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# COMMON-SENSE PSYCHOLOGY AND SCIENTIFIC PSYCHOLOGY

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For some years I have been concerned about the issues that arise from the interplay between common-sense psychology (CS- $\psi$ ) and scientific psychology (S- $\psi$ ). The invitation to write this chapter provides a welcome occasion to examine this interplay. The relevant writings are found under such rubrics as “common sense,” “naive psychology,” “ethnopsychology,” “indigenous psychologies,” and “implicit theories.” The issues seem to be of particular interest to social psychologists, and I have drawn on excellent essays by Farr (1981), Fletcher (1984), Furnham (1983), and Wegner & Vallacher (1981). Our area’s interest in these matters has been greatly stimulated by the fruitfulness of Heider’s (1958) analyses of naive psychology. Our interest also reflects the obvious fact that the interplay between CS- $\psi$  and S- $\psi$  involves social processes—the processes by which the common culture affects scien-

tists' thought and activities, and the reverse processes, by which the products of science modify the common culture. These observations notwithstanding, I would emphasize that the problems arising from the interplay are not unique to social psychology. As my examples will suggest, they occur in all psychological research areas that deal with molar behavior and that rely on common language.

## SOME GENERAL ISSUES

### *What is Common-Sense Psychology?*

Presumably we all know what  $S-\psi$  encompasses, but the scope of CS- $\psi$  requires some explanation. Examples will be useful. The first is John Houston's study of "lay knowledge of the principles of psychology" (Houston 1983, 1985). He constructed 21 multiple-choice questions about various memory and learning phenomena. The items were stated in everyday language, dealt with both human and animal learning, and referred to such phenomena as extinction, subjective organization, partial reinforcement, and secondary reinforcement. For example, the item on levels of processing read as follows:

- What should be the best way to think about words if you want to remember them?
- a. to think about what they sound like (e.g. think about where the accent falls, or whether the word has an "r" sound in it).
  - b. to think about what the word looks like (e.g. think about how many syllables the word has, or whether it has any curved letters in it).
  - c. to think about the meaning of the word (e.g. can it fit into your hand, or how pleasant is it).
  - d. all of these ways of thinking about words will lead to equal recall.

Houston first gave this "test" to 50 introductory psychology students before they had been formally exposed to the pertinent principles. On 15 of the 21 items, more students answered correctly than would be expected by chance. To reduce the likelihood that this result was attributable to general test-taking skills or to information gained in other college courses, Houston gave the questionnaire to a heterogeneous sample of 50 people found in a city park on a Sunday afternoon. They answered 16 of the items more accurately than expected by chance. (In both samples, the "levels of processing" item, above, was among those answered correctly more often than expected by chance.) Some caution must be exercised in interpreting "chance" levels for multiple-choice questions, but Houston's results make a strong case for his conclusion that "a great many of psychology's basic principles are self-evident. One gets the uneasy feeling that we have often been dealing with the obvious and did not know it" (1983:207).

A second example comes from questionnaires that Jorge Manzi and I have

given to the members of young heterosexual couples. We asked each member separately to rate their own and their partner's degree of involvement in the relationship. We then asked them to make similar ratings of each one's worrying that the other would leave the relationship, the likelihood that each would leave, and the amount of "say" each has in their relationship. In a parallel identical section of the questionnaire, we told them how two hypothetical people like themselves had rated their respective degrees of involvement (sometimes, high; sometimes, low, etc), and asked them to rate, for each member of that hypothetical pair, degree of worry, likelihood of leaving, and amount of "say." It will be apparent that the purpose of the first set of questions is to obtain their "reports" about their own relationships and that of the second set to obtain their "beliefs" about how such relationships work. (These are the beliefs shared among the respondents in the sample, inasmuch as the correlations between relative involvement and the other variables are calculated over the sample.) Without going into detail, it may be reported that over a number of different samples, the results from the report and the belief data yield essentially the same picture: The more involved partner is both reported and believed to be the one who is less likely to leave the relationship, more likely to worry about the partner's leaving, more likely to take steps to enhance the other's involvement, and less likely to have much say about the relationship's affairs. In other words, these people have beliefs or expectations about relationships that are consistent with what, by their reports, actually happens in relationships. Like Houston's data, these results suggest that the "principles" we derive from the study of interpersonal relationships—in this case, principles about the consequences of unequal dependence—are already part of common knowledge.

[The particular structure of the two-part questionnaire employed by Manzi and myself leads one to wonder whether the report data may not simply reflect the respondents' beliefs, or, conversely, whether the belief data may not reflect their experience in their own relationships—i.e. their reports. We examined these questions in a number of ways (as by varying order of presentation) and could find little evidence of influence in either direction. Taking advantage of the fact that we have two independent reports about each relationship (provided by its two members), we were able to find clear evidence that the reports do reflect a "reality" of each relationship, as defined consensually by the pair. These results bear on an issue discussed much later in this review, as to whether behavioral ratings reflect actual covariations in behavior or simply the semantic or logical relations implicit in everyday language.]

These two lines of work illustrate ways of studying  $CS-\psi$ , and they illustrate what is meant here by the term. From two research areas, we have examples of what Heider (1958) calls "naive psychology": ". . . the ordinary

person has a great and profound understanding of himself and other people which . . . [is]. . . unformulated or only vaguely conceived. . . . “it is expressed in our everyday language and experience. . . [and] will be referred to as common-sense or naive psychology. . . .” (pp. 2, 4). In a similar vein, Smedslund (1978:10) defines common-sense psychology as “the network of concepts pertaining to psychological phenomena, imbedded in ordinary language.”

Here I consider CS- $\psi$  to include common people’s ideas about their own and other persons’ behavior and about the antecedents and consequences of that behavior. These ideas are expressed in the labels and terms that we, as common folk, use to describe people and in the familiar sayings and stories that we tell each other about individuals, kinds of people, and people in general. In short, and as both Heider and Smedslund emphasize, CS- $\psi$  is embedded in and carried by our everyday language.

The foregoing specifies the particular “contents” of common thought that constitute CS- $\psi$ . Several authors (Fletcher 1984; Wegner & Vallacher 1977) have also included the “how,” or the processes of common thought in the CS- $\psi$  domain. I am uncomfortable with this inclusion because CS- $\psi$  then becomes equivalent to all of cognitive psychology. For example, like O’Hare & Duck (1981), I consider attribution theory to be “a psychology *about* common sense,” not common sense itself. Of course, there are everyday beliefs about how people make attributions, and S- $\psi$  may study such common-sense meta-attribution theories. It may also be true, as Gergen (1973) and O’Hare & Duck (1981) have suggested, that S- $\psi$  theory about how attributions are made has been subtly influenced by the uncritical incorporation of certain causal concepts from common thought. For example, my ANOVA model of attribution (1967) makes sense only if “persons” and “situations” are viewed as independent causal factors, and that independence has been repeatedly challenged.

### *The Interplay between CS- $\psi$ and S- $\psi$*

It sometimes serves our purposes explicitly to study “common sense,” as illustrated above by Houston’s and Kelley & Manzi’s work. However, much more frequent and important are the occasions when CS- $\psi$  enters into our S- $\psi$  work implicitly. We all are members of the common culture and users of the common language long before we become scientific psychologists. Insofar as we address our scientific efforts to the behavioral phenomena encompassed by common terms and beliefs, they inevitably influence the concepts and theories we develop for our scientific purposes. CS- $\psi$  affects our work when we communicate verbally with our subjects/respondents, when we interact with one another, and, of course, in our private verbally mediated thoughts.

The above statement emphasizes the effects of CS- $\psi$  upon our S- $\psi$  activi-

ties. This chapter focuses on those effects. It is also obvious that, over time, the ideas and information developed within the  $S-\psi$  realm are likely to influence the terms and beliefs in the  $CS-\psi$  realm. To supplement this chapter the interested reader may wish to refer to the rich literature on these effects. I merely mention a few here. (a) In their writings on “social representation,” Serge Moscovici and his colleagues (e.g. Moscovici 1961; Herzlich 1973) make detailed analyses of how such things as psychoanalytic concepts and medical knowledge have become incorporated into the common domain. (b) In a strong critique, Wallach & Wallach (1983) describe the dangers that  $S-\psi$  serves to legitimize ego-centrism and selfish behavior in the common culture. (c) In a commentary on social psychology’s role, Gergen (1973) emphasizes its probable quantitative impact on common thought and expresses a concern that common people’s acquisition of scientific knowledge will “alter the character of causal relations in social interaction” (p. 310). Schlenker (1974) takes strong issue with Gergen on these points. (d) Krech & Crutchfield (1948) observed that although “many people are impatient with psychology because it does not know enough” (p. 6), perhaps just as many are afraid that it knows or will know too much. This thought is echoed by Kazak & Reppucci (1980) in relation to the study of love: “There seems to be a strong fear that by studying love we will somehow destroy its ‘spontaneity and magical powers’” (p. 213).

The brief comments above may whet the reader’s appetite for essays on the influence of  $S-\psi$  upon  $CS-\psi$ . Here I turn my attention exclusively to effects of the opposite kind.

### *The Extent and Validity of $CS-\psi$*

In all that follows, questions about the extent and validity of  $CS-\psi$  will be in the background, if not at the focus, of my discussion: With respect to what kinds of phenomena may we expect  $CS-\psi$  to be most extensive and valid?

Of course, the validity of  $CS-\psi$  is largely irrelevant to the question of whether  $CS-\psi$  affects human behavior. Whether  $CS-\psi$  squares with empirical findings or not, we may assume that the way people conceptualize and explain behavior affects their behavior. However, validity is clearly an important issue when we find that  $S-\psi$  explicitly relies on  $CS-\psi$  or is subtly influenced by it.

To gain some sense of when  $CS-\psi$  is likely to be extensive and valid, it is useful to speculate about its origins. Like other human intellectual achievements,  $CS-\psi$  is created and transmitted by language-using adults. They have occasions to observe and learn about, at first and second hand, how people behave and the conditions under which variations in behavior occur. They think and converse about these matters, develop and use labels for behavior

and persons, and create stories and aphorisms about important regularities and deviations.

These CS- $\psi$ -generating intellectual and interactional activities occur under a wide variety of conditions. I would suggest that the most important ways those conditions vary are with respect to *level*, *familiarity*, and *personal involvement*. My hypothesis is that CS- $\psi$  is most likely to be both extensive and valid when it refers to events that exist at a middle level (rather than at a macro- or microlevel), that are familiar (rather than alien), and of which people are observers (rather than involved participants).

Space does not permit a thorough justification of my hypothesis, but the factor of level requires some explanation. Most of subjective daily life is carried on at what I am here calling a middle level, or [following Vollmer (1984); see below] *mesolevel*. This is the level of molar individual behavior (the level of planned, goal-directed activity), immediate and direct consequences, time-spans of minutes to days, and face-to-face interaction of small numbers of people. This level is the focus of attention in everyday life, and it provides information that permits conscious and deliberate processing. This also happens to be the level to which most social, motivational, and personality psychologists direct their attention. In contrast, *macrolevel* phenomena involve many people and long time spans (e.g. life-span developmental trends, institutional and historical changes, economic and political trends). *Microlevel* events include what might be described as “molecular” behavior—i.e. events that occur rapidly (in seconds or milliseconds), in small scales of magnitude and mass (e.g. small contractions of the facial muscles or shifts in eye fixation), and often invisibly (e.g. muscle innervations, gland secretions). [See Koffka (1935) for the distinction between molar and molecular behavior.]

Thus one claim of my hypothesis is that people are not very good at drawing valid macrolevel generalizations—i.e. generalizations about events that occur over broad time spans and/or in large populations. Social psychology has repeatedly demonstrated that common beliefs about groups of people—ethnic, national, gender stereotypes—are exaggerated at best, and wholly inaccurate at worst. Such beliefs illustrate errors in the “common wisdom” that occur at the macrolevel with respect to unfamiliar phenomena. Social psychology emphasizes that these beliefs are more strongly fed by outgroup prejudice and ingroup consensus than by factual information.

My hypothesis also asserts that common beliefs are often wrong at the microlevel. A behavioral example is provided by Kaye (1977), who observed that a mother nursing her infant tends to jiggle the child when it pauses during the process. Mothers believe, reasonably enough, that this stimulates the child to return immediately to the nursing. Kaye’s micro-analysis of the interaction process revealed that this belief is in error. Jiggling the infant actually delays

its return to nursing. The erroneous belief here illustrates the inability of mothers to process information with the precision that Kaye's observers, one looking at the mother and the other at the infant, were able to attain. It may also indicate that the psychology of the tiny infant is in some sense "alien" to the mother.

When I described to Greg Schmidt my hunch about the role of mesolevel conditions in the development of CS- $\psi$ , he pointed out that a similar idea has been proposed in evolutionary epistemology. Vollmer (1984) summarizes the point this way:

The world to which our cognitive apparatus was adapted during evolution is but a *section* of the real world. . . . Every organism has its own cognitive niche  $\bullet$  ambient, and so does man. Man's cognitive niche we shall call '*mesocosm*.' Our mesocosm is that section of the real world we cope with in perceiving and acting, sensually and motorially. . . . Mesocosm is, crudely speaking, a world of medium dimensions..." (p. 87). "... our sense organs, perceptual powers, structures of experience, ordinary language, and elementary inferential habits, are well adapted to this mesocosm and are *adequate* for mesocosmic needs. The same is true for our forms of intuition. Our powers of visualization are adapted to and fit everyday needs" (p. 88, emphases in original).

The behavioral categories assumed by CS- $\psi$  are thus well adapted (through evolutionary, cultural, and developmental processes) to the mesocosm constituted by molar behavior and interaction. It is at the mesolevel that S- $\psi$  will encounter an extensive supply of useful (i.e. somewhat valid) categories and theories. It is at the macrolevels of institutional and collective behavior and the microlevels of fine-grain behavior that S- $\psi$  is least likely to find useful CS- $\psi$  concepts and most likely to disconfirm CS- $\psi$  beliefs.

### *The Effects of CS- $\psi$ on S- $\psi$*

In discussing these effects, it is convenient to refer to certain components of the two realms that are parallel or analogous. The *scientific concepts* of S- $\psi$  parallel the *common terms* of everyday language; concomitantly, running parallel to *common beliefs* within the folk or popular culture are *scientific propositions* about how the phenomena related to certain concepts covary or are causally linked.

In the following sections, I first consider two parallel phenomena: (a) How common terms affect scientific concepts and (b) how common beliefs affect scientific propositions. Finally, I consider a more complex case, illustrating (c) how common terms may affect scientific propositions. Most of my examples come from social and interpersonal psychology. However, I emphasize again that the relevant phenomena are not unique to social psychology. They appear in all the fields of psychology that deal with molar behavior and use information available for encoding into everyday language by common people.

## SOME SPECIFIC ISSUES

### *How Common Terms May Affect Scientific Concepts*

Like any psychologist who has begun work in a new area, I have encountered the problem of terminology. How shall I denote the concepts particular to my work? The problem always sends me scurrying to the dictionary in search of common language that might work, but it also often makes me toy with the idea of inventing a new term (see below). Occasionally I later find that a term chosen earlier was not the best one: It has unintended implications, it is difficult to explain to readers, etc, but I am now stuck with it. We encounter similar problems as readers of technical terms that other psychologists have adopted. They do not have the proper connotations for us. We have to remind ourselves and our students that they are "not quite right." We can think of a better term and wish the earlier writer had used it instead.

The question of common terms also arises when we operationalize a concept. In research that relies on verbal reports, the fact that our scientific concept has been drawn directly from the common vocabulary tempts us to use it in our questions. For example, how might we assess the "commitment" to each other of two partners in a close relationship? An obvious way is to ask them. Sharing the term with them ("commitment" occurs in the ordinary speech of our typical respondent), we simply ask, "How committed to this relationship are you?" Our respondents rarely ask what "comitted" means, probably because they assume that we expect them to know the term.

During development, use, and operationalization of  $S-\psi$  concepts, common language is both a help and a hinderance. On the plus side, it suggests the categories into which our phenomena might usefully be sorted, it provides convenient terms for thinking about those categories, and it enables us to tap into the store of information our respondents may have about the phenomena. On the negative side, common terms may not sort things out in the most precise ways, may tempt us lazily to skip over preliminary conceptual analysis (i.e. to turn that job over to our respondents), and may encourage us to rely too greatly on verbal reports (thereby possibly delaying development of methods that could replace them).

These competing considerations leave most of us ambivalent about using common terms. Although uncomfortable with reliance on them, few of us are willing entirely to turn our backs on them. For example, in measuring "investment" in a relationship, Rusbult (1980a,b) asks questions about specific things "put into" the relationship (time, money, shared possessions, mutual friends), but she also uses the target concept itself ("... what is the size of your investment in this relationship?"). The latter is sometimes used as a marker variable, to determine whether the concrete measures are all tapping the desired common concept. In a similar manner, Berscheid et al (1989) used

their concrete measures of relationship “closeness” (detailed reports of time spent with the partner, number of activities done alone with the partner in past week, and degree to which respondent had been influenced by the partner in various decisions), but also a “subjective” measure that required the respondents directly to estimate the closeness of their relationship. This study is of special interest here because the investigators have a theory about the limitations of the common-sense concept of closeness. Their idea is that the common concept includes a strong component of “positive feeling,” which was not a part of the phenomenon conceptualized as “closeness” by the investigators. Berscheid et al were not surprised to find that the subjective measure was only marginally related to the aggregate of the concrete measures. Thus, even though people’s extensive knowledge about their own relationships makes it reasonable to ask them to judge their “closeness,” from the perspective of Berscheid et al this question is inappropriate because the lay conception of the term is askew from what theory indicates to be correct.

In operationalizing a concept, psychologists typically try to develop a set of measures or questionnaire items, working from the common term and trying to express or exemplify it in a variety of ways. A common procedure is to obtain ideas from prior research, one’s colleagues, and lay persons themselves. For example, in developing scales to measure “love” vs “liking,” Rubin (1970) scanned writings on love and research on interpersonal attraction. He then asked panels of students and colleagues to sort the items into love and liking categories.

This reasonable procedure sometimes has the unnoticed and unwanted result that the set of indicators comes to include variables measuring the antecedents and/or consequences of the focal variable. For example, Rubin’s (1970) liking scale includes not only such direct items as “\_\_\_\_\_ is one of the most likable people I know,” but also items about “\_\_\_\_\_’s intelligence and similarity to self” (which other investigators might wish to examine as antecedents of liking) and about “voting for \_\_\_\_\_ in a class or group election” (which other investigators might consider to be a consequence of liking).

This inclusion of antecedents and consequences reveals how the network of associations that exists around a common concept includes not only semantic synonyms but also an implicit theory about the causal network in which lay persons assume the referent phenomenon to exist. I develop this point further below. Here, we may note that an omnibus instrument intended to measure a focal concept may (a) pick up what other investigators, pursuing particular causal hypotheses, may wish to exclude; (b) have items in common with scales measuring different concepts; and (c) lose validity through the effects of irrelevant causal factors on certain component measures. (For example, in a measure of interpersonal attraction, items concerning the amount of time

one person spends with another—a presumed consequence of attraction—may reflect opportunities rather than positivity of attitudes.)

Another locus of ambivalence in the process of selecting *S-ψ* concepts is at the point of deciding whether to adopt a common term or develop a special “scientific” one. New terms are often necessary, precisely to enable us to escape from the grip of common terms that have inappropriate connotations. On the other hand, the invention and use of special terms opens *S-ψ* to the charge that it proliferates “scientific jargon.” This charge implies that the terms are unnecessary because they could readily be replaced by more common ones and that the new terms are motivated by a desire to create the mere trappings of “science.” For these various reasons, throughout our *S-ψ* work we often experience a tension between using common terms that are moderately appropriate for the job and inventing more precise scientific ones. Of course, one hopes that uncritical use of common terms or the phoniness of pseudo-scientific concepts will be exposed through scientific interaction. A more formal approach to some of these problems is provided by prototype analysis.

**PROTOTYPE ANALYSIS: A SYSTEMATIC PROCEDURE FOR MOVING FROM COMMON TERMS TO SCIENTIFIC CONCEPTS** This method is based on Eleanor Rosch’s (1978) research on category systems. Her theory is that category systems have both a horizontal and vertical dimension. For example, the category defined by the concept “love” includes (horizontally) a number of different examples—different manifestations of love. Those instances are differentiated as to how well they represent the category, ranging from instances that are prototypic of the category (e.g. caring) to other instances that are less so (e.g. protectiveness). The concept also exists (vertically) within a hierarchy comprised of a higher-order concept (e.g. attraction) that includes it along with other concepts (e.g. not only love, but also liking and respect) and of lower-order concepts included within it (specific kinds of love, such as romantic love or filial love).

Assessment of the horizontal dimension is illustrated by Buss & Craik’s study of the category of “dominance” (1980). In creating items for a personality scale, they asked an initial sample of ordinary people (undergraduate students) to think of the most dominant people they knew and to describe the behaviors that illustrate their dominance. A reduced version of the resulting long list of examples was then given to a second sample, who rated how good an example of “dominance” each item is. By this procedure, the items considered most characteristic of the category were identified. These prototypic items included the following:

He forbade her to leave the room.  
She demanded that he run an errand.

He assigned roles and got the game going.  
 On the auto trip, she decided which direction to take when they got lost.

Prototype analysis of category content is a formalization of methods researchers have long used for assembling test items. It provides a more systematic understanding of a concept than do the earlier methods. This procedure has been used with a number of different concepts and for a variety of purposes. In general, the goal is to extract from CS- $\psi$  the essence of everyday terms that lend themselves to S- $\psi$  uses. For example, in his study of the concepts of intelligence, creativity, and wisdom, Sternberg (1981, 1985) points out that the prototype analysis reveals in detail what people mean when, in judging each other, they use these terms. He also describes how the analysis may provide a basis for broadening the scientific concept. He found, for example, that common people place more emphasis on "the practical and worldly side" of intelligence than do typical tests of the construct.

Fehr (1988) compared the prototypic features of love and commitment in order to evaluate several current S- $\psi$  views about their interrelation. Thus, Fehr used the common categories to evaluate competing scientific views. Other researchers might not be comfortable with this implied dependency of scientific categories upon common categories, and might wish to have their theoretical ideas play a stronger role in setting up the S- $\psi$  categories. For example, certain S- $\psi$  theories of commitment view it as having two components, internal constraints (e.g. loyalty, living up to your word) and external constraints (e.g. social arrangements that keep one "trapped" within a relationship). Fehr's data show that the latter are rather peripheral to the common category, with "feeling trapped" having low ratings on prototypicality. Do these S- $\psi$  theories require a different term or might the common-sense category benefit from some theory-based reshaping?

Prototype analysis is useful for introducing CS- $\psi$  terms into S- $\psi$  discourse, but my impression is that the theory and procedures have not yet been fully exploited for this purpose. Explorations of the horizontal dimension have outpaced work on the vertical dimension.

An example of work on the vertical dimension (in this case, identifying the subcategories within a broad category) is provided by my own study of "dominance." When I examined the results of the Buss & Craik (1980) prototype analysis of "dominance," it struck me that the instances rated as most prototypic (see above) included two distinct types of phenomena that, on theoretical grounds, I would want to differentiate: Some of the prototypical items (e.g. the two listed first above) imply a promise-threat scenario and others (the two listed last) an initiative-taking scenario. [These two scenarios are based in two contrasting patterns of interdependence which, according to the Thibaut & Kelley (1959) theory, are basic in interpersonal relations.]

I tested my impression by having students evaluate the items Buss & Craik

had found to have the highest prototypicality ratings for dominance. This time the subjects were to judge whether each item most closely conformed to a schematic threat-promise scenario ("Person used threat or promise, implicitly or explicitly, to induce other to do something the person wanted") or to a schematic initiative scenario ("Person acted first, pre-empted, took initiative, and expected other to follow—to coordinate with, accommodate to person"). Of the items tested (which had previously been judged most prototypic of dominance), some were now judged to illustrate the threat-promise scenario and others the initiative scenario. Perhaps more important is the fact that when the schematic scenarios and a sample of Buss & Craik's items were rated on prototypicality for "dominance," the schematic items received ratings as high as the best of those derived from everyday examples of the concept. Thus it may be important to dissect common categories, rather than taking them as they come. This is what is involved in analyzing the vertical dimension of a category system. Such dissection can be aided by viewing common conceptual categories through the lenses of  $S-\psi$  theory. For example, theory can be used to construct conceptually prototypic examples which, like my schematic scenarios, can then be used to assess and compare the common-sense categories.

I am expressing here some uneasiness about undue dependence on common thought for clues about how  $S-\psi$  should slice up its phenomena. There must surely be an important role for  $S-\psi$  analysis that enables our conceptual work to come partially under the guidance of logical and theoretical considerations and to avoid total dependence on common terms. It would be unfortunate if the elegance of such procedures as prototype and hierarchical analysis led us to focus our attention on empirical analysis of common terms to the exclusion of theoretical analysis.

### *How Common Beliefs May Affect Scientific Propositions*

In their formulation of theoretical propositions, social and personality psychologists are influenced, implicitly or explicitly, by their own everyday experiences and by ideas that are part of the common culture. The terms in their hypotheses are based partly on common usage (as discussed above), and the postulated relationships among variables are influenced by informally observed and socially labeled covariations. The linkage between the  $CS-\psi$  and  $S-\psi$  domains revealed by the latter process raises many issues. I consider two below.

THE QUESTION OF "OBVIOUSNESS" "Psychology, it seems, is a science which specializes not in discovery but in re-discovery." Joynson's crisp observation (1974:34) provides the theme for this discussion. To the degree  $S-\psi$  draws its propositions from the mesolevel of everyday experience, com-

mon people will regard it as revealing facts they already know. (It may be noted that the pejorative label “obvious” reflects people’s “social representations” of science—the belief that science is an enterprise that specializes in revealing the unknown.) And in any case, whatever the source of  $S-\psi$  hypotheses, the vast extent of common knowledge at the mesolevel makes it probable that many scientific facts adduced at that level will be recognized by common people. One consequence of this recognition is that doubts are raised about the value of the  $S-\psi$  enterprise: It reveals no new information, only what people already know.

In social psychology we have a term for what is already known about human behavior—namely, “bubba psychology.” “Bubba” (sometimes spelled “bubbe”) is Yiddish for “grandmother.” One’s grandmother can recall a rich store of folk sayings and stories. When her grandson describes the findings from his recent doctoral dissertation, she readily assimilates them to some idea or theme from her experience, and may sarcastically inquire, “So what else is new?” Some social psychologists try to avoid doing “bubba psychology.” They’d generally like to surprise their grandmothers.

Of the many things to be said on this matter, I merely raise several interrelated points.

*What is “obvious” is not obvious* One wonders whether a person who labels a proposition “obvious” could have explicated the hypothesis in advance. And might not that same person have found the counter-proposition “obvious” as well? In describing how social scientists other than psychologists are sensitive to the charge that they never discover anything new, Farr (1981) writes:

Stouffer, for example, found that when he reported his survey findings concerning the opinions of American soldiers during World War II, military commanders typically responded by claiming that they already knew the information which he was reporting. On one significant occasion in his oral presentation he reversed all of his actual findings and met with the same response. Lazarsfeld . . . played a similar ruse on the readers in one of his articles. . . . The lesson which one can draw from these two relatively minor incidents concerns the versatility of the human listener in being able to ‘make sense’ both of actual data and of the opposite of these data” (p. 306).

Several phenomena are involved here. More is obvious in hindsight than in foresight (Fischhoff 1975). And as we will see below, common sense encompasses beliefs on both sides of many issues. We may also note that what is “obvious” undoubtedly changes over time (Brickman 1980), as people acquire new understandings of the principles of human behavior.

*Common-sense beliefs are self-contradictory* This fact accounts in part for the ease with which the charge of “obviousness” is leveled at generalizations about molar behavior. Common understanding encompasses numerous con-

contradictory principles. One common textbook strategy for immunizing student readers against uncritically rendering judgments of “obviousness” is to present a list of common aphorisms that are contradictory, for example, “birds of a feather flock together” versus “opposites attract,” “you can’t teach an old dog new tricks” versus “never too old to learn,” and “look before you leap” versus “he who hesitates is lost.”

The fact that mesolevel CS- $\psi$  is replete with contradictory ideas should probably be examined in the light of comments in S- $\psi$  to the effect that “All reasonable hypotheses are likely to be valid” (Gergen 1978:521), or, as McGuire expressed it in his seventh koan, “The opposite of a great truth is also true” (McGuire 1973:455). Both CS- $\psi$  and S- $\psi$  often identify relationships among variables that hold only under a restricted set of circumstances; the total circumstantial domain includes locations at which the opposite relationships hold.

*The consequences of concern about “obviousness”* To avoid the “obvious” one can seek out combinations of circumstances at the mesolevel that produce counterintuitive effects. In its reaction to the charge that it was “bubba psychology,” in the early 1960s social psychology developed a strong theme—one might say an ethos—of “demonstrating the non-obvious.” This was most notable in research on cognitive dissonance theory—in the demonstrations, for example, that under certain circumstances, small incentives produce greater effects than large ones.

Social psychology’s focus on the “non-obvious” has had many consequences: It has (a) generated excitement in the field, (b) identified unusual combinations of conditions that produce out-of-the-ordinary relationships between variables, (c) produced debates and counter-research on the plausibility of alternative, more “obvious” interpretations of findings, and (d) caused the development of various implicit strategies for lending the appearance of non-obviousness to research (e.g. by imputing false or oversimple beliefs to “common sense” in order to provide a straw man that the research can then demolish, or by creating gaps between the conceptualization of the work (cast in non-obvious terms) and its operationalization (rather obvious when stripped of its technical jargon). Finally, one might argue that the ethos of “non-obviousness” has played an important role in the reductionistic shift in social psychology—the shift from studying group, interpersonal, and molar behavioral phenomena at the mesolevel to studying molecular behavior and cognitive and emotional mediating processes. In a sense, the quest for a result that will surprise Bubba has produced work at the microlevel about which she is poorly informed and by which she is easily impressed. This shift occasionally worries me. I fear that widespread reductionism runs the risk of our losing sight of the structure of the molar processes for which mediational

analyses are needed; that is, I worry that the molar phenomena will no longer “set the agenda” for the mediational work. However, the agenda is probably sustained implicitly both by the prevalence of knowledge about molar phenomena, and by possession of this knowledge by even those scientists most inclined to reductionism.

*How to make science interesting* In a marvelous paper to which Dan Wegner referred me, Murray Davis (1971) argues that a theorist is considered great not because the theory is true but because it is interesting. “All *interesting* theories . . . constitute an attack on the taken-for-granted world of their audience” (p. 311, emphasis Davis’s). If a theory does not challenge their existing assumptions and practices, the audience will reject its value even while affirming its truth. The general form of an interesting proposition, according to Davis, is that it states (or implies) what *seems* to be true of a particular phenomenon and then specifies what *is* true. For example, Festinger’s central proposition in cognitive dissonance theory might be paraphrased as “What seems to be important in behavior is the pre-decision process; what is in fact important is the post-decision process.” From Davis’s perspective, the genius of great theorists lies in their ability to recognize the “assumption-ground” of their audience and to place their propositions in a refutative position relative to that ground.

Davis’s analysis provides a fascinating view of the problems of developing and presenting our propositions in ways that will interest our colleagues, students, and lay audiences. The analysis suggests both how to make our work truly more interesting (e.g. through sensitivity to the assumptions of our audiences) and how to make it *appear* more interesting (e.g. crudely illustrated by certain common writing gambits: “It has long been thought . . . But that is false . . . We have seen instead that. . .”).

*The importance of studying the “obvious”* When all is said and done, there are good—even (by Davis’s criteria) interesting—reasons for studying propositions that seem “obvious.” The contradictions found among common beliefs suggest that common sense is more attuned to the main effects of variables than to their interactions. This implies that an important task for S- $\psi$  research is to reconcile the contradictions by identifying the conditions under which the relation corresponding to a particular belief occurs and the contrasting conditions under which its opposite appears. The relevant “interesting proposition” prototype, from Davis’s analysis, is “What seems to be disorganized is, in reality, organized.” The scientist also often identifies background factors which, overlooked in the contrasts that common observers make, can be shown by broader comparisons to be important (Cheng & Novick 1990). The relevant Davis prototype is “What seem to be assorted

heterogeneous phenomena are, in reality, manifestations of a single element.” The classic example is provided by Newton’s “interesting” insights into the motions of apples and planets. The detection of organization underlying apparent disorganization and of singularity underlying apparent heterogeneity should be viewed as special cases of  $S-\psi$ ’s broader functions of systematizing and quantifying common sense. The systematization consists of developing general theory that reveals the framework within which the piecemeal ideas of common sense are located. This was clearly Heider’s goal in his analysis of naive psychology. I should also emphasize the gains that science makes by quantitative measurement of variables and by description of the relations among them in quantitative terms. The level of detailed observation and assembly of information necessary for developing integrative frameworks and precise quantitative formulations is what sets the  $S-\psi$  enterprise apart from everyday experience and enables  $S-\psi$  to begin with but then rise above  $CS-\psi$ .

**RELIANCE ON BELIEFS AS DATA ABOUT REALITY** To the degree common beliefs reflect accurately observed and encoded experience, whether in each individual’s direct experience or in the culture’s collective experience,  $S-\psi$  can rely on those beliefs as information about reality. This possibility holds forth many possible benefits, including economy of research, information about private and otherwise unobservable events, and information about dangerous and disturbing events that it may be unethical to create or observe.

Several different data sources are implied here, ranging from (a) people’s *reports* of what they and/or their associates do or have done in certain situations; (b) people’s *predictions* of what they or acquaintances would do in certain situations, which may be common or “hypothetical” ones; and (c) people’s *beliefs* about what certain kinds of persons do or would do in certain situations. The latter source involves procedures variously referred to as “hypothetical situations,” “vignettes,” “scenarios,” or “simulations.” In general, as we move from source *a* to *c* in the above list, our method shifts from reliance on memory-based reports of experienced events to reliance on beliefs about “what people do,” this last being the belief component of  $CS-\psi$ .

As suggested by an analogous but unresolved controversy in social psychology about the relative merits of laboratory experiments and role-playing procedures, we are not yet in a position to render confident judgments about the relative merits of these various procedures. However, it may be helpful to consider an example of the use of scenarios to gain access to thoughts and feelings that mediate behavior. The example is provided by Weiner’s (1980) scenario-based research on helping behavior. The procedure used situational descriptions based on the naturalistic experiment of Piliavin et al (1969). Subjects were asked to imagine themselves in a subway car, seeing a person, who was apparently either ill or drunk, stagger and fall. They were then asked

to rate the causes of the falling, what they would feel toward the victim, and whether they would be likely to go to the victim's aid. In other cases, the scenario procedure enabled Weiner to ask subjects to assume that they are experiencing certain feelings or thoughts about causality, and then to predict their degree of helping. The procedure yields results confirming Weiner's hypothesis that helping is mediated in part by a particular chain of attribution and emotion—one in which the victim is seen as *not* responsible for the problem and is regarded with pity. (Weiner does *not* assume that helping in such situations is solely mediated by attributions or "reasons".)

One is hard put to propose alternative, practicable ways to investigate such important mediational hypotheses. Yet one wonders about the relevance of the scenario-derived metacognitively mediated beliefs to the processes occurring in the course of the "real" situation. In response to such doubts, Weiner points out that the scenario simulation produces patterns of anticipated behavior (helping or not) that correspond to those actually observed by Piliavin et al.

As a basis for thinking about this problem, I return to my earlier hypothesis, that common beliefs are most likely to be veridical when they concern the mesolevel of behavioral phenomena, the familiar, and those events of which the person has principally been an uninterested observer. Helping a victim is a molar event and one with which we typically have had some first-hand and considerable second-hand experience (e.g. in what our elders have told us about when people should and do provide help). Beliefs about that behavior and its occurrence under various conditions should thus be fairly veridical. My doubts would be directed more at beliefs about the mediating processes, those being at the microlevel (fast moving, transient, not easily accessible). We might consider the possibility that in situations of the sort constructed by Piliavin et al, helping may typically be controlled by mediating processes that entail rules of thumb, learned habits, unconditioned reflexes, and such. These fast-acting mediators, well adapted to the exigencies of unanticipated and quickly developing situations, may well reflect the more "reasonable" (i.e. mediated by "reason") considerations picked up in the beliefs that the helping scenarios tap. Thus the scenario procedures may give the same "behavioral" results as the experiment, and the scenarios may also suggest the adaptive underpinnings of the actual mediational processes. However, those processes may involve shortcuts, response habits, etc that carry out the adaptive logic but do not directly involve it.

### *How Common Terms May Affect Scientific Propositions*

S- $\psi$ 's postulates about relations among variables may be influenced by explicit common beliefs about the causes and consequences of behavior. However, the propositions we develop may also be affected by relationships

*implicit* in everyday terms. The network of meaning in which common terms are embedded always includes subtle ideas about what goes with what and what causes what. Figuratively speaking, common psychological terms “reach out” in various directions, carrying with them other, associated terms and often an implicit theory about a causal sequence in which the referent phenomena occur. Their very usage induces people to think of patterns of associated meaning and of simple models of causality.

Two types of critiques leveled at the products of  $S-\psi$  stem from the phenomena just described. Both types assert that certain relationships that  $S-\psi$  takes as facts about behavior have their true source in common terms and their implications. The critiques claim that (a) patterns of semantic similarity have been taken to represent patterns of real behavior, and (b) the causal relationships implied in common concepts have formed the basis for “laws of behavior” that are in fact unfalsifiable.

**PATTERNS OF SEMANTIC SIMILARITY** This criticism was initiated by D’Andrade (1965) and vigorously advanced by Shweder (1975, 1977), both anthropologists. An example of their research is provided by Shweder (1975). Common people (students naive about the relevant  $S-\psi$  theory) are asked to judge the conceptual similarity between pairs of items from a personality or behavioral rating scale. Shweder then shows that the matrix of average similarity judgments (the *Conceptual Similarity Matrix*) corresponds rather closely to the matrix of inter-item correlations obtained when the scale items are used to obtain ratings of behavior (the *Rated Behavior Matrix*).

From some ten analyses of this sort, Shweder and D’Andrade conclude that the rating-based data on which  $S-\psi$  often relies reflect semantic similarity rather than behavioral covariation: “. . .the factors described in ratings are the property of trait terms as linguistic elements without being the property of the persons described by those terms” (Shweder 1975:457).  $CS-\psi$  judgments of “what *is like* what” are responsible for inferences made in  $S-\psi$  about “what *goes with* what.”

This critique was the center of a heated controversy in the 1970s, and the technical issues are too complicated to present here. Perhaps the final round of the debate was staged in papers by Block et al (1979) and Shweder & D’Andrade (1979). It now seems clear that (a) the critique does not cut the ground out from under individual-difference research, with its broad set of measurement and validation strategies, but (b) it undoubtedly has something important to say about the factors that contribute to the *memory-based* ratings (including self-ratings) commonly used in personality and social psychology.

The participants in this controversy have recognized that it raises but leaves unanswered the question of where the  $CS-\psi$  conceptual system comes from. This question takes us into a running philosophical debate in which “realist”

vs “constructionist” positions are counterposed. The latter position is expressed by Shweder (1977): “What is disputed is that the *categories* . . . into which people sort themselves and others can be induced from experience. Cultural *constructs* are not empirical generalizations” (p. 938, emphases in original). The realist position is expressed, though more moderately, by Block et al (1979): “Our own view is that although cultural constructs need not be evolved from living in the world, they very often are. . . . [I]n- individually evolved and yet consensual structurings of interpersonal experience provide the basis, we suggest, for many learned cultural constructs” (p. 1071). It must be emphasized that the realist-constructionist issue is independent of the critique summarized above. Common sense categories may, in general, reflect the structure of an underlying reality; but any particular data set (e.g. a set of behavioral ratings) may be only poorly anchored in the corresponding “behavioral reality.” Such an individual data set may instead be largely influenced by the common-sense category system.

**CAUSAL IMPLICATIONS OF CONCEPTS** The issue here is closely related to the preceding one except that the focus is on the causal sequences implied by particular terms. Psychologists have studied the causal theories implicit in common terms. Examples include Au’s (1986) research on the causes and consequences of events described by interpersonal verbs, and Shaver et al (1987) have noted the similar implications of common terms for emotions.

From an implicit recognition of the causal networks associated with common terms, both Smedslund (1978) and Ossorio (1981a,b) have written critiques of *S-ψ*. In essence, both argue (and I’ll use Smedslund’s statement of the position) that theories in psychology are often merely “explications of conceptual relationships imbedded in ordinary language (common sense). This conceptual network is *anterior* to both observation and theorizing. [In an] analogy between the tasks of pre-Euclidean geometry and contemporary psychology . . . [both] tasks are seen as involving explication of our implicit concepts of respectively space and people. One consequence of [this] view is that much psychological research is pointless since it attempts to verify logically necessary statements by empirical methods” (Smedslund 1978:1, emphasis in original).

Ossorio (1981a) illustrates his critique by examining the *S-ψ* formula “Frustration leads to aggression,” which he reformulates as, “Provocation by O elicits a correspondingly hostile response by P.” This, he asserts, “represents neither an empirical discovery nor a stipulative definition. Instead, it is a partial formulation of our familiar four-thousand-year-old concept of anger” (p. 49). In this, Ossorio implicitly recognizes the causal network implied by the term “anger,” shown by Shaver et al’s (1987) prototype analysis to include provocation and hostile responses as antecedents

and consequences, respectively. Ossorio's point is that a  $S-\psi$  proposition such as that above is a "non-empirical formula." "What is not empirical is the content of the formula and the logical interconnections among the elements of the formula..." (p. 52). Empirical research on such formulations is appropriate only to establish "their range of effective applicability."

Smedslund's critique can be illustrated by his translation of Bandura's (1977) self-efficacy theory into a set of common-sense theorems (Smedslund 1978). For example, Bandura's proposition that "The strength of people's convictions in their own effectiveness is likely to affect whether they will even try to cope with given situations" Smedslund restates, in part, in these terms: "If P wants to do something and believes with complete certainty that he can do it, then P will try to do it." This theorem, Smedslund notes, is "logically necessary" by virtue of the implication, built into the word "trying," that P both wants to do something and believes he can. The causal implications of the concept "trying" link it back to the two antecedents of "wanting" and "belief in ability." To describe trying as a consequence of wanting and belief is simply to provide what all users of the English language would regard as "an acceptable explanation." The proof of the theorem is found in the fact that the alternative assertion, that trying reflects a person's not believing and/or not wanting, would not be considered an acceptable explanation.

Smedslund's rendering of  $S-\psi$  theories involves stripping the common terms of the technical jargon in which they have been clothed, and then showing that the (simpler, more naked) propositions follow from the implications of those common terms. Like Ossorio, Smedslund assigns only a limited role to empirical research in relation to the "logically necessary theorems." It may provide reminders of the complexities of human behavior, yield assessment of practical procedures implied by the theorems, and determine whether the theorems apply to a particular set of real circumstances.

These critiques of  $S-\psi$  theories are essentially sophisticated ways of labeling them "obvious." As a consequence, Bandura's (1978) brief reply to Smedslund's critique is, in essence, a summary of the most weighty responses to the charge of "obviousness," implied in my earlier discussion.

Smedslund notes the strong similarity between the simple terms he finds useful and those identified by Heider (1958) in his "naive analysis of action." For example, Smedslund's argument, outlined above, is essentially a reprise of Heider's schematization of the relations among "intention," "can," and "trying." However, there are some sharp contrasts. Heider explicitly recognized that the implicational links are generated by the causal connotations of  $CS-\psi$  terms, describing these terms as "fundamental concept[s] . . . linked with causation." Furthermore, Heider viewed himself as contributing to the development of  $S-\psi$  theory where Smedslund is, in some sense, debunking it. Heider's analysis was framed to provide a foundation for theories such as

Bandura's, and in fact, it has provided the starting point for Weiner's (1986) attributional theory of motivation and emotion. Weiner's work shows how it is possible for *S-ψ* to refine and organize the common-sense notions, by explicitly and systematically analyzing the categories and dimensions of causality they imply. This suggests that *CS-ψ* can become a foundation for *S-ψ* theory. The creative work here lies in analyzing *CS-ψ* and revealing its underlying framework. Once any such theory is completed, we should hardly be surprised that, taken separately and viewed from the *CS-ψ* perspective, most of the specific *S-ψ* propositions will appear to be truisms.

An overview of the common thrust of Heider's, Ossorio's, and Smeds-lund's work is provided by Shotter & Burton (1983). They see this work as "concerned with constructing formulations within which to describe the structure of social behavior systematically." They designate the work as "descriptive formulation research, to emphasize that the main activity involved is a back and forth productive process working between explicit formulations and the implicit social knowledge such formulations are meant to specify and describe" (p. 272). This locates the effort squarely in a symmetrical interplay between *CS-ψ* and *S-ψ*. As to the value of the work, Shotter & Burton advance the provocative (and in my view, dubious) qualification that, being characterized by an ineradicable vagueness, *CS-ψ* is "a source of indefinitely many formulations. . . ." Thus the formulations of the three theorists discussed above are to be regarded "as exemplary and not as in any way definitive" (p. 278.) They write that Heider "is not *clarifying concepts* as he says, but is *constructing idealizations*, [because] . . . formulation involves the further specification of what is initially and intrinsically vague" (p. 279; emphasis in the original).

## CONCLUSION

In thinking about the overall influence of *CS-ψ* on *S-ψ*, I come to the same ambivalent conclusions as have many authors who have preceded me: It is impossible for us to avoid the effects of *CS-ψ*, but easy for us to be unaware of them. These effects provide us with both opportunities and risks. As Fletcher (1984) writes, "common sense is a valuable but inherently dangerous resource available to psychologists" (p. 203).

It is easy to overlook the risks entailed in the many *CS-ψ*-to-*S-ψ* linkages. However, to do so introduces ambiguity and confusion into our work. We must be more explicitly aware of the effects and more analytic in thinking about them. This chapter describes many instances of such awareness and various empirical and conceptual approaches to analyzing the linkages.

I have become convinced that despite the large literature bearing on the issues described in this chapter, they deserve more widespread attention than they presently receive. For example, although I am uncertain about where it

would fit into their curriculum, I would think that graduate students should be sensitized to how CS- $\psi$  intrudes into the early, fundamental stages of research—the stages at which we define our concepts and formulate our hypotheses.

The inevitable effects of CS- $\psi$  on S- $\psi$  are neither all good nor all bad. Proposals that we break entirely free from CS- $\psi$  are misguided. Discarding our CS- $\psi$  “baggage” would require us needlessly to separate ourselves from the vast sources of knowledge gained in the course of human history. And in any case, such suggestions are unrealistic: The thoughts, writings, and conversations of human researchers are heavily saturated with common language. Common-sense psychology constitutes both a bondage and a heritage for scientific psychology. Like all that we inherit, we have little or no choice in the matter. And like other inheritances, at the same time that it constrains and creates problems for us, it provides a useful and potentially rich foundation for development and growth.

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