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New Issues in the Study of Infant Categorization: A Reply to Husaim and Cohen*

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Husaim and Cohen's focus (*Merrill-Palmer Quarterly*, 1981, 27, 443–456) on the learning of ill-defined categories by infants is securely motivated. Still, some of the particular questions they pursue—namely, how many dimensions are used to form the categories and what is the salience hierarchy of the dimensions—are tricky and perhaps misleading. Underlying their design and analysis is the basic assumption that the dimensions or attributes of the stimulus as defined by the experimenter have psychological reality for the infants. This assumption is questioned. Infants may perceive different attributes in the stimulus or they may not articulate the stimulus into attributes at all.

The insight that many natural categories are structured by family-resemblance relations rather than based on selected defining attributes has provided an important new perspective for the study of the categorization process. Husaim and Cohen's (1981) paper represents one of the pioneering efforts to investigate the process of forming ill-defined categories in human infants. Like most ground-breaking efforts, the work raises at least as many questions and problems as it addresses. Accordingly, the primary purpose of my comments will be to direct attention to some of the hidden assumptions and controversial lines of argument in the paper so as to make more explicit the issues that may frame future analyses and investigations.

The motivation for studying infants' learning of ill-defined categories is uncontestable. Not only is it clear that many natural categories of human adults have a family-resemblance structure (Rosch & Mervis, 1975), but it is also clear that such a structure is particularly characteristic of the categories that children name early in language acquisition (as opposed to many categories that reflect technical concepts, which have a defining-attribute structure, and which are

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learned primarily at later developmental levels). It is certainly not difficult to believe that the processes that guide the acquisition of categories based on family resemblance may be different from the processes that guide the acquisition of categories based on selected defining attributes both because different formal models provide optimizing descriptions of the acquisition of these two types of categories and because, empirically, the acquisition of real-world categories based on the two structures occurs at least in part at different levels of development. If the acquisition of categories outside the laboratory is wholly or primarily the acquisition of family-resemblance categories, then laboratory studies of infant categorization should focus on these categories, as Husaim and Cohen do.

Until recently, the laboratory study of categorization—across developmental levels—has been dominated by investigations of the acquisition of categories based on a small number of defining attributes, most frequently one. The acquisition of this type of concept has been the focus of the majority of theoretical efforts in the field. Thus, it is not surprising that some of the familiar questions that have framed theoretical controversies concerning the acquisition of well-defined categories have reappeared in the new literature on the acquisition of ill-defined or family-resemblance-based concepts. Such questions are major foci of Husaim and Cohen's paper—namely, how many dimensions are used (by infants) in categorization, and what is the salience hierarchy of the dimensions—the kinds of questions posed many times in the older literatures on attention in learning and on the nature of hypothesis-testing strategies (e.g., Trabasso & Bower, 1968).

I will argue that these questions are not only trickier to answer than Husaim and Cohen's analysis suggests, but that they also might be misleading kinds of questions to pursue in the study of infant categorization. Both arguments center on the problem of assuming that the dimensions or attributes of the stimulus defined by the experimenter have psychological reality for the learner, in this case a 10-month-old infant. One way such an assumption can be wrong is that the dimensions declared by the experimenter may be different from the dimensions perceived by the learner. This may be explicated simply by reference to Husaim and Cohen's stimuli, two of which are shown in their Figure 1. These authors consider there to be four variable attributes in their drawings of fantastic animals—body size, neck length, leg length, and number of legs—each attribute having two values. However, equally plausible descriptions of the attribute structure of the stimulus set are easy to imagine: for example, three

attributes—body size, total height, and number of legs—total height, having more than two values; or, for example, two attributes—body size and amount of contour in the appendages; or even, one multi-valued attribute—total amount of contour.

On what basis can we assume that Husaim and Cohen's description is the psychologically appropriate one for their subjects? I believe there is more reason to doubt than to credit that assumption if only because an infinite number of descriptions are potential and many of them are plausible. Likely, a good deal of specific information about the nature of animals is required before neck length and leg length would emerge as separate dimensions, more information than a 10-month old has. Nor does it seem more plausible that the infant treats number-of-legs separately from length-of-legs rather than perceiving them together as a more global attribute of total amount of contour.

Actually, an inspection of the pattern of transfer responses to the test stimuli strongly suggests that the infants do not dimensionalize the stimuli in the same way as do Husaim and Cohen. Putting aside the responses to the prototypical stimuli, the most consistent pattern that I find in the transfer data is the tendency *to categorize pairs of complementary stimuli in the same way*. Specifically, the classification probabilities are remarkably similar within stimulus pair 9 and 10, within stimulus pair 11 and 12, and within stimulus pair 13 and 14, despite the fact that in all cases the stimuli within the pair share *no* values in common on the four attributes that the authors describe. Such a pattern is not only inconsistent with categorization based on a single attribute, as Husaim and Cohen emphasize, but it is also inconsistent with categorization based on a family resemblance structure no matter what the presumed underlying process is. That is why there is such a poor fit between the test data for these pairs and the predictions of the two models, independent-cue and context models, that provide plausible alternative accounts about how such a structure is acquired.¹ Both models predict a *negative* correlation between classification probabilities for complementary test stimuli, given the symmetric nature of the acquisition set; both models predict that the classification probabilities for complementary stimuli sum to 1.0 under these conditions. Thus, we are either led to conclude that Husaim and Cohen's infants have transferred incoherently

1. When the two prototypical stimuli (1111 and 0000) are omitted from the calculations, the two models only account respectively for 9% and 6% of the variance of the test-trial data.

or, more likely, that the authors have failed to describe their stimuli in a psychologically correct way.² Needless to say, this is a troublesome conclusion as it not only undermines specific conclusions about how many attributes the infants attended to and what the relative saliences of the attributes were, but it goes to the very heart of the paradigm—the assertion that the acquisition exemplars are organized by family resemblance structures. It is possible to solve the original task on the basis of a single attribute, namely, total amount of ink (contour) in the stimulus, always greater for Category A exemplars than for Category B exemplars (although, as far as I can tell this simple putative attribute is not sufficient to account for the confusing transfer data).³

If the correct definition of the stimulus is such a crucial issue for this type of work on categorization, is there any workable way to ensure it? The best I can do is suggest that its likelihood can be increased by defining and varying attributes that have a primary psychological status for human adults and that are independent of the meaning of the stimulus—attributes like size, color, and position of a single form. Since one limits considerably the choice of stimuli under this requirement, an alternative approach to constructing ill-defined categories is to find a stimulus set for which adults provide consensual judgments of the component attributes, to use these attributes as the basis for forming categories structured by family resemblance relations, and to show that adults cannot find a simple definition based on some newly discovered attribute by which to identify the categories so constituted. The stimulus sets used by Husain and Cohen would at least fail the last criterion as adults would easily discover amount-of-contour as the single-attribute basis for differentiating categories.

Now to a more radical and more subtle problem with the assumption that the experimenter-defined dimensions are used by the infant learner. Recent work suggests that in cases where dimensions play a primary role in guiding perception and cognition for the adult and for the older child, they do not for the preschooler; instead, perception and cognition in the young child are organized around

2. For example, if shape and size are more psychologically compelling dimensions of a rectangle than length and width, then two stimuli complementary on height and width may well be treated the same way because they are the same size or the same shape.

3. If the prototypes for the categories had been varied across subjects such that not all subjects' exemplars were based on the single pair 1111 and 0000, such an alternative account of the category structure would have been far more difficult to invent, even given the ambiguity of the correct stimulus description.

the overall similarity relations among the stimulus wholes rather than around selected dimensional components (e.g., Kemler, 1982; Shepp, 1978; Smith & Kemler, 1977). Extrapolating from the evidence with the preschoolers, the hypothesis suggests the dominance of overall similarity relations in infants. Consistent relations of overall similarity relations do *not* presuppose a dimensional basis—that is, knowledge of the overall similarity of multidimensional stimuli need not be computed from knowledge of component similarities on the individual dimensions. It can be apprehended directly, just as the similarity of stimuli that vary unidimensionally can be apprehended directly. In fact, for special multidimensional stimuli—called integral by Garner (e.g., 1974)—even adults apprehend similarity relations more easily than component dimensional relations. Current developmental evidence suggests a useful analogy between the way young children generally perceive multidimensional stimuli and the way adults perceive special integral stimuli like color patches.

The developmental evidence has general significance for the acquisition of category information for it suggests that young children are especially well prepared to learn categories based on a family resemblance structure and poorly prepared to learn categories in which one of a number of variable stimulus attributes is singled out as defining. This is because a strong family resemblance structure implies a strong similarity structure (on the average, high similarity of instances within categories, low similarity of instances in different categories), whereas a defining attribute criterion implies a rather poor similarity structure (e.g., high similarity of items in different categories). Accordingly, categories based on strong family resemblances should be more easily learned by human infants than those based on criterial attributes—a prediction in search of a test—because infants should be more attuned to overall relations of similarity than to component dimensions if the developmental hypothesis is correct. Such speculation invites further research, like Husaim and Cohen's, on infant learning of ill-defined categories.

However, if the developmental hypothesis is correct and overall similarity relations dominate component dimensional relations in the infant, then questions such as how many dimensions can (does) the infant attend to and what is the relative salience of these dimensions, may be misleading. The apparent answers achieved by operating on the data in the ways that Husaim and Cohen suggest may have a different meaning than they intend. For example, if subjects are apprehending the stimuli as integral wholes, relatable by overall similarity, then the expectation is that *all* dimensions that vary in a de-

tectable way will *influence* learning rate and transfer patterns since each discriminable dimension will contribute to the pattern of overall similarity relations within the stimulus set. Thus, a red square and an orange pentagon are less similar to one another than a red square is to an orange square, whether or not one presumes that the dimensions of color and form themselves have a separate psychological reality. Though an investigator might be tempted to conclude that an infant whose categorizations are influenced by both color differences and form differences is paying attention to the two dimensions of color and form, these dimensions may be integral from the subject's point of view, so that differences on both dimensions are apprehended as a single unit. To imply that the subject is dividing attention between the dimensions would be quite inappropriate.

Likewise, calculations of the relative saliences of the dimensions, such as the ones that Husain and Cohen propose, would also be misleading. If the infant is only sensitive to overall similarity relations, still some of the experimenter-defined dimensions, as realized in the stimulus set, can contribute differentially to learning and transfer patterns—and thus be judged more salient by the proposed operations—simply because certain dimensional differences are perceived as larger differences in overall similarity than others. For example, it is likely that an orange-red square and a red-orange square would be more often categorized together than would an orange-red square and an orange-red circle simply because the first two are more similar overall to one another than are the latter two. Thus, evidence that they are so categorized does not unambiguously imply that the dimension of form is more salient for the learner than the dimension of color, a statement that implies the psychological reality of the dimensions for the subject. “Dimensional preferences,” if they are reversible by manipulating within-dimension similarity, do not reveal any genuine psychological properties of the dimensions at all, but rather may constitute information about psychological relations of overall similarity in the particular stimulus set.

The foregoing remarks lead both to agreements and disagreements with Husain and Cohen's program of research. There is clear consensus that the laboratory study of the learning of ill-defined categories by infants, the overarching thrust of their program, is of major significance. My reasons are these: (a) that many natural human concepts are organized in this way, (b) that infants are preferentially exposed to and learn just those concepts that are organized by strong family resemblance relations, and (c) that infants may be especially well-prepared to learn such categories because they are more

attuned to the overall similarity relations than to the dimensional relations of multidimensional stimuli (and, overall similarity relations especially afford categories based on family resemblances). Husaim and Cohen explicitly mention *a*; would agree, I suspect, with *b*; but have given insufficient attention to *c*. Failing to entertain the possibility that the psychological stimulus for the infant is different from the one declared by the experimenter—whether because it is not articulated into dimensions at all or because it is articulated into different dimensions than those the experimenter names—undermines the possibility of drawing strong inferences from the laboratory study of infant categorization. Husaim and Cohen have broken ground; exploration with refined tools needs to follow.

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