tractable multivariate distributions often do not exist. Because of the specialized nature of applications in this area, this topic is not pursued any further here.

25.8. Example: The Effect of Training on Earnings

The National Supported Work (NSW) demonstration project, conducted in the 1970s, measured the impact of training on earnings by a randomized experiment that assigned some individuals to receive training (a treatment group) and others to receive no training (a control group). The effect of training could then be measured by direct comparison of sample means of posttreatment earnings for the treatment and control groups.

As was discussed in Chapter 3, randomized experiments are relatively rare in the social sciences. More often an observational sample is used with some individuals observed to receive a treatment while others do not. Comparison of the treated with the nontreated must then control for differences in observed characteristics, and possibly in unobserved characteristics.

To determine the adequacy of standard microeconometric methods for observational data, Lalonde (1986) contrasted outcomes for the NSW treated group with those for control groups drawn from two national surveys. He obtained results that differed substantially from the experimental results that contrasted the NSW treated and control groups, and he concluded that the observational methods were unreliable.

Dehejia and Wahba (1999, 2002) reanalyzed a subset of the Lalonde data using alternative matching methods, which they argued led to conclusions from observational data that were considerably closer to those from experimental data. In this section we use their data from Dehejia and Wahba (1999) to illustrate the application of methods introduced in Sections 25.2 to 25.5 that control only for selection on observables.

25.8.1. Dehejia and Wahba Data

The treated sample is one of 185 males who received training during 1976–1977. The control group consists of 2,490 male household heads under the age of 55 who are not retired, drawn from the PSID. Dehejia and Wahba (1999) call these two samples the RE74 subsample (of the NSW treated) and the PSID-1 sample (of nontreated). The treatment indicator variable D is defined as $D = 1$ if training is received (so the observation is in the treated sample) and $D = 0$ if no training was received (and the observation is in the control sample).

Summary statistics for key variables are given in Table 25.3. The treated group differs considerably from the control group, being disproportionately black (84%) with less than a high school degree (71%) and unemployed in the pre-treatment year 1975 (71%). Estimates of the effect of training should control for these differences.

25.8.2. Control Function Approach

Various estimates of the effect of training on earnings are given in Table 25.4.

The outcome of interest is posttreatment earnings, RE78. One possible measure of the effect of training is the mean difference in RE78 between NSW treated and PSID

Table 25.3. *Training Impact: Sample Means in Treated and Control Samples*[a]

Variable	Definition	Treated	Control
AGE	Age in years	25.82	34.85
EDUC	Education in years	10.35	12.12
NODEGREE	1 if EDUC < 12	0.71	0.31
BLACK	1 if race is black	0.84	0.25
HISP	1 if Hispanic	0.06	0.03
MARR	1 if married	0.19	0.87
U74	1 if unemployed in 1974	0.60	0.10
U75	1 if unemployed in 1975	0.71	0.09
RE74	Real earnings in 1974 (in 1982 $)	2,096	19,429
RE75	Real earnings in 1975 (in 1982 $)	1,532	19,063
RE78	Real earnings in 1978 (in 1982 $)	6,349	21,554
D	1 if received training (treatment)	1.00	0.00
Sample size		185	2,490

[a] Data are the same as in table 1 of Dehejia and Wahba (1999). The treated group is the RE74 subsample of the NSW. The control group is the PSID-1 sample of male household heads under 55 years and not yet retired. Treatment occurred in 1976–1977.

control individuals, leading to the estimate $\$6{,}349 - \$21{,}554 = -\$15{,}205$. This is called a **treatment–control comparison** estimator as it mimics the analysis in an experimental setting. It can equivalently be computed as the coefficient of the treatment indicator D in OLS regression of RE78 on an intercept and D, using a combined treatment–control sample.

The large treatment estimate is misleading as it mostly reflects the difference in the types of individuals in the two samples – the control sample individuals are not good controls. This difference can be controlled for by including pretreatment characteristics as regressors, and estimating by OLS

$$\text{RE78}_i = \mathbf{x}_i'\boldsymbol{\beta} + \alpha D_i + u_i, \quad i = 1, \ldots, 2675. \tag{25.76}$$

This leads to a much smaller estimated treatment effect $\widehat{\alpha} = \$218$ when, following Dehejia and Wahba, the regressors $\mathbf{x}$ are specified to be an intercept, AGE, AGESQ, EDUC, NODEGREE, BLACK, HISP, RE74, and RE75. This approach is called the **control function estimator** in Section 25.3.3.

25.8.3. Differences in Differences

A second approach is a **before–after comparison**, which looks at the difference between posttreatment earnings RE78 and pretreatment earnings RE75. Using mean earnings for the treated group leads to the difference estimate $\$6{,}349 - \$1{,}532 = \$4{,}817$.

This estimate may be misleading as it reflects all changes over this time period, such as an improved economy, and not just training. The **difference-in-differences estimator**, considered in Section 25.5, additionally calculates a similar quantity for the control group, $\$21{,}554 - \$19{,}063 = \$2{,}491$, and uses this as a measure of

Table 25.4. *Training Impact: Various Estimates of Treatment Effect*

Method	Definition	Estimate	St. Error[a]
Treatment–control comparison	$\overline{\text{RE78}}_{D=1} - \overline{\text{RE78}}_{D=0}$	−15,205	656
Control function estimator	$\widehat{\alpha}$ from OLS regression (25.76)	218	768
Before–after comparison	$\overline{\text{RE78}}_{D=1} - \overline{\text{RE75}}_{D=1}$	4,817	625
Differences-in-differences	$\widehat{\alpha}$ from OLS regression (25.77)	2,326	749
Propensity score	See Section 25.8.4	995	–

[a] Standard errors for the first four estimates are computed using heteroskedastic-consistent standard errors from the appropriate OLS regression.

nontreatment related changes over time in earnings, so that the change over time solely due to treatment is \$4,817− \$2,491 = \$2,326.

The DID estimator can be shown to be equivalent to the estimate of α in the OLS regression

$$\text{RE}_{it} = \phi + \delta \text{D78}_{it} + \gamma\alpha \text{D}_i + \alpha \text{D78}_{it} \times \text{D}_i + u_i, \quad i = 1, \ldots, 2675, \ t = 75, 78. \tag{25.77}$$

Here $\text{RE}_{i,75}$ denotes earnings in the pretreatment period and $\text{RE}_{i,78}$ denotes earnings in the posttreatment period, so the regression is one with 5,350 earnings observations. The indicator variable D78_{it} equals one in the posttreatment period, the indicator variable D_i equals one if the individual is in the treated sample, and the interaction term $\text{D78}_{it} \times D_i$ equals one for treated individuals in the posttreatment period.

More generally, the intercept ϕ in (25.77) can be replaced by $\mathbf{x}_{it}'\boldsymbol{\beta}$. This makes no difference in this example where regressors are time-invariant so that $\mathbf{x}_{it} = \mathbf{x}_i$. The method can be applied to repeated cross-section data (see Section 22.6.2) as it does not require that individuals in the treated and control groups be observed in both 1975 and 1978.

25.8.4. Simple Propensity Score Estimate

A third approach compares the outcome RE78 for a treated individual with a counterfactual prediction of RE78 if the same treated individual had not in fact received the treatment. The initial treatment–control estimate of \$15,205 is an oversimplified example that uses as counterfactual the average of RE78 in the control group (\$21,554). Better counterfactuals can be generated by specifying a regression model. For example, the regression (25.76) specifies $\text{E}[\text{RE78}|\mathbf{x}]$ to equal $\mathbf{x}'\boldsymbol{\beta} + \boldsymbol{\alpha}$, if treated, with counterfactual $\mathbf{x}'\boldsymbol{\beta}$, if not treated. This places restrictions on both the effect of regressors $\mathbf{x}$ and on the effect of treatment, which, conditional on $\mathbf{x}$, is assumed to be constant across individuals.

The treatment effects literature emphasizes counterfactuals that do not rely on such strong assumptions. An obvious approach is to compare treated and untreated individuals with the same value of $\mathbf{x}$, but in practice such **matching on regressors** is not possible if several regressors are felt to be relevant and these regressors take a number of different values.