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Security of infantile attachment as assessed in the "strange situation": Its study and biological interpretation

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Abstract: The Strange Situation procedure was developed by Ainsworth two decades ago as a means of assessing the security of infant–parent attachment. Users of the procedure claim that it provides a way of determining whether the infant has developed species-appropriate adaptive behavior as a result of rearing in an evolutionarily appropriate context, characterized by a sensitively responsive parent. Only when the parent behaves in the sensitive, species-appropriate fashion is the baby said to behave in the adaptive or secure fashion. Furthermore, when infants are observed repeatedly in the Strange Situation, the pattern of behavior is said to be highly similar, and this pattern is said to predict the infants' future behavior in a diverse array of contexts. After an exhaustive review of the literature, it is shown that these popular claims are empirically unsupported in their strong form, and that the interpretations in terms of biological adaptation are misguided. There is little reliable evidence about the specific dimensions of parental behavior that affect Strange Situation behavior, although there does appear to be some relationship between these constructs. Temporal stability in security of attachment is high only when there is stability in family and caretaking circumstances. Likewise, patterns of Strange Situation behavior only have substantial predictive validity in similarly stable families. Implications for future research and theorizing – particularly as they relate to the use of evolutionary biology in psychological theory – are discussed.

Keywords: adaptation; attachment; development; early experience; evolution; infancy; maternal behavior; mother–infant attachment; sociobiology

That behavior can be explained in terms of evolutionary adaptation is almost a truism. When it comes to particular cases, however, explaining behavior in terms of adaptive significance becomes both theoretically and methodologically problematic. This is particularly the case with social and developmental psychology, in which there are so many possible adaptive relationships and interpretations. Consider, for example, the attachment behavior of infants. Is attachment behavior "beneficial" to the infant, the mother, or some group – such as the family? If beneficial to the infant, does attachment play a role in ensuring individual survival, in maturation, or in something else? Perhaps attachment behavior is not adaptive at all, even if it was adaptive in some ancestral environment. Possibly, attachment behavior has multiple functions.

Because of these complexities, psychologists have made slow progress in their attempts to use the principles of evolutionary biology as aids to understanding human behavior and development. This is not to say that evolutionary biology has not been useful. As illustrated by previous treatments in this journal (e.g. Eibl-Eibesfeldt 1979; Lumsden & Wilson 1982; Plotkin & Odling-Smee, 1981; Rajecki, Lamb & Obmascher 1978; Symons, 1980; van den Berghe 1983), evolutionary theory can help, both by suggesting testable hypotheses and by ruling out explanations, such as those based on speculations about adaptations "for the good of the species." In any case, of course, it is not legitimate to assume that the explanation in terms of adaptive function is correct, or to presuppose that any given function is "the" role of the behavior. The theory's predictions must be tested against the empirical evidence and against alternative interpretations. In this paper, we discuss one area of research in which the principles of evolutionary biology have been invoked to explain certain developmental phenomena. Our goal here is to evaluate both the empirical evidence and the interpretation of infant behavior in terms of principles derived from evolutionary biology. This exercise is valuable, not only in its own right, but also as an illustration of

some of the interpretative and methodological problems that have hampered progress in developmental psychology.

Our focus is on the "Strange Situation," a procedure designed to tap a central aspect of socioemotional development, the security of infant–parent attachment. Much of the enthusiasm about this procedure is attributable to claims that:

a. individual differences in the way infants behave in the Strange Situation are lawfully and interpretably related to prior patterns of parent-infant interaction;

b. from an evolutionary adaptational perspective, one can see some patterns of Strange Situation behavior as adaptive and others as maladaptive;

c. infants seen more than once in the Strange Situation tend to behave in the same fashion each time, indicating that the procedure taps some stable dimension rather than something temporary; and

d. individual differences in Strange Situation behavior predict behavioral differences in other contexts as much as several years later.

Most recent discussions of the Strange Situation have treated these claims as if they were well grounded empirically. The purpose of this review is to evaluate the empirical evidence.

The Strange Situation procedure

The Strange Situation was initially designed to assess infants' (a) use of an adult as a "secure base" from which to explore, (b) reactions to a stranger, and (c) reactions to separation and reunion. The emphasis was on secure base behavior because "one of the most important criteria of a healthy attachment was ability to use the mother as a secure base for exploration" (Ainsworth & Wittig 1969, p. 112). The procedure involves 7 episodes, each lasting 3 minutes, arranged to create increasing amounts of stress for the baby so that researchers can observe how infants organize their behavior around attachment figures when distressed (see Ainsworth, Blehar, Waters & Wall 1978 for further details). Initially, parent and baby are alone together (episode 1). They are joined by a female stranger (episode 2) who engages the infant's attention so that the parent can leave the room (episode 3). When the parent returns, the stranger leaves (episode 4); she is followed 3 minutes later by the parent (episode 5). The infant is then joined by the stranger (episode 6) and then by the parent, whose entrance signals the departure of the stranger (episode 7). Videotaped or narrative records of the session are then reviewed and scored on 6 rating scales: proximity or contact seeking, contact maintaining, resistance, avoidance, search, and distance interaction. Judges then classify the infant into one of 3 groups (A, B, C) and 8 subgroups (A_1, A_2, B_1, B_2, B_3, B_4, C_1, C_2) representing an overall judgment regarding the infant's behavior (see Table 1). The B-group infants are considered "securely attached"; they greet the parent upon reunion either by seeking proximity or contact or by distal interaction (e.g., smiling, vocalizing, or waving). Infants in the four B-subgroups differ in the degree of proximity or contact they seek. They range from the B_1 infants, who are content with distance interaction, to the B_4 infants who are highly concerned about achieving contact and, indeed, are slow to take comfort even when in co
A- and C-group infants are considered inse
tached. The A-group infants are called avoida
they tend to avoid or ignore their parents r
seeking interaction, especially upon reunion.
are conspicuously avoidant, whereas A_2 infa
avoidant and proximity-seeking behavior. Th
infants are called resistant because they mingle
and contact-seeking behavior with angry, rej
havior, especially upon reunion after the bri
tions.[1] Those in the C_2 subgroup are distinguis
passivity of their proximity-seeking behaviors w
infants are much more active in both proximi
and resistance. For example, the C_2 infants m
cry helplessly instead of crawling toward th
reaching to be picked up, whereas the C_1 in
away or hit the parent. Most research on th
Situation has involved relating the classification
on the basis of Strange Situation behavior to m
infantile or maternal behavior in prior, c
raneous, or subsequent observations.

Focus of the review

The Strange Situation has been widely acclaime
it seemed to enable researchers to identify and
central developmental construct – the quality c
of infant–parent attachment. Furthermore,
differences in the security of attachment seeme
related to prior patterns of infant–parent inte
finding consistent with popular beliefs that
social relationships are shaped by their earli
experiences (e.g. Ainsworth 1973; Freud 1940
1971). The association between parental and
havior also seemed consistent with a theory der
evolutionary biology (Ainsworth 1979a; Bowlby
which secure infant behavior is considered sp
propriate and the consequence of rearing by a
behaving in the species-appropriate fashion.
insecure infant behavior is viewed as maladaptiv
result of interaction with a parent whose beha
ates from the species-appropriate pattern. In
individual differences in the security of attach
assessed in the Strange Situation, appeared t
individual differences in various other aspec
cioemotional development – confirming theore
dictions about the formative centrality of th
caretaker relationship.

The goal of this paper is to evaluate the empi
for these claims by conducting an exhaustive ar
evaluation of the literature. We begin with re
the antecedents of Strange Situation behavior,
concerned with the relationship to prior pattern
er–infant interaction. In the next section w
discuss quantitative tests of the A, B, C clas
system devised by Ainsworth; this is followed by
sion of the temporal stability of individual diffe
Strange Situation behavior. Next comes a revi
growing literature on the predictive validity anc
correlates of Strange Situation behavior. Then v
the argument that sensitively responsive matern
ior constitutes the form of care that human inf
evolved to expect. The evaluation of this claim

Table 1. *Patterns of interactive behavior and crying in the Strange Situation*

	Behavior to mother on reunion[a]				Crying	
	Proximity seeking	Contact maintaining	Proximity avoiding	Contact resisting	Preseparation/Separation/Reunion	Additional characteristics
Avoidant A1	Low	Low	High	Low	Low/low or high/low	Avoidance is the same or greater on second reunion.
A2	Moderate to high	Low	High	Low to moderate	Low/low or high/low	Avoidance is the same or greater on second reunion.
Secure B1	Low to moderate	Low	Low	Low	Low/low/low	Positive greeting to mother on reunion and active distance interaction.
B2	Low to moderate	Low to moderate	Low to moderate	Low	Low/low to moderate/low	Avoidance decreases on second reunion. May show proximity seeking in preseparation episodes.
B3	High	High	Low	Low	Low/moderate to high/low	Proximity seeking and contact maintaining vary directly with separation distress. Recovery from distress before 2 min and return to play is typical.
B4	High	High	Low	Low	Low/high/low to moderate	Proximity and attention to mother throughout.
Ambivalent C1	High	High	Low	High	Low to moderate/high/moderate to high	Difficult to comfort on reunion. Strong resistance of contact with stranger during separation. Often angry toward mother on reunion.
C2	Low to moderate	Low to moderate	Low	High	Low to moderate/high/moderate to high	Exploratory behavior is weak throughout. Difficult to comfort on reunion.

[a]Scored on 7-point scales, scale points anchored to behavioral descriptions selected from typed transcripts of the behavior of 1-year-olds in the Strange Situation.
Source: Sroufe & Waters (1977). Reprinted by permission of the Society for Research in Child Development.

an examination of evolutionary theory and has implications for other attempts to use principles of evolutionary biology as aids to understanding human development. Finally, we summarize our conclusions and suggest directions for future research and theory.

Although all of the studies reviewed here use the Strange Situation procedure, the issues raised are of broad significance to students of behavior and development. First, the review addresses the long-term significance of early experience. Our conclusion is that relationships between early experiences and later outcomes have been demonstrated only when there is continuity in the circumstances that apparently produce the outcomes in question. This suggests that early experiences per se may not be crucial determinants, and that future attempts to study the effects of early experiences must also consider the occurrence of intervening events which may ameliorate, accentuate, or maintain the "effects" of early experiences. Second, we discuss the usefulness of evolutionary biology as a means of elucidating the development of individual differences in juvenile behavior. Although the principles of evolutionary biology are promising tools, we are forced to conclude that they have been of limited use thus far to proponents of the Strange Situation methodol-

ogy, because the relevant principles appear to have been misunderstood or misapplied. Finally, we consider attempts to explain *how* parental behavior affects child development. This is a topic of major concern to developmentalists, and the focus of much speculation and research. Evidence on the antecedents of Strange Situation behavior appears to be as inconclusive as that regarding any other aspect of development.

Antecedents of Strange Situation behavior

The central hypothesis under consideration in this section is that securely attached infants have more sensitively responsive caretakers than infants who behave insecurely in the Strange Situation. As Sroufe and Waters (1982) have written,

> It has been shown . . . that attachment classification is related to earlier assessments of maternal behavior. This is critical to the validity of the attachment construct. It is therefore important to point out that this has been confirmed in a number of studies across social classes and even cultures. *Caregiver responsiveness to the infant's signals predicts quality of attachment; infant temperament does not.* (p. 744; italics in original)

Claims concerning the origins of Strange Situation behavior in prior patterns of infant–parent interaction are derived primarily from Ainsworth's longitudinal study, which involved 26 infants from middle-class homes (see Ainsworth 1979a; 1979b; Ainsworth et al. 1978; Ainsworth, Bell & Stayton, 1974, for reviews).[2] Each infant–mother dyad was observed at home, usually for 4 hours every 3 weeks throughout the first year by the same 1 of 4 observers, who made notes on their interaction during the visits and later dictated a narrative account based on these notes. The narrative accounts included objective reports of the interaction, evaluative comments by the observer, and a summary of pertinent statements made by the mother. When the infants were 51 weeks old, they were observed in the Strange Situation by 2 or 3 individuals who dictated accounts of or made notes on the infants' and adults' behavior. The narratives were later collated into a single account (Ainsworth & Wittig 1969). No conventional measures of interobserver reliability during home observations were ever obtained. In the Strange Situation, interobserver agreement was calculated for some frequency measures, but not for any of the interactive scales used in the classification of Strange Situation behavior.

The 3 groups and 7 original subgroups were derived by seeking similarities among two or more infants, who were then defined as members of a subgroup. Subsequently, Ainsworth and her colleagues searched for similarities between subgroup clusters, as a result of which the groups were born. In the initial attempts to identify groups and subgroups, the focus was on responses to separation, but it later shifted to reunion behavior. Of the 23 infants, 13 were considered secure (1 as B_1, 3 as B_2, 9 as B_3), 6 avoidant (4 as A_1, 2 as A_2), and 4 resistant (2 as C_1, 2 as C_2).[3]

Ainsworth and her colleagues then explored relationships between Strange Situation behavior and patterns of infant behavior at home (see Ainsworth et al. 1978, Chap. 7). For example, Ainsworth et al. (1971) divided their sample into 5 groups by assessing the quality of tl attachment-exploration balance at home du fourth-quarter observations. Comparisons sugge the B – especially B_3 – infants exhibited a bette ment-exploration balance at home, with most and C-group infants apparently failing to use the ers as secure bases for exploration. However, t B_1, and B_2 infants were not well distinguished another on this index.

Ainsworth then proceeded to focus on matern ior, scoring transcontextual dimensions such as " sensitivity" (Ainsworth et al. 1971; Ainsworth Stayton 1972; Ainsworth et al. 1974) as well as be specific contexts – such as separation and (Stayton & Ainsworth 1973), feeding (Ainsworth 1969), discipline and socialization (Stayton, E Ainsworth 1971), crying and distress relief Ainsworth 1972), face-to-face interaction (Blehar, man & Ainsworth 1977), physical contact int (Ainsworth 1979a; Ainsworth et al. 1972), and the of maternal affection (Tracy & Ainsworth 1981). two rating scales were constructed to assess behavior during the first quarter-year of life, with score on each scale being based on the 4–6 visit that quarter (Ainsworth et al. 1978). Since this w nally intended to be a hypothesis-generating stud were defined after examination of the narrative than a priori. Only 6 of these 22 scales were discu Ainsworth and Bell (1969), and only 4 scales deal feeding were considered by Ainsworth et al. (19 results obtained using the remaining scales have r reported. Four transcontextual scales were rat the fourth-quarter narratives – sensitivity, coop acceptance, and accessibility – because they especially related to individual differences in th response to the mother" (Ainsworth et al. 1978, Later 2 broad ratings pertaining to physical-cor teraction, 1 related to emotional expressiveness maternal rigidity, were made (Ainsworth et al Ainsworth et al. (1978) discuss at least 10 oth quantitative measures, tapping either the propo opportunities in which mothers behaved in certa or the average duration of events such as "unresp ness to distress," scored using narratives from eit first or fourth quarter-year.

The summary of findings by Ainsworth et al only discusses results involving these 26 measur concerning only one quarter), but it is not clear how many different measures were scored fr narratives. Although interobserver reliability assessed, interrater reliability was usually highly tory, and efforts were made to keep raters naive re other information about the families and about the ic hypotheses.

Not surprisingly, the analyses of behavior in contexts generally yielded results consistent wit obtained in analyses of the broad transcontextual nal characteristics. In each case, the principal din underlying individual differences in infant–mot teraction had to do with the mother's sensitiv "appropriate" responsiveness to infant cues. For ple, of 14 fourth-quarter measures of maternal b discussed by Ainsworth et al. (1978), 8 showed sig differences between the B and A groups, and 8

differences between the B and C groups. The largest differences were on the 4 transcontextual dimensions, with the B-group mothers being much more sensitive, accessible, cooperative, and accepting than the mothers of A- and C-group infants. All but 3 of the 17 first-quarter and whole-year measures reported by Ainsworth et al. (1978) yielded differences between the B- and non–B-group mothers. Eight of the 12 measures of maternal or dyadic behavior during face-to-face interaction in the first quarter revealed differences between the mothers of B_3 infants and the mothers of C and A infants (Blehar et al. 1977). In the latter analyses the B_1 and B_2 infants were excluded out of "a desire to obtain the clearest possible contrast between infants who had developed the most [B_3] and least [A, C] secure attachments" (Blehar et al. 1977, p. 186), although their means differed significantly from those of the B_3 infants on only 1 of 12 measures.[4]

The transcripts from Ainsworth's study were later analyzed by Main and Stadtman (1981), who coded maternal aversion to physical contact from the first-quarter narratives. As with most of Ainsworth's maternal measures there was fairly high to high stability over time (r = .51, .67, .72) and moderate to high correlation with ratings of the infants' overt aggression to their mothers (r = .44) and of the extent to which anger dominated the infants' mood (r = .65). Data from other samples suggested moderate correlations between contemporaneous assessments of maternal aversion to contact and infant conflict behavior.

Overall, the findings obtained in Ainsworth's longitudinal study are considerably weaker than they appeared at first glance. First, the measures were not mutually exclusive; indeed, intercorrelations among them – especially the transcontextual dimensions – were extremely high. For example, Ainsworth and Bell (1969, p. 159) reported intercorrelations among 6 maternal rating scales, of which *all* were above .80 and nearly half were at or above .90! Despite the proliferation of measures, therefore, the evidence suggests – as Ainsworth et al. (1978, p. 159) have acknowledged – that there is really one primary dimension tapped by all the measures used in this study: the harmony of mother–infant interaction. Furthermore, all measures concerning each context were scored from the same narratives and often involved examining the same behavioral sequences. Since the measures were nonindependent, the reliability of the results cannot be assessed by determining what proportion of the measures show significant group differences. Moreover, the extraordinarily low ratio of subjects to variables and the conceptual and statistical interrelations among the variables ensure that individual significance tests provide no real protection against capitalization on chance.

Second, because the measures were not derived on a priori theoretical grounds but after an examination of the narratives, all results must be replicated in independent samples before they can be considered reliable. The use of naive raters provides very little protection against capitalization on differences detected during the measurement construction phase, because these measures were typically derived by individuals who were familiar with the classification status of the infants and with the general theoretical framework. The fact that group differences later "emerged" on measures chosen in this fashion is not surprising.

Third, despite striking differences in infant behavior, the mothers of babies in groups A and C differed from each other less than they did from the mothers of babies in group B. Whatever the reason for the similarities between A- and C-group means, the absence of clear differences between these groups precludes a conclusion that behavior in the Strange Situation is lawfully determined by prior patterns of infant-mother interaction. Just as the absence of clear differences between the A and C mothers is problematic, subgroup differences likewise preclude simple statements about the groups.

Fourth, observer reliability was never assessed in the homes and was inadequately assessed in the Strange Situation, leaving substantial opportunity for contamination of data that should, for analytic purposes, have been independent. At least some of the individuals who observed behavior in the Strange Situation were aware of the infants' prior behavior at home, even if they were unaware of the specific measures that would be derived later. Thus the two data sets are not independent of one another. Furthermore, the fact that the same individuals observed each family throughout the year without quantitative checks on reliability means that there was extensive opportunity for bias to color all data concerning a given family – especially since the observers made subjective evaluations that may have affected the ratings or codings. The more quantitative measures are also suspect because details were often recorded imprecisely, and all the duration measures had to be estimated years later. All in all, even with high agreement between independent raters, who were naive about hypotheses and Strange Situation classifications, the unknown reliability of the raw data is critical.

Fifth, Ainsworth and her colleagues tend to overinterpret and overgeneralize small differences between tiny groups. For example, from a net difference of .7 physical interventions per 4-hour visit between A and B infants, it is concluded that "mothers of A babies . . . more frequently use forcible physical interventions" (Ainsworth et al. 1978, p. 147), even though the finding does not reach conventional levels of significance. A sample of 23 mothers and infants is simply too small to reveal reliable and generalizable differences between 3 groups and 7 subgroups.

For all these reasons, the results of this initial longitudinal study must be viewed with great caution. The absence of controls for bias, the failure to estimate observer reliability, the post hoc nature of the research, the small sample (and group) sizes, and the paucity of differences between A and C infants and mothers all mean that this project must be viewed *solely* as a hypothesis-generating pilot study – not as a hypothesis-testing investigation. The findings can only obtain generalizability when replicated in independent studies in which bias is controlled and a priori predictions are tested and verified.

There have recently been reports of two longitudinal studies designed to replicate Ainsworth's findings. The largest attempted replication (Egeland & Farber, in preparation) began with 267 primiparae (mothers of first-borns) and their infants, 212 of whom were observed in the Strange Situation at 12 months and 197 of whom were assessed again at 18 months. Many of the mothers were poor and the majority (62%) were single. Fifty-five percent of the infants were boys. Both prenatally and 3

months postnatally, the mothers completed a battery of psychological tests assessing aspects of their personality, attitudes, and life stresses.[5] These tests yielded at least 15 scores each time.[6] Maternal reports of life stresses were obtained at 12 and 18 months. Nurses' ratings of the newborns and their mothers were factor analyzed to yield 4 factor scores assessing infant and maternal characteristics. Infant characteristics were also measured by the Brazelton Neonatal Behavioral Assessment Scale (6 scores derived from 2 administrations; see Vaughn, Crichton & Egeland 1982), maternal reports of infant temperament at 6 months (presumably yielding 9 scores), and Bayley mental and motor scale scores at 9 months.[7] Mother–infant interaction was assessed by observations of feeding at 3 months (once) and 6 months (twice) and of infant-mother play at 6 months (once). Ratings made after each observation were separately factor analyzed to yield a reduced number of measures. A total of 3 factors were derived from the 3-month feeding, 2 from the 6-month feeding (scores from the 2 observations were averaged before factor analysis; see Vaughn et al. 1982), and 3 from the 6-month play session (Vaughn, Taraldson, Crichton & Egeland 1980). In addition, maternal sensitivity and cooperation were rated during the play and feeding sessions at 6 months using Ainsworth's scales.

Eleven of the 60 or more tests relating maternal characteristics to Strange Situation classifications yielded significant group differences. However, in no case were the same measures significantly related to both 12- and 18-month classifications. On 9 measures, the B-group means were significantly distinguished from those of only one of the insecure groups; the one exception was that the 18-month B-group mothers were less aggressive than the A- and C-group mothers, both prenatally and postnatally. Further, pre- *and* postnatal scores on only *two* measures were related to either one of the classifications. Among boys, only 5 of the 60 tests revealed significant relationships between attachment security and maternal characteristics (and in only 3 cases were post hoc contrasts significant), whereas among girls, 9 tests (6 contrasts) were significant. In the tests involving the subsamples of boys and girls, only 5 of the significant effects were comparable to those obtained for the full sample, and *none* of these was significant for *both* boys and girls. These results, taken together, suggest no reliable or consistent relationships between maternal characteristics and security of attachment.

Five measures of infant characteristics showed significant group differences (using either 12- or 18-month classifications), although only 2 revealed significant post hoc contrasts distinguishing B-group infants from the others. There were more relationships (4 vs. 1) to 12-month than to 18-month classifications, but in *no case* was the same factor related to both 12- and 18-month assessments. Separate comparisons involving the male and female subgroups yielded 3 significant analyses of variance for boys and 5 for girls, with only 3 of the 8 yielding a pattern of group differences similar to that for the sample as a whole. Once again, *none* of the differences characterized both subsamples and the total sample. There was thus no clear and consistent pattern of relationships between infant characteristics and the security of infant-mother attachment.[8]

Six of the original 3-month feeding variables (rather than factor scores), 6 of the 6-month feeding variables and 3 of the 6-month play variables were significantl related to the 12-month classifications, whereas only 1, 3 and 1 of the respective variable sets were related to th 18-month assessments. Of these 20 variables, 12 yielde significant B- vs. non–B-group contrasts. In *no instanc* was the same variable related to both assessments c attachment security. Because different factors were ger erated for the 3- and 6-month feedings, it is not possible t compare these findings directly. Of the 19 significar overall relationships for boys and 6 for girls, 8 of those fc boys and 3 of those for girls matched those for the tot sample, but there were only 2 cases in which the sam measures yielded significant effects for boys, girls, an the whole sample. In almost every case, pairwise cor trasts revealed different effects among boys, girls, and th total sample. On the 2 Ainsworth rating scales significa group differences consistent with Ainsworth's were a parent for the sample as a whole and for boys onl However, these scales were related to the 12-month b not to the 18-month Strange Situation classification

Overall, this study provided very little insight into tl antecedents of Strange Situation behavior and on equivocal support for hypotheses generated by Ain worth's investigation. Not only were there relatively fe significant group differences, but there were also unpr dicted sex differences, further suggesting that the fe significant findings were unreliable. Furthermore, a though one might expect fewer significant relationshi between the independent variables and 18-month clas fications than between these measures and the 12-mon assessments simply because of attenuation attributable the impact of intervening events, there is no a pric reason to expect *different* patterns of association with tl two assessments of attachment security. Given the hi rates of stress and instability in their sample, Egeland a Farber (in preparation) might well have asked whether was reasonable to expect many relationships betwe early maternal characteristics and later security of attac ment. Clearer insight might have been obtained if tl maternal characteristics were assessed closer in time the assessments of attachment security. Almost all of t significant group differences were in the predicted dire tion, with B-group mothers being more perceptive, se sitive, and empathic. Unfortunately, however, the a thors did not indicate how great a proportion of t variance on the various (mostly nonindependent) me sures was associated with later security of attachment; inspection of means suggests that the proportion of va ance explained was minor. Multiple regression p cedures (with the male and female subgroups treated replication samples) might have given greater insight ir the psychological importance of the relationships ported by Egeland and Farber (in preparation) wh multivariate analyses of variance would have helped the evaluation of chance results.

Analyzing the data from a subsample (N = 104) Egeland's sample, Vaughn, Gove, and Egeland (19 compared the attachment status at 12 and 18 months infants whose mothers had returned to work by months ("Early Work", N = 34), between 12 and months ("Late Work", N = 18), or not at all ("No Wor N = 52). The number of insecure attachments was significantly greater in the work groups, but there wa

shift in distribution of avoidant and resistant relationships, such that there were disproportionate numbers of avoidant infants in the "Early Work" (but not "Late Work") group. These results suggest that early maternal employment tends to ensure that insecure relationships are avoidant, but they do not indicate why these relationships tended to be insecure in the first place.

Joffe (1981) drew data from another subsample involving 112 of those infants seen in the Strange Situation at 12 months and 69 of those seen at 18 months. Security of attachment was related to 17 measures of maternal behavior and 4 measures of infant behavior in "prohibition situations" also occurring at 12 and 18 months. On the 12-month data, there were significant B- vs. non–B-group differences on 1 of the infant measures and 1 of the maternal measures, and significant A vs. C differences on 2 infant and 4 maternal behavior measures (one-tailed tests). On the 18-month data, there were significant B- vs. non–B-group differences on 1 infant and 9 maternal measures, but no significant A vs. C differences. It is not clear whether the raters were blind with respect to attachment classifications and scores on other ratings. The small number of significant differences and the fact that only one difference was found in both the 12- and 18-month data (B groups were consistently more compliant than non-B infants) suggest that there were few if any reliable relationships observed. Even if significant effects had been observed, they would not have elucidated the antecedents of Strange Situation behavior, however, since the assessments of maternal and infant behavior were contemporaneous.

Analyzing data from the whole sample, Pastor, Vaughn, Dodds, and Egeland (1981) found no overall relationship between maternal living arrangements and security of attachment at 12 months, although secure attachments were more common at 18 months when mothers were living with husbands or boyfriends than when they either had no such relationships, or lived separately from the men. As with Egeland and Farber's (in preparation) and Joffe's (1981) results, these findings are difficult to evaluate because of differences in the correlates of 12- and 18-month security of attachment.

The second attempted replication of Ainsworth's findings was conducted in Bielefeld in northern Germany by Karin and Klaus Grossmann (1982). These researchers followed 49 mothers and infants from birth. Two-hour home observations were conducted by the authors when the infants were 2, 6, and 10 months old. Narratives based on the notes made by both observers constituted the data base. No measures of interobserver reliability were reported. Later, two assistants coded from the 10-month narratives infant crying and maternal responsiveness, behaviors involving close bodily contact, the infants' reactions to the mother's comings and goings, maternal sensitivity, and maternal cooperation, in each case using Ainsworth's rating or coding conventions. The 2- and 6-month visits were also rated for maternal sensitivity while maternal acceptance or rejection was rated from interviews.

At 10 months, the majority of the 11 maternal measures were significantly correlated with the overall rating of sensitivity, as in Ainsworth's sample. As in Baltimore, the infants of sensitive mothers cried less than infants of insensitive mothers, sought close bodily contact more often, responded more positively to being picked up, and did not protest when put down again. When the maternal variables were related to Strange Situation classifications, however, Grossmann and Grossmann (1982) found that the mothers of B-group infants were significantly more sensitive than those of C- but not A-group infants at 2 months; significantly more sensitive than both at 6 months; and equally sensitive at 10 months. Furthermore, the magnitude of the differences between group means was substantially greater in Baltimore (about 4 points on an 8-point scale) than in Bielefeld (a maximum of 2 points on the same scale). Grossmann and Grossmann (1982) also found that the mothers of B_3 infants were almost as insensitive as the A-group infants' mothers at 10 months, whereas these mothers were consistently the most sensitive in Ainsworth's sample. One wonders, therefore, why these infants behaved in the B_3 pattern despite the maternal "insensitivity" in the period preceding observation in the Strange Situation.

Grossmann and Grossmann noted that German mothers are more concerned than American mothers about making their babies independent, and that independence training could be considered insensitive on Ainsworth's measure. This could explain the large number of A-group infants in this sample, the unusually low correlations between maternal acceptance and sensitivity (Grossmann, Grossmann, Huber & Wartner 1981), and the puzzling absence of a relationship between sensitivity at 10 months – when independence training was proceeding in earnest – and Strange Situation behavior. On the other hand, these findings raise questions about the appropriateness of value-laden terms like "insensitivity" when we may be observing cultural variations in the goals and practices of parents.

Further relevant findings were reported by Crockenberg (1981), who related infant irritability in the neonatal period, maternal responsiveness to distress at 3 months, and maternal reports of social support at 3 months to Strange Situation behavior at 12 months in a sample of 48 dyads. The relationships between social support and security of attachment and between maternal responsiveness and security of attachment were significant by chi-square analysis, but the effects of maternal responsiveness were evident only when social support was low, and the effects of social support varied depending on the degree of infant irritability. Multiple regression analyses predicting resistance, avoidance, and proximity seeking in the reunion episodes showed that "maternal responsiveness predicted proximity seeking, predicted resistance only when it was extracted prior to social support, and failed to predict avoidance" (Crockenberg 1981, p. 861). Maternal responsiveness thus appeared to be significant, although its effects could only be understood in the context of other important variables.

Using data from the longitudinal study described above, Egeland and Sroufe (1981) reported that when maternal care was extremely poor – bordering on abusive or neglectful – infants were more likely to be insecurely attached. The pattern of effects was different for the 12- and 18-month attachment assessments, however: C-type relationships were associated with abuse or neglect at 12 months, whereas A-type relationships were most common among the maltreated infants at 18 months. Somewhat similar results were reported by Lamb, Gaens-

bauer, Malkin, and Shultz (in preparation) using data from a sample of 62 infants, 23 of whom had been abused or neglected by their mothers; most (16/23) of these abused infants were rated avoidant when assessed at (on average) 18 months. Using a normal sample, Estes (1981; see also Estes, Lamb, Thompson & Dickstein 1981) reported that the mothers of securely attached (B_1 and B_2) infants were more "expressively involved" in their infants' performance during an assessment of sociability at 19 months than were the mothers of C-group or B_3 and B_4 infants. The mothers of avoidant infants were almost as involved as the mother of B_1 and B_2 infants, however, and when the groups were compared, only the mothers of C-group infants differed significantly from the A- or B-group infants' mothers.

Tolan and Tomasini (1977; see also Main, Tomasini & Tolan 1979) observed 40 infants in the Strange Situation at 12 months, and in a play session with mother and an unfamiliar adult at 21 months.[9] Videotapes of the play sessions were repeatedly viewed to allow the rating of maternal sensitivity and acceptance using Ainsworth's scales. A narrative account was also dictated by one of the raters, and was later used for rating maternal anger, aversion to contact, and expressiveness. Two slides of the mother's face were also taken, and maternal expressiveness was again rated from these slides. Six of the 7 ratings revealed significant group differences between B_3- and non–B-group infants, but apparently none of the differences was significant when all secure (B-group) infants were compared with the insecure (non–B-group) infants. Interpretation of the differences reported is also problematic because of the high intercorrelations among the measures, and because 40% of the (secure) B group were excluded in their statistical analyses (i.e. the non-B_3 infants). Furthermore, since maternal characteristics were rated 8 months *after* the Strange Situation, the study provides a very indirect way of assessing the origins of infant behavior in the Strange Situation! The same can be said of Matas, Arend, and Sroufe's (1978) report that the mothers of 23 infants classified as securely attached at 18 months provided more support and higher quality assistance during tool-use tasks when the children were 24 months old than did the mothers of 15 insecurely attached (A and C) infants. These findings were later replicated by Sroufe and Rosenberg (1982), who studied infants and mothers from Egeland's longitudinal study.

Finally, Sagi, Lamb, Lewkowicz, Shoham, Dvir, and Estes (in press) reported that infants raised in communal settings on Israeli kibbutzim were much more likely to form insecure C-group relationships to mothers, fathers, and *metaplot* (i.e. caretakers) than infants raised in more traditional nuclear family settings. Since the kibbutz arrangement prevents infants from learning to count on specific people, ensures that the infants' cries may go unanswered for long periods (especially at night), the large number of C-group infants could be consistent with predictions that insecure relationships should predominate when infant care deviates from the normal range. However, there are as yet no data concerning the validity of Strange Situation assessments in this cultural context. Furthermore, it is puzzling that there was no significant increase in the number of insecure (A- and C-group) infants, only an apparent change in the relative frequency of C- and A-group infants.

Summary. Evidently, the empirical evidence regar the antecedents of Strange Situation behavior is q weak. Overall, there is some reason to believe mothers who behave in a fashion considered soc desirable by Americans tend to have infants who l behave "securely" in the Strange Situation. Simil major deviations from these patterns of behavior see increase the likelihood of insecure attachments. Ho er, when one seeks to determine what specific aspec maternal behavior are critical, the evidence is incor sive because (a) there are no *specific* replications from study to the next, (b) there are too many – m nonindependent – measures in most studies, and (c clearest evidence comes from comparisons betw abused and nonabused infants rather than from stu exploring variations within the normal range. On small portion of the relevant measures reveals signifi differences, and even within a given study the s measures are often not related in similar fashion in s rate subsamples (e.g. boys and girls), in multiple ass ments of maternal characteristics, or to consecutive sessments in the Strange Situation. When the s dimensions are assessed more than once, either in same or different contexts, the correlates of Str Situation behavior are inconsistent, even within a si study. Thus these studies provide little evidence cerning the *specific dimensions* of maternal behavior are of formative importance. Another serious proble that researchers have failed to identify distinct and licable antecedents of the A and C patterns of inse behavior. These patterns of infant behavior are so cle different that the failure to explain how their origins d not only from B-group infants but also from one anoth surely significant. Similarly, given Ainsworth et (1971) observation that "the subgroups . . . [offer] . much more significant basis of classification of indivi differences than . . . the more broadly defined r groups" (p. 22), it is noteworthy that specific anteced of subgroup patterns have not been identified.

The evidence from intensive longitudinal studies those of Ainsworth and Grossmann is also rend questionable by the absence of information about terobserver reliability. Further, researchers repor statistically significant results seldom indicate how gr proportion of the variance has been explained, bu most cases the portion explained appears to have b small.

Of course, this does not mean that Strange Situa behavior may not have its origins in prior parental bel ior – only that there is very little direct empirical sup for this hypothesis. Researchers have *not* shown u aspects of maternal behavior are of formative importa Until more persuasive data are available, it would hoove reviewers and theorists to remember that A worth's exciting hypotheses remain unproven.

Quantitative consistency of Strange Situation classifications

Because Ainsworth's criteria for classifying infants in groups and 8 subgroups play a key role in Strange Si tion research, it is important to determine how sistently the A-, B-, and C-group labels are use

summarize individual differences in Strange Situation behavior. Specifically, we need to ask whether members of any group or subgroup resemble each other on theoretically relevant variables more than they resemble members of other groups or subgroups.

Using cluster analysis, we recently explored the quantitative consistency of the Ainsworth system (Gardner & Thompson 1983). We found some truth to the claim that the A, B, and C groups usefully summarize individual differences in the variables considered most relevant by Ainsworth and her colleagues. The subgroups tended to fall into distinct clusters, although subclusters within larger clusters frequently did not respect the subgroup boundaries. When more than one subgroup fell into a cluster, they tended to be "contiguous" subgroups (e.g. B_3 and B_4). More interesting, perhaps, is the degree to which the clusters *do not* reproduce the traditional groupings. As in D. B. Connell's (1976) cluster analysis, the boundaries between the clusters did not generally correspond to the A-B and B-C boundaries. In addition, the C subgroups did not reflect a high degree of coherence, and in the non-American samples many subjects did not closely resemble any of the others. Differences within the B group were typically greater than the differences between the A and B groups. Moreover, the division between the cluster containing the As and the "distal" Bs (B_1 and sometimes B_2) and the cluster containing the "proximal" Bs (B_3, B_4) and sometimes some Cs tended to fall in the middle of the B_2 subgroup. The "fit" turned out to be best for a 12-month American sample, the same kind of sample used by Ainsworth in the development of the system. With variation in age or culture, the quality of "fit" declined. Finally, the lack of homogeneity in the C group raised questions concerning the coherence and validity of this group, although the finding is consistent with Ainsworth et al.'s (1971) remarks regarding the heterogeneity of this group. These findings suggest that the A, B, and C groups may represent, not distinct types of infants, but at best an underlying continuum (or several continua) which has (have) been artificially trichotomized (see J. P. Connell & Goldsmith 1982). This implies that researchers should keep open the possibility of developing new ways to group or classify subjects. The cluster analyses suggested that Ainsworth's categories do not constitute an optimal system for summarizing individual differences in Strange Situation behavior.

The temporal stability of attachment classifications

It has been claimed (e.g. Sroufe & Waters 1977; Waters 1983) that evidence of high stability is of crucial importance for the attachment construct, since security of attachment is determined by consistent dimensions of mother–infant interaction. In contrast, it has been argued (e.g. Thompson & Lamb 1983b; Thompson, Lamb & Estes 1982; 1983; Vaughn, Egeland, Sroufe & Waters 1979) that if the classifications accurately reflect interactive quality, they *should* be sensitive to events or circumstances that influence the quality of interaction. If the security of attachment remained stable in the face of stressful events likely to disrupt interaction, for example, it would vitiate claims that the Strange Situation is a sensitive index of the mother–infant relationship.

In the first study to examine the temporal stability of attachment classifications, Waters (1978) sought to show that, contrary to Masters and Wellman's (1974) claim, some measures of mother–infant interaction could show stability over time. He found that 48 out of 50 infants (96%) obtained the same classification at both 12 and 18 months. Stability of subgroup classifications was 53%. The interpretation of these findings depends on the nature of the sample, but unfortunately Waters (1977; 1978) provided little information about these families. He described the sample as lower middle to upper middle class with all families intact and all mothers between 22 and 30 years old. Later Waters (1983) indicated that no special recruitment procedures were used. However, Vaughn et al. (1979) later described Waters's sample as stable in paternal employment, residence, and marital status during the study. This report noted:

> Waters (1978) selected stable middle-class families deliberately. Attachment relationships were viewed as arising from and being maintained by interaction and were expected to be most stable when environmental supports for interaction were stable and when unanticipated changes in stress were minimized. It was important to maximize the chances for stable relationships for two reasons. (Vaughn et al. 1979, p. 971)

Such precautions seem reasonable, given Waters's desire to show that measures of mother–infant interaction *could* show stability, but they limit the generalizability of his estimate of stability.

The effects of family circumstances on the stability of attachment classification were illustrated by Vaughn et al. (1979), who studied socioeconomically disadvantaged, lower-income families. Only about half the mothers were living with a male partner. When 100 of the 267 mothers and infants in the sample were observed in the Strange Situation at 12 and 18 months, only 62% obtained the same classification at both ages.[10]

From a 44-item checklist completed by the mothers (adapted by Egeland, Breitenbucher, Dodds, Pastor & Rosenberg 1979 from Cochrane & Robertson 1973), Vaughn et al. (1979) calculated weighted ratings of the severity of life stresses experienced between the 12-month and 18-month observations. Total scores could thus reflect a small number of highly stressful events or a larger number of minor life changes.

To relate the stress scores to stability in the child's attachment classification, Vaughn et al. (1979) compared 4 groups: (1) infants who changed from securely to insecurely attached (N = 10); (2) infants who changed from insecurely to securely attached (N = 21); (3) infants who were securely attached at both ages (N = 45); and (4) infants who were insecurely attached at both ages (N = 24). One significant difference was found: Mothers of group 1 reported significantly higher life stress than the mothers of group 3. Mean scores for the other two groups fell in between these two. There were no differences between families in which infants changed from insecure to secure and those in which the infants remained insecure.

Subsequently, Farber (1981) assessed attachment stability for 89 mother–infant pairs combined with the 100 families studied by Vaughn et al. (1979).[11] Stability was 60%, and Farber sought to relate changes in the security

of attachment to measures of the mothers, the infants, and their interaction, obtained both prenatally and at various points in the baby's first year. Of more than 200 measures, only a few ratings of prenatal maternal personality were consistently associated with changes in the security of attachment. In light of the large number of statistical tests, these findings can only be considered suggestive until replicated.

In another study, Thompson et al. (1982) examined the stability of attachment between 12 and 19 months in an unselected middle-class sample of 43 dyads. Somewhat surprisingly, they found greater instability than did Waters (1978) and Vaughn et al. (1979): Only 53% of their sample obtained the same attachment classifications at both ages. Stability of subgroup classifications was 26%. Two related events that influence mother-infant interaction – maternal employment and the onset of "regular nonmaternal care" – were significantly associated with changes in the security of attachment. By contrast, critical experiences (such as separations) and changes in general family conditions (such as moving to a new home) were not associated with changes in attachment status. Contrary to Vaughn et al.'s (1979) findings, changing family circumstances were associated with changes in attachment status from secure to insecure as well as from insecure to secure. Thompson et al. (1982; 1983; Thompson & Lamb 1983b) suggested that this may reflect differences in the types of stresses faced by middle- and lower-class families. The severe stresses encountered by disadvantaged families may permit few really constructive resolutions, whereas the less traumatic events encountered by middle-class families may permit responses that have beneficial effects on mother–infant interaction.

Thompson et al. (1982) also reported that maternal employment and nonmaternal care beginning during the *first* year were also associated with changes in the security of attachment during the *second* year; they suggested that short-term changes in mother–infant interaction (resulting, for example, from the baby's entrance into day care; see Blanchard & Main 1979) may have influenced the initial Strange Situation assessments. This would indicate that the Strange Situation is sensitive to short-lived fluctuations in mother–infant interaction as well as to more enduring characteristics.

Two other studies also explored the stability of attachment classifications during the second year, using modifications of Ainsworth's system. D. B. Connell (1976) examined the stability of attachment classifications from 12 to 18 months in a sample of 47 middle-class infants and their mothers. To classify infants at each age, he used Ainsworth's data to compute weighted equations by which infants could be classified on the basis of various discrete behavioral measures and interactive ratings. However, Connell eliminated from Ainsworth's sample infants in the B_1 and B_4 subgroups because they tended to cluster with A-group and C-group infants, respectively. Connell then applied these equations to the interactive scores obtained by infants in his sample to classify the infants. The resulting distribution of infants across groups was unusual (i.e. 30% avoidant vs. 15–20% in most studies; 4% resistant vs. 10–15% in other studies) and may have resulted from his classification procedure. Connell's decision to eliminate variance by excluding the B_1 and B_4 classification options makes his finding that 81% of the infants were similarly classified at both ages d to evaluate.

Main and Weston (1981) also examined the stab attachment classifications from 12 to 18 months. H er, they created a new category consisting of infan were "difficult to classify" using Ainsworth's system Connell, then, Main and Weston may have artil increased stability by eliminating variance in the c cation system. In addition, their sample selectio cedures may also have biased the estimate of sta Families were selected on the basis of maternal parental occupation, infant birth weight, birth con tions, and maternal age. No infants spending mor 25 hours weekly in day care were included, only the mothers worked even part-time, and the father more involved in play and caretaking than the usu selecting out families characterized by circums related to changes in attachment status in the Tho et al. (1982) study, Main and Weston probabl increased the likelihood of stability. The figures c stability for mothers (N = 15) and 87% for fathers (N are thus neither surprising nor generalizable.

Summary. By and large, these studies show that st and change in the security of attachment are pred related to changes in the life circumstances of m and babies. One cannot interpret any estimate bility, or define a "normative" level of stability, w knowledge of these life circumstances. The sensiti the classification system to changing family circums suggests that Strange Situation assessments may i reflect the *current* (not necessarily *enduring*) st mother–infant interaction. The fact that tempor bility is not always high need not undermine the ness of the Strange Situation classification proced fact, evidence concerning the effects of changi circumstances on the security of attachments ma vide important information regarding the validity procedure.

Predictive validity of Strange Situation classifications

> The predictive validity of Ainsworth's attachmer sifications is firmly established. . . . Dozens of s have illustrated the convergent and discriminan ity of Ainsworth attachment assessments. In nc in which the assessments have been done by t persons following Ainsworth's procedures has been a failure to find expectable relations. Wha procedure can boast such a record? (Sroufe & 1982, pp. 744–45; italics in original)

The Strange Situation procedure has become p in part because of claims that Strange Situation be predicts important aspects of the child's behavior a as several years later. Two implicit hypotheses cc ing predictive validity can be entertained. First, th such as Sroufe (1978; in press) propose that the sensitive period in the first year of life during wh quality of parent–infant interaction has a disprop ate impact on later development. Thus "the beh organization evolved with respect to an early de mental issue lays the ground work for subsequent ioral organizations. . . . The nature of the earlier

ioral organization, with attachment promoting exploration, makes the smooth movement to more autonomous functioning virtually inevitable" (Sroufe, in press, ms, p. 4). Second, one could propose that the effects of early parent–infant interaction are clearly evident only when there is continuity in the quality of care. In such circumstances, the effects of early patterns of interaction are maintained by consistency in the quality of care such that later effects may be attributable to either previous or contemporaneous influences. This second perspective implies that predictive validity can only be investigated and understood when one records both early formative influences *and* contemporaneous influences or intervening factors known to be associated with changes in the patterns of care. When rearing conditions change, early events may have no predictive value at all. It is important to distinguish between these two hypotheses because although most researchers and reviewers believe that the first hypothesis has been verified, we believe that the evidence is more consistent with the second.

The Minneapolis middle-class sample. The most widely cited studies concerned with the predictive validity of Strange Situation classifications were conducted in Minneapolis by Sroufe and his colleagues. These studies have involved either of two samples: a middle-class sample recruited by Matas et al. (1978) and the poverty sample studied by Egeland and Farber (in preparation) and Vaughn et al. (1979).

In the earlier study, Matas et al. (1978) related security of attachment at 18 months to measures of the child's and the mother's behavior at 24 months in play and problem-solving situations. All were drawn from a list of parents who volunteered to participate in studies shortly after their infants were born. Such families are likely to be stable middle-class families (especially if they can still be located 18 months later) as was indicated by high temporal stability (100%) in attachment classification in a subsample of 14 of the 38 infants studied.

The securely attached infants engaged in more symbolic play than avoidant or resistant infants. In the problem-solving situation, the securely attached infants were more enthusiastic and compliant than non–B-group infants; they also ignored less and exhibited fewer frustration behaviors, showed more positive affect, and engaged in less negativism, crying or whining, aggression toward the mother, and negative affect. The avoidant and secure groups also differed in the expected direction on 5 of 7 measures tested, but there were no reported comparisons between B- and C-group infants, despite the theoretical importance of such comparisons. In all, 10 of 18 infant behaviors and both maternal ratings (supportive presence, quality of assistance) revealed significant group differences between B and non-B groups. There were no significant group differences in Developmental Quotient (DQ) between securely and insecurely attached infants. Factor analysis of the infant and maternal variables yielded 5 factors ("competence," "temperament," "DQ," and 2 unnamed factors), the first and third of which were related to security of attachment in the expected direction.[12]

These results are certainly both impressive and interesting, although they raise questions about the locus of the stability. Is it, as the authors claim, that securely attached infants become "better adapted" toddlers, or simply that the mothers who fostered secure behavior at 18 months continue to provide an appropriate context for the child 6 months later? It is impossible to tell, since the sample is one in which there is likely to be continuity in the quality of care. It is of interest that the group differences in maternal behavior were greater than the differences between groups of infants. At the very least, it would be valuable to know whether group differences in infant behavior remain when differences correlated with the contemporaneous maternal ratings are partialed out. Note that none of the measures was reported to distinguish between the A- and C-group infants.[13]

Waters, Wippman & Sroufe (1979) further studied 18 month olds (N = 36) drawn from the sample studied by Matas et al. (1978). Four mother-directed behaviors indicating positive interaction and 3 nonindependent ratings of "affective sharing" were scored from episode 1 of the Strange Situation. Additional ratings of affective sharing were obtained in a 10-minute free-play session at 24 months from 45 infants (including 30 of those involved in the first phase) whose attachments were assessed at 18 months. The securely attached infants were more likely to smile at their mothers at 18 months than were insecurely attached infants, but there were no significant differences in the frequencies of showing or giving toys or looking at the mother. Various combinations of these behaviors also revealed no significant group differences. There were significant differences between the secure and insecure groups in affective sharing at both 18 and 24 months, but apparently none of the measures revealed significant differences between avoidant and resistant infants, if these differences were even tested.

Arend, Gove, and Sroufe (1979) later relocated 26 of Matas et al.'s sample when they were between 54 and 70 months old. Twelve were initially secure and 14 insecure (8 A, 6 C), suggesting selective attrition of the securely attached infants. Each child was described, using a 100-item Q-set, by a nursery school or kindergarten teacher who had known the child at least 8 months. Block and Block (1980) had derived "composite Q-sort definitions of both ego resiliency and ego-undercontrol" (Arend et al. 1979, p. 952) with which the child's scores on the relevant items were correlated to yield ego-resiliency and ego-control scores. In addition, a 90-minute battery of tests was used to derive composite measures of ego resiliency and ego control. The 2 independent measures of ego resiliency and ego control were modestly but significantly correlated, whereas the scores for resiliency did not correlate with those for control. Both the Q-sort and laboratory measures showed the securely attached children to be more ego resilient than the anxiously attached children. A composite score comprising the 4 infant measures (but not the maternal measures) loading highest on the Matas et al. competence factor was significantly correlated with the laboratory measure of resiliency, and marginally correlated with the teacher-derived resiliency score ($p < .06$). The 24-month maternal ratings from the Matas et al. study were significantly correlated with the laboratory measure of resiliency. There were no overall group differences on the ego-control dimension – perhaps, as the authors claim, because both overcontrol and undercontrol are undesirable, meaning that one would expect to find B-group children neither high nor low on

this dimension. A post hoc analysis revealed that the resistant infants were high and the avoidant infants low on ego control; the B-group infants fell in between with a mean close to that of the C-group children. The 3 additional measures of curiosity all showed the securely attached children to be more curious; 2 of these measures were significantly correlated with the teacher-derived measure of ego resiliency.

There were thus few differences between avoidant and resistant infants but reliable differences between secure and insecure infants. The interpretation of these differences is problematic, however, since this was a select, stable, middle-class sample, and Arend et al. candidly discussed the importance of continuity in patterns of caretaking influencing the predictive utility of attachment classification. Quite plausibly, this continuity in rearing conditions is critical, as suggested by many of the findings reported in this section.

The Minneapolis disadvantaged sample. Much more extensive attempts have been made to assess predictive validity using the inner-city poverty sample described by Egeland and Farber (in preparation) and Vaughn et al. (1979). In the first follow-up study, Pastor (1980; 1981) observed 62 of the children in the sample at 20 to 23 months. Although the published report (Pastor 1981) fails to mention it, subjects were included only when the child obtained the same attachment classification at both 12 and 18 months (Pastor 1980). Twelve A-B pairs, 13 B-B pairs, and 12 C-B pairs were studied, with one member of each pair considered a target and the other (always a B-toddler) considered a control. A Bayley assessment at 24 months revealed no group differences in DQ.

Six 5-point rating scales were scored from videotapes by naive observers. Interobserver reliability was not specified, other than that it was "significantly better than chance by the Lawlis-Lu chi-square test ($p \leq .01$)" (Pastor 1981, p. 329). Twelve discrete categories of peer-directed behavior and 16 of mother–child interaction were also coded. Four of the 6 ratings revealed significant group differences, with the B-group children scoring higher on overall sociability, orientation to peer, orientation to mother, and mother supportiveness than the A- and C-group infants, between whom no differences were found. The A- and B-group children made more social bids and ignored fewer offers by peers than those in group C, while B-group children redirected their own activities after an object struggle more than A-group children did. Six of the 16 discrete measures of mother–child interaction revealed significant differences, but in only two cases were the B-group children distinguished from both of the insecure groups. Since few of the discrete behavior measures were independent of one another and there were only three significant group differences on fully independent measures, these data suggest equivocal or no reliable group differences on the discrete behavior measures. The only group differences, therefore, were on several rating scales.

Later, Sroufe and Rosenberg (1982) attempted to replicate Matas et al.'s findings using subjects from Egeland's sample. Infants who were securely attached at 18 months were later (at 24 months) more enthusiastic, affectively positive, and compliant; spent less time away from the task; showed less negative affect; and received more positive scores on 2 global ratings than those w insecurely attached. Looking at the overall pa findings, we can conclude that for this lower sample experiencing greater socioeconomic ins the tenor of the results matches Matas et al. findings, although there were few specific repl Of even greater concern is the fact that there we measures distinguishing between 24 month o were securely or insecurely attached at 12 mon sumably this was because many of these subject enced changes in attachment status between 1 months (Vaughn et al. 1979). Thus these findings that attachment classifications have substantial p validity only when there is continuity in the q care or in the security of attachment relationsh interpretation is supported by Erickson and ((1981). Using the same data, they found that the attached infants were more compliant at 24 mor the insecurely attached infants, but *only when t stability between 12 and 18 months in the se attachment*. The avoidant and resistant infants differ from one another.

A later follow-up of subjects in the same sa curred when the children were around 4 years ol invited 39 of the children to participate in nursery school program. The children were div two groups. Group 2 contained 24 children, w numbers classified as avoidant, resistant, an when seen in the Strange Situation at 18 months. one of the 24 had the same classification at both months; the other 3 changed classification bet and 18 months, but there was consistency bet 18-month Strange Situation and 24-month too sessments in these 3 cases. Nine of the 15 ch group 1 had the same classification at 12 and 18 r Bs, 2 As), 2 shifted from one insecure classificati other, and the remaining 4 were insecure in or ment and secure in the other. Overall, there subsample was a highly stable subset of Egela ple, deliberately selected (Sroufe, in press). Th were equated on IQ, age, race, and (for grou

An enormous amount of data was gathere variety of procedures; scores on at least 110 were collected from a sample of 39 children. were blind with respect to attachment status an hypotheses.

When data from both groups were combine measures yielded significant differences betwe and the non–B-group infants. The B-group inf scored higher on the ego-resiliency and self-e sorts (which intercorrelated highly, $r = .85$); ratings of agency and positive affect; lower on affect, on multiple ratings of dependency, and o of their seating relative to the teacher; and composite measures of positive and negative although few of the 60 specific behavioral items behavior" and "wandering" are two that are m revealed significant differences. The B-grou were also ranked higher in social competence, friends, and popularity (on sociometric instrum on ratings of social skills, compliance, and empa *one* of these measures (empathy) yielded d between the A and C groups, but the significa post hoc comparison was not reported. Howev

basis of informal written comments by the teachers, it was later possible to distinguish the A and C groups; the As were deemed hostile, isolated, and disconnected, while the Cs were rated impulsive and helpless. Sroufe does not say whether descriptions of the Bs were similarly classified, whether they were ever misclassified, or whether a B class was available when the raters were determining whether the teachers' impressions were A-like or C-like. A- and C-group infants were also differentially classified in terms of both the 5 "most characteristic"items from the Block Q-sorts and teacher responses recorded on descriptive checklists. The fact that these measures provided the only way of distinguishing between As and Cs is cause for concern because as Sroufe himself wrote: "*A reasonable and testable prediction is that the various patterns of adaptation shown by avoidant infants will represent meaningful developmental outcomes and that the set of adaptational patterns shown by avoidant children will be distinct (probabilistically) from the set of patterns shown by resistant infants*" (in press, ms, p. 8; italics in original). In fact, neither this study, nor any of the others reviewed here, has yielded strong support for this prediction.

Nevertheless, this study appeared to yield strong evidence of differences between the B- and non–B-group children – differences evident on a substantial portion of the (albeit nonindependent) measures. However, the specific sample was carefully selected from the larger sample to ensure stability of classifications. As suggested in an earlier section, this means that there was probably substantial continuity in the quality of care, whether good or bad. It is thus impossible to tell whether the differences among preschoolers are attributable to differences in earlier rather than contemporaneous patterns of parent-child relationships, since continuity may extend to the preschool years also. This problem is extremely important, given Sroufe's desire to attribute later differences in child behavior to *earlier* patterns of maternal behavior and mother–child interaction. Sroufe's interpretation could only be sustained if there were associations between 12- or 18-month classifications and later behavior in samples in which marked discontinuity in quality of care was evident. Further, in light of differences between the first and second class groups, the decision to combine them for some analyses is questionable. No mention is made of the independence or nonindependence of the measures, and the data are selectively reported – sometimes for separate classes, and sometimes only for the combined sample. This makes evaluation of the findings difficult at best.

Exploratory and cognitive competence. Several students of Ainsworth pioneered research on the predictive validity of the Strange Situation. The first was Silvia Bell (1970), who assessed 33 infants at home on tests of object and person permanence 3 times between 8.5 and 11 months. One week after the third testing session, the infants were observed in the Strange Situation. Twenty-three subjects "showed a preponderance toward discrepancies in favor of person permanence . . . [positive decalage] . . . Seven subjects . . . tended to show discrepancies in the opposite direction . . . [negative decalage] . . . three babies [showed] no significant differences by the third testing session" (p. 301). "Babies in Group B were the only ones to show a positive decalage, and all but one of them had such a decalage. All but one of the babies in Groups A and C had a negative decalage" (p. 303). Babies in the positive decalage group had significantly higher object concept scores at every test session than those in the negative decalage groups.

Unfortunately, these strikingly clear results are seriously compromised by the fact that the Strange Situation classifications were based on narratives dictated by Bell, who was also the person responsible for the assessments of object and person permanence. The potential for bias was thus substantial. In addition, the study is not really a test of predictive validity, as decalage was assessed prior to or roughly contemporaneously with the Strange Situation. At best, this was a study of external correlates in which the findings were compromised by the failure to control for investigator bias.

Another Ainsworth student, Thomas Pentz (1975), observed 31 mothers and children in the Strange Situation when the children were 28 months old; he assessed language acquisition at both 28 and 36 months. No significant relationship was found between security of attachment and language acquisition. There are three reasons why this may have been the case. First, the Strange Situation may not be valid for assessing security of attachment in 28 month olds: It was developed for use with 12 month olds. Second, the hypothesis linking maternal sensitivity (indirectly inferred from the Strange Situation behavior of the child) to language development may be incorrect, or the indirect means of testing it may have obscured whatever relationship exists. Third, the groups may have been too small and heterogeneous to permit a sensitive test of the hypothesis.

Tracy, Farish, and Bretherton (1980) studied the relationship between attachment security at 13 months and exploratory competence (12 months) in a sample of 40 infants. Only 1 of 16 tests revealed a significant group difference: about the number one would expect by chance. Only 2 of 16 tests of differences in mother-directed behaviors in the exploratory context yielded significant differences, suggesting low transcontextual consistency as well. Similar issues were explored by Belsky and Garduque (1982) in a sample of 60 infants seen in the Strange Situation and a play session with one parent at 12 months and the other at 13 months. The securely attached infants engaged in more play and more "transitional" play and showed less disparity between the highest level of play generated spontaneously and the highest level elicited. Unfortunately, differences between avoidant and resistant infants were not tested. Although more impressive than Tracy et al.'s, these data likewise do not demonstrate predictive validity, because the assessments of attachment security and exploration were contemporaneous.

Finally, Hazen and Durrett (1982) explored the relationships between security of attachment, exploratory competence, and cognitive mapping in a sample of 28 children who were seen in the Strange Situation at 12 months and in a laboratory "playhouse" between 30 and 34 months. Hazen and Durrett reported that "the children who had been classified as securely attached were more active explorers and higher in cognitive mapping abilities than those classified as anxiously attached" (p. 756), but their findings do not support this strong conclusion. First, the *secure* B_1 and B_4 subgroups were com-

bined with the A and C groups respectively, and were considered *insecurely* attached for purposes of analysis. We do not know whether significant differences would have emerged if the A, B, and C groups were compared, as would be necessary to support Hazen and Durrett's conclusion. Second, on only 1 of 5 measures of exploration and 1 of 3 measures of cognitive mapping did the B_2 and B_3 infants' scores differ from those of both the "avoidant" (A_1, A_2, B_1) and "resistant" (B_4, C_1, C_2) infants. There were no "group" differences on three measures, and differences between the "secure" and only one of the "anxious" groups on 3 others. Contrary to Hazen and Durrett's conclusion, therefore, the report contained no unambiguous evidence regarding the predictive validity of the three conventional attachment security groups.

Cooperation and compliance. Main (1973) observed 40 infants in the Strange Situation at 12 months and related their behavior in this context to performance in a Bayley test session at 20.5 months and in an hour-long play session with a habituated stranger at 21 months. Seven infant behaviors were scored from the Strange Situation narratives (avoidance, resistance, crying, touching, vocalizing, smiling, looking). Three scores were derived from the Bayley assessment (Developmental Quotient, cooperation, gamelike spirit) and 17 from the play session (6 pertaining to exploration, 5 to play with the unfamiliar adult, and 6 to the semiotic function), but no information about observer reliability was reported. Scores in the Bayley test cluster and the semiotic function cluster were highly intercorrelated. Two summary measures – *exploration* and level of cognitive functioning and *affect;* playfulness and anger – were also derived and their components were used as sets in multiple regression analyses. Five measures of maternal behavior revealed no group differences, although additional ratings of the transcripts and videotapes later indicated substantial group differences in maternal behavior (Londerville & Main 1981; Main et al. 1979).

For the purpose of analysis, A- and C-group infants were grouped together in one insecure, non-B, group (N = 15). All of the Bayley test measures, 4 of the 6 exploration measures, 3 of the 5 playmate play measures, and 1 of the 6 semiotic function measures showed significant B vs. non-B differences – all favoring the B-group infants, who were more playful, exploratory, sociable, and cognitively competent. In light of the different correlates of avoidance and resistance, it is unfortunate that Main did not distinguish between the A and C groups.

Main's findings are difficult to interpret because of the significant B- vs. non–B-group differences in DQ. Such differences have not been found in any other studies (Egeland & Farber, in preparation;[15] Joffe 1981; Matas et al. 1978; Pastor 1981; Waters et al. 1979). At the very least, it would be necessary to control (by covariation procedures) for differences in DQ and *then* examine group differences on the other measures. This precaution is especially pertinent in light of the common correlation between DQ and sociability or cooperation (Lamb 1982a), and the probable association between DQ and the measure of exploratory competence in this study. In the absence of such analyses, we cannot say whether Main found differences between children who were mentally different, or whether there were in ferences attributable to the security of infan attachment.

Main's data were later reanalyzed by Londe Main (1981), who focused on maternal training and filial compliance. Intercoder agreement and coders were blind to scores on other mea Strange Situation classifications. Again, unfo the non–B-group infants were lumped toge single insecure group. With the exception o number of siblings, all 9 toddler variables as composite measure were significantly related i pected direction to security of attachment; se tached infants were more compliant and cooper less disobedient and troublesome than the i attached infants. Two of the 4 maternal variables voice and forcefulness of physical intervention likewise related to security of attachment in the direction. Resistance to the stranger was unr compliance, but resistance to the mother at 1 was negatively related to cooperation with th tester and compliance with the mother. Lik (1973) results, however, these findings are d interpret in the absence of statistical control effects of DQ.

In their study involving 74 dyads, Maslin a (1982) found some relation between attachmen at 12 months and qualities of mother–child in and compliance at 13 and 24 months. Effects we edly evident on 26% of the measures analy unfortunately few were identified, so their inde cannot be assessed. Some effects were evide group means (A, B, C) were compared; oth evident only when the two insecure groups w bined. All reported differences were in the direction, but the report (which was prepared f presentation) contains very little information specific measures and about the procedures protect against rater bias or halo effects. Thus, ings cannot be properly evaluated until a fuller available. However, the findings suggest that attached infants later have more harmonious ships with and are more compliant with their than insecurely attached infants – at least in a nantly middle-class, stable context.

Sociability with unfamiliar adults. Main and (1981) observed 61 infants in the Strange Situa their mothers and fathers at 12 and 18 months. of the infants were seen first with their mother with their fathers. One week before the Strange Situation, 44 children were seen in a pla (with their mothers present) in which a clown a to evoke apprehension, delight, and "concern tion." The infants' degree of "relatedness" to t and of "conflict behavior" during the session w from the videotapes by naive coders. Only 1 of 2 who were securely attached to their mothers conflict behavior, compared with 11 (56%) o insecurely attached or "unclassified" infants. U same subsample, it was found that security mother overrode the relationship with the fath

gard to conflict. The same was true for relatedness to the stranger; in both cases, performance was better for those rated secure than for those rated insecure with their mothers, regardless of the babies' attachment status with their fathers. However, there were apparently no significant differences in relatedness between avoidant, resistant, or "unclassified nonsecure" infants. The report provides no information about relationships between security of attachment and the other variables. In any event, this study is not informative regarding the predictive validity of Strange Situation behavior. At best, it provides evidence regarding the construct validity or external correlates of Strange Situation behavior. The information is limited, however, by the lack of detail regarding the constructs assessed and by confounds in the procedure.

Thompson and Lamb (1983c) observed 43 middle-class infants and their mothers in the Strange Situation at 12.5 and 19.5 months. Immediately prior to each Strange Situation, infants were observed in a brief standardized procedure designed to assess sociability toward unfamiliar adults (Lamb 1982a). Following Easterbrooks and Lamb (1979), Thompson and Lamb distinguished B_1B_2 from B_3B_4 infants for analytic purposes. At each age, the B_1B_2 infants were most sociable and the C_2 and B_3B_4 infants least sociable, even though, given the frequency of changes in classification (see the section on temporal stability), different infants fell in each of the groups each time. When the attachment classification changed over time, the two sociability scores were not significantly correlated, but they were highly correlated when the attachment classification was the same at both ages. These findings suggest that continuity in patterns of parent–child interaction, rather than stability in some characteristic of the child, may account for the predictive validity observed in this and other studies.

Lamb et al. (1982) related sociability to Strange Situation classifications in a sample of 51 Swedish infants and their parents. As predicted (Easterbrooks & Lamb 1979; Thompson & Lamb 1983c), infants who had B_1B_2 relationships with their fathers were significantly more sociable than those with B_3B_4 or A relationships. There was also a tendency for infants who were securely attached to their fathers to be more sociable than those who were insecurely attached. The security of infant–mother attachment was unrelated to sociability, however. There was also no effect of degree of involvement in caretaking, and, contrary to Main and Weston's (1981) findings, no evidence that those who were securely or insecurely attached to both parents were especially high or low (respectively) in sociability.

Sociability and social competence with peers. In addition to Pastor (1980; 1981), whose work was discussed earlier, several other researchers have studied the relationship between security of attachment and later interactive skills with peers. Lieberman (1977) attempted to relate the quality of the child-mother relationship to preschoolers' social competence with peers. Since both the 3-year Strange Situation and the home-based measure of attachment security are of unknown validity, these data provide equivocal evidence concerning (a) the relation between security of attachment and peer competence, and (b) the predictive validity of Strange Situation behavior. Thus they are not discussed more fully here.

More impressive findings concerning the relation between security of attachment and peer competence were reported by Waters et al. (1979), who filmed 15-month-olds (N = 32) and their mothers in a novel situation involving 5–10 minutes of free play, the entrance of a stranger, a 1-minute separation, and then a mother–infant reunion. To rate security of attachment, information about reunion behavior was supplemented by measures of separation and preseparation behavior. Although the Strange Situation procedure was not used, the 20 "securely attached" infants indeed sound like B-group infants, and the 12 "anxiously attached" like A- and C-group infants. Unfortunately, the avoidant and resistant infants were lumped together in a single "anxious" group. These groups did not differ on Bayley Scale assessments of DQ at 14 months or on Stanford-Binet assessments at 36 months.

When the children were 3.5 years old, naive observers performed Q-sort assessments on the basis of a 5-week observation in a preschool setting. The mean of the scores assigned by two independent raters (r = .61) was used in group comparisons involving two 12-item criterion Q-scales assessing peer competence and ego-strength/effectance. Three items were later reassigned, however, because they correlated better with the set other than the one to which they were initially assigned. Composite scores were computed by summing scores on the items in each set. The two composite scores were highly correlated (r = .61). Eleven of the 12 peer-competence items, as well as the composite score, distinguished the two attachment groups. Only 5 of the 12 ego strength/effectance items distinguished the two groups, as did the summary score. Since the items in each set were highly intercorrelated, neither the number of variables revealing significant group differences nor the statistical significance of differences on the composite measures are very informative. However, while the magnitude of the group differences cannot be assessed, there is evidence of group differences, especially in peer competence, two years after group assignment.

In their study, Easterbrooks and Lamb (1979) focused on differences between 18-month-old infants in the B_1, B_2, B_3, and B_4 subgroups within the secure (B) group in order to test the validity of these subgroup distinctions. "Focal" infants from three derived "groups" (B_1 & B_2, B_3, B_4) were observed in an unfamiliar playroom with an unfamiliar "foil" playmate who was always drawn from either the B_1 or B_2 subgroup to ensure comparable playmates for all subjects. Of 21 discrete behavioral measures and 3 composite measures of peer interaction, there were significant overall group differences on 2 of the discrete measures and 2 of the composite measures, with the focal B_1 B_2 infants spending more time interacting with and being close to their peers than focal B_3 and B_4 infants. Pairwise contrasts revealed significant B_1 B_2 vs. B_3 differences on 7 measures (including all 4 of those showing overall differences) and B_1 B_2 vs. B_4 differences on 5 measures, including only 2 of the 4 showing overall differences. As expected, the B_1 B_2 infants also spent less time in the peer session touching and being near their mothers, indicating that there was some transsituationa

consistency in responses to mothers, since these infants are also noted for distal interaction in the Strange Situation. These differences in mother-directed behavior make it difficult to interpret the group differences on peer-interaction measures. It would be important to know, however, whether group differences on the peer-interaction measures remained when variance attributable to differences in contemporaneous mother-directed behavior was partialed out.

Summary. The strongest evidence regarding predictive validity has been obtained in studies involving samples in which stability in family and child-care circumstances could either be assumed or was actually ensured by subject-recruitment procedures. The data do *not* support the hypothesis that early experiences during a sensitive period in the first year have long-term implications. Rather, it seems that when there is continuity in parental behavior and other circumstances likely to influence child development, patterns of child behavior are maintained. The implication is that current or recent rather than early patterns of child–parent interaction are the correlates or antecedents of observed differences in child behavior. Presumably, clearer and more consistent relationships between parental and filial behavior would be obtained if parental behavior were assessed contemporaneously instead of being estimated indirectly by earlier assessments in the Strange Situation supplemented by evidence of (or assumptions regarding) temporal stability in family and child-care circumstances.

A second problem lies in analytic and methodological procedures that essentially stack the deck to ensure later differences between children earlier classified as securely or insecurely attached. Selective sample recruitment is but one example of a general tendency. Hazen and Durrett (1982), for example, grouped the B_1 infants with the A-group infants and the B_4 infants with those in the C group for analytic purposes; the results of any comparisons among these derived groups are uninterpretable. Waters et al. (1979) reassigned items from one composite measure to another; this procedure effectively capitalized, in a post hoc fashion, on whatever group differences existed. Similarly, Sroufe (in press) continued deriving measures until some were found to distinguish between A- and C-group infants. The same study illustrates another problem characteristic of research in this area: Many measures were used, but attention was focused almost exclusively on those revealing group differences rather than on the number of measures that failed to reveal the expected group differences. The problem is compounded by the vagueness of hypotheses and of poorly defined rating scales, which allows researchers to explain, post hoc, why comparable measures reveal contradictory findings (e.g. Matas et al. 1978) or why only a subsample of the measures revealed differences consistent with the hypotheses. In addition, the absence of reliable differences between A- and C-group infants, even though these should be found if Strange Situation classifications can be said to have predictive validity, is rarely remarked upon, but is of crucial importance. Finally, the interobserver reliability of critical measures sometimes appears to be very low: In Pastor's (1981) study, for example, significant group differences were evident almost exclusively on rating scales that may have been unreliable. This is especially pr where there is potential for observer bias or a pervading multiple ratings by observers. In o (e.g. Easterbrooks & Lamb 1979; Main & We Pastor 1981), nominally blind raters, familia Strange Situation classification system, could h ed the classification status of the subjects observing by assuming some transcontextual co This would be especially problematic when b the two contexts was assessed roughly conte ously. Near-contemporaneous assessments, do not constitute studies of predictive validi they may suggest some kind of transcontextual cy in infant behavior (e.g., Belsky & Gardu Easterbrooks & Lamb 1979; Lamb, Hwang Frodi 1982; Thompson & Lamb 1983c). The t dictive validity" is even more inappropriate "outcome" is assessed weeks or months befor posed antecedent (e.g. Main & Weston 1981; 1980)!

Despite these problems, there do appear t reliable relationships between Strange Situat ior and children's later characteristics, at leas non–B-group comparisons. The data show, hov there is temporal continuity in children's cha *only when they are maintained by continuity teristics of the rearing environments.* C Sroufe's (1979) conclusions, there is no suppor regarding the long-term effect of experiences early sensitive period.

The interpretation of Strange Situation b

In attempting to explain why these patterns of a behavior should exist, some psychologists (1979a; Main 1981; Sroufe 1979; in press; Wate 1982) have interpreted them in terms of the p evolutionary biology. Thus: "The behavior of t attached infant and his responsive mother, in iar and unfamiliar surroundings, can be recogn expected evolutionary outcome of infant attac attachment behavior and of a reciprocal mate ior system which are preadapted to each ot worth 1979a, p. 37). "To the extent that t environment of rearing departs from the envi which a baby's behavior is preadapted, behavi lies may be expected to occur" (Ainsworth 1 Thus the B pattern of behavior is normative b *adaptive*, since infants behave in a way tha their chances of survival in the species' envi evolutionary adaptedness. They seek proximi or interaction with the attachment figures return after an absence, maintain interaction are present, and use them as secure bases fro explore. The A and C patterns, by contrast, ar maladaptive or pathological, because these in behave in a fashion that maximizes the pos survival. These interpretations are based o (1969) claim that selection by predation ha infants who are predisposed to emit proximity behaviors (such as smiling and crying) to whic predisposed to respond. Attaining the prote imity that is of survival value for the infant dep

prompt and appropriate responsiveness of the adult to the infant's signals.

This interpretation of infant behavior in the Strange Situation raises many important questions. Let us begin with some general theoretical considerations concerning the concept of adaptation. Natural selection tends to favor traits that increase inclusive fitness – the reproductive success of individuals or their close relatives, who share many genes through common descent (Hamilton 1964). Survival per se is not selected for: Selection is for relative success in the context of lifetime reproduction (for a review of life history evolution see Charlesworth 1980; Stearns 1976; 1977). A key notion in evolutionary thinking is that fitness always involves tradeoffs. In many species, for example, the expenditure of resources in reproduction involves increased risk of mortality to the reproducer (Stearns 1976). Natural selection favors an optimal balance between parental survival and the production and care of offspring, with "optimal" defined by the balance that maximizes lifetime parental fitness (Charnov & Krebs 1974). This implies that parents are not necessarily selected to maximize the survival and development of individual offspring, but to divide parental resources among the entire brood so as to maximize parental fitness gains over the whole family (Alexander 1964; Ghiselin 1974; Trivers 1974). For this reason, the interests of offspring and parents are often in conflict.

Attachment theory focuses on factors that enhance the survival of individual infants and takes for granted that adults can be expected to behave appropriately. This assumption is dubious at best: Why (from a fitness point of view) should adults care for infants in the first place? The answer is that they should be willing to expend resources and take risks for their own children and close relatives so as to ensure the propagation and survival of their genes. Unrelated adults should not be willing to invest resources in, or take risks for, infants unless some reciprocity exists or the adults are unable to distinguish kin from nonkin. Consequently, infants (at least when they have the choice) should direct attachment behavior only toward individuals who have shown, by their prior actions, a willingness to bear the risks and costs of child care. There will be circumstances, however, in which it would not be in the fitness interest even of these adults to aid, give resources to, or even remain near their youngsters. Only if one takes into account the alternative opportunities for fitness gain can the adult's actions toward a specific youngster be understood in a natural selection sense.

Several implications of these principles bear directly on the interpretation of behavior in the Strange Situation. First, even though adults may well have been selected to protect their offspring under most circumstances, with attachment relationships mediating this protection, we cannot assume that adults will always behave in this way. The tradeoffs between parental reproduction and parental survival (and hence opportunities for future reproduction) are subtle, and it is critical that the relative costs and benefits to both the adult and the individual infant be assessed when determining the adaptive significance of attachment behavior. The assessment of these costs and benefits has never been attempted.

Second, it is not clear whether "adaptive" attachment behavior is believed to bring fitness advantages to infants in contemporary times. Ainsworth (e.g. 1979a) focuses her discussion on the ultimate value of attachment behavior in the environment of evolutionary adaptedness, a largely unknown setting. Matters are even less clear when we consider claims regarding the long-term adaptive consequences of Strange Situation behavior. Ainsworth and Bowlby imply that attachment "failure" heightened the risk of death for the child in the environment of evolutionary adaptedness. In other words, survival through childhood is the focus. But Ainsworth's (1974) discussion of the "secure base phenomenon" suggests that attachment may also play a key role in child development *beyond survival*. For example, securely attached children can use attachment figures as a "secure base" for exploration of their environment. By facilitating cognitive and social development, this may substantially affect later development (Ainsworth & Bell 1974). Main (1981; Main & Weston 1982) argues that the function of avoidant behavior is the maintenance of "flexible behavioral organization." Sroufe (1978; 1979; Sroufe & Matas n.d.) even argues that the predictive validity of Strange Situation behavior (see below) is attributable to a "continuity of adaptation." Those who behave adaptively in infancy, he proposes, also behave adaptively as toddlers or preschoolers, even though the survival-relevant tasks of the different age periods may be very different. Sroufe contends that "a healthy pattern of adaptation is one which promotes a flexible, effective behavioral organization with respect to subsequent issues [in behavioral development]; an unhealthy pattern is one which does not" (in press, ms, p. 5).

With respect to the view that infant attachment is survival-relevant adaptation, there are several conceptual shifts represented in Sroufe's proposal. Most important, Sroufe implies a view of adaptive infant behavior radically different from that originally proposed by Bowlby (1969), who was concerned with *juvenile adaptations* rather than *ontogenetic* or *developmental adaptations*. Juvenile adaptations (like the "selection funnel" discussed by Konner 1977) are traits that aid individuals through their youth. Once this stage is passed, these traits have little effect or value. Fitness, however, refers not just to survival and development through the prereproductive years, but to lifetime reproductive success. Many things children do are in fact necessary for effective functioning as reproductive adults, and these are ontogenetic adaptations. *Ontogenetic adaptations can only be understood by considering what the traits mean for reproductive success when adulthood is reached.* Sroufe's concept of adaptiveness – the maintenance of flexible behavioral organization – appears to refer to an ontogenetic adaptation, a mechanism (essentially, ego strength) that develops to serve the organism throughout the res life, and is thus distinctly different from Bowlby's conc of adaptation for survival – a juvenile adaptation.

In fact, it is not clear that Sroufe and Main h evolutionary fitness in mind when they discuss the "ad tiveness" of flexible and effective behavioral organizati The claim that secure attachment promotes flexi effective behavioral organization appears to endow infant with an excessively nonspecific adaptation, g for any and all environmental tests. It is perhaps misle ing to reify adaptiveness into a general trait, wher more appropriately refers to the consequences for vival of specific patterns of behavior pertinent to part

lar stages of the life span. In addition, "health" in the mental hygiene or psychiatric sense may have no connection with evolutionary fitness. For all we know, psychopaths may leave as many (or more) offspring as "flexible," well socialized individuals. In all, the concept of adaptation needs to be defined carefully and consistently.

The modern biological view of adaptation also leads us to question the belief that there is a single, normative pattern of parental behavior forming the sole adaptive niche for human infants. As mentioned before, the assumption that adults can always be expected to respond appropriately is dubious at best. In general, they should be willing to expend resources and take risks for their own children and close relatives, and even then parental behavior toward any given infant will be a function of, among other things, the social context of child rearing; the temporal, energetic, and physical resources available to the parent; the alternative uses the parent could make of these resources; and the parent's sex, since the costs and benefits of parental investment are different for males and females (Symons 1980; Trivers 1974). In a recent article, Hinde (1982) has clearly identified the fallacy of viewing a single pattern of parental behavior as adaptive:

> The picture of an environment of evolutionary adaptedness serves well enough as a first stage in our thinking. But as we go beyond that, we must accept that individuals differ and society is complex, and that mothers and babies will be programmed not simply to form one sort of relationship but a range of possible relationships according to circumstances. So we must be concerned not with normal mothers and deviant mothers but with a *range* of styles and a capacity to select appropriately between them.
>
> At one level of approximation, there are general properties of mothering necessary whatever the circumstances. At a more precise level, the optimal mothering behavior will differ according to the sex of the infant, its ordinal position in the family, its mother's social status, caregiving contributions from other family members, the state of physical resources, and so on. *Natural selection must surely have operated to produce conditional maternal strategies, not stereotyping.* (p. 71; italics in original)

The same applies to individual differences in infant behavior: There is no reason to believe that the B pattern is necessarily "more adaptive" than either the A or C pattern. Indeed, these patterns may represent adjustments by infants to varying styles of parental care.[16] The A, B, and C patterns may in fact be equally adaptive, such that in specific circumstances, the avoidant, secure, and resistant patterns represent appropriate adjustments designed to maximize the infants' chances of living to reproductive maturity. Indeed, there may be no differences between A, B, and C infants in adulthood because these behavioral patterns may simply represent alternative pathways to maturity, given the constraints and characteristics of specific rearing environments.

Cross-cultural differences in the distribution of children into A, B, and C groups may indicate that the existing categories of Strange Situation behavior are not consequences of correspondence to or deviation from a single pattern of adaptive parenting. Whereas most US samples yield approximately 20% A-, 65% B-, and 15% C-group infants, the North German sample re Grossmann et al. (1981) contained a far higher of "avoidant" children and many fewer secur (50% A, 33% B, 12% C, and 6% unclassified). T collected from several Israeli kibbutzim (Sag press) produced an exceptional number of C b tive to American norms (9% A, 56% B, 35% C). to believe that the B pattern is normative an typical, then these differences, if replicable, the North German and Israeli kibbutzim cu pathological environments for child rearing. Tl sion seems premature, however, in the abse dence regarding the predictive validity of Stra tion classifications in these cultures.

These considerations make us sympath Hinde's views regarding individual differences ment. It is probably more useful to view then tional strategies rather than as variations fro normative pattern. However, the meaning (ferences remains uncertain, and a thorough re of the fitness consequences of individual diff attachment is clearly in order. For example, it assumed that infantile proximity seeking er response to selection by predation (see Guber although this seems likely. Main (1981) ha provide such a reevaluation by attempting to d between proximate and ultimate causes of av havior. However, the suggestion that "avoid appear simply because an extreme of anger (an grows psychologically intolerable for the indiv 681) does considerable violence to the Strange data: Behaviorally, avoidant children are cha by the *absence* of apparent anger or distress. causes for avoidant behavior other than su achieved through the maintenance of proximity been considered.

In any event, a coherent interpretation in evolutionary principles may simply be too muc of Ainsworth's classification system. Remembe A, B, and C groups or patterns were not descrit basis of predictions concerning an adaptive behavior and two possible maladaptive pat Ainsworth et al. (1978) admit, the classificatic grew out of attempts to scan the raw narratives of similarities among pairs or groups of infan groups were originally devised to reflect low, or high degrees of separation distress (Ainswor tig 1969). Only later did the focus shift to reuni ior – "not because of preconceived theoretic tions but because behavior in the reunion contributed the most convincing evidence of behaviors, in contrast to a continuous distribut one or even two major dimensions" (Ainswo 1978, p. 59). Thus the classification system was in origin, rather than theoretically motivated. I based on only 20 minutes of behavior from a sing of 23 infants – hardly an adequate data base for g an exhaustive and "species-typical" set of categc is underscored by recent acknowledgments, proponents of the system (e.g. Main & Weston 1 infants do not always fit into the available c Presumably, an entirely different classificatio might have emerged if it had been derived

intuitive search for clusters using Grossmann et al.'s (1981) North German sample, or Sagi et al.'s (in press) sample of infants from Israeli kibbutzim.

Finally, from the perspective of evolutionary biology, it is also unclear why one would expect to find – and thus search for – groups or clusters rather than a dimension or several dimensions. In nature, variation usually appears continuous rather than categorical, and Ainsworth and her colleagues have not explained why they wanted or expected to find categories or groups (J. P. Connell & Goldsmith 1982). Since users of the classification system agree that both good and poor representatives of any group or subgroup abound, one wonders what information is lost by forcing these into an arbitrarily limited number of groups. At the very least it seems essential to demonstrate that the existing categorical system provides a better means of assessing infants than continuous dimensions would.

Summary. Clearly, theorists may have erred in their claim that the secure (B) pattern of behavior in the Strange Situation can be considered species-appropriate while the insecure (A and C) patterns represent maladaptive or pathological deviations. There is actually no reason to believe that the B pattern is more adaptive (in a lifetime fitness sense) than either of the insecure patterns. Unfortunately, attachment theorists tend to use the terms "adaptive" and "adaptation" in a very loose fashion, confusing the strict biological and looser psychological meanings of the terms. Further, two sorts of biological adaptation – the juvenile and the ontogenetic – are not clearly distinguished. Whereas Bowlby's initial formulation of the attachment process was concerned with juvenile adaptation, more recent discussions have instead portrayed patterns of attachment behavior as ontogenetic adaptations. Another problem is the assumption that there is likely to be only one species-appropriate or "adaptive" pattern of behavior. More plausibly, there should be a flexible array of possibilities, the selection among which depends on the specific environment in which children live and on their inherent characteristics. A related problem is the assumption that there should be a discrete number of possible options – both adaptive and pathological – rather than a continuous range of possibilities. Such a situation would be contrary to what is more common in nature, and it appears indefensible in light of the fact that the categories were empirically derived rather than theoretically generated.

Finally, there is only weak empirical support for the central assumption that the adaptive (secure) pattern develops when the parent has previously behaved in the sensitive species-appropriate fashion. The fact that this central assumption has been found wanting empirically as well as conceptually undermines the entire attempt to "explain" Strange Situation behavior in the light of principles drawn from evolutionary biology.

Conclusion

Strong claims regarding the antecedents, interpretation, temporal stability, consistency, and predictive validity of Strange Situation behavior are only partly supported by the empirical and theoretical literature. The evidence regarding the antecedents of Strange Situation behavior fell farthest short of the popular claims. There is no consistent evidence that variations in parental behavior within the normal range are systematically associated with specific patterns of Strange Situation behavior. Several studies provided tantalizing hints that more socially desirable maternal behavior was associated with secure infant attachment, but the lack of consistency from study to study and from assessment to assessment precludes insight into the specific patterns of maternal behavior that are of formative significance. Further evidence that parental behavior does in some way affect Strange Situation behavior comes from studies showing that changes in family circumstances and caretaking arrangements can produce changes in attachment security. There is also some evidence that extremely deviant patterns of child rearing, such as neglectful and abusive parental behavior, are associated with increases in the probability of insecure attachments. On the other hand, there are also cultural variations in the frequency of "insecure attachments," which suggests that there may be multiple antecedents of Strange Situation behavior. Whether or not this is true, we clearly need more and better research if we are to say anything specific about the antecedents of Strange Situation behavior.

That Strange Situation behavior may be meaningful is suggested by the results of studies focused on the stability and predictive validity of Strange Situation behavior. These studies show that Strange Situation classifications *can* be extremely stable over time. However, when family circumstances and caretaking arrangements change, so too, in many cases, do the patterns of Strange Situation behavior. This finding suggests that (a) Strange Situation behavior is indeed affected by patterns of infant-parent interaction, even if the specific dimensions of importance remain unidentified; (b) the Strange Situation assesses the *current* status of the infant-parent relationship, not some characteristic of interaction during an early sensitive period; and (c) later behavior can only be predicted when there is continuity in the circumstances that maintain specific patterns of child behavior. Predictive validity has only been demonstrated in cases in which such stability could either be assumed or was actually ensured by selective subject recruitment. As a result, the term "predictive validity" is not wholly appropriate; the later behavior may be determined by current patterns of parent–child interaction which, because they are stable over time, are also associated with specific prior patterns of Strange Situation behavior. More detailed interpretation is precluded by the fact that researchers have yet to determine how and which patterns of parent–infant interaction relate to later child characteristics.

Nevertheless, knowledge of Strange Situation behavior and knowledge of continuity or discontinuity in family and caretaking circumstances taken together do allow us to explain some portion of the variance in later child behavior. What we need now are findings that advance our understanding by allowing us to interpret Strange Situation behavior. This demands not only hypothesis-driven studies focused on the antecedents and consequences of Strange Situation behavior, but also a reconceptualization of the Strange Situation. Our review suggests several potentially valuable new directions.

First, we need to explore alternatives to the classification procedure that has hitherto been popular. As explained above, there is no theoretical reason for distinguishing between these patterns, no evidence that the existing groups optimally represent the variance in Strange Situation behavior, and no evidence that any distinctions other than those between securely attached and insecurely attached infants have any validity. Consequently, we are currently attempting to develop a measurement system based on continuous dimensions to summarize individual differences in Strange Situation behavior (Gardner, Lamb & Thompson, in preparation). This represents one of several possible alternatives for scoring and studying individual differences in Strange Situation behavior; only further research on the antecedents and consequences of differences in these dimensions will show whether or not they represent an improvement over the existing classification system. As J. P. Connell and Goldsmith (1982) observed, researchers in this area have adopted an "hourglass" methodology – relating antecedent and consequent events only to Strange Situation classifications. Unless alternative possibilities are entertained and compared, we will never learn how good the existing system is.

Second, we need to reevaluate our reliance on a single 20-minute session which, however carefully designed, can never hope to capture all significant variance in infant behavior. A scoring system that included assessment of infant social behavior in a variety of ecologically and evolutionarily valid contexts would be desirable. Among the contexts one would want to sample are: encounters with strange adults in the presence and absence of attachment figures;[17] reactions to separations from and reunions with attachment figures in the presence and absence of other attachment figures and in the presence and absence of other potential stressors; introductions to novel toys and environments; introductions to unfamiliar animals (such as dogs) in the presence and absence of attachment figures; and behavior in relation to attachment and nonattachment figures in a variety of caretaking contexts such as feeding, play, and soothing. An appraisal of behavior based on observations in diverse contexts is likely to be more reliable and more valid than assessment in any single context.

Third, we need to consider the psychological comparability of the same procedures for different infants. Specifically, some avoidant and B_1 infants seem substantially less stressed by the Strange Situation than most B_3, B_4, C_1, and C_2 infants. Until we know *why* these differences exist, we should refrain from describing some infants as avoidant, others as secure, and others as ambivalent or resistant. Differences may arise from a combination of temperament, previous experiences of separation, and the parents' leave-taking style, among other possibilities, as well as from differences in the quality of infant–parent relationships – the only explanation seriously considered heretofore.

When undertaking this research, we need to discard the notion that one pattern of Strange Situation behavior is evolutionarily adaptive while others are maladaptive. Previous attempts to interpret Strange Situation behavior in light of evolutionary considerations have been marred by a misunderstanding of evolutionary principles and natural selection. Furthermore, although it seems reasonable to view parental behavior as an impo
ence on infant behavior, it is unlikely tha
explanations derived from evolutionary biolog
be sufficient in themselves to explain the
behavior one sees in the Strange Situation.
mechanisms, analyzed through the study of le
tingencies or social cognition, must also be c
Unfortunately, theorists have mistakenly ass
because ethological theory permitted a profo
into normative patterns of behavior, the pr
evolutionary biology considered in isolation s
vide equivalent insight into the origins of
differences in infant development. Evolutiona
however, demands an evaluation not only of
influenced predispositions but also of the co
provided by the specific environments or
which the individuals must manifest these
tions. The first and penultimate sections of
force us to admit substantial ignorance reg
interpretation and evaluation of individual dif
Strange Situation behavior.

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dence and reprint requests should be addressed
author.

NOTES

1. Ainsworth, Bell, and Stayton (1971, p. 24) no
the beginning group C was considered a heteroger
distinguished from the other groups only by what
specified as 'maladaptive behavior.'" Later, ho
worth et al. (1978, p. 58) wrote: "instead of the loos
of 'maladaptive' it was now perceived that Gro
shared, in addition to strong interest in proximity t
with the mother in the reunion episodes, a tendenc
angry resistance to the mother upon reunion."

2. For three infants, behavior in the Strange
missing or was considered atypical (e.g. the child w
observed), and thus relationships between home
Situation behavior are based on only 23 infants

3. The B_4 subgroup was added to the classific
after Bell's (1970) study.

4. Note that in various analyses, Ainsworth
leagues sometimes chose to compare B- with
infants, sometimes B_3 with insecure (A and C) i
times they compared all three groups (A, B, C). S
tency may be acceptable in a hypothesis-generat
which one is trying to maximize the number of si
potentially interesting findings, but it does of cou
on chance and thus leads to overstatements and
izations.

5. In several instances, fuller details about th
measures and the manner in which they were
contained in other reports from the study, rathe
Egeland and Farber report.

6. The total number of scores generated is not
scores mentioned in the results – for example, su
desire for motherhood, and tension/irritability –
in the methods section.

7. In the results section, only 12 scores tapping
teristics were used in comparisons among the
groups; it is not clear which scores were dropp

8. In a previous report involving 100 of these
only the 12-month classifications, Waters, Vaughn

(1980) analyzed data from the Brazelton scale assessments rather differently. They reported that 12 of the 47 items from the first (7-day) Brazelton assessments distinguished B-group infants from C_1 infants (N = 11). Most differences had to do with orientation, motor maturity, and regulation, with the C-group infants being lower in muscle tone, attentiveness, and orientation than the B-group infants. However, the same differences were not evident when the second (10-day) Brazelton scores were related to Strange Situation behavior, since by this time the performance of the C-group infants had improved. Unlike Waters et al., Egeland and Farber (in preparation) used factor scores rather than individual item scores, considered the whole sample rather than a subsample, and combined scores from the two Brazelton assessments instead of treating them separately. Even allowing for these differences, there is a surprising lack of convergence between their results and those earlier reported by Waters et al. (1980). Since Waters et al. were not able to obtain the same results with both Brazelton assessments, and Egeland and Farber reported no reliable relationships between Brazelton scores and Strange Situation behavior, there is no evidence that neonatal assessments (rather than factor scores) are related in any consistent way to later Strange Situation behavior.

9. Only 38 were included in Tolan and Tomasini's analyses.

10. Both Waters (1978) and Vaughn et al. (1979) reported significant stability despite widely varying stability estimates (i.e. 96% vs. 62%). Both used Cohen's Kappa statistic (Cohen 1960; 1968; Fleiss, Cohen & Everitt 1969) to test significance. A more appropriate statistic for this purpose would be lambda (Goodman & Kruskal 1954), which is an index of predictive association. Lambda is designed to reflect the reduction in the probability of error involved in specifying one categorical variable (e.g. 18-month classifications) given knowledge about another categorical variable (e.g. 12-month classifications). In Waters's (1978) sample, knowledge of the 12-month classifications reduced predictive error in the 18-month classifications by 89%. In Vaughn et al.'s (1979) study, however, knowledge of the 12-month classifications improved prediction of 18-month classifications by only 3%. In Thompson et al.'s (1982) study (described below), there was essentially no gain in predictive accuracy attributable to knowledge of the earlier classifications.

11. Farber indicates that these 189 families constituted the entire corpus of infants seen twice in the Strange Situation.

12. The two maternal ratings, compliance, and the negative of ignoring were the major loadings on the first factor, while DQ and the negative of aggression were key loadings on the third factor.

13. Easterbrooks and Lamb (see Lamb 1982b) later attempted to replicate these findings in a sample of 36 month olds. Neither measures of child behavior in the problem-solving context nor measures of peer sociability were related to security of attachment, perhaps because the modified Strange Situation devised for use with 3 year olds (the mother turned off the lights when she left the room) was not appropriate for assessing security of attachment among children of this age.

14. Usually several teachers rated each child. For 72% of the cases, average intercorrelations among the Q-sorts exceeded .50.

15. Egeland and Farber (in preparation) reported that babies classified in the C group at 12 months obtained lower Bayley scores at 9 months than those classified in the B group, but there was no significant B- vs. non–B-group difference and no significant relationship to 18-month Strange Situation classifications.

16. Lamb (1981a; 1981b), for example, has suggested that all three patterns can be seen as behavioral manifestations of the infants' expectations regarding the likely behavior of their parents in the Strange Situation. Main (1981) has described the avoidant pattern as a conditional strategy for maintaining proximity under conditions of maternal rejection.

17. Although stranger reactions could be measured in the Strange Situation, the opportunities are limited. Reactions to strangers also play little role in the Strange Situation classifications.

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Ever since Hippocrates . . .

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Reading recent papers on the Strange Situation may well lead to feelings of *déjà vu* – we have seen these arguments many times before. Questions have been raised that speak to the basic psychometric characteristics of the test – Lamb et al. suggest that the items in the test are too limited and that its reliability and predictive validity are lower than claimed, concerns similar to those raised about the adequacy of intelligence tests. Others have also questioned the content and construct validity of the Strange Situation (e.g. Chess & Thomas 1982a; 1982b; Goldsmith & Campos 1982).

Feelings of *déjà vu* may also arise because of the similarity between the arguments over the Strange Situation and those over trait theories of personality, with Lamb et al.'s article being an infant-oriented version of Mischel's *Personality and Assessment* (1968). Trait and type theories have great antiquity; the humoral theory of the four temperaments is traceable to Hippocrates. But there is a "consistency paradox" (Mischel & Peake 1983), a conflict between personality theorists who insist that consistent types of traits should exist and researchers who have repeatedly failed to find evidence of cross-situational consistency in behavior. Lamb et al.'s analysis of the Strange Situation indicates that both within- and across-situational consistency is lower than claimed and that what consistency is shown is a function of stability in the home situation. Thus, where Ainsworth, Sroufe, Waters, Main, and others see consistency as reflecting a type of attachment bond, Lamb et al. and others see it as reflecting largely stable situations. Of course, none of the parties concerned would claim either that traits show complete cross-situational stability or that situations determine all. The complete situationalist, after all, would have to claim something along the lines of a lifelong tabula rasa. But type or trait theories are essentially main-effects models trying to survive in an increasingly interactional and transactional world.

Some of the problems in demonstrating typological consistency in the Strange Situation are attributable to the nature of the situation itself whereas others apply to type and trait research in general. This commentary considers two rather different issues: first, difficulties in specifying strangeness, and second, some suggestions for revising the approach to and methods of Strange Situation research.

Can "strange" be standardized? Strangeness (novelty) presents unique problems for researchers, whether in terms of a strange person, a strange stimulus, or a strange situation. Strangeness, as has often been pointed out (e.g. Brown 1978), is not a property of stimuli, but of an interaction between stimuli and the past experience of the observer. Depending on that past experience, a given situation may be perceived on a continuum

from being mildly novel, evoking little distress and perhaps approach, to being highly novel, evoking great distress, avoidance, and even disorganized behavior (Hebb 1946). In animal research (e.g. Brown & Hamilton 1977), rearing conditions can be programmed so that a test situation is both operationally and psychologically consistent in terms of strangeness. In research with human infants, however, rearing conditions are uncontrolled, with the result that strangeness may be operationally clear but psychologically fuzzy. Even with humans, strangeness can be manipulated so as to produce either distress or approach (Rheingold & Eckerman 1973). Compounding the problem, the Strange Situation may be highly novel for some *mothers* as well. Thus, for some infants and mothers, the situation may be novel enough to evoke anxiety and disorganized behavior that interfere with the production of normally adaptive behavior.

Strangeness, then, is an idiographic, not a nomothetic, characteristic, and the Strange Situation may present different challenges to different infants and mothers independently of their attachment relationship. For this reason reaction to novelty is one moderator variable (Saunders 1956) that may affect Strange Situation behavior and lead to reduced validity through misclassification.

Future directions. Both the importance of the Bowlby-Ainsworth theory of attachment and the large literature on the Strange Situation suggest that the test will continue to be used regardless of its limitations. The problem is to improve the technique to increase its reliability and validity. Mischel and Peake (1983) describe two different responses to the consistency paradox. One is to develop improved methods and the other is to reconceptualize our view of person–situation relationships.

At least two methodological changes may be used within the traditional Ainsworth A, B, C classification system. One, traditional within experimental psychology and suggested by Epstein (1983) for use in personality research, is to use repeated measures to increase the reliability of measurement. Psychological tests measure, at best, not only traits, but temporary states of the organism as well. Change from one test to another may reflect either low reliability of the test or a change in state. Averaging across repeated testings increases the stability of measurement. Because of the nature of strangeness, the repeated tests might well be in different situations with different strange people. It should be noted that this suggestion is similar to that of Lamb et al.

Second, relationships between the A, B, and C types of attachment and both past and future behavior are relatively weak, particularly the A-C contrast. This could result either from an inappropriate classification system or from problems in applying it. Both the methods used in classifying infants into attachment types and the inability to assign all infants to one of the three categories (Main & Weston 1981) suggest that the categories of attachment types may be fuzzy. Mischel and his coworkers (Cantor & Mischel 1979; Mischel & Peake 1983) suggest that many of our categorical systems are fuzzy – members of one category do not all share characteristics that are mutually exclusive of those held by members of other categories. Categories have a "family resemblance structure, a pattern of overlapping similarities . . . categorical decisions are probabilistic and members of one category will vary in membership (prototypicality)" (Mischel & Peake 1983, p. 240). Classifying borderline cases leads to overlaps between categories and reduces accuracy. Mischel suggests that only the clearest exemplars of a category, the "prototypical members," should be selected for study and that the rest of the cases should be omitted. Thus, if only infants who were the clearest examples of A, B, or C categories were studied, more lawful relationships might be seen. It should be realized that any such relationships will hold for only those prototypes, making the system more nomothetic and less applicable to the normal range of individual variation.

But these methodological changes may not be sufficient to make Strange Situation behavior fully meaningfu tion of the entire system may be needed, as has by J. P. Connell and Goldsmith (1982) and L viously, factors other than attachment affect beh situations, and some form of multivariate, multi transactional approach seems called for. The st ing technique of Connell and Goldsmith (1982) i systems analysis is another (Haith 1982), and been developed by personality researchers (se several examples). It is clear, however, that unle A infants are found to have an excess of black bile beyond Hippocrates in our understanding of th of behavior.

What do we learn from the Strang Situation?

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In their critical review of the Strange Situat Lamb and his associates make a valuable contrib opmental psychology in at least two directions. F procedure has become a widely used method of infant's presumed attachment to the mother. takes only a short time, is easily learned by an and has clear and objective criteria for rating. W been made for the significance of the findings. Fc a careful and detailed critique such as Lamb an have undertaken in order to determine how fa literature actually supports the claims made f Situation procedure, was needed. Second, La consider several general theoretical issues in th view of the psychological literature on attachr These include the question of the evolutionary any, of attachment behavior and the interpretati on findings of stability over time of specific patt ment behavior in individual infants.

With regard to the first point, in my judgme have made a convincing case for their conclusic claims regarding the antecedents, interpretation bility, consistency, and predictive validity of Str behavior are only partly supported by the emp oretical literature." A caveat is thus in order for using this test procedure. They cannot just rely o generated by the previous studies and claims but dently document the significance of their own f for example, a finding that different groups o differences in their Strange Situation ratings ca cally lead to the conclusion that these difference cant and specific differences in prior patterns of interaction. The Strange Situation procedure is n but caution in the interpretation of results is c pending definitive research data.

With regard to the second point, the gene issues, Lamb and his colleagues make a telling looseness with which attachment theorists use biological adaptation. There is no real evidence claim made by Ainsworth and others that the beh in the Strange Situation procedure identified adaptive in a biological evolutionary sense. Lamb cogent criticism of the "assumption that there is l one species-appropriate or 'adaptive' pattern of b plausibly, there should be a flexible array of pc selection among which depends on the specific c which children live and on their inherent charac developmental psychiatrist Robert Emde has r

formulation: "Is it not likely that what is especially adaptive is a *variability and range* of behavior? . . . Indeed the vulnerable infant may be the one who is consistently 'modal' or who otherwise has a narrow range of behavioral variability over time" (1978, p. 136). With regard to the issue of stability over time, Lamb et al.'s review leads them to conclude that "temporal stability in security of attachment is high only when there is stability in family and caretaking circumstances. Likewise, patterns of Strange Situation behavior only have substantial predictive validity in similarly stable families." This judgment is entirely consistent with current interactional-transactional concepts, in which stability of any psychological attribute over time – whether it be attachment behavior, temperament and cognitive patterns, among others – is not to be viewed as a manifestation of a fixed characteristic of the child, but rather as the result of continuity in the child– environment interaction.

A number of additional points of interest are suggested by this review. I can only touch briefly on two that have been of special interest to me. First, there is the lesson to be learned as to the inevitable limitations of behavioral data obtained from highly structured, time limited, psychological laboratory test procedures. Such studies are attractive because they provide data that are easily quantifiable and replicable. But, as McCall has put it, "What value is our knowledge if it is not relevant to real children growing up in real families and in real neighborhoods?" (1977, p. 334). In applying this caution specifically to the Strange Situation procedure, Rutter has warned about drawing conclusions from "curious procedures involving mother, caretakers and strangers not only going in and out of rooms every minute for reasons quite obscure to the child but also not initiating interactions in the way they might usually do" (1981, p. 160). The behavior of any individual child can vary so greatly depending upon the context, and the meaning of that context to that particular child, that it is just not possible to pigeonhole children on the basis of their behavior in one special context over a very limited period.

Finally, I would emphasize the importance of considering the influence of temperamental individuality in evaluating the significance of an infant's behavior in the Strange Situation procedure. I would certainly consider that the temperamental categories of approach versus withdrawal, adaptability, quality of mood, intensity, and activity level will be important aspects of the child's reactions to a stranger and to the mother's leaving and returning (Chess & Thomas 1982a, p. 220).

On a model for assessing the security of infantile attachment: Issues of observer reliability and validity

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Lamb and his colleagues are to be highly commended for their comprehensive and critical review of the Strange Situation paradigm for assessing the adaptive quality of the infant's attachment to the mother. My comments are restricted to a discussion of several fundamental biostatistical issues within the broader context of reliability and validity assessments. While my remarks focus directly upon themes raised in the Lamb et al. review, the biostatistical or methodologic issues raised can also be expected to have relevance within the broader context of behavioral and biomedical research designs.

The objectives of this commentary are to focus specifically upon issues of validity, appropriate interrater reliability statistics, and guidelines for differentiating interreliability estimates that are substantively significant from those that are merely statistically significant.

Validity. At perhaps its most fundamental level, validity in the context of the Lamb et al. review addresses the important question, Does the Strange Situation paradigm really measure what it purports to measure, namely, the adaptive quality of infants' attachment to their mothers? The required research design would call for the careful training of unbiased judges to make the necessary assessments about types and levels of infantile attachment patterns. In such a context, I would agree with Fleiss that in the "absence of a laboratory test that might provide a standard against which to assess the correctness of judgment, one must rely on the degree of agreement between different judges for information about error" (Fleiss 1975, p. 651).

Interrater reliability. Given, then, the necessity for obtaining the very best estimates of interrater reliability of the various measures of the security of infantile attachment, it is rather disappointing that so many of the studies the authors review are so deficient in this respect. Moreover, in the few instances in which reliability estimates are obtained, the statistics are often inappropriate, and there is often no differentiation between what Lamb and his colleagues refer to as statistical and practical levels of significance. One should hasten to add that this basic deficiency is by no means peculiar to the field of infantile attachment behavior but is more the rule than the exception in many (probably most) areas of behavioral science and medical research. It might therefore be instructive rather to review briefly reliability designs that pertain to most studies and the statistics most appropriate to them. Because my commentary must be brief, I merely cite relevant research to make many of my points. The interested reader will have to go to these sources for more specific information, as required.

Taking almost as a given that one wishes to measure rater agreement rather than mere association, that one is also interested in controlling for the amount of agreement expected on the basis of chance alone, one still needs to answer several additional questions about the reliability research design: (1) On what scale of measurement are the reliability assessments being made (i.e. nominal, ordinal, continuous, or mixed)? (2) How many judges will make the independent assessments (here, of infantile attachment behaviors)? (3) Will the same or different sets of judges rate each subject? Once these questions are answered, one is in a position to classify appropriately the type of reliability research design that will be used. As an example, given the constraints and contingencies of one investigator's research objectives he might conceive of the infantile attachment categories as nominal; have three independent ratings performed on each subject; but not always be able to employ the same three raters. As we shall later see, such specific information will allow the investigator to choose both an appropriate statistical approach and appropriate computer programs to undertake the reliability assessments.

Prototypic rater–subject reliability research designs. The recommended statistical approaches can be justified on the basis of the findings of Fleiss (1975) who, in comparing numerous available chance-corrected statistics for assessing levels of interrater reliability, showed that with only minor adjustments, most of the available statistics reduce mathematically to kappa and that kappa (unlike its competitors) has been generalized to cover a wide range of possible rater reliability research designs. One might add that since the Fleiss (1975) publication a number of empirical studies have been conducted both to test the mathematical assumptions underlying various of the kappa-type statistics and to provide minimal sample size requirements (e.g. Cicchetti 1981; Cicchetti & Fleiss 1977; Fleiss & Cicchetti 1978; Fleiss, Nee & Landis 1979). In addition, several investigators have provided guidelines for differentiating between the aforementioned statistical and practical (substantive, or clinical) levels of interrater reliability (e.g. Cicchetti & Sparrow 1981; Fleiss 1981; Landis & Koch 1977).

There are a reasonably small number of basic interrater reliability designs which will, fortunately, satisfy most investiga-

Table 1 (Cicchetti). *A classification of inter- or intraobserver reliability statistics as a function of rater-subject designs and scale of measurement*

Scale of measurement	No. of raters	Same or different raters	Statistic of choice	Authors of computer pr
Nominal	2 only	Same	Kappa (Cohen 1960; Fleiss, Cohen & Everitt 1969)	Cicchetti, Aivano & Vit Cicchetti & Heavens chetti, Lee, Fontana (1978); Heavens & C (1978)
Nominal	≥ 2	Same	Generalizations of kappa (Conger 1980; Davies & Fleiss 1982; Landis & Koch 1977)	Landis, Kemp, Stanish (1978)
Nominal	≥ 2	Different	Generalizations of kappa (Fleiss 1971; Fleiss, Nee & Landis 1979; Landis & Koch 1977)	Cicchetti, Heavens & I (1983)
Ordinal or continuous	2 only ≥ 2	Same	Intraclass r, Model II (Bartko 1966; 1974; Lahey, Downey & Saal 1983; Shrout & Fleiss 1979)	Cicchetti, Aivano & Vi
Ordinal or continuous	2 only ≥ 2	Different	Intraclass r, Model I (Bartko 1966, 1974; Lahey, Downey & Saal 1983; Shrout & Fleiss 1979)	Cicchetti, Aivano & Vi

tors' needs. These are given in Table 1 along with the recommended statistic (the statistic of choice) and the source of the required computer programs. (There are, of course, certain omissions from this table, such as the imaginative kappa-type generalization of Fleiss and Cuzick, 1979, for a rater reliability research design in which the data are classified on nominal-dichotomous scales and the judges do not always make the same number of ratings per subject.)

Assumptions and minimal sample-size requirements. In two recent investigations it has been shown by extensive Monte Carlo or computer simulation studies that the assumptions underlying the mathematical distribution of kappa are valid for sample sizes that vary, depending upon the number of categories of classification. The formula for arriving at these minimal Ns is simply $N_{min} = 2k^2$ in which k refers to the number of categories of classification. In round numbers, the approximate minimal sample sizes required for 2, 3, . . . 10 categories of classification are 10, 20, 25, 30, 50, 75, 125, 160, and 200, respectively (Cicchetti 1976; 1981; Cicchetti & Fleiss 1977).

Statistical versus practical levels of significance of interrater reliability coefficients. The general form of kappa can be defined simply as: kappa = (PO−PC)/(1−PC), in which PO is the observed proportion of interrater agreement, PC the expected proportion, and (1−PC) the maximum difference possible between PO and PC. The index, when divided by its appropriate standard error (e.g. Fleiss & Cicchetti 1978; Fleiss, Cohen & Everitt 1969) can be evaluated for level of statistical significance by direct reference to tables of areas under the normal distribution.

In critiquing the Pastor (1981) study of mother-infant attachment, Lamb and his coauthors note that "interobserver reliability was not specified, other than it was 'significantly better than chance by the Lawlis-Lu chi-square test ($p \leq .01$).'"

A lesser point first: The Lawlis-Lu reliability statistic makes the somewhat unwarranted assumption that "every judgment has the same probability of occurring under the hypothesis that the judges have no understanding of the scale being applied and their ratings are purely random" (Lawlis & Lu 1972, pp. 17–18).

Lamb et al.'s major point is quite well taken. Because interrater reliability statistics simply assess the extent to which levels of chance-corrected interrater agreement are significantly greater than zero (a minimal requirement at best), a number of research investigators have suggested benchmarks or guic terpreting the practical, clinical, or substantive levels for interpreting the meaning of kappa c statistics (e.g. Burdock, Fleiss & Hardesty 1963 Conn 1976; Fleiss 1975; Landis & Koch 1977; Tyre Cicchetti, Cohen & Remington 1979). These gu recently simplified to define: poor agreement agreement as .40–.59; good as .60–.74; and exc 1.00 (Cicchetti & Sparrow 1981; Fleiss 1981).

Stranger in a strange situation: Cor by a comparative psychologist

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One of the fascinating features of the paper by Lan the conclusions they draw from their in-depth a Strange Situation test are similar, and at times conclusions derived from experimental studies o ence with animals. Table 1 shows, in schema procedure typically used in studies involving the tion test. Birth is designated as occurring at T_1. designated as T_2 (often between 12 and 18 m infants are administered the test, which is purp sure of individual differences in attachment presumed cause of these individual differences i the mother–infant relationship up to the time during the interval T_{1-2}). Though Lamb et al. pro that assumption and point out other factors that m as well, it is likely that the majority of the varian mother–infant interaction. On the basis of their the infants are placed into one of three major gr and are then assigned to a subgroup within the n After this, some interval of time, T_{2-3} (typically months), lapses. Finally at T_3 the infants (1) are Strange Situation to determine temporal stability administered one or more criterion tests to predictive validity of the Strange Situation mea

Table 1 (Denenberg). *Schematic layout of Strange Situation experiment*

T_1	T_2	T_3
Birth	Administer Strange Situation test to measure individual differences in attachment behavior	Retest to determine temporal stability and/or administer criterion measures to determine predictive validity

From their extensive survey of the literature Lamb et al. conclude that "temporal stability in security of attachment is high only when there is stability in family and caretaking circumstances. Likewise, patterns of Strange Situation behavior only have substantial predictive validity in similarly stable families." It is evident, therefore, that events transpiring during the interval T_{2-3} critically affect the outcome measures at time T_3. If social and environmental conditions during T_{2-3} are like they were at the time of testing (T_2, which, in turn, reflects the social and environmental conditions between birth and testing), then performance at T_3 is related to performance at T_2. Otherwise the relationship is weak or nonexistent.

Essentially the same conclusions have been reached by those of us who study early experiences in animals. If subjects are given various forms of stimulation starting at T_1, are then reared under standard laboratory conditions starting at T_2, and are then given criterion tests at T_3 (usually adulthood), one typically finds significant effects related back to the experimental manipulations during the T_{1-2} interval (Denenberg 1969). Thus, by keeping the environmental and social conditions constant during T_{2-3}, the individual and group differences generated by our experimental procedures during T_{1-2} have measurable effects at T_3.

Suppose one goes through the same experimental manipulations starting at T_1, but now *varies* the environmental and experiential conditions during T_{2-3}, and uses the same tests at T_3. We have here an analogue to the human situation in which the family conditions do not remain stable during T_{2-3}. Under these conditions one typically finds less clear relationships between the experimental interventions during the T_{1-2} interval and test scores obtained at T_3 (Denenberg 1977; 1982). These findings parallel those summarized by Lamb et al. However, unlike the human studies, we *know* the reason for the lack of relationship: The decrement is attributable to the various *changes* introduced, in a systematic and balanced fashion, during the T_{2-3} interval. That is, we have designed a factorial experiment in which different experiences occur at differing times in development. I have called such studies "programming life histories" (Denenberg 1970).

One of the major findings of that series of studies is that different experiences *interact* over time and yield data that are often a nonlinear resultant of the accumulated set of experiences. Thus, when Lamb et al. conclude that the relationship between T_2 and T_3 only holds when the conditions during T_{2-3} are held stable, this means, logically, that events and conditions occurring during the interval between T_2 and T_3 are interacting (in the statistical sense). Lamb et al. touch upon this point in their section on temporal stability, in which they state that the Strange Situation test, to be maximally useful, has to be sensitive to interactions over time.

The conclusion that experiences during early development are statistically interactive rather than statistically additive has some rather profound consequences for those involved in developmental theory and research. One major conclusion, derived from animal studies, is that the findings of significant interactions over time are not consistent with the hypothesis that the effects of early experiences are irreversible, which is an assumption underlying the critical period hypothesis (Denenberg 1964; 1968; 1982). It follows, therefore, that the effects of early experience are not invariant, since later experiences are able to modify earlier ones (Denenberg 1977; 1982). Lamb et al. arrived at a similar conclusion in their section entitled "Focus of the Review": "early experiences per se may not be crucial determinants, and . . . future attempts to study the effects of early experiences must also consider the occurrence of intervening events which may ameliorate, accentuate, or maintain the 'effects' of early experiences." I am not certain what they mean when they state that early experiences may not be "crucial determinants." If they mean that early experiences have no lasting effects, then they are almost surely wrong. If they mean that early experiences must be viewed in the context of later experiences in a statistical interaction framework, then they are definitely right.

Lamb et al. question the assumption that data from the Strange Situation test support the critical period hypothesis for human infants (see their section on predictive validity). I fully agree. Those who invoke the concept of critical periods in discussions of human development typically do not understand the concept or do not know the research literature investigating this phenomenon (Denenberg 1968; 1982). I am not aware of any study involving human infants that presents convincing evidence for the existence of critical periods for social interaction patterns, including mother–infant interactions.

The finding that experiences interact statistically over time carries with it another major consequence: namely, the correlation coefficient, which is the sine qua non of the human developmental psychologist, is an insensitive and insufficient statistic to use as an analytical tool. The basis for this conclusion is developed elsewhere (Denenberg 1977; 1979) and can be briefly sketched here. The calculation of a zero-order correlation coefficient is equivalent to the test on a main effect in the analysis of variance. In fact, under certain conditions, they can be shown to be algebraically the same. Main effects can only be meaningfully interpreted when interactions are absent or only weakly present. Indeed, it is common to find situations in which main effects are insignificant, yet interactions are highly significant (e.g. when curves cross each other). The finding of an insignificant main effect is equivalent to finding an insignificant correlation coefficient across two time points in development (T_2, T_3). Unfortunately the student of human development usually does not have the additional information needed to carry out the equivalent of a test of interaction. Thus, the lack of a significant correlation may mean (1) there is no relationship, or (2) a relationship is present but events occurring during the T_{2-3} interval have generated interactional effects that mask the underlying relationship. The latter appears to be the case for the Strange Situation test. Lamb et al. point out that there is good temporal stability and predictive validity for the one condition in which there is stability in the family (the equivalent to a simple effect in the analysis of variance), and lower correlations or none when this condition does not obtain (which implies the presence of weak and strong interactions, respectively).

There is one final consequence from the findings of interaction. Since the correlation coefficient is an insufficient statistic to characterize developmental processes over time, it follows that linear cause–effect models of development are also insufficient as a base for theorizing about developmental phenomena. It is necessary to go to a more complex model, and general systems theory has been suggested as such a model (Denenberg 1979; 1982; Thoman, Acebo & Becker 1983). While on this topic, let me mention that I agree with Lamb et al.'s criticisms of the attempt to use principles of evolutionary biology as a theoretical framework to interpret behavior in the Strange Situation. Such principles are applicable to populations, not individuals, and thus cannot be used as explanatory vehicles for individual cases.

Also, this is not properly a theory, since it is not capable of refutation.

Several implications follow from the above. First, the Strange Situation test appears to be sensitive to statistical interactional events and, thus, is a useful instrument for the study of complex social processes during development. Second, it is necessary to obtain information on events occurring during the T_{2-3} interval that can have an interactional impact upon Strange Situation behavior. One way to obtain this information is to follow one or a few subjects intensively, rather than take a large number of subjects and test them on two occasions. There has been a resurgence of interest in single-subject research designs (Denenberg 1979; Kazdin & Tuma 1982; Thoman 1981), and the Strange Situation would appear to lend itself to this form of research strategy. Third, we have to get beyond linearity in our developmental models and construct a theoretical structure that reflects the complexities we find in developmental research. Finally, those who study the human infant have to give up the parochial view that the human is unique, which is their basis for ignoring the animal literature on behavioral development. The animal experiments I cited have been in the literature for the past 15–20 years, yet human developmentalists still make erroneous or misleading statements about the effects of early experiences and the nature of critical periods (Denenberg 1982).

Lamb et al., through their own research as well as this critical review, have contributed significantly to a deeper understanding of the strengths and weaknesses of the Strange Situation test, and its theoretical underpinnings. The target article can also be viewed in the broader perspective of raising significant challenges concerning the assumptions and philosophical basis of much of the research in infant and child development. Such a challenge is timely and, if accepted by researchers in the field, can have salutary consequences.

Correlations in search of a theory: Interpreting the predictive validity of security of attachment

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It is often noted that security of attachment in infancy correlates with toddler and child behavior (e.g. Ainsworth, Blehar, Waters & Wall 1978; Lewis, Feiring, McGuffog & Jaskir 1984). The review by Lamb et al. seems to agree that the Strange Situation classifications – especially the secure versus insecure dichotomy – does correlate with later behaviors. Although alternative methods for assessing security of attachment (J. P. Connell & Goldsmith 1982; Gardner, Lamb & Thompson, in preparation) may come to produce new and perhaps even more discriminating measures of attachment security, it is abundantly clear that Ainsworth's Strange Situation classification method is an important "marker" instrument (Ainsworth 1979c) which detects individual differences that account, at least statistically, for variation in later behavior. Indeed, the measure of attachment security created by Ainsworth and her colleagues is a pioneering effort which has greatly facilitated the investigation of socioemotional development, caregiving, and parent-child interaction.

In considering the interpretation of these correlations, Lamb et al. delineate two hypotheses. The first, attributed to attachment theorists such as Sroufe, emphasizes the direct impact of early attachment security on later behavior. The second, favored by Lamb and his colleagues, proposes that early attachment security correlates with later behavior because patterns of parental care giving which affect early attachment security persist and influence the child's later behavior.

The presentation of these two lines of influence as distinct and apparently competing hypotheses is something of a sin tion not only of the range of theories that *can* be gene explain the correlations of early attachment security w behavior, but also of the explanations that already *ha* proposed. In fact, most attachment researchers hyp that both early security of attachment and later qu caregiving influence later behavior (e.g. Arend, Gove & 1979; Pastor 1981; Sroufe & Waters 1982). For exan though Lamb et al. contend that Matas and her colleagu the simple claim that "securely attached infants becom adapted' toddlers," Matas et al.'s interpretation of the tion between attachment security at 18 months and com at 24 months states that "the issue of continuity of variables is at least as important as continuity of infant v (Matas, Arend & Sroufe 1978, p. 555).

Nonetheless, it is certainly important to inquire relative impact of these hypothesized sources of in Lamb et al. argue that the correlational studies provic support for the continuity of care explanation than for attachment security explanation. Although it is possibl continuity of care hypothesis is the more effective exp the evidence presented by Lamb and his colleagues tc this contention does not appear to justify their ent Rather, the extant correlational investigations appe generally consistent with either theory, but do not co detailed longitudinal data necessary to discriminate co ly between the influence of early attachment security a later parental care giving.

First of all, some studies do not include direct obse measures of later care-giving quality. Although some i tors have assessed the degree of stress and change d time frame of the study, these measures do not r specific behaviors involved in care giving per se. The a such care-giving data prevents the direct investi whether later care giving has an impact upon later bel key element of the continuity of care theory.

In addition, the direct observational measuremen giving quality around the time when early attachment assessed is found in even fewer studies. If quality of directly measured at the time that attachment is asse not possible to distinguish between the effects of giving and early attachment security. It is not appropr security of attachment as a measure of parental ca Although these two variables do correlate, they ar isomorphic, either conceptually or operationally. Fur the lack of observational data on early care giving – e later care giving information is collected – makes it i to evaluate directly the temporal continuity of car another key element of the continuity of care theor

For example, the shifts in attachment and the stresses and changes observed within the time fram the Minnesota studies (Sroufe & Rosenberg 1982 Egeland, Sroufe & Waters 1979) may indicate that care giving have occurred over time. But an altera attachment or in the conditions of everyday life does sarily imply that care giving itself has changed. (Thompson & Lamb 1983c; Thompson, Lamb & E which noted shifts of attachment from 12.5 to 19.5 m collect parental reports about gross changes in care example, maternal employment and substitute care ing this interval. Such data begin to approach t information that is needed in such studies, but the ap of direct observational data on care giving does v discriminatory power of both this study and the study in comparing the early attachment and contin theories.

A further difficulty is that it is necessary to consi the two hypotheses delineated by Lamb et al. but o tial lines of influence as well. For example, it has bee that the Strange Situation classification of attachm

may be, at least in part, a reflection of temperament (Chess & Thomas 1982a; Rothbart & Derryberry 1981). It may also be important to consider the possibility that early attachment security influences later parental care giving, which in turn affects the child's behavior, or that correlations of later care giving with later behavior are spurious.

It seems apparent that early security of attachment, as assessed using Ainsworth's Strange Situation classification scheme, is an effective "marker" variable which can predict behavior later in childhood. The correlational studies have provided some strong hints as to possible explanations of the observed patterns of association. But the leap from these suggestions to a more definitive comparison of alternative explanations seems to be beyond the range of the results that have been reported from correlational studies.

It may be possible that more specific and sophisticated analyses of such correlational data will aid in evaluating alternative theories, but there is a clear need for new studies that provide detailed longitudinal data on care giving, security of attachment, and child behavior at frequent intervals during infancy, toddlerhood, and childhood. It is essential that such investigations be designed explicitly to discriminate among alternative explanations of why early security of attachment correlates with later behavior. At the present time, however, the rigorous consideration of alternative theoretical explanations of the predictive power of Ainsworth's Strange Situation classifications is a new frontier which, to a large degree, has yet to be fully explored.

Asking the right questions

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If Lamb et al.'s critique of the Strange Situation is on target – and it seems to be – then it will probably contribute to curtailing this line of research. For one thing, the suggestions made for its rescue are too cumbersome: continuous variation rather than ABC groups would mean large numbers, problems with computer programs, and the elimination of simple "good versus bad" attachments. Shall we weep, then, for the research being planned and even funded, all perhaps to end in naught? Or shall we weep rather for the science of psychology, where such fads come and go as a matter of course (whatever happened to oral, anal, and genital fixations? stimulated vs. unstimulated infants? or; in older kids, field dependence vs. field independence?)? Psychologists are consumed with explaining individual differences, and unless they also are geneticists, they do not do that by resorting to the concept of continuous variation. Instead they find important consistent inducers of differences so that the panoply of human types can be discontinuously categorized. The field thus alternates between making discontinuities out of continuities and a restitution phase in which continuities are made of discontinuities.

As for Lamb et al., they end their critique by offering improved ways of asking the same or similar questions. I am reminded that Konrad Lorenz once said, in response to critics of his concept of imprinting, that if you want to study imprinting, study it in birds that imprint and not in domestic chicks – and, we can add, study it in their natural environment.

Sluckin (1965), for example, published an entire monograph on laboratory studies of imprinting without once discussing why this phenomenon is interesting or important. Data gathering has become an end in itself, and generative thinking (as opposed to analysis) has been permanently suspended.

There is something similar in the target article. Despite the authors' avowed biological stance, nowhere do we find a discussion of the fear of strangers and strange situations as phylogenetically adaptive phenomena, with obvious homologues reaching far down the phyletic scale. Instead we get some currently standard biological injunctions that to demonstrate biological adaptation one must show reproductive advantage in the adult. If biologists can't do that very well with sexuality itself (e.g. Williams 1975) or with dominance-submission hierarchies in baboons (Hausfater 1973), why ask it of these behaviors? As for questions one *should* ask, why not start with the phenomenon itself?

There is, for example, evidence that tall strangers elicit more fear than short ones, that children in orphanages have a reduced rear response, that an approaching stranger is more fearful than a stationary one, and that identical twins show greater concordance in their fear reactions to strangers than do fraternal twins (Freedman 1974). The theoretical organization of data such as these should take us well beyond where we are now. I certainly agree with Lamb et al. that prediction of later mental health on the basis of early fear response is perhaps the least interesting problem, even if such studies are the easiest to fund.

As for methodology, I have watched the Strange Situation procedure as run by a graduate student trained at Johns Hopkins and was appalled at the bemused coolness of both experimenters and mothers as their children cried their eyes out behind the one-way glass. This adult pathology was far more striking than the experimental procedure, and it was clear that natural protective responses were being held in check by an atmosphere of "scientism." Conclusion? The Ainsworth procedure is probably alright as a dignostic tool in the hands of a good clinician, but if one wants to study the relationship between fear of strangers (or strange situations) and attachment, I'd say, do it in *non*laboratory situations. One may or may not end up with a bar graph and p-values this way, but one will at least be forced to address the right questions.

How to think about the evolution of behavioral development

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Lamb et al. have discussed various difficulties with the evolutionary interpretation of attachment behavior. As they point out, the epistemological problems are by no means unique to the Strange Situation, nor even to developmental psychology. They are fundamental considerations whenever we deal with adaptive significance. Rejecting Bowlby's (1969) basic premise concerning species-level adaptations exemplifies a return to Darwinian principles that began in the 1960s and has led to a new kind of "individualism" in the study of adaptation (see Ghiselin 1974). The notion that the fitness of offspring may conflict with the fitness of parents allows one to explain phenomena ranging from the dimorphism of gametes to aspects of weaning behavior in mammals.

One can view the epistemological problems in either of two ways. On the one hand we can develop some guidelines, such as "Do not confuse proximate factors with ultimate ones." On the other hand we can also develop some investigative strategies, such as "Ask what has happened to ancestral populations in their actual environments." The former are largely cautionary: They help us to avoid certain common mistakes. They tell us how *not* to do research. However, they do not tell us what we should have been doing in the first place. The following discussion addresses this issue.

Rather than asking what something is "for," we need to consider what has happened in its evolutionary history. By shifting to the historical point of view, we avoid the pitfalls of

teleological thinking and focus on the real causes of evolutionary change. Organisms do that which favored their ancestors' reproductive success. A shift from survival to reproductive success in turn allows us to focus upon the one most crucial process that determines what will evolve. It induces us to ask the question of who does the reproducing and how. As what counts is long-term not short-term reproductive success, we are compelled to consider processes that take place over the entire life cycle, and during a series of generations. Since reproductive success is relative to that of conspecifics, the social aspect must take on fundamental significance. Our "environment of adaptedness" is largely composed of other human beings. Even maintenance activities are best envisioned as contributing to reproduction, for survival is anything but an end in itself. As Lamb et al. point out, what to us appears pathological may be perfectly "normal" from a Darwinian point of view if the phenomenon in question somehow, however indirectly or deviously, enhances fitness.

Ontogenetic processes ought to be considered an integral part of evolution. The connection is even closer than is generally appreciated. Evolution results from successive modifications in the manner of development. A series of life cycles, with variations, passes through a series of environments. Selection in those environments determines which variants will preponderate in successive generations. Although reproductive success is contingent upon completing the life cycle, it is equally contingent upon effectual functioning at all stages of that cycle. Therefore the adaptive significance of preadult behavior ought to be considered from several perspectives. Lamb et al. draw the important distinction between ontogenetic and adult adaptations; it may help to expand upon the same theme.

Socially acceptable bigotry leads us to regard childhood as existing "in order to produce adults." Given this assumption, everything a child does should be interpreted as a mechanism for acquiring the wherewithal for functioning in later life. This gives us what Lamb et al. call adult adaptations. That such exist is scarcely worth questioning. What matters is the alternatives, and the underlying assumptions. The most obvious is ontogenetic adaptations, which may be roughly defined as features that maintain the young organism and further its survival and continued growth and development. The placenta allows the mammalian foetus to obtain food, as does the sucking reflex of the neonate. On the other hand, there is nothing strictly necessary about sucking teats as a means of obtaining food. Grownups have a quite different mode of nutrition. It is perfectly possible, and widely maintained, that breast feeding is conducive to normal development and the good health of the infant. Why, apart from empirical evidence, are we likely to believe this? One possibility is the notion that the ancestral pattern of behavior is the "natural" one and not apt to clash with the infant's needs and impulses. An infant innately disposed to suckle will be less frustrated, confused, or whatever. Disturbance of the usual maintenance activity patterns would prevent or restrict normal activity in general.

On the other hand there is also a notion that events occurring at an early stage are necessary conditions for the occurrence of something that will happen at a later stage. Such a developmental event may be called a "morphogenetic adaptation." These are of fundamental significance to embryological theory. Consider the analogy of building a house. The foundation allows one to construct the walls, and once the walls are up it becomes possible to add a roof. The notochord becomes a foundation for the spinal column. Likewise much of child behavior might be the foundation for adult behavior – or perhaps scaffolding to be discarded at the approach of maturity. The notion that normal development should be allowed to take its course has a sound rationale in morphogenetic principles. The whole science of teratology makes sense only when we see how the ordinary developmental patterns are modified into pathological ones. It is common knowledge that birth defects result when chemicals act at some critical stage in embryogenesis. It stands to reason that analogous situations occur in the psychologic ment of the young. But it does not follow that eacl developmental event, morphological or psycholog crucial and necessary role in development. Even in t a certain amount of developmental flexibility and r capacity is built into the organism.

Another aspect of the problem concerns adapti features. Among these are historical vestiges. A de pattern might exist because it was useful to remot but have no current function. There are several r ontogeny sometimes recapitulates phylogeny, or at to. To reach a certain condition, a particular sequen may be expedient or even necessary. One way multicellular organism is by starting with a sing dividing it. This has occurred both phylogenetica togenetically. But ontogenies have deviated by deletions and intercalations. Older authors assun organism simply had to pass through ancestral c order to attain normal development. This represent distinguish between historical vestiges and morph aptations. G. Stanley Hall thought that bullying o children is part of normal development, hence a ne In his autobiography Hall (1923) admits to having siblings when young. Perhaps his views had ex roots. Be this as it may, he was making a very comm The principle of recapitulation, particularly as it wa by Haeckel (1866), does not assert that ontogenetic to be repeated if maturity is to be attained. Emb from ancestral conditions when, for example, yol larval feeding mechanism. To attain a proper synt bryology and evolution one needs a sophisticated un of the underlying processes and the history of Merely superimposing plausible reasons, whether morphogenetic, will not suffice.

All this leaves unanswered the question of what ment behavior really plays in the lives of children relates to the behavior of adults. It may or may not ontogenetic adaptation. It could have several functi it is facultative. The answers can only be provided l research.

Discovery and proof in attachment r

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We understand Lamb et al.'s critical efforts to against the misuse of a single methodological proce measurement of quality of attachment. While it is this view, particularly if one has been unsuccessf quick results by using it experimentally, one must the fact that the Strange Situation was clearly al qualitative differences in the behavior patterns of have had more or less harmonious relationship mothers. It did in fact validate the approximately prior intensive observations in the homes of the cl also Bowlby 1973.] Indeed, Ainsworth's major cont improve our understanding of how maternal sensit utes to harmonious mother–infant relationships. avoided their mothers after two brief separations v pected to the scientific world as infants who were toward their attachment figure. Those infants wh reestablish close bodily proximity were the exp

The qualitative differences of the infants' beha needed an explanation. It was related to a grea variables throughout the first year (Ainsworth, Bl & Wall 1978) and a great number of variables ir years (Sroufe 1983).

In a way Lamb et al.'s target article is a rehash of the old dispute about whether discovery or proof is more important to science. Within the framework of certain biological and clinical expectations, discovery research tries to fit many detailed observations into a developmental picture of emerging social-emotional relationship patterns. There is, on the other hand, proof research. Within the framework of certain methodological and statistical expectations attempts are made to fit many detailed hypotheses into a set of rules and clearly defined variables. Of course, discovery and proof belong together in the steady circle of gaining knowledge and proving that it is correct. But if proof is demanded before discovery is complete, it may do harm. It is harmful when the prepared mind's integrative powers as a means of discovering laws, similarities, causes and purposes (Riedl 1980) are dismissed as invalid as well as unreliable on mere methodological grounds. This is being done by Lamb et al. Variables in discovery research are the best integration of observations in terms of hypothetical realism (Campbell 1966). Nothing could be tested without them (Grossmann 1981).

An attempt to balance out the two aspects of scientific research is missing in Lamb et al.'s contribution. To the degree that the Strange Situation has been used as "the" operationalization of quality of attachment, we believe that it has its merits in finding correlates of the "secure" or "insecure" attachment behavior patterns of infants. But the "hourglass" methodology is strictly the outcome of confusing the method with the thing itself. The Strange Situation has been used as a methodological short-cut to measure the underlying quality of infant–parent attachment. This, however, is not inherent in the attachment concept as conceptualized over the past 30 years in London, Uganda (Ainsworth 1967), and Baltimore (see Ainsworth et al. 1978). Instead, from a European view, it appears to be inherent in a scientific community that presses for fast results. If experiments using the Strange Situation classifications as independent variables do not produce the intended results, the researcher can draw two diametrically opposite conclusions: Lamb et al.'s conclusion is to dismantle the Strange Situation as a valid procedure. The other conclusion, adopted by most other researchers cited by Lamb, is to look for convergent findings to strengthen the bases of the attachment concept.

There is a narrow view of attachment which concentrates on protection and close bodily contact only. And there is a wider view which includes the secure base concept, and the balance between security and exploration, and which "provides an observational orientation as well as an empirical conceptualization which is the actual major breakthrough of Mary Ainsworth's work" (Grossmann, Schwan & Grossmann, in preparation). It is, of course, a tedious task to find out what qualities of togetherness are actually captured by the Strange Situation.

Lamb et al. repeatedly criticize the presumed lack of interobserver reliability of the home observations on which the narrative reports are based. If accepted, this argument may lead to a light-hearted dismissal of the great insights gained from the Baltimore study. In fact, the reports do indeed reflect enormous differences in style of writing that existed between Ainsworth's original four observers. But instead of inferring invalidity from assumed bias among observers, we ourselves reevaluated some of the original Baltimore narratives and came up with an impressively close correspondence, even across languages and cultures! In fact, one of Ainsworth's original observers, Inge Bretherton, agreed as closely with our German narratives – which were quite different again in style, in length, and in quality – as we ourselves agreed with the original Baltimore narratives (Grossmann, Grossmann, Spangler, Suess & Unzner, in press). Regardless of their individual features, the home observations were done before the validation procedure – the Strange Situation – was carried out. Whatever bias the home observers may have had, it could not have influenced the Strange Situation results.

Our more constructive solution, then, was concerned with the content and meaning of the observations, not with methodological prescriptions for their own sake. We were able to replicate independently the main findings of the Baltimore study in northern Germany. We always used two observers, who pooled their notes and memories when preparing the narratives. Who is closer to reality?

The other methodological issues raised by Lamb et al. are to be seen in the same light. There is simply no point in counting how many scales were actually constructed. Any statement that about 5% of them could turn out to be significantly related to any other independent measure by mere chance shows that the author is more concerned about methodology than about a concept. For anyone trying to understand what is going on in terms of "mutual regulation of social behavior" (Bühler 1965, p. 40; see Grossmann 1983), the significance lies in the exhaustive conceptualization of the meaning generated from the interactive flux of behavior (Grossmann et al., in preparation). Hypothesis testing comes next and only next. This is of prime concern for all researchers whose work has all too often been dismissed by Lamb as "difficult to interpret" or "inconclusive." In fact, to us the concept of harmony of mother–infant interaction is of such convincing importance that efforts toward extensive description must continue. It may eventually make the Strange Situation superfluous, once its potential for contributing to an understanding of relationships has been exhausted.

In our view the Baltimore study, of which the Strange Situation was one validating part, is scientific observational research in the best tradition of ethology; it is directed toward the understanding of the complex emotional and intersubjective development of infants. It encouraged the integration of mother–infant behavior patterns believed to be of great importance for the child's future development.

In summary, our own commitment to the attachment concept is to discover infants' and children's patterns of togetherness with their parents and to find out how significant they are for their emotional, social, and motivational development. In our own research, for example, we find that three factors contribute to the infants' Strange Situation behavior pattern: their ability to orient toward objects and the tester as newborns, to maternal sensitivity at two and six months of age, and to cultural demands on self-reliance that start when the infants begin to crawl (Grossmann et al., in press). In due time we hope to learn which of these influences will have a longer lasting effect for the ensuing relationship. Efforts to determine what hampers harmony and what it does to those concerned will continue. As we have already stated;

> the Strange Situation has been a valid test for their (Ainsworth et al. 1978) home observations. On the basis of this validity the Strange Situation as an observation based measure has been profitably used by a number of researchers. It implies, however, a short-cut method or even a narrowing of the original attachment concept. It may well be that by equalizing a wider attachment concept with a narrow operationalization by a single standardized assessment procedure, much of its rich background and potential of the original attachment concept may be lost. (Grossmann et al., in press, p. 34)

The evolution of ethological attachment theory

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The goal set by Lamb et al. was "to evaluate both the empirical evidence and the interpretation of infant behavior in terms of principles derived from evolutionary biology" using Bowlby's (1969) "ethological" theory of infant–parent attachment as an illustrative example. Bowlby's theory has always raised perplex-

ing research issues. Attachments are social phenomena and thus have emergent properties that do not reduce to the traits of individual parents or infants. What then is the unit of selection, and what phenotypic characters have in fact been selected for? Furthermore, attachment formation is a developmental phenomenon, and thus what is to be explained is not a stable condition but rather an epigenetic process, which presents further complications for evolutionary analyses.

The central contribution of Lamb et al.'s target article is that it reminds psychologists who pay lip service to the forces of natural selection that they must determine what evidence is needed to support or refute particular hypotheses. Proximate as well as "ultimate" mechanisms must be sought and the multiple functions of behavior must be acknowledged. The concept of adaptation must be used parsimoniously, only when other explanations can be safely ruled out (Williams 1966). These concerns have been raised repeatedly with respect to the topic of infant–parent attachment (e.g. Cairns 1972; Hay 1980; L. B. Murphy 1964), but they are well worth making once again. Unfortunately, Lamb et al. do not acknowledge that these difficult issues that plague analyses of human attachment are similarly troublesome for biologists who study social behavior in other species (cf. Alexander & Tinkle 1981). Nor do they suggest profitable directions for new theory or research. Rather, the bulk of their essay is a laborious critique of studies that have used Mary Ainsworth's Strange Situation, a procedure that evaluates individual differences in attachment relations from an ontogenetic, not a phylogenetic, perspective.

Lamb et al. link this review to their theoretical concerns by contending that Bowlby's evolutionary viewpoint leads directly to the following propositions: (1) Normative patterns of infant behavior tapped in the Strange Situation are biologically adaptive; (2) those patterns derive from earlier child-rearing techniques; (3) individual differences in attachment relationships are stable over time; and (4) those differences predict subsequent adjustment. Thus attachment theory is viewed as a monolithic entity rather than as a fairly diverse school of thought that has itself evolved over time. This misapprehension may have been fostered by Ainsworth's tendency to deemphasize her own unique contributions to her mentor's theory and by the continued use of the increasingly inappropriate term "ethological" to describe attachment research. Nonetheless, even a cursory review of the history of attachment theory reveals that these four propositions do not follow directly from Bowlby's attempts at evolutionary analysis. Thus the evidence examined does not seem to address Lamb et al.'s overall goal.

Bowlby's (1958; 1969) theory combined psychoanalytic insights and concern for clinical implications of "maternal deprivation" with concepts drawn from cognitive psychology and European ethology. He sought to discuss the psychoanalytic topic of object relations in the light of what was actually known about infants and about behavior evolution. In attempting to describe normal parent–child relations, he shifted to a phylogenetic level of analysis and began to speculate about "the environment of evolutionary adaptedness," "monotropy," and the like. It does a disservice to Bowlby's interdisciplinary scholarship, however, to assert that he naively confused "biologically adaptive" with "psychologically healthy"; the 1969 volume contains extended discussions of the distinctions between phylogeny and ontogeny, biological function, and predictable outcome.

Thus the first proposition identified by Lamb et al. is an oversimplification of Bowlby's thought; the remaining three derive from later contributions to the theory. Attachment does not leave a fossil record, and thus possibilities for critical tests of evolutionary hypotheses are severely limited. Rather, contemporary attachment research draws more on Ainsworth's ideas about personality development than on Bowlby's notions about evolution. The data are compatible with his views, but neither uphold nor challenge any particular biological principles.

Ainsworth's work introduced an important new element into the theory, the concept of attachment security; she has that her interest in security partially derives from the fo tions of her other mentor, the Canadian personality t William Blatz (e.g. Blatz 1966). She devised the Strange tion to assess security operationally, and she correlatec assessments with earlier observations of parent–infant i tion. Her analysis tested for an ontogenetic process, biological function, and thus was not a direct implica Bowlby's perspective.

Much contemporary research, that pertaining to the th fourth propositions about the stability and predictive val attachment, stems not from Bowlby's theory but from lenge posed to attachment research in the mid-1970s. I Masters and Wellman questioned the construct vali attachment, contending that individual differences in ment behavior were not stable over time and that the r indexes were not interrelated. In rebuttal, Sroufe and (1977) argued that discrete behaviors were not stable o second year of life but that the security classifications fr Strange Situation were; furthermore, they maintain these individual differences were coherent rather than over longer periods, security of attachment predictin achievements in other domains. Clearly, Sroufe and W defense of the theory was itself coherent rather than stat respect to Bowlby's original formulations. Indeed, in the recent statement, Waters and Sroufe (1983) seem to be d Mischel's (1968) views of personality, not alternat Bowlby's evolutionary analyses.

In sum, over the 1970s, ethological attachment the evolved gradually into a theory of personality develo Lamb et al.'s attempt to link current research to B ethological perspective seems strained. Rather, the ex critique of the empirical research should be evaluated i of developmental, not evolutionary, hypotheses. At this analysis, the critical tone of the essay makes it seen controversial than it really is. The concerns raised abo cedural details and generalizability of the findings fro studies lead simply to three mild proposals that are fun tally compatible with the goals of Ainsworth and her coll (1) Obtain more process-level information about the a ents of security that can supplement Ainsworth's pio research in Baltimore and the more recent studies i neapolis and Baltimore; (2) document the various fact maintain or disrupt the security of already established ment relationships; and (3) specify alternative develo models that could account for the observed correlati tween attachment and later indexes of social competer Waters, Hay & Richters, in press, for a more detailed co tion of this problem).

In making these recommendations, Lamb and his co are clearly calling for an updated account of the social processes at work in attachment formation and per development, not a new theoretical analysis of the evol social behavior. Thus these recommendations will not b authors any closer to the goal set at the outset. But, the that's not necessarily bad. Lamb et al.'s essay serves to developmental psychologists that we have much to lea infants and parents in our own time, without speculati what occurred on the unobservable, primordial savan

Caveats on the use of evolutionary con

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Authors and their readers may differ in what they consic the most significant features of a manuscript; two p themes argued by Lamb et al. produced such strong res

that, whether these authors agree with me or not, I am compelled to elaborate their argument.

First, on the matter of the relation between a particular set of early experiences and later behavior: The charming tales of geese and cranes that court their keepers (to whom they were imprinted as hatchlings) have beguiled us all. There are probably instances when a particular experience at a particular developmental stage may have a lasting effect. But, at least in mammals with extensive parental care, there is an incredible amount of developmental buffering such that very different circumstances can produce similar outcomes. Given time, Rhesus macaques, severely deprived as infants, may subsequently behave distinguishably from normally reared peers (Novak & Harlow 1975). This is not to argue that early influences are irrelevant: They obviously can affect subsequent behavior. The issue is whether the effects are short-lived or manifested throughout life, whether they are irreversible or readily eclipsed. Imprinting in ducklings, it may be recalled, was once thought to occur only between 12 and 24 hours after hatching and then to fix subsequent adult sexual preferences irreversibly. Subsequent investigation, however, has revealed both greater variability in the supposed critical period for imprinting and constraints independent of early posthatch experience in preferences for particular mates. Students of human behavior are apparently more readily imprinted with simplistic animal models than are their infants. The second point made by Lamb et al., which I believe requires a spotlight and a fervent "amen," concerns the game of discovering adaptations. It is great fun, anyone can play, and everyone wins. Why, in lemurs, do *Lemur catta* mothers use other females as caretakers more readily than do *Lemur fulous* females? Why does one carry its infant parallel to the body axis while the other more commonly carries it across the axis? How many different reasons can you suggest? As Dr. Pangloss would put it, since all things are for the best, each of these characteristics must be adaptive.

Evolutionary biology is an exciting field, and many of the concepts developed by its surveyors have yielded rich dividends. It does not, however, further the field of evolutionary biology or add to our understanding of developmental processes if we simply transpose explanations from one level to the other. The notion of adaptiveness or fitness can be precisely defined: When done by the evolutionary biologist it then becomes a concept inapplicable to the question of whether it is desirable for babies to be of Type A rather than B.

Lamb et al. may think their major contribution was in critically reviewing Ainsworth's conclusions, which they find unsupported. I think their major service lay in their caveats on the introduction of evolutionary concepts into developmental theories.

Infantile attachment: The forest and the trees

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Who can argue against changing the insecure attachment of an infant to a secure one? Yet who can hope to accomplish the change without reliably measuring the normal and the deviant in attachment, or without knowing the factors that may precipitate or frustrate the change? The prime virtue of Lamb et al.'s target article is the message that commitment to socially desirable change must go hand in hand with commitment to scientific understanding. The paper is a thorough and searching documentation that neither measurement nor agents of change have yet been sufficiently refined or understood by researchers of infantile attachment. Unfortunately, the causal analysis of this state of affairs by Lamb et al. and the remedy they offer leave a lot to be desired.

Lamb et al. are certainly right about the necessity of drawing clear lines of relation between theory, experimental procedure, and data. For effective scientific use, a set of experimental procedures must tap real empirical attributes that reflect on an explanatory theory or paradigm. But when it comes to the study of human behavior, the popularity of particular procedures and interpretations is also influenced by additional, quite important yet grossly complicating factors.

The history of the behavioral sciences is replete with trends, movements, and widespread use of experimental procedures that originated in extrascientific conceptions about the way the world is and what it ought to be. The current controversy over Mead's (1928) *Coming of Age* (Freeman 1983) is a good case in point. Still, it seems to us altogether proper and acceptable that scientists who approach human behavior from the perspectives of clinical intervention, as Ainsworth and Bowlby certainly and most competently do, are motivated by the need to understand normal behavior and the causes of deviations from it. Lamb et al. fail to appreciate the unique features and potentialities of this perspective.

Bowlby's and Ainsworth's ideas and procedures deserve the deepest respect precisely because of their concern with empirical indices of the normal and the deviant in early human development. It is almost needless to stress that such indices are practically nonexistent outside the concerted research effort that grew out of the work of Bowlby and Ainsworth, and that this is so despite the fact that early development is the key word in much of current psychiatric theory and practice. Bowlby's and Ainsworth's efforts at bridging the gap between ethology and psychiatry have been most productive and are highly commendable. Their fundamental assumption of mutual attachment between mothers (or caretakers) and infants, and their belief that deviant manifestations of early attachment must have antecedent causes and should be relatable to subsequent behavioral difficulties are no less promising today than they were 15 years ago. None of these has been negated by new data. Rather, the new findings reviewed by Lamb et al. point to some serious methodological shortcomings and suggest that testing the broad assumptions of early attachment will require reduction to specific, more narrowly focused postulates and paradigms. As regards the analysis of Ainsworth's work by Lamb et al. in terms of even broader concepts of "adaptation," this seems to us altogether superfluous and irrelevant.

It is altogether unclear to us just what inclusive fitness or cost–benefit analysis (both ultimately needing reduction to flow of genes and variations of gene frequencies in particular populations) would teach us about the ontogeny of secure and insecure attachment, or what the related processes and procedures might have to do with deviant early attachment as the source of subsequent behavioral problems.

If we accept that attachment behaviors have evolved, as we must, we should also accept that the phylogenetic and genetic histories of species and organisms have something to do with the ontogenetic variations of their early attachments. From these perspectives, Ainsworth is entirely right in stating that to the extent the rearing environment departs from that to which human babies are preadapted, behavior anomalies should be expected. This is indeed what the data about deviant attachment resulting from serious neglect or abuse of infants by caretakers indicate. The task now is to define what variations of early environment are relatable to what variations of attachment and what genetic influences are implicit in the presumed evolutionary preadaptation of attachment. It is doubtful that kin selection and genetic cost–benefit analysis can help us much in this task.

The mechansims of attachment are immensely redundant even in simple species. They also always involve reciprocal actions and interactions between mothers and the young. Optimal human mothering is a matter of very variable combinations of highly redundant behavioral mechanisms in which the episodic and developmental influences of external environment

must play as much of a role as do constitutional and prior developmental variations among particular infants and mothers. The discussion by Lamb et al. of this complex process of interaction in terms of kin selection or reproductive cost and benefit borders on the silly. This part of their paper is but the creation of a straw man who is then irreverently and irrelevantly slain by the sentence, "for all we know, psychopaths may leave as many (or more) offspring as 'flexible' well socialized individuals." Why raise the issue in the first place? Surely it is not what Ainsworth and Bowlby have in mind when they talk about evolutionary preadaptedness or when they try to consider the processes of attachment from perspectives of evolutionary biology.

The relevance of evolutionary biology to early attachment in humans seems to boil down to the following questions: Do the processes of attachment, which ethologists find at every twist and turn of neonatal behavior at grossly different levels of evolution, play a role in human development? And do the genotypic variations among infants and mothers contribute to phenotypic variations of their attachments? Lamb et al. skillfully document the fact that we do not as yet have sensitive enough measures of the fine variations of attachment by which we might try to answer such questions. The problem, however, may not be solved by increasing the categories of secure and insecure attachment, or by trying to distribute attachment as a continuous variable. Rather, the concept of secure and insecure attachment may simply be too broad for effective experimental analysis. It is a forest that obscures the trees. At this juncture we may need to pay attention to the trees, to the variations of those particular and narrowly definable factors that promote proximity and indicate attachment. The most immediate and obvious among them is the infant's reflex and social smile and the mother's response to it.

The reflex smile of infants and its development into the social smile during the first few months of life has been equated with approach tendencies, contentment behaviors, and imprinting in the neonates of lower organisms. The infant's smile is a signal that unfailingly elicits a variety of maternal responses, such as touching, vocalizing, looking, and, of course, smiling back. This behavior has been examined from perspectives of communicative and evolutionary significance (Eibl-Eibesfeldt 1970; Freedman 1974). It offers an excellent model for considering the joint evolutionary–biological and developmental–psychological components of early human attachment.

The smile is species typical. It occurs in all human cultures and in nearly all humans despite serious biological deficits such as blindness, deafness, or mental retardation (Darwin 1872; Freedman 1964; 1974; Spitz 1945; Spitz & Wolf 1946; Thompson 1941). It can be elicited through each sensory modality, which speaks of great genetic redundancy and implies an important adaptive function. The early reflex smile, which can be observed hours or even minutes after birth, undergoes considerable change during development. From the early involvement of the muscles around the mouth, it gradually expands to employ muscles of the cheeks, around the eyes, and of the entire face. Some authors believe that the reflex smile and the later social smile have nothing to do with each other (Ambrose 1960; Dargasies 1962). Others (Hayashi 1972; Wolff 1963; 1966) assume a functional continuity between the early reflex and the later social smile. The fact remains, however, that by about three weeks of age infants exhibit a smile in response to social stimuli, particularly to the visual patterns of the human face (Kaila 1932; L'Aillier 1961; Spitz & Wolf 1946). This response involves the same facial movements as those used in the reflex smile. Mothers invariably recognize this behavior as smiling and respond accordingly, usually by a smile. If there is such a thing as infantile attachment in humans, the signaling characteristics and communicative processes of this exchange of smiles between infant and mother must be part of it.

A recent study (Kovach & Kovach 1983a; 1983b) was designed to test the reciprocity of maternal behaviors and infant Results indicated significant correlations between freq of early reflex and later social smiles, but only in highly situations – only following feeding when the infant was in the mother's arms. Social smiles by infants at the expe er were observed as early as 8 to 10 days of age in 5 o infants. In general, there were considerable episodic an opmental variations among babies. The infant smile co with the maternal smile during and after feeding. Yet, deal of infant social smiling seemed spontaneous and u to maternal manipulations or responses. However, w infants smiled, mothers tended to smile back, indicat infants initiate social interaction with a smile from the stages of postnatal life.

Measurement of maternal attitudes and their relatio infant social smiling indicated the following: Mothers w scores on most factor scales of maternal attitude had bab exhibited a broad range of social smile frequencies, f through medium to high. Mothers who scored low on factors tended to have babies who smiled significant Apparently, well-adapted mothers provided the infants environment that allowed the expression of the full individual variations in social smiling. The less well mothers, if the attitude scale was indeed measuring n adaptiveness, seemed to restrict or depress parts of the individuality by inhibiting or hindering the develop frequent smiles.

In general, these data suggested that even such a behavior as smiling is highly complex and redundantl ated. Yet, focusing on such a behavior may help us ir standing the joint constitutional and environmental so individual variation in early behavioral development t been so effectively studied by ethologists in lower or Bowlby and Ainsworth have made a significant start in the gap between ethology and developmental psycholog tifying the causes and consequences of "healthy" and early attachment in humans, which has been the central along in Bowlby's and Ainsworth's research and theoriz continue to need the exchange of ideas and data ethology and developmental psychology. The time al ripe for shifting the focus from global constructs of attachment to the sources and consequences of individ tion among isolated factors and well-circumscribed pro the complex, highly redundant, and reciprocating me of human mothering and infantile attachment.

Reification and "statification" in attach theory and research

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Truth. Beauty. Motherhood. Apple pie. Some conc jects, or characteristics are either above criticism or difficult to deal with from a critical perspective becaus somehow imbued with a highly affective quality. At theory, and, by extension, procedures for the asses attachment relations, have this characteristic. One con is that there are far fewer critical reviews than are prob for the development of valid or at least heuristic psy theory (e.g. Masters & Wellman 1974; Rajecki, Lan mascher 1978). This point has been made elsewh specific reference to attachment (Masters 1978), but it resurrection in light of the current review by La because it may serve to highlight contributions that criticisms. In the present instance, Lamb et al. have close look at and a critical review of the concept of the s

attachment and the theoretical perspective in which it is most often viewed.

One point addressed by Lamb et al. merits some amplification. Infants' reactions to the comings and goings of strangers and care givers in a 20-minute behavioral assessment paradigm cannot capture the richness of behavior and emotion that is inherent in the concept of human attachment. This is so clearly true that it verges on being a platitude. Attachment theory, from either an evolutionary or social learning point of view, has nevertheless failed to generate many alternative or complementary hypotheses regarding the inherent processes or behavioral indices of the formation or maintenance of social attachments (see Cairns 1979 for a notable exception). Attention is only now being brought to bear on factors influencing the course of attachment, as noted above. The theoretical as well as empirical elucidation of mediating factors for attachment behavior should augment the range and number of contexts and behaviors, at various ages, that should be studied to broaden the spectrum of analysis and increase our understanding of attachment phenomena in infants, children, and adults.

Let me now comment on an aspect of the concept of attachment that Lamb et al. did not explicitly deal with, one that reflects on my call (above) for enrichment of the attachment concept. Many years ago, Clark Hull (1943) warned of the dangers inherent in the reification of a concept. Reification is the substantiation, conceptually, of a theoretical concept into a "thing" that "has" properties, "allows" certain behaviors to occur, or perhaps even "causes" them. I believe that the concept of human infant attachment has become reified to a degree that not only makes criticism difficult and infrequent but also prevents valid and important reconceptualizations in the light of emerging data or theory.

One potential consequence of reification is that reasoning about a theoretical construct can go somewhat awry without a writer, reader, or practitioner becoming aware of it. Consider the following example of deductive reasoning that might occur during the rating of an infant's Strange Situation behavior: "If infants seek some sort of contact, proximal or distal, with their care givers during reunion, they are securely attached. This infant sought contact during reunion; therefore the infant is securely attached." Following the lead of Johnson (1954) and Bechtold (1959), consider now a similar syllogism: "If 'Old Dog Tray' was run through a large and powerful sausage-grinder, he is dead; he is dead, therefore, he was sausaged" (Johnson 1954, p. 723, quoted in Bechtold 1959). Reification, in general, makes this sort of reasoning easier when using theoretically determined behavioral indices of a construct. At the very least, the example also indicates how the elements of scoring the Strange Situation behavior of infants may not recognize the possibility of factors other than an internal trait (state?) of secure, anxious, or avoidant attachment as determinants of the infant's behavior.

Related to the sorts of reasoning problems that may stem from reification of a construct is the sort of embellishment a construct may enjoy when it is thought about as a "thing." Consider quotes provided by Lamb et al. from major writers in the area of attachment. Ainsworth and Wittig (1969, p. 112) wrote, "One of the most important criteria of a healthy attachment was the ability to use the mother as a secure base for exploration," and Sroufe has written, "The nature of the earlier behavioral organization, with attachment promoting exploration, makes the smooth movement to more autonomous functioning virtually inevitable" (Sroufe, in press, ms, p. 4). In both of these quotes attachment seems clearly to be considered as if it were an entity that is almost tangible: One can have *a* healthy attachment, and attachment can itself *promote* exploratory behavior. In writing about psychodynamic constructs, MacCorquodale and Meehl (1948) have illustrated how reified nomenclature can influence thinking and the development of theory:

> A concept like libido or sensor or superego may be introduced initially . . . as a merely conventional designation for a class of observable properties or occurrences. But somewhere in the course of theoretical discussion, we find that these words are being used as hypothetical constructs instead. We find that the libido has acquired certain hydraulic properties. . . . What began as a name for an intervening variable is finally a name for a "something" which has a host of causal properties. These properties are not made explicit initially, but it is clear that the concept is to be used in an explanatory way which requires that the properties exist. (MacCorquodale & Meehl, 1948, p. 105).

In the present context, consider how *an* attachment may *be* secure, anxious, avoidant, or healthy, can *promote* exploration and so forth.

Finally, I would like to propose another consequence of reification that has implications for the study of attachment behavior. This is what could be termed the "statification" of a concept, a tendency to assume both a construct and its behavioral manifestations will be reliable and stable across time, at least once initial development is past. This is likely to be particularly true of constructs for which there are deemed to be significant individual differences, as in the security of a given infant's attachment to a care giver. In the present instance, once one has developed *an* attachment that falls into one class of security, the rather automatic assumption is that it will be stable across time, predictive of other categories of behavior (in both the near and distant future), and related to a finite set of interesting and theoretically relevant antecedents.

One problem with the statification of a construct is that it may allow for social learning antecedents, but once stasis is assumed there is little consideration of possible social learning *concomitants*, elements of current contexts or ongoing experience that may modify behavior, influence its correlates, or even be responsible for the continuity that is unwittingly attributed to the static nature of an internal construct. The conclusion by Lamb et al. that continuity in the security of attachment is influenced by stability in the family and other caretaking influences has significant implications for the overall construct of attachment: If continuity is influenced by the stability of contexts then concepts such as the security of attachment (and perhaps of attachment "bonds" in general) should not be considered solely as the static consequence of some evolutionary or social learning antecedents but also as a mutable, flexible disposition whose nature and associated behaviors continue to be responsive to social learning. This opens up a wealth of new and interesting questions having to do with the *course* of attachment-relevant emotion, cognition, and behavior. This course includes the development, maintenance, and even the dissolution of attachment relations at all points in the life cycle (Duck 1982; Gottman & Levenson, in press).

Security of infantile attachment: The person–situation debate revisited

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As a major pioneer in the field of infantile attachment, Ainsworth provided a paradigm for measuring a complex phenomenon, accomplished extensive research that was procedurally difficult (by being longitudinal, etc.), and offered theoretical concepts that have obviously had a major scientific impact. Lamb et al. have now provided a much needed critical review of current procedures, data, and interpretations as well as thoughtful suggestions for improvements. Many of the issues raised by Lamb et al. (predictive validity, cross-situational behavioral consistency, subjective observer attributions, temporal stability in individual characteristics vs. stability in environmental conditions, etc.) are reminiscent of those generated

by Mischel's (1968) attack on trait theory in the field of personality. Some of the insights gained in that earlier person versus situation controversy may be helpful in assessing the current arguments on infantile attachment.

Many of our comments seek primarily to reemphasize points that Lamb et al. do not dwell upon. First, as Cronbach and Meehl (1955) have pointed out, negative evidence in the search for construct validity can mean: (a) that the measures used are unreliable; (b) that the experimental design failed to test the theory; or (c) that the theory is incorrect. Lamb et al. convincingly argue that many of the studies used to support Ainsworth's ideas are replete with methodological and statistical problems; ironically, the more the data suffer from measurement and design problems, the less they can be used to discredit a theoretical position. Certainly the methodological problems must be addressed and corrected, but there is little reason at this point to abandon wholly the conceptual underpinnings of Ainsworth's work.

Second, any attempt to identify specific parent–infant behavioral patterns that relate to later characteristics of the child should consider infant temperament. Thomas, Chess, and Birch (1968) have concluded that temperament interacts with the environment to determine specific behavioral patterns that emerge in the course of development. Thus a failure to consider fully such organismic differences can only cloud the understanding of relationships between parental behavior and infantile attachment.

Third, sex differences need to be more fully explored. As Lamb et al. note, such differences were unpredicted in previous research. However, rather than constituting negative evidence for any theoretical position, these differences demand modifications while promising further insights into attachment processes.

Regarding the issues of behavioral stability and predictive validity, it should be remembered that the same behavior may have different meanings at different ages (and for the two sexes), and that what appear to be very different behaviors can nevertheless be functionally equivalent. The baby who cries when frustrated at 3 months may be exhibiting assertive, active, independent behavior; the same response when the child is frustrated at 18 months may represent just the opposite. Researchers might do better to look for such things as "coping behaviors" (attempts to change or restructure the situation) in order to maintain consistency in classification of the child.

The fact that behavioral changes occur is really irrelevant to whether early experiences are formative and predictive for later behavior. Instead, the issue is whether different patterns, levels, and directions of change occur for infants who differ in early attachment relationships. As Thomas and Chess (1980) have pointed out, developmental research should emphasize "the identification of patterns of change and continuity, as they coexist, as they interact dynamically at the same age-period and sequentially, [and] as their manifestations are similar or different between individuals and groups" (p. 134).

Finally, we take minor issue with Lamb et al.'s argument that "there is no reason to believe that the B pattern is necessarily 'more adaptive' than either the A or C pattern." It is difficult to imagine specific circumstances in which the avoidant and resistant patterns "represent appropriate adjustments designed to maximize the infants' chances of living to reproductive maturity," and unfortunately the authors themselves do not suggest any such circumstances. Experimental evidence regarding evolutionary adaptiveness is always rare, of course, but the correlation between insecure attachment and neglectful or abusive parental behavior is pertinent here. Abusive parental behavior may not cause insecurity in infant attachment; rather avoidant and resistant infants may be more likely to generate – or possibly fail to inhibit – parental abuse (a similar relationship may exist between the temperamentally "difficult" infant and parental abuse). If so, then adaptive advantage most certa lies with secure attachments.

Learning in the context of evolutionary biology: In search of synthesis

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The relationship between theory and research. Disciplined piricism requires the use of theory, however informal or pre inary, for only a theory can point to the relationships an events that are important and that can provide guideline identifying and controlling irrelevant factors (i.e. artifacts). constructive vein, empirical findings are routinely referred to the theory for its evaluation and modification. Lamb et a not appear to be adherents of the attachment approac Ainsworth and her associates. In the above frame Lamb c survey claims based on empirical evidence generated by Strange Situation, a procedure devised by Ainsworth anc workers to assess the security of infant-to-parent attachment to predict diverse social-developmental outcomes as well evaluate the related role of the evolutionary hypothesis these timely appraisals, Lamb et al. are generally on ground, documenting conclusions that the popular claims a the security of infant attachment as assessed in the Str Situation are, for the most part, empirically unsupported that the proponents of the Strange Situation methodology misunderstood and misapplied the relevant evolutionary pr ples. Some of the new directions proposed invite compa with the theoretical and empirical approaches of the pre commentators.

Critique of procedure and methodology. The Strange Situ procedure was developed to generate select infant–paren teraction data in a standard way. In a context in which ther diverse child-behavior indicators of attachment and situatio which those attachment indicators may be generated tha reasonable under variants of attachment theory (Gewirtz 1 the Strange Situation has been emphasized by Ainsworth her associates to assess the quality of infant attachment parent and as the main basis for claims about later s development. Ainsworth extended Bowlby's (1958, 1969) t ry and brought attachment issues under experimental scr in the laboratory. Pioneering efforts such as the one undert by Ainsworth and her colleagues are vulnerable to retrospe methodological and procedural scrutiny and criticism. Lar al. do not always appear to have taken this contextual poin account in their criticism. They challenge the evidenc support of the Ainsworth position on methodological gro or, failing to identify methodological shortcomings, the vance alternative hypotheses to account for results. Users c Strange Situation procedure may be better suited to respo these criticisms in a historical context. Even so, it seems that the Lamb et al. critique would have benefited by cons ing in more adequate detail the historical-contextual fact which Ainsworth developed the Strange Situation proce the merits of that procedure compared to others, anc heuristic value of Ainsworth's contributions, as well a zeitgeist.

Proximate developmental processes and the role of lea In our view, the Lamb et al. analyses would have been more compelling had they considered adequately the r early social learning. In the conclusion, Lamb et al. appear sufficiently concerned on this point to suggest that "le contingencies or social cognition, must also be conside

Further, the authors' case that behavioral stability in time exists only where there is stability in life circumstances is exactly what an operant-learning process would predict. In contrast to the "process gap" in some early ethological approaches, we have developed an instrumental-conditioning model for the simultaneous acquisition of behavioral attachment of parent to infant and infant to parent (e.g. Gewirtz 1961; 1969). A series of paradigmatic experiments on these reciprocal-influence processes has been reported (Gewirtz & Boyd 1977). From a comparative perspective, other analyses compatible with this approach and with an emphasis on learning mechanisms underlying infant attachment have been offered (Hoffman & DePaulo 1977; Hoffman & Ratner 1973). More recently we have examined the role of early social and attachment learning processes in the frame of organic and cultural evolution, and have elucidated the concurrent influences of proximate and ultimate mechanisms in behavioral development (Gewirtz & Petrovich 1982; Petrovich 1978a). Moving closer to the point, the variegated patterns of responding in a Strange Situation that are reviewed by Lamb et al. can be explained efficiently by attending to how such outcomes can result from experiences denoting maturationally prepared constrained learning.

Ultimate developmental processes and modern evolutionary theory. The Darwinian view of creation characteristic of early ethology and embedded in speculative formulations about adaptation such as those "for the good of the species" has given way to Neodarwinian syntheses and a related emphasis on evolutionarily stable strategies and conditional probabilities in development that are derived from population genetics and quantitative evolutionary biology (e.g. Petrovich 1978b). Lamb et al. raise the correct questions about the use by Ainsworth and her associates of the term "evolution" and "adaptive," but they stop short of sketching out the new synthesis required. Admittedly, "adaptive" is a troublesome term, incurring problems of teleology in its use, if ecological contingencies of survival value are not specified. Thus, ethologists have been careful to distinguish between "teleology" under which the purposes of behavior are only *assumed* and "teleonomy" under which the contingencies manifesting the survival value of behavior are *demonstrated*. In our consideration elsewhere of these and related issues (Gewirtz & Petrovich 1982), we have included the relevant comparative literature and the concepts of inclusive fitness, kin selection, and parental investment (e.g. Dawkins & Carlisle 1976; Hamilton 1964; Lumsden & Wilson 1981; Trivers 1974). Consistent with the modern view of behavioral development, various modes of adaptation may be outcomes as much of specific experiences rooted in learning and tradition as of biological, genetically programmed processes. A response may result as readily from interaction between social environment and an infant's capacity to learn, given systematic environmental consequences of behavior shaping and supporting the behavior complex (proximate causation), as it may result from naturally selected genetic changes over time in the frequency of appropriate genes in the species gene pool (ultimate causation). It is of course also possible that conditional responses may result from the coaction of these processes. Even so, analyses of the processes in given ecological settings are required.

Some metatheoretical observations. The Lamb et al. review of the data generated by the Strange Situation test has shown the fruitfulness of the theoretical approach to attachment advanced by Ainsworth and her associates. Fecundity is surely one positive indicator of a theory's utility. At the same time, however one reads their claim, Lamb et al. have (1) questioned that the empirical evidence generated by the Strange Situation supports strong forms of the popular claims of Ainsworth and her associates and (2) asserted that interpretations of those data in terms of biological adaptation have been oversimplified. These arguments and conclusions may illustrate directly some problem patterns that Lamb et al. indicate have plagued developmental psychology. Another problem pattern is only implied in the Lamb et al. analyses. As often happens when a particular theoretical or methodological orientation gains ascendancy in science, its adherents may develop a zeal that extends beyond the bounds dictated by empirical evidence. Theoretical speculation may become reified, and the orientation may take on cultlike qualities of orthodoxy. Research studies or alternative views may then be impeached or rejected out of hand for not conforming to the framework of, or indoctrination required by, the system. Psychoanalysis comes to mind as an example. It has seemed that the zealously held attachment paradigm generating Strange Situation data has often been at risk for this type of exclusionary orthodoxy. Such a stance would surely impede progress toward understanding attachment processes in general and Strange Situation behavior in particular. It is hoped the many-faceted Lamb et al. critique will reverse rather than reinforce such exclusionary tendencies. Even so, such considerations and their impact on developmental theory and method may best be left to those focusing on the history or sociology of psychological science.

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On inferring evolutionary adaptation

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It should be emphasized that although Lamb et al. challenge earlier conceptions concerning the biological or sociobiological (evolutionary) origins of contemporary patterns of attachment behavior in human infants, it is evident that they still embrace a position that presumes that Darwinian natural selection was involved. For instance, although the authors caution that one cannot simply assume that certain infantile responses emerged as a consequence of selection by predation, to them "this seems likely." Their own evolutionary perspective may thus appear to be an improved version, but this sort of approach may yet contain fundamental flaws. This possibility was suggested to me by Colwell and King (1983) in their explication of the practice of *inferring* genetic mechanisms as the basis for human behavioral traits.

According to the Colwell-King analysis, three steps are necessary in any such attempt at inference, all in order to avoid the serious error of *mis*attributing a trait in question to evolutionary adaptation:

1. Identify a behavioral pattern in an animal species;
2. Make an experimental or rhetorical case that individuals who possess this behavior have (or would have) a reproductive or survival edge over those who do not;
3. Show that the behavior in question is transmitted genetically from one generation to the next.

In the words of Colwell and King (1983, p. 231):

> Reproductive advantage of one phenotype over another is a necessary condition for natural selection, but is never a sufficient condition. Genetic transmission of phenotypic differences is an equally necessary condition for natural selection (and likewise not a sufficient one, because phenotypes may not differ in fitness). Both reproductive advantage and genetic transmission are necessary for natural selection to act, and together they are sufficient.

Now, in terms of human infant attachment, steps 1 and 2 have been carried out. The familiar A-B-C categories have been documented in a number of human societies (step 1), and the review at hand convincingly makes the case that some or all of these styles convey an advantage to the human child (step 2). But having got this far it becomes tempting – as Colwell and King point out – to *quickly* infer that natural selection from alternative genotypes *must* have been responsible for the prevalence or persistence of the behavior. This temptation can be said to lead to a trap, and it is a trap into which many theorists in the area of infantile attachment seem to have willingly fallen. As we have seen, without the completion of step 3 (above), one cannot *indisputably* claim that genetic selection is at the heart of patterns of human infant attachment, and this commentator knows of no data that directly bear on step 3.

Why, in the absence of the criterion called for in step 3, might the inference of genetic mechanisms be fallacious? Just because there are alternative explanations for the transmission of any behavior, the principal one being cultural transmission. The possibility that patterns of attachment might be culturally transmitted becomes clearer when the idea of "lineage-following traits" is considered. Traits that are transmitted genetically obviously follow lineages; those that are culturally transmitted need not be lineage followers, but apparently often are. A speaker's language is a lineage-following, culturally transmitted trait since most children learn to speak from their parents or other close kin. Colwell and King go on to list various behavioral and psychological traits that are culturally transmitted yet follow lineages, including diet, religious practices, sexual practices, sex-typed behavior, and even the more personal characteristics of temperament, perceptual and psychomotor skills, memory, extraversion–introversion, and others.

Indeed, some of these lineage-following, culturally transmitted traits (such as wealth) can influence reproductive success among lineages. In other cases, lineages may differ on some trait, and coincidentally differ in reproductive success. Given these circumstances, *cultural* transmission of a trait can actually lead to *genetic* evolution, but *not* via Darwinian natural selection. As Colwell and King (1983, p. 235) put it: "In this way, a culturally based trait responsible for conferring greater reproduction on a lineage increases in relative frequency in the population and *drags behind it completely unrelated changes in gene frequency*" (emphasis added). Thus, if some lineage in a population has a higher reproductive output this could result in a change in population-wide gene frequency (genetic evolution), to the extent that the trait could become "universal" in population, all without natural selection entering the picture.

In short, "cultural transmission may account fully for variations in human behaviors, even those that have reproductive and evolutionary consequences" (Colwell & King 1983, p. 245). Further, regarding the behavior of offspring of possessors of such a trait; "Even if descendants or collateral relations also bear the trait, although it is rare or absent in other lineages in the same population, and even if bonafide genetic differences exist among lineages, the genetic differences need not be the basis of the behavior" (p. 235).

As noted, there are doubtless no hard data on the behavior genetics of human infant attachment; nor are there pedigrees available – if for no other reason than that most of the first generation of subjects in this literature have not yet reached an age at which they can be expected to reproduce. Therefore, it is, and probably will be for a long time, impossible to satisfy step 3 in the inferential process outlined above. Developmentalists should be counseled to restrain themselves with respect to the "adaptive significance" of infantile attachment. Firm conclusions about natural selection and genetic transmission will have to wait.

Is it any more plausible to suggest that patterns of attachment are really transmitted culturally? I do not know. There might be some learning theorists who would be willing to speak to that point. But perhaps the traits (patterns) seen in attachme *not transmitted at all*, in the sense that we have encour here. If human parental behavior can only occupy a c range (from sensitivity to cruelty) why should we ev anything but some corresponding range (A-B-C categor the response of human infants? This is suggested in the d sion of a continuum (or continua) of attachment behavior Lamb et al. review. What more can a one year old be exp to do?

Until the time when there is some conclusive evidenc infantile attachment in humans *is* genetically transmitte that it *does* owe its apparent universality to natural sele social developmentalists might do better to concentrate on accessible aspects of the phenomenon. If we psychologis forget the fuss over biological and sociobiological possibili this area (until the proper time), at least there would be or thing to worry about.

Bonding behaviours, behavioural binds, a biological bases

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In Ainsworth's (1979a) classification of infants on the ba their attachment or bonding behaviours in the Strange Sitt test lacks veridicality, then the correlations with anteceden consequent mother–infant interactions can never achiev significance. The review article by Lamb et al. suggest there is a degree of veridicality for the three major cl Types A, B, and C, especially if they are considered as s what arbitrary distinctions in an underlying dimension analysis given by Ainsworth, Blehar, Waters, and Wall (Table 14, p. 108) suggests two underlying dimensions of n nal behaviour that could at their extremes produce Type C infants. Maternal rejection could produce an appr avoidance conflict in the infant, and a maternal failure to pr adequate contact comfort could produce frustrative aggre Both types also seem to involve a further dimension of propriate timing of maternal responses, attributable to centric (Type A) and ineptly inconsistent (Type C) respon This shared dimension of mistimed responding may expla occasions when the data fail to separate Type A and C infan an extent, then, Ainsworth's classification and postulated nations seem consistent with the data and are plausible. W ever the causal antecedents may be, it seems reasonal accept the reviewers' suggestion that the test behaviours r sent the prevailing contemporary patterns of the infant teraction with their mothers. Ainsworth, Bell, and St (1971) compared abstracted classes of infant behaviour dimensions of maternal behaviours in the home with the pendently abstracted classes of Strange Situation tes haviours. Later Ainsworth et al. (1978) correlated mothe infant home behaviours with the three test classes or discriminant functions. Now if the Strange Situation test s reflects current home interactions, then it might be effective to use these interactions to establish classes or d sions and to use the results to analyse the test data. This give less ambiguous subgroupings and in addition might su changes in the test to reveal such groupings more clea would also make the purpose of the test apparent – nam give a quick and convenient measure of the prevailing er–infant attachment interactions. Such a test would not be as an indicator of a fixed attribute of the infant with pred power.

Nonetheless, the test will have some predictive pow

earlier and current interactions predispose subsequent responding. This may come about through processes such as canalization (G. Murphy 1947) and snowballing (Tomkins 1963) in the manner of epigenetic processes (Salzen 1968). In addition Type A and C classes suggest that the "behavioural bind" (Tomkins 1963) could contribute to the occurrence and stability of some interactions. Thus the Type A infant response of avoidance of a rejecting mother will ensure that mother and infant continue to respond in the same ways. Similarly the Type C infant response of resistance to inept handling makes it difficult for the mother to become more adept. In both cases mother and infant are in a behavioural bind; each behaves in a way that elicits responses that reactivate the same unfortunate behaviours. To bonding, therefore, may be added behavioural binds that, like canalization and snowballing, should lead to resistance to changing circumstances. Lamb et al. rightly point out that much of the evidence for the stability of infant attachment behaviour in the Strange Situation test is confounded with stable circumstances for mother and infant. Furthermore, later behavioural characteristics are reliably related only if there is continuity of the rearing environment at least for Type B versus non-B comparisons. The review also shows that where circumstances change there seem to be predictable changes in the security behaviour. But it is not clear how substantial such changed circumstances must be, nor how closely they must relate to the specific interaction behaviours of mother and infant. In the study by Thompson, Lamb, and Estes (1982) the significant changes were in maternal employment and in regular nonmaternal care. Both represent substantial changes in the care-giving interaction experience.

The stability and frequency of Type B infants, at least in the U.S. data, suggest a behavioural norm. It is fair to recall that Ainsworth (1979a) has noted the possible role of maternal behavioural norms in moderating potential temperamental differences in infant behaviours – a form of phenotypic buffering. Ainsworth believes that, despite the obvious contribution of cultural norms in such a process, the Type B pattern is a biological norm because of its adaptive character. Arguments about adaptiveness and survival value are too hypothetical to serve as evidence and simply invite the criticisms made in the review. But support for a biological norm must rest on the existence of homologous patterns of behaviour in other primates under normal species conditions and under experimental circumstances. Indeed Bowlby (1969) based his views on comparative evidence, and subsequent primate studies have been used in the same way (Rutter 1979; Salzen 1978). Furthermore, the Bowlby-Ainsworth phylogenetic view implies that species normal patterns will require least ontogenetic adjustment (adaptation?) by the mother and infant but especially the latter since the infant is least able to adjust at the start. Clearly all three types of infant adjust to their mothers, but Type B seems to involve least adjustment since mother and infant behaviours "fit" or mutually consummate from the start. In this sense Types A and C are deviations from an optimum both within the U.S. samples and in the two non-U.S. ones. However it would be a value judgment to say that such deviations are pathological because in all cases different competing interactions and priorities of the mothers have to be taken into consideration and it then becomes more convenient to talk of styles of mothering (Hinde 1982). More cross-cultural data are needed to determine the types and distribution of different interaction patterns, and within cultures more broadly based samples and clinical cases need examination. The questions at issue are how flexible the infant can be in coping with different styles of mothering and to what extent this flexibility is lost in subsequent social and emotional interactions or personality. The importance of the answers to these questions requires that the evidence be examined very critically, and the review by Lamb et al. has certainly done this and at the same time brought some order into an increasingly confusing data mass.

Authors' Response

Studying the security of infant–adult attachment: A reprise

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Diverse issues and questions are raised in the acc panying commentaries and to facilitate comprehens we respond to them here in four sections. First, briefly review what we did and did not state in our tar article, because many of the commentators appear have misunderstood our position on several crucial sues. Second, we discuss new data that have come to attention since completion of the review. In the th section, we focus on theoretical issues – particularly th pertaining to evolution and behavioral ecology. Fina we identify crucial topics for future research.

We are disappointed that many of the principal c tributors to the Strange Situation literature chose no write commentaries on our review. We sought publ tion in the *Behavioral and Brain Sciences* precisely cause it provides a forum for scholarly debate on cri issues. Open and public discussion – building on research and theory – seems essential if research in area is to advance. Thus the decision of some princ researchers in the area not to submit invited comn taries does not augur well for the field.

Restatement

Reading the commentaries suggests that some scien saw in our review conclusions and statements at vari with the content of the article. In particular, some c mentators (e.g. **Freedman** and **Grossmann & Gr mann**) apparently believed that we considered Strange Situation procedure invalid or worthless. H ever, the data do not support this view and our co sions (as **Chess** noted) were accordingly much more cumspect.

Antecedents. First, although many theorists, incl ourselves (e.g. Lamb 1981b; Lamb & Easterbrooks Thompson & Lamb 1983a), have suggested tha sensitivity or insensitivity of adult behavior import shapes Strange Situation behavior, we found no re evidence that this is the case. As we (and **Mills & Eis** observed, this does not mean that Strange Situ behavior is unaffected by parental behavior, but r that the hypothesis has not yet been verified. It s likely, however, that a number of other factors – ir ing temperament (**Chess, Feinman, Masters**) and experience with strangers, separation, and unfa contexts – are also important, with the relative i tance of these factors varying between individual

across cultures. As Mills & Eiserer, Hay, and Kovach & Kovach emphasize, this means that we need more research, not the abandonment of research in this area.

Some commentators (Grossmann & Grossmann, Petrovich & Gewirtz) suggested that we denigrated Ainsworth's pioneering research. On the contrary: We emphasized that Ainsworth's longitudinal study was a remarkably provocative and heuristically significant one. Moreover, we agree with Grossmann & Grossmann that the Ainsworth work should be seen as hypothesis-generating research – indeed, we described the study in these terms in the target article. We are, like Grossmann & Grossmann, keenly aware of the importance of both discovery and "proof" in the achievement of scientific progress. The point of our review was to evaluate the widely discussed hypotheses concerning Strange Situation behavior – now 10–20 years old – in the context of the available evidence. There was no reason to omit the Ainsworth study from consideration and methodological scrutiny in this context, particularly since its findings are increasingly represented within the scientific literature as well-established, generalizable findings. To reiterate: We concluded that the available evidence does not clearly support the hypotheses put forward by Ainsworth and others. This does not necessarily mean that parental behavior is unrelated to Strange Situation behavior, but it must lead us to *question* the notion that maternal sensitivity is the crucial and perhaps exclusive determinant of Strange Situation behavior.

Temporal stability and predictive validity. Second, we stated quite clearly in the target article that individual differences in Strange Situation behavior *do* predict later child behavior (including later Strange Situation behavior), provided there is continuity or stability in the family circumstances and child-rearing arrangements that appear to affect Strange Situation behavior. Contrary, perhaps, to Brown's impression, this need not mean that attachment bonds are not involved – only that relationships are sensitive to changes in the partners and in their social contexts. As Klopfer and Denenberg reiterate approvingly, early experiences seldom have effects that remain immutable in the face of powerful later experiences, and this conclusion appears to be borne out by the Strange Situation literature – provided we assume that Strange Situation behavior reflects some aspect or aspects of early experience. However, the reliable correlations between Strange Situation behavior and later behavior may be attributable to stable aspects of parental behavior that are correlated with both earlier and later child characteristics, rather than to the enduring effects of early experiences per se. Unfortunately, as Feinman points out, we do not yet have direct observational data indicating that changing patterns of parental behavior produce changes in Strange Situation behavior. This is an important area for future research, as indicated more fully in the final section of our response. We agree with Denenberg that early experiences are likely to be influential by way of interactions with later experiences, and are pleased that the experimental research he cites is consistent with our interpretation.

Quantitative consistency. Third, we suggested that the three group–eight subgroup classification system devised by Ainsworth, Blehar, Waters and Wall (1978) might not provide the best way of representing individual di ferences in Strange Situation behavior. We suggeste that although there is still much work to be done, mult ple continua, rather than categorical classes, might b most useful, and we do not share Freedman's belief th continuous variables are necessarily more cumberson than categorical ones. Imperfect as it may be, howeve the Ainsworth system has been remarkably successful i many respects – most notably, in research on the o togenetic stability of individual differences. Now 20 yea old, Ainsworth's system has shown strengths that mal the development of a better system a challenging tas

Biological meaning. Fourth, we questioned the appropr ateness of claims that one pattern of Strange Situatic behavior is adaptive while others are maladaptive. Un we understand better the determinants of Strange Situ tion behavior and can specify – at least hypothetically its fitness consequences, any speculation about the evol tionary meaning of individual differences in infanti behavior is of limited value. We also criticized the a sumption that human parents and infants have evolved form intimate, harmonious relationships, because mo ern evolutionary theory provides no grounds for assur ing this. Theoretical models suggest that conflict a deception can enhance fitness as much as cooperati does (see Ghiselin). In fact, Parker and MacNair (197 argue that parental sensitivity to offspring needs can grounds for conflict in the relationship. If it is fitne enhancing for the parent to provide for an offspring response to the latter's expression of need, the offspri may attempt to enhance its own fitness by requesti more resources than it is in the parent's interests provide. In sum, because the behavioral patterns c served in the Strange Situation do not have appare implications for reproductive success, interpretations terms of the principles of evolutionary biology are ina propriate at this time. As in the case of Bowlby's (19 masterful synthesis, of course, evolutionary thinking m help us in our conceptualization of the issues and prin ples involved in understanding infant behavior and dev opment, but these principles will not in themsel explain individual differences in Strange Situati behavior.

Both Hay and Kovach & Kovach take us to task overstating Ainsworth's commitment to an evolution adaptational approach, suggesting that perhaps Ai worth's support of Bowlby's (1969) ethological theory us to misperceive the role of evolutionary principles her thinking about Strange Situation behavior. Actua as pointed out in our review, the suggestion that pattern of Strange Situation behavior is adaptive w others are maladaptive has repeatedly been made in literature (e.g. Ainsworth 1979a; Main 1981; Sro 1979), which is why we chose to evaluate the cohere and integrity of these claims. Hay's criticisms, therefc are misdirected: It is not our but rather *others'* publis "attempt to link current research to Bowlby's etholog perspective [that] seems strained."

New data

Antecedents. In a recent longitudinal study of 60 mid class mothers and their infants, Belsky, Rovine,

Taylor (1983) related characteristics of interaction at 1, 3, and 9 months to Strange Situation behavior at 12 or 13 months. Belsky et al. tested the hypotheses that securely attached (B) infants would experience intermediate levels of stimulation, with avoidant (A) infants being overstimulated and resistant (C) infants being understimulated; resistant infants would have the least responsive mothers; avoidant infants would have least physical contact with their mothers; and A and C infants would be most irritable during the first months of life. Fourteen behavioral categories, most recorded with high reliability, were observed at home during 45-minute naturalistic sessions in which only mothers, children, and observers were present, and a theory-guided summary measure of reciprocal interaction created by summing scores on 9 behaviors was used (Belsky, Taylor, & Rovine 1983). Linear trend analyses across the three groups revealed a significant effect at 9 months, but no significant effect at either 1 or 3 months, although the means ranked similarly at all three ages. At 9 months, the avoidant children experienced the most, and resistant children the least, reciprocal interaction. Further analyses distinguishing the infantile and maternal behavioral components of the composite measure revealed similar trends on the summary measures of maternal behavior (significant only at 9 months) but no effects on the summary measure of infant behavior. The mothers of resistant infants also proved significantly less responsive to infant distress signals at 3 and 9 months, and significantly less responsive to positive vocalizations at 9 months, than the mothers of B-group infants. No significant differences were observed between A- and B-group infants in the frequency of close bodily contact, perhaps, as the authors suggested, because their measure was inappropriate. A- and C-group infants (combined) displayed more distress than B-group infants at both 3 and 9 months, although the A infants' distress was higher at 3 months and the C infants' at 9 months. Thus three of the four hypotheses advanced by Belsky et al. received at least some empirical support.

In focusing on the amount of stimulation (viewed as a novel index of maternal sensitivity) as a determinant of Strange Situation behavior, Belsky, Rovine & Taylor (1983) provide an interesting perspective on the antecedents of attachment security. The findings must be viewed cautiously until replicated, however, both because they were not reliable (although consistent) across all observations, and because in different (i.e. triadic) contexts, using a less detailed observation system, no systematic relationships were obtained between comparable indices of maternal and paternal behavior and subsequent Strange Situation behavior (Belsky, personal communication).

In another recent study, Miyake, Chen, Ujiie, Tajima, Satoh, and Takahaski (1981–82) studied 29 first-born, middle-class Japanese children and their mothers. When observed in the Strange Situation at 12 months, 19 were classified as B and 10 as C – a remarkably high proportion of C-group infants. The procedure aroused much more stress among these Japanese infants (as it may among Israeli infants; see Sagi, Lamb, Lewkowicz, Shoham, Dvir & Estes, in press) than in most U.S. samples – suggesting that the Strange Situation may not have psychologically similar meaning to infants in different cultures. This is probably attributable to both cultural differences in rearing practices and temperamenta differences. Temperamental factors were emphasized by Miyake et al. Other observations of these infants betweer birth and 30 months "suggested that there is a strong temperamental variable that is stable and preserved from the newborn period through to 23 months of age and tha tends to be associated with the type C infant" (Miyake e al. 1981–82, p. 27). In the newborn period, 5 out of 6 C and 7 out of 11 Bs were very much upset by the removal o a pacifier; at 1 and 3 months 1 of 12 Bs, compared with out of 7 Cs, showed high levels (undefined) of distres during unstructured home observations; at 7 months 6 o 7 Cs, compared with 3 out of 8 Bs, were upset by th entrance of a stranger, while 4 of 8 Bs and 6 of 7 Cs wer upset by separation from the mother; during the sam sequences at 7 months, the future Cs showed greate heart-rate reactions than did the future Bs; at 7 months mothers interrupted the "free play" of future Cs mor than did the mothers of future Bs, who were mor responsive, but there were no group differences in leve of stimulation or in levels of "effective stimulation"; and a 11 months, future Cs ran to their mothers more (and s played less) in a free-play context than future Bs did. "C the C's tested, 7 were female and 3 males, while ther were only 4 B females. This may indicate a correlatio among being a female, being a C and being fearful an inhibited" (Miyake et al. 1981–82, p. 28). Like most of th other associations reported here, however, the sex di ference is probably not statistically significant – indee no statistical tests were reported (except concerning con pliance), presumably because the sample size was s small.

Although not conclusive, these results support o contention that Strange Situation behavior may mea different things in different cultures, and that facto other than maternal behavior (e.g. temperament) m shape Strange Situation behavior. Observations of mate nal behavior yielded findings generally consistent wi Ainsworth's (though the measures differed) but inconsi tent with Belsky, Rovine & Taylor's (1983) hypothese Grossmann and Grossmann (1983, p. 10) have noted th "the avoidance in our children is not sufficiently e plained by the somewhat lower sensitivity of our future children's mothers at 2 and 6 months. It is also due differences in cultural expectancies, either between c tures at the same time or within cultures at differe times." The unusually high numbers of C infants in Isr (Sagi et al., in press) and Japan likewise suggest importa cultural differences in the meaning and interpretation Strange Situation behavior.

Finally, Rosen and Cicchetti (1983) have reported increased incidence of avoidant behavior among m treated children observed in the Strange Situation. T finding is consistent with that reported by Lan Gaensbauer, Malkin, and Schulz (in preparation) a supports the hypothesis that aberrant parental behavio associated with "insecure" Strange Situation behavic

The consequences of stability. Jacobson, Wille, Tian and Aytch (1983) observed 107 infants in the Stra Situation at 18 months and then observed 15 As, 15 and 15 Cs interacting for 25 minutes with an unfami same-sex B infant at 23.5 months. Group differences c of 9 measures of peer and mother interaction were ex

ined. The B and A infants engaged in more onlooker behavior than did the C infants, who engaged in more solitary play *and* more positive interaction with peers then either B or A infants; indeed the B infants engaged in the *least* positive interaction with peers. The A infants engaged in the most and the C infants least positive interaction with their mothers. Unfortunately, the significance of pairwise comparisons was not reported for any of these analyses. Even in their absence, however, it is clear that Pastor's (1980; 1981) and Waters, Wippman, and Sroufe's (1979) findings concerning the greater peer competence of B-group toddlers were not replicated.

Miyake et al. (1981–82) also investigated the predictive validity of Strange Situation behavior. They reported that at 16 and 20 months, B infants were more compliant with maternal commands and attempts to delay gratification whereas at 23 months, 4 of 10 Bs and 7 of 10 Cs were fearful of a stranger. The findings concerning compliance appear consistent with those of Londerville and Main (1981) and Matas, Arend, and Sroufe (1978), although Miyake et al. were unable to replicate Londerville and Main's findings with the specific measures they employed.

Summary. The results of these new studies provide general support for the conclusions reached in the target article. Differences in characteristics of parent–child interaction at home appear related to Strange Situation behavior, but the exact nature of the relationship remains unclear. Temperament too may be important. Whatever their origins, however, individual differences in Strange Situation behavior do appear to predict individual differences in later behavior – at least in socioeconomically stable samples. Relationships between Strange Situation behavior and peer interaction skills appear less reliable than relationships with other outcome measures.

On the usefulness of adaptational theory

In his commentary, **Ghiselin** alludes to three key issues (two of which we mentioned in our review) concerning the usefulness of evolutionary theory in the understanding of behavioral development. First, our understanding of natural selection has improved dramatically in the last two decades, and we have come to realize that the implications of a natural selection approach to the study of attachment are both broader and more complex than initially realized. Second, evolutionary approaches typically involve the consideration of present day function and historical constraint: Dealing with both issues at once is both challenging and perplexing. Third, many of the relevant theoretical issues apply generally to development in the broadest sense, and thus developmental psychologists might benefit greatly from close attention to advances in other fields.

Ghiselin's remarks are especially pertinent to the commentaries of **Rajecki** and **Hay**, both of whom appear to think that an evolutionary approach necessarily precludes an analysis of social learning or experiential mechanisms (broadly conceived). As Ghiselin points out, what evolves is a pattern of development through the life span. In humans this ontogenetic pattern includes many continuing processes of social learning, and this fact must itself be viewed in evolutionary perspective. Our view is not that evolutionary adaptational thinking is necessarily inappropriate (**Klopfer, Salzen**) but rather that the evol ary thinking underlying contemporary attachment t is sometimes dated and sometimes confused witl cepts of adaptation borrowed from other fields psychiatry). Readers in search of a good current intı tion to this literature are urged to consult Dal Wilson (1983).

It is not yet clear whether our understanding o vidual differences will be advanced by incorporati perspective and principles of evolutionary biology. ever, we believe that evolutionary theory can be h in devising verifiable refutable hypotheses to gui ture research and that it would behoove developm ists to attempt this. Unfortunately, this is no eas and we have made slow progress in this regard (Pet & **Gewirtz**).

Conclusion

The Strange Situation was developed more tha decades ago and in the last decade has become th important technique in research on early socioem development. In the target article, we sought b determine whether research on the Strange Situati answered the questions that motivated it, and to late further research in this area. We can identify a four directions for research using the Strange Situ

a. Longitudinal studies of parental behavior, ent–child attachment, and child development. I review, we identified alternative hypotheses conc the causal paths governing the associations betwee Strange Situation behavior and later childhood bel On the one hand, the remarkable effectiveness Strange Situation in predicting later behavior cou tain because it taps the quality of parent–infant in tion at a sensitive, formative period. On the other the continuity between Strange Situation behavi later child behavior could obtain because both are lated with contemporaneous patterns of parent- interaction, and it is the continuity of parental care that explains the prediction. A third, probably realistic, hypothesis is that early experiences, lik stitutional differences, shape children's reactions sequent experiences and also influence the types o riences they encounter. In this way, early expe interact with later experiences to shape behavi **Denenberg**). The challenge is to specify these i tions more systematically and to conduct hypc testing studies.

Unfortunately, longitudinal investigations usi Strange Situation are rarely designed to compa merits of rival hypotheses. To contrast the rival h eses outlined above, as **Feinman** notes, we need t contemporaneously parental behavior, the quality ent–infant interaction, Strange Situation behavi child behavior on two or more occasions. Moreove such longitudinal studies are quasi experiments than true experiments, they must employ measu systems carefully designed for use in structural reg statistical analyses to ensure that alternative causa nations can be tested (e.g. Nesselroade & Baltes Because constructs such as temperament and soci rience may significantly affect the development of

ment as well as Strange Situation behavior, these too must be tested through the comparison of rival hypotheses in future research. The field has matured to the point that we need not only exploratory studies but also systematic attempts to decide between competing hypotheses and theories.

b. Cross-cultural research. Increasingly, the Strange Situation is being used outside the United States, and it is clear that the distribution of infants into classification groups and subgroups differs among cultures. It has accordingly been suggested that infants from different cultures experience the Strange Situation in different ways. Is this difference quantitative (e.g. both American and Israeli infants find the procedure stressful, the Israelis simply find it more stressful), or qualitative (e.g. German infants understand that the separation will be temporary, and do not regard the stranger as a threat)? If the difference is qualitative, is it possible or even desirable to develop culturally specific assessment procedures that are functionally equivalent and produce comparable indices of attachment in different cultures?

Cross-culturally divergent findings also suggest important substantive questions concerning the varied goals and means of child rearing in different cultures. Do parents in different cultures aim to develop different constellations of personality and behavior in their infants? Do they desire different affective qualities in their attachment relationships? International discussion and collaborative research will be essential in addressing these questions.

c. The assessment of attachment. Our suggestion that we develop continuous measures for summarizing Strange Situation behavior would be, at best, only a beginning to innovative research on the measurement of attachment relationships. We need to develop and integrate measures of behavior in both laboratory and nonlaboratory contexts, combining information across contexts to create a more reliable measure of individual or dyadic function, rather than simply correlating Strange Situation behavior and indices of behavior in other contexts, as has been the popular strategy. Such composite measures, which take into account aspects of behavior in diverse contexts, are likely to be more reliable, powerful, and valid than measures of behavior in any one context. Cicchetti's masterful review of reliability assessment should be of great use to researchers undertaking the construction of such measures. Also important, as Brown reiterates, is the need to ensure that standardized situations are in fact psychologically comparable for different subjects whose prior experiences and temperament may lead them to perceive apparently similar situations quite differently.

Freedman's call for more naturalistic research is well taken and consistent with others' appeals (Bronfenbrenner 1979; McCall 1977; Weisz 1978). Like Bronfenbrenner, McCall, and Weisz, however, we believe that both laboratory and field research are valuable and necessary. A strength of the Strange Situation research has been the search for meaningful associations between behavior at home and in the laboratory.

d. Evolutionary theory. Attachment theory took a great stride forward when John Bowlby synthesized ideas from ethology, psychoanalysis, and cybernetics. This encourages us to hope that new insights can be gained if we once again undertake the daunting task of synthesizing contemporary evolutionary and psychological theory. In evolutionary theory, at least, there are important theoretical resources that developmentalists have yet to mine: for example, life-span evolutionary theory (Stearns 1976; 1977) or parent–offspring conflict theory (Parker & MacNair 1979). Patience will be critical: Even if interesting hypotheses are developed, it may be a long time before feasible empirical tests can be devised.

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