ABSTRACT

Using findings on the unstability of previously
stable physical activity levels of young children as a kind of case
study to aid thought about the trait-dimensional approach to
developmental continuity and discontinuity, this discussion explores
the applicability of a dual theory of concept formation to the
problem of personal stability and change. The dual theory of concept
formation posits two entities: core categorical identity attributes
and personality prototypes. Prototypes, such as "masculinity" and
"femininity," are construed as fuzzy sets made up of bundles of
attributes, no one of which is either necessary or sufficient for
classifying an instance as belonging to a particular category.
Categorical identity attributes are viewed as more fundamental,
binary distinctions, such as "male" and "female." Application of
prototype theory to personality theory and to the issue of continuity
in personal characteristics led to the judgment that prototype
theory, unsupplemented, is useful but inadequate. It is argued that
core categorical concepts, rather than prototypes, operate to define
individual identity in many respects, especially for those aspects of
identity that derive from group membership. It is concluded that: (1)
core identity is constant over time; (2) personality prototypes to
which individuals are assigned have a basis in the realities of their
behavior; and (3) circular processes between familiar partners are
central to the development of stable personal characteristics.
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PERSON CONSTANCY WITHIN DEVELOPMENTAL CHANGE

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Let me begin with some bits of data from the longitudinal study which Carol Jacklin and I have carried out with three cohorts of children from birth to age six. One of our objectives was to assess activity level at several ages, and I want to use activity level as a kind of case study to help us think about the trait-dimensional approach to continuity and discontinuity. Most of our sample were enrolled in nursery school during the year following their fourth birthdays. We went to the many nursery schools in which our sample children were enrolled, and observed them at play during unstructured free play periods. On the day of our visit, we observed our sample child for three seven-minute periods of outdoor play, and three of indoor play. During each observation period, we scored the child's activity level every ten seconds, so that each child's total activity level score is based on the average of 252 ten-second intervals. At each interval, we rated the child's activity level on a seven-point scale. Running and jumping were rated high on this scale, skipping fairly high, walking at an intermediate point, sitting lower, and so forth. Our coders were instructed to focus on the amount of energy being expended by the child at each rating. With training, we were able to achieve a very high inter-coder reliability on these activity ratings. In addition to the ratings based on free-play observations, we had another measure of activity level. Each of our sample children was brought into our mobile lab, along with two same-sex playmates. The room was thickly carpeted and equipped with a small trampoline and a beachball; half way through the session we introduced a Bobo doll. In our pilot work we had found that this set of props was well suited to eliciting highly active play from many but not all children, and we therefore thought we might see a considerable range in activity level among our sample children in this situation. We were not disappointed.

There was a modest and significant correlation between the amount of activity our target children showed with two playmates in the mobile lab and their activity ratings during free play periods in the nursery school. Furthermore activity level outdoors and activity level indoors correlated at a fairly strong and significant level. Thus we were able to aggregate scores from three sources -- indoor play, outdoor play and play in a same-sex triad in a semi-structured situation -- into a single activity level score. For a sub-sample of children, scores were obtained on two occasions one week apart. The three component activity scores, and also the composite score showed quite strong one-week stability. The short-term stability of the composite was .77, and of outdoor play alone was .82. Thus is appears that activity level was a useful trait dimension, in the sense that we could identify some children who were stably more active than others, and some who were characteristically low in the amount of energy they expended during play. So far, so good.

When our children had reached the age of six and a half, we observeed them again, this time on the playground of whatever school they were attending. Once again, we rated activity level for each of a sample of ten-second intervals. The schools were no longer arranging for indoor play periods, so we could no longer get a measure based on indoor play. We did observe the children in their classrooms, and scored them for restlessness in their seats. That is, we recorded the frequency of fidgeting and also of larger body movements while in their seats. We did not find that either of our in-seat restlessness scores was related to outdoor activity level. This is not really surprising: our classroom measures were designed to reflect any degree of hyperactivity that the child might display, and as we all know, hyperactivity is more a matter of poor attention focus and lack of behavioral organization than of total energy expenditure. In any case, we had to settle on outdoor play activity as being the score most comparable to the one we had used in nursery school. However the greatest difficulty we encountered in our effort to measure activity level at age 6 1/2 was the lack of short-term stability. For 45 children, we returned a week after our first observation to assess the children's

playground activity again. The correlation between activity levels on the two occasions was low and insignificant, despite the fact that our inter-rater reliability was very high.

I realize that one can't make too much of negative findings. Our measurements may have been weaker in a number of respects at age 6 1/2 than they were at 4 1/2. I am aware, too, that others have reported finding stability in activity level at early grade school age, although those studies have usually used more global ratings than the multiple time-sampled observations we employed. Whatever the reason, we did not find short-term stability at age 6 1/2, and finding stability in activity level at one age and not two years later stimulates us to think about what developmental changes might have occurred in the meaning of the dimension. I should mention that the range of scores at the two ages was very similar, so the drop in stability could not be explained as an outcome of restricted range. Not surprisingly, in view of the lack of stability of our age 6 1/2 scores, we found no relationship at all between a child's activity level at age 4 1/2 and his or her activity level at age 6 1/2 ($r = .06$).

Why did activity scores fail to distinguish among our children two years after nursery school, when they had done so at nursery-school age? We may find some clues in the nature of the activities the children were engaged in at the two ages. At age 6 1/2 we might find that a group of boys were playing a chasing game on our first visit, and would therefore all of them would receive high scores for activity. On the second visit, however, these same boys might be simply talking together; or they might be participating in an organized ball game in which most of an individual child's time was spent waiting to catch a ball or awaiting a turn at bat. In other words, at age 6-1/2, a child's activity level at any given moment seemed to be primarily a function of what was required by the structure of the game a child was engaged in.

Furthermore, even when not engaged in organized games, a child's activity level was, to a considerable extent, a function of what the child's playmates were doing. Between the ages of 4 1/2 and 6 1/2, we saw a strong increase in the amount of interactive play, and a decrease in solitary play. Thus, in a sense, at the younger age a child's activity level was more often a function of the child's individual choices of play activities; at age 6 1/2, when the children were more often playing in dyads or groups, one could say that a child's individuality did not express itself so clearly. The whole group would be engaged in either highly active or less active endeavors, and the individual children's activity level would reflect the groups's activity. In their meta analysis of sex differences in activity level, Eaton and Enns (1985) make the point that sex differences are greatest when children are playing with peers, rather than when they are functioning individually. I would interpret this to mean that boys stimulate other boys to relatively high levels of activity, but that when playing in groups, all the boys tend to be active to the same degree, so that while sex differences are exaggerated, within-sex individual differences are suppressed. In other words, a group play setting may not be a very good situation in which to identify some children who are temperamentally more active than others. Parenthetically, it may be worth noting that a similar pattern, of strong sex differences but very weak with-sex stability of individual differences, was found with respect to children's preference for male or female playmates (Maccoby & Jacklin, 1987).

This is not to say that group settings are poor situations for identifying any and all individual differences. A child's sociometric position in a group, or position on a dominance hierarchy, are examples of stable individual characteristics that manifest themselves primarily in group settings. My point is simply that the increase with age in the degree to which children do participate in groups changes the salience or utility of certain attributes for identifying individual differences. The utility of an individual difference dimension can wax or wane with age, depending on its relevance to the settings or activities in which children are spending increasing or decreasing amounts of time.

Obviously, we have gotten to the point where we must think about the criteria that we are willing to use to define a characteristic whose continuity or discontinuity we wish to study. I have been using activity level as an exemplar, and have suggested that the amount of energy expenditure during play may become less useful with age as a criterion for identifying individual differences. However energy expenditure during play is not the only member of the set of attributes that we cluster into our concept of activity level. Students of temperament (e.g., Buss & Plomin, 1975;1984) include both tempo and vigor of activity, and would include briskness of walking, vigor of knocking on a door, etc. If we think of activity level as a prototype, we realize that in order to be classified as a member of the class of active persons, an individual does not have to display all the attributes that are clustered into the activity prototype. Indeed the specific attributes the individual displays may change from age to age, but so long as the individual manifests one or more of the characteristics which are part of the activity prototype, the individual will continue to be seen as active, by others and by the self.

Let us consider prototype theory a little more closely as it applies to personality theory and to the issue of continuity in personal characteristics. Mischel and Peake, in their 1982 Psychological Review paper, begin with reference to the so-called consistency paradox: the fact that most of us believe that other people are characterized by broad personality dispositions,such as aggressiveness, or conscienciousness, or sensitivity to the moods and needs of others -- dispositions that manifest themselves in a variety of situations and with a variety of social partners. The paradox is, in the Mischel view, that research evidence does not support a large degree of cross-situational consistency. Drawing on the work of Nancy Cantor, Mischel argues that in describing other persons we make use of personality prototypes, so that if a person manifests an attribute that is part of the cluster of attributes that make up a prototype, we then attribute the whole of the cluster to the person. Initially,

one might think that this sort of analysis leads to the conclusion that personal consistency lies entirely in the mind of the beholder -- that people are not consistent and don't really have personality traits; they only appear to have, because other people categorize their behavior and base their expectations about the person's probable behavior on the prototype to which they have assigned the person. But the fact is that several different observers will all categorize an individual the same way. Different observers will agree that this person is consciencious and that person is not. It must be the case then, that different observers are using the same prototypes, and that there is something in the real behavior of the individuals being observed that triggers similar categorizing by different observers.

Mischel and Peake distinguish strongly between temporal continuity and cross-situational consistency. They argue that people who are functioning well should adapt themselves to the different requirements of different situations, so that they do not show much consistency in the personality traits they manifest in different situations. If they encounter the same situation repeatedly over time, however, they develop a characteristic mode of functioning in that situation so that their behavior in that situation is highly predictable. Mischel and Peake argue that observers are likely to mistake cross-time consistency for cross-situational consistency. That is, they use accurate information about cross-time consistency to infer the existence of a prototypical personality trait, and incorrectly assume that this trait will manifest itself in a variety of situations. Thus a student who knows another student only through class-room acquaintance notes that this other student is never late for class. He codes his classmate as a punctual person, not knowing that this same person, who is always on time for class, is actually quite likely to arrive late for appointments with friends. Once the punctuality prototype has been invoked for this acquaintance, the observer may not change his mind if he actually observes an instance in which the acquaintance is late for an appointment. Once a personality attribution has been made, it is resistant to disconfirming information. In this sense, our use of prototypes for describing other persons is similar to other stereotyping processes.

The basic point that emerges from the Mischel analysis is that people really do behave consistently over time, so long as they are in the same situation. The consistency is not all in the mind of the beholder, but it is fairly narrow, and it is to be expected that individuals will vary greatly in their behavior according to situational demands. How do these ideas apply to developmental change? I do not believe that the distinction between cross-time and cross-situation consistency is nearly as useful for developmental data as it may for understanding adult personality. It may help us to understand *dis*continuity, but not to predict or understand whatever continuity may be present. In our own activity data described earlier, we can say that the reason children showed no continuity over the two-year span from nursery school to first grade is that the situation had changed so greatly. We could not even get a measure of indoor-play activity at age 6 1/2 because indoor play periods were no longer provided in the schools. And children were now more often engaged in group, rather than solitary, enterprises. It is not so easy to see, though, why an individual child should not be behaving the same way on two occasions a week apart when observations are made on the same playground with the same group of available playmates. Drawing on Mischel's own data, we find something even more surprising. In his series of experiments on delay of gratification, conducted over several years, Mishel accumulated data on a large number of preschoolers. While his studies were not originally designed to measure individual differences, it was possible to identify in each experiment certain children who were better able than others to wait for a delayed reward. Mischel was able to locate nearly a hundred of these children ten or more years later, when they were in junior or senior highschool, and to have Q-sort ratings made of them. Significant correlations emerged between children's competence as teen-agers and the way they had behaved in a delay-of-reward test when they were in preschool. The teen-age characteristics most strongly predicted were mainly what we would identify as ego functions: the ability to concentrate on a task, to use and respond to reasoning, not to go to pieces under stress, to be resourceful, to plan ahead, to show curiosity and interest in exploring new situations, etc. These are

data to delight the heart of Jack Block, or any other searcher for core personality traits that endure over time.

Can we understand these findings by utilizing the distinction between cross-time and cross-situational consistency? We can say that we have cross-time consistency here, and this would be compatible with the view that it is mainly cross-time consistency that we ought to expect. But it seems clear that we also have cross-situational consistency. By no stretch of the imagination could we say that a 14-year-old child, in showing cogent reasoning when faced with a problem to be solved, is functioning in the same situation as the one which existed in Mischel's experimental room when the child was aged four. I think we can safely say, in fact, that 14-year-old children are *never* in the "same" situation they were in ten years before. Even if we should replicate a Mischel-type experiment at this later age, the children would have changed so greatly in their level of understanding of the exprimenter's procedure and purposes as to change the nature of the experience drastically. And certainly, Q-sort ratings are based on the whole range of situations in which the rater has seen the subject function. So: any longidudinal stability we find, I believe, is always a compound of cross-time and cross-situational consistency, and the two cannot be distinguished in any useful way. This is not to deny Mischel's basic point: that our behavior does undoubtedly vary greatly from one situation to another. This fact, however, does not preclude consistency and continuity that span both time and situation.

I want to return to the matter of the prototypical representations that we invoke to classify our own and other people's personalities. These classification processes are very real, and I believe we should think of them not as something that produce illusions of consistency where none exists, but rather as processes that helps to create and support consistency in our behvior. If other people develop certain expecations of us, based on the pretypical labels they have assigned to us, we know that their

expectations can influence us to behave in ways that match their expectations (Snyder & Swann, 1978). Furthermore our own prototypes of our own personalities lead us to believe that we can or should undertake certain behaviors and avoid other. But forming prototypes is probably not the only way we make categorical distinctions that are relevant to personal continuity. I invite you to think for a moment about alternative viewpoints.

In a fascinating paper, Armstrong, Gleitman and Gleitman (1983) have argued that there probably are two quite distinct ways in which we form concepts. Some concepts are binary categorical distinctions. Others are prototypes -- fuzzy sets -- made up of bundles of attributes, no one of which is either necessary or sufficient for classifying an instance as belonging in the category. And a given concept can have both kinds of meanings. Let me give an example they use: the concept "grandmother". One meaning of this term is "mother of a parent". By this definition, an individual either is or is not a grandmother, and there are no variations in degree. The other meaning of the word "grandmother" is a prototype: a middle-aged woman with grey hair, not very tall, perhaps a little plump, wearing glasses, with a warm smile, standing at an open door with her arms outstretched in a welcoming gesture. This concept is graded rather than binary; that is, an individual may be more or less grandmotherly. Thus Marlene Deitrich is indubitably a grandmother by the core categorical definition, but she does not approach the prototype very closely.

If we think about these two kinds of categories in terms of the way we classify ourselves and other persons, it leads us to distinguish between core identity and personality traits. Each of is and always will be the same individual, in the sense of being the person who was born at a certain time at a certain place to a certain mother into a certain family. Since birth we have accumulated a uniquely defining personal history. When we recognize another person, we have determined that person's identity, not assigned him or her to a prototype. Having recognized the person, however, we may then

invoke prototypes to clothe the person's identity with graded attributes. I would argue that our gender is a core categoical attribute, part of our identity. We are either male or female, and our system of categorizing ourselves and others according to gender does not allow for degrees of maleness or femaleness. Masculinity and femininity, on the other hand, are personality prototypes, and both men and women vary according to how closely they approach the prototype of a masculine or feminine person. The content of these prototypes undoubtedly changes with age: our concept of the feminine little girl is not the same as our concept of the feminine woman.

The two kinds of concepts that we form for classifying ourselves and other individuals -- the binary ones that have to do with our identity and the graded ones that we use to describe our personalities -- coexist in our throught processes. I don't believe we yet have a very clear picture about how they relate to eachother. Let me tell you about an experiment done at Stanford recently that illustrates how they might conjoin. This experiment drew on the work on natural kinds (Ellen Markman, Frank Keil),and was done by Susan Gelman and Pamela Collman (Gelman, Collman & Maccoby, 1986). Preschool children with a mean age of 4 yrs., 7 months were shown picture sets such as the one shown here:

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For each picture set, the upper two children were identified as to gender, and assigned attributes. Some of the attributes were familiar in their connection to gender, while others were unfamiliar. For example, one item was: "this boy will grow up to be a daddy; this girl will grow up to be a mommy." An unfamiliar item was: "This boy has little seeds inside; this girl has little eggs inside." Then the experimenter pointed to the picture of the third child, and asked the child to infer either the gender catetory or the attribute. Thus for the second item above, the child was asked one of the following two questions:

"This child has seeds inside. Is this child a boy or a girl?" Or: "This child is a boy. Does he have seeds or eggs inside?"

You will note that the information given about the third child would call for an inference that runs counter to perceptual similarity. It turned out to be easier for children to answer the second question than the first; in fact, these four-year-olds did not do better than chance when asked to infer gender from an attribute, while they performed quite accurately when they were given the gender category and asked to infer an attribute. Children are able to classify themselves and others as to gender at a very young age -- probably some time between the second and third birthdays (Thompson, 1975). The findings I have just described suggest that once the binary distinction has been made, the gender label can become a kind of magnet around which incoming gender-relevant information can be organized into protypical clusters.

What is the relevance of all this to the question of personal continuity over the life span? Brim and Kagan, in their 1980 book, asked why it is that we all believe so strongly in personal consistency when there is so much evidence against it -- so much reason to believe that our behavioral dispositions change greatly as we move through the various phases of our life cycle. Perhaps part of the explanation lies in the fact that our identity does indeed remain constant, although we add defining criteria to it. The girl who was born at a certain time and place becomes the woman who married at a certain time and place and gave birth to a child at a certain time and place. But the new events do not change her identity. For each person, there is a uniquely defining set of descriptors, and in identifying ourselves and others we must ignore variations in behavior, appearance, and psychological abilities and dispositions that are not criterial for the person's identity. Thus we must deal with much information about another person in much the same way that a child must deal with the length of the line in a number constancy experiment: the spacing of items is not criterial for number, and must be ignored.

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In the same way, we can know ourselves and others to be the same persons, regardless of the fact that we are growing taller, changing body shape, acquiring new skills, and undergoing considerable change in the bundle of personality attributes that distinguishes us from other persons.

If we may assume that constancy operates with respect to our core identities, we can then consider whether it operates with respect to personality prototypes as well. I think it does. That is, we can continue to see ourselves as relatively active or relatively inactive, despite changes that occur with age in which attributes in the activity cluster are being manifest. In the same way, the content of the prototype of individuals showing secure attachments changes drastically with age, but we can still assimilate the behavior of an adult who constantly seeks demonstrations of others' affection to the same prototype we applied to an excessively clingy child. I have no doubt that the way we use prototypes causes us to exaggerate the amount of continuity that exists across time. I also do not doubt that, as Diana Baumrind claims, entirely new clusters of attributes emerge with age --clusters that cannot be assimilated to any cluster that was useful for describing individual consistencies at earlier ages. The empirical questions remain what they have always been: to trace the connections across ages in the characteristics individuals display. But I think two things are evident from our analysis of the meaning of individuality: Our core identity is constant over time. And, the personality prototypes to which we assign people have a basis in the realities of their behavior. When we identify continuities in logitudinal studies, such continuities imply both temporal and cross-situational continuity. The prototypes we use in cognizing one another's personalities have considerable importance, not only because they enable us to know what to expect and how to react in our interactions with familiar other persons, but also because our expectations influence the behavior of those whom we have categorized, stabilizing their behavior in expected directions. Once again, circular processes between familiar partners emerge as central to the development of stable personal characteristics.

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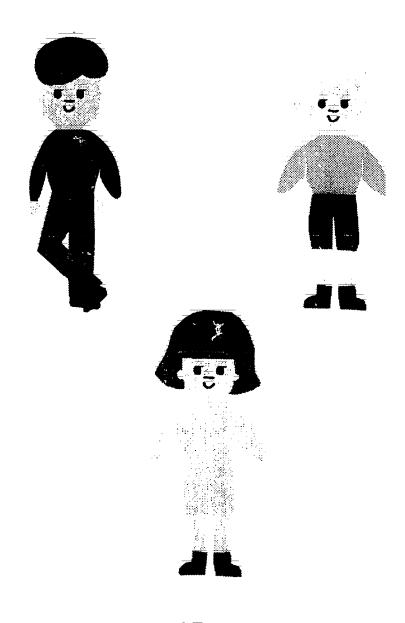
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