Chapter 19

RESEARCH METHODS IN ADULT PSYCHOPATHOLOGY

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A VULNERABILITY-STRESS FRAMEWORK FOR PSYCHOPATHOLOGY RESEARCH

Why does an individual develop a particular psychological disorder? Historically, researchers and theorists sought the causes of psychopathology either in factors internal to the person or in factors found in the external environment. However, the majority of current psychopathology researchers have adopted a vulnerability-stress model in which it is acknowledged that most psychological disorders are caused by a combination of constitutional and environmental factors. It is recognized that various environmental insults, such as stressful life events, early childhood traumas, brain injuries, viruses, and poor parenting can precipitate the development of behavioral disorders in some individuals. But not everyone succumbs to such stress in the same way; because of genetic, personality, cognitive, biological, or behavioral predispositions, "risk factors," or "diatheses," some people are more vulnerable to developing a psychological disorder when confronted with the same environmental stress than others (see Kraemer et al., 1997, for a discussion of the definitions and types of risk factors). Thus, the guiding principle of the vulnerability-stress model of psychopathology is that environmental stress triggers a predisposed person's vulnerability, such that the vulnerability is converted into psychopathology (e.g., Abramson, Metalsky, & Alloy, 1989; Meehl, 1962; Monroe & Simons, 1991; Zubin & Spring, 1977).

Most current theories of psychopathology adopt a vulnerability-stress framework either explicitly or implicitly. For example, since the classic publications of Meehl (1962) and Rosenthal (1970), vulnerability-stress theories in which a genetic predisposition to schizophrenia is transformed via environmental stress into schizophrenia itself have dominated the field for the past 30 years. More recent vulnerability-stress models of schizophrenia (e.g., Walker & Diforio, 1997; Weinberger, 1987) have focused on the neural mechanisms by which stressful life events, prenatal injuries, and birth complications

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convert the genetic diathesis for schizophrenia into the full-blown disorder. In depression research, aside from genetic diathesis-stress models (e.g., Kendler et al., 1995), recent psychological theories have emphasized the role of negative cognitive styles, personality characteristics, or interpersonal strategies as vulnerability factors that increase individuals' risk for developing depression when they encounter stressful life events (e.g., Abramson et al., 1989; Beck, 1987; Blatt & Zuroff, 1992; Joiner, 1995). Theories of panic attacks and panic disorder also have included diathesis-stress models in which anxiety sensitivity, the disposition to believe that autonomic arousal has harmful consequences, is hypothesized to make people vulnerable to developing panic attacks when confronted with stressful life events (e.g., Schmidt, Lerew, & Jackson, 1997). Even posttraumatic stress disorder (PTSD), originally conceptualized in the Diagnostic and Statistical Manual of Mental Disorders, 3rd edition (DSM-III; American Psychiatric Association, 1980) as a normative response to extreme environmental stress, is now recognized to require personal vulnerability (e.g., genetic risk, particular personality traits) for its occurrence as well (Yehuda & McFarlane, 1995). Similar vulnerability-stress approaches abound in research on alcoholism and drug use, dissociative, somatoform, eating, and other anxiety disorders (Alloy, Jacobson, & Acocella, 1998).

Consequently, in this chapter, we use a vulnerability-stress framework to guide our discussion of methods in psychopathology research. In the sections that follow, we first discuss the crucial role of theory in determining the choice of appropriate designs for research studies, including the need to be cognizant of the hypothesized causal relations among vulnerabilities, stressors, mediators, moderators, and disorder outcomes in the psychopathology models to be tested. Next, we describe an "ideal" design for testing vulnerability-stress models, as well as the practical and ethical problems associated with such an ideal design. We then review actual research designs used in psychopathology research, including experimental and quasi-experimental designs and correlational designs that are either cross-sectional, retrospective, longitudinal, prospective, or involve high-risk strategies. We evaluate the adequacy of these various designs for testing vulnerability-stress models and the kinds of inferences that may legitimately be drawn from them. We end with a discussion of difficult conceptual and methodological issues (e.g., the interdependence of the vulnerability, stress, and disorder, the stability of the vulnerability, disorder subtypes) that must be addressed in the design of studies to evaluate vulnerability-stress models of psychopathology.

THE ROLE OF THEORY IN THE CHOICE OF RESEARCH DESIGN

Although some psychopathology research is purely exploratory, in general, most research efforts are guided by theory or, at least, by some empirically testable hypothesis. Indeed, in the absence of theory, investigators are left to conduct "fishing expeditions" in which even a strong observed association between some variable and an abnormal behavior of interest is potentially uninterpretable. For example, the recent finding that the course of schizophrenia is more benign in developing countries than in westernized countries (Jablensky et al., 1992) is difficult to understand without some hypotheses about the factors (e.g., family structure, culture, treatment response) that influence the
maintenance versus remission of schizophrenic symptoms. Popper (1963) described the necessity of theory well:

The belief that we can start with pure observation alone, without anything in the nature of a theory is absurd. . . . Twenty-five years ago I tried to bring home the same point to a group of physics students in Vienna by beginning a lecture with the following instructions: Take a pencil and paper; carefully observe, and write down what you have observed. They asked of course, what [italics in original] I wanted them to observe. . . . Observation is always selection. It needs a chosen object, a definite task, an interest, a point of view, a problem. (p. 46; quoted in Follette & Houts, 1996, p. 1122)

Theory and research influence one another reciprocally (Follette & Houts, 1996; Skinner, 1981). Theory guides the selection of research design and the interpretation of any obtained empirical findings. In turn, research findings may lead to the further elaboration, modification, or abandonment of particular theories. For our present purposes, it is important to emphasize that the theory specifies the hypotheses to be tested and determines the optimal research designs so that the research study affords a fair opportunity to subject the theory to “grave danger of refutation” (Meehl, 1978; Popper, 1963, 1972). In particular, with respect to vulnerability-stress models of psychopathology, investigators must appreciate the kinds of causal relations specified in particular theories and use these hypothesized causal relations to guide their choice of optimal research strategies (Abramson, Metalsky, & Alloy, 1988; Alloy, Hartlage, & Abramson, 1988).

Hypothesized Causal Relations in Vulnerability-Stress Models of Psychopathology

In vulnerability-stress models, the hypothesized logical and temporal relations between proposed causes and the disorder of interest influence the choice of research design to test the model. Are the hypothesized vulnerability and stress necessary, sufficient, or contributory causes of the disorder? Do they exert their causal effects close in time to the onset of disorder or in the more distant past? Do they combine additively or do they interact synergistically to increase the likelihood of disorder?

A necessary cause of a disorder is an etiological factor (E) that must be present or have occurred for the disorder (D) to occur; or, mathematically speaking, Probability \( (E/D) = 1.00 \). The disorder cannot occur if the etiological factor did not occur. However, the disorder is not required to occur when the necessary cause has occurred (i.e., the cause is necessary but not sufficient). A sufficient cause of a disorder is an etiological factor whose occurrence guarantees the occurrence of the disorder; or, mathematically speaking, Probability \( (D/E) = 1.00 \). If the disorder does not occur, then the etiological factor must not have occurred. However, the disorder may occur in the absence of a sufficient cause (i.e., the cause is sufficient but not necessary). A contributory cause of a disorder is an etiological factor that increases the likelihood that the disorder will occur,

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1This conditional probability should read: “The probability of the presence (or occurrence) of the etiological factor given the presence (or occurrence) of the disorder is equal to 1.00.”
but is neither necessary nor sufficient for its occurrence (Abramson et al., 1988, 1989). Mathematically speaking, Probability (D/E) > Probability (D/not E), where Probability (E/D) < 1.00 (i.e., not necessary) and Probability (D/E) < 1.00 (i.e., not sufficient).

Consider the effect of the hypothesized logical relations between causes and disorder on the choice of research strategy. As an example, in genetic vulnerability-stress theories of alcoholism (e.g., Finn & Pihl, 1987; Levenson, Oyama, & Meek, 1987), ingestion of alcohol (the stress) is considered to be a necessary, but not a sufficient, cause of the development of alcoholism among genetically predisposed persons. Consequently, a study that compared alcoholics and nonalcoholics on their likelihood of exposure to alcohol prior to the onset of their disorder would be an appropriate test of these theories. In contrast, a study that compared the likelihood of alcoholism in people exposed versus nonexposed to alcohol would not provide an adequate test of these theories because alcohol exposure is not a sufficient cause of alcoholism in these theories and, therefore, some people exposed to alcohol are not expected to become alcoholic. Alternatively, in the hopelessness theory of depression (Abramson et al., 1989), hopelessness is viewed as a sufficient, but not a necessary, cause of depression. Thus, a prospective study that compared people who were hopeless versus hopeful on their likelihood of becoming depressed would be an appropriate research design to test this aspect of the theory, whereas a comparison of depressed versus nondepressed individuals on their likelihood of exhibiting hopelessness would not be appropriate because hopelessness is not a necessary cause of depression, and, therefore, some depressed persons may not be hopeless.

In addition to varying in their logical relation to the occurrence of a disorder, causes also may vary in their temporal relation to the occurrence of the disorder. In a sequence of events leading to the occurrence of a disorder, some causes (distal causes) operate toward the beginning of the sequence, distant from the occurrence of the disorder, whereas others operate toward the end of the sequence, proximate to the occurrence of the disorder (proximal causes; Abramson et al., 1988, 1989). For example, in some recent vulnerability-stress models of schizophrenia (e.g., Bracha, Torrey, Gottesman, Bigelow, & Cunniff, 1992; Mednick, Machon, Huttunen, & Bonett, 1988), prenatal exposure to viral infection or other brain-injuring trauma is conceptualized to be a stressor that leads to the development of schizophrenia in young adulthood in genetically predisposed individuals: Thus, prenatal viral exposure is a very distal hypothesized cause of schizophrenia. An appropriate research design to test this hypothesis might include a retrospective, follow-back study of adult schizophrenics and normal comparison participants to examine their mothers' exposure to viral infection while the participants were in utero, or better yet, a prospective study of infants exposed and not exposed to viral infection while in utero followed longitudinally through the age of risk for the onset of schizophrenia. However, given the distal nature of this hypothesized cause, a cross-sectional, retrospective, or prospective research design that assessed viral exposure in schizophrenics versus normal controls in childhood or adulthood would not be an adequate test of this model. In contrast, most vulnerability-stress theories of depression hypothesize that negative life events that occur proximal to the onset of depression serve as the stress that triggers the onset of the depressive episode. Consequently, studies that examine the association between recent, but not distant, negative events and depression onset would be required to test these theories.
Given that vulnerability-stress models hypothesize that a combination of personal predispositions and environmental inputs causes disorder, an adequate research design would involve the manipulation or assessment of both vulnerability and stress in the same study. However, the exact nature of the vulnerability-stress combination featured in particular models is an important consideration as well (Abramson, Alloy, & Hogan, 1997; Monroe & Simons, 1991). In some models, the vulnerability and stress are conceptualized as combining in an additive fashion according to a “titration” model, such that the degree of vulnerability and the degree of stress summate, with either combinations of low vulnerability compensated for by high stress or low stress compensated for by high vulnerability leading to disorder, as seen in Figure 19.1. Alternatively, vulnerability and stress may be postulated as truly interacting, so that the two have a synergism beyond their separate effects, with only combinations of high vulnerability and high stress leading to disorder, as shown in Figure 19.2. Whether the vulnerability-stress model is additive or interactive has implications for the levels (high, low, or both) of predisposition and stress that must be sampled or manipulated in research to test the model, as well as for the statistical procedures used to analyze the obtained data.

Figure 19.1. Additive model of vulnerability-stress interaction. In an additive model, levels of vulnerability and stress summate to increase the likelihood of disorder, such that either a combination of low vulnerability compensated for by high stress or low stress compensated for by high vulnerability leads to disorder.

Figure 19.2. Interactive model of vulnerability-stress interaction. In an interactive model, levels of vulnerability and stress have a synergistic effect, such that only a combination of high vulnerability and high stress leads to disorder.


Mediators versus Moderators in Vulnerability-Stress Models of Psychopathology

The presence of third variables that act as either moderators or mediators in vulnerability-stress models is also an important consideration in research strategy and the choice of statistical analysis (Baron & Kenny, 1986; Holmbeck, 1997). A moderator is a third variable that affects the relationship between the independent variable (i.e., the vulnerability or stress or both) and the dependent variable (disorder). In essence, a moderator interacts with the vulnerability or stress (or both) and affects the direction or strength of the relationship between the vulnerability-stress combination and disorder (see Figure 19.3, bottom). For example, some recent schizophrenia research has shown that men are one and a half times more likely than women to develop schizophrenia (Iacono & Beiser, 1992a, 1992b). Thus, a person's sex may be a moderator of the vulnerability and stress effects on schizophrenia. To the extent that a psychopathology theory proposes a moderator of the vulnerability/stress – disorder relation, it is crucial to include either a manipulation of the hypothesized moderator or an assessment of the vulnerability/stress – disorder association across different sampled levels of the moderator in one's research
design. In our current example, one would need to examine the genetic risk/birth complication prediction of schizophrenia in men and women separately. (The reader is referred to Farrell, and Holmbeck & Shapera, both in this volume, for the appropriate statistical procedures for testing moderators and mediators.)

Alternatively, a mediator is a third variable that accounts for the relation between an independent variable (vulnerability, stress, or their combination) and the dependent variable (disorder). The mediator is the mechanism or psychological process by which the vulnerability-stress combination causes the disorder (see Figure 19.3, top). Whereas moderators specify the conditions under which a vulnerability-stress combination will lead to disorder, mediators specify how or why the vulnerability-stress combination leads to disorder (Baron & Kenny, 1986; Holmbeck, 1997). To continue with the schizophrenia example, Walker and Diforio (1997) propose that damage to the hippocampus brought about by prenatal or birth injuries (the stress) in individuals with an inherited abnormality in dopamine receptors (the genetic vulnerability) leads to a dopaminergic hypersensitivity to the cortisol release that occurs in response to stressful life events, which, in turn, leads to the symptoms of schizophrenia. Here, the dopaminergic hypersensitivity to stress serves as the mediator or mechanism by which the combination of genetic predisposition and early brain injury causes schizophrenia. Ideally, research studies designed to test vulnerability-stress models containing mediators require multiple independent and converging measures of the proposed mediator (Baron & Kenny, 1986; Campbell & Fiske, 1959) as well as appropriate statistical procedures for testing mediation (see Baron & Kenny, 1986; Holmbeck, 1997, for statistical strategies).
An “Ideal” Research Design for Testing Vulnerability-Stress Models of Psychopathology

Every psychologist designing a research study hopes to find the perfect design to test his or her hypotheses. Unfortunately, due to ethical, practical, or financial constraints, the perfect design is often impossible to implement. What might an example of an “ideal” design for testing a vulnerability-stress model look like?

Imagine you are a researcher interested in testing Walker and Diforio’s (1997) model of schizophrenia. As noted above, the diathesis in this model is a genetic vulnerability that, in combination with prenatal or birth injuries, may lead to dopaminergic hypersensitivity to cortisol. This neurochemical problem, which is activated by stressful life events, brings on the symptoms of schizophrenia.

What would be the “ideal” design to test this model? To begin with, you would need two groups: a genetic high-risk (one or both parents with schizophrenia) and a genetic low-risk (no family history of schizophrenia) group. In the not too distant future, with the completion of the human genome mapping project and the advent of genetic engineering, it might be possible to actually manipulate participants’ genetic vulnerability by giving one group “schizophrenia genes” and the other group “normal genes.” The hypothesized stress in the model occurs before or during the birth process; therefore, you would need to identify your participants before their birth. You would then need to assign participants from both groups randomly to either a prenatal or birth trauma experimental condition or a nontrauma control condition. Participants in the experimental condition would be exposed to traumatic prenatal experiences (such as the influenza virus) or to injurious birth conditions, and participants in the control condition would be shielded from these experiences. To control for the possible effects of nongenetic family factors in schizophrenia, it would be helpful to pair each high-risk participant with a low-risk participant who experienced similar prenatal and birth conditions aside from the manipulated trauma, and to raise pairs together in the same family. Finally, as they approached the typical age-at-onset of schizophrenia, some of the pairs of participants (chosen at random, of course) would be exposed to stressful life events, and other pairs would be protected from all life stress.

Your statistical analyses would be designed to determine whether high-risk participants who experienced both prenatal or birth trauma and later life stressors were more likely than their low-risk counterparts to develop schizophrenia; whether high-risk participants who experienced early injury developed schizophrenia more often than high-risk participants who did not have this experience; and whether experiencing later life stress put high-risk, early trauma participants in greater danger of developing schizophrenia. Moreover, you would assess the integrity and sensitivity of each participant’s dopaminergic system shortly after birth and again near the age-at-risk for schizophrenia (both baseline and in response to neurochemical and stressful life event challenges) and conduct analyses to examine whether dopaminergic hypersensitivity mediated the effects of the combination of genetic risk, prenatal/birth trauma, and stressful life events on the likelihood of onset of schizophrenia.

Thus, the “ideal” design for testing the causal hypotheses of a vulnerability-stress model of a disorder is one that experimentally manipulates both the hypothesized vulnerability and the stress, such as our example above. However, it should be obvious that
such a design cannot and should not be used. Not only is it impossible to implement, but the ethical, legal, and financial considerations are staggering. Although this example may be somewhat extreme, similar issues arise in every psychopathology study. Ideal designs are unrealistic in research where real people are used as participants. All we can do is try to create a solid design that minimizes alternative interpretations and confounding factors, while staying within practical and ethical boundaries (Bersoff & Bersoff, this volume). The remainder of this chapter is devoted to that endeavor.

**EXPERIMENTAL AND QUASI-EXPERIMENTAL DESIGNS IN PSYCHOPATHOLOGY RESEARCH**

There are two basic types of research designs used in clinical psychology: correlational or passive observational designs, and experiments (Kazdin, this volume). Correlational or observational designs are those in which the researcher systematically studies relationships among variables without trying to manipulate these relationships. Correlational designs will be discussed more fully below. Experiments are those studies in which the researcher actively manipulates at least one variable and measures the effect of this manipulation on another variable. Therapy-outcome studies (see Kendall, Flannery-Schroeder, & Ford, this volume) are a good example of experimental designs in clinical psychology; the researcher actively provides treatment (the manipulation) to research participants and then measures the effects of this treatment on psychopathology (the dependent variable). In psychopathology research, experiments are typically designed either to induce psychopathological states or milder analogues of disorder or to reduce or mitigate such states via manipulation of vulnerability, stress, or both.

Of course, even if a researcher has developed a carefully designed manipulation, he or she cannot assume that the manipulation “worked” as expected. Thus, an important component of a well-designed experiment is a manipulation check or a measurement of the effectiveness of the manipulation. For example, an investigator interested in the effect of exposure to threat (e.g., a live cobra) on memory for stimuli in the environmental context might check on the effectiveness of the manipulation by assessing subjective, behavioral, and physiological indices of anxiety before and after the threat exposure. It is only by knowing the degree of effectiveness of the manipulation that the researcher can interpret any obtained difference or lack of difference in contextual memory between the experimental (exposed to threat) and control (no exposure to threat) groups.

Experimental designs come in two varieties: true experiments and quasi-experiments. In a true experiment, research participants are assigned at random to the different conditions (e.g., experimental and control groups). Each participant has an equal likelihood of being assigned to any of the groups. An essential characteristic of a true experiment is a control group that is equivalent to the treatment group on all dimensions except for the one being manipulated (see below for more discussion of control groups). True experiments provide the researcher with better control over the independent variable than any other research design, and for this reason, many researchers view the true experiment as the preferred design for making strong inferences regarding the causal role of vulnerability and stress in disorder. Our “ideal” design above is an example of a
true experiment. However, frequently, a true experiment is not possible. Often, researchers who study vulnerability-stress models of psychopathology want to compare high-risk (vulnerable) and low-risk (nonvulnerable) individuals to help determine whether and how vulnerability contributes to the development of disorder. These investigators may not be able to assign participants to high- and low-risk conditions for ethical or practical reasons; thus, random assignment is impossible. In cases such as this, researchers resort to using quasi-experimental designs. A quasi-experiment is identical to a true experiment except that assignment of participants to groups is not done randomly, but is instead conducted in a systematic way based on participants' a priori characteristics. For example, the investigator may select participants with and without panic disorder, rather than randomly assigning normal participants to a panic-induction versus a control condition. However, even when there are no obvious confounding differences between the pathological and control groups, there is no guarantee that the groups are equivalent with the exception of the presence of disorder.

**Issues of Participant Selection**

Researchers study subgroups of people. These subgroups are called samples, and the job of the researcher is to use statistical techniques to make inferences from samples to the entire population of individuals they are interested in understanding. The extent to which the investigator is safely able to make these inferences is called external validity. To maximize external validity, experimenters may use random selection of research participants from a population. Random selection is a strategy for choosing participants in which each member of the population is equally likely to be included in the sample. For example, in a study of genetic predisposition to generalized anxiety disorder, every person with the genetic vulnerability would have an equal likelihood of ending up in the study.

Where random selection is possible, it enables researchers to use small samples and still make valid inferences about the population. However, random selection is often impossible to achieve. In the above example, random selection would require that the researchers include at-risk people of all ages, nationalities, and language groups. Participants would have to come from all parts of the world, and people without telephones or mailing addresses would have to be contacted and chosen to participate. Needless to say, this would be prohibitively expensive if not completely impossible. For this reason, researchers often use convenience samples rather than random samples. A convenience sample is a group of research participants chosen, at least in part, because the researcher has access to them. Many psychopathology studies, for example, use samples of university students because many researchers work at universities.

Most psychopathology research utilizes convenience samples, and this often does not pose problems. However, some questions are worth asking before one decides to settle for an easily selected, local sample. If a college sample is being used, are college-age individuals, or individuals with the intellectual and financial resources to go to college, as likely as others to exhibit the signs of a particular disorder? If the study is being conducted in an urban setting, are the relevant stresses similar in urban and rural environments? Are people who are willing to participate in the study meaningfully different from those who are unwilling? Most generally, do the factors that make particular
individuals convenient members of the sample also affect their vulnerability, exposure to stress, or likelihood of exhibiting the disorder? If so, then findings obtained from the study may not lead to valid inferences about the population.

**Participant Matching**

One problem psychopathology researchers sometimes encounter is that the hypothesized causes of disorder (vulnerability, stress) that they are interested in studying may themselves be correlated with other variables. This situation is not problematic in all cases, but it poses serious difficulties when the correlated “third variables” actually predict the disorder of interest. For example, depression is more common among women than men (Nolen-Hoeksema, 1987). A researcher interested in testing the interpersonal theory of depression (e.g., Coyne, 1976; Joiner, 1995) may run into problems if it turns out that a reassurance-seeking or negative feedback-seeking style (the hypothesized vulnerabilities) is more common among women than men. If this were the case, the finding that people with the negative interpersonal style are more likely to become depressed than those without this style may be due to gender differences, rather than to the interpersonal strategies per se.

One way to combat this problem is to use *participant matching*. In this technique, the study groups are equated on potential “third variables.” Matching can be carried out on either a samplewide or individual basis. In samplewide matching, the investigator ensures that the study groups as a whole are equated on potentially confounding variables (after the study, groups are compared and found not to differ). For example, in a study on interpersonal aspects of depression, high and low reassurance-seeking groups can be equated on gender, ethnic group, and so on. In individual matching, each participant in one group is paired (matched) on a case-by-case basis with a participant from another group who has similar characteristics. For example, each female participant high on reassurance-seeking may be paired with a female participant low on reassurance-seeking with whom she shares similar ethnic and class backgrounds. The same may then be done with the male participants. With individual matching, specific research designs may be used, but with either type of matching, the investigator strives to ensure that the high- and low-risk groups are balanced in terms of gender, class, and ethnicity. Under these circumstances, differences in the rates of depression between the two groups are more likely to be due to differences in interpersonal style than to differences in any of these other potentially confounding factors. It is important to consider, however, that matching may create as many problems as it remedies if one or both of the groups become unrepresentative of the population from which they were drawn in order to achieve matching (Huesmann, 1982). As an illustration, to match a schizophrenic and normal control group on IQ, it may be necessary to select high-IQ schizophrenics or low-IQ normals, leaving one or the other group unrepresentative of its corresponding population.

**Internal and External Validity**

To draw valid inferences and clearly demonstrate a particular relationship among vulnerability, stress, and resultant disorder, investigators must operationalize these
constructs by defining them in specific, quantifiable terms. This simplification and isolation of variables is essential to minimizing the ambiguity of research findings; without it, confounding variables enable a variety of alternative interpretations that could explain an observed relation among vulnerability, stress, and disorder. Internal validity, a crucial facet of well-conducted research, refers to the extent to which a study rules out alternative explanations of the findings. This type of experimental validity is addressed by the question: To what extent can the specific variable of interest (e.g., vulnerability, stress, and their combination), rather than extraneous influences, be considered to account for the results (Kazdin, 1998)? One of the best ways to ensure internal validity is to include appropriate control groups in the study design. We discuss control groups in the next section.

Factors other than the independent variables (vulnerability and stress) that could explain research results are referred to as threats to internal validity. Although numerous threats to internal validity have been identified (Cook & Campbell, 1979; Kazdin, 1998), several are particularly relevant to psychopathology research. Testing refers to the effects of repeated testing or assessment in which task performance or symptom endorsement may be influenced by practice or familiarity. Instrumentation involves changes in measurement instruments or procedures over time and is especially relevant to longitudinal data. Statistical regression refers to the tendency for extreme scores to revert toward the mean of a distribution with repeated administration of a measure. The threat of attrition involves the differential loss of participants across conditions within a study that may influence group score. Finally, maturation refers to processes within the participant that change over time (e.g., remission of disorder, aging, fatigue). Note that maturation is not necessarily a threat to internal validity; in fact, it may be the focus of study, particularly in certain quasi-experimental designs in which there is no treatment group or experimental condition.

External validity refers to the extent to which research findings generalize beyond the specific conditions of a study to other populations, settings, conditions, and so on. This dimension of experimental validity is addressed by the question: To what extent can these results be extended or generalized to people, settings, measures, and/or characteristics other than those used in this particular study (Kazdin, 1998)? Threats to external validity indicate the boundaries of a research finding, the parameters that constrain a demonstrated relationship among vulnerability, stress, and disorder.

Of the various threats to external validity that have been identified, two are particularly relevant to vulnerability-stress models. Reactivity of assessment involves limitations to generalizability that occur when a participant’s awareness of being assessed influences his or her responses. Timing of measurement refers to the contingency of results on the point in time when assessment instruments were administered. For example, as described earlier in our discussion of distal versus proximal causes, assessment of life events two years before onset of disorder as the relevant stress in a study designed to test a vulnerability-stress model that features recent life events would provide misleading findings with regard to the validity of the model.

An important issue related to external validity is the discrepancy among researchers’ methods of operationalizing constructs of study. Differences in operational definitions can lead to different conclusions about vulnerability-stress disorder relations and impede comparability with other findings. For example, the study of childhood depression
has commonly involved child self-report of depressive symptoms, parents' ratings of the child's depression, and interview-based psychiatric diagnosis. Kazdin (1989) found that when all three methods are compared for the same sample, there is little overlap among the children identified as depressed.

A natural tension exists between internal and external validity, such that greater control over threats to internal validity generally result in decreased generalizability of the findings. The trade-offs between emphasizing internal versus external validity must be considered within the context of the specific hypotheses of a given vulnerability-stress model. Given that one must first obtain an unambiguous finding before one can generalize about it, internal validity often takes logical precedence over external validity. However, highly controlled experimental conditions rarely approximate the real-life environment in which disorders actually occur. Further, the failure to generalize inspires further theoretical elaboration and empirical research to clarify these emerging facets of psychological disorders.

**Control Groups**

If an investigator were interested in examining the role of perfectionism as a vulnerability for obsessive-compulsive disorder (Frost & Gross, 1993; Frost, Steketee, Cohn, & Griess, 1994), he or she might expose a group of participants to unsolvable problems in the laboratory (guaranteeing that they would make errors) and examine the effect of this treatment on their level of obsessive-compulsive symptoms. Imagine that the researcher found that the participants showed an average of three symptoms following unsolvable problems. How could he or she determine whether this is different from what would have happened if they had not made errors on unsolvable problems? To answer this question, the researcher would need to compare this group to another group of people who had not been exposed to the unsolvable problems. This second group would be the control group. In an experimental design, a control group is a group of research participants who do not receive the experimental manipulation and whose outcomes can be compared to those of the experimental group. In a correlational study, a control group is a group of participants who differ from the group of interest and whose outcomes can serve as a comparison. For example, in a correlational study of the impact of an alcoholic parent on family functioning, the control group might consist of families in which there is no alcoholic parent. Ideally, a control group should be similar to the experimental group on all dimensions except for the experimental manipulation.

There are many different kinds of control groups, and the type to use depends largely on the research questions being asked. In studies testing vulnerability-stress models, the choice of appropriate control groups can be quite complex. Two examples will illustrate why selection of control groups can be a tricky but important question for researchers.

Imagine a researcher who is interested in studying anxiety sensitivity as a vulnerability factor for the development of panic attacks (Schmidt et al., 1997). He or she conducts a prospective study (see below) to determine whether individuals who measure high on anxiety sensitivity but have not yet had a panic attack are more likely to develop panic than other individuals. What other individuals should be included in the study? One possibility is that the investigator could compare high-anxiety-sensitivity participants to
people whose anxiety sensitivity is unusually low. An advantage to this approach is that the statistical power will be good; in other words, if there are differences in development of panic between high- and low-anxiety-sensitivity individuals, the researcher will more likely be able to detect those differences than if he or she had used a randomly selected control group. However, what the researcher gains in increased power is paid for in loss of external validity. Knowing that high-anxiety-sensitivity people develop panic more often than low-anxiety-sensitivity people tells us nothing about how high-sensitivity people compare to the average individual. Perhaps high-anxiety-sensitivity people are not actually at increased risk compared to the average person, but instead, low-anxiety-sensitivity people are especially unlikely to develop panic.

One way the researcher could combat this problem is to sample individuals at random and treat anxiety sensitivity as a continuous variable. In other words, he or she might simply measure the correlation between anxiety sensitivity and panic without dividing people into groups. The advantage of this approach is that the results are generalizable to the population at large. However, the researcher runs the risk of not having many participants with high enough vulnerability to lead to development of panic, or of needing a prohibitively large sample to achieve an appropriate statistical conclusion.

Which of these approaches is better? The answer lies in the model for how anxiety sensitivity increases risk for panic. Does vulnerability increase in a continuous fashion, with most people experiencing at least a little risk but some experiencing substantially more vulnerability? If this is what the theory states, then tests should reflect this by treating risk as continuous rather than grouping people by risk status. On the other hand, if the theory holds that there is a threshold beyond which anxiety sensitivity becomes a vulnerability factor, then it is appropriate to include a high-risk group and a low-risk comparison group.

Different issues can arise in tests of vulnerability-stress models. Imagine, for example, that an investigator wants to test whether people with schizophrenia are more likely to have experienced birth complications than nonschizophrenic individuals (Cannon et al., 1993). To whom should the researcher compare the schizophrenic group? If the investigator includes a nonpathological group only, he or she can determine whether birth complications are associated with psychopathology but cannot be certain that they are associated with schizophrenia more than with another disorder. For this reason, it may be wise to include a specificity control group, a group of people with another disorder. Once again, the hypothesis to be tested should guide the design. If the theory states that birth complications contribute to schizophrenia but not other disorders, a specificity control group may be necessary. If the theory states that schizophrenia is one of various disorders caused, in part, by birth complications, then a nonpathological control group may be sufficient.

**Posttest-Only versus Pretest-Posttest Designs**

In experimental designs, another important choice the investigator must make is whether or not to include a pretest prior to the experimental manipulation. The posttest-only design, although not the most common, is the most basic experimental design. Generally, participants from a single population are assigned randomly to the various conditions of the independent variable(s) under study (i.e., procedures or interventions) and measured
after these conditions are applied. This design may be quite adequate in certain research contexts in which a pretest is not necessary (e.g., brief laboratory experiments using random assignment and a large sample size), not feasible (because of practical or ethical reasons), or undesirable (where pretest sensitization may influence the effects of the independent variable; Kazdin, 1998). A major drawback of this design is that without a pretest, potentially critical information about participants (e.g., traits, demographics, level of functioning) prior to the experiment remains unknown and cannot be used to match participants on relevant dimensions to equalize the groups and rule out alternative interpretations, to predict differential response to an intervention, or to examine differential attrition across groups. A second limitation of posttest-only designs is that one cannot assess change in behavior as a function of the intervention or manipulation.

Consequently, psychopathology researchers often utilize pretest-posttest designs to overcome these limitations. Participants who meet particular levels of disorder or vulnerability to disorder are selected prior to the delivery of the independent variable(s) and pretest measurement is used to establish group equivalence. Additionally, the use of a pretest confers statistical advantages by allowing within-subject error variance to be estimated separately from between-subject variance, thereby yielding more powerful statistical tests of the effects of the independent variable (Farrell, this volume). Further, more precise predictions can be examined because pretest data enables the investigator to move beyond simply establishing group differences in the direction of examining the unique impact of independent variables. As mentioned above, the major limitation of this design is the potential effect of the pretest in sensitizing the participant to the conditions of the independent variable.

Analogue Research Designs

Analogue research in psychopathology involves studies of situations that are analogous to real life and, thus, provide a model of how disorders may develop. Analogue designs may employ any of four methods: studies of subclinical phenomena among individuals exhibiting various degrees of psychological symptoms that do not reach diagnostic threshold; the experimental induction of pathological states in normals; animal models of psychopathology; and computer simulation techniques (for review, see Sher & Trull, 1996). An advantage of analogue studies is that the investigator can manipulate potential causal variables (e.g., vulnerability or stress) that could not be manipulated ethically with actually disordered individuals. Whereas analogue designs generally involve controlled experimental or quasi-experimental designs that maximize internal validity, the generalizability of findings from subclinical to clinical populations, from temporarily induced pathological states in normals to naturally occurring disorders, and from animals to humans (see Suomi, this volume) is a fundamental issue.

Given the virtual impossibility of completely modeling human behavior using laboratory analogues, researchers have argued that certain criteria must be established to validate analogue models. For example, Abramson and Seligman (1977) suggested four criteria essential to the validity of human or animal laboratory analogues of psychopathology: (a) a thorough description of the disorder's essential features, including the causes, symptoms, prevention, and treatment; (b) a demonstrated similarity between
the analogue model and the actual disorder; (c) an adequate demonstration of the similarity of physiology, cause, prevention, and cure; and (d) specificity of the laboratory analogue in describing a particular disorder rather than others.

CORRELATIONAL DESIGNS IN PSYCHOPATHOLOGY RESEARCH

Correlational or observational designs are those in which the researcher systematically studies relationships (correlations) among variables without manipulating the relationships. In correlational studies, the investigator examines relationships that have occurred naturally. For example, if a researcher wanted to test the hypothesis that early parental loss creates a vulnerability to depression, he or she could not ethically manipulate the death of participants' parents. Instead, the investigator could examine the association between the age of participants when one of their parents happened to die with the likelihood of developing depression by age 50 in a correlational design. Correlational studies may be conducted in either a cross-sectional or longitudinal fashion. Cross-sectional studies involve observation of the relationships among independent and dependent variables (e.g., vulnerability, stress, and disorder) measured at one point in time, whereas longitudinal studies involve observation of relationships among variables measured at multiple points over time.

Cross-Sectional Case-Control Designs

One type of correlational design used frequently in psychopathology research is the case-control design, in which people with a disorder (cases) are compared with people who do not have the disorder in question (controls) on variables of interest (e.g., vulnerability, stress). Cross-sectional correlational designs are popular in part because they are relatively easy and inexpensive to conduct. Compared to experimental designs, however, cross-sectional case-control studies provide a weak basis for drawing causal inferences. This weakness is because they can establish only one of the three conditions for causal inference (covariation of causes and outcomes, temporal precedence of causes, and elimination of plausible alternative causes), namely, covariation of potential causes (vulnerability, stress) with outcomes (disorder). Even if one observes a correlation between potential vulnerability factors or stressors and disorder, cross-sectional case-control studies cannot establish whether the vulnerability and/or stress temporally preceded disorder and are less able to rule out plausible alternative causes.

One strategy employed by many investigators of vulnerability-stress models is to try to establish cross-sectional relationships between the presence of disorder and vulnerability or stress through the use of correlational designs first, before testing the theory with more difficult and expensive experimental or longitudinal designs that permit stronger causal inferences. However, this strategy can mislead an investigator about a theory's validity. For example, if the hypothesized vulnerability or stressor occurs prior to onset of the disorder and does not overlap in time with the disorder, then use of this strategy may lead the researcher to reject the model when, in fact, an appropriate longitudinal design would have supported the model. Thus, as discussed earlier, it is
crucial to take into account the logical and temporal relations between hypothesized causes and disorder in choosing a suitable research design for testing a vulnerability-stress theory.

Longitudinal Designs

Correlational studies that are longitudinal allow the researcher to specify the time-order relationship among vulnerability, stress, and disorder because the same participants are studied on more than one occasion over time. Longitudinal designs can be retrospective, looking backward in time, or prospective, looking forward in time.

Retrospective and Follow-Back Studies

In a typical retrospective study, disordered cases and nondisordered controls are selected and asked to recall information about past stress or past vulnerability factors. Although such studies may suggest that posited vulnerabilities and stressors are related to disorder and, possibly, preceded disorder in time, a major shortcoming is that retrospective recall is subject to forgetting as well as systematic biases based on the participants' knowledge of their current disorder status. As an example, people with social phobia may recall more humiliating experiences in their past in an effort to explain their current social anxiety, a bias called "effort after meaning" (G. W. Brown & Harris, 1978), than will nonsocially phobic controls. Even temporal precedence of vulnerability and stress factors is more difficult to establish with retrospective designs because it is not possible to distinguish with certainty between the hypothesis that past vulnerability or stress contributed to the present disorder or that early signs of the disorder in the past contributed to the past occurrence of vulnerability or stress (Alloy, Lipman, & Abramson, 1992).

In follow-back studies, cases and controls are identified and then preexisting records (e.g., doctors' records, school records) of their experiences or characteristics are located, rather than risking the possibility of recall biases by having the participants or other informants recall the relevant past history. However, even preexisting records may contain systematic biases related to participants' current disorder status, as, for example, when school records are incomplete for currently disordered participants because of school truancy, which, in turn, may be related to their adult outcome (Achenbach, 1982). Thus, in sum, retrospective studies can provide useful information but they are not optimal, as a rule, for testing vulnerability-stress models of disorder.

Remitted Disorder Studies

In remitted disorder or postmorbid studies, individuals who have recovered or remitted from a disorder are compared to currently symptomatic cases and normal controls. This comparison can be made in a cross-sectional version of the design or, more commonly, in a longitudinal version. In longitudinal versions of the design, disordered cases are compared to nondisordered controls both when the cases are symptomatic and then, after follow-up, when the cases' symptoms have remitted. In addition, longitudinal within-subject comparisons are also usually conducted of cases in the symptomatic versus the remitted states. In psychopathology research, remitted disorder studies are most
commonly used to test whether certain variables act as vulnerability factors for the disorder in question (e.g., Barnett & Gotlin, 1988; Cornblatt & Keilp, 1994). The logic is that if some biological or psychological characteristic is a vulnerability factor for a disorder, then this characteristic should be highly stable and persist beyond remission of a current episode of the disorder. On the other hand, if the characteristic is a symptom or concomitant of the disorder, then it should be present during the episode of the disorder but dissipate upon remission of the episode (Alloy, Abramson, & Just, 1995; Alloy et al., 1992; Just, Abramson, & Alloy, in press). For example, if attentional dysfunction is a vulnerability factor for schizophrenia, as some theories have suggested (e.g., Cornblatt & Keilp, 1994), then attentional problems should be present both when a schizophrenic is actively symptomatic as well as when his or her symptoms have abated.

Remitted disorder designs may be one of the research strategies of choice for testing whether vulnerabilities or stressors worsen or otherwise change as a consequence of experiencing an episode of disorder. They may also be extremely useful in determining whether vulnerabilities and stressors that are present following an episode of a disorder (regardless of whether they were present prior to the initial onset of the disorder) predict relapses or recurrences of the disorder. However, Just et al. (in press; Alloy et al., 1995) provided four reasons why remitted designs are problematic for testing whether certain characteristics act as vulnerabilities for onset of a disorder. Here, we discuss two of these reasons with broadest applicability to a range of disorders. The first, and most telling, reason why remitted disorder studies are nonoptimal is that they cannot distinguish whether a particular characteristic is a vulnerability factor or a consequence of disorder. Even if remitted disorder studies found that a hypothesized vulnerability factor was present during the remitted state, such results would still leave uncertain whether the characteristic was present before the episode and contributed to its onset or, instead, developed as a result of the disorder. If remitted disorder studies do not permit inferences about the validity of vulnerability hypotheses regardless of what results are obtained, then they are not optimal designs for testing these hypotheses.

A second problem with remitted disorder designs is that they are based on the assumption that the hypothesized vulnerability is an immutable (unchanging) trait and, thus, should be exhibited in the remitted as well as the symptomatic state. Although this may be appropriate for theories that propose genetic diatheses for disorder, not all vulnerability-stress models feature an immutable trait as the vulnerability. Thus, if a particular vulnerability-stress model features a vulnerability factor that may change over time, remitted disorder designs would not provide an appropriate test of the vulnerability hypothesis of such a model (Alloy et al., 1995; Just et al., in press). We discuss the methodological implications of the relative stability of hypothesized vulnerabilities further in the section “Stability.”

Prosp ective Studies

A prospective study is a specific kind of longitudinal design in which the hypothesized causes of a disorder (vulnerabilities, stressors) are assessed (in a correlational version of the design) or manipulated (in an experimental version of the design) prior to the measurement of the dependent variable (the disorder) at some later point in time. Prospective studies, even if they do not include an experimental manipulation, are one
of the best designs for testing vulnerability-stress models of disorder because they not only can establish covariation between hypothesized causes and disorder but also allow for the relatively unambiguous determination that the hypothesized causes preceded the occurrence of disorder (two of the conditions necessary for establishing causality). However, even in prospective studies, plausible alternative causes ("third variables") must still be ruled out either by assessing the potential alternative causes and controlling for them statistically, by matching participants on potential alternative causes, or by manipulating the potential alternative causes and observing their effects on disorder. Prospective studies may involve a single follow-up of participants who vary initially on vulnerability and stress, but they provide better tests of vulnerability-stress models when they include repeated assessments of vulnerability, stress, and disorder over time.

Prospective studies have several advantages over other research designs (e.g., Cannon & Mednick, 1993). First, the participants have not yet experienced confounding effects of the relevant disorder such as medication, hospitalization, or symptoms that could; in turn, affect the measurement of vulnerability or stress. Second, much bias is eliminated from the assessments of vulnerability and stress because neither the participant, other informants, nor the researcher knows who will develop the disorder. Finally, information from the assessments of vulnerability and stress is current and does not depend on the participants' or other informants' recollection.

**High-Risk Studies**

One of the most powerful forms of prospective, longitudinal research in psychopathology is the high-risk design. *High-risk designs* involve the prospective, longitudinal study of people who have a high probability of developing a disorder of interest in the future because they possess the hypothesized vulnerability for the disorder. In *genetic high-risk studies*, used to test genetic vulnerability-stress models of disorder, participants (usually children) who do not initially have the disorder of interest but who are at risk for developing the disorder because they have a parent, twin, or other first-degree relative with the disorder are followed prospectively along with a comparison group of participants with normal first-degree relatives who are at hypothesized low genetic risk for the disorder (e.g., Cannon & Mednick, 1993; Mednick & Silverton, 1988; see Moldin, this volume). Another type of high-risk design that has been used increasingly in recent years is the *behavioral high-risk design* (e.g., Alloy et al., 1992; Chapman, Chapman, Kwapis, Eckblad, & Zinser, 1994; Depue et al., 1981). In this paradigm, nondiordered participants are chosen not because of genetic risk, but because they possess some behavioral characteristic hypothesized to make them vulnerable to developing a particular disorder in the future and are followed prospectively, along with a comparison group of individuals who score low on the hypothesized behavioral vulnerability.

In addition to all of the general advantages of prospective studies, high-risk designs have the additional feature that they have two built-in control groups: low-risk participants and high-risk participants who do not develop the disorder. The latter comparison group allows the investigator to study the role of stress as a factor that determines which high-risk participants develop the disorder as well as the role of potential moderating variables that serve to protect some high-risk participants from developing the disorder.
DIFFICULT CONCEPTUAL/METHODOLOGICAL ISSUES IN PSYCHOPATHOLOGY RESEARCH

In addition to choosing an appropriate research design from among the options reviewed above, an investigator who wishes to test hypotheses derived from a vulnerability-stress model also must confront several difficult conceptual issues with important methodological implications that complicate the implementation of these designs. Although there are many conceptual/methodological issues to be considered in vulnerability-stress research, we discuss some of the most important ones here.

Interdependence of Vulnerability, Stress, and Disorder

It is often assumed that the hypothesized vulnerability, stress, and resultant disorder are independent of one another. That is, the predisposition to a disorder is assumed to lie dormant until activated by independent environmental inputs, and then the interaction of these constitutional and environmental factors is thought to lead to the development of the disorder. In reality, however, this situation rarely occurs. Instead, vulnerability, stress, and disorder are typically interdependent in ways that greatly complicate researchers’ ability to test such models of psychopathology (Monroe & Simons, 1991). We examine some of these interdependencies in turn.

Effects of Vulnerability on Stress

Hypothesized vulnerabilities, whether genetic, biological, behavioral, or cognitive in nature, may influence either the measurement or the actual occurrence of hypothesized stressors. For example, the assessment of hypothesized stressors such as poor parenting or stressful life events may be affected by perceptual or reporting biases engendered by cognitive or personality diatheses. To illustrate further, the predispositions featured in cognitive vulnerability-stress models of depression (e.g., Abramson et al., 1989; Beck, 1987) involve a style to perceive and interpret life experiences negatively. Individuals who exhibit such negative cognitive styles may be more likely than those who do not possess this vulnerability to report relatively benign experiences as negative events and to perceive negative life events that do occur as especially aversive or “stressful” (Monroe & Simons, 1991). Thus, in general, the attempt to measure the featured stressors in vulnerability-stress models and to test the stressors’ contribution to the development of disorder may be confounded by vulnerability-based perceptual or reporting biases. Such confounding could lead to statistical interactions between diathesis and stress that do not actually provide support for the vulnerability-stress model because the vulnerability is counted twice, once in the measure of vulnerability and again in the measure of stress (Cohen & Wills, 1985; Monroe & Simons, 1991; Thoits, 1982).

The potential confounding effects of vulnerabilities are not limited to the measurement of stress; vulnerabilities may affect the occurrence of the hypothesized stressors as well. Many stressors featured in various vulnerability-stress models of disorder are at least partially under people’s control. Individual differences in vulnerability may influence the environments to which people expose themselves as well as actually contribute to the production of some stressors. Consider, as examples, that someone high in perfectionism, a hypothesized diathesis for obsessive-compulsive disorder in one
theory (Frost & Gross, 1993; Frost et al., 1994), may tend to expose himself or herself to situations in which mistakes (a triggering stressor for OCD) are inevitable, or that someone high in dependency, a hypothesized predisposition to depression in some interpersonal theories (Blatt & Zuroff, 1992; Hirschfeld et al., 1989), may engage in excessive reassurance-seeking that eventuates in his or her rejection (a triggering stressor for depression) by significant others (e.g., Coyne, 1976; Joiner, 1995). Even stressors that occur early in life may be affected by vulnerability. A schizophrenic woman may not only pass on her genetic vulnerability to her offspring, but her genetic risk may also lead her to behave in ways (e.g., poor prenatal care) that increase the chances her offspring will be exposed to birth complications (a hypothesized stressor for schizophrenia) as well. In sum, exposure to stress may not be random but, rather, may be systematically influenced by a person's vulnerability (Monroe & Sîmons, 1991).

How is an investigator to cope with the potential effects of vulnerability on stress? There are no easy solutions, but psychopathology researchers have suggested several useful procedures. One recommendation to increase the degree of independence between vulnerability and measured stress is to utilize investigator-based rather than respondent-based assessment procedures (e.g., G. W. Brown & Harris, 1986; Dohrenwend, Link, Kern, Shroot, & Markowitz, 1990), whereby hypothesized stressors such as poor parenting or stressful life events are assessed via direct observation or the investigator's ratings, respectively, rather than the person's self-report. To begin to address the potential impact of vulnerability on the actual generation of stress, researchers suggest two approaches. First, high- versus low-vulnerable persons can be compared on their exposure to stress (measured according to investigator ratings) prior to the onset of disorder to determine whether differences in vulnerability-driven stress generation must be included as additional pathways in the vulnerability-stress model of a disorder (Monroe & Sîmons, 1991). Second, investigators can rate the degree to which stress is independent of versus dependent on the person's behavior or vulnerability and include only independent stressors (e.g., uncontrollable or fateful stressors) in their tests of vulnerability-stress predictions (G. W. Brown & Harris, 1986; Shroot et al., 1989).

**Effects of Disorder on Stress**

Not only can vulnerability influence the measurement and occurrence of stress, but disorder, too, may be confounded with stress in these same two ways. The disorder itself or early subsyndromal symptoms of the disorder may affect stressor measurement, as in the case of an individual with paranoid symptoms who perceives and reports the benign comments of others as hostile communications. Behavioral manifestations of disorder also can generate stress that, in turn, further exacerbates the disorder (Hammen, 1991). Consider, for example, a socially phobic individual whose blushing and trembling during a speech interferes with her performance and, thus, increases her anxiety further (Schneier, 1991; Uhde, Tancer, Black, & Brown, 1991). The potential contamination of stress by disorder may be addressed methodologically with the same procedures described above for the confounding of stress by vulnerability. In addition, however, the potential contaminating effects of disorder on stress call for the use of prospective, longitudinal designs in which stress is assessed prior to the onset of disorder and, therefore, is unconfounded by disorder.
Effects of Disorder on Vulnerability

The occurrence of disorder or subsyndromal early manifestations of disorder can also influence a person's level of vulnerability. Specifically, vulnerability may initially develop or preexisting vulnerability may be exacerbated as a consequence of disorder, similar to the manner in which a physical injury may leave a scar (e.g., Rohde, Lewinsohn, & Seeley, 1990; Zeiss & Lewinsohn, 1988). As an example, the experience of a panic attack may worsen an individual's anxiety sensitivity (a hypothesized vulnerability factor for panic disorder; Schmidt et al., 1997) or may create anxiety sensitivity where it did not previously exist.

Investigators have utilized two approaches to deal with the potential effects of disorder on vulnerability. The first, and preferred, approach is to adopt a prospective, longitudinal design in which participants who vary in their levels of vulnerability but who do not currently have the disorder of interest are followed through a period of risk to examine the likelihood of disorder onset (Alloy et al., 1995; Just et al., in press). A further issue with this solution, however, is whether one should select currently nondisordered participants or, even more stringently, never disordered participants. Never disordered participants provide perhaps the cleanest test of vulnerability effects on the likelihood of disorder onset, uncontaminated by "scars" from prior occurrences of the disorder. However, by selecting participants who have no prior history of the disorder, one might be left with an unrepresentative group of high-risk participants who, despite being vulnerable to the disorder, have been protected by unknown factors from actually developing the disorder (Alloy et al., 1995). This would lead to an overly conservative test of a vulnerability hypothesis.

The second approach for dealing with possible confounding effects of current or past disorder on vulnerability is to control for disorder statistically by including measures of current or past disorder as a covariate in analyses predicting future disorder from vulnerability (and stress). However, the covariate approach also provides a very conservative test of the vulnerability hypothesis. The problem is that any variance in future disorder onset that is shared between vulnerability and current or past disorder is allocated to current or past disorder rather than to vulnerability, even though the vulnerability hypothesis predicts that such shared variance should exist (assuming that the sample is experiencing some stress). In such a case, an obtained relationship between vulnerability and future disorder onset may vanish when measures of current or past disorder are used as covariates, even when the vulnerability hypothesis is, in fact, correct. Use of this covariate approach, then, may lead to pseudofalsification of a valid vulnerability hypothesis (see Meehl, 1971). Thus, whether to select out or control statistically for past or current disorder in tests of vulnerability-stress models remains a very thorny question.

Issues in the Assessment of Vulnerability

A powerful test of any vulnerability-stress model requires adequate measurement of the hypothesized vulnerability factor. Although many issues arise in measuring vulnerability (e.g., internal consistency), we discuss two that are particularly important in a theory-based approach: priming and stability.
Primling

Just as researchers of physical health problems have discovered that some biological vulnerabilities to physical disorders cannot be detected easily when a person is in a baseline or resting state, psychopathologists (e.g., Depue & Monroe, 1983) have suggested that some vulnerabilities to various mental disorders lie "latent" until activated by appropriate stimuli. For example, in Beck's (1967, 1987) theory, the negative self-schema, a hypothesized cognitive vulnerability factor for depression, is characterized as latent until activated by the occurrence of a life event that is relevant to the content embodied in the self-schema. More recently, several theorists (e.g., Persons & Miranda, 1992; Segal & Ingram, 1994) have further proposed that a negative mood state also may activate or prime cognitive vulnerabilities for depression.

Adequate measurement of a hypothesized latent vulnerability requires a challenge protocol in which the relevant biological or psychological system is primed environmentally, pharmacologically, or in some other way (Depue & Monroe, 1983). To be useful, a theory featuring a latent vulnerability factor must specify the prime(s) hypothesized to activate the vulnerability. Thus, to continue with the depression example, a researcher wishing to test Beck's (1967, 1987) vulnerability-stress model would need to prime negative self-schemata with negative events or mood to provide a measure of vulnerability with high theoretical fidelity. Currently, controversy surrounds Beck's priming hypothesis because some prospective (e.g., Alloy et al., in press; Alloy, Abramson, Murray, Whitehouse, & Hogan, 1997; G. P. Brown, Hammen, Craske, & Wickens, 1995) and laboratory (Dykman, 1997) studies have found that even when measured in an unprimed state, cognitive vulnerabilities still predict depression onset and/or increases. Future research is needed to determine which hypothesized vulnerabilities for various disorders actually do require priming for adequate measurement.

Stability

Another important measurement issue is the stability of the hypothesized vulnerability factor. Different measurement strategies are required for vulnerabilities that do not change over time compared to those that may change. Historically, psychopathologists often have inferred, incorrectly we would argue, that to qualify as a vulnerability factor, a variable must possess traitlike stability. Perhaps this assumption arose from adoption of the term diathesis, which nicely denotes an individual difference variable but, unfortunately, also may connote traitlike stability given that this term sometimes is used in medicine to refer to a fixed characteristic providing risk for a disorder. Consequently, we prefer the term vulnerability to diathesis because the former is less likely to connote stability than the latter.

An example from medicine illuminates that an individual difference vulnerability factor for disease need not necessarily show traitlike stability and, instead, may show transient or long-term fluctuations. Consider the role the immune system plays in vulnerability to disease. A person's vulnerability to various diseases is in part a function of the integrity of the various arms of his or her immune system. Marked immune suppression increases vulnerability for a host of diseases, from influenza at the mild end to cancer at the severe end of the continuum. Yet, the integrity of the various branches of a person's immune system can change over time (e.g., Coe, 1994; Schindler, 1991).
Similarly, it is possible that at least some vulnerabilities for various psychopathologies may change over time. Consistent with this view, some schizophrenia researchers have begun to suggest that vulnerability for schizophrenia may vary over time within a given individual as well as across individuals (see Gooding & Iacono, 1995, for an excellent discussion of the evolution of the concept of vulnerability in the field of schizophrenia). For example, Prescott and Gottesman (1993) have entertained the possibility that schizophrenes can switch on and off in a given person over time.

The possibility that some vulnerabilities may not be immutable or unchanging over time has critical implications for the design of longitudinal studies. One cannot assume that someone at hypothesized high risk for the relevant disorder at the outset of the study also will be at high risk later in the study or that a low-risk person will remain at low risk. Therefore, an ideal longitudinal study would include assessment of vulnerability at multiple times throughout the follow-up period along with corresponding assessment of disorder to determine whether changing levels of vulnerability predicted expected increases and decreases in the likelihood of disorder. A powerful theory of a given disorder would describe the conditions under which vulnerability is hypothesized to change and thereby specify the optimal times for reassessing vulnerability and consequent disorder during the study.

**Issues in the Assessment of Stress**

Although vulnerability-stress models of psychopathology rest on the simple premise that stress activates and transforms a preexistent predisposition into disorder, the multifaceted nature of stress and its complex interrelations with disorder and vulnerability introduce formidable challenges to its assessment. Given that many vulnerability-stress models of disorder focus on life events as the hypothesized stressor, this section outlines basic conceptual and methodological issues encountered by prominent approaches to life-stress assessment and highlights current methods to standardize measurement.

*Respondent- versus Investigator-Based Approaches*

Numerous biological and psychosocial constructs have been employed in the study of stressful life experiences. Although there is no consensus as to a single definition of stress, the construct is generally viewed as a transaction between an individual and his or her environment (Monroe & Roberts, 1990). For psychosocial models in particular, the nature of this transaction—the individual’s subjective experience in the context of an objective environment—is a fundamental source of theoretical and methodological problems that are represented by two major approaches to stress measurement. **Respondent-based** approaches adopt a subjective reference point by utilizing the self-report of the individual experiencing the stressor. This approach regards respondents as the most qualified with respect to evaluating stressful circumstances in their own lives and, consequently, relies extensively on the individual’s perception and interpretation of an event. In contrast, **investigator-based** approaches are anchored in objective characteristics of a stressor and the environmental context in which events occur. Here, the goal is to assess the nature and magnitude of an event without the contaminating influence of an individual’s idiosyncratic perceptual tendencies (G. W. Brown, 1981). To illustrate the difference between the two approaches, consider two participants
who each indicate on a life events checklist that they experienced the stressor of “a serious illness in the family” in the past month. One participant is referring to her mother’s diagnosis of cancer and the other to his cousin’s earache. In a respondent-based approach to the measure of stress, the participant’s subjective report would be accepted as his or her level of stress. In an investigator-based approach, a set of a priori criteria would be developed for deciding whether the participant’s actual experience (mother’s cancer or cousin’s earache) qualified as an example of “a serious illness in the family.”

Although respondent-based methods and the use of self-report measures are most commonly used in life-stress research, the interdependence of vulnerability, stress, and disorder discussed earlier renders this approach quite problematic for vulnerability-stress models. The potential fusion of independent (stress, vulnerability) and dependent (disorder) variables that accompanies respondent-based methods has motivated researchers to examine the stressor and diathesis components separately to more clearly examine their relations to disorder. It is worth emphasizing that the focus of investigator-based methods on environmental facets of stress in no way diminishes the significance of subjective perception. On the contrary, vulnerability-stress models that adopt the investigator-based measurement approach often view perceptions as extremely important in mediating the effect of objective characteristics of the environment on the onset, course, and remission of disorder. In fact, by separating these potentially confounded components, investigators have begun to demonstrate, for example, that participants’ cognitive styles influence the generation of life events, the threshold for reporting life events, and the rating of their severity (Dohrenwend et al., 1990; Simons, Angell, Monroe, & Thase, 1993). Investigator-based approaches simply maintain that the effects of individual differences in vulnerability can only be examined meaningfully within a system of measurement that clearly distinguishes between objective characteristics of stress and an individual’s perceptions of it (Monroe & Roberts, 1990).

Definitional and Procedural Issues in Operationalizing Stress

To distinguish between diathesis and stressor components, studies designed to test vulnerability-stress models must establish a set of procedures and operational criteria for assessing stressful events. To address the most fundamental issue of what constitutes an event, researchers must utilize several strategies. First, definitional criteria for life events must be specified to set a threshold for what environmental conditions qualify as stressors. Then, threshold conditions that distinguish events according to their magnitude (e.g., major events, minor events, hassles, chronic difficulties) must be established. The extensive work of G. W. Brown and Harris (1978, 1989) in the development of the Bedford College Life Events and Difficulties Schedule (LEDS) and rating system exemplifies efforts to standardize event definitions. Based on the notion of contextual threat and the average expectable impact of a stressor given an individual’s specific biographical circumstances, the LEDS system controls for individual differences in perception by utilizing consensually derived definitions and ratings of stress.

A related issue that arises in the operationalization of stress involves how to go about representing the complexity of stressful circumstances. In other words, at what point should different aspects of a stressful encounter be considered separate events? Again, a
set of clear guidelines and procedures must be established. To date, such guidelines have focused on the determination of whether various facets of an experience involve distinct implications with respect to coping, threat, or consequences. Temporal features of the event occurrence can also guide the appropriate partitioning of complex experiences. For example, researchers assessing life events may adopt a “one-day rule” for those cases that could be recorded as single or multiple events. That is, if the events occurred within the same day (e.g., broken leg and contusions) and do not pose significantly distinct demands on the individual with respect to coping or consequences, they may be represented as a single event (e.g., an accident).

Issues in the Assessment of Disorder

Formidable conceptual issues with important methodological implications also surround the assessment of disorder in vulnerability-stress models. We consider two of the most relevant issues here: the continuity versus discontinuity of disorder and disorder subtypes.

Continuity versus Discontinuity of Disorder

Among clinical researchers, there is much debate about whether psychopathological disorders are continuous or discontinuous with milder psychological distress or dysfunction. The essence of the continuity issue is whether mild and moderate distress or dysfunction differ quantitatively (in degree) or qualitatively (in kind) from syndromal disorder that meets diagnostic criteria (Coyne, 1994; Flett, Vredenburg, & Krames, 1997). Are milder forms of dysfunction distributed throughout the normal population linearly related to diagnosed cases of disorder, or are diagnosed cases distinct entities? The continuity debate is relevant to most forms of psychopathology, including depression (Compas, Ey, & Grant, 1993; Coyne, 1994; Flett et al., 1997), bipolar disorder (Depue et al., 1981), schizophrenia (Lenzenweger & Korfine, 1992), panic disorder (Norton, Cox, & Malan, 1992), social phobia (Davidson, Hughes, George, & Blazer, 1994), obsessive-compulsive disorder (Gibbs, 1996), eating disorders (Shisslak, Crago, & Estes, 1995), and personality disorders (Widiger & Costa, 1994), to name a few. The reader is referred to these references for the current empirical status of the debate for each of these disorders. It is important to distinguish the continuity issue from the comparability issue (Flett et al., 1997; Kendall & Flannery-Schroeder, 1995). Whereas continuity concerns the degree of similarity among various levels of dysfunction within a sample, comparability concerns the generalizability of findings across samples (e.g., between college student and clinical samples), a form of external validity.

The continuity issue has important implications for selection of optimal samples and measurement procedures in psychopathology research. In guiding their choice of research sample and assessment instruments, researchers who wish to test particular vulnerability-stress models of disorder need to carefully consider whether the model in question hypothesizes explicitly or assumes implicitly continuity or discontinuity between subsyndromal forms of the disorder and cases that meet diagnostic threshold. If the model hypothesizes continuity, then studies could appropriately include nonclinical or clinical samples and optimally would use dimensional assessment procedures,
whether self-report, observational, or interviewer-rated, that allow for measurement of the full range of severity along the continuum of disturbance. Alternatively, models that assume discontinuity require the use of measurement tools that allow for decisions about diagnostic “caseness,” such as semistructured diagnostic interviews (e.g., Schedule for Affective Disorders and Schizophrenia [SADS], Endicott & Spitzer, 1978; Structured Clinical Interview for DSM-IV [SCID], Spitzer & Endicott, 1995).

**Subtypes of Disorder**

A second important consideration in assessing disorder is whether the disorder in question is unitary or is heterogeneous with one or more subtypes. Clinicians and researchers alike have long suggested that several different forms of psychopathology, including depression (e.g., Craighead, 1980), schizophrenia (Crow, Cross, Johnstone, & Owen, 1982; Kraepelin, 1923), obsessive-compulsive disorder (Rachman & Hodgson, 1980), and alcoholism (Cloninger, Bohman, & Sigvardsson, 1981), to name a few, are actually heterogeneous entities. Some vulnerability-stress models specify etiological pathways hypothesized to lead to the development of a subtype of a heterogeneous disorder; for example, the hopelessness theory (Abramson et al., 1989) predicts the development of the subtype of “hopelessness depression” rather than depression in general, and Cloninger and colleagues’ (Cloninger et al., 1981) genetic model predicts a familial subtype of male alcoholism rather than alcoholism in general. Even when a particular vulnerability-stress model does not predict explicitly to a disorder subtype, the possibility that a disorder may, in fact, be heterogeneous with multiple subtypes has important implications for research designed to test the model.

First, research designs (whether cross-sectional, retrospective, or prospective) that compare the likelihood of exhibiting the hypothesized vulnerability or stress in individuals with the general disorder versus nondisordered controls may frequently yield negative results, despite the fact that the vulnerability-stress model is correct, when the general disorder contains subtypes (Abramson et al., 1988; Alloy et al., 1988). This is because not all members of the general disorder category (e.g., schizophrenia) will show the relevant vulnerability or stress if the vulnerability-stress model only predicts to a subtype of the general disorder (e.g., paranoid schizophrenia). Given that some schizophrenics (e.g., catatonic and disorganized types) would not be expected to show the paranoia-relevant vulnerability or stress, whether one obtains differences between a heterogeneous group of schizophrenics and nonschizophrenics on vulnerability or stress depends on the prevalence of the relevant subtype (i.e., paranoid type) in the particular sample of general schizophrenics. Moreover, the failure to recognize and assess subtypes of the general disorder can lead to the inability to replicate research findings. This is because the magnitude and consistency of vulnerability or stress differences between disordered and nondisordered participants will vary across studies, depending on the base rate of the relevant subtype in each study sample of participants with the heterogeneous, general disorder (Abramson et al., 1988; Buchsbaum & Rieder, 1979). Finally, even prospective studies designed to test whether the interaction of hypothesized vulnerability and stress leads to the future development of a disorder can lead to inappropriate rejection of the theoretical model if the model predicts a subtype of the heterogeneous disorder and the researcher has failed to assess the subtype specifically.
CONCLUSION

In reading through this chapter, one might become overwhelmed by the difficult choices of research design and the challenging conceptual and methodological issues an investigator faces in conducting psychopathology research. However, if the reader keeps several general points in mind, the task may not seem so daunting; indeed, it can be fun. First and foremost, as we began this chapter, we end by emphasizing that the choice of research design and the strategies for tackling conceptual and methodological challenges should be guided by the theory or hypothesis to be tested. Second, at present, it is not possible to conduct the “ideal” study, so all research studies are bound to have some shortcomings. By conducting a program of research, an investigator can overcome the limitations of any individual study and gather a body of evidence that leads to valid conclusions. Finally, the excitement and importance of discovering and validating potential risk factors, triggering agents, potentiating and protective factors, and mediating mechanisms for psychopathological disorders can frequently promote elegant and sophisticated solutions to the research design issues covered herein.

REFERENCES


