4 Instrumental-variables designs

An instrumental-variables design relies on the idea of as-if random in yet another way. Consider the challenge of inferring the impact of a given independent variable on a particular dependent variable—where this inference is made more difficult, given the strong possibility that reciprocal causation or confounding may pose a problem for causal inference. The solution offered by the instrumental-variables design is to find an additional variable—an instrument—that is correlated with the independent variable but could not be influenced by the dependent variable or correlated with its other causes. Thus, units are assigned at random or as-if at random, not to the key independent variable of interest, but rather to this instrumental variable.

Recall, for instance, Angrist's (1990 a) study of military conscription discussed in the Introduction. Eligibility for the Vietnam draft was randomly assigned to young men, via numbers from 1 to 366 that were matched to each potential draftee's birth date; men with lottery numbers above a particular cutoff value were not subject to the draft. Comparing men with lottery numbers above and below the cutoff estimates the effect of draft eligibility. This is "intention-to-treat" analysis, as described in Box 4.1: males are compared according their draft eligibility status, regardless of whether they actually served in the military. Intention to treat is a key principle of natural-experimental analysis, and intention-to-treat analysis should usually be reported in write-ups of research results.[1]

However, intention-to-treat analysis estimates the impact of draft eligibility, not actual military service. Since many soldiers who were draft eligible did not serve, while some who were not drafted volunteered, the effects of actual service may differ from the effects of eligibility for service. Intention-to-treat analysis will typically produce a conservative estimate of the effects of service.

[1] The jargon stems from medical trials, in which researchers intend to administer a treatment to those assigned to the treatment group (some of whom may fail to comply with the protocol). See Freedman, Petitti, and Robins (2004) for an example on breast-cancer screening.

Box 4.1 The intention-to-treat principle

In natural experiments, true randomization—or an as-if random process—sorts units such as individuals or municipalities into treatment and control groups. Yet, this does not imply that all units in the study group are actually exposed to the treatment regime that corresponds to these groups. For instance, the Vietnam-era draft lottery established draft eligibility, but not all draft-eligible men served in the military. Policy-makers use a lottery to assign students to receive vouchers for private schools in Colombia, yet some students who receive vouchers may fail to enroll in private schools. In regression-discontinuity designs, a population index or other pre-treatment score might establish eligibility for a program, but some individuals eligible for a program might opt out. Comparisons of people who self-select into treatment and those who do not receive the treatment are a bad idea: such comparisons are subject to confounding and may thus lead to misleading inferences about the effect of treatment.

Analysis of many natural experiments thus depends on the *intention-to-treat* principle. Here, the groups created by the (as-if) randomization are compared, regardless of the choice of individual units to opt in or opt out of a program. In Angrist's (1990 a) study of the Vietnam-era draft lottery, draft-eligible men may be compared to draft-ineligible men—whether they actually served in the military or not. Intention to treat is one of the most important principles of experimental and natural-experimental analysis. Although subjects may be heterogeneous in their response to treatment assignment, the intention-to-treat analysis makes no statistical adjustments for heterogeneity. Instead, randomization or as-if randomization is relied upon to balance the treatment and control groups, up to random error. Intention-to-treat analysis is also useful for purposes of significance testing, that is, for assessing whether observed differences between the treatment and control groups could reasonably have arisen by chance. These topics are discussed further in Chapters 5 and 6.

Instrumental-variables analysis provides an alternative that is often useful. This technique estimates the average effect of actual military service for a particular set of potential soldiers—those who would serve only if they are drafted. Such people are called Compliers, because they comply with the treatment condition to which they are assigned (Imbens and Angrist 1994; Freedman 2006).

Why does this procedure work? In brief, the lottery gives us an instrumental variable—draft eligibility—that is correlated with the treatment variable (actual military service) but that could not be influenced by the dependent variable or correlated with its other causes. Notice that the impact of service on labor-market earnings or political attitudes is typically subject to confounding: those who choose to serve in the military may be different than those who do not, in ways that matter for earnings (Chapter 1). Thus, the group that receives treatment includes soldiers who would choose to serve whether or not they are drafted—that is, volunteers—while the group that receives the control regime does not include any people of this type. Self-selection therefore destroys the *ex ante* symmetry between the treatment and control groups: if propensity to volunteer for the military is correlated with potential earnings or political attitudes, then comparing people who serve in the military with people who do not leads to a biased estimate of the effects of service.

By contrast, randomization to the draft lottery restores the symmetry, since draft eligibility is randomly assigned and is therefore uncorrelated with such confounders, up to random error. The instrumental-variables procedure works because the proportion of Compliers is about the same in the assigned-to-treatment and assigned-to-control groups—due to random assignment. By examining the proportion of units in each group who actually take the treatment, we can estimate the proportion of Compliers in the study group. Using techniques discussed in detail in Chapter 5, we can then adjust the intention-to-treat analysis to estimate the effects of military service for men who would serve only if drafted.

As this discussion makes clear, instrumental variables might be viewed as an analytic strategy, rather than as a distinct type of natural-experimental design—one that may be used to estimate quantities such as the effect of treatment on Compliers in standard natural experiments in which there is imperfect compliance with treatment assignment. Nonetheless, because of the importance instrumental-variables analysis has assumed in recent years—and because some analysts mainly use natural experiments to generate instrumental variables—it is worth discussing this form of design in a separate chapter in Part I of the book. Moreover, as the discussion in subsequent chapters makes clear, instrumental-variables designs often raise specific issues of interpretation. The discussion in this chapter will therefore provide a useful reference point.

The logic of instrumental-variables analysis sometimes carries through to natural experiments with as-if randomization. In effect, the instrumental variable is treated as though it "assigns" units to values of the independent variable in a way that is as-if random, even though often no explicit randomization occurred. However, important conditions must be met for valid

Table 4.1 Selected sources of instrumental-variables designs

Source of instrumental variable	Units in study group	Treatment variable	Outcome variables
Lotteries			
Military drafts	Soldiers	Military service	Earnings, attitudes
Prize lotteries	Lottery players	Overall income	Political attitudes
Judge lotteries	Prisoners	Prison terms	Recidivism
Training invitations	Job-seekers	Job trainings	Wages
School vouchers	Students	Private-school attendance	Educational achievement
Weather shocks			
Rainfall growth	Countries	Economic growth	Civil war
Natural disasters	Countries	Oil prices	Democracy
Age			
Quarter-of-birth	Students	Education	Earnings
Twin studies			
Twin births	Mothers	Number of children	Earnings
Institutional variation			
Electoral cycles	States	Police presence	Crime
Land tenure types	States	Inequality	Public goods
Historical shocks			
Deaths of leaders	Countries	Colonial annexation	Development
Colonial settler mortality	Countries	Current institutions	Economic growth

Note: The table provides a non-exhaustive list of sources of instrumental-variables designs. See Tables 4.2 and 4.3 for references to specific studies.

instrumental-variables analysis. Without true randomization, the approach requires validating as-if randomization; as with other natural experiments, this can be tricky. Moreover, if regression models are used, the assumptions behind the models may or may not be plausible: depending on the application, instrumental-variables designs may be more "design based" or more "model based."

Other assumptions are critical as well, whether treatment assignment is truly randomized or only as-if random. For instance, the instrument must affect the outcome only by influencing treatment receipt, and this so-called "exclusion restriction" may or may not hold: for instance, draft eligibility might shape later earnings not only through actual military service but also through other channels (say, educational receipt). Finally, whether the causal effect of actual treatment for Compliers is an interesting and important parameter depends on the context and research question.

Such analytic and interpretive issues are discussed in much more detail later in the book.[2] The main goal in this chapter, as in the previous two chapters, is instead to survey applications, as a way of motivating discussion of how analysts discover instrumental-variables designs. To this end, Table 4.1 lists several generic sources of instrumental variables, while Tables 4.2 and 4.3 list specific studies that use instrumental variables in true experiments and natural experiments, respectively. The rest of this chapter briefly discusses applications of instrumental variables in true experiments, where the usage parallels that discussed for Angrist's draft lottery study; I then turn to natural experiments, organizing the discussion by the source of the instrumental variable, as in Table 4.1.

4.1 Instrumental-variables designs: true experiments

Instrumental variables are used in true randomized experiments in which some subjects do not comply with treatment assignment. In fact, one of the best ways to understand the logic of instrumental-variables analysis is by analogy to true experiments, in which a random process like a flip of a coin determines which subjects are assigned to treatment—so subjects assigned to receive the treatment are, on average, just like subjects assigned to control. Even in experiments there can be confounding, however, if subjects who accept the treatment are compared to those who refuse it. The decision to accept treatment—e.g., to take the drug if assigned to the treatment regime in a medical trial—is made by the subjects, not the experimenter, and those who choose to accept treatment may be unlike those who do not, in ways that matter for outcomes.

Analysts should therefore compare subjects randomly assigned to treatment to those randomly assigned to control—following Campbell's (1984) admonition to "analyze 'em as you randomize 'em." Again, however, intention-to-treat analysis does not take account of the fact that not all subjects receive the treatment condition to which they are assigned. In true experiments, as in some observational studies, instrumental-variables analysis may be used to estimate the effect of treatment on Compliers—those subjects who follow the treatment regime to which they are assigned. In true experiments, treatment assignment often satisfies two key requirements for an instrumental variable: it is statistically independent of unobserved causes of

[2] See especially Chapters 5 and 8–10.

Table 4.2 Selected instrumental-variables designs (true experiments)

Authors	Substantive focus	Source of instrument
Bloom et al. (1997)	Effect of job training participation on earnings	Random assignment of admission to training program
Burghardt et al. (2001)	Effect of participation in Job Corps program on earnings	Random assignment of admission to training program
Howell et al. (2000)	Effect of enrollment in private school on achievement test scores	Random assignment of offer of school voucher
Krueger (1999)	Effect of class size on achievement test scores	Random assignment to smaller or larger class
Powers and Swinton (1984)	Effect of hours of study on achievement test scores	Random mailing of test preparation materials
Permutt and Hebel (1984, 1989)	Effect of maternal smoking on birth weight	Random assignment of free smoker's counseling

Note: This non-exhaustive list includes published and unpublished studies in political science, economics, and cognate disciplines that analyze randomized controlled experiments, using treatment assignment as an instrument for treatment receipt.

the dependent variable, due to randomization, and it plausibly affects the outcome only through its effect on treatment receipt. Instrumental-variables analysis of true experimental data is a common strategy; Table 4.2 gives examples of a few such studies.[3]

4.2 Instrumental-variables designs: natural experiments

In observational studies, in which assignment to treatment is not under the control of the researcher, the problem of confounding is typically severe, because units self-select into the treatment and control groups. Instrumental-variables analysis can be used to recover the effect of an "endogenous" treatment, that is, a treatment variable that is correlated with confounders. Just as in true experiments, a valid instrumental variable must be independent of other causes of the dependent variable, and it must influence exposure to treatment but have no direct effect on the outcome, other than through its

[3] Some of the studies in Table 4.2 are known as "encouragement" designs, because subjects are randomly assigned to receive encouragement to comply with some treatment—for instance, they are sent test preparation materials to encourage them to study for tests (Powers and Swinton 1984). In such studies, the encouragement to perform some activity serves as an instrumental variable for actual performance of the activity.

Table 4.3 Selected instrumental-variables designs (natural experiments)

Authors	Random or as-if random?	Substantive focus	Source of instrument	Country
Acemoglu, Johnson, and Robinson (2001)	As-if	Effects of institutions on economic growth	Colonial settler mortality rates	Cross-national
Angrist (1990a)	Random	Effect of military service on later labor-market earnings	Randomized draft lottery numbers in the Vietnam war	US
Angrist and Evans (1998)	As-if	Effect of fertility on labor supply	Sibling-sex composition	US
Angrist and Krueger (1991)	As-if	Effect of years of schooling on earnings	Age-based school enrollment laws (quarter-of-birth is the instrument)	US
Bronars and Grogger (1994)	As-if	Effect of fertility on education and labor supply	Occurrence of twin births	US
Card (1995)	As-if	Effect of years of schooling on earnings	Proximity to college	US
Doherty, Green, and Gerber (2005)	Random	Effect of income on political attitudes	Random assignment of lottery winnings, among lottery players	US
Duflo (2001)	As-if	Effect of individual years of schooling on earnings	Region and time variation in school construction	Indonesia
Evans and Ringel (1999)	As-if	Effects of maternal smoking on birth weight	Variation in state cigarette taxes	US
Green and Winik (2010)	Random	Effects of incarceration and probation on recidivism	Random assignment of judges to cases	US
Gruber (2000)	As-if	Effect of disability insurance replacement rates on labor supply	Region and time variation in benefit rules	US
Hidalgo et al. (2010)	As-if	Effects of economic conditions on land invasions in Brazil	Shocks to economic conditions due to rainfall patterns	Brazil
Kling (2006)	Random	Effects of prison term length on employment and earnings	Random assignment of federal judges to cases	US
Levitt (1997)	As-if	Effects of policing on crime	Electoral cycles	US
McClellan, McNeil, and Newhouse (1994)	As-if	Effect of heart attack surgery on health	Proximity to cardiac care centers	US

Table 4.3 (*cont.*)

Authors	Random or as-if random?	Substantive focus	Source of instrument	Country
Miguel, Satyanath and Sergenti (2004)	As-if	Economic growth and civil conflict	Shocks to economic conditions due to rainfall patterns	Cross-national (Africa)
Ramsay (2011)	As-if	Effects of oil price on democracy	Shocks to oil price due to damage from natural disasters	Cross-national

Note: This non-exhaustive list includes published and unpublished studies in political science, economics, and cognate disciplines that have used ostensible natural experiments to generate instrumental variables.

effect on exposure to treatment.[4] These are often strong assumptions, which can be only partially validated from data.

Such instrumental variables can arise both in natural experiments with true randomization—such as those involving lotteries—and those with as-if randomization—for instance, in which weather shocks or other sources of instrumental variables are employed. Table 4.3 lists some examples, several of which I discuss in this section.

4.2.1 Lotteries

Randomized lotteries sometimes supply instrumental variables. In Chapter 2, for instance, I discussed the study by Doherty, Green, and Gerber (2006), who study the relationship between lottery winnings and political attitudes. In this context, we have a standard natural experiment, because the treatment—levels of lottery winnings—is randomly assigned among lottery players (given the kind and number of lottery tickets bought).

However, this lottery study could also provide the foundation for an instrumental-variables design.[5] For example, the relationship between overall income and political attitudes may be subject to confounding, since many factors—such as family background—may shape both income and attitudes. However, here we have an instrumental variable—lottery winnings—that is correlated with overall income and presumably independent of other causes of

[4] The latter condition is sometimes called an "exclusion restriction," in reference to the exclusion of the instrumental variable from a causal equation governing the outcome.

[5] Doherty, Green, and Gerber (2005) use instrumental variables.

political attitudes. This example underscores that whether a given study adopts a standard natural-experimental or an instrumental-variables design depends on the question being asked—for example, whether the question of interest concerns the effect of lottery winnings or of overall income. A number of different assumptions must be met for this instrumental-variables design to be valid; the strengths and limitations of instrumental variables analysis in this context are discussed further in Chapter 9 (see also Dunning 2008c).

Similarly, instrumental-variables designs may arise from lotteries in which there is imperfect compliance with treatment assignment—as in the case of the Vietnam draft lottery. Vouchers for private secondary school in Colombia were allocated by lottery, but not all winners of vouchers used them (while some students who did not receive vouchers paid for private school themselves). Here, vouchers can provide an instrumental variable for private-school attendance, for instance in a study of the effect of private secondary schooling on educational attainment (Angrist et al. 2002). Assignment to a voucher is correlated with enrollment in private secondary schools, and it is presumably independent of other influences on educational attainment—due to the randomization of the lottery. For valid use of an instrumental-variables design in this context, assignment to a voucher must not have a direct effect on educational attainment—above and beyond its influence on private secondary-school enrollment rates—and other assumptions may be required.[6]

4.2.2 Weather shocks

As-if randomization can also provide the basis for instrumental-variables designs. For example, some analysts use weather-induced shocks as instruments for a range of independent variables, from economic growth to commodities prices. Miguel, Satyanath, and Sergenti (2004) study the effect of economic growth on the probability of civil war in Africa, using annual change in rainfall as an instrumental variable. Reciprocal causation poses a major problem in this research—civil war causes economies to grow more slowly—and many difficult-to-measure omitted variables may affect both economic growth and the likelihood of civil war. As Miguel, Satyanath, and Sergenti (2004: 726) point out, "the existing literature does not adequately address the endogeneity of economic variables to civil war and thus does not convincingly establish a causal relationship. In addition to endogeneity,

[6] See Chapters 5 and 9.

omitted variables—for example, government institutional quality—may drive both economic outcomes and conflict, producing misleading cross-country estimates."

However, year-to-year variation in rainfall is plausibly as-if random vis-a-vis these other social and political processes, and it is correlated with economic growth. In other words, year-on-year variation in rainfall "assigns" African countries to rates of economic growth—if only probabilistically—so the predicted value of growth based on changes in rainfall can be analyzed in place of actual economic growth rates. If rainfall is independent of all determinants of civil war other than economic growth, instrumental-variables analysis may allow estimation of the effect of economic growth on conflict, at least for those countries whose growth performance is shaped by variation in rainfall.

Of course, rainfall may or may not be independent of other sources of armed conflict, and it may or may not influence conflict only through its effect on growth (Sovey and Green 2009). If floods wash away the roads, soldiers may not fight, so rainfall might have a direct influence on conflict, above and beyond its effect on growth.[7] Moreover, variation in rainfall may also influence growth only in particular sectors, such as agriculture, and the effect of agricultural growth on civil war may be quite different than the effects of growth in the urban sector (Dunning 2008c). If the model linking economic growth to conflict is incorrectly specified, using rainfall to instrument for growth may capture idiosyncratic rather than general effects.[8] Thus, caution may be advised when extrapolating results or making policy recommendations.

A similar approach is found in Hidalgo et al. (2010), who study the effects of economic growth on land invasions in Brazil. Arguing that reverse causality or omitted variables could be a concern—for instance, land invasions could influence growth, and unmeasured institutions could influence both growth and invasions—these authors use rainfall growth as an instrumental variable for economic growth. The authors find that decreases in growth, instrumented by rainfall, indeed encourage land invasions. Again, this application illuminates characteristic strengths and limitations of instrumental-variables designs. Rainfall shocks may or may not be as-if random; rainfall may or may not influence land invasions only through its effect on growth; and variation in rainfall may also influence growth only in particular sectors, such as agriculture, which may have idiosyncratic effects on the likelihood

[7] Miguel, Satyanath, and Sergenti (2004) consider and dismiss some such violations of the exclusion restriction.

[8] See Chapter 9 for a more detailed discussion.

of invasions (Dunning 2008c). Horiuchi and Saito (2009) use rainfall to instrument for voter turnout in Japan, in a study of the effects of turnout on federal transfers to municipalities.

Ramsay (2011) asks whether oil wealth engenders authoritarian government, as per arguments made by students of the so-called "resource curse" (Ross 2001; see also Dunning 2008d and Haber and Menaldo 2011 for dissenting views). While the political regime type may not determine countries' endowments of natural resources like oil—and while confounding variables associated with political regimes may not be closely associated with natural resource endowments—the amount of oil-based revenue available in a given country may well be a function of features of the political system. However, shocks to oil price due to worldwide damage from natural disasters may be as-if random for oil producers; as in other instrumental-variables designs, they may assign countries to levels of oil revenue in a particular year, in a way that is as-if random. If so, natural disasters may be used to instrument for oil revenue, in a study of the effects of oil revenue on the political regime type (Ramsay 2011).

4.2.3 Historical or institutional variation induced by deaths

Other apparently random, or as good as random, events—for instance, the death of political leaders from natural causes—may sometimes provide the basis for instrumental-variables analysis. Consider, for instance, the natural experiment of Iyer (2010), who compares the long-term developmental effects of two kinds of British rule in India: direct colonial control, in which British administrators collected taxes and administered local governance themselves, and indirect rule, in which native princes collected revenue on behalf of the British but otherwise retained substantial autonomy in matters of internal administration.[9]

A direct comparison of districts in India that were formerly under direct British rule and those under the so-called "native" or "princely" states suggests that the former do better, today, on a range of socioeconomic variables. Districts that were under direct rule during the colonial period are significantly more populated and denser today than districts under indirect rule (Table 4.4)—perhaps suggesting heightened processes of

[9] Princely states enjoyed substantial autonomy in internal administration, though not in matters of defense or external policy. They were absorbed into a single administrative structure after independence in 1947, but they retained control during the colonial period through the second half of the nineteenth century and first half of the twentieth century.

Table 4.4 Direct and indirect colonial rule in India

Variable	Direct rule: mean	Indirect rule: mean	Difference of means (SE)
Log (population)	14.42	13.83	0.59 (0.16)
Population density (persons/km^2)	279.47	169.20	110.27 (41.66)
Proportion illiterate	0.32	0.28	0.04 (0.03)
Mean annual rainfall (mm)	1,503.41	1,079.16	424.25 (151.08)

Note: The table shows the difference of means on key covariates across areas subject to direct and indirect colonial rule in India. SE, standard error.
Source: Based on Iyer (2010).

urbanization associated with socioeconomic development.[10] Moreover, they have also exhibited significantly higher agricultural investment and productivity in the postcolonial period. For example, in the several decades after independence, the average proportions of irrigated land, intensity of fertilizer usage, usage of high-yielding crop varieties, and total agricultural yields were higher in areas formerly under direct British control (Iyer 2010: 693, tables 3–4). Finally, while areas under direct rule provide slightly less in the way of public goods than areas formerly under indirect rule, the difference is not statistically significant. One might therefore conclude that direct British rule had a salutary impact on long-term development.

Yet, is this effect causal? Table 4.4 suggests that annexation of districts by the British was hardly random: for instance, districts under direct rule have about half-again as much annual rainfall as districts left to princely rulers. Annexation was a selective process, and the British may have targeted areas that were likely to be more favorable to agriculture, perhaps because these would generate greater land revenues for the colonial government (Iyer 2010: 698). Confounding factors associated with annexation and economic outcomes could therefore explain long-term developmental contrasts between areas subject to direct and indirect rule.

To confront the problem of confounding, Iyer (2010) relies on an instrumental-variables design. Between 1848 and 1856, the Governor-General of India, Lord Dalhousie, enacted a new policy regarding annexation of native states, announcing:

> I hold that on all occasions where heirs natural shall fail, the territory should be made to lapse and adoption should not be permitted, excepting in those cases in which some strong political reason may render it expedient to depart from this general rule.

[10] However, there is no significant difference in the proportion of the population that is literate across the two types of districts (Table 4.4).

In other words, according to this so-called Doctrine of Lapse, annexation would result from the death of a ruler without a natural (nonadopted) heir. In total, 20 districts of modern-day India were ruled by colonial-era princes who died without natural heirs during Lord Dalhousie's rule; of these, 16 were permanently placed under direct rule, implying that 16/20 or 80 percent of the districts "assigned" to direct rule by the Doctrine of Lapse in fact experienced direct rule. Only 18 of the remaining 161 districts lying under native states were annexed during the colonial period, for a proportion of about 0.11. Thus, assignment to direct rule through the Doctrine of Lapse is strongly associated with actually experiencing direct rule.[11]

Districts in which the ruler died without a natural heir can be compared to districts in which no such heirless death occurred, during Lord Dalhousie's tenure as Governor. This "intention-to-treat" analysis (Box 4.1) shows a nontrivial negative effect of assignment to direct British rule: the former set of districts had significantly fewer middle schools, health centers, and roads than the latter districts in the postcolonial period. Indeed, the former districts measured five percentage points lower on a combined measure of public goods provision than the latter districts, suggesting a substantively important effect of assignment to direct rule.[12]

Iyer's instrumental-variables analysis—in which assignment to annexation by the British under the Doctrine of Lapse serves as an instrument for actual annexation—thus also suggests that direct colonial rule significantly lowered the quantity of public goods. (As we will see in Chapters 5 and 6, intention-to-treat and instrumental-variables analysis will produce estimates with the same sign and statistical significance; in effect, the instrumental-variables analysis simply upweights the intention-to-treat analysis to account for imperfect compliance with treatment assignment.) In other words, the claim that direct rule led to better development outcomes is not supported by careful scrutiny of the evidence.

As always, of course, these conclusions are only as good as the assumptions. The natural experiment presumes that the presence or absence of an heir is as-if random; in other words, the long-term development outcomes that areas *would* experience under direct and indirect rule (i.e., the potential outcomes under treatment and control) are statistically independent of

[11] The "net crossover rate," which estimates the proportion of compliers in the study group (Chapter 5), is therefore $0.8 - 0.11 = 0.69$.

[12] However, the intention-to-treat analyses (aka reduced form regressions) are not presented without control variables, which may substantially undercut their credibility. See Iyer (2010: 705, table 10).

whether their ruler in fact died with or without an heir. Iyer suggests that while heirless death of rulers was fairly common during British rule, the Doctrine of Lapse was only enforced between 1848 and 1857, during Lord Dalhousie's rule. Together with other evidence, this may reinforce the plausibility that whether a ruler happened to die without an heir during this period is as good as random—though it is always possible that places where the ruler died without a natural heir could differ in unobservable ways that are related to long-term development. Techniques for evaluating the plausibility of as-if random are presented in Chapter 8.[13]

To use the absence of an heir as a valid instrumental variable for direct British rule, moreover, Iyer must also posit an "exclusion restriction," as discussed in subsequent chapters. That is, the presence or absence of an heir must not have a direct impact on long-term development outcomes. Iyer shows that princely states in which the ruler died without a natural heir during other historical periods—when Lord Dalhousie's doctrine of lapse was not in effect—do not do significantly worse in terms of providing public goods or lowering infant mortality. Thus, she argues, differential experience with colonial rule—and not the fact of a ruler's heirless death itself—plausibly explains why districts annexed under the Doctrine of Lapse did worse than districts retained by native states.[14]

The importance of these assumptions arises as well in other instrumental-variables designs, for instance, when historical or institutional variables are used as instrumental variables for present-day institutions or economic conditions. Acemoglu, Johnson, and Robinson (2001), in a well-known study of the effects of institutional arrangements on countries' economic performance, use colonial settler mortality rates as an instrumental variable for current institutions. These authors argue that settler mortality rates during colonial years do not affect current economic performance in former colonies, except through their effect on current institutions; they also argue that settler mortality is as good as randomly assigned, at least conditional on covariates. Since neither assumption is verifiable from the data, a combination of historical evidence and a priori reasoning must be used to try to validate, at least partially, these core assumptions. The portion of current institutions that is related to past settler mortality rates may also have idiosyncratic effects on economic growth, which could limit the generalizability of the findings

[13] Another study to leverage the deaths of leaders in office is Jones and Olken (2005), who ask whether such leadership transitions affect countries' growth rates.

[14] Several other familiar assumptions must also be invoked, for instance, the assumption of "no-Defiers" (Chapter 5); see Exercise 5.1.

(Dunning 2008c). Moreover, the response of one country to colonial institutions must be invariant to the assignment of institutions of other countries.[15] The importance and potential limitations of the underlying assumptions of such instrumental-variables analyses will be discussed in subsequent chapters.

4.3 Conclusion

In recent years, instrumental variables have been used to estimate causal effects in many substantive domains. As the examples surveyed in this chapter suggest, instrumental variables can provide an important tool, because they help to confront the problem of confounding—a first-order issue in the social sciences. The examples discussed in this chapter also suggest that ideas generated in one context have been exported and modified in another, sometimes to good effect.

Yet, whether an instrumental variable is valid and useful may depend on the research question and setting. Detailed institutional knowledge is often needed in each new context to evaluate the validity of the technique. The use of instrumental variables often requires strong assumptions, which can be only partially validated from data. Some empirical tests can be performed to assess the central assumption that the instrumental variable is as good as randomly assigned; for instance, the instrument may be shown to be uncorrelated with pre-treatment covariates (those that are determined before the intervention). A priori reasoning and detailed knowledge of the empirical context may also play an important role. In observational studies, because there is often no actual randomization, the validity of as-if random assignment is difficult to validate; this assertion may be classified along a spectrum from “less plausible” to “more plausible” (Chapter 8), but it is difficult to validate the placement of any given study on such a spectrum.

Additional issues arise in many applications, often in connection with the use of multiple regression models (Chapter 9). For instance, concerns about the endogeneity of a single treatment variable will typically lead researchers to use instrumental-variables regression. Yet analysts typically do not discuss the possible endogeneity of other covariates in their multiple regression models. (One reason may be that the number of instruments must equal or surpass the number of endogenous variables, and good instruments are difficult to find.) Furthermore, instruments that are truly random may not be strongly related

[15] This is the so-called “stable unit-treatment value assumption” (SUTVA) discussed in Chapters 5 and 9.

to an endogenous treatment; in this case, substantial small-sample bias can arise. One recommendation for practice may be to report "reduced-form" results. (Reduced-form is a synonym for intention-to-treat; here, the outcome is regressed directly on the instrumental variable.)

Another recommendation may be to report instrumental-variables regressions without covariates; with one endogenous treatment variable and one valid instrument, including covariates can be unnecessary and even be harmful. The estimand should be carefully defined, and analysts should consider difficulties that may arise when extrapolating results to other contexts and types of subjects. In multiple-regression models, the statistical model itself must be validated, to the extent possible; with regression, the identification of causal effects depends not just on the exogeneity of instrumental variables in relation to a posited regression model but also on the validity of the underlying model itself.

Finally, it is important to emphasize that neither of the core criteria for a valid instrumental variable—that it is statistically independent of unobserved causes of the dependent variable and that it only affects the dependent variable through its effect on the endogenous treatment—are directly testable from data. Analysts using instrumental variables should defend these assertions using evidence and reasoning, to the extent possible. Yet especially outside of the experimental context, instrumental-variables estimates should be interpreted with an appropriate degree of caution. I will return further to these themes in subsequent chapters.[16]

Exercises

4.1) As described in the text of this chapter, Iyer (2010) compares former native states in India in which the prince died without a natural heir during Lord Dalhousie's rule—and which were therefore eligible for annexation by the British—with areas in which native rulers did not have an heirless death.

- (a) What is "intention-to-treat" analysis in this context? In particular, what groups are compared in an intention-to-treat analysis of Iyer's (2010) natural experiment?
- (b) How is intention-to-treat analysis related to instrumental-variables analysis?

[16] See especially Chapters 5 and 8–10.