 0$$

that, if accepted, brings to the conclusion of *no selection on unobservables*. Finally, by putting:

$$\lambda_1(\mathbf{q}\boldsymbol{\theta}) = \frac{\phi(\mathbf{q}\boldsymbol{\theta})}{\Phi(\mathbf{q}\boldsymbol{\theta})} \quad \text{and} \quad \lambda_0(\mathbf{q}\boldsymbol{\theta}) = \frac{\phi(\mathbf{q}\boldsymbol{\theta})}{1-\Phi(\mathbf{q}\boldsymbol{\theta})}$$

we can write the regression as:

$$\mathrm{E}(y \mid \mathbf{x}, z, w) = \mu_0 + \alpha w + \mathbf{x}\boldsymbol{\beta}_0 + w\,(\mathbf{x} - \boldsymbol{\mu}_{\mathbf{x}})\boldsymbol{\beta} + \rho_1 w \lambda_1(\mathbf{q}\boldsymbol{\theta}) + \rho_0 (1-w)\lambda_0(\mathbf{q}\boldsymbol{\theta})$$

Given the two-step estimation of the previous equation, once recovered all the parameters , it is possible to calculate the usual *causal parameters*. It is immediate to see, that:

$$ATE = \alpha$$
$$ATE(\mathbf{x}) = \alpha + (\mathbf{x} - \bar{\mathbf{x}})\boldsymbol{\beta}$$

Since it follows the same procedure as seen in the case of Control-function Case 2. Nevertheless, ATET(**x**), ATET, ATENT(**x**) and ATENT assume a different form compared to Control-function Case 2. It is immediate to show that:

$$ATET(\mathbf{x}) = \left[\alpha + (\mathbf{x} - \bar{\mathbf{x}})\boldsymbol{\beta} + (\rho_1 + \rho_0)\cdot\lambda_1(\mathbf{q}\boldsymbol{\theta})\right]_{(w=1)}$$
$$ATET = \alpha + \frac{1}{\sum_{i=1}^{N} w_i}\sum_{i=1}^{N} w_i(\mathbf{x}_i - \bar{\mathbf{x}})\boldsymbol{\beta} + (\rho_1 + \rho_0)\cdot\frac{1}{\sum_{i=1}^{N} w_i}\sum_{i=1}^{N} w_i \cdot \lambda_1(\mathbf{q}\boldsymbol{\theta})$$

and:

$$ATENT(\mathbf{x}) = \left[\alpha + (\mathbf{x} - \bar{\mathbf{x}})\hat{\boldsymbol{\beta}} + (\rho_1 + \rho_0)\cdot\lambda_0(\mathbf{q}\hat{\boldsymbol{\theta}})\right]_{(w=1)}$$

$$ATENT = \alpha + \frac{1}{\sum_{i=1}^{N}(1-w_i)}\sum_{i=1}^{N}(1-w_i)(\mathbf{x}_i - \bar{\mathbf{x}})\boldsymbol{\beta} - (\rho_1 + \rho_0)\cdot\frac{1}{\sum_{i=1}^{N}(1-w_i)}\sum_{i=1}^{N}(1-w_i)\cdot\lambda_{0i}(\mathbf{q}\boldsymbol{\theta})$$

Given the estimation of α, ρ_1, ρ_0, $\boldsymbol{\beta}$, λ_1, λ_0 from the previous *two-step* procedure, all these causal effects can be calculated. Standard errors for ATET and ATENT can be obtained by bootstrapping.

5. The STATA command `ivtreatreg`

The STATA routine `ivtreatreg` estimates the five binary treatment models presented above, *with* and *without* idiosyncratic (or heterogeneous) average treatment effect. Depending on the model specified, `ivtreatreg` provides consistent estimation of Average Treatment Effects either under the hypothesis of "selection on observables" (using the Control-function regression) or "selection on unobservables" (by using one of the three Intrumental-Variables (IV) models or the Heckman's Selection-Model). Conditional on a pre-specified subset of exogenous variables - thought of as those driving the heterogeneous response to treatment - `ivtreatreg` calculates for each specific model the Average Treatment Effect (ATE), the Average Treatment Effect on Treated (ATET) and the Average Treatment Effect on Non-Treated (ATENT), as well as the estimates of these parameters conditional on the observable factors **x** (i.e., ATE(**x**), ATET(**x**) and ATENT(**x**)).

The syntax of the command is fairly simple and takes on this form:

Syntax of ivtreatreg

ivtreatreg *outcome treatment* [*varlist*] [*if*] [*in*] [*weight*], **model(***modeltype***)**
[**hetero(***varlist_h***) iv(***varlist_iv***) conf(***number***) graphic vce(robust) const(noconstant) head(noheader)**]

fweights, **iweights**, and **pweights** are allowed; see weight.

where:

outcome specifies the target variable that is the object of the evaluation.

treatment specifies the binary (i.e. taking 0=treated or 1=untreated) treatment variable.

varlist defines the list of exogenous variables that are considered as observable confounders.

The present routine allows for specifying a series of convenient options of different importance:

Options of ivtreatreg

model(*modeltype***)** specifies the treatment model to be estimated, where *modeltype* must be one of the following (and abovementioned) five models: "cf-ols", "direct-2sls", "probit-2sls", "probit-ols", "heckit". It is always required to specify one model.

modeltype_options	description
Modeltype	
cf-ols	Control-function regression estimated by ordinary least squares
direct-2sls	IV regression estimated by direct two-stage least squares
probit-2sls	IV regression estimated by Probit and two-stage least squares
probit-ols	IV two-step regression estimated by Probit and ordinary least squares
heckit	Heckman two-step selection model

hetero(*varlist_h***)** specifies the variables over which to calculate the idyosincratic Average Treatment Effect ATE(x), ATET(x) and ATENT(x), where x=*varlist_*h. It is optional for all models. When this option is not specified, the command estimates the specified model without heterogeneous average effect. Observe that *varlist_h* should be the same set or a subset of the variables specified in *varlist*.

iv(*varlist_iv***)** specifies the variable(s) to be used as instruments. This option is strictly required only for "direct-2sls", "probit-2sls" and "probit-ols", while it is optional for "heckit".

graphic allows for a graphical representation of the density distributions of ATE(x), ATET(x) and ATENT(x). It is optional for all models and gives an outcome only if variables into **hetero()** are specified.

vce(robust) allows for robust regression standard errors. It is optional for all models.

beta reports standardized beta coefficients. It is optional for all models.

const(noconstant) suppresses regression constant term. It is optional for all models.

conf(*number***)** sets the confidence level equal to the specified number. The default is number=95.

The routine creates also a number of variables that can be fruitfully used to inspect further into data:

_ws_*varname_h* are the additional regressors used in model's regression when **hetero(***varlist_h***)** is specified. They are created for all models.

_z_*varname_h* are the instrumental-variables used in model's regression when **hetero(***varlist_h***)** and **iv(***varlist_iv***)** are specified. They are created only in IV models.

ATE(x) is an estimate of the idiosyncratic Average Treatment Effect.

ATET(x) is an estimate of the idiosyncratic Average Treatment Effect on treated.

ATENT(x) is an estimate of the idiosyncratic Average Treatment Effect on Non-Treated.

G_fv is the predicted probability from the Probit regression, conditional on the observable confounders used.

_wL0, **wL1** are the Heckman correction-terms.

Interestingly, `ivtreatreg` returns also some useful scalars:

r(N_tot) is the total number of (used) observations.

r(N_treated) is the number of (used) treated units.

r(N_untreated) is the number of (used) untreated units.

r(ate) is the value of the Average Treatment Effect.

r(atet) is the value of the Average Treatment Effect on Treated.

r(atent) is the value of the Average Treatment Effect on Non-treated.

Finally, some remarks are useful before using this routine:

The treatment has to be a 0/1 binary variable (1 = treated, 0 = untreated).

The standard errors for ATET and ATENT may be obtained via bootstrapping.

When option **hetero()** is not specified, ATE(x), ATET(x) and ATENT(x) are one singleton number equal to
ATE=ATET=ATENT.

Since when hetero is not specified in model "heckit" `ivtreatreg` uses the in-built command treatreg, the following has to be taken into account: (i) option beta and option head(noheader) are not allowed; (ii) option vce takes this sintax: vce(*vcetype*), where *vcetype* may be "conventional", "bootstrap", or "jackknife".

Please remember to use the update query command before running this program to make sure you have an up-to-date version of Stata installed.

6. Using `ivtreatreg` in practice: an application to the relation between *education* and *fertility*

In order to see how `ivtreatreg` actually works, we consider an instructional dataset called FERTIL2.DTA accompanying the manual *Introductory Econometrics: A Modern Approach*, by Wooldridge (2000) collecting cross-sectional data on 4,361 women of childbearing age in Botswana. It is freely downloadable at http://fmwww.bc.edu/ec-p/data/wooldridge/FERTIL2.dta and a description of this dataset is presented below.

Table 1. Description of the dataset FERTIL2.DTA.

Name of the dataset:	**FERTIL2.DTA**
Number of observations:	**4,361**
Number of variables:	**28**

Variable name	**Variable label**
mnthborn	**month woman born**
yearborn	**year woman born**
age	**age in years**
electric	**=1 if has electricity**
radio	**=1 if has radio**
tv	**=1 if has tv**
bicycle	**=1 if has bicycle**
educ	**years of education**
ceb	**children ever born**
agefbrth	**age at first birth**
children	**number of living children**
knowmeth	**=1 if know about birth control**
usemeth	**=1 if ever use birth control**
monthfm	**month of first marriage**
yearfm	**year of first marriage**
agefm	**age at first marriage**
idlnchld	**'ideal' number of children**
heduc	**husband's years of education**
agesq	**age^2**
urban	**=1 if live in urban area**

```
urb_educ        urban*educ
spirit          =1 if religion == spirit
protest         =1 if religion == protestant
catholic        =1 if religion == catholic
frsthalf        =1 if mnthborn <= 6
educ0           =1 if educ == 0
evermarr        =1 if ever married
educ7           =1 if educ >= 7
-------------------------------------------------------------------------------
```

This dataset contains 28 variables on various woman and family characteristics. In this exercise, we are in particular interested in evaluating the impact of the variable `educ7` (taking value 1 if a woman has more than or exactly *seven* years of education and 0 otherwise) on the number of family children (`children`). Several conditioning (or confounding) observable factors are included in the dataset, such as: the age of the woman (`age`), whether or not the family owns a TV (`tv`), whether or not the woman lives in a city (`urban`), and so forth. In order to inquiry into the relation between education and fertility and according to Wooldridge (2002, example 18.3, p. 624) we estimate the following specification for each of the *five* models implemented by `ivtreatreg`:

```
set more off
xi: ivtreatreg children  educ7 age agesq evermarr urban electric tv  ,   ///
hetero(age agesq evermarr urban) iv(frsthalf) model(modeltype) graphic
```

As proposed by Wooldridge (2002) this specification adopts - as instrumental variable - the covariate `frsthalf` taking value 1 if the woman was born in the first six month of the year and zero otherwise. This variable is (partially) correlated with `educ7`, but should not have any direct relation with the number of family children.

The simple difference-in-mean estimator (the mean of children in the group of more educated women, the treated ones, *minus* the mean of children in the group of less educated women, the untreated ones) is -1.77 with a t-value of -28.46. It means that more educated women show – without *ceteris paribus* conditions – about two children less than lower educated ones. By adding confounding factors in the regression specification, we get the OLS estimate of ATE that, again in absence of heterogeneous treatment, is -0.394 with a t-value of -7.94: it is still significant, but the magnitude, as expected, reduces considerably compared to the difference-in-mean estimation thus showing that confounders are relevant. When we consider OLS estimation with heterogeneity, we get an ATE equal to 0.67 still significant at 1% (see column on CF-OLS in Table 4).

When we consider IV estimation, results change dramatically. As working example of how to use `ivtreatreg`, we estimate previous specification in the case of `probit-2sls` (with heterogeneous treatment response). The main outcome is reported in Table 2, where both results from the probit first-step and the IV regression of the second-step are set out. Results on the probit show that `frsthalf` is partially fairly correlated with `educ7`, thus it can be reliably used as instrument for this variable. Step 2 shows that the ATE (again, the coefficient of `educ7`) is no more significant and, above all, it changes the sign by becoming positive and equal to 0.30.

Table 2. Results form `ivtreateg` when probit-2SLS is the specified model and treatment heterogeneous response is assumed.

```
------------------------------------------------------------------------------
Step 1. Probit regression                       Number of obs   =       4358
                                                LR chi2(7)      =    1130.84
                                                Prob > chi2     =     0.0000
Log likelihood =  -2428.384                     Pseudo R2       =     0.1889

------------------------------------------------------------------------------
       educ7 |      Coef.   Std. Err.      z    P>|z|     [95% Conf. Interval]
-------------+----------------------------------------------------------------
    frsthalf |  -.2206627   .0418563    -5.27   0.000    -.3026995   -.1386259
         age |  -.0150337   .0174845    -0.86   0.390    -.0493027    .0192354
       agesq |  -.0007325   .0002897    -2.53   0.011    -.0013003   -.0001647
    evermarr |  -.2972879   .0486734    -6.11   0.000     -.392686   -.2018898
       urban |   .2998122   .0432321     6.93   0.000     .2150789    .3845456
    electric |   .4246668   .0751255     5.65   0.000     .2774235      .57191
          tv |   .9281707   .0977462     9.50   0.000     .7365915     1.11975
       _cons |    1.13537   .2440057     4.65   0.000     .6571273    1.613612
------------------------------------------------------------------------------
Step 2. Instrumental variables (2SLS) regression

      Source |       SS       df       MS              Number of obs =    4358
-------------+------------------------------           F( 11,  4346) =  448.51
       Model |  10198.4139    11  927.128534           Prob > F      =  0.0000
    Residual |  11311.6182  4346  2.60276536           R-squared     =  0.4741
-------------+------------------------------           Adj R-squared =  0.4728
       Total |  21510.0321  4357  4.93689055           Root MSE      =  1.6133

------------------------------------------------------------------------------
    children |      Coef.   Std. Err.      t    P>|t|     [95% Conf. Interval]
-------------+----------------------------------------------------------------
       educ7 |   .3004007   .4995617     0.60   0.548    -.6789951    1.279797
     _ws_age |  -.8428913   .1368854    -6.16   0.000    -1.111256   -.5745262
   _ws_agesq |    .011469   .0019061     6.02   0.000      .007732    .0152059
_ws_evermarr |  -.8979833   .2856655    -3.14   0.002    -1.458033   -.3379333
   _ws_urban |   .4167504   .2316103     1.80   0.072     -.037324    .8708247
         age |    .859302   .0966912     8.89   0.000      .669738    1.048866
       agesq |    -.01003   .0012496    -8.03   0.000    -.0124799   -.0075801
    evermarr |   1.253709   .1586299     7.90   0.000     .9427132    1.564704
       urban |  -.5313325   .1379893    -3.85   0.000     -.801862    -.260803
    electric |  -.2392104   .1010705    -2.37   0.018      -.43736   -.0410608
          tv |  -.2348937   .1478488    -1.59   0.112    -.5247528    .0549653
       _cons |   -13.7584   1.876365    -7.33   0.000    -17.43704   -10.07977
------------------------------------------------------------------------------
Instrumented:  educ7 _ws_age _ws_agesq _ws_evermarr _ws_urban
Instruments:   age agesq evermarr urban electric tv G_fv _z_age _z_agesq
               _z_evermarr _z_urban
------------------------------------------------------------------------------
```

This result is in line with the IV estimation obtained by Wooldridge. Nevertheless, having assumed heterogeneous response to treatment allows now to calculate also the ATET and ATENT and to inspect into the cross-unit distribution of these effects. First of all, **`ivtreateg`** returns these parameters as scalars (along with treated and untreated sample size):

```
. return list
scalars:
          r(N_untreat) =  1937
            r(N_treat) =  2421
              r(N_tot) =  4358
              r(atent) =  -.4468834318603838
               r(atet) =  .898290019555276
                r(ate) =  .3004007408742051
```

In order to get the standard errors for testing ATET and ATENT significance, a bootstrap procedure can be easily implemented as follows:

```
. xi: bootstrap atet=r(atet) atent=r(atent), rep(100): ///
> ivtreatreg children  educ7 age agesq evermarr urban electric tv  , ///
> hetero(age agesq evermarr urban) iv(frsthalf) model(probit-2sls)
```

Table 3 shows the result. As it can be immediate to see, both ATET and ATENT are not significant and show values quite different but not too much far from that of ATE.

Table 3. Bootstrap standard errors for ATET(x) and ATENT(x) using `ivtreateg` with model probit-2sls.

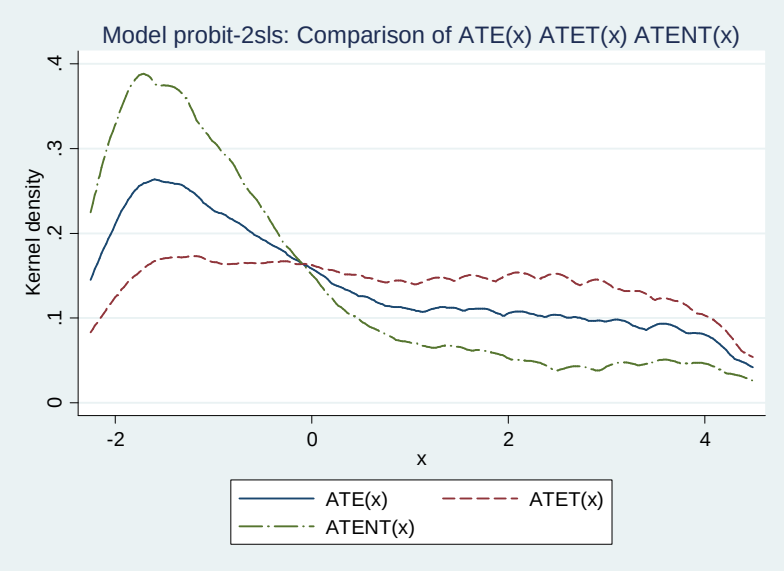
```
-------------------------------------------------------------------------------
Bootstrap results                                 Number of obs      =      4358
                                                  Replications       =       100

      command:  ivtreatreg children educ7 age agesq evermarr urban electric tv,
                hetero(age agesq evermarr urban) iv(frsthalf) model(probit-2sls)
        atet:  r(atet)
       atent:  r(atent)
-------------------------------------------------------------------------------
             |   Observed   Bootstrap                         Normal-based
             |      Coef.   Std. Err.      z    P>|z|     [95% Conf. Interval]
-------------+-----------------------------------------------------------------
        atet |     .89829   .5488267     1.64   0.102    -.1773905    1.973971
       atent |  -.4468834   .4124428    -1.08   0.279    -1.255257    .3614897
-------------------------------------------------------------------------------
```

Furthermore, a simple check should show that ATE = ATET $P(w=1)$ + ATENT $P(w=0)$:

```
. di "ATE= " (r(N_treat)/r(N_tot))*r(atet)+(r(N_untreat)/r(N_tot))*r(atent)
ATE= .30040086
```

that confirms the expected result. Finally, we may analyze the distribution of ATE(**x**), ATET(**x**) and ATENT(**x**) in this case and Figure 2 shows the result.

Figure 1. Distribution of ATE(x), ATET(x) and ATENT(x) in model probit-2sls.



What emerges is that ATET(**x**) shows a substantially uniform distribution, while both ATE(**x**) and ATENT(**x**) a distribution more concentrated on negative values. In particular ATENT(**x**) shows the highest modal value around -2.2 children, thus predicting that less educated women would have been less fertile if they had been more educated.

Table 4 shows ATE results for all the five models, and also for the simple "Difference-in-Mean" (`t-test`). The ATE obtained by IV methods is always not significant, but it has a positive sign only for `probit-2sls`. The rest of ATEs present always negative sign: it means that more educated women would have been more fertile if they had been less educated. The case of `heckit` is a little more puzzling as the result is significant and very close to the difference-in-mean estimation that is highly suspected to be bias. This could be due to the fact that the identification condition of `heckit` are not met in this dataset.

Table 4. Estimation of the ATE for the five models estimated by `ivtreatreg`.

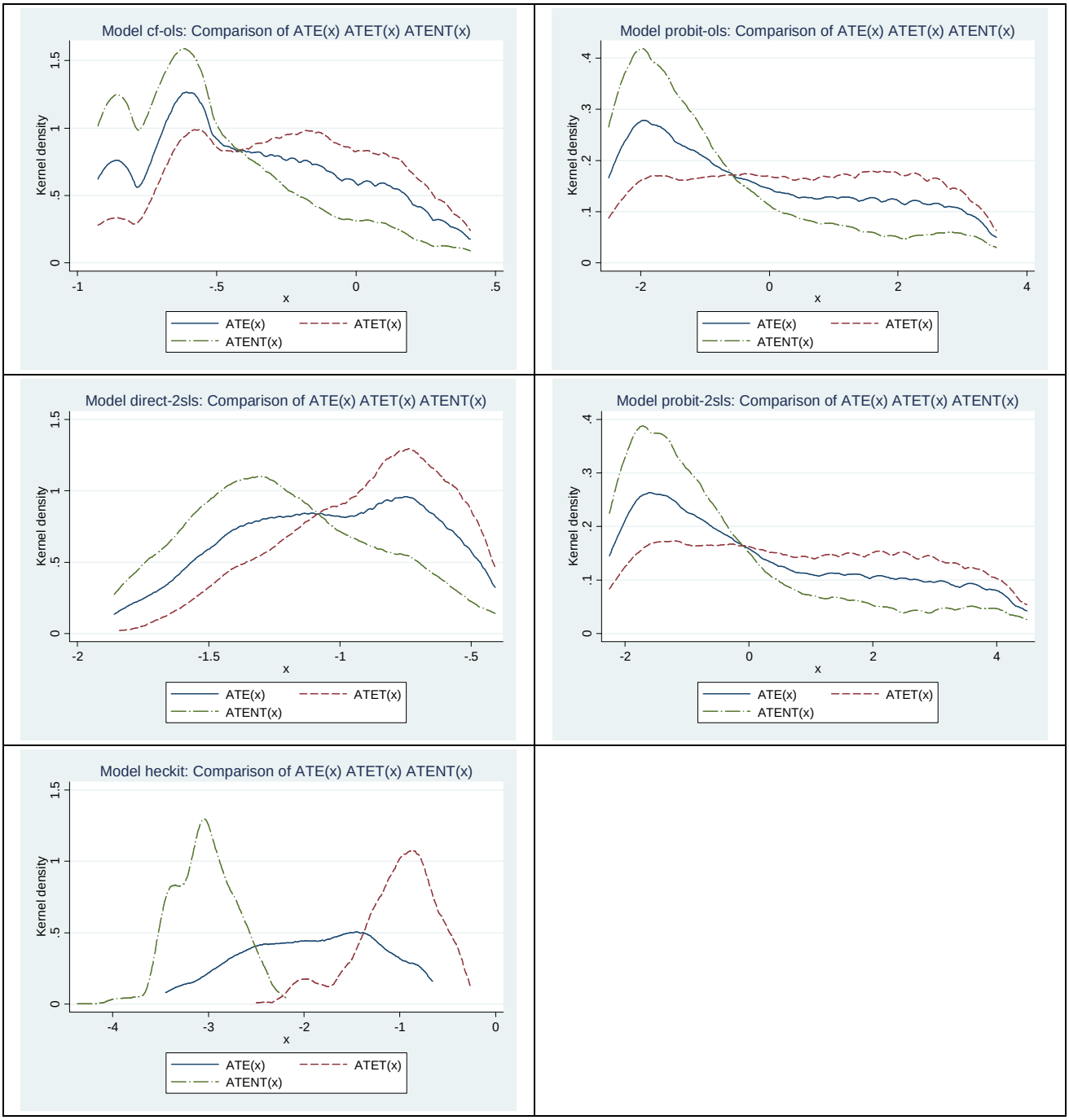
Variable	T-TEST	CF-OLS	PROBIT-OLS	DIRECT-2SLS	PROBIT_2SLS	HECKIT
educ7	-1.770***	-0.372***		-1.044	0.300	-1.915***
	0.06219	0.05020		0.66626	0.49956	0.39871
	-28.46	-7.42		-1.57	0.60	-4.80
G_fv			-0.11395			
			0.50330			
			-0.23			

legend: b/se/t

Figure 2, finally, shows the plot of the average treatment effect distribution for each method. By and large, these distributions follow a similar pattern, although `direct-2sls` and `heckit` show some appreciable differences. The `heckit`, in particular, shows a pattern very different with a strong demarcation between the plot of treated and untreated units. As such, it seems not to a reliable estimation procedure and this should deserve further inspection. Observe, finally, that the distributions for `direct-2sls` are largely more uniform than in the other cases where a strong left-side inflation dominates with the ATENT(x) more concentrated on negative values that

ATET(x) on positive ones. What this might mean? It seems that the *counterfactual condition* of these women is not the same: on average, if a less educated woman became more educated, then their fertility would decrease more than the increase in fertility of more educated women becoming (in a virtual sense) less educated.

Figure 2. Distribution of ATE(x), ATET(x) and ATENT(x) for the five models estimated by `ivtreatreg`.



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