C H A P T E R   5

Causation and Research Design

Identifying causes, figuring out why things happen, is the goal of most social science research. Unfortunately, valid explanations of the causes of social phenomena do not come easily. Since the 1990s, violent crime victimization rates according to the National Crime Victimization Survey have been declining steadily (Catalano 2006). However, decreases in homicide rates have not been uniform across race or ethnicity, age, or geographic location (Ousey & Lee 2004). And in some cities, rates of violence have begun to increase tremendously. For example, by June 1 of 2006, there had already been 803 shooting victims in Philadelphia, which compared to 697 shooting victims by the same time in 2005 (*Philadelphia Inquirer* 2006). Similar increases have been observed in other small cities including Sacramento, CA, Syracuse, NY, and Boston, MA (Stone 2006).

Is the recent rise in violence observed in some cities due to "anger over the Sept. 11 terrorist attack and the economic downturn" (Kershaw 2002:A10)? The release of hard-core convicts who had been imprisoned during the crime wave of the 1980s and early 1990s (Liptak 2004)? Simply a "crime-drop party is over" phenomenon, as criminologist James Alan Fox has suggested (cited in Lichtblau 2000:A2)? And why has the violent crime rate continued its downward trend in some cities like New York City (Dewan 2004a:A25)? Is it

because of Compstat, the city's computer program that identifies to police where crimes are clustering (Dewan 2004b:A1; Kaplan 2002:A3)? Or should credit be given to New York's "Safe Streets, Safe Cities" program, which increased the ranks of police officers (Rashbaum 2002)? What about better emergency room care causing a decline in homicides (Harris et al. 2002)? And what about the decline in usage of crack cocaine on the streets of New York City (Dewan 2004b:C16)? To determine which of these possibilities could contribute to the increase or decline of serious crime, we must design our research strategies carefully.

In this chapter, we first discuss the meaning of causation from two different perspectives—nomothetic and idiographic—and then review the criteria for achieving causally valid explanations. During this review, we give special attention to several key distinctions in research design that are related to our ability to come to causal conclusions: the use of an experimental or nonexperimental design, and reliance on a cross-sectional or longitudinal design. By the end of the chapter, you should have a good grasp of the different meanings of causation and be able to ask the right questions to determine whether causal inferences are likely to be valid. You also may have a better answer about the causes of crime and violence.

CAUSAL EXPLANATION

A cause is an explanation for some characteristic, attitude, or behavior of groups, individuals, or other entities (such as families, organizations, or cities) or for events. Most social scientists seek causal explanations that reflect tests of the types of hypotheses with which you are familiar (see Chapter 3): The independent variable is the presumed cause, and the dependent variable is the potential effect. For example, the study by Sampson and Raudenbush (2001) tested whether disorder in urban neighborhoods (the independent variable) leads to crime (the dependent variable). (As you know, they concluded that it did not, at least not directly.) This type of causal explanation is termed *nomothetic*.

A different type of cause is the focus of some qualitative research (see Chapter 8) and our everyday conversations about causes. In this type of causal explanation, termed *idiographic*, individual events or the behaviors of individuals are explained with a series of related, prior events. For example, you might explain a particular crime as resulting from several incidents in the life of the perpetrator that resulted in a tendency toward violence, coupled with stress resulting from a failed marriage, and a chance meeting.

Nomothetic Causal Explanation

A **nomothetic causal explanation** is one involving the belief that variation in an independent variable will be followed by variation in the dependent variable, when all other things are equal (*ceteris paribus*). In this perspective, researchers who claim a causal effect have concluded that the value of cases on the dependent variable differs from what their value would have been in the absence of variation in the independent variable. For instance, researchers might claim that the likelihood of committing violent crimes is higher for individuals who were abused as children than it would be if these same individuals had not been abused as children. Or, researchers might claim that the likelihood of committing violent crimes is higher for individuals exposed to media violence than it would be if these same individuals

EXHIBIT 5.1   Association: Noise Intensity for Two Groups in an Experiment
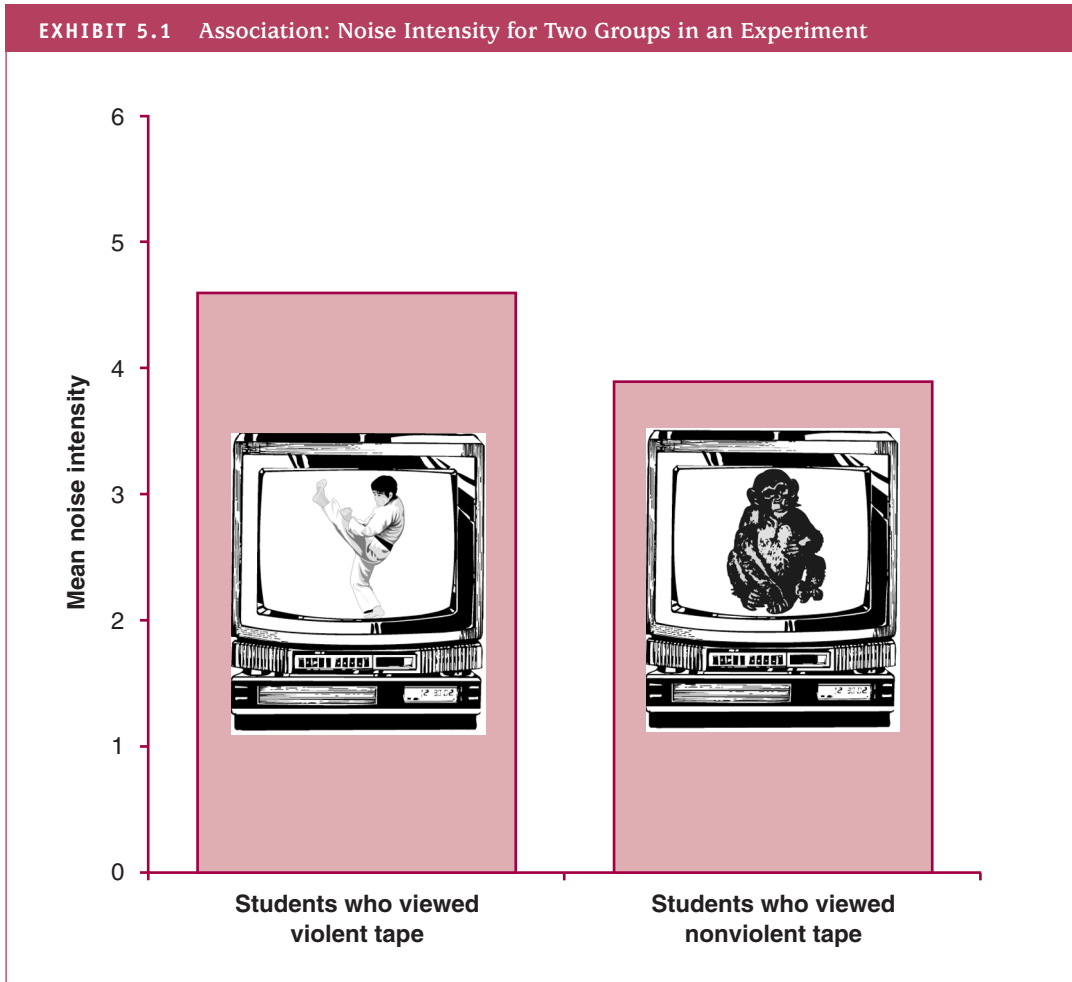
Source: Adapted from Bushman, 1995.

had not been exposed to media violence. The situation as it would have been in the absence of variation in the independent variable is termed the **counterfactual** (see Exhibit 5.1).

Of course, the fundamental difficulty with this perspective is that we never really know what would have happened at the same time to the same people (or groups, cities, and so on) if the independent variable had not varied, because it did. We cannot rerun real-life scenarios (King, Keohane, & Verba 1994). We could observe the aggressiveness of people's behavior before and after they were exposed to media violence. But this comparison involves an earlier time period, when, by definition, the people and their circumstances were not exactly the same.

But we do not need to give up hope! Far from it. We can design research to create conditions that are comparable indeed, so that we can confidently assert our conclusions *ceteris*

paribus, other things being equal. We can examine the impact on the dependent variable of variation in the independent variable alone, even though we will not be able to compare the same people at the same time in exactly the same circumstances except for the variation in the independent variable. And by knowing the ideal standard of comparability, we can improve our research designs and strengthen our causal conclusions even when we cannot come so close to living up to the meaning of *ceteris paribus*.

Quantitative researchers seek to test nomothetic causal explanations with either experimental or nonexperimental research designs. However, the way in which experimental and nonexperimental designs attempt to identify causes differs quite a bit. It is very hard to meet some of the criteria for achieving valid nomothetic causal explanations using a nonexperimental design. Most of the rest of this chapter is devoted to a review of these causal criteria and a discussion of how experimental and nonexperimental designs can help to establish them.

Causal effect (nomothetic perspective) When variation in one phenomenon, an independent variable, leads to or results, on average, in variation in another phenomenon, the dependent variable.

Example of a nomothetic causal effect: Individuals arrested for domestic assault tend to commit fewer subsequent assaults than do similar individuals who are accused in the same circumstances but not arrested.

Idiographic Causal Explanation

The other meaning of the term *cause* is one that we have in mind very often in everyday speech. This is **idiographic causal explanation**: the concrete, individual sequence of events, thoughts, or actions that resulted in a particular outcome for a particular individual or that led to a particular event (Hage & Meeker 1988). An idiographic explanation also may be termed an *individualist* or a *historicist explanation*.

Causal effect (idiographic perspective) When a series of concrete events, thoughts, or actions result in a particular event or individual outcome.

Example of an idiographic causal effect: An individual is neglected by his parents. He comes to distrust others, has trouble maintaining friendships, has trouble in school, and eventually gets addicted to heroin. To support his habit, he starts selling drugs and is ultimately arrested and convicted for drug trafficking.

A causal explanation that is idiographic includes statements of initial conditions and then relates a series of events at different times that led to the outcome, or causal effect. This narrative or story, is the critical element in an idiographic explanation, which may therefore be classified as narrative reasoning (Richardson 1995:200–201). Idiographic explanations focus on particular social actors, in particular social places, at particular social times

(Abbott 1992). Idiographic explanations are also typically very concerned with context, with understanding the particular outcome as part of a larger set of interrelated circumstances. Idiographic explanations thus can be termed holistic.

Idiographic explanation is deterministic, focusing on what caused a particular event to occur or what caused a particular case to change. As in nomothetic explanations, idiographic causal explanations can involve counterfactuals, by trying to identify what would have happened if a different circumstance had occurred. But unlike in nomothetic explanations, in idiographic explanations the notion of a probabilistic relationship, an average effect, does not really apply. A deterministic cause has an effect in every case under consideration.

Anderson's (1990) field research in a poor urban community produced a narrative account of how drug addiction can result in a downward slide into residential instability and crime:

> When addicts deplete their resources, they may go to those closest to them, drawing them into their schemes. . . . The family may put up with the person for a while. They provide money if they can. . . . They come to realize that the person is on drugs. . . . Slowly the reality sets in more and more completely, and the family becomes drained of both financial and emotional resources. . . . Close relatives lose faith and begin to see the person as untrustworthy and weak. Eventually the addict begins to "mess up" in a variety of ways, taking furniture from the house [and] anything of value. . . . Relatives and friends begin to see the person . . . as "out there" in the streets. . . . One deviant act leads to another. (Pp. 86–87)

An idiographic explanation like Anderson's (1990) pays close attention to time order and causal mechanisms. Nonetheless, it is difficult to make a convincing case that one particular causal narrative should be chosen over an alternative narrative (Abbott 1992). Does low self-esteem result in vulnerability to the appeals of drug dealers, or does a chance drug encounter precipitate a slide in self-esteem? The prudent causal analyst remains open to alternative explanations.

RESEARCH DESIGNS AND CRITERIA FOR CAUSAL EXPLANATIONS

In the movie *Money Train*, two men spray the inside of a subway token booth with a flammable liquid, blowing up the toll booth and killing the collector. In 1995, while the movie was still showing in theaters, a similar incident actually occurred in a New York City subway. The toll collector was hospitalized with widespread third-degree burns. The media violence, it was soon alleged, had caused the crime. How would you evaluate this claim? What evidence do we need to develop a valid conclusion about a hypothesized causal effect? Imagine a friend saying, after reading about the *Money Train* incident, "See, media violence causes people to commit crimes." Of course, after reading Chapter, 1 you would not be so quick to jump to such a conclusion. "Don't overgeneralize," you would remind yourself. When your friend insists, "But I recall that type of thing happening before," you might even suspect selective observation. As a blossoming criminological researcher, you now know that if we want to have confidence in the validity of our causal statements, we must meet a higher standard.

How research is designed influences our ability to draw causal conclusions. In this section, we will introduce the features that need to be considered in a research design in order to evaluate how well it can support nomothetic causal conclusions.

Five criteria must be considered when deciding whether a causal connection exists. When a research design leaves one or more of the criteria unmet, we may have some important doubts about causal assertions the researcher may have made. The first three of the criteria are generally considered the necessary and most important basis for identifying a nomothetic causal effect: empirical association, appropriate time order, and nonspuriousness. The other two criteria, identifying a causal mechanism and specifying the context in which the effect occurs, can also considerably strengthen causal explanations although many do not consider them as requirements for establishing a causal relationship.

Conditions necessary for determining causality:

1. empirical association

2. appropriate time order

3. nonspuriousness

Conditions important in specifying causal relationships:

1. mechanism

2. context

We will use Brad Bushman's (1995) experiment on media violence and aggression to illustrate the five criteria for establishing causal relationships. Bushman's study focused in part on this specific research question: Do individuals who view a violent videotape act more aggressively than individuals who view a nonviolent videotape?

Undergraduate psychology students were recruited to watch a 15-minute videotape in a screening room, one student at a time. Half of the students watched a movie excerpt that was violent (from *Karate Kid III*), and half watched a nonviolent movie excerpt (from *Gorillas in the Mist*). After viewing the videotape, the students were told that they were to compete with another student, in a different room, on a reaction-time task. When the students saw a light cue, they were to react by trying to click a computer mouse faster than their opponent. On a computer screen, the students set a level of radio static that their opponents would hear when the opponents reacted more slowly. The students themselves heard this same type of noise when they reacted more slowly than their opponents, at the intensity level supposedly set by their opponents.

Each student in the study participated in 25 trials, or competitions, with the unseen opponent. Their aggressiveness was operationalized as the intensity of noise that they set for their opponents over the course of the 25 trials. The louder the noise level they set, the more aggressively they were considered to be behaving toward their opponents. The question that we will focus on first is whether students who watched the violent video behaved more aggressively than those who watched the nonviolent video.

Association

The results of Bushman's (1995) experiment are represented in Exhibit 5.1. The average intensity of noise administered to the opponent was indeed higher for students who watched the violent videotape than for those who watched the nonviolent videotape. But is Bushman justified in concluding from these results that viewing a violent videotape increased aggressive behavior in his subjects? Would this conclusion have any greater claim to causal validity than the statement that your friend made in response to the Money Train incident? Perhaps it would.

If for no other reason, we can have greater confidence in Bushman's (1995) conclusion because he did not observe just one student who watched a violent video and then acted aggressively, as was true in the *Money Train* incident. Instead, Bushman observed a number of students, some of whom watched a violent video and some of whom did not. So his conclusion is based on finding an **association** between the independent variable (viewing of a violent videotape) and the dependent variable (likelihood of aggressive behavior).

Time Order

Association is a necessary criterion for establishing a causal effect, but it is not sufficient. Suppose you find in a survey that most people who have committed violent crimes have also watched the movie *Money Train*, and that most people who have not committed violent crimes have not watched the movie. You believe you have found an association between watching the movie and committing violent crimes. But imagine you learn that the movie was released after the crimes were committed. Thus, those people in your survey who said they had seen the movie had actually committed their crimes before the movie characters committed their crimes. Watching the movie, then, could not possibly have led to the crimes. Perhaps the criminals watched the movie because committing violent crimes made them interested in violent movies.

This discussion points to the importance of the criterion of **time order**. To conclude that causation was involved, we must see that cases were exposed to variation in the independent variable *before* variation in the dependent variable. Bushman's (1995) experiment satisfied this criterion because he controlled the variation in the independent variable: All the students saw the videotape excerpts (which varied in violent content) before their level of aggressiveness was measured.

Nonspuriousness

Even when research establishes that two variables are associated and that variation in the independent variable precedes variation in the dependent variable, we cannot be sure we identified a causal relationship between the two variables. Have you heard the old adage "Correlation does not prove causation"? It is meant to remind us that an association between two variables might be caused by something else. If we measure children's shoe sizes and their academic knowledge, for example, we will find a positive association. However, the association results from the fact that older children have larger feet as well as more academic knowledge. Shoe size does not cause knowledge or vice versa.

Before we conclude that variation in an independent variable causes variation in a dependent variable, we must have reason to believe that the relationship is nonspurious. **Nonspuriousness** is a relationship between two variables that is not due to variation in a third variable. When this third variable, an **extraneous variable**, causes the variation, it is said to have created a **spurious relationship** between the independent and dependent variables. We must design our research so that we can see what happens to the dependent variable when only the independent variable varies. If we cannot do this, there are other statistical methods we must use to control the effects of other variables we also believe are related to our dependent variable. (You will be relieved to know that a discussion of these statistical techniques is way beyond the scope of this text!)

In reality, then, the fact that someone blew up a toll booth after seeing the movie *Money Train* might be related to the fact that he was already feeling enraged against society. This led him to seek out a violent movie for entertainment purposes (see Exhibit 5.2). Thus, seeing the violent movie itself in no way led him to commit the crime. We must be sure that all three conditions of association, time order, and nonspuriousness are met before we make such claims.

Does Bushman's (1995) claim of a causal effect rest on any stronger ground? To evaluate nonspuriousness, you need to know about one more feature of his experiment. He assigned students to watch either the violent video or the nonviolent video randomly, that is, by the toss of a coin. Because he used **random assignment**, the characteristics and attitudes that students already possessed when they were recruited for the experiment could

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EXHIBIT 5.2   A Spurious Relationship

Spurious relationship

**The extraneous variable creates
the spurious relationship**
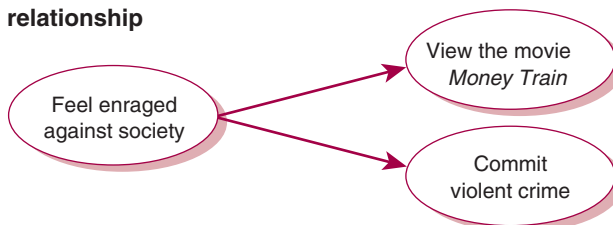
not influence either of the two videos they watched. As a result, the students' characteristics and attitudes could not explain why one group reacted differently from the other after watching the videos. In fact, because Bushman used 296 students in his experiment, it is highly unlikely that the violent video group and the nonviolent video group differed in any relevant way at the outset, even on the basis of chance. This experimental research design meets the criterion of nonspuriousness. Bushman's conclusion that viewing video violence causes aggressive behavior thus rests on firm ground indeed.

Causal (internal) validity is achieved by meeting the criteria of association, time order, and nonspuriousness. Others, however, believe that two additional criteria should also be considered: mechanism and context.

Mechanism

Confidence in a conclusion that two variables have a causal connection will be strengthened if a **mechanism**, some discernable means of creating a connection, can be identified (Cook & Campbell 1979:35; Marini & Singer 1988). Many social scientists (and scientists in other fields) argue that a causal explanation is not adequate until a causal mechanism is identified. What process or mechanism actually is responsible for the relationship between the independent and dependent variables?

Bushman (1995) did not empirically identify a causal mechanism in his experiment, but he did suggest a possible causal mechanism for the effect of watching violent videos. Before we can explain this causal mechanism, we have to tell you about one more aspect of his research. He just was not interested in whether viewing violent films resulted in aggressive behavior. Actually, his primary hypothesis was that individuals who are predisposed to aggression before the study began would be more influenced by a violent film than individuals who were not aggressive at the outset. And that is what happened: Individuals who were predisposed to aggression became more aggressive after watching Bushman's violent video, but individuals who were not predisposed to aggression did not become more aggressive.

After the experiment, Bushman (1995) proposed a causal mechanism to explain why aggressive individuals became even more aggressive after watching the film:

> High trait aggressive individuals [people predisposed to aggression] are more susceptible to the effects of violent media than are low trait aggressive individuals because they possess a relatively large network of aggressive associations that can be activated by violent cues. Habitual exposure to television violence might be partially responsible. (P. 959)

Note that this explanation relies more on speculation than on the actual empirical evidence from this particular experiment. Nonetheless, by proposing a reasonable causal mechanism that connects the variation in the independent and dependent variables, Bushman (1995) strengthens the argument for the causal validity of his conclusions.

It is often possible to go beyond speculation by designing research to test one or more possible causal mechanisms. Perhaps other researchers will design a new study to measure directly the size of individuals' networks of aggressive associations that Bushman (1995) contends are part of the mechanism by which video violence influences aggressive behavior.

Context

In the social world, it is virtually impossible to claim that one and only one independent variable is responsible for causing or affecting a dependent variable. Stated another way, no cause can be separated from the larger **context** in which it occurs. A cause is really only one of a set of interrelated factors required for the effect (Hage & Meeker 1988; Papineau 1978). When relationships among variables differ across geographic units like counties or across other social settings, researchers say there is a **contextual effect**. Identification of the context in which a causal relationship occurs can help us to understand that relationship.

Some researchers argue that we do not fully understand the causal effect of media violence on behavioral aggression unless we have identified these other related factors. As we have just seen, Bushman (1995) proposed at the outset of his research at least one other condition: Media violence would increase aggression only among individuals who were already predisposed to aggression.

Identification of the context in which a causal effect occurs is not a criterion for a valid causal conclusion. Some contextual factors may not turn out to be causes of the effect being investigated. The question for researchers is, "How many contexts should we investigate?" In a classic study of children's aggressive behavior in response to media violence, Bandura, Ross, and Ross (1963) examined several contextual factors. They found that effects varied with the children's gender and with the gender of the opponent toward whom they acted aggressively, but not with whether they saw a real (acted) or filmed violent incident. For example, children reacted more aggressively after observing men committing violent acts than after observing women committing these same acts. But Bandura et al. did not address the role of violence within the children's families or the role of participation in sports or many other factors that could be involved in children's responses to media violence. Bandura et al. strengthened their conclusions by focusing on a few likely contextual factors.

Specifying the context for a causal effect helps us to understand that effect, but it is a process that can never really be complete. We can always ask what else might be important: In which country was the study conducted? What are the ages of the study participants? We need to carefully review the results of prior research and the implications of relevant theory to determine what contextual factors are likely to be important in a causal relationship. Our confidence in causal conclusions will be stronger when we know these factors are taken into account.

In summary, before researchers can infer a causal relationship between two variables, three criteria are essential: empirical association, appropriate time order, and nonspuriousness. After these three conditions have been met, two other criteria are also important: causal mechanism and context.

RESEARCH DESIGNS AND CAUSALITY

How research is designed influences our ability to draw causal conclusions. Obviously, if you conclude that playing violent video games causes violent behavior after watching your

8-year-old nephew playing a violent video game and then hitting his 4-year-old brother, you would be on shaky empirical ground. In this section, we will introduce features that need to be considered in a research design in order to evaluate how well it can support nomothetic causal conclusions.

True Experiments

In a true experiment, the time order is determined by the researcher. The experimental design provides the most powerful design for testing causal hypotheses about the effect of a treatment or some other variable whose values can be manipulated by the researchers. It is so powerful for testing causal hypotheses because it allows us to establish the three criteria for causality with a great deal of confidence. The Bushman (1995) study we examined in the last section was a true experiment.

True experiments must have at least three things:

1. Two comparison groups, one receiving the experimental condition (e.g., treatment or intervention) termed the experimental group and the other receiving no treatment or intervention or another form thereof, termed the control group.

2. Random assignment to the two (or more) comparison groups.

3. Assessment of change in the dependent variable for both groups after the experimental condition has been received.

The combination of these features permits us to have much greater confidence in the validity of causal conclusions than is possible in other research designs. Confidence in the validity of an experiment's findings is further enhanced by identification of the causal mechanism and control over the context of an experiment. We will discuss a experimental designs in more detail in the next chapter (see Chapter 6). For now, we want to highlight how true experimental designs lend themselves to meeting the criteria necessary for causality.

Causality and True Experimental Designs

A prerequisite for meeting each of the three criteria to identify causal relations is maintaining control over the conditions subjects are exposed to after they are assigned to the experimental and comparison groups. If these conditions begin to differ, the variation between the experimental and comparison groups will not be what was intended. Even a subsequent difference in the distribution of cases on the dependent variable will not provide clear evidence of the effect of the independent variable. Such unintended variation is often not much of a problem in laboratory experiments where the researcher has almost complete control over the conditions and can ensure that these conditions are nearly identical for both groups. But control over conditions can become a very big concern for experiments that are conducted in the field in real-world settings, such as Sherman and Berk's (1984) study of the deterrent effects of arrest on intimate partner assaults.

Let us examine how well true experiments meet the criteria necessary for establishing causality in greater detail:

Association between the hypothesized independent and dependent variables. As you have seen, experiments can provide unambiguous evidence of association by randomly assigning subjects to experimental and comparison groups.

Time order of effects of one variable on the others. Unquestionably, the independent variable (treatment of condition) preceded the posttest measures the experiments described so far. For example, arrest for partner abuse preceded recidivism in the Sherman and Berk (1984) study, and the exposure to media violence preceded the aggression in the Bushman (1995) experiment. In experiments with a pretest, time order can be established by comparing posttest to pretest scores. In experiments with random assignment of subjects to the experimental and comparison groups, time order can be established by comparison of posttest scores only.

Nonspurious relationships between variables. Nonspuriousness is difficult to establish; some would say it is impossible to establish in nonexperimental designs. The random assignment of subjects to experimental and comparison groups makes true experiments powerful designs for testing causal hypotheses. Random assignment controls a host of possible extraneous influences that can create misleading, spurious relationships in both experimental and nonexperimental data. If we determine that a design has used randomization successfully, we can be much more confident in the causal conclusions.

Mechanism that creates the causal effect. The features of true experiment do not, in themselves, allow identification of causal mechanisms; as a result there can be some ambiguity about how the independent variable influenced the dependent variable and the causal conclusions.

Context in which change occurs. Control over conditions is more feasible in many experimental designs than it is in nonexperimental designs, but it is often difficult to control conditions in field experiments. In the next chapter, you will learn how the lack of control over experimental conditions can threaten internal validity.

Nonexperimental Designs

Nonexperimental research designs can be either cross-sectional or longitudinal. In a **cross-sectional research design**, all data are collected at one point in time. Identifying the **time order** of effects—what happened first, and so on—is critical for developing a causal analysis, but can be an insurmountable problem with a cross-sectional design. In **longitudinal research designs**, data are collected at two or more points in time, and so identification of the time order of effects can be quite straightforward. An experiment, of course, is a type of longitudinal design because subjects are observed at 2 or more points in time.

Cross-Sectional Designs

Much of the research you have encountered so far in this text has been cross-sectional. Although each of surveys and interviews take some time to carry out, if they measure the actions, attitudes, and characteristics of respondents at only one time, they are considered cross-sectional. The name comes from the idea that a snapshot from a cross-section of the population is obtained at one point in time.

As you learned in chapter 3, Sampson and Raudenbush (1999) used a very ambitious cross-sectional design to study the effect of visible public social and physical disorder on the crime rate in Chicago neighborhoods. Their theoretical framework focused on the concept of informal social control: the ability of residents to regulate social activity in their neighborhoods through their collective efforts according to desired principles. They believed that informal social control would vary between neighborhoods, and they hypothesized that it was the strength of informal social control that would explain variation in crime rates rather than just the visible sign of disorder. They contrasted this prediction to the "broken windows" theory: the belief that signs of disorder themselves cause crime. Their findings supported their hypothesis: both visible disorder and crime were consequences of low levels of informal social control (measured with an index of "collective efficacy"). One did not cause the other (see Exhibit 5.3).


EXHIBIT 5.3   The Effect of Informal Social Control
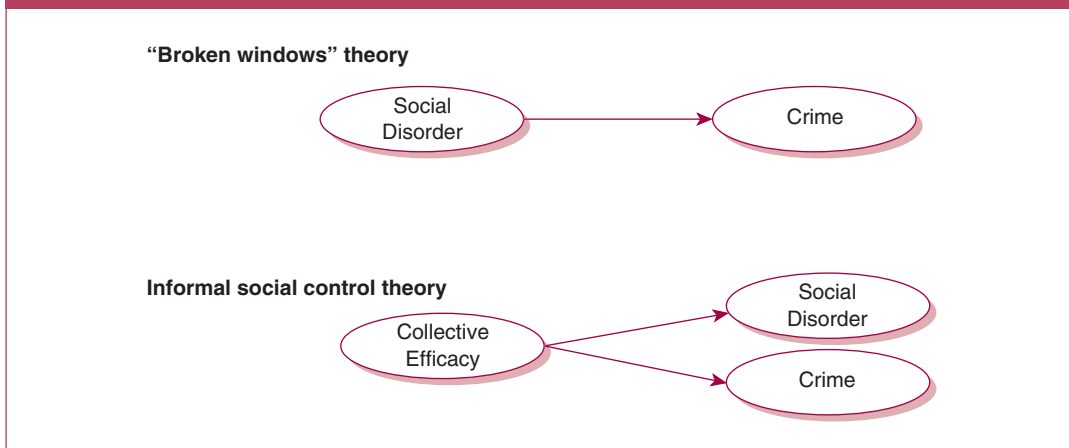
Source: Based on Sampson & Raudenbush 1999:635.

In spite of these compelling findings (see Exhibit 5.4), Sampson and Raudenbush's (1999) cross-sectional design could not establish directly that the variation in the crime rate occurred after variation in informal social control. Maybe it was a high crime rate that led residents to stop trying to exert much control over deviant activities in the neighborhood, perhaps because of fear of crime. It is difficult to discount such a possibility when only cross-sectional data are available.

EXHIBIT 5.4 Effect of Social Disorder and Collective Efficacy on Personal Violent Crimes
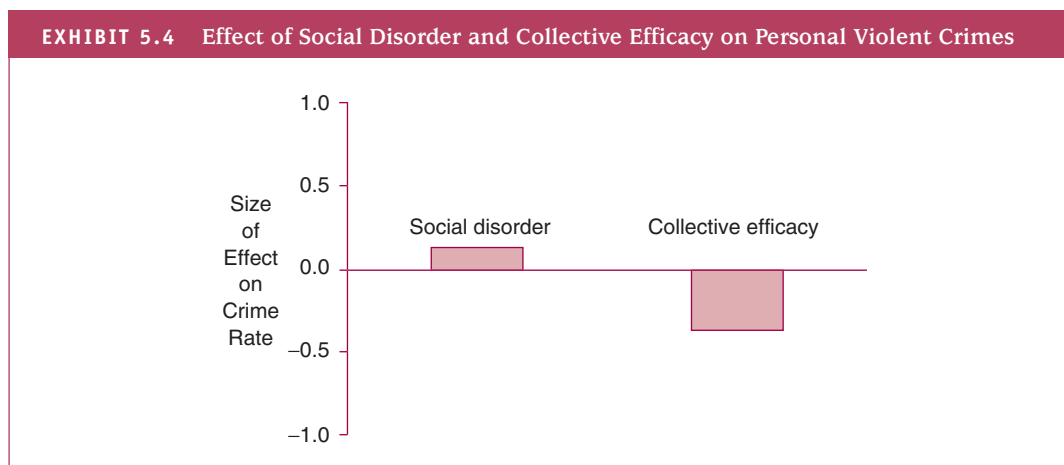
Source: Adapted from Sampson & Raudenbush 1999.

There are four special circumstances in which we can be more confident in drawing conclusions about time order on the basis of cross-sectional data. Because in these special circumstances the data can be ordered in time, they might even be thought of as longitudinal designs (Campbell 1992).

The independent variable is fixed at some point prior the variation in the dependent variable. So-called demographic variables that are determined at birth—such as sex, race, and age— are fixed in this way. So are variables like education and marital status, if we know when the value of cases on these variables was established and if we know that the value of cases on the dependent variable was set some time afterward. For example, say we hypothesize that educational opportunities in prison affect recidivism rates. Let us say we believe those inmates who are provided with greater educational and vocational opportunities in prison will be less likely to reoffend after release from prison. If we know that respondents completed their vocational or other educational training before leaving prison, we would satisfy the time order requirement even if we were to measure education at the same time we measure recidivism after release. However, if some respondents possibly went back to school after prison release, the time order requirement would not be satisfied.

We believe that respondents can give us reliable reports of what happened to them or what they thought at some earlier point in time. Horney, Osgood, and Marshall (1995) provide an interesting example of the use of such retrospective data. The researchers wanted to identify how criminal activity varies in response to changes in life circumstances. They interviewed 658 newly convicted male offenders sentenced to a Nebraska state prison. In a 45- to 90-minute interview, they recorded each inmate's report of his life circumstances and of his criminal activities for the preceding 2 to 3 years. They then found that criminal involvement was related strongly to adverse changes in life circumstances, such as marital separation or drug use. Retrospective data are often inadequate for measuring variation in past psychological states or behaviors, however, because what we recall about our feeling or actions in the past

is likely to influenced by what we feel in the present. For example, retrospective reports by both adult alcoholics and their parents appear to greatly overestimate the frequency of childhood problems (Vaillant 1995). People cannot report reliably the frequency and timing of many past events, from hospitalization to hours worked. However, retrospective data tend to be reliable when it concerns major, persistent experiences in the past, such as what type of school someone went to or how a person's family was structured (Campbell 1992).

Our measures are based on records that contain information on cases in earlier periods. Government, agency, and organizational records are an excellent source of time-ordered data after the fact. However, sloppy record keeping and changes in data-collection policies can lead to inconsistencies, which must be taken into account. Another weakness of such archival data is that they usually contain measures of only a fraction of the variables that we think are important.

We know that the value of the dependent variable was similar for all cases prior to the treatment. For example, we may hypothesize that an anger management program (independent variable) improves the conflict resolution abilities (dependent variable) of individuals arrested for intimate partner assault. If we know that none of the arrested individuals could employ verbal techniques for resolving conflict prior to the training program, we can be confident that any subsequent variation in their ability do so did not precede exposure to the training program. This is one way that traditional experiments establish time order: Two or more equivalent groups are formed prior to exposing one of them to some treatment.
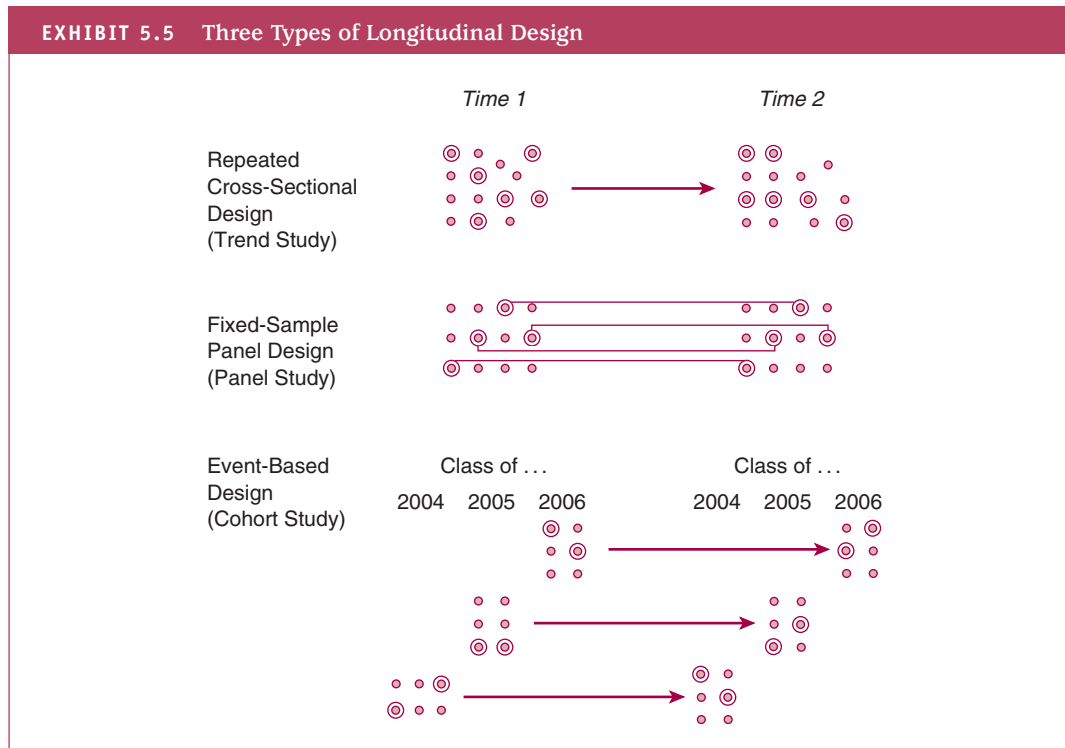
Longitudinal Designs

In longitudinal research, data are collected at 2 or more points in time and, as such, data can be ordered in time. By measuring the value of cases on an independent variable and a dependent variable at different times, the researcher can determine whether variation in the independent variable precedes variation in the dependent variable.

In some longitudinal designs, the same sample (or panel) is followed over time; in other designs, sample members are rotated or completely replaced. The population from which the sample is selected may be defined broadly, as when a longitudinal survey of the general population is conducted. Or the population may be defined narrowly, as when members of a specific age group are sampled at multiple points in time. The frequency of follow-up measurement can vary, ranging from a before-and-after design with just one follow-up to studies in which various indicators are measured every month for many years.

Certainly it is more difficult to collect data at two or more points in time than at one time. Quite frequently researchers simply cannot, or are unwilling to, delay completion of a study for even 1 year in order to collect follow-up data. But think of the many research questions that really should involve a much longer follow-up period: Does community-oriented policing decrease rates of violent crime? What is the impact of job training in prison on recidivism rates? How effective are batterer-treatment programs for individuals convicted of intimate partner assault? Do parenting programs for young mothers and fathers reduce the likelihood of their children becoming delinquent? It is safe to say that we will never have enough longitudinal data to answer many important research questions. Nonetheless, the value of longitudinal data is so great that every effort should be made to develop longitudinal research designs when they are appropriate for the research question asked. The

EXHIBIT 5.5   Three Types of Longitudinal Design

following discussion of the three major types of longitudinal designs will give you a sense of the possibilities (see Exhibit 5.5).

Repeated Cross-Sectional Designs

Studies that use a **repeated cross-sectional design**, also known as **trend studies**, have become fixtures of the political arena around election time. Particularly in presidential election years, we have all become accustomed to reading weekly, even daily, reports on the percentage of the population that supports each candidate. Similar polls are conducted to track sentiment on many other social issues. For example, a 1993 poll reported that 52% of adult Americans supported a ban on the possession of handguns, compared to 41% in a similar poll conducted in 1991. According to pollster Louis Harris, this increase indicated a "sea change" in public attitudes (cited in Barringer 1993). Another researcher said, "It shows that peoples are responding to their experience [of an increase in handgun-related killings]" (cited in Barringer 1993:A14).

Repeated cross-sectional design (trend study) A type of longitudinal study in which data are collected at two or more points in time from different samples of the same population.

Repeated cross-sectional surveys are conducted as follows:

1. A sample is drawn from a population at Time 1, and data are collected from the sample.
2. As time passes, some people leave the population and others enter it.
3. At Time 2, a different sample is drawn from this population.

These features make the repeated cross-sectional design appropriate when the goal is to determine whether a population has changed over time. Has racial tolerance increased among Americans in the past 20 years? Are prisons more likely to have drug-treatment programs available today than they were in the 1950s? These questions concern changes in the population as a whole, not changes in individuals within the population. We want to know whether racial tolerance increased in society, not whether this change was due to migration that brought more racially tolerant people into the country or to individual U.S. citizens becoming more tolerant. We are asking whether state prisons overall are more likely to have drug-treatment programs available today than they were a decade or two decades ago, not whether any such increase was due to an increase in prisoner needs or to individual prisons changing their program availability. When we do need to know whether individuals in the population changed, we must turn to a panel design.

Fixed-Sample Panel Designs

Panel designs allow us to identify changes in individuals, groups, or whatever we are studying. This is the process for conducting **fixed-sample panel designs**:

1. A sample (called a panel) is drawn from a population at Time 1, and data are collected from the sample.
2. As time passes, some panel members become unavailable for follow-up, and the population changes.
3. At time 2, data are collected from the same people as at Time 1 (the panel), except for those people who cannot be located.

> *Fixed-sample panel design (panel study)* A type of longitudinal study in which data are collected from the same individuals—the panel—at two or more points in time. In another type of panel design, panel members who leave are replaced with new members.

Because a panel design follows the same individuals, it is better than a repeated cross-sectional design for testing causal hypotheses. For example, Sampson and Laub (1990) used a fixed-sample panel design to investigate the effect of childhood deviance on adult crime. They studied a sample of white males in Boston when the subjects were between 10 and 17 years old and then followed up when the subjects were in their adult years. Data were collected from multiple sources, including the subjects themselves and criminal justice

records. Sampson and Laub (p. 614) found that children who had been committed to a correctional school for persistent delinquency were much more likely than other children in the study to commit crimes as adults: 61% were arrested between the ages of 25 and 32, compared to 14% of those who had not been in correctional schools as juveniles. In this study, juvenile delinquency unquestionably occurred before adult criminality. If the researchers had used a cross-sectional design to study the past of adults, the juvenile delin-quency measure might have been biased by memory lapses, by self-serving recollections about behavior as juveniles, or by loss of agency records.

If you now wonder why every longitudinal study is not designed as a panel study, you have understood the advantages of panel designs. However, remember that this design does not in itself establish causality. Variation in both the independent variable and the dependent variables may be due to some other variable, even to earlier variation in what is considered the dependent variable. In the example in Exhibit 5.6, there is a hypothesized association between delinquency in the 11th grade and grades obtained in the 12th grade (the dependent variable). The time order is clear. However, both variables are consequences of grades obtained in the 7th grade. The apparent effect of 11th-grade delinquency on 12th-grade grades is spurious because of variation in the dependent variable (grades) at an earlier time.

Panel designs are also a challenge to implement successfully, and often are not even attempted, because of two major difficulties:

Expense and attrition. It can be difficult, and very expensive, to keep track of individuals over a long period, and inevitably the proportion of panel members who can be located for follow-up will decline over time. Panel studies often lose more than one quarter of their members through attrition (Miller 1991:170), and those who are lost are often not

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EXHIBIT 5.6   Causality in Panel Studies
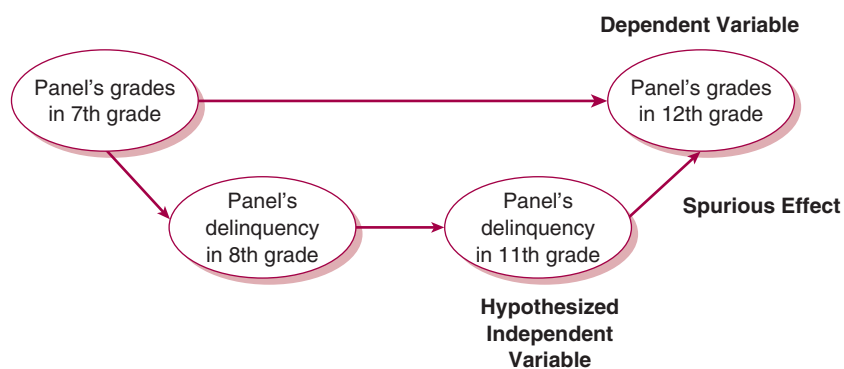
Although delinquency in the 11th grade and grades in the 12th grade are clearly associated and the time order is clear, causality cannot be assumed. In reality, grades in the 7th grade also play a role.

necessarily like those who remain in the panel. As a result, a high rate of subject attrition may mean that the follow-up sample will no longer be representative of the population from which it was drawn and may no longer provide a sound basis for estimating change. Subjects who were lost to follow-up may have been those who changed the most, or the least, over time. For example, between 5% and 66% of subjects are lost in substance abuse prevention studies, and the dropouts typically had begun the study with higher rates of tobacco and marijuana use (Snow, Tebes, & Arthur 1992:804).

It does help to compare the baseline characteristics of those who are interviewed at follow-up with characteristics of those lost to follow-up. If these two groups of panel members were not very different at baseline, it is less likely that changes had anything to do with characteristics of the missing panel members. Even better, subject attrition can be reduced substantially if sufficient staff can be used to keep track of panel members. In their panel study, Sampson and Laub (1990) lost only 12% of the juveniles in the original sample (8% if you do not count those who had died).

Subject fatigue. Panel members may grow weary of repeated interviews and drop out of the study, or they may become so used to answering the standard questions in the survey that they start giving stock answers rather than actually thinking about their current feelings or actions (Campbell 1992). This is called the problem of **subject fatigue**. Fortunately, subjects do not often seem to become fatigued in this way, particularly if the research staff have maintained positive relations with the subjects. For example, at the end of an 18-month-long experimental study of housing alternatives for persons with mental illness who had been homeless, only 3 or 4 individuals (out of 93 who could still be located) refused to participate in the fourth and final round of interviews. The interviews took a total of about 5 hours to complete, and participants received about $50 for their time (Schutt, Goldfinger, & Penk 1997).

Because panel studies are so useful, social researchers have developed increasingly effective techniques for keeping track of individuals and overcoming subject fatigue. But when resources do not permit use of these techniques to maintain an adequate panel, repeated cross-sectional designs usually can be employed at a cost that is not a great deal higher than that of a one-time-only cross-sectional study. The payoff in explanatory power should be well worth the cost.

Event-Based Designs

In an **event-based design**, often called a *cohort study*, the follow-up samples (at one or more times) are selected from the same **cohort**: people who all have experienced a similar event or a common starting point. Examples include the following:

- *Birth cohorts*: those who share a common period of birth (those born in the 1940s, 1950s, 1960s, etc.)
- *Seniority cohorts*: those who have worked at the same place for about 5 years, about 10 years, and so on
- *School cohorts*: freshmen, sophomores, juniors, and seniors

> ***Event-based design (cohort study)*** A type of longitudinal study in which data are collected at two or more points in time from individuals in a cohort.
>
> ***Cohort*** Individuals or groups with a common starting point. Examples include college class of 1997, people who graduated from high school in the 1980s, Federal Bureau of Prisons' employees who started work between the years 1990 and 2000, and people who were born in the late 1940s or the 1950s (the "baby boom generation").

An event-based design can be a type of repeated cross-sectional design or a type of panel design. In an event-based repeated cross-sectional design, separate samples are drawn from the same cohort at two or more different times. In an event-based panel design, the same individuals from the same cohort are studies at two or more different times.

Causality in Nonexperimental Designs

How well do the research designs just described satisfy the criteria necessary to determine causality? Although it is relatively easy to establish that an empirical association exists between an independent and dependent variable in these designs, the other criteria are much more difficult to assess.

Let us first illustrate the importance of time-order and nonspuriousness using research that has examined the factors related to the gender and crime relationship. Based on both victimization data and official police reports, data indicate that males commit the majority of all crime. Why is this? Gottfredson and Hirschi's General Theory of Crime (GTC) contends that the reason males engage in more criminality is because they have lower levels of self-control than females. They also contend that socialization of children by parents is the primary factor in the development of self-control. However, based on a critique of the GTC by Miller and Burack (1993) and the power-control theory (Hagan, Gillis, and Simpson 1985), Blackwell and Piquero (2005) hypothesized that the power relationships that exist between parents in a household (e.g., patriarchy) would also affect the socialization experiences of boys and girls, and ultimately their levels of self-control. To summarize briefly, Blackwell and Piquero examined the factors related to self-control acquisition in childhood using a sample of adults. Using this same sample of adults, they then examined the extent to which low-self control predicted the propensity of for criminal offending. In a nutshell, they sought to explain the origins of self-control as well as the effects of self-control on criminal offending, and how all this may be different for males and females from patriarchal families and for males and females from more egalitarian families. Using a random sample of 350 adults from Oklahoma City in 1994, they found that there were indeed differences in the way power relationships between parents affected the acquisition of self-control for males and females. They also found, however, that there were essentially no differences in the ability of self-control to predict criminal aspirations; males and females with low self-control were more likely to self-report that they would engage in criminal behavior than their higher self-control counterparts.

Do these findings establish that low self-control leads to crime through poor social-ization of children by parents? Well, there are many assumptions being made here that we hope you can see right away. First, this study relied on the recollections of adults about their childhood socialization. It also assumed that levels of low self-control were subse-quent to parental socialization and preceded individuals' aspirations to offend (time order). This may very well be the case. It may be that those adults who were more likely to offend had inadequate socialization, which created low self-control. However, it may be that offending behavior during their adolescence led to weak attachments to family and high attachments to other delinquent peers like themselves, which also decreased levels of self-control. In this case, the delinquent offending and peer associations would be a third variable responsible for both the low self-control and the criminal aspirations in adulthood (e.g., spurious relationship). The problem, of course, is that with cross-sec-tional data like this, the correct time-order cannot be established and it is difficult to control for the effects of all important factors. Blackwell and Piquero (2005) stated this limitation well when they concluded, "Future research should attempt to examine the changing nature of parental socialization and self-control across gender in longitudinal studies" (p. 15).

To reduce the risk of spuriousness, Blackwell and Piquero (2005) used the technique of **statistical control**. Exhibit 5.7 represents the important concept of statistical control with a hypothetical study of the relationship between attending a boot camp in prison (a highly regimented, discipline-focused rehabilitation program) and the likelihood of committing crimes after prison (the recidivism rate). In Exhibit 5.7, the data for all prisoners show that prisoners who attended boot camp were less likely to return to committing crimes after they left prison. However, as the more detailed data show, more female prisoners attended boot camp than male prisoners, so gender may have played a significant role in recidivism. The researchers, however, reduced the risk of spuriousness by using two statistical control meth-ods: They examined the association between attending boot camp and post-prison crimi-nality for men and for women. After doing this, researchers determine that boot camp did not reduce recidivism. It just appeared to do so, because women were more likely to attend boot camp and less likely to commit crimes after prison, regardless of whether they attended boot camp.

Similarly, Sampson and Raudenbush (1999) designed their study, in part, to determine whether the apparent effect of visible disorder on crime—the "broken windows" thesis—was spurious due to the effect of informal social control (see Exhibit 5.3). Exhibit 5.8 shows how statistical control was used to test this possibility. The data for all neighborhoods show that neighborhoods with much visible disorder had higher crime rates than those with less visible disorder. However, when we examine the relationship between visible disorder and neighborhood crime rate separately for neighborhoods with high and low levels of infor-mal social control, that is, when we statistically control for social control level, we see that the crime rate no longer varies with visible disorder. Therefore, we must conclude that the apparent effect of "broken windows" was spurious due to level of informal social control. Neighborhoods with low levels of social control were more likely to have high levels of vis-ible social and physical disorder, and they were also more likely to have a high crime rate, but the visible disorder itself did not alter the crime rate.

EXHIBIT 5.7   The Use of Statistical Control to Reduce Spuriousness
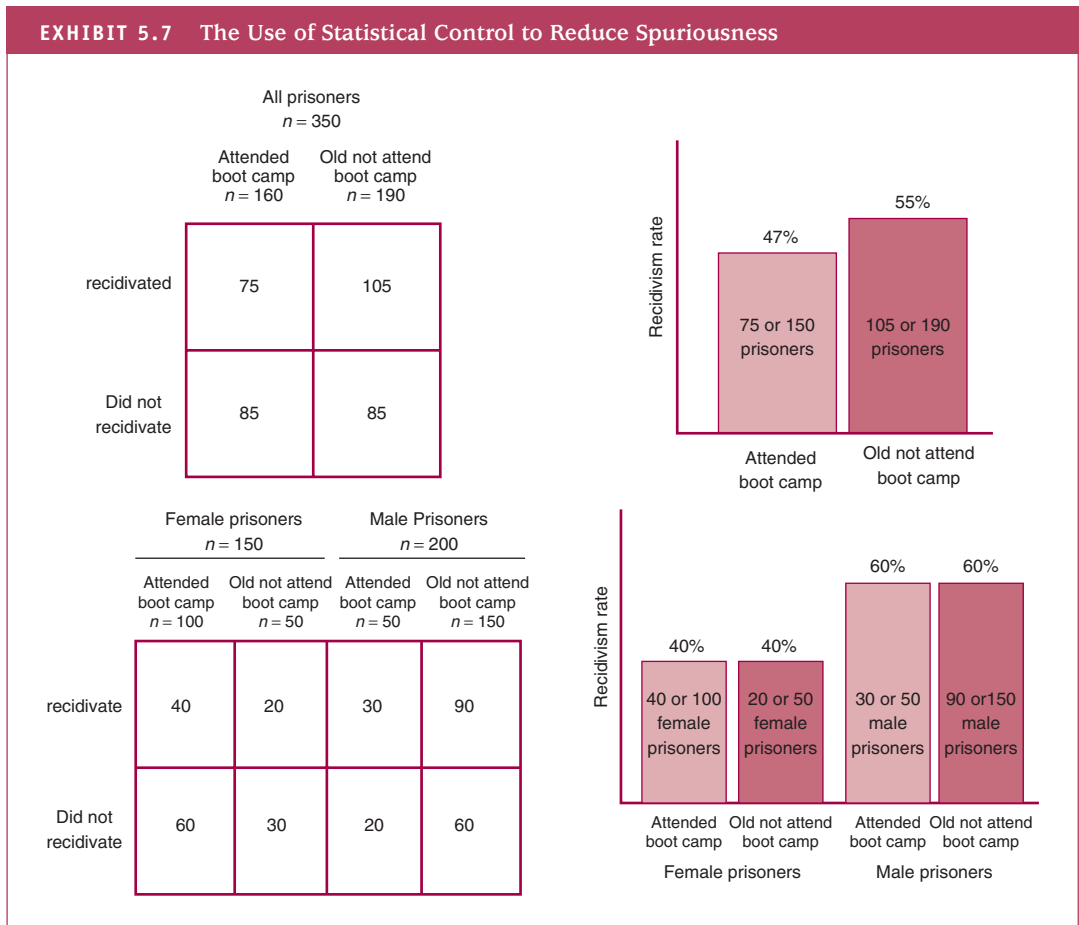
Statistical control A technique used in nonexperimental research to reduce the risk of spuriousness. One variable is held constant so the relationship between two or more other variables can be assessed without the influence of variation in the control variable.

Example of statistical control: Sampson (1987) found that the relationship between rates of family disruption and violent crimes held true for cities with similar levels of joblessness (the control variable). So the rate of joblessness could not have caused the association between family disruption and violent crime.

Our confidence in causal conclusions based on nonexperimental research also increases with identification of a causal mechanism. These mechanisms are called **intervening variables** in nonexperimental research, and help us to understand how variation in the independent variable results in variation in the dependent variable. For example, in a study that reanalyzed data from Glueck and Glueck's (1950) pathbreaking study of juvenile delinquency, Sampson and

EXHIBIT 5.8   The Use of Statistical Control to Reduce Spuriousness
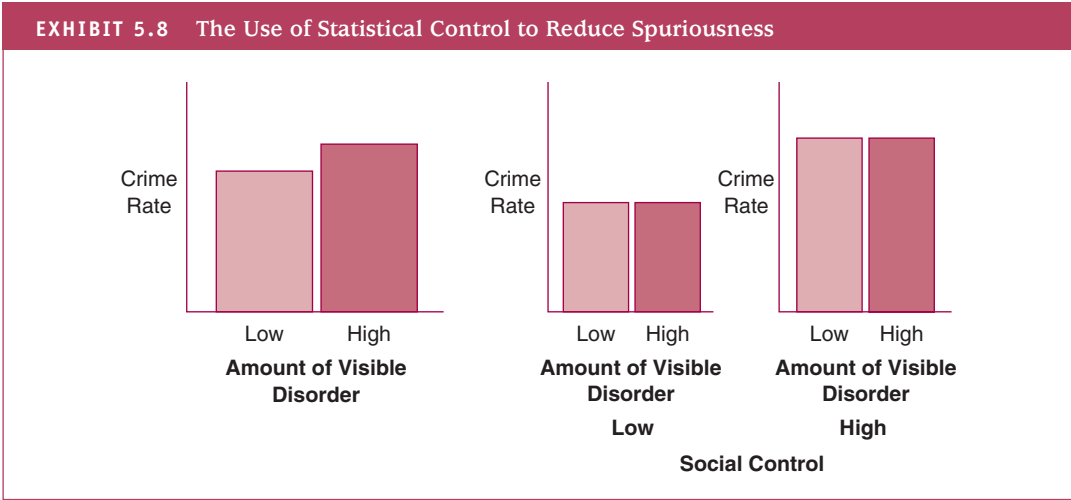
Source: Based on Sampson & Raudenbush 1999.

Laub (1994) found that children who grew up with such structural disadvantages as family poverty and geographic mobility were more likely to become juvenile delinquents. Why did this occur? Their analysis indicated that these structural disadvantages led to lower levels of informal social control in the family (less parent-child attachment, less maternal supervision, and more erratic or harsh discipline). Lower levels of informal social control resulted in a higher probability of delinquency (see Exhibit 5.9). Informal social control intervened in the relationship between structural context and delinquency.

Of course, identification of one (or two or three) intervening variables does not end the possibilities for clarifying the causal mechanisms. You might ask why structural

EXHIBIT 5.9   Intervening Variables in Nonexperimental Research: Structural Disadvantage and Juvenile Delinquency
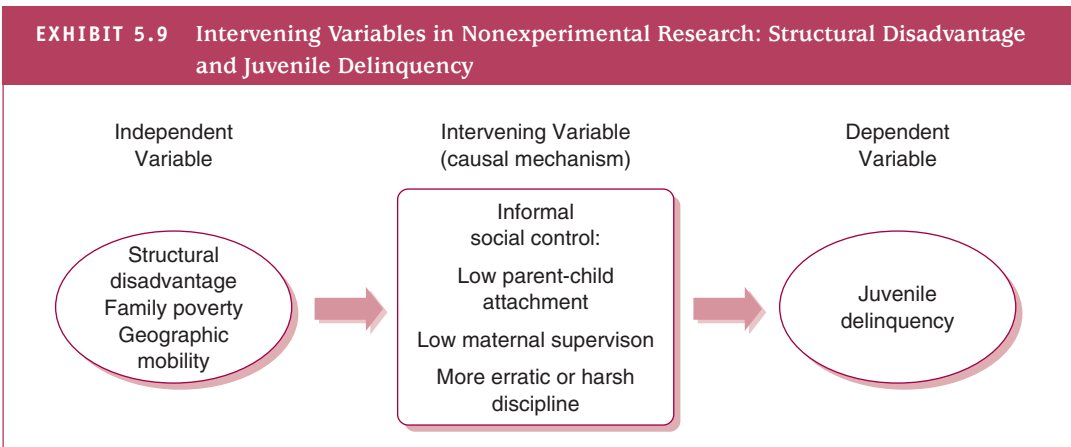
Source: Based on Sampson & Laub 1994.

disadvantage tends to result in lower levels of family social control or how family social control influences delinquency. You could then conduct research to identify the mechanisms that link, for example, family social control and juvenile delinquency. (Perhaps the children feel they are not cared for, so they become less concerned with conforming to social expectations.) This process could go on and on. The point is that identification of a mechanism through which the independent variable influences the dependent variable increases our confidence in the conclusion that a causal connection does indeed exist.

When you think about the role of variables in causal relationships, do not confuse variables that cause spurious relationships with variables that intervene in causal relationships, even though both are third variables that do not appear in the initial hypothesis. Intervening variables help explain the relationship between the independent variable (juvenile delinquency) and the dependent variable (adult criminality).

Nonexperimental research can be a very effective tool for exploring the context in which causal effects occur. Administering surveys in many different settings and to different types of individuals is usually much easier than administering various experiments. The difficulty of establishing nonspuriousness does not rule out using nonexperimental data to evaluate causal hypotheses. In fact, when enough nonexperimental data are collected to allow tests of multiple implications of the same causal hypothesis, the results can be very convincing (Freedman 1991).

In any case, nonexperimental tests of causal hypotheses will continue to be popular because the practical and ethical problems in randomly assigning people to different conditions preclude the test of many important hypotheses with an experimental design. Just remember to carefully consider possible sources of spuriousness and other problems when evaluating causal claims based on individual nonexperimental studies. In general, conclusions about causal effects based on nonexperimental studies are more likely to be valid if the comparison group is very similar to the group receiving the treatment of interest or if many potentially important variables are statistically controlled.

CONCLUSION

In this chapter, you have learned about two alternative meanings of causation (nomothetic and idiographic). You have studied the five criteria used to evaluate the extent to which particular research designs may achieve causally valid findings. You have learned how our ability to meet these criteria is shaped by research design features including the use of true experimental designs, the use of a cross-sectional or longitudinal designs, and use statistical control to deal with the problem of spuriousness. You have also seen why the distinction between experimental and nonexperimental designs has so many consequences for how, and how well, we are able to meet nomothetic criteria for causation.

It is important to remember that the results of any particular study are part of an always-changing body of empirical knowledge about social reality. Thus, our understandings of causal relationships are always partial. Researchers always wonder whether they have omitted some relevant variables from their controls, whether their experimental results would differ if the experiment were conducted in another setting, or whether they have overlooked a critical historical event. But by using consistent definitions of terms and maintaining clear

standards for establishing the validity of research results, and by expecting the same of others who do research, social researchers can contribute to a growing body of knowledge that can reliably guide social policy and social understanding.

When you read the results of a social scientific study, you should now be able to evaluate critically the validity of the study's findings. If you plan to engage in social research, you should now be able to plan an approach that will lead to valid findings. And with a good understanding of three dimensions of validity (measurement validity, generalizability, and causal validity) under your belt, and with sensitivity also to the goal of "authenticity," you are ready to focus on the major methods of data collection used by social scientists.

KEY TERMS

Association	Idiographic causal explanation
Causal effect (idiographic perspective)	Intervening variable
Causal effect (nomothetic perspective)	Longitudinal research design
Ceteris paribus	Mechanism
Cohort	Nomothetic causal explanation
Cohort study	Nonspuriousness
Context	Random assignment
Contextual effect	Repeated cross-sectional design (trend study)
Counterfactual	Spurious relationship
Cross-sectional research design	Statistical control
Event-based design (cohort study)	Subject fatigue
Extraneous variable	Time order
Fixed-sample panel design (panel study)	Trend study

HIGHLIGHTS

- Causation can be defined in either nomothetic or idiographic terms. Nomothetic causal explanations deal with effects on average. Idiographic causal explanations deal with the sequence of events that led to a particular outcome.

- The concept of nomothetic causal explanation relies on a comparison. The value of cases on the dependent variable is measured after they have been exposed to variation in an independent variable. This measurement is compared to what the value of cases on the dependent variable would have been if they had not been exposed to the variation in the independent variable (the counterfactual). The validity of nomothetic causal conclusions rests on how closely the comparison group comes to the ideal counterfactual.

- From a nomothetic perspective, three criteria are generally viewed as necessary for identifying a causal relationship: association between the variables, proper time order, and nonspuriousness of the association. In addition, the basis for concluding that a causal relationship exists is strengthened by identification of a causal mechanism and the context for the relationship.

- Association between two variables is in itself insufficient evidence of a causal relationship. This point is commonly made with the expression "Correlation does not prove causation."

- Experiments use random assignment to make comparison groups as similar as possible at the outset of an experiment in order to reduce the risk of spurious effects due to extraneous variables.

- Nonexperimental designs use statistical controls to reduce the risk of spuriousness. A variable is controlled when it is held constant so that the association between the independent and dependent variables can be assessed without being influenced by the control variable.

- Ethical and practical constraints often preclude the use of experimental designs.

- Idiographic causal explanations can be difficult to identify, because the starting and ending points of particular events and the determination of which events act as causes in particular sequences may be ambiguous.

- Longitudinal designs are usually preferable to cross-sectional designs for establishing the time order of effects. Longitudinal designs vary in terms of whether the same people are measured at different times, how the population of interests is defined, and how frequently follow-up measurements are taken. Fixed-sample panel designs provide the strongest test for the time order of effects, but they can be difficult to carry out successfully because of their expense as well as subject attrition and fatigue.

EXERCISES

1. Review articles in several newspapers, copying down all causal assertions. These might range from assertions that community policing was related to decreasing rates of violence, that the stock market declined because of uncertainty in the Middle East, or to explanations about why a murder was committed. Inspect the articles carefully, noting all evidence used to support the causal assertions. Are the explanations nomothetic, idiographic, or a combination of both? Which criteria for establishing casualty in a nomothetic framework are met? How satisfactory are the idiographic explanations? What other potentially important influences on the reported outcome have been overlooked?

2. Select several research articles in professional journals that assert, or imply, that they have identified a causal relationship between two or more variables. Are all the criteria for establishing the existence of a causal relationship met? Find a study in which subjects were assigned randomly to experimental and comparison groups to reduce the risk of spurious influences on the supposedly causal relationship. How convinced are you by the study? Find a survey study that makes causal assertions based on the relationships, or correlations, among variables. What variables have been statistically controlled? List other variables that might be influencing the relationship but that have not been controlled. How convinced are you by the study?

3. Search *Sociological Abstracts* or another index to the social literature for several articles on studies using any type of longitudinal design. You will be searching for article titles that use words like "longitudinal," "panel," "trend," or "over time." How successful were the researchers in carrying out the design? What steps did the researchers who used a panel design take to minimize panel attrition? How convinced are you by those using repeated cross-sectional designs that they have identified a process of change in individuals? Did any researchers use retrospective questions? How did they defend the validity of these measures?

5. Go to the book's Study Site, http://www.pineforge.com/isw5/learning.htm, and choose two research articles that include some attention to causality (as indicated by a check in that

column of the article matrix). Describe the approach taken in each article to establishing causality. How do the approaches differ from each other? Which approach seems stronger to you?

6. To assist you in completing the Web Exercises, please access the Study Site at http://www.pineforge.com/isw5 where you will find the Web Exercises with accompanying links. You will find other useful study materials like self-quizzes and e-flashcards for each chapter, along with a group of carefully selected articles from research journals that illustrate the major concepts and techniques presented in the book.

DEVELOPING A RESEARCH PROPOSAL

How will you try to establish the causal effects you hypothesize?

1. Identify at least one hypothesis involving what you expect is a causal relationship.

2. Identify key variables that should be controlled in your survey design in order to increase your ability to avoid arriving at a spurious conclusion about the hypothesized causal effect. Draw on relevant research literature and social theory to identify these variables.

3. Add a longitudinal component to your research design. Explain why you decided to use this particular longitudinal design.

4. Review the criteria for establishing a nomothetic causal effect and discuss your ability to satisfy each one. Include in your discussion some consideration of how well your design will avoid each of the threats to experimental validity.

Student Study Site

The Companion Website for *The Practice of Research in Criminology and Criminal Justice*, Third Edition
    http://www.sagepub.com/prccj3
    Visit the Web-based student study site to enhance your understanding of the chapter content and to discover additional resources that will take your learning one step further. You can enhance your understanding of the chapters by using the comprehensive study material, which includes e-flashcards, web exercises, practice self-tests, and more. You will also find special features, such as Learning from Journal Articles, which incorporates SAGE's online journal collection, and Russ Schutt's popular Interactive Exercises.

WEB EXERCISES

1. Go to the Disaster Center website, http://www.disastercenter.com/crime/. Review the crime rate nationally, and, by picking out links to state reports, compare the recent crime rates in two states. Report on the prevalence of the crimes you have examined. Propose a causal explanation for variation in crime between states, over time, or both. What research design would you propose to test this explanation? Explain.

2. Go to Crime Stoppers USA's Web site at http:/www.crimestopusa.com/. Check out "About Us" and "Crime Stoppers." How is CSUSA "fighting crime?" What does CSUSA's approach

assume about the cause of crime? Do you think CSUSA's approach to fighting crime is based on valid conclusions about causality? Explain.

3. What are the latest trends in crime? Write a short statement after inspecting the FBI's Uniform Crime Reports at www.fbi.gov (go to the "statistics" section under "reports and publications"). You will need to use Adobe Acrobat Reader to access some of these reports (those in PDF format). Follow the instructions on the site if you are not familiar with this program.

ETHICS EXERCISES

1. Bushman (1995) tested the impact of watching a violent video on students' level of aggressiveness. He found that watching the violent video increased aggressiveness. Do you consider it ethical to expose subjects to an intervention that might increase their aggressiveness? Are there any situations in which you would not approve of such research? Any types of subjects you would exclude from such research? Any limits you would draw on the type of intervention that could be tested? Would you impose any requirements for debriefing?

2. Horney, Osgood, and Marshall (1995) surveyed inmates in a state prison. Federal regulations require special safeguards for research on prisoners. Do you think special safeguards are necessary? Why or why not? What type of research would you allow with prisoners: Experiments, surveys, observational studies? Do you think it is possible for prisoners to give "voluntary consent" to research participation? What procedures might help to make prisoners' consent to research truly voluntary?

SPSS EXERCISES

We can use the GSS2004mini data to learn how causal hypotheses can be evaluated with nonexperimental data.

1. Specify four hypotheses in which CAPPUN is the dependent variable and the independent variable is also measured with a question in the GSS2004. The independent variables should have no more than 10 valid values (check the variable list).

   a. Inspect the frequency distributions of each independent variable in your hypotheses. If it appears at any have little valid data or were coded with more than 10 categories, substitute another independent variable.

   b. Generate crosstabulations that show the association between CAPPUN and each of the independent variables. Make sure that CAPPUN is the row variable and that you select "Column Percents."

   c. Does support for capital punishment vary across the categories of any of the independent variables. By how much? Would you conclude that there is an association, as hypothesized, for any pairs of variables?

   d. Might one of the associations you have just identified be spurious due to the effect of a third variable? What might such an extraneous variable be? Look through the variable list and find a variable that might play this role. If you cannot think of any possible extraneous variables, or if you did not find an association in support of any of your hypotheses, try this: Examine the association between CAPPUN and WRKSTAT2. In the next step, control for sex (gender).

The idea is that there is an association between work status and support for capital punishment that might be spurious due to the effect of sex (gender). Proceed with the following steps:

(1) Select Analyze/Descriptive statistical/Crosstabs.
(2) In the Crosstabs window, highlight CAPPUN and then click the right arrow to move it into Rows, Move WRKSTAT2 into Columns and SEX into Layer 1 of 1.
(3) Select Cells/Percentages Column/Continue/OK.

Is the association between employment status and support for capital punishment affected by gender? Do you conclude that the association between CAPPUN and WRKSTSAT2 seems to be spurious due to the effect of SEX?

2. Does the association between support for capital punishment and any of your independent variables vary with social context? Marian Borg (1997) concluded that it did. Test this by reviewing the association between attitude toward African Americans (RACPUSH2) and CAPPUN. Follow the procedures in SPSS Exercise 1d, but click RACPUSH2 into columns and REGION4 into Layer 1 of 1. (You must first return the variables used previously to the variables list.) Take a while to study this complex three-variable table. Does the association between CAPPUN and RACPUSH2 vary with region? How would you interpret this finding?

3. Now, how about the influence of an astrological sign on support for capital punishment? Create a crosstabulation in which ZODIAC is the independent (column) variable and CAPPUN is the dependent (row) variable (with column percents). What do you make of the results?