Chapter 1

Fundamental Issues

This chapter first considers the need for a framework to integrate the various levels of analysis necessary for understanding the development of a psychopathology. Such a framework raises a key issue: the mind–body problem in psychopathology. After examining this issue, we turn to the use and validation of diagnoses, and conclude with fallacies that have impeded understanding of this difficult topic.

ECUMENISM VERSUS INTEGRATION: THE NEED FOR A NEW FRAMEWORK

Despite considerable empirical progress in the field of psychopathology in the last few decades, we lack a satisfactory comprehensive theory. The 20th century began with a comprehensive theory of psychopathology, Freud's psychoanalytic theory, which dominated the field for at least 50 years. Its focus on the importance of early relationships and development are still important insights, and are still being investigated by attachment theorists. Psychoanalysis as a treatment has evolved into interpersonal forms of psychotherapy, some of which are empirically validated. Yet psychoanalytic theory's shortcomings as a scientific explanation of psychopathologies are now well known. Within psychology,

psychoanalytic theory was replaced by learning and then by cognitive-behavioral theories of psychopathology. Within psychiatry, Freudian theory has largely been replaced by biological psychiatry. But neither biological psychiatry nor cognitive-behavioral psychology offers a comprehensive theory for understanding psychopathology.

Biological psychiatry has the advantage of placing the brain squarely in the center of the understanding of psychopathology, but it often has been too reductionistic: too focused on single causes (e.g., alterations in a given neurotransmitter) or on a single level of analysis (e.g., synapses).

Current psychological theories have the advantage of dealing with interpersonal and social contexts that shape the development of a psychopathology, but they are weak at explaining individual differences and mostly ignore the brain.

This state of affairs is often reflected in contemporary abnormal psychology or psychiatry books by an uneasy ecumenism. Psychological and physiological theories are laid out side by side but rarely integrated. Some texts speak of a "biopsychosocial" model, but this model is usually an ecumenical umbrella for covering disparate approaches rather than an integration.

So we are at an interesting point in the history of the science of psychopathology. Previous comprehensive theoretical paradigms have failed; new empirical methods are rapidly producing data that need to be accounted for, but current theories of psychopathology are inadequate for the task of integration. At the same time, a new scientific paradigm is emerging in cognitive neuroscience. However, to deal with the development of psychopathology, cognitive neuroscience needs to be broadened in three key ways: (1) It must focus explicitly on individual differences; (2) it must integrate emotion and social influences into the study of cognition; and (3) it must incorporate development. Although this book does not pretend to offer a complete new theory of psychopathology, it does attempt to lay out the conceptual and empirical constraints that a new theory of psychopathology will have to meet, and to show how a cognitive neuroscientific approach can satisfy those constraints.

Hence, a basic message of this book is that we need a way to integrate research on the biological and psychological mechanisms involved in developmental psychopathologies. Consider a child with mild depression, or dysthymia. A biologist might seek the explanation for this clinical condition in differences in receptors for neurotransmitters. But

a psychologist might seek the explanation in differences in attachment security. These very different ways of thinking about the same clinical phenomenon are not necessarily competing explanations. Rather, they may be complementary, each operating at a different level of analysis. However, for either the biologist or the psychologist to think about how these two explanations relate to each other is not straightforward because a theoretical framework for integrating these different levels of explanation is only beginning to emerge.

This neuroscientific framework seeks to relate behavior and mind to the brain. It is important to realize that every psychopathology requires us to solve the brain–behavior or mind–body problem. It is not enough to frame an explanation of a psychopathology purely in terms of mental or psychological constructs. To do so ignores the brain. At the same time, to frame an explanation purely in terms of brain variables such as receptor efficiencies or densities is not enough. To do so reveals a naive reductionism, because even if the causal brain variables were known, we would still need to know how these brain differences lead to changes in behavior.

The important overall point of a neuroscientific perspective is that analyses of normal or abnormal function need to be informed by an understanding of the brain structures and processes that implement the function. In other words, "hardware" matters and provides important constraints for developmental theories, whether they are theories of neo-Piagetian cognitive operations or internal working models in attachment theory. So taking a neuroscientific perspective forces us to confront a latent "dualism" in much of developmental psychology, the assumption that analyses of behavioral function can proceed completely independently of analyses of brain. As Patricia Goldman-Rakic (1987a) aptly said in discussing the relation between neuroscience and developmental psychology, "The 'empty organism' has long since been filled with intentionality and information-processing skills, but not necessarily with a central nervous system" (p. 601).

To draw out the implications of this point, let us take as an example a hypothetical developmental psychopathology that is *entirely* determined by the social (i.e., interpersonal) environment—no genetic influence; no traumatic, toxic, or other noninterpersonal environmental alteration of brain development. It is very easy to catch oneself thinking that in such a case the pathogenetic social influences are registered somewhere other than the brain—in the attachment system, in object relations, or what have you. The point of a neuroscientific perspective is

that *all* social influences affect brain development in some way or another, and all psychological constructs are implemented by brain mechanisms. For instance, the neuropsychology of traumatic social experiences such as loss, neglect, and abuse is becoming fairly well understood. Such traumas can cause very persistent changes in brain development. Moreover, positive social experiences also affect brain development and function. Humans are social animals and are therefore "open" systems, dependent on social relations. So taking a neuroscientific perspective does not limit the unit of analysis to an individual person (or his or her nervous system). Psychopathology may exist in an individual, a dyad, or a social group; I am simply arguing that a neuroscientific perspective is relevant in each case. For instance, it has been shown that an individual baboon's neurochemistry changes when its position in the dominance hierarchy changes (Sapolsky, 1994).

These considerations mean that the familiar clinical distinction between "functional" and "organic" is misleading and, in a strict sense, fundamentally incorrect. There is no autonomous substrate for functional pathologies, nor does the functional–organic distinction neatly divide disorders either by treatability or mode of treatment. For instance, it is frequently assumed that functional disorders call for behavioral treatments and are more amenable to treatment, whereas organic ones are less treatable and call for biological interventions. However, many counterexamples, such as phenylketonuria (PKU) on the one hand, and multiple personality disorder on the other, can be cited.

One can discern this functional versus organic assumption in contemporary psychopathology textbooks. The most heritable disorders, such as bipolar illness or autism, are thought of as "biological" disorders, whereas less heritable disorders, such as dysthymia or phobias, are thought of as functional disorders produced by socialization or experience. As we will see, there is a striking absence of psychological theories for the development of bipolar disorder and fewer psychological treatments. Also, the success of early psychological treatments for autism questions this assumption. The main point is that as soon as we accept this assumption, we have given up on a universal theory of psychopathology.

It is also important to emphasize that a commitment to a neuroscientific perspective does not commit one to a belief in single, deterministic causes for developmental psychopathology. Developmental psychopathologies are complex behavioral disorders in two senses: The disrupted behaviors are complex, and the multiple developmental path-

ways that led to the disruption are complex. For most psychopathologies, multiple risk and protective factors, both genetic and environmental, affect outcome in a probabilistic rather than deterministic fashion. Both normal and abnormal development result from the self-organizing properties of complex systems, so single causes are unlikely, and interactions and nonlinearities are to be expected.

So, clearly, my point about the relevance of brain mechanisms for understanding a purely social pathology is not an argument for reductionism. Risk factors will be found at different levels of analysis for different developmental psychopathologies: the molecular level for some, and the attachment system for others. But all risk factors act on the same complex developmental system that cannot be eliminated from an explanation. Thus, no level of analysis is entirely autonomous or encapsulated; interpersonal systems do not exist in some "social ether" outside of human organisms. Learning and using such systems is constrained by the real human brain, which evolved for just that function, among others. Moreover, dynamic principles that describe network properties within a brain may well have some utility in describing the dynamics of social networks.

Our claims about the relevance of neuroscience for purely social pathologies are integrative rather than reductionistic. The point is that neuroscience potentially provides a broad-enough paradigm to encompass *all* of developmental psychopathology. While a complete explanation for some pathologies may emphasize different levels of analysis than the explanation of others, all can (and need to) fit within the same broad paradigm. A pathology that is caused in part by genetic influences will require an explanation that begins at the deepest explanatory level, with an altered DNA sequence, and proceeds across many levels of analysis, up to the level of observable behavior. In contrast, a pathology that is completely caused by aberrant parenting may require fewer levels of analysis, but these levels will overlap with those used in the previous example; we should not have to invoke a totally different paradigm. Moreover, aberrant parenting may change gene expression and brain development.

In summary, the argument is that we need a new framework or paradigm for understanding the development of psychopathology, and cognitive neuroscience provides that framework. As discussed by O'Reilly and Munakata (2000), there are two complementary aspects of a cognitive neuroscience approach: physical reductionism and reconstructionism. *Both* aspects are needed for a comprehensive understanding of

psychopathology. Unlike most contemporary psychological theories of psychopathology (e.g., cognitive-behavioral or developmental theories), cognitive neuroscience is explicitly committed to physical reductionism: The components of cognition and behavior must be reduced to their physical substrate, the brain, just as key aspects of living organisms have been reduced to molecular biology. Biological psychiatry has applied physical reductionism to psychopathology with noteworthy success, but physical reductionism alone cannot give us an explanation of complex behavior. Unlike much of biological psychiatry, cognitive neuroscience is also explicitly committed to reconstructionism: an account of how interactions among the elementary units (i.e., neurons) of the nervous system give rise to the phenomena of cognition and behavior. Achieving this reconstruction has been greatly aided by the development of neural network models, which, as we will see, have provided a much deeper functional understanding of how complex cognitive phenomena arise from the interaction of neuron-like elements. (But, unfortunately, there are relatively few neural network models of psychopathology.) In short, current approaches to psychopathology are either functional theories unrelated to the brain or biological reductions that do not attempt to reconstruct function. Although both approaches have made empirical progress and have led to the development of effective treatments, they cannot by themselves be integrated. A different framework is needed to accomplish that, one that includes both physical reductionism *and* reconstructionism.

Applying a cognitive neuroscientific approach to psychopathology will enrich both fields and change the boundaries of what we currently consider to be psychopathology. We have just discussed how the field of psychopathology will be enriched. One benefit to cognitive neuroscience is that it will have to include emotion and arousal in its models. As we will see, there is a notable paucity of neural network models of mood and anxiety disorders. In terms of boundaries, the current artificial division between psychiatric and neurological disorders reflects the misleading functional versus organic distinction discussed earlier. For example, some developmental disorders, such as dyslexia and mental retardation, are not always considered psychopathologies. Some neurological disorders, such as attentional neglect or the alien hand syndrome, are virtually never considered psychopathologies. This artificial division exists in spite of the fact that psychopathology is traditionally defined as an alteration in thought, mood, or behavior that impairs adaptive functioning; clearly, developmental and neurological disorders

fit this definition. A cognitive neuroscientific approach aspires to explain all these kinds of disorders—traditional psychopathologies, developmental disorders, and neurological disorders—with similar models. At the same time, how we think about developmental and neurological disorders will be enriched by thinking about how social and emotional influences alter their course and affect treatment.

In taking a neuroscientific perspective on either abnormal or normal behavioral development, several levels of analysis need to be considered. We organize the discussion of specific psychopathologies into four broad categories—etiology, brain mechanisms, neuropsychology, and the symptom or surface levels (Pennington, 1991)—similar to the four levels proposed by Morton and Frith (1995) in their framework for analyzing developmental psychopathologies.

Before discussing these levels in more detail, it is important to see how they are causally related (Figure 1). Neuroscientists sometimes assume that the causal arrows only run in one direction across these levels of analysis, as depicted in the top part of Figure 1. Etiological factors—namely, gene variants and environmental risk factors—change brain development, which in turn changes neuropsychological development, which leads to changes in behavior. Some of these changes in behavior are the symptoms that define a given disorder. However, the situation is not that simple. A child's behavior changes his or her experience, which in turn changes brain development and the social environment's response to the child, which in turn affects his or her development. Although experience and environment ordinarily do not change genes (i.e., their DNA sequences), such factors can definitely influence gene expression. For example, early stress experiences change the expression of the gene that produces the glucocorticoid receptor (Meaney et al., 1996). Glucocorticoids are hormones important to the stress response. So a more realistic model is provided in the bottom part of Figure 1, where we see that the causal arrows run in *both* directions.

Four Levels of Analysis

Now I discuss these levels of analysis in more detail. The *etiological* level is concerned with genetic and environmental influences that cause the pathology in question. Genetic and environmental influences may act independently, but they may also interact or correlate with each other—the latter situation having some similarities to what developmental psychopathologists call "transactions" (Pennington & Ozonoff,

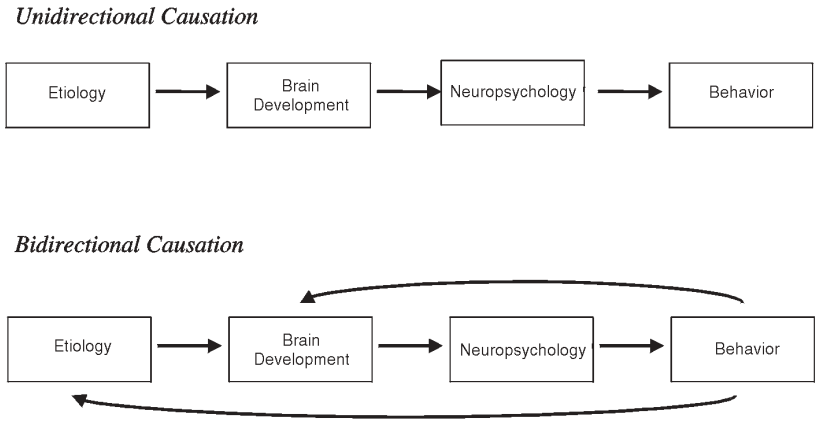

FIGURE 1. Models of causation.

1991). An obvious but frequently overlooked methodological point is that clear answers about environmental etiologies cannot be obtained without controlling for genetic influences. Unfortunately, many existing studies of supposed environmental influences on developmental psychopathology include only non-twin, biological families, in which genetic and environmental influence are inherently confounded. Likewise, until recently, studies of genetic influences on both psychopathology and normal behavior have been indirect, relying on quasi-experiments such as twin and adoption studies, and utilizing fairly simple additive models of genetic and environmental influences. It is now clear that virtually all psychopathologies are caused by a mix of both genetic and environmental factors that likely interact in the process of development to produce a psychopathology, but empirical methods for detecting such interactions have been very limited. Recent advances have made it possible to measure genetic influences on psychopathology directly, as will be discussed later, and to conduct longitudinal studies of individuals with risk alleles to examine gene $\times$ environment interactions (Plomin & Rutter, 1998).

The next level of analysis concerns how these etiological influences act on the development of *brain mechanisms*. One of the important recent discoveries in neuroscience is that early experience plays a very important role in sculpting the connectivity of the developing brain; with about 10^{11} neurons and a total of about 10^{15} connections between

them, it is logically impossible for 10^5 genes to specify neuronal location and connections in a hardwired fashion (Changeux, 1985). Instead, the developing brain overproduces neurons, dendrites, and synapses, and then lets experience "select" which elements to preserve through a kind of "neural Darwinism" (Edelman, 1987). Later experience also changes brain structure both by adding or subtracting dendrites and synapses, and by modifying existing synapses (Greenough, Black, & Wallace, 1987). So a fundamental account of how experience alters brain structure is emerging within neuroscience; this account is of obvious relevance to psychopathologists who ponder why some experiences are so formative and others are so surprisingly neutral in their long-term effects.

On the genetics side, the substantial heritabilities found for many normal and abnormal individual differences in behavior mean that there are genetically caused variations in brain structure and function within our species. What aspects of brain development are likely targets for genetic influence? Although it has been shown that genetic influences on behavior can "turn on" across the lifespan (Plomin, 1990), it is likely that many genetic influences on brain development, especially those important for developmental psychopathologies, act on early brain developmental processes, such as neuronal proliferation, migration, and differentiation, as well as the formation and distribution of receptors for neurochemicals. There exist numerous animal examples of specific genetic mutations that affect the development of specific brain structures, such as the mouse mutants with specific cerebellar and hippocampal malformations (Changeux, 1985). There is neuropathological evidence of similar early alterations of brain structures in some human pathologies, such as dyslexia and schizophrenia (Nowakowski, 1987). Such mutations not only affect neuronal migration and lamination in a specific brain structure but also alter neural connectivity more widely and presumably alter the computational properties of neural networks. Hence, there is a resolution to the apparent paradox of how a seemingly small, early change in brain development can have major effects despite the sometimes impressive plasticity of the developing brain given a later (and larger) acquired lesion. Other psychopathologies may involve alterations in brain structure on a finer scale, such as changes in the structure or distribution of receptors. Since receptors are proteins coded by genes, variation in their structure is under genetic control.

The next level of analysis, *neuropsychology*, bridges the chasm separating brain and behavior, mind and body, making this level of analysis

the most difficult conceptually. Although there are levels of analysis within neuropsychology, by and large, neuropsychology has been focused on a sufficiently molar level of behavioral analysis that the behavioral categories it studies are not completely outside the view of functional psychology. Yet unlike functional psychology, these categories are constrained in neuropsychology by what we know about brain function. Thus, neuropsychology finds spatial cognition an acceptable category but has rejected categories such as a general-purpose short- or long-term memory, and has generally avoided categories such as the self, will, and object relations. (But the fact that both folk and real psychologists use these latter concepts—at times effectively—to predict and to explain behavior is a phenomenon for which neuropsychology must eventually account; see Dennett, 1987.) One can think of neuropsychology as a kind of amalgam of concepts and categories from cognitive psychology, developmental psychology, and neuroscience. Sometimes this amalgam leads to a coherent explanation of the connection between brain and behavior, and sometimes not, as we see when considering specific disorders. Or we could think of neuropsychology as a kind of scaffolding; once we have completed the edifice of neuroscience, neuropsychology in its present form may be nowhere within it. One reason for this eventual outcome is that most of current neuropsychology is not computational. The long-term goal of neuroscience is to provide a computational account of molar functions that explains our current, preliminary notions about the cognitive architecture in terms of the workings of neural systems (Arbib, 1989). To summarize, current neuropsychology is concerned mostly about molar functions that we can recognize, with the constraint that these functions fit what we currently know about how the real brain works; eventually, current neuropsychology will be replaced by a more precise, computational account of how the real brain accomplishes these functions.

For a psychopathologist, it is important that neuropsychology provide an underlying level of behavioral analysis that is closer to and more consistent with brain mechanisms than either the phenomenological account of a syndrome given in the language of symptoms or the purely psychological account couched in terms of Freudian or cognitive-behavioral constructs. In Chapters 3–5, which deal with reviews of specific disorders, we find a great deal of variability in our neuropsychological understanding of psychopathologies. The cognitive neuropsychology of dyslexia is sophisticated and includes neural network models, whereas the neuropsychology of mood and anxiety disorders is

much less well developed. In reviewing specific disorders, I label this level of analysis "neuropsychology" and critique both neuropsychological and purely psychological theories. My goal is to highlight the considerable work that is needed to attain a cognitive neuroscientific understanding of psychopathologies.

The *symptom or surface* is the last level of analysis, the one at which most current developmental psychopathologies are defined. A psychopathology is a syndromal cluster of defining symptoms, a putative cluster or "hump" in the continuum of multivariate behavioral space, for which an explanation is sought. The other, lower levels of analysis considered earlier can (1) provide this explanation; (2) organize symptoms according to which of them are primary, secondary, correlated, and artifactual (Pennington, 1991; Rapin, 1987); (3) redefine syndrome boundaries; (4) clarify comorbidities; and (5) explain developmental continuities and discontinuities in the symptoms of a disorder (Pennington & Ozonoff, 1991). In reviewing each of the specific developmental psychopathologies in Chapters 3–5, I consider each of these four levels and also demonstrate how deeper levels of analysis can clarify issues and problems at more superficial levels.

THE MIND–BODY PROBLEM IN PSYCHOPATHOLOGY

The foregoing discussion brings up an issue that is important to consider in greater detail, namely, the mind–body problem in psychopathology. One key point is that work at different levels is interactive and mutually constraining. Genetic and brain studies cannot proceed without a carefully defined behavioral or neuropsychological phenotype, but discoveries at the genetic or brain level can force revisions in phenotypic definitions or boundaries (Pennington, 1997). We see many examples of this key point in the review of research on specific disorders. Such interactions across levels of analysis help to demonstrate the power of a unified approach. I referred earlier to the latent dualism in much of psychology, which means that mental constructs are frequently studied without any consideration for how they are implemented by the brain. A neuroscientific approach to understanding behavior forces us to give up this latent dualism. To draw out this point, let us consider two recent examples of the neural implementation of psychological constructs. Each of these examples may seem initially surprising; I am arguing that this surprise is diagnostic of our latent dualism.

The first example comes from a neuroimaging study of the treatment of obsessive–compulsive disorder (OCD). Both cognitive-behavioral psychotherapy and medications are known to be effective treatments for OCD. In this study, patients' cerebral glucose metabolism was imaged by means of positron emission tomography (PET) scans before and after treatment (Schwartz, Stoessel, Baxter, Martin, & Phelps, 1996). Some patients were treated with psychotherapy and others with medication. Regardless of the type of treatment, patients who responded favorably to treatment showed metabolic changes in the same brain structures. What may seem surprising about this result is that (1) psychotherapy changes brain metabolism, and (2) psychotherapy and medication affect the same neural systems. But unless we are dualists, we know that psychotherapy has to change brain function to change behavior. Because altered activity of certain brain structures (e.g., the basal ganglia) produces the symptoms of OCD, a successful treatment must alter the activity of these critical brain structures. Undoubtedly, the exact means by which psychotherapy and medication produce this similar effect are different. Medication directly alters neurotransmitter levels and hence the activity of certain structures. Psychotherapy teaches strategies for managing obsessive thoughts and compulsive behaviors. But to work, these strategies must somehow affect brain activation.

The second example concerns how a person's personality influences his or her brain's reaction (as measured by functional magnetic resonance imaging [fMRI]) to positive and negative emotional stimuli (Canli et al., 2001). Previous studies have shown that exposure to emotional stimuli activates parts of the brain that process emotions, such as the amygdala, the frontal cortex, and the anterior cingulate gyrus. This study demonstrated that this activation varies as a function of the subject's personality. Two personality dimensions were considered: extraversion (i.e., the tendency to be sociable and optimistic) and neuroticism (i.e., the tendency to be anxious and socially insecure). Subjects with high extraversion had greater brain responses to positive than to negative pictures, unlike subjects with low extraversion. Subjects with high neuroticism had greater brain responses to negative than to positive pictures. What may be surprising about this example is that a psychological construct (personality) mediates brain activity in response to basic emotional stimuli. But, once again, the psychological construct cannot affect behavior unless it affects brain function.

In summary, these examples make it clear that psychological con-

structs, like expectations or personality, are mediated by the brain, and that altering an individual's psychology (as happens in psychotherapy) changes his or her brain activity. An integrated account of psychopathology must show how both biological and psychological factors influence brain function. The cited examples make it clear that we must take mind–brain relations seriously, and they argue against two possible solutions to the mind–body problem: dualism and reductive materialism. Dualism does not work because it does not provide a way for mind and brain to interact. Reductive materialism does not work because there is not a simple one-to-one relation between psychological states and brain states. But these examples do not tell us which of the remaining solutions to the mind–brain or mind–body problems is correct. Philosophers (e.g., Churchland, 1988) have distinguished several possible solutions besides dualism and reductive materialism, including behaviorism, functionalism, and eliminative materialism. It remains to be seen whether any of these possibilities will work. One interesting point to bear in mind is that none of these solutions to the mind–body problem considers the role of development. Perhaps we cannot solve the mind–body problem without considering how the mind–brain develops. I return to this point later in this chapter, but now discuss issues involved in psychiatric diagnoses.

TO DIAGNOSE OR NOT TO DIAGNOSE?

With few exceptions, the study of developmental psychopathologies begins at the symptom level of analysis. Unlike complex, multifactorial medical disorders such as diabetes or coronary artery disease, whose diagnostic definition depends on pathophysiology, psychopathologies are defined at the symptom level; that is, diagnostic categories for psychopathology are behaviorally defined and purely descriptive. They are based on clinical phenomenology—reports and observations about the behavior and experience of patients. Because understanding of the etiology and pathophysiology of mental illnesses has been so limited, diagnostic reliability could only be achieved at the symptom level. Earlier diagnostic definitions based on presumed underlying psychodynamic mechanisms were found to lack reliability and validity. Hence, moving back to descriptive, behavioral definitions of psychopathologies was, somewhat ironically, a major step *forward* scientifically, one necessitated by the very limited understanding of underlying mechanisms. Obvi-

ously, a long-term goal for a scientific psychiatry is to move beyond description to a nosology based on underlying causal processes. One of the main goals of this book is to review our progress toward reaching that goal and to suggest how diagnostic categories will be reformulated as we learn more about their neuroscience.

So psychiatric diagnoses begin when clinicians notice that certain signs and symptoms occur together in certain patients more often than they should by chance, and categorize this cluster of signs and symptoms as a syndrome. This is a dangerous moment for scientific understanding for several reasons.

First, what counts as a sign or a symptom must be defined relative to an empirically based, normative developmental framework (Achenbach, 1991). Some symptoms that at first glance may appear serious can be quite common at certain developmental stages. Kanner (1945, qtd. in Lapouse & Monk, 1958, p. 1136) commented that "a multitude of early breath holders, nail biters, nose pickers and casual masturbators . . . develop into reasonably happy and well-adjusted adults." Lapouse and Monk (1958) found in a random sample of 6- to 12-year-old children that symptoms of anxiety, overactivity, and irritability were quite common, each affecting between 43% and 49% of the sample according to maternal report. With such base rates, a "syndrome" consisting of the presence of all three kinds of symptoms would be found in about 10% of the sample just by chance alone! Such a syndrome would obviously not require a deeper scientific explanation.

Second, even if this first requirement (that signs and symptoms be rare in a random, same-age sample) were met, documentation of greater than chance clustering of symptoms is rarely formally evaluated and tested (initially at least) in population as opposed to clinic samples. Clinicians' memories may be biased toward remembering the striking co-occurrences of symptoms and not the many counterexamples. Referral biases may produce co-occurrences of signs and symptoms in clinic samples that would not be found in population samples. In other words, some co-occurrences may be an artifact of recall or referral biases and not a reflection of the true state of nature.

Third, naming a syndrome can confer a false sense of validity on the diagnostic category, and, worse yet, the impression that there is an explanation for the deviant behavior. (The idea that a name provides an explanation is called the "nominal fallacy," which is discussed later.)

Fourth, nearly all dimensions of behavior are normally distributed, so where we set cutoffs on this continuum for determining the presence

of a symptom or diagnosis is somewhat arbitrary. At the same time, both epidemiological research and allocation of treatment resources require that we determine who is and who is not a case. So we need to use diagnoses but must remember that they are provisional. For example, a fundamental issue in research on developmental psychopathology is whether the processes that produce individual behavioral differences lying at the unfavorable, extreme end of the distribution are distinct from the processes that produce individual differences across the rest of the distribution (so-called "normal" variations). We later review methods for addressing this important question. For now, it is important to emphasize that although we use current diagnostic categories in research on developmental psychopathology, we are not prejudging this issue.

A closely related issue is the typological thinking implicit in categorical notions of pathology and normality. One of Darwin's important contributions was to replace typological notions with the concept of variation in a population (Mayr, 2000). Most individual differences in behavior are normally distributed. What we call a "psychopathology" is just an extreme region of a multivariate space, with a somewhat arbitrary threshold for extremity. What we call "normality" is just a central tendency in this multivariate space. So very few "normal" individuals would be close to the mean on all the dimensions on this multivariate space. Moreover, the definitions of categories of psychopathology in the *Diagnostic and Statistical Manual of Mental Disorders* (DSM) are not "monothetic"; they do not consist of a brief set of necessary and sufficient features that all members of the category must share. Instead, they are "polythetic," defined by the presence of a critical number of features in a longer list, few of which are necessary, and none of which are sufficient for the diagnosis (Blashfield & Livelsey, 1999). As a result, members of a given diagnostic category will vary in which features they possess, with some pairs of individuals with the same diagnosis even having nonoverlapping features. For example, the diagnostic category of conduct disorder provides one of the more extreme examples of this situation. There are 15 symptoms of conduct disorder, any three of which are sufficient for the diagnosis. Therefore, five different children could each qualify for this diagnosis without sharing a single symptom. So neither "normal" individuals nor individuals with a given psychopathology are types.

Fifth, what counts as a sign or symptom depends in part on cultural and subcultural values. Culture can undoubtedly affect the percep-

tion, manifestation, and treatment of psychopathology. For an epidemiologist, a case is someone in need of intervention (Costello & Angold, 1995), but the determination of who needs intervention occurs in a cultural context, which, unfortunately, includes the level of resources available for interventions. In the context of developmental psychopathology, a case is someone who is not meeting developmental goals. The definition of developmental goals is in turn based on empirical studies of development. While there are undoubtedly some human universals in developmental goals, there will still be cultural variation in these goals and in how much deviation from them is seen as needing intervention.

Another concern about diagnoses has to do with labeling. For some mental health practitioners, diagnoses are aversive because they do not capture the individuality of the patient's problems. Robin Morris (1984) has said, "Every child is like all other children, like some other children, and like no other children"; that is, some characteristics are species-typical, others are typical of groups within the species, and still others are unique to individuals. It is important for diagnosticians and therapists to have a good handle on which characteristics fall into which category. Some patients have symptoms that they feel are unique to them but are in fact virtually species-typical. Other symptoms are fairly specific to a particular diagnosis, and still others are unique to a given patient. Although a good clinician must be aware and make use of a patient's unique attributes, scientific progress in understanding and treating psychopathology depends on there being "middle-level" variation—differentiating characteristics of groups within our species. If not, clinical work is reduced either just to treating the problems in living that everyone faces or to recreating the field for each unique individual. On the one hand, we say there are no psychopathologies because everyone is "in the same boat." On the other hand, we say there are no psychopathologies because everyone is different. A science of psychopathology is not tenable at either extreme. Although there is much confusion and many limitations in the current state of knowledge about psychopathologies in children, this state of affairs hardly means that a science of developmental psychopathology is impossible.

Another potential criticism of this approach to diagnosis is that it is based on the "medical model," which is assumed to posit a single model of physical causality for all behavioral disorders. I have already discussed this issue in the previous section. Moreover, as Meehl (1973) has pointed out, there is no single medical model. Recent medical research

on disorders such as heart disease espouses a multifactorial causal model and acknowledges the contribution of genetic, psychological, and cultural factors to etiology. Thus, the medical model that has been castigated by social scientists may increasingly be a straw man. Moreover, our search for the causes of psychopathologies should be just as broad as the search for causes of "medical" disorders and not be hampered by an a priori assumption of what kinds of causes will prove important.

Finally, it is important to remember that the patient has the diagnosis rather than the diagnosis having the patient (Achenbach, 1982); that is, most diagnoses do not provide an explanation for every aspect of the patient's being. A related point is that nosologies classify disorders, not people. Thus, it is important to use "people-friendly" language in talking about diagnoses. Saying "a person with autism" has a distinctly different connotation than saying "an autistic."

In summary, it is very important to remember that behaviorally defined diagnoses are provisional, hypothetical constructs that must be validated. As scientific knowledge accumulates, some currently separate diagnoses will be lumped together, some single diagnoses will be split into two separate diagnoses, and some diagnoses may even disappear altogether. Eventually, the current descriptive, behaviorally defined nosology will be replaced by one that defines psychopathologies in terms of empirically validated causal mechanisms. We next consider how psychiatric diagnoses are validated.

ESTABLISHING THE VALIDITY OF DIAGNOSES

A set of diagnoses, such as those in DSM-IV-TR, constitute a nosology, which is just a classification system or taxonomy for diagnoses. This section considers the issues involved in validating both a nosological scheme and individual syndromes within a nosology. These issues have been well articulated by Fletcher (1985) and Rapin (1987), and I draw on their discussions.

The basic goals of a nosology are to identify clusters of symptoms that reliably co-occur and identify groups of patients that are homogeneous at the level of etiology, pathogenesis, or treatment. These two goals concern internal and external validity, respectively.

Internal validity might also be termed "internal consistency" or reliability. Fletcher (1985) lists five criteria for the internal validity of a

nosology: (1) coverage or number of patients classified, (2) homogeneity of the diagnoses, (3) reliability of the classification procedures, (4) replicability across techniques, and (5) replication in other samples. Clearly, a sample- or test-specific diagnosis would necessarily lack reliability. In the last two decades, considerable progress has been made in descriptive psychiatry, and we now have nosologies for child and adult psychopathologies that satisfy these criteria for internal validity.

External validity essentially concerns the explanatory significance of a diagnosis. A subtype may be reliable in terms of the variables used to define it but not have a distinctive relation to any external variables of interest. Fletcher (1985) lists three possible criteria for external validity: (1) differential response to treatment, (2) clinical meaningfulness, and (3) differential relation to processing measures independent of those used to define the diagnosis, such as neuropsychological measures. To this list, we would add (4) differential etiology, (5) differential pathogenesis, and (6) differential prognosis or developmental course.

Fletcher (1985) emphasizes that the search for external validity is essentially a hypothesis-generating and testing affair, much like the search for construct validity (Cronbach & Meehl, 1955). A valid syndrome is a fruitful hypothesis about how to "parse" the domains of both disordered and normal behavior, and the various levels of the underlying causes of behavior. If a syndrome is valid, then it will satisfy tests of both convergent and discriminant validity across levels of analysis: etiology, brain mechanisms, neuropsychology, and symptoms. The ultimate goal of syndrome analysis is to discover a meaningful causal chain across these different levels of analysis. We would like to know which etiologies specifically cause the diagnosis in question, what aspects of brain development they perturb, what deficit in neuropsychological processes this leads to, how this underlying neuropsychological deficit leads to the primary (or core) and secondary symptoms of the disorder, how the symptoms and underlying deficit change with development, and how all of this information helps explain the response to treatment. Thus, a valid syndrome is a construct below the level of observable behaviors or symptoms that provides a meaningful explanation of why certain symptoms co-occur in different patterns across development, and why some treatments are efficacious and others are not.

The concepts of convergent and discriminant validity are closely related to the concept of external validity. We might expect that an ideal nosology would have a complete and unique set of external, converging

validators for each of its different syndromes, thereby guaranteeing discriminant validity. However, it has become increasingly clear that complete specificity is not found for psychopathologies. Basically, this lack of specificity is a consequence of the fact that these are complex disorders, as discussed earlier, that lack single causes. So risk and protective factors may be specific to a given disorder, shared by a few disorders, or shared by all disorders (generic). If we cross this distinction with the fact that risk and protective factors are either genetic or environmental, we can see that there are six possible types of risk (and protective) factors (Table 1). On the genetic side, some genetic variations are specific to a single syndrome (e.g., fragile X syndrome), some are shared by a few disorders (e.g., an allele of the serotonin transporter gene appears to be a risk factor for both anxiety and depression), and some turn out to be generic. The same distinctions apply to environmental risk factors: Exposure to a frightening stimulus (e.g., a snake) is an environmental risk event that is specific to a single disorder; stress increases the risk for several disorders; and low socioeconomic status increases the risk for most disorders.

It is more difficult to provide examples of all six kinds of protective factors. Besides the normal alleles of genes that have known risk alleles, we do not know of other specific genes that protect against psychopathology, but it is likely that such genes exist. On the environmental side, we know the most about generic protective factors: good pre- and postnatal care, good nutrition, and good parenting. Again, it is likely that there are more specific protective factors. Identifying both genetic and environmental specific protective factors should have a major impact on the prevention of psychopathology.

A specific psychopathology most likely results from a particular combination of all six kinds of risk factors, not from factors that are all specific to that particular disorder. Similarly, we should expect some overlap in the brain mechanisms underlying different psychopathologies and in their neuropsychology. This state of affairs means that we face the difficult challenge of explaining the overlapping developmental pathways that lead from partly shared risk factors and brain mechanisms to different disorders. The discussion of specific psychopathologies in Chapters 3–5 gives examples of these overlaps at different levels of analysis. This lack of specificity also means that we should expect comorbidity or co-occurrence of developmental psychopathologies, which is indeed the case. I next briefly discuss how to analyze comorbidity.

TESTING THE BASIS OF COMORBIDITY

Explanations for comorbidity have been discussed by Caron and Rutter (1991) and Faraone, Tsuang, and Tsuang (1999b). Essentially, two disorders may co-occur for artifactual reasons or because there is a genuine causal relation between them. Artifactual reasons for comorbidity need to be ruled out before undertaking the usually more arduous process of testing causal hypotheses. Possible artifactual reasons include referral biases, rater biases, and definitional overlap. Comorbidity is more likely to occur in a clinical sample than in a population sample because individuals with more disorders are more likely to seek help (Berkson, 1946). Therefore, some comorbidities observed in clinical samples are simply a product of Berkson's bias. Rater biases may produce artifacts, especially since most psychopathologies are defined by self- or other-report of symptoms. A rater who is very concerned about one set of symptoms may be more likely to endorse other symptoms (a "halo" effect), artifactually producing a comorbidity. Finally, the actual items that define psychopathologies overlap to some extent across disorders. This definitional overlap would also artifactually produce the phenomenon of comorbidity.

Some hypotheses to explain a nonartifactual comorbidity include the following: (1) The two disorders share a risk factor that is consequently not specific to either disorder; (2) one disorder causes at least the symptoms of the second disorder; (3) there is an etiological subtype in which a shared risk factor produces both disorders, but other cases of each disorder do not share risk factors; and (4) there is nonrandom mating such that individuals with transmissible risk factors (either ge-

TABLE 1. Types of Risk Factors

	Genetic	Environmental
Specific	Fragile X mutation	Phobic exposure (e.g., to a snake)
Shared	Allele of serotonin transporter gene	Stress
Generic	?	Low socioeconomic status

netic or environmental) for one disorder are more likely to have children with individuals with transmissible risk factors for the other disorder. In this last case, there would not be a direct causal relation between the two disorders in a comorbid offspring. A more detailed and quantitative treatment of models of comorbidity is contained in Neale and Kendler (1995).

The main methods for testing these four hypotheses include family, twin, and molecular approaches, which are discussed in more detail later. Neuropsychological methods are also helpful, especially for testing the second hypothesis (e.g., Pennington, Grossier, & Welsh, 1993). A family design allows one to evaluate whether there is nonrandom mating and to test whether the two disorders are transmitted independently. To support the nonrandom mating hypothesis, both of these conditions must be satisfied. The presence of a shared familial risk factor, postulated by both hypotheses 1 and 3, will be reflected in nonindependent transmission of the two disorders in families. Specifically, to support hypothesis 1, relatives of probands with only one disorder should have increased rates of *both* that disorder and the other disorder. To support hypothesis 3, in contrast, only relatives of probands with both disorders should be at increased risk for both disorders. Relatives of probands with only one disorder should exhibit increased rates of only that disorder, not the other disorder. However, if the subtype is infrequent, large samples may be needed to test its influence on transmission. Bivariate twin analyses, described later, can determine if the shared familial factor is genetic. If it is, bivariate molecular methods can be used to determine which genes act pleiotropically to produce the two disorders (e.g., Willcutt et al., in press).

In summary, analyzing comorbidity is a crucial task for understanding the development of psychopathology. As we will see, nonartifactual comorbidities appear to be the rule rather than the exception in this field, and evidence is accumulating for genetic risk factors that exert a causal effect on more than one disorder (i.e., pleiotropy). For example, there is evidence for genetic factors that increase the risk for both anxiety and depression, and we have just found that the dyslexia locus on the short arm of chromosome 6 also influences attention-deficit/hyperactivity disorder (ADHD) (Willcutt et al., in press). Such results could lead to a shift in syndrome boundaries and at the very least, influence how we define the phenotype in molecular studies.

WHY A DEVELOPMENTAL APPROACH?

One of the core axioms of this book is that we cannot achieve a complete scientific understanding of psychopathology without knowing how it develops. So the reader may rightly ask, "Why not?" One might imagine that with regard to schizophrenia, if we had total knowledge of the various causal risk factors, as well as total knowledge of the adult brain phenotype, then we would understand the disease. However, the only way to understand how to get from the risk factors to the adult brain and behavioral phenotype is to see how these risk factors change brain *development*. Both brain and behavioral development are very complex interactive processes, so the significance of any risk factor can only be understood by considering its timing and how it interacts with the developmental process.

A developmental approach is necessary at each of the four levels of analysis discussed earlier. At the *etiological* level, the timing of both expression of a risk allele and environmental risk factors will influence their effect on behavioral outcomes. As discussed later in the Neurobiology section of this chapter, the timing of early visual deprivation (an environmental risk factor) critically determines whether it will disrupt the development of an important aspect of visual function, stereopsis (i.e., 3D vision). It is well known that even dominant, single-gene disorders, such as tuberous sclerosis, vary widely across individuals in their phenotypic effects, from a few barely detectable skin lesions to a profound developmental disability such as autism. Why this is the case is poorly understood, but an explanation will very likely depend on a developmental, epigenetic account of how the expression of the tuberous sclerosis gene interacts with the rest of the individual's genome and environmental risk factors.

At the *neurobiological* level, it is well known that the timing of prenatal exposure to teratogens (e.g., alcohol, cocaine, and nicotine) and postnatal exposure to toxins (e.g., lead) determines their effects on brain development. The same appears to be true for social risk factors, such as traumatic stress and deprivation, although the brain effects are not as well worked out. A fairly striking example of this phenomenon (and of how the mind can affect the body) is provided by psychosocial dwarfism, in which young children exposed to chronic severe social stress (e.g., abuse or loss) fail to grow (Vazquez & Lopez, 2001). The underlying mechanism involves the stress hormone, cortisol, which decreases levels of growth hormone. The effect depends on the timing and

duration of the social stress. Older children exposed to similar social stress do not exhibit dwarfism. Younger children with psychosocial dwarfism show rapid catch-up growth if removed from the stressful environment but are nonetheless at risk for shortness, cognitive changes, and later depression and anxiety. These long-term effects are more severe if the child remains in the stressful environment past age 5.

At the level of *neuropsychology*, a developmental approach is important for knowing when different neuropsychological functions develop, how they are mediated by brain structures, and how they can be appropriately measured. Localization of brain functions is not innate or static, but changes with both development and the particular environment to which an individual is exposed. For example, in a congenitally deaf individual who has learned a manual sign language, auditory language cortex subserves visual processing of these manual signs (Neville, 1990). In neural network models of cognitive functions, the effects of damage vary considerably depending on whether the damage occurs before or after the network is trained (Thomas & Karmiloff-Smith, 2002). So damage interacts with the developmental process itself.

These examples return us to a claim I raised earlier, namely, that solving the mind–body problem will depend on a developmental approach. If localization of brain functions varies with development, and if the function of artificial neural networks varies with their training and development, then it seems unlikely that we can fully understand brain–behavior relations in the mature human or animal without taking a developmental approach.

Finally, at the level of *symptoms*, a developmental approach is needed because the manifestation of a given psychopathology changes depending on the developmental stage of the individual. The young child with autism who does not engage in pretend play becomes the isolated adult who is obsessed with computers. The overactive toddler with ADHD becomes the adult with poor planning skills.

WHY A SCIENCE OF PSYCHOPATHOLOGY IS DIFFICULT

The preceding sections illustrate some of the conceptual and empirical challenges that a science of psychopathology must meet. In this section, I focus on two main reasons why a science of psychopathology is difficult: (1) Such a science has the extra task of explaining why there are

individual differences in a given psychological function, and (2) the study of individual human differences is necessarily quasi-experimental.

Before we can understand individual differences in a given psychological domain (e.g., emotion regulation), we have to have a fairly mature model of normal development in that domain. But understanding individual differences then imposes an extra task, because it requires us to focus on etiology. Our model of normal development would be a neurocomputational model that might also specify the brain systems involved and generally the kinds of environmental inputs needed, but it would not be a detailed model of the *etiology* of normal development in that domain. It would not attempt to specify genes, specific environmental inputs, and epigenetic interactions that lead to a developing brain capable of those kinds of neurocomputations. However, understanding individual differences in a psychological domain requires us to focus on etiology to a much greater extent.

The second source of difficulty is the quasi-experimental nature of the designs that can be used to study human individual differences. The most commonly used method for investigating developmental psychopathologies is the case–control design, in which a group with a given psychopathology is compared to a group lacking that psychopathology on some variable of interest. The important point is that case–control designs are quasi-experimental (Campbell & Stanley, 1966) because the manipulation of group does not involve random assignment. If we could randomly assign diagnoses to individuals, then we could be sure that presence versus absence of the diagnosis is the only factor that distinguishes cases from controls. If we then found that the two groups differed on the variable of interest, then we could be sure of a true causal relation between that variable and diagnosis. (Notice, however, that we would still not know the direction of effects.) Instead, the "manipulation" of group in a case–control design is not experimental, so it is unlikely that cases and controls differ only with respect to diagnosis. To attempt to compensate for this fundamental problem, psychopathology researchers should control for other differences between the groups (i.e., the differences of which they are aware), either by matching other variables or covarying such variables. However, these precautions can never completely compensate for the lack of random assignment. No matter how careful the matching or how extensive the covarying, it is always possible that the finding of a significant group difference on some variable in a case–control design is due to an undetected differ-

ence—some other variable that is confounded with group membership. So we can never be sure from the results of a case–control design alone that we have found a true causal relation between diagnosis and some variable.

Sometimes the limitations of case–control designs are forgotten when other, more biological levels of analysis are examined, such as brain structure or function, or alleles of a genetic locus. It is tempting to think that if differences are found at such levels, then they must be primary and part of the causal pathway leading to the disorder. So, if case–control designs find differences at these levels, then we may be tempted to make stronger causal inference than if only cognition or behavior were being examined. But this is a logical error. A brain difference found in a case–control study is, at best, a true correlate of the disorder, one that may only be secondary, since having a developmental disorder alters environmental input to the developing brain. (At worst, the finding is an artifact of some uncontrolled difference between cases and controls.) A difference in allele frequencies at a given genetic locus between cases and controls is unlikely to be secondary to having the disorder (because experience does not change DNA sequences, unless the experience includes exposure to mutagens, such as ionizing radiation), but it could still be due to an uncontrolled difference between cases and controls, such as genetic background. For example, an initial finding that severe alcoholism is associated with a particular allele of the gene for the dopamine 2 (D2) receptor was found to be likely due to an artifact of ethnic stratification differences between case and control groups (Kidd, 1993). I return to this issue in the review of brain and genetic association studies of specific disorders in Chapters 3–5.

So understanding psychopathology is empirically difficult. It is also conceptually difficult, partly because of fallacies and conceptual errors to which we readily succumb when trying to explain abnormal behavior. Some of these fallacies are considered in the next section.

WHAT COUNTS AS AN EXPLANATION

As the foregoing discussion makes clear, the difficult goal of a neuroscientific approach is to provide a comprehensive explanation of psychopathologies. In this section, we consider how a comprehensive neuroscientific explanation relates to other ways psychopathologies have been explained. A crucial issue in such a comprehensive explana-

tion is how to integrate different levels of explanation, including both top-down and bottom-up accounts of psychopathology. A neuroscientific explanation begins with etiological factors that are far removed from the patient's experience and then works forward through the developmental process, and eventually to the symptoms that define the disorders. Traditional psychological approaches work in the opposite direction. They begin with the patient's experience and work backwards, mainly seeking psychological mechanisms that explain the patient's symptoms. Often these mechanisms are beliefs and expectations, perhaps unconscious, that affect the patient's experience and behavior. At first glance, these two ways of explaining psychopathology seem quite different, even incompatible, but since the evidence demonstrates that both bottom-up and top-down factors influence the development of psychopathology, both must be included in a comprehensive account.

Another way of making this same point is to say that we, as humans, are predisposed to explain others' behavior as rational and intentional, given their beliefs and desires. We grant personhood to other people and explain their behavior according to an implicit theory. So we can call such explanations "personal" in this sense. Traditional top-down psychological explanations of abnormal behavior, whether psychoanalytic or cognitive-behavioral, are essentially such personal explanations, but with a wider context. What seems irrational becomes rational given more knowledge of a person's early history, particular learning experiences, or cognitive distortions.

In contrast, a neuroscientific approach to psychopathology makes a more radical claim, namely, that some of the explanation for irrational behavior is "subpersonal." In other words, some of the causes of a given psychopathology lie completely outside of individuals' beliefs and desires, and are completely inaccessible to their phenomenology, no matter how many years of therapy they undertake. Just as the causes of diabetes, heart disease, multiple sclerosis, and Alzheimer's disease are subpersonal, so too are some of the causes of depression, schizophrenia, and ADHD.

A further complication is that scientific explanations of psychopathology, whether of the bottom-up or top-down variety, must contend with potential errors provided by an explanatory framework we all share as humans, namely, folk psychology. Humans inevitably seek explanations of human behavior, and everyday folk psychology provides a stock of ready-made explanations that we use almost unconsciously.

Psychopathology is so puzzling and painful that it is nearly inevitable that patients, relatives, and even clinicians resort to attempts to construct folk psychological explanations. Obviously, psychologists also seek explanations of human behavior, but in doing so, they must first clear their minds of such ready-made explanations. In addition, the public's stigmatizing reaction to psychopathology occurs in part because individuals with psychopathology often violate everyday folk psychology, which holds that a person's behavior is rational in the sense that it is goal-directed and thus understandable in terms of that person's current beliefs and desires. In psychopathology, beliefs and desires may be unusual or even bizarre, and the capacity for planning and executing behavior in accordance with goals may be impaired. One of the important roles for psychologists in the 21st century will be to educate the public about our growing scientific understanding of how psychopathology develops, so as to reduce the stigma currently associated with mental illness.

One potential error to avoid in thinking about the development of psychopathology is the assumption that the beliefs and desires of patients will usually provide a complete explanation of their abnormal behavior. Although belief–desire psychology works very well in everyday social interactions, using it to "explain" psychopathological behavioral borders is often not very informative. For instance, consider the following statements: "Mary tried to kill herself because she believed life was not worth living"; "John tried to kill the President because he desired Jodie Foster's attention." In the first statement, suicidal behavior characteristic of severe depression is explained by a belief. In the second, homicidal behavior in an individual with schizophrenia is explained by a desire. Neither explanation is very satisfactory. What we would really like to know is why these individuals came to have an unusual belief or desire, and why they were willing to act on them. But to answer these questions, we need to move beyond beliefs and desires. So belief–desire psychology has a normative or species-typical aspect; we accept it as a good explanation of what an ordinary person would do in a given situation. Of course, belief–desire psychology does not really provide a scientific explanation of species-typical human behavior. Such an explanation will require evolutionary theory, neuroscience, and cultural anthropology, among other disciplines.

So relying on folk psychology to explain psychopathology commits what I call the "phenomenological fallacy," which is the belief that an explanation for abnormal behavior may be found through close atten-

tion to patients' experience of the disorder and their report of symptoms. Part of this report includes patients' usually inaccurate hypotheses about why they are having these symptoms, since people inevitably construct explanations, rationalizations, and attributions for their behavior. Although, in some cases, such erroneous cognitive constructs that arise in *response* to a disorder such as depression or posttraumatic stress disorder serve to perpetuate the disorder, as will be discussed later, in most cases, the actual cause of a psychopathology lies outside the phenomenology of a patient; therefore, it is a fallacy to look for the cause in the patient's phenomenology.

At the same time, partly subpersonal causation for psychopathology raises complex issues for how both practitioners and patients think of the disorder being treated and relate to each other. While parts of folk psychology are undoubtedly misleading about the causes of psychopathology, effective treatment must begin with a patient's and family's understanding of the illness at hand. These issues are captured very movingly in Luhrmann's (2000) *Of Two Minds*, an ethnographic study of the current split between biological and psychodynamic approaches in psychiatric training. Just because some of the causes of psychopathology are subpersonal does not mean that psychiatric treatment should be reduced to dispensing medications. The patient as a person needs to be fully engaged in his or her treatment.

In addition, descriptive psychiatry has undoubtedly made considerable progress by paying close attention to the phenomenology of disorders, and by developing interviews and rating systems to classify disorders reliably. But, as I said in a previous section, this can only be the first step toward a scientific understanding of psychopathology; eventually, our classification of psychopathologies will be based on underlying causal mechanisms, not on surface behaviors. Moreover, what counts as a discrete disorder at the phenomenological level may not be similarly discrete at other levels of analysis. Disorders with different phenomenologies, such as anxiety and depression, appear to derive from common genetic risk factors, and disorders with the same phenomenology may have different etiologies (we say that they are "phenocopies" of each other).

As mentioned earlier, having a reliable descriptive taxonomy for psychopathologies can lead to the illusion that having a name for a disorder provides an explanation. This illusion, called the *nominal fallacy*, is classically illustrated by a character in Molière's *Imaginary Invalid*, who explained that a sleeping potion worked because it had "dorma-

tive" powers. As this example makes clear, the nominal fallacy works best if the name that provides the explanation is rather obscure. Although making an accurate diagnosis can be quite helpful in many ways, clinicians should guard against assuming that just naming the disorder provides an explanation for it.

A final fallacy to consider is the fallacy of *reification*, which means inappropriately turning an abstract notion into a concrete thing. Some abstract notions may have no physical basis at all (e.g., deities such as Zeus), while others may be concretized in the wrong fashion. For example, in the Musée des Augustins in Toulouse, there is a statue by Eugene Thivier (1845–1920) titled *La Cauchemar*, which means "the nightmare." The statue portrays a young woman in tormented sleep, with a griffin-like monster perched on her hip. The statue beautifully captures the agony of the nightmares that we all experience, and that are much more common and intense in depression, anxiety, and other psychopathologies. But it is obviously an intended reification, a metaphor in stone, not because it asserts that nightmares have a concrete physical basis, which neuroscientists are discovering in the brain, but because it asserts the wrong physical basis for nightmares (monsters).

The possibilities for reification are more subtle in the case of diagnostic constructs. One error is to forget that the current diagnostic constructs are provisional hypotheses in search of validation and to assume that they too are "carved in stone." The thing that we roughly point to when using a term such as "schizophrenia" undoubtedly exists, but it may not exist in the form currently envisioned by our diagnostic constructs, which in turn select which symptoms count in the definition of schizophrenia. As research progresses, the definition and understanding of schizophrenia will undoubtedly change.

To make this point more clearly, let us consider two historical examples. In the 19th century many inmates of asylums had what was called "general paresis," a mental disorder with a progressive, deteriorating course ending in death. General paresis was a provisional and imprecise label for what was eventually found to be tertiary syphilis. Once this was understood, the key defining characteristic of this disorder became the presence of infection with the spirochete bacteria.

An earlier and more obvious example of the reification of psychopathology is provided by the story of the Gadarene swine in the New Testament. Jesus encounters a man "possessed by demons." After talking with these demons (perhaps the man's thought-disordered verbalizations or his multiple personalities), Jesus cast them out of the man

and into an innocent herd of swine, which promptly rushed headlong into a nearby pond and drowned themselves.

In both these cases, the psychopathology is a real thing, but the label used for it is only a metaphor with inaccurate implications. We do not have to search so far back in history to find many examples of similar errors in thinking about developmental psychopathology: "Refrigerator" mothers who fail to deliver their infants from a normal stage of autistic development as an explanation for autism, and seeing things "backward" as an explanation for dyslexia, both come to mind. These reifications are inaccurate and sometimes implicit hypotheses about the nature of a psychopathology. They are harmful in that they may strongly influence diagnosis, treatment, and research. Our descriptive taxonomies for psychopathologies are not theory-neutral; instead, symptoms used to define a disorder depend in part on current conceptions of the disorder. So while our current understanding of psychopathology cannot be theory-neutral, we should always base it very explicitly on a network of hypotheses, some of which have better empirical support than others.

As discussed earlier, another reification error is to assume that diagnoses are discrete, that a sharp boundary exists between psychopathology and normal function. But it could equally well be the case that disorders are just extreme points on a normal distribution, whose variance is determined by a set of risk and protective factors acting on a set of brain systems that mediate both normal and abnormal behavior in that domain. In fact, a comprehensive developmental theory requires a broad-enough set of processes to encompass both normal and abnormal development. If a current theory of development does not explain a given psychopathology, or if a theory of a psychopathology does not explain how it develops, then more work is needed.

A third reification error is to assume that diagnoses are completely separate from each other, with each having totally distinct etiologies, brain mechanisms, and underlying psychological processes. This view of diagnoses would be correct if there were single, specific causes for each diagnosis at each level of analysis. But the field has searched in vain for such single causes of psychopathology. At the genetic level, the OGOD (one gene, one disorder) hypothesis (Skuse, 1997) has been rejected. At the brain level, attempts to explain disorders in terms of single neurotransmitters or single brain regions have failed. At the neuropsychological level, most disorders involve deficits in multiple neuropsychological functions. These various single-cause hypotheses involve

reification in that they localize the cause for a complex behavioral phenomenon in a single factor (or level of analysis), instead of acknowledging that the phenomenon is an emergent property of the interactions of factors and levels in development.

As we will see, emerging evidence indicates overlap between disorders, such as anxiety and depression, at all levels of analysis, so what makes two disorders differ may be much more subtle than previously thought. As discussed earlier, disorders are likely regions with fuzzy boundaries in a continuous multivariate space. What will likely distinguish disorders is the weighting of different risk factors, not a distinct set of risk factors, and the different epigenetic and developmental interactions that result from that particular weighting. Small differences in initial conditions may lead to large differences in outcome given the nonlinearity of development.

The goals in this chapter have been to present the basic issues that must be addressed in a scientific understanding of developmental psychopathology and to expose those errors in thinking that almost inevitably occur when we confront the complex and poorly understood phenomenon of psychopathology. Scientific understanding of this phenomenon is difficult because it lies "close to home," affecting either ourselves or those we care about, and because it is so inextricably intertwined with how we think about our own human nature. These issues and errors are elaborated in Chapters 3–5, which discuss specific psychopathologies. I now turn to methods of syndrome analysis, the topic of Chapter 2.