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From Helplessness To Hope: The Seminal Career Of Martin Seligman

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✦ CHAPTER 1 ✦

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THIS BOOK explores a specific field of psychological research, but it also celebrates the profound contributions to this field of Martin E. P. Seligman. Therefore, the book blends the history of this research enterprise and Seligman's own intellectual history. This chapter reviews the modest origins of the phenomenon of "learned helplessness" in the animal laboratory, its extensions to human beings (especially those displaying dramatic failures of adaptation), and its eventual emergence as "learned optimism." The remainder of the book documents two major themes. First, the insights arising out of research on learned helplessness have been extended to almost every domain of modern psychology. And second, Seligman has played a significant role in almost all of these extensions. In fact, this book makes a fitting tribute to the man whose fingerprints appear on every chapter.

Although the research discussed in this book focuses on optimism and hope, the research story does not begin there. Rather, it begins with the opposite end of the pole—helplessness. As will become apparent, Seligman is now a strong proponent of the development of a positive psychology, but the historic, intellectual seeds of the view that underlies this new emphasis are very much in negative psychology. The critical first step in thinking that made this development possible was an appreciation of the negative consequences of the inability to control important environmental events. It is this inability that produces the learned helplessness phenomenon.

The history of research on learned helplessness and learned optimism as well as Seligman's own involvement in these areas reflects a large element of chance. Furthermore, the development of research in this area also illustrates two other important lessons in how science actually proceeds. First, it is often difficult to predict at the outset where research will lead. Work on learned helplessness began in the animal laboratory and for several years was directed at deep theoretical issues in the psychology of learning and not at depression, academic achievement, and other significant human phenomena. And second, the history of learned helplessness research demonstrates the continuity between basic and applied research in the way that it has moved effortlessly between fundamental issues in learning, cognition, and motivation on the one hand, and attempts to deal with problems of human adaptation and obstacles to the achievement of human potential on the other.

LEARNED HELPLESSNESS IN ANIMALS

Early Experiments

Learned helplessness research, and Seligman's own work, began in the mid-1960s in the animal learning laboratory of Richard L. Solomon at the University of Pennsylvania. At that time, the focus in the Solomon laboratory was on the rigorous testing of a new theory designed to explain the occurrence of avoidance learning. In avoidance learning, some warning signal (e.g., a light) precedes the onset of an aversive stimulus (e.g., a shock) by a short period of time (e.g., ten seconds). A response by the animal (e.g., jumping a hurdle) after the aversive event has started enables it to escape the aversive stimulus. And a response during the warning signal enables it to *avoid* the aversive event.

Animals readily learn to make avoidance responses in such experiments, and this fact created a significant theoretical puzzle. According to the dominant theories of the day, for a response to be learned, some event had to occur that reinforced it. The reinforcer for escape responses was obvious—termination of shock. But what was the reinforcer for avoidance responses? This also seems obvious—the absence of shock. But not so fast. If the absence of shock is a reinforcer, then why doesn't

it reinforce everything an animal does? After all, before the experiment, the animal went through its life seeking food, grooming, sleeping, exploring, and each of these behaviors was accompanied by the absence of shock. While this sort of account is clearly absurd, it demonstrates why calling the absence of shock a reinforcer is problematic. If the absence of shock constitutes a reinforcer in the avoidance experiment, it must be because the shock is otherwise *expected*. This account makes obvious sense. The animal expects something (shock) to happen if it doesn't respond. So it responds, thereby preventing the "expected" event. It is thus the absence of this expected aversive event that is the reinforcer of avoidance.

For researchers and theorists of the day, dominated as they were by the principles of behaviorism, the problem with this account was that a major aim of their enterprise was to explain behavior without having to appeal to mental entities like "expectations." Solomon and his students typified this enterprise and developed a theory—*two-process theory*—to do that job for avoidance learning. The theory argued that fear becomes classically conditioned (Process 1) to the warning signal on the early trials before the animal has learned to jump the hurdle. The warning signal and the shock are paired together in Pavlovian fashion on those trials. Avoidance responses do not occur until later trials, and when they do, they *escape* the fear-provoking warning signal (Process 2) and are followed by a rapid reduction in conditioned fear. The theory thus maintained that the animal does not really learn to "avoid" the aversive event. Rather, the so-called "avoidance" response is really an escape response; the animal, motivated by conditioned fear produced by the warning signal, escapes this fear.

Solomon and his students attempted to test this explanation of avoidance learning with what was called a "transfer of control" experiment, in which the intention was to conduct straightforward classical conditioning of fear by pairing a neutral stimulus (e.g., a light) with an aversive stimulus. Then, in a different environment, avoidance learning would be conducted using some other stimulus (e.g., a tone) as the warning signal. After the avoidance response was well established and the animal was responding reliably to the tone, the crucial third phase of the experiment would be conducted. The light would be turned on

during the avoidance procedure, and the question was whether the animal would now perform the avoidance response, even though the light had never been used as a warning signal in the avoidance apparatus.

This was a key prediction made by two-process theory: If "avoidance" responding was really "escape" from a fear-provoking warning signal, then any time you presented such a signal, it ought to trigger the already learned avoidance response. However, when Leaf and Overmier, graduate students in the Solomon laboratory, set out to test the prediction, they had difficulty in conducting the experiment. The problem was that when, after classical conditioning of fear had been established, the animals were exposed to an avoidance procedure, they often failed to learn to avoid shock. Indeed, they often failed even to learn to escape shock (Overmier, 1968; Overmier and Leaf, 1965). This was quite surprising given that such tasks are typically learned rapidly.

Because having learned to avoid shock was a precondition for testing this key prediction, the prediction could not be tested. The solution to the problem, as it turned out, was to reverse phases 1 and 2 and conduct the avoidance training first and the classical conditioning second. This was indeed done, and the testing of two-process theory proceeded successfully. It seemed that somehow the prior occurrence of classical conditioning interfered with the learning of the instrumental escape and avoidance responses.

For researchers committed to rigorous testing of two-process theory, this peculiar, accidentally discovered order effect was largely a methodological nuisance. However, another graduate student in the Solomon laboratory (Seligman), and a graduate student in Henry Gleitman's laboratory, which was right next door (Maier), thought that the "nuisance" deserved study in its own right and might even be more interesting than the theory that was being tested. The question was what was it about the shock animals received during classical conditioning that interfered with subsequent learning?

It is the very defining feature of classical conditioning that the behavior of the subject has no impact on the occurrence of the unconditioned stimulus (the UCS) or its properties. Could this have been important? This question led to what is arguably the single most important experiment in the entire literature concerning helplessness and

optimism. Overmier and Seligman (1967) first gave animals a series of either escapable shocks—shocks that could be terminated by a response—or exactly matched but inescapable shocks, as in classical conditioning. The animals were later tested for escape and avoidance learning in a different apparatus. It turned out that the animals that had initially received escapable shock learned normally, while those that had initially received physically identical inescapable shocks failed to learn. This demonstration was quickly followed by experiments in which it was found that an experience of escapable shock "immunized" animals so that a later exposure to inescapable shock was without effect on later learning (Seligman and Maier, 1967), and ideas about control, helplessness, and optimism were born.

Learned Helplessness Theory

Why should inescapable shock interfere with later learning? The process of attempting to answer this question became a crossroads for Seligman and Maier. Explanatory concepts existed within the behaviorist theories that dominated the 1960s that could provide an "explanation" (e.g., Bracewell and Black, 1974). However, the explanation seemed contorted and inelegant, and seemed to trivialize the phenomenon. If one added to this a growing disenchantment with the pinched behaviorist theories of the time, as well as two personalities who wanted to "push the envelope," it was over-determined that a new theory would be developed.

Seligman and Maier reasoned that it must be something about what the animal *learned* about inescapable shock that was critical, rather than the shock per se, because inescapable and escapable shocks were physically identical, yet had drastically different effects. What could the animals be learning? Seligman and Maier together pondered this seemingly easy-to-answer question for months, consulted scholars in various disciplines, and could not come up with a meaningful answer within the context of learning theory. Presumably, the key was that the shock was inescapable. But what did that really mean? How can that fact about shock be learned? This question obviously does not seem difficult now, and probably would not have seemed all that difficult back then, to

someone not fortunate enough to have been immersed in the learning theories of the day. However, Seligman and Maier *were* immersed in those theories. And those theories emphasized what might be called "magic moments" of temporal conjunction of conditioned stimulus and unconditioned stimulus, response and reinforcer. The language of "control" and "lack of control" that seems so natural today was completely absent in the 1960s and early 1970s. An organism could never learn about lack of control if it were locked into processing the world as a series of these magic moments.

Influenced by some revolutionary experiments and ideas coming from another lab mate (e.g., Rescorla, 1967), Seligman and Maier ultimately reasoned that the animal must be learning that responding and shock termination are *independent*. This required that organisms be sensitive to the probability of an outcome (e.g., shock termination) given that they had made some response, to the probability of the outcome given that they had not made that response, and to the relation between these two probabilities. Act and outcome were independent when these two probabilities were equal, and Seligman and Maier argued that this is what the animal learns about inescapable shock—that shock termination is independent of voluntary responses.

It was not long before Seligman and Maier realized that the comparison of these two probabilities defined a dimension that could be called "behavioral control over environmental events." Learning about this dimension—the "computing" of probabilities—is quite far removed from the "magic moments" of earlier theories. However, this still did not explain why animals exposed to inescapable shock later fail to learn to escape. Seligman and Maier argued that the learning that shock termination is independent of behavior has two major consequences. First, this learning interferes with the subsequent formation of associations between the escape response and shock termination. Second, this learning undermines the motivation to attempt to escape. This entire set of conjectures was first published in a chapter by Maier, Seligman, and Solomon (1969) and was collectively called the *learned helplessness hypothesis*.

Research from 1970–1985

Animal research on learned helplessness in the next 10–15 years went in two different directions. The first one focused on the behavioral phenomenon itself and revolved around its generality and limits. Was the interference with escape learning produced by inescapable shock restricted to escaping shock, or would the organism also fail to escape other aversive events? Would an inescapable event other than electric shock produce the same phenomenon? Did uncontrollable aversive events affect aspects of behavior other than escape learning? Did uncontrollable positive events produce analogous outcomes? How long did the effects persist? What was the range of species that showed helplessness phenomena? Could helplessness be demonstrated in humans? Questions such as these were addressed by a growing number of investigators, and answers to these questions indicated that the phenomenon was quite robust and general (see Maier and Seligman, 1976).

The second direction concentrated on theory testing. The learned helplessness theory initially not only met with great resistance but also generated quite a controversy. This should be no surprise since the assumptions about the nature of the learning process made by the theory were opposed to the ideas that were then dominant. In addition to criticizing the ideas involved in the theory of learned helplessness, opponents suggested alternative explanations of the basic interference with escape learning produced by inescapable shock. There were two categories of alternative theories. One category was behavioral. As a class, these theories argued that exposure to inescapable shock taught organisms some response that interfered with the one they were later required to learn. The second category was neurochemical, derived from some pioneering work by Weiss (Weiss, 1968; Weiss, Stone, & Harrell, 1970). These theories argued that inescapable shock depleted a neurotransmitter, typically norepinephrine, that was necessary for the mediation of movement. Therefore, helplessness was not the result of an interference with learning per se, but rather it was the result of neurochemically based movement impairment. What the behavioral and neurochemical accounts have in common is an appeal to peripheral (movement based) rather than central (learning based) mechanisms to explain interference.

The idea that the learned helplessness phenomenon could be explained by the learning of interfering motor responses was relatively easy to disprove (Maier, 1970). However, the neurochemical depletion and movement deficit ideas were more challenging. It became clear that animals that had been exposed to uncontrollable aversive events *did* later move less in the presence of aversive events than did other animals. However, this could be explained readily by both theories. Reduced motivation to escape consequent to learning uncontrollability, as well as depletion of transmitters required for movement, would predict reduced movement.

The difference between the views thus came to be focused on whether or not there was a true interference with associative processes, *as well as* a reduction in movement, following exposure to uncontrollable aversive events. The difficulty was that the learning tasks used in learned helplessness experiments confounded poor learning with reduced movement. That is, all the tasks that had been used required active motor output (e.g., jumping over the hurdle) as the index of learning. A series of experiments attempted to resolve this issue by assessing learning in tasks in which there was either no correlation (Jackson, Alexander, & Maier, 1980), or even a negative correlation between learning and movement (Minor, Jackson, & Maier, 1984). In the latter category of study, the behavior needed to escape was the *withholding* of an active motor response, and animals previously exposed to uncontrollable stressors continued to emit this active motor response, thereby failing to learn to escape. Here, failure to learn was reflected in *greater* movement rather than reduced movement.

Synthesis

Despite this research, there still were numerous difficulties for learned helplessness theory. First, even at the level of psychological theory, learned helplessness was vague concerning the mechanism by which uncontrollable stressors produce later associative interference. Exactly what was interfered with? Second, despite the existence of a true associative interference, movement per se was nevertheless often still reduced. Third, learned helplessness theory had no satisfactory expla-

nation for many of the behavioral effects of uncontrollable stressors that occurred in addition to interference with escape learning. Why should uncontrollable stressors reduce aggressiveness, interfere with maternal behavior, exaggerate fear conditioning, reduce food and water intake, and increase responsiveness to opiate drugs such as morphine? The list of consequences of uncontrollable stressors is long, and reduced incentive to escape and associative interference can not explain all, or even most of them.

Purely behavioral research continues to make progress on the nature of the alterations in associative processes that are produced by exposure to uncontrollable stressors. The bulk of the evidence suggests that exposure to uncontrollable stressors produces an attentional shift away from "internal," response-produced cues and toward external cues (Lee & Maier, 1988). This might suggest that uncontrollable stressors produce a change in learning style, not a deficit per se. Indeed, it might be expected that uncontrollably stressed organisms would learn better than normals in tasks requiring detailed attention to external cues, and this is actually the case (Lee & Maier, 1988).

However, purely behavioral research has not been able to provide much insight into how uncontrollable stressors alter other types of behavior. Here, neuroscience and neurochemistry have been able to provide great advances. A number of investigators have elucidated the neural and neurochemical consequences of uncontrollability in fine detail (e.g., Anisman, Zalcman, Shanks, & Kacharko, 1991; Maswood, Barter, Watkins, & Maier, 1998; Petty, Kramer, & Moeller, 1994; Simson & Weiss, 1988). The tremendous explosion of knowledge concerning how the brain works and how it regulates behavior has made it possible to tie the neurochemical consequences of uncontrollability to the behavioral consequences that occur. It is now possible, for example, to state *why* uncontrollable stressors reduce aggressiveness rather than increase it, *why* they increase fear, and so on. Indeed, a knowledge of the underlying neurocircuitry has allowed a priori prediction of new, unexplored consequences of uncontrollability. For example, from what is known about neurocircuitry, one can make the counter-intuitive prediction that uncontrollably stressed animals will find opiate drugs, but not stimulants, more rewarding and addictive than normal animals (Will,

Watkins, and Maier, 1998). Such a prediction would be unlikely based on purely behavioral knowledge.

Future Directions

The powerful techniques now available to researchers in the neurosciences, added to rapidly accumulating detailed knowledge, suggest that animal research in the foreseeable future will be focused at the neurobiological level. This neurobiological emphasis is also related to the medical need to develop animal models of pathology. Effective biomedical research requires animal models, and the neurobiological work on learned helplessness indicates that it may be an especially useful model of a number of psychopathologies (e.g., Basoglu & Mineka, 1992). The fact that learned helplessness in animals has been proposed as a model of a number of different disorders should not be disturbing. Disorders such as depression, post-traumatic stress disorder, and the like are syndromal nosological categories, not biological entities. A given biological phenomenon, like learned helplessness, could be common to a number of disorders. Furthermore, it could reveal the common core cause of a diversity of pathologies. It is a real testimony to the power and importance of basic research and theory to realize that these broad and exciting new directions, full of potential significance for application, have flowed from the accidental discovery that dogs with a certain history were unable to learn what for other dogs was a trivial task. But it should also be clear that for progress to be made, "accidents" like this have to happen to the right people, and Seligman is such a person.

LEARNED HELPLESSNESS IN HUMANS

Mapping the behavioral scope of learned helplessness in animals, articulating the theory, and defending it against the many challenges that arose would have been more than enough to keep even the most energetic scientist busy for years—but not Seligman. For as the developments just described were unfolding, Seligman was also taking the

helplessness phenomenon in an entirely different direction, by asking about its scope and character in human beings.

The earliest studies of human helplessness were strictly analogous to those done with animals, exposing human research participants to aversive events—typically bursts of white noise—that could neither be escaped nor avoided. Participants were then tested on tasks that *could* be mastered, for example, unscrambling anagrams. And just as with animals, the reliable finding was that relative to individuals who either had no previous experimental experience, or experience with controllable events, those who had experienced uncontrollability often showed deficits, including negative affect, slower problem-solving, more failures to master tasks, and perseveration with unproductive strategies.

One of the most widely cited papers from this early era of human helplessness research was by Seligman and Hiroto (Hiroto & Seligman, 1975), which reported four parallel studies testing the transfer of helplessness deficits from one sort of pretreatment to a second sort of test task. Two pretreatment tasks were used, an "instrumental" pretreatment, in which participants had to press buttons to terminate a noise, and a "cognitive" pretreatment, in which participants had to solve concept-identification problems. Two test tasks were used, one "instrumental" (moving a lever to escape or avoid a noise) and the other "cognitive" (unscrambling anagrams). The four studies were made up of all possible combinations of the pretreatments and test tasks.

It was hardly surprising that deficits were evident when the test tasks were similar to the pretreatments; these results would be predicted by almost any theory. More interesting were the findings that deficits were *also* evident when the test tasks and the pretreatments were dissimilar. Using jargon popular at the time, Hiroto and Seligman concluded that "cross-modal helplessness" had been produced and that this was strong support for the theory that helplessness involved learning that one did not have control over events.

In retrospect, the distinction between "instrumental" and "cognitive" pretreatments and test tasks may seem artificial, but it provides a useful reminder that Hiroto and Seligman did their experiments at a time when there was still broad skepticism in the field about the necessity or utility of mentalistic accounts of behavior that relied on things like

"expectations." The cognitive revolution was still being fought, and behaviorism had yet to surrender. The investigation of learned helplessness, among humans as well as animals, was one of the important battlegrounds.

Perhaps because helplessness theory was fighting a rear-guard action against behaviorism, the early research in helplessness paid scant attention to the *detailed* development of helplessness theory as an alternative. The original helplessness story was a very simple, straightforward account of how experience with uncontrollable events produces subsequent deficits. Helplessness theory argued that uncontrollable events produce an expectation of response-outcome independence, which in turn produces a variety of deficits, and left it at that. Introducing the construct of "expectation" was a sufficient departure from prevailing theory that researchers did not scrutinize this construct until some time later. Indeed, very few early studies even tried to measure expectations, despite the central role they were accorded in helplessness theory. Animals, of course, cannot directly report on what they expect. But people can. Yet, perhaps because helplessness theory was firmly grounded in the animal learning tradition, the earliest investigators of human helplessness did not turn immediately to such contemporary theoretical frames as Rotter's social learning theory (1954). Rotter (1966, 1975) wrote extensively about generalized expectancies (such as locus of control and interpersonal trust) and provided means with which to investigate them.

Cronbach (1957) distinguished between two traditions of scientific psychology—an experimental tradition, emphasizing external objective events and their effects on behavior, and a correlational tradition, emphasizing internal subjective events and their effects. Cronbach called for the unification of these traditions, acknowledging that it would be difficult to do so. Researchers in each tradition not only ask different questions about behavior, but also use different research and analytical strategies. Seligman led other helplessness researchers to bridge the gap described by Cronbach when he asked whether the helplessness phenomenon, as produced in animal and human laboratories, was similar to certain failures of human adaptation.

The best-known of these applications was Seligman's proposal that learned helplessness played a causal role in depression (1974, 1975).

Though animal models of psychopathology had been around at least since the time of Pavlov, Seligman significantly advanced such efforts by specifying explicitly a set of rigorous ground rules for establishing the goodness of a laboratory model (e.g., Miller, Rosellini, & Seligman, 1977). According to Seligman, it was critical that researchers move back and forth between the model and the clinical phenomenon, evaluating the parallels vis-à-vis symptoms, causes, treatments, and preventions. Even today, some researchers touting animal models of various maladies, psychological and physical, do not sufficiently validate their models against the actual clinical phenomena they purport to clarify. A wry comment by Judah Folkman, the laboratory researcher whose studies of a possible new type of cancer treatment received massive publicity in the spring of 1998, exemplifies this well: "We know a great deal about how to cure cancer in mice." In contrast, Seligman knows a great deal about helplessness and its consequences in dogs, rats, and human beings.

Helplessness and Attributional Style

A major turning point in the development of research and theory on human helplessness came in 1978, when Seligman, in collaboration with Abramson and Teasdale, published a revised theory of helplessness and depression (Abramson, Seligman, & Teasdale, 1978). The new theory incorporated ideas from attribution theory, which for years had been the private domain of social psychology. From this point forward, simple claims about the relation among experience, expectations, and behavior were replaced by efforts designed to detail the processes by which expectations were formed. The revised theory started with an attempt to make sense of some anomalous data. As researchers investigated parallels between learned helplessness on the one hand and failures of adaptation like depression on the other, it became clear that problems like depression were more complex than the helplessness theory allowed. Sometimes human helplessness following uncontrollability was chronic; other times, it was transient. Sometimes human helplessness was pervasive; other times, it was circumscribed. And sometimes human helplessness was marked by a striking loss of self-esteem; other times, it was not. The original helplessness theory was silent regarding these variations.

Abramson, Seligman, and Teasdale (1978) revised the learned helplessness theory as it applied to people, and especially to depression, by proposing that when individuals encounter an uncontrollable aversive event, they ask themselves why. The answer people give to this question—the causal attribution they entertain—sets the parameters for the helplessness that ensues. Three dimensions of causal attribution were claimed to be important. If the attributed cause were *stable* ("it's going to last forever") rather than unstable, then helplessness would be long-lasting. If it were *global* ("it's going to undermine everything") rather than specific, then helplessness would be general. And if the causal attribution were *internal* ("it's me") rather than external, then helplessness would be accompanied by a loss of self-esteem. The pattern of causal attributions for a particular instance of uncontrollability would affect a person's expectations for the future. And these expectations would in turn affect the person's behavior.

Reality or social consensus may sometimes dictate the causal explanation that a person embraces; but in more ambiguous circumstances, the individual relies on habitual tendencies to explain bad events in a given way, a personality characteristic described by Seligman as explanatory (or attributional) style (Peterson & Seligman, 1984). Those people who tend to offer stable and global explanations for bad events are not only at risk for helplessness, but also for the failures of adaptation in which helplessness figures in the wake of uncontrollability. Those people who tend to offer internal explanations for bad events are at risk for self-esteem loss in the wake of uncontrollability.

This revised account of learned helplessness—the attributional reformulation—is an explanation of human problems that presupposes that people are rational, acting "logically" in accordance with their interpretation of the causes of events. The rationality inherent in the processes proposed by the attributional reformulation of helplessness theory may be what allows it to be used in the service of a positive psychology. For this rationality can explain resilience as readily as helplessness, hope as well as despair, and good cheer as well as depression. It tells us how to intervene to undo passivity as well as how to prevent passivity in the first place. In all cases, how a person *thinks* about the things he or she experiences is taken seriously. At the same time, it is important to

stress that one of the potent determinants of explanatory style is reality, so that interventions cannot be so simple as just urging people to "think positive" when the world in which they live is relentlessly negative.

The attribution reformulation of helplessness theory is a diathesis-stress theory, proposing that the conjunction of objective bad events (the stress) and a pessimistic explanatory style (the diathesis) is necessary for negative behavioral outcomes to ensue. This position builds from the roots of the helplessness approach in the experimental psychology of animal learning, where bad events—the stress (e.g., uncontrollable electric shocks)—are presented to research participants. When guiding research with people, the attributional reformulation has usually focused on the cognitive diathesis, which proves a consistent correlate of expected outcomes.

When one of us (Peterson) originally went to the University of Pennsylvania in 1979 to work with Seligman, Seligman supervised both a thriving animal laboratory and a thriving human laboratory. The attributional reformulation of helplessness theory had just been proposed, an Attributional Style Questionnaire (ASQ) to measure attributional style had been created, and the initial investigation of explanatory style had just been published (Seligman, Abramson, Semmel, & von Baeyer, 1979). Over the years, explanatory style research has become increasingly popular, and many ways to measure this individual difference variable are now available. The original Attributional Style Questionnaire (Peterson, Semmel, von Baeyer, Abramson, Metalsky, & Seligman, 1982) was expanded, to boost reliability (Peterson & Villanova, 1988), and then simplified (Dykema, Bergbower, Doctora, & Peterson, 1996), to facilitate use with general population samples. A Children's ASQ with a forced choice format was developed by Kaslow and Tanenbaum (Seligman, Peterson, Kaslow, Tanenbaum, Alloy, & Abramson, 1984) and then refined (Thompson, Kaslow, Weiss, & Nolen-Hoeksema, 1998). Forced-choice measures suitable for use with adults are in the process of being created (Reivich, 1995). A content analysis strategy dubbed the CAVE technique (Content Analysis ofVerbatim Explanations) was created that allowed pre-existing written or spoken material to be scored for explanatory style (Peterson, Schulman, Castellon, & Seligman, 1992). Still other strategies have been reported, including ways to score Minnesota

Multiphasic Personality Inventory (MMPI) responses (Colligan, Offord, Malinchoc, Schulman, & Seligman, 1994) and Thematic Apperception Test (TAT) protocols (Peterson & Ulrey, 1994) for explanatory style.

In their monograph *Learned Helplessness: A Theory for the Age of Personal Control*, Peterson, Maier, and Seligman (1993) took a step back from the details of current helplessness research and tried to account for its popularity. One of the reasons they cited was the availability of relatively simple and straightforward methods for conducting helplessness research. The family of explanatory style measures just described exemplifies this point. Countless researchers have used one or another of these approaches to investigate the correlates of pessimistic versus optimistic explanatory style. Some of this work has been theory-driven, for example, the several hundred investigations of explanatory style and depression (Sweeney, Anderson, & Bailey, 1986). But other studies have been exploratory and opportunistic, such as Peterson's investigations of explanatory style as a risk factor for traumatic injuries (see Chapter 8). In any case, explanatory style has emerged from its connection to helplessness as a personality characteristic in its own right, one that is broadly associated with coping, adaptation, and well-being (Buchanan & Seligman, 1995).

Important questions about explanatory style remain, of course (Peterson, 1991). For example, what are the origins of explanatory style, especially its pessimistic manifestation? One can readily explain why a pessimistic style maintains itself, because of the vicious cycles it can set into motion, but why would anyone start out with this view of the causes of events? Here research has only begun to scratch the surface, but there are hints that the explanatory styles of parents and children converge (Seligman et al., 1984). Also, failure and trauma early in life seem to foreshadow pessimism later in life (Peterson, Maier, & Seligman, 1993). Finally, in a study of twins, Schulman, Keith, and Seligman (1993) reported that explanatory style was moderately heritable. We should probably not interpret this to mean that there is a specific causal attribution gene waiting to be discovered. However, some of the factors that lead to success or failure in life—intelligence, attractiveness, health, physical prowess, and the like—are indisputably heritable, and the

experiences that they enable or block may well shape the individual's explanatory style.

Perhaps more important, especially for a future positive psychology of optimism and hope, is the question of how an optimistic explanatory style can be encouraged. Intervention studies by Seligman's research group demonstrate that children can be taught with cognitive-behavioral exercises to look at events in a more optimistic fashion, and that this instruction seems to have long-term benefits (see Chapter 11). And Beck's cognitive therapy for depression has the effect of making pessimistic individuals more optimistic; changes in explanatory style seem to go in lock-step with symptom relief and may confer protection against the recurrence of depression (Seligman, Castellon, Cacciola, Schulman, Luborsky, Ollove, & Downing, 1988).

As the multiple available measures of explanatory style facilitate rapid progress, the downside of these measures could be premature preoccupation with the particular constellation of attributional dimensions hypothesized by Abramson, Seligman, and Teasdale in their reformulation of helplessness theory. To be sure, the internality, stability, and globality of causal attributions are important, but they are hardly the only attributional dimensions of potential interest (Weiner, 1986). There is no doubt that whether events are inside the skin or outside (internality), whether they persist across time or not (stability), and whether they generalize across situations or not (globality) are key dimensions of the psychological universe. But the universe must have more key dimensions than this. What are they?

For example, what about the controllability of events? One might think that perceived controllability should have been included as an additional dimension measured by the ASQ and its descendants, but the reformulation of helplessness theory—inspired as it was by data from controlled laboratory experiments—took it for granted that the controllability or uncontrollability of events was objectively known, and not a part of experience open to interpretation and sensitive to individual differences. As the revised theory is increasingly applied outside the laboratory, this neglect of controllability as a possible dimension of explanatory style becomes more and more unfortunate. For example,

studies suggest that ostensible "pessimism" (internal, stable, and global causal explanations) concerning bad events seen as controllable may have desirable consequences (Sellers & Peterson, 1993), perhaps because they constitute an assertion that things can be different.

Similarly, what about the importance (e.g., magnitude or severity) of the events about which attributions are made? Again, the reformulation did not consider this parameter because in laboratory experiments the aversive events to which research participants were exposed were held constant. The earliest version of the ASQ *did* ask respondents to rate the importance of the hypothetical events that were presented, but these ratings rarely proved interesting, perhaps because they did not span a broad enough range of possible importance.

This neglect of the importance of events in helplessness theory has recently been rectified. In their hopelessness theory of depression, Abramson, Metalsky, and Alloy (1989) made the importance of events a cornerstone in what has become the next major reformulation of helplessness theory. Stoplights and pollen counts are uncontrollable and aversive, but it is doubtful that depression is ever precipitated by these events, no matter how they are interpreted. They are simply not important enough.

Along similar lines, the original ASQ deliberately included achievement-related and interpersonal types of events, under the assumption that explanatory style might show some domain specificity. Again, this distinction rarely proved interesting, largely because respondents treated these different sorts of events as very similar. But perhaps young adult college students in the United States, the typical participants in the earliest studies of explanatory style, are likely to conflate achievement and interpersonal outcomes. Good grades and high salaries can boost one's social status, turning achievement into something social. And interpersonal success can be reflected by how many "best" friends one has, turning social circumstances into opportunities for achievement. As the attributional reformulation of helplessness is extended into other settings—other age groups, other socioeconomic classes, and other cultures—it would be wise to revisit the possibility that there are domain-specific explanatory styles. It is already clear, for example, that the internal versus external distinction can prove problematic when

assessed in collectivist cultures, where the distinction between self (internal) and others (external) is not given the same significance that it has in the contemporary United States (cf. Miller, 1984).

One of the interesting extensions of the attributional reformulation targets these other populations. The ASQ has been translated into a variety of languages, and investigators have begun to explore explanatory style in other cultures. To date, results are largely consistent with the findings from the United States. A pessimistic explanatory style has undesirable correlates, whereas an optimistic explanatory style has desirable ones. But this research has been oriented toward confirmation, and the possibility that the ASQ (and of course the accompanying attributional reformulation of helplessness) needs to be made more culturally appropriate has yet to be considered seriously. We anticipate that an exciting chapter in the learned helplessness tradition will be written when such cross-cultural investigations are begun in earnest.

What about the role of objective events? As psychologists in the learned helplessness tradition poise themselves to help develop a positive social science (Seligman, 1998), it is crucial that the role of reality be remembered. From the uncontrollable events that predispose people to helplessness to the actual causal texture of the world that (sometimes) dictates a particular type of causal attribution, the outcomes of interest to "positive" social scientists are as much influenced by what the situation does or does not afford as they are by attitudes of hope and optimism. One would not want to stroll into Lebanon, Northern Ireland, the former Yugoslavia, or West Philadelphia and advise their residents simply to be more optimistic in their outlooks. A positive social science needs to encourage appropriate changes in social conditions so that optimism can exist as a viable worldview.

CONCLUSION

This chapter tried to accomplish two things: provide a brief history, as background for the compelling contributions to this book, and establish a sense of the magnitude of Martin Seligman's contributions to date. It is true that much about helplessness and optimism still needs to

be explored. Helplessness and its consequences are not as simple as people once thought, and many details need to be worked out. Many big questions remain to be answered, and some big questions have yet to be asked. Nevertheless, what is already notable about the research spawned by learned helplessness, especially Seligman's contributions to that research, is that they constitute a rare example of what Cronbach envisioned forty years ago.

We suggest an informative exercise: Access PsycINFO or PsycLIT, and enter "au=Seligman, Martin E. P." Your first thought may be "Oh, the E. P. stands for "ever publishing," but the sheer number of articles that Seligman has had a hand in is not the point. Rather, look at the range of journals in which Seligman and his colleagues have published studies of learned helplessness, from *Journal of Comparative and Physiological Psychology* to *Journal of Consulting and Clinical Psychology*, from *Journal of Experimental Psychology* to *Journal of Personality and Social Psychology*, from *Behaviour Research and Therapy* to *Cognitive Therapy and Research*, from *American Psychologist* to *Journal of Abnormal Psychology*, from *Psychology Today* to *Science*. With Seligman taking the lead, the phenomenon of learned helplessness has produced research as broad in scope as is psychology itself.

To locate Seligman further in the broad history of psychology, we must also note that he is among the rare psychologists who has heeded the plea George Miller (1969) made in his American Psychological Association Presidential speech to "give psychology away." Many serious researchers are reluctant to take their ideas to the general public, but Seligman, the president of APA in 1998, has never been shy about doing so. His popular books are as laudable as his monographs and journal articles. They make difficult and important ideas accessible to untrained readers without oversimplifying or pandering to current fashion. The point is that Seligman has always had something worth giving away. His basic research has legitimized his popular presentations. At the same time, his writing for the general public has stimulated interest in basic research among several generations of academic psychologists, and it continues to do so.

Finally, as evidenced by this book and by Seligman's other recent contributions (e.g., Seligman, 1998), Seligman is leading his many stu-

dents and collaborators in the next major new direction—taking the insights gained from research on learned helplessness and putting them to use in creating a psychology that emphasizes the nurture of what is good rather than just the repair of what is broken. Seligman's most recent endeavor is a call for psychology to be as focused on strength as weakness, as interested in building the best things in life as repairing the worst, and as concerned with making the lives of normal people fulfilling as healing pathology. He dubs this "positive psychology," and although his interest in this topic stems from decades of studying what can go wrong with the human condition, positive psychology represents a radical reframing of Seligman's research program.

To be sure, everything learned about helplessness and its relation to depression, failure, and illness informs our knowledge of the absence of these conditions (Peterson, Maier, & Seligman, 1993). But there is more to positive psychology than the study of what does not go wrong. The "neutral" points of the typical outcome measures in the learned helplessness research tradition signify not being depressed, not failing, and not being ill. To extend past findings beyond these neutral points and offer conclusions about emotional fulfillment, achievement, and wellness, positive psychologists must study not just *independent* variables that pertain to strength but also appropriate *dependent* variables. And different questions must be posed by positive psychologists than have been asked by researchers who work within a disease or deficiency framework. Possible topics of interest to positive psychology include hope, creativity, optimism, happiness, flow, courage, emotional intelligence, giftedness, genius, future-mindedness, interpersonal skills, and honesty.

In approaching these topics, positive psychologists should heed certain lessons from past attempts to understand human strengths and skills. From the fate of humanistic psychology of the 1960s and 1970s, positive psychologists should learn the importance of relying on empirical research. Humanists were skeptical about the scientific method and what it could yield yet were unable to offer an alternative other than the unsupported assertion that people were good. In contrast, positive psychologists must see both strength and weakness as authentic and as amenable to scientific understanding.

From the fate of past studies of creativity, positive psychologists should

learn to keep an open mind about appropriate operational definitions of the topics of interest to them. Although self-report questionnaire measures are often useful and valid, not everything of interest to positive psychologists can be so assessed. Already we are seeing researchers searching for questionnaire measures of emotional intelligence (Schutte et al., 1998), but there is good reason to suspect that these measures will need to be supplemented if not altogether replaced by more complex measures that rely on the observation of actual behavior (Davies, Stankov, & Roberts, 1998).

From the fate of intelligence testing throughout the twentieth century, positive psychologists should learn several lessons as well. Desirable psychological states should not be reified; they should not be studied out of context; and they should not be used to rank order all people along a single continuum. Most importantly, positive psychologists should not regard what is "right" about people as unalterable, the result of fortuitous genetic or environmental circumstances. Indeed, the central goal of positive psychology is to cultivate and encourage the good psychological life for all people. That would be a legacy worthy of Martin Seligman.

REFERENCES

Abramson, L.Y., Metalsky, G.I., & Alloy, L.B. (1989). Hopelessness depression: A theory-based subtype of depression. *Psychological Review, 96,* 358–372.

Abramson, L.Y., Seligman, M.E.P., & Teasdale, J.D. (1978). Learned helplessness in humans: Critique and reformulation. *Journal of Abnormal Psychology, 87,* 49–74.

Anisman, H., Zalcman, S., Shanks, N., & Kacharko, R.M. (1991). *Multisystem regulation of performance deficits induced by stressors: An animal model of depression.* Clifton, NJ: Humana Press.

Basoglu, M., & Mineka, S. (1992). *The role of uncontrollable and unpredictable stress in post-traumatic stress response in torture survivors.* Cambridge: Cambridge University Press.

Bracewell, R.J., & Black, A.H. (1974). The effects of restraint and noncontingent pre-shock on subsequent escape learning in the rat. *Learning and Motivation, 5,* 53–69.

Buchanan, G.M., & Seligman, M.E.P. (Eds.) (1995). *Explanatory style.* Hillsdale, NJ: Erlbaum.

Colligan, R.C., Offord, K.P., Malinchoc, M., Schulman, P., & Seligman, M.E.P. (1994). CAVEing the MMPI for an optimism-pessimism scale: Seligman's attributional model and the assessment of explanatory style. *Journal of Clinical Psychology, 50,* 71–95.

Cronbach, L.J. (1957). The two disciplines of scientific psychology. *American Psychologist, 12,* 671–684.

Davies, M., Stankov, L., & Roberts, R.D. (1998). Emotional intelligence: In search of an elusive construct. *Journal of Personality and Social Psychology, 75,* 989–1015.

Dykema, K., Bergbower, K., Doctora, J.D., & Peterson, C. (1996). An Attributional Style Questionnaire for general use. *Journal of Psychoeducational Assessment, 14,* 100–108.

Hiroto, D.S., & Seligman, M.E.P. (1975). Generality of learned helplessness in man. *Journal of Personality and Social Psychology, 31,* 311–327.

Jackson, R.L., Alexander, J.H., & Maier, S.F. (1980). Learned helplessness, inactivity, and associative deficits: Effects of inescapable shock on response choice escape learning. *Journal of Experimental Psychology: Animal Behavior Processes, 6,* 1–20.

Lee, R.K.K., & Maier, S.F. (1988). Inescapable shock and attention to internal versus external cues in a water escape discrimination task. *Journal of Experimental Psychology: Animal Behavior Processes, 14,* 302–311.

Maier, S.F. (1970). Failure to escape traumatic shock: Incompatible skeletal motor response or learned helplessness. *Learning and Motivation, 1,* 157–170.

Maier, S.F., & Seligman, M.E.P. (1976). Learned helplessness: Theory and evidence. *Journal of Experimental Psychology: General, 105,* 3–46.

Maier, S.F., Seligman, M.E.P., & Solomon, R. (1969). *Pavlovian fear conditioning and learned helplessness.* New York: Appleton-Century-Crofts.

Maswood, S., Barter, J.E., Watkins, L.R., & Maier, S.F. (1998). Exposure to inescapable but not to escapable stress increases extracellular levels of 5-HT in the dorsal raphe nucleus of the rat. *Brain Research, 783,* 115–120.

Miller, G.A. (1969). Psychology as a means of promoting human welfare. *American Psychologist, 24,* 1063–1075.

Miller, J.G. (1984). Culture and the development of everyday social explanation. *Journal of Personality and Social Psychology, 46,* 961–978.

Miller, W.R., Rosellini, R.A., & Seligman, M.E.P. (1977). Learned helplessness and depression. In J.D. Maser & M.E.P. Seligman (Eds.), *Psychopathology: Experimental models* (pp. 104–130). San Francisco: Freeman.

Minor, T.R., Jackson, R.L., & Maier, S.F. (1984). Effects of task irrelevant cues and reinforcement delay on choice escape learning following inescapable shock: Evidence for a deficit in selective attention. *Journal of Experimental Psychology: Animal Behavior Processes, 10,* 543–556.

Overmier, J.B. (1968). Interference with avoidance behavior: Failure to avoid traumatic shock. *Journal of Experimental Psychology, 78,* 340–343.

Overmier, J.B., & Leaf, R.C. (1965). Effects of discriminative Pavlovian fear conditioning upon previously or subsequently acquired avoidance responding. *Journal of Comparative and Physiological Psychology, 60,* 213–217.

Overmier, J.B., & Seligman, M.E.P. (1967). Effects of inescapable shock on subsequent escape and avoidance behavior. *Journal of Comparative Physiology and Psychology, 63,* 23–33.

Peterson, C. (1991). Meaning and measurement of explanatory style. *Psychological Inquiry, 2,* 1–10.

Peterson, C., Maier, S.F., & Seligman, M.E.P. (1993). *Learned helplessness: A theory for the age of personal control.* New York: Oxford University Press.

Peterson, C., Schulman, P., Castellon, C., & Seligman, M.E.P. (1992). CAVE: Content analysis of verbatim explanations. In C. P. Smith (Ed.), *Motivation and personality: Handbook of thematic content analysis* (pp. 383–392). New York: Cambridge University Press.

Peterson, C., & Seligman, M.E.P. (1984). Causal explanations as a risk factor for depression: Theory and evidence. *Psychological Review, 91,* 347–374.

Peterson, C., Semmel, A., von Baeyer, C., Abramson, L.Y., Metalsky, G.I., & Seligman, M.E.P. (1982). The Attributional Style Questionnaire. *Cognitive Therapy and Research, 6,* 287–299.

Peterson, C., & Ulrey, L.M. (1994). Can explanatory style be scored from TAT protocols? *Personality and Social Psychology Bulletin, 20,* 102–106.

Peterson, C., & Villanova, P. (1988). An expanded Attributional Style Questionnaire. *Journal of Abnormal Psychology, 97,* 87–89.

Petty, F., Kramer, G., & Moeller, M. (1994). Does learned helplessness induction by Haloperidol involve serotonin mediation? *Pharmacology, Biochemistry, & Behavior, 48,* 671–676.

Reivich, K. (1995). The measurement of explanatory style. In G.M. Buchanan & M.E.P. Seligman (Eds.), *Explanatory style* (pp. 21–47). Hillsdale, NJ: Erlbaum.

Rescorla, R.A. (1967). Pavlovian conditioning and its proper control procedures. *Psychological Review, 74,* 71–80.

Rotter, J.B. (1954). *Social learning and clinical psychology.* Englewood Cliffs, NJ: Prentice-Hall.

Rotter, J.B. (1966). Generalized expectancies for internal versus external control of reinforcement. *Psychological Monographs, 81*(1, Whole No. 609).

Rotter, J.B. (1975). Some problems and misconceptions related to the construct of internal versus external reinforcement. *Journal of Consulting and Clinical Psychology, 43,* 56–67.

Schulman, O., Keith, D., & Seligman, M.E.P. (1993). Is optimism heritable? A study of twins. *Behaviour Research and Therapy, 31,* 569–574.

Schutte, N.S., Malouff, J.M., Hall, L.E., Haggerty, D.J., Cooper, J.T., Golden, C.J., & Dornheim, L. (1998). Development and validation of a measure of emotional intelligence. *Personality and Individual Differences, 25,* 167–177.

Seligman, M.E.P. (1974). Depression and learned helplessness. In R.J. Friedman & M.M. Katz (Eds.), *The psychology of depression: Contemporary theory and research* (pp. 83–113). Washington, DC: Winston.

Seligman, M.E.P. (1975). *Helplessness: On depression, development, and death.* San Francisco: Freeman.

Seligman, M.E.P. (1998). Positive social science. *APA Monitor, 29(4),* 2, 5.

Seligman, M.E.P., Abramson, L.Y., Semmel, A., & von Baeyer, C. (1979). Depressive attributional style. *Journal of Abnormal Psychology, 88,* 242–247.

Seligman, M.E.P., Castellon, C., Cacciola, J., Schulman, P., Luborsky, L., Ollove, M., & Downing, R. (1988). Explanatory style change during cognitive therapy for unipolar depression. *Journal of Abnormal Psychology, 97,* 13–18.

Seligman, M.E.P., & Maier, S.F. (1967). Failure to escape traumatic shock. *Journal of Experimental Psychology, 74,* 1–9.

Seligman, M.E.P., Peterson, C., Kaslow, N.J., Tanenbaum, R.L., Alloy, L.B., & Abramson, L.Y. (1984). Attributional style and depressive symptoms among children. *Journal of Abnormal Psychology, 93,* 235–238.

Sellers, R.M., & Peterson, C. (1993). Explanatory style and coping with controllable events by student-athletes. *Cognition and Emotion, 7,* 431–441.

Simson, P.E., & Weiss, J.M. (1988). Altered activity of the locus coeruleus in an animal model of depression. *Neuropharmacology, 1,* 287–295.

Sweeney, P.D., Anderson, K., & Bailey, S. (1986). Attributional style in depression: A meta-analytic review. *Journal of Personality and Social Psychology, 50,* 974–991.

Thompson, M., Kaslow, N.J., Weiss, B., & Nolen-Hoeksema, S. (1998). Children's Attributional Style Questionnaire-Revised (CASQ-R): Psychometric examination. Unpublished manuscript, Emory University.

Weiner, B. (1986). *An attributional theory of motivation and emotion.* New York: Springer-Verlag.

Weiss, J.M. (1968). Effects of coping responses on stress. *Journal of Comparative and Physiological Psychology, 65,* 251–260.

Weiss, J.M., Stone, E.A., & Harrell, N. (1970). Coping behavior and brain norepinephrine in rats. *Journal of Comparative and Physiological Psychology, 72,* 153–160.

Will, M.J., Watkins, L.R., & Maier, S.F. (1998). Uncontrollable stress potentiates morphine's rewarding properties. *Pharmacology, Biochemistry, & Behavior, 60,* 655–664.