



Science and the Reformulated Learned-Helplessness Model of Depression

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Concurrent with the publication of the reformulated model (RM) of learned helplessness (LH) and depression (Abramson, Seligman, & Teasdale, 1978), Costello (1978) warned about a bandwagon effect among depression researchers, who had seemingly adopted the LH approach with relatively little scrutiny of the theory or data (see also Ripper, 1977). Although those original fears of a bandwagon effect were directed at the original LH model rather than the attributional one, it is now apparent that this warning applies equally to the new version, RM. We can think of no major area of research in which the theory and the data purportedly supporting the theory are as unrelated as in studies of the LH model of depression. Yet, studies that bear only a marginal relation to the theory continue to be published, and claims in those articles about how the data confirm the model continue to be accepted almost without question.

When initially asked to comment on Chris Peterson's defense of the explanatory style concept, we were somewhat reluctant. Past attempts by our research group and by others to refine and advance attributional models of depression and related problems in living have had relatively little effect on the research or theorizing in the LH and depression area. Colleagues have variously attributed this perseverance of the field to the bandwagon phenomenon, or to the *Zeitgeist*. Also, we have recently published a smaller scale defense of the broad notion that attributional style is both valid and valuable as a theoretical construct (Anderson, Jennings, & Arnoult, 1988), and the main points in that article overlap considerably with Peterson's valid ones. Finally, our current research is focused on quite different problems in social psychology. Thus, preparation of this critique required a major shift in thinking and literature reviewing. That shift has proved interesting, however, and we hope our reentry into the area proves useful to others. We hope that the recent reflections of Peterson and others (Abramson, Metalsky, & Alloy, 1989) signal a shift in the *Zeitgeist* toward a period of greater reflection on and consolidation of all the empirical findings in this domain. Certainly, our views have changed on several issues in recent months.

We see both strengths and weaknesses in RM as a model of LH and depression, and in Peterson's article, with the greater part being strengths. The vast array of experimental data generated by the LH paradigm and by other attributional models of motivation and emotion (e.g., Weiner, 1986) and the nonexperimental attributional research on depression and related problems in living (e.g., Anderson et al., 1988; Sweeney, Anderson, & Bailey, 1986) have overwhelmingly demonstrated the importance of attributions in determining people's reactions to life events. The fact that most of the studies do not test the specific predictions of RM does not detract from the importance of the general attributional perspective. Similarly, Peterson's article identifies several important categories of criticisms leveled not only at RM, but at the general attributional perspective.

We do not have the space to address all the major points raised by Peterson; neither can we fully elaborate on other

points needing attention in this area. Thus, we first list points on which we are in substantial agreement with Peterson. Then, we tackle in more detail those that need further thought and refinement. Finally, we briefly mention several smaller points of contention.

Points of Agreement

Historical or Cultural Limitations

It seems unlikely that any culture or historical period has ever existed in which causal explanation was not a major part of everyday life. The types of explanations generated certainly do vary over both historical and cultural variables. For example, witchcraft as an explanation for unusual behavior has varied from culture to culture and over time. However, we would expect the attribution dimension effects to be essentially the same.

Consistency of Explanatory (or Attributional) Style

Data on the Attributional Style Assessment Test (ASAT; Anderson et al., 1988) and on other attributional style measures (Feather, 1983; Feather & Tiggemann, 1984) agree with Peterson's assessment of the Attributional Style Questionnaire (ASQ); all show modest but acceptable levels of consistency. Improvements can be made by increasing the number of items (e.g., Peterson & Villanova, 1988) and by selecting items that fit the target population. Attributions for failure on a blind date are not as relevant for a population of midcareer executives as for the typical college student.

Explanatory (or Attributional) Style and Spontaneous Attributions

We are convinced that attributional style does predispose natural or spontaneous attributions. In addition to the kinds of evidence Peterson specified, we accept as supporting evidence studies in which attributional style has the predicted effects on behaviors and other nonattributional variables, even when public attributions for the target event have not been made (e.g., Anderson, 1983b), or when postexperiment public attributions don't exactly correspond. There are two reasons for this. First, public attributions for a particular event are very susceptible to impression management concerns. Second, there is no valid reason to assume that all important attributional processing is available in consciousness.

Low Correlations

For most theory testing in psychology, the particular magnitude of a correlation (or more appropriately, partial correla-

tion) is largely irrelevant. Our major concern is that the competing measures (e.g., controllability and globality) show roughly the same reliability, so that one theory does not "win" merely on the basis of statistical whim. For practical applications, though, effect size is important. Peterson's comments are on target.

Major Points of Disagreement

Controllability as a Major Dimension of Attributional Style

Concerns about the failure to include or test the controllability dimension of attributions appeared in the same issue as RM (Wortman & Dintzer, 1978). Peterson's defense of the failure to even consider controllability as a dimension misses the points raised by Wortman and Dintzer and by later authors (e.g., Anderson & Arnoult, 1985a, 1985b). The historical explanation, that the chosen dimensions were targeted at LH (rather than depression), is informative but largely irrelevant. Our guess is that early researchers regarded situations as controllable or uncontrollable, and thus thought that there was no need to assess the subjects' perceptions of controllability. This assumption may even be true for the laboratory analogue if one restricts perceptions of controllability to the just-failed task. However, virtually all theoretical writings on LH state or imply that perceptions of future controllability are crucial determinants of LH and (by extension) depression effects. We agree. From a future-oriented perspective, though, the situations used in the experimental studies as well as the hypothetical situations in the ASQ and other style measures are not uniformly seen by subjects as uncontrollable; hence, neither are the perceived causes. Thus, we think it important to assess the perceived controllability of the attributed cause of the experienced event, in a future-oriented fashion. We also think that RM can accommodate such a theoretical modification with little difficulty.

This brings us to Peterson's second defense. He claims that "... perceptions of the uncontrollability of events ... are usually inferred from the particular causal explanations that a person offers. A bad event believed to have an internal, stable, and global cause is arguably one that will be regarded as uncontrollable ...". Two points are relevant here. First, one must distinguish between perceived controllability of an event in the past and a person's perceived ability to control the major cause of that event in future situations. A person can perceive that a past event was uncontrollable, but still perceive the cause of the event and the future occurrence of the event as controllable. One way this can happen is when the initial event "teaches" the person something of value for future task performances. For example, a person trying to persuade a stranger to donate blood may learn, in initial failure trials, that extensive discussions of pain are not persuasive (Anderson, 1983b). The initial failures may be perceived as uncontrollable ("I didn't know"), but the cause (use of an inappropriate strategy) is more controllable ("Now that I do know, I'll try something else and do better").

Second, Peterson seems to be saying that the controllability dimension is subsumed by the other dimensions. (Other writings in this domain have implied that locus and stability define controllability perceptions.) In a later section of the article, Peterson argues that "[an] appropriate way to show that explanatory style is *not* a sensible construct is to

measure this variable explicitly and then to find . . . that it is confounded by some other variable." We maintain that this is exactly what has been done with the controllability dimension, both outside the attributional style context (e.g., Anderson, 1983a, 1985) and in studies of attributional style (e.g., Anderson & Arnoult, 1985b). Not only were the ASQ dimensions confounded with controllability, but in tests pitting them against each other, the controllability dimension consistently added significant unique increments in the prediction of depression, loneliness, and shyness. Thus, the controllability dimension is not subsumed by the other dimensions.

The final defense offered for failing to use a controllability dimension involves a report of an unpublished study. Unfortunately, the (necessarily) brief description prevents us from being able to accurately evaluate it, but on the surface it seems unreasonable to ignore all the relevant published studies on the perceived dimensionality of causes (see Weiner's, 1986, review) in favor of this one. Although there are some problems with past dimensionality studies (see Anderson, 1989), those studies as well as my own results (Anderson, 1989) demonstrate that some type of controllability dimension is a major part of attributional perceptions. Mikulincer and Caspy's (1986) phenomenological analysis of helplessness similarly points to the primacy of thoughts concerning controllability.

Confounded Variables

Confounded dimensions. By now it is obvious to most researchers in this area that various attributional dimensions are not empirically orthogonal. Early statements to this effect include those by Passer, Kelley, and Michela (1978) and Anderson (1983a, 1985). The consequences of this lack of independence have also been spelled out in these publications and by Weiner (1986). Peterson acknowledges this dimensional confounding, but does not elucidate its importance for testing theory. If a theory proposes that a particular dimension is important in producing some effect, then a test of that theory requires that confounded effects of other dimensions be partialled out first. For instance, if globality is proposed to be related to current depression levels (as it must be, if the more specific prediction that globality influences the range of depression is true), then it should correlate significantly with depression even after the effects of stability, locus (internality), and controllability have been partialled out. That is, the simple zero-order correlation is irrelevant to the test of the globality dimension. This is true even if the confounded dimensions are predicted to relate to the criterion in the same way. The target dimension (e.g., globality) is included in the theory because of a theoretical belief that it contributes something unique to the understanding and prediction of the target problem (e.g., depression). If the dimension does not contribute a significant unique piece, then the specific theoretical prediction has been disconfirmed.

As noted earlier, most studies fail to assess a major attributional dimension (controllability), so unique and confounded variance cannot be apportioned at all. Even those experimental studies that purport to manipulate attributions along only one dimension cannot be seen as adequate tests *unless* they provide evidence that the manipulated attributions differ only on the target dimension, or statistical controls for confounded dimensions are used. For example, one might ma-

nipulate ability and effort attributions with the intent to test the stability hypothesis. But ability and effort differ on both stability and controllability, and perhaps on perceived locus. Any obtained effect may be due to stability, controllability, locus, or some combination.

Does this mean that all the hundreds of LH and attribution studies that have failed to control for various dimensional confounds are worthless? The answer depends on the particular question asked. If the question focuses on a specific model, then the answer is yes: studies that fail to test the unique contribution of a particular dimension also fail to test that specific model. However, if the question is aimed at a more general level (e.g., "Do attributions play an important role?"), then the answer is no, the study is not worthless. In our view, dimensionally confounded studies are quite valuable in demonstrating that attributions play a major role in LH and in depression, even though they do not adequately test the specific dimensions proposed.¹

Confounded criteria. This line of reasoning points to a related problem with most research in this area. RM specifically predicts that each attributional dimension will have certain specific effects. Stability of attributions (and attributional style) is predicted to influence the stability of LH deficits (and depression); globality of attributions (and attributional style) is predicted to influence the range of LH deficits (and depression); locus of attributions (and attributional style) is predicted to influence self-esteem. The problem is that the criterion variables are also frequently confounded. Global measures of depression tap into several of the more specific criteria that supposedly relate specifically to a particular attributional dimension. With few exceptions, neither LH studies nor depression studies in this area have tested these specific predictions with appropriate regression or partial-correlation techniques.

In sum, the confounding of criteria and of dimensions, measured and unmeasured (i.e., controllability), has resulted in failure to test RM. Although space and time limitations preclude an exhaustive review, we have located a few studies that have attempted to disentangle these confounds. Frequently, they have yielded results contradictory to RM and have had practically no effect on the field as a whole. Sometimes they have been interpreted as supporting RM, a result we attribute to the bandwagon effect. A few examples may clarify these points.

Attributions and manipulated failures. Our initial search uncovered five studies (Alloy, Peterson, Abramson, & Seligman, 1984; Mikulincer, 1986, Experiments 1, 2, & 3; Pasahow, 1980) meeting three criteria:

1. Attributions were assessed in the context of some manipulated failures.

¹This problem exists in our own studies involving performance at interpersonal tasks as a function of experimentally or naturalistically generated attributions (e.g., Anderson et al., 1988). Because we have not measured and controlled for all attribution dimensions, we cannot make strong dimensional claims, though we favor a controllability one. However, the main point of our past work of this type, and we suspect of similar work by many others, was that attributions and attributional styles are important determinants of reactions to failures. For this purpose the "confounded dimensions" problem is largely irrelevant.

2. Some attempt was made to control for the effects of confounded dimensions either by partialing out confounded variance or by demonstrating that attribution manipulations did not produce systematic dimension changes on the wrong dimensions.
3. The globality effect on generalization of helplessness deficits was the target of the investigation.

Four of the five studies found no relation between perceived attributional globality and performance deficits in the subsequent test task. Although none of the studies measured controllability attributional style, Mikulincer's did assess future control expectancies and found that these mediated the generalization of performance deficits in all three experiments.

Attributions and problems in living. We located four studies (Anderson & Arnoult, 1985b; Eaves & Rush, 1984; Persons & Rao, 1985; Tennen & Herzberger, 1987) meeting two criteria:

1. Attributions were linked to problems in living (i.e., depression, loneliness, or shyness).
2. Some attempt was made to unconfound attribution dimensions, or to unconfound criterion variables relevant to RM.

Both the Eaves and Rush (1984) and the Tennen and Herzberger (1987) studies provided tests of hypotheses linking specific dimensions to specific criteria. Eaves and Rush showed that stability of attributional style for negative events is associated with duration of depression episode, supporting RM. However, the authors also reported that locus and globality were significantly related to duration of depression, contradicting the model. Tennen and Herzberger showed that locus attributional style was linked to depression even after self-esteem was partialled out. The model, of course, specifies that locus is relevant only to self-esteem deficits.

Only one study included controllability attributional style (Anderson & Arnoult, 1985a, 1985b). Criterion variables included depression, loneliness, and shyness. The main findings of relevance here were:

1. Each dimension by itself was significantly related to the various problems in living.
2. The attributional style dimensions were moderately intercorrelated.
3. Globality contributed little unique variance to the prediction of these problems in living.
4. Controllability was the most important attributional dimension as a unique predictor.

Persons and Rao (1985) similarly demonstrated how zero-order correlations can disappear or even reverse direction when confounded attribution dimensions are statistically controlled.

We also found two studies of attributional style and depression that reported complete correlation matrices (on stability, locus, and globality) so that even though no statistical unconfounding was reported in the articles we were able to do so ourselves (Feather, 1983; Peterson & Villanova, 1988). For Feather's data, none of the attributional style-depression relations was significant after confounded variance was re-

moved. For the Peterson and Villanova (1988) data, only the globality–depression relation remained significant.

In short, the confounding problems are not mere technicalities; they seriously compromise the value of studies as tests of specific models. Without measures of controllability attributional style, and without adequate controls for confounding, all these studies become tests of general attributional models, rather than tests of specific theories.

Globality in LH Versus Depression Models

We distinguish between single-event attributions and attributional style in our analysis of globality; our thesis (supported by our data) is that globality is irrelevant as a style dimension. Conceptually, the value of the globality dimension lies in its ability to specify when maladaptive attributions should produce depressive effects across different types of situations. For example, attributing a failure at Task 1 to some uncontrollable cause that also happens to be global should increase the likelihood of motivational and performance deficits at Task 2. The more types of situations that are relevant to the initial attribution, or alternatively, the more global the initial attribution, the broader the range of Task 2s that will yield deficits. So far, we are in complete agreement with RM.

Consider what it means, however, to measure attributional style. To have a maladaptive attributional style, one must make maladaptive attributions across a range of situations. For example, one must make uncontrollable attributions for failure across several quite different situations. Globality is inherently a part of the measure of style. The wider the range of situations sampled, the more the concept of globality is captured in the style measure.

Why then do the zero-order correlations between globality and depression sometimes work? The main reason is because the globality dimension is confounded with other dimensions that are truly causal in producing depression deficits. Partialing out the effects of the other “active” dimensions results in globality attributional style being superfluous. Its value is already captured by the style measure. One implication of this analysis is that globality attributional style is likely to add unique increments to the prediction of depression to the extent that style is measured narrowly. The extreme case, of course, is when only one situation is considered, which is what happens in laboratory studies of LH. Here, globality of a maladaptive attribution may be important in predicting the generalization of effects even after partialing out other dimensional effects (e.g., locus).

Interactions of Attribution Dimensions

How do attributional style dimensions combine in creating LH and depression reactions? Examination of various theoretical statements does not yield a clear answer. All would agree that consistently attributing failures to major character flaws (internal, stable, uncontrollable, global) is less adaptive than attributing failures to factors that the person thinks he or she can control in the future. But how do the effects of attribution dimensions combine? Is there a unique pattern that is particularly maladaptive? Or do the effects simply combine additively? The procedure of summing dimensions, as is frequently done with the ASQ, implicitly assumes (a) an additive model (b) in which the dimensions are equally

weighted. However, one could as easily derive an interactional (or synergistic) model from the several theoretical positions (cf. Carver, 1989). Our view is that the dimensions must combine interactively to some extent. There is nothing inherently depressing about attributing an initial failure at some task to some internal cause. It depends on other features of the cause, such as its controllability or stability. Indeed, Abramson et al. (1989) recently implied similar attributional dimension interactions in the context of their discussion of how internal attributions for failure will influence self-esteem only when the attribution is also global and stable. Our recent work provides explicit rationales for several types of interactions and empirical evidence in support of these suggestions (Anderson & Riger, in press). We believe that the RM must address these interaction issues.

Other Points of Disagreement

There are several other points on which we disagree with Peterson or which we think deserve clarification. Although these are of less import than those in the previous section, they warrant careful attention.

Longitudinal Versus Cross-Sectional Data

Peterson and many other scholars have suggested that cross-sectional studies of the attribution–depression relation are less valuable than other designs, particularly longitudinal. Although we agree, we would also like to point out that the increment in value for longitudinal designs is not as great as commonly believed. It depends on the theoretical questions being tested. The vulnerability model does make some specific longitudinal predictions about who should get more and who should get less depressed over time. But it also makes a host of cross-sectional predictions. Current attributional style should predict current level of depression, with particular dimensions predicting particular aspects of depression, because of the long-term operation of the vulnerability. Thus, in testing questions regarding key attributional dimensions (e.g., What *are* the key dimensions?) or their specific links to certain criteria, cross-sectional data are entirely appropriate.

Independence of Attributional Styles for Good and Bad Events

Peterson claims that attributional styles for good and bad events are basically independent. This claim is based on one published study (Peterson & Seligman, 1984) with a fairly small sample size. In our studies, attributional styles for good and bad events are significantly correlated for each dimension. Data from our most recent study, which used a version of the ASAT, are presented in Table 1 (Anderson & Riger, in press). The sample size is more than 650, and each correlation is significant beyond the .0001 level. For example, the correlation between locus attributional style for interpersonal successes with locus attributional style for interpersonal failures was .36. We suspect that users of the ASQ would find similar correlations in large-sample studies, although differences between the ASAT and the ASQ may also be involved.

Table 1. *Correlations Between Attributional Style for Success and Failure, by Situation Type and Attribution Dimension*

Situation Type	Locus	Stability	Globality	Controllability
Success-Interpersonal/ Failure-Interpersonal	.36	.26	.42	.45
Success-Noninterpersonal/ Failure-Noninterpersonal	.37	.30	.58	.52
Success-Interpersonal/ Failure-Noninterpersonal	.31	.31	.37	.43
Success-Noninterpersonal/ Failure-Interpersonal	.29	.33	.46	.36

Note: From Anderson and Riger (in press).

Role of Attributional Style for Good Versus Bad Events in Depression

No one has clearly addressed the role of attributional style for good events. Some articles on RM state that there is a maladaptive attributional style for good events that is essentially the opposite of the maladaptive style for bad events. How this fits in with RM is never specified, however. (This ambivalence is acknowledged in Peterson's target article.) Our reading of the original theoretical statements suggests that if LH (and depression) results from perceptions of lack of control, then the model must predict that perceptions of lack of control for success should relate to depression in the same way as perceptions of lack of control for failure relate to depression. Unfortunately, the model is unclear as to what attributional patterns predict perceptions of uncontrollability of success. Certainly, external attributions are (on average) seen as less controllable. But locus is supposed to be related only to self-esteem. Stability may also be related to perceptions of controllability, but some unstable causes are seen as controllable (effort) whereas others are less controllable (mood). Obviously, changes in the good-events model are necessary, and we are pleased to see the flexibility shown by Peterson and others in thinking about this problem. One simple solution would be to include in the model controllability as an attribution dimension.

One other aspect of the good-events problem warrants comment. Studies using the ASQ have generally found somewhat weaker relations between good events and depression than between bad events and depression (e.g., Sweeney et al., 1986). Our studies using the dimensional version of the ASAT have found the good-event relations to be equal to or slightly stronger than the bad-event relations. Differences between the ASAT and ASQ situations may be involved. The ASQ good events may not be as sensitive, for example. Alternatively, the ASAT bad events may be relatively less effective.

Chauvinism

Finally, we object to the charge that those who urge consideration of dimensions other than those originally proposed are being chauvinistic. Science is a collegial enterprise, with the overriding goal of developing as accurate and useful a way of understanding the world as possible. When data contradict a theory, it is the theory that must give way. As a

colleague keeps reminding us, facts are friendly. In the present case, we would agree that the empirical arguments (i.e., what are the facts?) are far from over. But many of the "facts" lead us to call, as have others, for a closer look at the assumptions of RM and the data purportedly supporting it. We believe the most productive approach would be to modify the model to accommodate the controllability data, but we would be satisfied with appropriate empirical tests. Even if we are empirically contradicted, such tests and related theoretical discussions will improve the field's understanding of the role of attributions in motivational and affective problems. We hope that this exchange will foster such advances.

Notes

We thank P. Paul Heppner for his comments on a draft of this commentary.

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