CHAPTER 21

Assessing Unconfoundedness

21.1 INTRODUCTION

The previous three chapters assume a regular assignment mechanism, requiring the assignment mechanism to be individualistic, probabilistic, and unconfounded. In this chapter we maintain the first two conditions, which are often uncontroversial, and focus on the plausibility of the third, most controversial assumption, unconfoundedness. Formally, unconfoundedness requires that the probability of treatment assignment is free of dependence on the potential outcomes. Specifically, the super-population version implies, by Theorem 12.1, first, that the conditional distribution of the outcome under the control treatment, $Y_i(0)$, given receipt of the active treatment and given covariates, is identical to its distribution conditional on receipt of the control treatment and conditional on covariates, and second, that, analogously, the conditional distribution of the outcome under the active treatment, $Y_i(1)$, given receipt of the control treatment and conditional on covariates, is identical to its distribution given receipt of the active treatment and conditional on covariates. Informally, unconfoundedness requires that we have a sufficiently rich set of pre-treatment variables so that adjusting for differences in values for observed pre-treatment variables removes systematic biases from comparisons between treated and control units. This critical assumption is not testable. The issue is that the data are not directly informative about the distribution of the control outcome $Y_i(0)$ for those who received the active treatment (for those with $W_i = 1$, we never observe $Y_i(0)$), nor are they directly informative about the distribution of the active treatment outcome given receipt of the control treatment (for those with $W_i = 0$, we never observe $Y_i(1)$). Thus, the data cannot directly provide evidence on the validity of the unconfoundedness assumption. Nevertheless, here we consider ways to assess the plausibility of this assumption from the data at hand.

The analyses discussed in this chapter are *supporting* or *supplementary analyses* that can, depending on their results, increase or reduce the credibility of the main analyses. These supporting analyses focus on estimating, and doing inference for, "pseudo"-causal estimands with *a priori* known values, under assumptions more restrictive than unconfoundedness. If these analyses suggest that the null hypotheses assessing whether these pseudo-causal effects are equal to their null values are not supported by the data, then the unconfoundedness assumption will be viewed as less plausible than in cases where

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these null hypotheses are supported by the data. How much the results of these analyses change our assessment of the unconfoundedness assumption depends on specific aspects of the substantive application at hand, in particular on the richness of the set of pre-treatment variables, their number and type.

The results of these assessments of the unconfoundedness assumption may suggest that unconfoundedness is less plausible than we thought beforehand, and thus that important pre-treatment differences between treated and control units may not have been measured. An important point is that finding pseudo-causal effects different from their known values generally will *not* suggest an alternative approach to estimating the causal estimands. Establishing that methods based on adjusting for observed differences between control and treated units may be unlikely to be adequate for drawing credible causal inferences does not imply the existence of credible alternative methods for causal inferences based on alternative assumptions. The implication may, therefore, be that, given the current data, it is simply not possible to estimate credibly and precisely the causal effects of interest and that one may either have to abandon any attempt to do so without either additional information or without richer data, or at least should be explicit about the lack of credibility.

The specific methods discussed in this chapter are divided here into three classes. The first class of methods can be viewed as comprising a *design* approach, not requiring data on the outcome variable. We partition the full set of pre-treatment variables into two parts, the first set consisting of some selected pre-treatment variables, and the second set consisting of the remaining covariates. Typically the first set consists of a single pre-treatment variable, but in principal it can consist of multiple pre-treatment variables. It takes the first set of selected pre-treatment variables and analyzes them as *pseudo-outcomes*, known *a priori* not to be affected by versus treatment assignment. These pre-treatment variables will be viewed in this approach as "proxy" variables for the potential outcomes, variables likely to be statistically associated with the potential outcomes. In principle, such a proxy variable can be any pre-treatment variable. However, the most compelling case arises when the proxy variable is a lagged outcome, that is, a measure of the same substantive quantity as the outcome but measured at a point in time prior to the receipt of treatment. We then assess the null hypothesis that there are no systematic differences in the pseudo-outcome by treatment status, after adjusting for the second set of covariates. We refer to this hypothesis as *pseudo-outcome unconfoundedness*. A finding that one cannot reject the null hypothesis that these pseudo-causal effects are zero (or small) lends credibility to the unconfoundedness assumption based on the full set of covariates. Again, this approach is not a direct assessment of the unconfoundedness assumption. Even if the null hypothesis of no treatment effect on the pseudo-outcome is found to be implausible, one cannot be confident that the hypothesis underlying the planned main analysis, unconfoundedness, is violated. Nevertheless, if two conditions are satisfied, we will argue that the assessment is informative about the credibility of the analysis under unconfoundedness. The first is that the pseudo-outcome used in this assessment is a good proxy for one of the potential outcomes. The second condition is that *subset unconfoundedness* (which requires that the pseudo-outcome is not required for unconfoundedness to hold) is plausible – in other words, that there should be an *a priori* argument that the pseudo-outcome is not essential for removing

biases in comparisons between treated and control units given the second set of pre-treatment variables. This is often most compelling if the pseudo-outcome differs from some of the remaining pre-treatment variables only in the time of measurement. In cases where all the pre-treatment variables are qualitatively different, it may be more difficult to argue that any of the pre-treatment variables can be omitted without leading to violations of unconfoundedness. This first approach to assessing unconfoundedness is a *design* approach that does not use data on the outcome variable.

The second approach to assessing the unconfoundedness assumption focuses on pseudo-causal effects for the original outcome. Instead of focusing on causal effects of the actual treatment, this approach analyzes the effects of a pseudo-treatment that is known *a priori* not to have a causal effect. We refer to it as a *semi-design* approach, using only outcome data for the units in the control group. Originally proposed by Rosenbaum (1987), this approach relies on the presence of multiple control groups. Suppose the researcher has available two potential comparison groups consisting of units not exposed to the active treatment. In the main analysis, one may have combined the two comparison groups into a single control group to estimate the treatment effects of interest. However, one can also compare the two comparison groups to each other, arbitrarily viewing one of them as being a "pseudo-treatment" group and the other as a "clean" control group under the stronger version of unconfoundedness that we call *group unconfoundedness*. Because neither group did, in fact, receive any active treatment, there should be no causal effects of this pseudo-treatment. Statistical evidence of the presence of systematic differences between the two control groups after adjusting for pre-treatment differences (non-zero "pseudo-causal effects") implies that unconfoundedness is violated for at least one of the comparison groups. Finding no evidence of a difference between the two groups does not imply the unconfoundedness assumption is valid, because it could be that both comparison groups exhibit the same bias for comparing the actual treated and control units after adjusting for differences in pre-treatment variables. However, if *a priori* any potential biases in treatment-control differences between the two control groups are judged to be different, the finding that the hypothesis of a zero effect of the pseudo-treatment is consistent with the data makes the analysis under unconfoundedness more plausible. The key for the force of this approach is to have control groups that are systematically different and, as a result, are likely to exhibit different biases in treatment-control comparisons, if they have any biases at all.

The third class of methods focuses on the robustness of estimates of causal effects to the choice of pre-treatment variables. Here we require outcome data for both the treatment and control groups, and so this is not a design-stage analysis. We again partition the full set of pre-treatment variables into two parts, the first set consisting of some selected pre-treatment variables, and the second set consisting of the remaining covariates. We then assume subset unconfoundedness, where unconfoundedness is assumed to hold conditional only on the remaining set of pre-treatment variables. Given subset unconfoundedness, we estimate the causal effects of the treatment on the actual outcome and compare the results to those based on (full) unconfoundedness to assess the null hypothesis that both unconfoundedness and subset unconfoundedness hold. If we find substantial and statistically significant differences, we would conclude that either (*i*) the first set of selected pre-treatment variables is critical for unconfoundedness (subset unconfoundedness is violated), or (*ii*) unconfoundedness does not hold. If

we *a priori* view that the substantive difference between unconfoundedness and subset unconfoundedness is minor, the implication is that we should be concerned with the unconfoundedness assumption. Clearly this assertion depends on the context and the nature of the variables in the two sets.

In all three approaches, we are interested in assessing the presence and magnitude of causal effects under an unconfoundedness assumption. In practice, we often focus on testing whether an estimated average pseudo-causal effect under unconfoundedness is different from its presumed known value (typically zero). It should be noted, though, that in principle the interest here is in assessing whether there is *any* effect of the (pseudo)-treatment on the (pseudo)-outcome different from its presumed known value, not just a zero average difference. That is, we may therefore wish to go beyond studying average effects and also investigate differences in distributions of outcomes, as well as average outcomes by subpopulations. In doing so, we are interested in both statistically and substantially significant differences between the comparison groups.

The remainder of this chapter is organized as follows. In Section 21.3, we discuss the role of pseudo-outcomes for assessing the unconfoundedness assumption. In the next section, Section 21.4, we discuss how one can exploit the presence of multiple control groups. In Section 21.5 we focus on assessing the robustness of the causal effect estimates to changes in the set of pre-treatment variables. In Section 21.6 we illustrate some of the methods using the Imbens-Rubin-Sacerdote lottery data, which we previously used in Chapters 14, 17, and 19. Section 21.7 concludes.

21.2 SETUP

The setup in this section is largely the same as in the previous chapters. For unit i we postulate the existence of two potential outcomes $Y_i(0)$ and $Y_i(1)$, a treatment indicator $W_i \in \{0, 1\}$, and a vector of covariates or pre-treatment variables X_i. We observe the triple consisting of the vector of covariates X_i, the treatment indicator W_i, and the realized and observed outcome

$$Y_i^{\text{obs}} = Y_i(W_i) = \begin{cases} Y_i(0) & \text{if } W_i = 0, \\ Y_i(1) & \text{if } W_i = 1. \end{cases}$$

We consider the super-population unconfoundedness assumption,

$$W_i \perp\!\!\!\perp Y_i(0), Y_i(1) \,\Big|\, X_i \quad \text{(unconfoundedness)}, \tag{21.1}$$

where the dependence on the parameter ϕ is suppressed. This assumption is not testable, as discussed in Chapter 12.

21.3 ESTIMATING EFFECTS ON PSEUDO-OUTCOMES

In this section we discuss the first approach to assessing unconfoundedness, where we focus on tests for causal effects on pseudo-outcomes. This is an approach that can be used

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at the design stage, without access to outcome data. First we introduce some additional notation. We partition the vector of covariates X_i into two parts, the first denoted by X_i^{p} ("p" for pseudo), and the remainder denoted by X_i^{r}, so that the full vector of pre-treatment variables can be written as $X_i = (X_i^{\text{p}}, X_i^{\text{r}})$.

Instead of testing whether the conditional independence assumption in (21.1) holds, which we showed before is impossible to do from the data at hand, we shall test whether the following conditional independence relation, which we label *pseudo-outcome unconfoundedness*, holds:

$$W_i \perp\!\!\!\perp X_i^{\text{p}} \,\Big|\, X_i^{\text{r}} \quad \text{(pseudo-outcome unconfoundedness).} \tag{21.2}$$

The two issues are, first, the interpretation of assumption (21.2) and specifically its connection to full unconfoundedness (21.1), which is of primary interest, and second, the implementation of the assessment.

21.3.1 Interpretation

The first issue concerns the link between the conditional independence relation in (21.2) and unconfoundedness in (21.1). This link is indirect, because unconfoundedness cannot be tested directly. Here we lay out the arguments for the connection. First consider an additional version of unconfoundedness, which we label *subset unconfoundedness*

$$W_i \perp\!\!\!\perp \big(Y_i(0), Y_i(1)\big) \,\Big|\, X_i^{\text{r}} \quad \text{(subset unconfoundedness).} \tag{21.3}$$

Subset unconfoundedness is not testable for the same reasons full unconfoundedness is not testable: we do not observe $Y_i(0)$ if $W_i = 1$, and we do not observe $Y_i(1)$ if $W_i = 0$. Here we explore an alternative approach to assess it. Suppose we have a proxy for either of the potential outcomes, and in particular a proxy or *pseudo-outcome* whose value is observed irrespective of the realized treatment status; one can test independence of that proxy variable and the treatment indicator W_i. We use the selected pre-treatment variable X_i^{p} as such a pseudo-outcome or proxy variable. For example, we view X_i^{p} as a proxy for $Y_i(0)$, and assess (21.3) by testing (21.2), which involves only observed variables.

The most compelling applications of these assessments are settings where the two steps in going from unconfoundedness (21.1) to the testable condition (21.2) are plausible. One such example occurs when X_i contains multiple lagged measures of the outcome, as in the Imbens-Rubin-Sacerdote lottery study that we use to illustrate these methods in this chapter. The pre-treatment variables X_i in that application consist of some time-invariant pre-treatment variables V_i (e.g., age, education), and some lagged outcomes (earnings), $(Y_{i,-1}, \ldots, Y_{i,-T})$. One can implement these ideas using earnings for one of the most recent pre-program years $(Y_{i,-1}, \ldots, Y_{i,-T})$ as the pseudo-outcome X_i^{p}. Under unconfoundedness, $Y_i(0)$ is independent of W_i given $Y_{i,-1}, \ldots, Y_{i,-6}$ and V_i, which would suggest that it is also plausible that $Y_{i,-1}$ is independent of W_i given $Y_{i,-2}, \ldots, Y_{i,-6}$ and V_i. Given those arguments, one can plausibly assess

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unconfoundedness by testing whether

$$W_i \perp\!\!\!\perp Y_{i,-1} \,\big|\, V_i, Y_{i,-2}, \ldots, Y_{i,-6}. \tag{21.4}$$

The claim now is that finding that (21.4) is not supported by the data would lower the credibility of an analysis that relies on unconfoundedness (21.1), relative to a finding that the relation in (21.4) is consistent with the data.

21.3.2 Implementation

Now we turn again to the implementation of this assessment of unconfoundedness. One approach to test the conditional independence assumption in (21.2) is to estimate the average difference in $X_i^{\rm p}$ by treatment status, after adjusting for differences in $X_i^{\rm r}$. This is exactly the same problem as estimating the average effect of the treatment, using $X_i^{\rm p}$ as the pseudo-outcome and $X_i^{\rm r}$ as the vector of pre-treatment variables. We can do this using any of the methods discussed in the previous chapters, such as blocking or matching, ideally in combination with model-based adjustment.

The main limitation of this approach, testing whether an adjusted average difference is equal to zero, is that it does not test all aspects of the conditional independence restriction. It effectively tests only whether

$$\mathbb{E}\Big[\mathbb{E}\left[X_i^{\rm p}\big|\, W_i=1, X_i^{\rm r}\right] - \mathbb{E}\left[X_i^{\rm p}\big|\, W_i=0, X_i^{\rm r}\right]\Big] = 0.$$

Pseudo-outcome unconfoundedness (21.2) implies two additional sets of restrictions. First, of all, it implies that

$$\mathbb{E}\Big[\mathbb{E}\left[g(X_i^{\rm p})\big|\, W_i=1, X_i^{\rm r}\right] - \mathbb{E}\left[g(X_i^{\rm p})\big|\, W_i=0, X_i^{\rm r}\right]\Big] = 0,$$

for any function $g(\,\cdot\,)$, not just the identity function. We can implement this by comparing average outcomes for different transformations of the pseudo-outcome and testing jointly whether any of the averages effects are zero. For example, for a pseudo-outcome bounded between zero and one, one might test jointly whether the effects of the treatment on $1_{X_i^{\rm p}\leq 0.2}$, $1_{X_i^{\rm p}\leq 0.4}$, $1_{X_i^{\rm p}\leq 0.6}$, and $1_{X_i^{\rm p}\leq 0.8}$ are all zero. For non-negative outcomes such as earnings, we may wish to test whether the average value of earnings, as well as the fraction of individuals with positive earnings, are equal in treatment and control groups. Of course one has to be careful here doing multiple comparisons, because in that case some contrasts may appear substantial just by chance.

Second, the conditional independence restriction in (21.2) implies that, not only on average, but conditional on $X_i^{\rm r}=x^{\rm r}$, for all $x^{\rm r}$,

$$\mathbb{E}\left[X_i^{\rm p}\big|\, W_i=1, X_i^{\rm r}=x^{\rm r}\right] - \mathbb{E}\left[X_i^{\rm p}\big|\, W_i=0, X_i^{\rm r}=x^{\rm r}\right] = 0.$$

One can therefore also consider tests of the restriction

$$\mathbb{E}\Big[\mathbb{E}\left[g(X_i^{\rm p})\big|\, W_i=1, X_i^{\rm r}\right] - \mathbb{E}\left[g(X_i^{\rm p})\big|\, W_i=0, X_i^{\rm r}\right]\Big|\, X_i^{\rm r}\in\mathbb{X}_j^{\rm r}\Big] = 0, \tag{21.5}$$

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for some partitioning $\{\mathbb{X}^{\mathrm{r}}_j\}_{j=1}^J$ of the support $\mathbb{X}^{\mathrm{r}}$ of the set of remaining covariates X^{r}_i. That is, rather than testing whether the overall average effect of the treatment on the pseudo-outcome differs from zero, one might wish to test the null hypothesis that the average effect of the treatment on the pseudo-outcome in subpopulations differ from zero.

21.4 ESTIMATING EFFECTS OF PSEUDO-TREATMENTS

We now discuss the second approach to assessing unconfoundedness, which focuses on tests for non-zero causal effects of pseudo-treatments.

21.4.1 Setup

This approach to assess the plausibility of the unconfoundedness assumption relies on the presence of additional control information, specifically, a two-component control group. For this approach, we require outcome data for the control group but not for the treatment group. It could therefore be called a *semi-design stage* method. We change notation in a subtle way. Let G_i be an indicator variable denoting the generalized treatment group that unit i is a member of. This indicator variable takes on three or more values. For ease of exposition we focus on the case with two control groups and thus three values for G_i, $G_i \in \{c_1, c_2, t\}$. Units with $G_i = c_1$ or c_2 receive the control treatment, $W_i = 0$, and units with $G_i = t$ receive the active treatment, $W_i = 1$:

$$W_i = \begin{cases} 0 & \text{if } G_i = c_1, c_2, \\ 1 & \text{if } G_i = t. \end{cases}$$

Unconfoundedness only requires that

$$W_i \perp\!\!\!\perp \big(Y_i(0), Y_i(1)\big) \,\Big|\, X_i, \tag{21.6}$$

which is not testable with the data at hand. Instead we focus on testing an implication of the stronger conditional independence relation, which we label *group unconfoundedness*:

$$G_i \perp\!\!\!\perp \big(Y_i(0), Y_i(1)\big) \,\Big|\, X_i, \quad \text{(Group Unconfoundedness)} \tag{21.7}$$

This independence condition implies unconfoundedness, but in contrast to unconfoundedness, it has testable restrictions. In particular, we focus on the implication that

$$G_i \perp\!\!\!\perp Y_i(0) \,\Big|\, X_i, G_i \in \{c_1, c_2\},$$

which is equivalent to

$$G_i \perp\!\!\!\perp Y^{\mathrm{obs}}_i \,\Big|\, X_i, G_i \in \{c_1, c_2\}, \tag{21.8}$$

because $G_i \in \{c_1, c_2\}$ implies that $W_i = 0$, and thus $Y^{\mathrm{obs}}_i = Y_i(W_i) = Y_i(0)$. This conditional independence condition has the same form as (21.2), and we test it in the same

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fashion. Again we discuss first the link between (21.8) and unconfoundedness, (21.1), and second the implementation of tests of this conditional independence assumption.

21.4.2 Interpretation

Because condition (21.12) is strictly stronger than unconfoundedness, (21.1), the question is whether there are interesting settings where the weaker and untestable condition of unconfoundedness holds but not the stronger condition. To discuss this question, it is useful to consider two alternative unconfoundedness-like conditional independence conditions, both of which are implied by (21.6):

$$W_i \perp\!\!\!\perp \big(Y_i(0), Y_i(1)\big) \,\Big|\, X_i, G_i \in \{c_1, t\}, \tag{21.9}$$

and

$$W_i \perp\!\!\!\perp \big(Y_i(0), Y_i(1)\big) \,\Big|\, X_i, G_i \in \{c_2, t\}. \tag{21.10}$$

If (21.9) holds, then we can estimate causal effects by invoking the unconfoundedness assumption using only the first control group. Similarly, if (21.10) holds, then we can estimate causal effects by invoking the unconfoundedness assumption using only the second control group. The point is that it is difficult to envision a situation where unconfoundedness based on the two comparison groups (21.6) holds, but using only one of the two comparison groups the unconfoundedness condition fails (i.e., neither (21.9) nor (21.10) holds). So, in practice, if unconfoundedness holds, typically also the stronger condition (21.6) would hold, and we have the testable implication (21.8). Again, there is no theorem here, but an implication that when stronger conditional independence assumptions are false, weaker conditional independence assumptions are more likely also to be false.

21.4.3 Implementation

The implementation of the test follows the same pattern as the implementation of the tests of (21.2). We test whether there is a difference in average values of Y_i^{obs} between the two control groups, after adjusting for differences in X_i. That is, we effectively test whether

$$\mathbb{E}\Big[\mathbb{E}\Big[Y_i^{\text{obs}}\Big| G_i = c_1, X_i\Big] - \mathbb{E}\Big[Y_i^{\text{obs}}\Big| G_i = c_2, X_i\Big]\Big] = 0.$$

We can then extend the test by simultaneously testing whether the average value of transformations of the form $g(Y_i^{\text{obs}})$ differs by group, that is, whether

$$\mathbb{E}\Big[\mathbb{E}\Big[g(Y_i^{\text{obs}})\Big| G_i = c_1, X_i\Big] - \mathbb{E}\Big[g(Y_i^{\text{obs}})\Big| G_i = c_2, X_i\Big]\Big] = 0.$$

In addition we can extend the tests by assessing the null hypothesis whether, given a partition $\{\mathbb{X}_j\}_{j=1}^J$ of the support $\mathbb{X}$ of X_i,

$$\mathbb{E}\Big[\mathbb{E}\Big[g(Y_i^{\text{obs}})\Big| W_i = 1, X_i\Big] - \mathbb{E}\Big[g(Y_i^{\text{obs}})\Big| W_i = 0, X_i\Big]\Big| X_i \in \mathbb{X}_j\Big] = 0, \tag{21.11}$$

for all subsets $\mathbb{X}_j$, for $j = 1, \ldots, J$.

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21.5 ROBUSTNESS TO THE SET OF PRE-TREATMENT VARIABLES

Here we discuss the third approach to assessing unconfoundedness: investigating the sensitivity of the estimates of causal effects to the choice of pre-treatment variables used for adjustments.

21.5.1 Subset Unconfoundedness and Robustness

We use the same notion of partitioning the set of pre-treatment variables into two parts that we introduced in Section 21.3. Again let us consider subset unconfoundedness:

$$W_i \perp\!\!\!\perp Y_i(0), Y_i(1) \,\Big|\, X_i^{\mathrm{r}} \quad \text{(subset unconfoundedness)}. \tag{21.12}$$

If this subset unconfoundedness condition were to hold, one could use the adjustment methods described in Chapters 17 and 18, using only the subset of covariates X_i^{r}, instead of the full vector of pre-treatment variables X_i to obtain approximately unbiased estimates of treatment effects. Although this is not a formal result, subset unconfoundedness in (21.3) is intuitively a more restrictive condition than the original unconfoundedness condition (21.1). One has to be careful because it is theoretically possible that conditional on a subset of the covariates (e.g., X_i^{r}) subset unconfoundedness (21.3) holds, but at the same time, unconfoundedness (21.1) does not hold conditional on the full set of covariates $(X_i^{\mathrm{r}}, X_i^{\mathrm{p}})$. In practice, however, this situation is rare if all covariates are proper pre-treatment variables. For example, it is difficult to imagine in an evaluation of a labor market program where unconfoundedness would hold given age, last year's earnings, and the level of education, but not hold if one additionally conditions on sex. Generally having subpopulations that are more homogeneous in pre-treatment variables improves the plausibility of unconfoundedness, although, again, theoretically it is possible that the biases are exactly canceled out if one of the pre-treatment variables is omitted from the analysis. This possibility, however, appears to be of little practical interest.

The main concern for the application of this approach is not this remote possibility of canceling biases but the very real possibility that the pseudo-outcome X_i^{p} may be critical to unconfoundedness, and so that (21.1) may hold, but not (21.3). This is likely to be the case if X_i^{p} is qualitatively different from the variables in X_i^{r}. Again this reinforces the idea that this approach is most valuable when X_i contains several variables that differ from each other only in their time of measurement.

On its own, the assumption of subset unconfoundedness is not directly testable for the same reason that unconfoundedness is not testable: it restricts distributions of missing potential outcomes in terms of distributions of observed potential outcomes. However, the combination of the two assumptions, subset unconfoundedness (21.3) and unconfoundedness (21.1), both not testable on their own, does have testable implications. The combination implies that adjusting for differences in the subset of covariates X_i^{r} and adjusting for differences in the full set of covariates X_i should give similar point estimates (but not necessarily precisions). Thus, we can compare point estimates based on adjusting for the full set of covariates and the subset of covariates. If we find that the results are statistically different for the two sets of covariates, it must be that at least one

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of the two assumptions, (full) unconfoundedness or subset unconfoundedness, does not hold. The fact that, in that case, the presence of $X_i^{\rm p}$ is critical for the adjustment suggests that there may be concerns about unconfoundedness in general. On the other hand, if we find that the point estimates based on the two assumptions are similar, one may be more confident in the underlying unconfoundedness assumption.

One of the leading examples occurs when X_i contains multiple lagged measures of the outcome. For example, in the evaluation of the effect of a labor market program or lottery on annual earnings, one might have observations on earnings for multiple years prior to the program. Consider the Imbens-Rubin-Sacerdote lottery data, where we have six years of annual earnings prior to winning the lottery. Denote these lagged outcomes by $Y_{i,-1}, \ldots, Y_{i,-6}$, where $Y_{i,-1}$ is the most recent and $Y_{i,-6}$ is the most distant (in time) pre-lottery earnings measure, and denote the remaining covariates by V_i, so that $X_i = (Y_{i,-1}, \ldots, Y_{i,-6}, V_i)$. Unconfoundedness corresponds to the independence relation

$$W_i \perp\!\!\!\perp Y_i(0), Y_i(1) \,\Big|\, V_i, Y_{i,-1}, Y_{i,-2}, \ldots, Y_{i,-6}. \tag{21.13}$$

This assumption is not testable with the data at hand. However, one could implement the foregoing ideas using earnings for the most recent pre-program year $Y_{i,-1}$ as the selected pre-treatment variable $X_i^{\rm p}$, so that the vector of remaining pre-treatment variables $X_i^{\rm r}$ would still include the five prior years of pre-program earnings, $Y_{i,-2}, \ldots, Y_{i,-6}$, and the additional pre-treatment variables V_i. In that case, one might reasonably argue that, on *a priori* grounds, unconfoundedness is viewed as reasonable given the presence of six years of pre-program earnings (i.e., (21.13) holds), and it is plausible that it would also hold given only five years of pre-program earnings, so that also

$$W_i \perp\!\!\!\perp Y_i(0), Y_i(1) \,\Big|\, V_i, Y_{i,-2}, \ldots, Y_{i,-6}. \tag{21.14}$$

21.5.2 Implementation

Here we discuss the implementation of this First we focus on a specific testable implication, and then we discuss more general results. Let $\tau_{\rm SP} = \mathbb{E}[Y_i(1) - Y_i(0)]$ be the super-population average causal effect of the treatment. Under (super-population) unconfoundedness,

$$\tau_{\rm sp} = \mathbb{E}\Big[\mathbb{E}\left[Y_i^{\rm obs} \,\Big|\, W_i = 1, X_i\right] - \mathbb{E}\left[Y_i^{\rm obs} \,\Big|\, W_i = 0, X_i\right]\Big].$$

Under subset unconfoundedness, it is also true that adjusting solely for differences in $X_i^{\rm r}$ removes biases from comparisons between treated and control units:

$$\tau_{\rm sp} = \mathbb{E}\Big[\mathbb{E}\left[Y_i^{\rm obs} \,\Big|\, W_i = 1, X_i^{\rm r}\right] - \mathbb{E}\left[Y_i^{\rm obs} \,\Big|\, W_i = 0, X_i^{\rm r}\right]\Big].$$

These two results imply the testable restriction that

$$\begin{aligned}\mathbb{E}\Big[\mathbb{E}\left[Y_i^{\rm obs} \,\Big|\, W_i = 1, X_i\right] - \mathbb{E}\left[Y_i^{\rm obs} \,\Big|\, W_i = 0, X_i\right]\Big] \\ = \mathbb{E}\Big[\mathbb{E}\left[Y_i^{\rm obs} \,\Big|\, W_i = 1, X_i^{\rm r}\right] - \mathbb{E}\left[Y_i^{\rm obs} \,\Big|\, W_i = 0, X_i^{\rm r}\right]\Big].\end{aligned}$$

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We can implement this assessment by estimating the average effect of the treatment using the full set of covariates and comparing that to the estimate of the average treatment effect based on the subset of covariates.

We compare two quantities that both estimate the average causal effect under the combination of two assumptions, unconfoundedness and subset unconfoundedness. If both assumptions hold, the treatment effects should also be identical for subpopulations and for causal estimands other than the average effect, and for comparisons of both potential outcomes separately. We can capture these additional implications by focusing on comparisons of more general estimands. Let $f_{Y_i^{\text{obs}}|W_i=w,X_i=x}(y|w,x)$ be the conditional distribution in the super-population of Y_i^{obs} conditional on $W_i = w$ and the full set of covariates $X_i = x$, and similarly for $f_{Y_i^{\text{obs}}|W_i=w,X_i^{\text{r}}=x^{\text{r}}}(y|w,x^{\text{r}})$, where we only condition on X_i^{r}. By definition, the distribution conditioning on the subset of the covariates X_i^{r} can be written as

$$f_{Y_i^{\text{obs}}|W_i=w,X_i^{\text{r}}=x^{\text{r}}}(y|w,x^{\text{r}}) = \int f_{Y_i^{\text{obs}}|W_i=w,X_i=x}(y|w,x) \cdot f_{X_i|W_i=w,X_i^{\text{r}}=x^{\text{r}}}(x|w,x^{\text{r}})dx.$$

At its most general level, the implication of the combination of the two assumptions, unconfoundedness and subset unconfoundedness, is that

$$f_{Y_i^{\text{obs}}|W_i=w,X_i^{\text{r}}=x^{\text{r}}}(y|w,x^{\text{r}}) = \int f_{Y_i^{\text{obs}}|W_i=w,X_i=x}(y|w,x) \cdot f_{X_i|X_i^{\text{r}}=x^{\text{r}}}(x|x^{\text{r}})dx.$$

Hence, the hypothesis that is being assessed is whether the conditioning on $W_i = w$ in the conditional distribution of X_i given $X_i^{\text{r}} = x^{\text{r}}$ in this integral matters:

$$\int f_{Y_i^{\text{obs}}|W_i=w,X_i=x}(y|w,x) \cdot \big(f_{X_i|W_i,X_i^{\text{r}}=x^{\text{r}}}(x|w=1,x^{\text{r}}) - f_{X_i|W_i,X_i^{\text{r}}=x^{\text{r}}}(x|w=0,x^{\text{r}})\big)dx = 0. \tag{21.15}$$

Directly comparing the two complete conditional distributions is complicated, so here we focus on a different set of comparisons. Let $\mathbb{X}^{\text{r}}$ be the support of X_i^{r}, and let $\mathbb{X}_1^{\text{r}}, \ldots, \mathbb{X}_J^{\text{r}}$ partition this space. Then consider, for some function $g(\cdot)$ of the outcome, conditional on $X_i^{\text{r}} \in \mathbb{X}_j^{\text{r}}$ the conditional average outcome,

$$\mathbb{E}\left[g(Y_i^{\text{obs}}) \,\middle|\, W_i = w, X_i^{\text{r}} \in \mathbb{X}_j^{\text{r}} \right]. \tag{21.16}$$

If we maintain both assumptions, unconfoundedness and subset unconfoundedness, we can estimate the expectation in (21.16) in two different ways. First, under unconfoundedness, it is equal to

$$\mathbb{E}\Big[\mathbb{E}\left[g(Y_i^{\text{obs}}) \,\middle|\, W_i = w, X_i \right] \Big| W_i = 0, X_i^{\text{r}} \in \mathbb{X}_j^{\text{r}} \Big]. \tag{21.17}$$

Second, under subset unconfoundedness, the expectation in (21.17) is also equal to

$$\mathbb{E}\Big[\mathbb{E}\left[g(Y_i^{\text{obs}}) \,\middle|\, W_i = w, X_i^{\text{r}} \right] \Big| W_i = 1, X_i^{\text{r}} \in \mathbb{X}_j^{\text{r}} \Big],$$

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leading to the restriction that for all functions $g(\cdot)$, for all subsets $\mathbb{X}_j^{\mathrm{r}}$ and for both $w = 0, 1$,

$$\mathbb{E}\Big[\mathbb{E}\Big[g(Y_i^{\mathrm{obs}})\Big|W_i = w, X_i\Big]\Big|W_i = 0, X_i^{\mathrm{r}} \in \mathbb{X}_j^{\mathrm{r}}\Big] = \mathbb{E}\Big[\mathbb{E}\Big[g(Y_i^{\mathrm{obs}})\Big|W_i = w, X_i^{\mathrm{r}}\Big]\Big|W_i = 1, X_i^{\mathrm{r}} \in \mathbb{X}_j^{\mathrm{r}}\Big].$$

To gain some insight into this approach, let us consider a simple case. In this example we focus on the case with $g(y) = y$, leading to the restriction

$$\mathbb{E}\Big[\mathbb{E}\Big[Y_i^{\mathrm{obs}}\Big|W_i = 1, X_i\Big]\Big|W_i = 0, X_i^{\mathrm{r}} \in \mathbb{X}_j^{\mathrm{r}}\Big] - \mathbb{E}\Big[\mathbb{E}\Big[Y_i^{\mathrm{obs}}\Big|W_i = 1, X_i^{\mathrm{r}}\Big]\Big|W_i = 1, X_i^{\mathrm{r}} \in \mathbb{X}_j^{\mathrm{r}}\Big]. \tag{21.18}$$

Moreover, suppose that the conditional expectations are linear, $\mathbb{E}[Y_i^{\mathrm{obs}}|W_i = 1, X_i] = X_i\beta_{\mathrm{t}}$, with $\beta_{\mathrm{t}} = (\beta_{\mathrm{t}}^{\mathrm{p}}, \beta_{\mathrm{t}}^{\mathrm{r}})$ corresponding to X_i^{p} and X_i^{r}, so that $\mathbb{E}[Y_i^{\mathrm{obs}}|W_i = 1, X_i] = X_i^{\mathrm{p}}\beta_{\mathrm{t}}^{\mathrm{p}} + X_i^{\mathrm{r}}\beta_{\mathrm{t}}^{\mathrm{r}}$. Then:

$$\mathbb{E}\Big[\mathbb{E}\Big[Y_i^{\mathrm{obs}}\Big|W_i = 1, X_i\Big]\Big|W_i = 0, X_i^{\mathrm{r}} \in \mathbb{X}_j^{\mathrm{r}}\Big] = \mathbb{E}\Big[X_i^{\mathrm{p}}\beta_{\mathrm{t}}^{\mathrm{p}} + X_i^{\mathrm{r}}\beta_{\mathrm{t}}^{\mathrm{r}}\Big|W_i = 0, X_i^{\mathrm{r}} \in \mathbb{X}_j^{\mathrm{r}}\Big]$$

$$= X_i^{\mathrm{r}}\beta_{\mathrm{t}}^{\mathrm{r}} + \mathbb{E}\Big[X_i^{\mathrm{p}}\Big|W_i = 0, X_i^{\mathrm{r}} \in \mathbb{X}_j^{\mathrm{r}}\Big]\beta_{\mathrm{t}}^{\mathrm{p}},$$

and the difference (21.18) reduces to

$$\big(\mathbb{E}[X_i^{\mathrm{p}}|W_i = 0, X_i^{\mathrm{r}}] - \mathbb{E}[X_i^{\mathrm{p}}|W_i = 1, X_i^{\mathrm{r}}]\big)\beta_{\mathrm{t}}^{\mathrm{p}}. \tag{21.19}$$

In this linear conditional expectation case, the combination of unconfoundedness and subset unconfoundedness implies that the association (i.e., correlation here) of the selected covariates X_i^{p} with the outcome conditional on the remaining covariates is zero on average.

21.6 THE IMBENS-RUBIN-SACERDOTE LOTTERY DATA

In this section we apply the methods developed in this chapter to the lottery data previously analyzed in Chapter 13. We start with the full sample of 496 individuals. In Table 21.1 we present summary statistics for these 496 individuals. We focus on estimates based on the blocking or subclassification methods from Chapter 17, after using the trimming approach from Chapter 16. In that chapter we estimated the average effect of winning a big prize on average earnings for the next six years to be approximately −$6,000 per year, with a standard error of approximately $1,000. In this section we investigate the plausibility of the unconfoundedness assumption for this data set.

21.6.1 Testing for Effects on Pseudo Outcomes

The data are well-suited for using the methods discussed in Section 21.3, because the data set contains information on earnings (whose value after winning the lottery is the outcome of primary interest) for six years prior to winning the lottery prize, making

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Table 21.1. *Summary Statistics for Selected Lottery Sample for the IRS Lottery Data*

Variable	Label	All ($N = 496$)		Non-Winners ($N_t = 259$)	Winners ($N_c = 237$)		Nor
		Mean	(S.D.)	Mean	Mean	[t-Stat]	Dif
Year Won	(X_1)	6.23	(1.18)	6.38	6.06	−3.0	−0.27
Tickets Bought	(X_2)	3.33	(2.86)	2.19	4.57	9.9	0.90
Age	(X_3)	50.22	(13.68)	53.21	46.95	−5.2	−0.47
Male	(X_4)	0.63	(0.48)	0.67	0.58	−2.1	−0.19
Years of Schooling	(X_5)	13.73	(2.20)	14.43	12.97	−7.8	−0.70
Working Then	(X_6)	0.78	(0.41)	0.77	0.80	0.9	0.08
Earnings Year -6	(Y_{-6})	13.84	(13.36)	15.56	11.97	−3.0	−0.27
Earnings Year -5	(Y_{-5})	14.12	(13.76)	15.96	12.12	−3.2	−0.28
Earnings Year -4	(Y_{-4})	14.21	(14.06)	16.20	12.04	−3.4	−0.30
Earnings Year -3	(Y_{-3})	14.80	(14.77)	16.62	12.82	−2.9	−0.26
Earnings Year -2	(Y_{-2})	15.62	(15.27)	17.58	13.48	−3.0	−0.27
Earnings Year -1	(Y_{-1})	16.31	(15.70)	18.00	14.47	−2.5	−0.23
Pos Earnings Year -6	($Y_{-6} > 0$)	0.69	(0.46)	0.69	0.70	0.3	0.03
Pos Earnings Year -5	($Y_{-5} > 0$)	0.71	(0.45)	0.68	0.74	1.6	0.14
Pos Earnings Year -4	($Y_{-4} > 0$)	0.71	(0.45)	0.69	0.73	1.1	0.10
Pos Earnings Year -3	($Y_{-3} > 0$)	0.70	(0.46)	0.68	0.73	1.4	0.13
Pos Earnings Year -2	($Y_{-2} > 0$)	0.71	(0.46)	0.68	0.74	1.6	0.15
Pos Earnings Year -1	($Y_{-1} > 0$)	0.71	(0.45)	0.69	0.74	1.2	0.10

these variables attractive candidates to play the role of pseudo-outcomes. In this first set of assessments, we focus on analyses using either $X_i^{\rm p} = Y_{i,-1}$ (earnings in the last year before winning) as the selected pre-treatment variable, or $X_i^{\rm p} = Y_{i,-6}$ (earnings in the sixth year before winning) as the selected pre-treatment variable, and in each case the remaining pre-treatment variables as $X_i^{\rm r}$.

The first analysis is design-based and uses $X_i^{\rm p} = Y_{i,-1}$ as the pseudo-outcome. First, note that the difference in average prior earnings for winners and losers is $14.47 - 18.00 = -3.53$ (in thousands of dollars; see Table 21.1). This raw difference is substantial, relative to the estimated effect of winning the lottery of −5.74, and it is statistically significantly different from zero at conventional significance levels. Because this difference cannot be a causal effect of winning the lottery, it must be due to pre-existing differences between the winners and the losers. The question is whether adjusting for the remaining pre-treatment variables removes this difference.

To implement the analyses discussed in this chapter, recall that in the analysis that led to the point estimate of −5.74, we included automatically in the propensity score the selected covariates X_{i2} (`Tickets Bought`), X_{i5} (`Years of Schooling`), X_{i6} (`Working Then`), and the most recent earnings, $Y_{i,-1}$. To make the analysis with the pseudo-outcome as similar as possible to the main analyses, we always include in the propensity score the covariates X_{i2}, X_{i5}, X_{i6}, and the most recent earnings $Y_{i,-2}$. The blocking estimate based on this setup is

$$\hat{\tau}^{\rm strat} = -0.53 \quad (\widehat{\rm s.e.} = 0.58),$$

This is statistically not significantly different from zero at conventional significance levels, and substantively unimportant. It is also small compared to the effect we find for the actual outcome, that is, −5.74.

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Table 21.2. ***Estimates of Average Treatment Effect on Pseudo-outcome for the IRS Lottery Data***

Pseudo-Outcome	Remaining Covariates	Selected Covariates	Est	$(\widehat{\text{s.e.}})$
Y_{-1}	$X_1,\ldots,X_6,Y_{-6},\ldots,Y_{-2},Y_{-6}>0,\ldots,Y_{-2}>0$	X_2,X_5,X_6,Y_{-2}	−0.53	(0.58)
$\frac{Y_{-1}+Y_{-2}}{2}$	$X_1,\ldots,X_6,Y_{-6},\ldots,Y_{-3},Y_{-6}>0,\ldots,Y_{-3}>0$	X_2,X_5,X_6,Y_{-3}	−1.16	(0.71)
$\frac{Y_{-1}+Y_{-2}+Y_{-3}}{3}$	$X_1,\ldots,X_6,Y_{-6},Y_{-5},Y_{-4},Y_{-6}>0,Y_{-5}>0,Y_{-4}>0$	X_2,X_5,X_6,Y_{-4}	−0.39	(0.77)
$\frac{Y_{-1}+\ldots+Y_{-4}}{4}$	$X_1,\ldots,X_6,Y_{-6},Y_{-5},Y_{-6}>0,Y_{-5}>0$	X_2,X_5,X_6,Y_{-5}	−0.56	(0.89)
$\frac{Y_{-1}+\ldots+Y_{-5}}{5}$	$X_1,\ldots,X_6,Y_{-6},Y_{-6}>0$	X_2,X_5,X_6,Y_{-6}	−0.49	(0.87)
$\frac{Y_{-1}+\ldots+Y_{-6}}{6}$	$X_1,\ldots,X_6$	X_2,X_5,X_6	−2.56	(1.55)
Actual outcome Y	$X_1,\ldots,X_6,Y_{-6},\ldots,Y_{-1},Y_{-6}>0,\ldots,Y_{-1}>0$	X_2,X_5,X_6,Y_{-1}	−5.74	(1.14)

Next, we repeat this for five additional choices of the pseudo-outcome. In each of the five additional analyses we take the average of the J most recent pre-treatment earnings as the pseudo-outcome, and use the remaining pre-treatment earnings as pre-treatment variables. The results for all six tests are in Table 21.2. We find that, as long as there are some pre-treatment earnings left in the set of covariates used to remove biases, the estimates are statistically and substantively insignificant. Only with the average of all pre-treatment earnings used as the pseudo-outcome, so that there are no earnings variables among the remaining pre-treatment variables to be used in the adjustment, do we find a substantially and statistically significant estimate. In that case, the point estimate is −2.56 with an estimated standard error of 1.55. It appears that some measures of pre-treatment earnings are required to remove biases and make unconfoundedness plausible, but we do not appear to need more than one such measure.

Finally, we return to the case with the pseudo-outcome equal to the most recent pre-treatment earnings. Now we look at both the effect on the pseudo-outcome, and on the indicator that the pseudo-outcome is positive. In addition, we do this separately for those with positive and zero earnings in the second year prior to winning the lottery (the most recent pre-treatment year left in the set of pre-treatment variables) in order to assess whether the distribution of the pseudo-outcome differs between treatment groups conditional on covariates, excluding the pseudo-outcomes. The number of individuals with positive earnings in the second year prior to winning the lottery is 351, with 145 individuals with zero earnings in that year. In Table 21.3 we present the four estimates separately, as well as a p-value for the overall test. The p-value of 0.13 suggests that there is relatively little evidence that the distributions of the most recent pre-treatment earnings differ by treatment group conditional on the remaining pre-treatment variables.

Overall the pseudo-outcome assessments suggest that, with the rich set of covariates used, for the selected sample with overlap, the unconfoundedness assumption may be a reasonable assumption, and therefore the estimate of −5.74 for the effect of winning the lottery on subsequent earnings is credible.

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Table 21.3. ***Estimates of Average Treatment Effect on Transformations of Pseudo-Outcome for Subpopulations for the IRS Lottery Data***

Pseudo-Outcome	Subpopulation	Est	$\widehat{(\text{s.e.})}$
$\mathbf{1}_{Y_{-1}=0}$	$Y_{-2}=0$	−0.05	(0.04)
$\mathbf{1}_{Y_{-1}=0}$	$Y_{-2}>0$	−0.04	(0.03)
Y_{-1}	$Y_{-2}=0$	−1.46	(0.92)
Y_{-1}	$Y_{-2}>0$	−0.59	(0.58)
		statistic	p-value
Combined statistic (chi-squared, df 4)		5.51	(0.24)

21.6.2 Assessing Effects of Pseudo-Treatments

Next we investigate the plausibility of unconfoundedness through the second approach of testing for the presence of effects of pseudo-treatments, the so-called semi-design approach. In the context of the lottery study it would have been most useful to have a comparison group, say of individuals who did not play the lottery at all. Such individuals might have been expected to be quite different from lottery players in terms of earnings levels and growth. Then we would have two possible control groups: first lottery players who did not win a major prize ("losers"), and second non-lottery players. Then we could have compared the outcome distributions for these two possible control groups. In that case finding that the observed covariates were sufficient to remove differences between non-lottery players and losers would have lent substantial support to the results based on unconfoundedness, precisely because non-lottery players and lottery players might *a priori* have been expected to be quite different in terms of their unobserved economic behavior. However, in the lottery sample we do not have a second control group for whom we are confident that there is no causal effect. Nevertheless, we have a subsample that is almost as good as that, and which we will use for that purpose. Specifically, a subset of the treatment group of lottery winners will be used to serve as such a pseudo-control group. We take the subsample of winners whose yearly prizes were relatively small. For this subset we expect, *a priori*, the causal effects of winning to be modest.

First we define what we mean by "small yearly prize winners." In our sample the median yearly prize is 31.8, and the average is 55.2, all in thousands of dollars. The 75$^{\text{th}}$ percentile is 63.0 per year. First, we take the subsample of 111 yearly winners who won an annual prize less than or equal to $30,000. Even if there is some effect of such a prize on subsequent earnings, one would expect it to be modest compared to the effects of a bigger prize.

Thus, for the purpose of this illustrative analysis, we view those who won more than $30,000 per year as members of the treatment group, and both winners who won a large enough amount to be paid in yearly installments, but less than $30,000 per year, and losers as part of the control group.

The results for these analyses are in Table 21.4. For the winners of prizes less than $30,000, we find that the differences from the losers, after adjusting for all observed covariates, are substantially small and statistically insignificant at conventional levels.

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Table 21.4. ***Estimates of Average Difference in Outcomes for Controls and Small Winners (less than $30,000) for the IRS Lottery Data***

Outcome	Subpopulation	Est	$\widehat{(\text{s.e.})}$
Y_i	All	-0.82	(1.37)
$\mathbf{1}_{Y_i=0}$	$Y_{i,-1} = 0$	-0.02	(0.05)
$\mathbf{1}_{Y_i=0}$	$Y_{i,-1} > 0$	0.07	(0.05)
Y_i	$Y_{i,-1} = 0$	-1.18	(1.10)
Y_i	$Y_{i,-1} > 0$	-0.16	(0.69)
		statistic	p-value
Combined statistic (chi-squared, dof 4)		1.24	(0.87)

21.6.3 Assessing Robustness

Finally, we carry out the robustness analysis from Section 21.5.2. To make the analysis fully comparable to those in Chapter 17, we start by trimming the sample using $X_i^{\text{r}} = Z_i, Y_{i,-2}, \ldots, Y_{i,-6}$ as the pre-treatment variables to create a common sample with which to compare confoundedness and subset unconfoundedness. Starting with the full sample with 259 control units and 237 treated units, for a total of 496 units, this leads to a trimmed sample with $N_{\text{c}} = 179$ control units and $N_{\text{t}} = 148$ treated units for a total of $N = 327$ units. Given the trimmed sample, we estimate the average treatment effect, first using the full set of covariates (justified by unconfoundedness), and second using the restricted set of covariates (justified by subset unconfoundedness). The estimates, based on subclassification on the estimated propensity score with additional adjustment within the blocks by linear regression, are

$$\hat{\tau}^{X}_{\text{sp}} = -6.94 \ \ (\widehat{\text{s.e.}} = 1.20), \qquad \hat{\tau}^{X^{\text{r}}}_{\text{sp}} = -5.92 \ \ (\widehat{\text{s.e.}} = 1.16),$$

for the estimate based on the full and restricted sets of covariates respectively, and based on the selected sample of 327 units. The difference in the estimates is relatively modest, supportive of unconfoundedness.

We also look directly at the differences in adjusted average outcomes,

$$\big(\mathbb{E}[X_i^{\text{p}}|W_i = 0, X_i^{\text{r}}] - \mathbb{E}[X_i^{\text{p}}|W_i = 1, X_i^{\text{r}}]\big)\beta_{\text{t}}^{\text{p}}.$$

Approximating the two conditional expectations by linear functions,

$$\mathbb{E}[X_i^{\text{p}}|W_i = 0, X_i^{\text{r}}] = X_i^{\text{r}}\gamma_{\text{c}}, \qquad \mathbb{E}[X_i^{\text{p}}|W_i = 1, X_i^{\text{r}}] = X_i^{\text{r}}\gamma_{\text{t}},$$

we find

$$\frac{1}{N}\sum_{i=1}^{N}\big(\hat{\mathbb{E}}[X_i^{\text{p}}|W_i = 0, X_i^{\text{r}}] - \hat{\mathbb{E}}[X_i^{\text{p}}|W_i = 1, X_i^{\text{r}}]\big)\hat{\beta}_{\text{t}}^{\text{p}} = -0.13 \ \ \widehat{\text{s.e.}} = 0.12$$

and

$$\frac{1}{N}\sum_{i=1}^{N}\big(\hat{\mathbb{E}}[X_i^{\text{p}}|W_i = 0, X_i^{\text{r}}] - \hat{\mathbb{E}}[X_i^{\text{p}}|W_i = 1, X_i^{\text{r}}]\big)\hat{\beta}_{\text{c}}^{\text{p}} = -0.19 \ \ \widehat{\text{s.e.}} = 0.11,$$

in both cases small relative to the average causal effect estimate of -5.74.

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Again the overall conclusion from these supporting analyses is that unconfoundedness appears to be credible for this data set.

21.7 CONCLUSION

In this chapter we discuss how one can assess the critical unconfoundedness assumption. Although this assumption is not testable, there are three broad classes of methods that can, in some settings, be used to assess whether unconfoundedness is plausible. One of the three classes is design based, not requiring the use of outcome data. One is semi-design, only using control outcome data. The third uses treated and control outcome data. All three classes estimate pseudo-causal effects known, or presumably known, to be equal to zero. If one cannot reject the null hypothesis that (all of) these pseudo-causal effects are equal to zero, one may, cautiously, and with caveats, proceed and accept the unconfoundedness assumption. Rejections of the hypotheses of zero effects, however, do not suggest alternatives to the unconfoundedness assumption. Instead such rejections may simply suggest that it may be impossible to obtain credible inferences regarding the causal effects of interest with the data at hand.

NOTES

Rosenbaum (1987) was one of the first to stress formally the benefits of having multiple control groups when assessing unconfoundedness. His ideas have been used in a variety of ways. Sometimes researchers use the multiple control groups to obtain multiple estimates of the effects of interest and compare those. For a leading example, pre-dating Rosenbaum's work, see Lalonde (1986). Lalonde was interested in comparing experimental estimates to non-experimental estimates of a job-training program. For his non-experimental estimates, he uses comparison groups constructed from the Panel Study of Income Dynamics (PSID) and from the Current Population Survey (CPS). Lalonde then compares estimates of the average effect of the treatment, the job-training program, based on the two different comparison groups. This is a somewhat indirect way of comparing the adjusted differences between the two comparison groups that we discuss in the current chapter.

The idea of using estimates of the effect of the treatment on pseudo-outcomes known not to be affected by the intervention has also a long history. Most often this is in the context of settings with lagged outcomes where one analyzes the data as if the intervention has occurred prior to the time it was actually implemented. See, for example, Heckman and Hotz (1989) and Crump, Hotz, Imbens, and Mitnik (2008).