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I am submitting herewith a dissertation written by Don Fernando Azevedo entitled "The State-Trait Distinction and Insular Mothers: A New Look at Maternal Responding." I have examined the final copy of this dissertation for form and content and recommend that it be accepted in partial fulfillment of the requirements for the degree of Doctor of Philosophy, with a major in Psychology.

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THE STATE-TRAIT DISTINCTION AND INSULAR MOTHERS:
A NEW LOOK AT MATERNAL RESPONDING

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Doctor of Philosophy
Degree
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Don Fernando Azevedo

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DEDICATION

To Janice Ellen Korner Azevedo, my wife and best friend. This dissertation is largely the result of her dedication in keeping my mind, soul, and body all together even when the process insisted they should be scattered to the winds. Thanks for laughing with me.

"If you don't dream, it can't come true."

Carlos A. Azevedo

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ABSTRACT

The state versus trait issue has been argued by almost every psychological researcher, directly or indirectly, since the beginning of recorded psychology. Early researchers in this century studied groups of individuals with some diagnosable psychopathology and based their trait definitions on the observation of behavior. The primary research approach to this problem in the latter part of this century has been with college sophomores and used self-report, paper and pencil tasks. The basic process consisted of theoretically generating a set of behaviors that "defined" a trait label already chosen. This approach created systematic biases in judgments, constructs, and semantics which often accounted for more of the variance found than an actual clustering of the variables. This research was proposed to develop a methodology suitable for a series of research projects studying the possible trait like behavior in insular mothers as they interact with their children. This would return to a behaviorally based approach to the study of traits and the use of a subjects with some diagnosable psychopathology. The insular mother is

generally described as single, of low income, and has at least one oppositional child. She is isolated from social contact; what contact she does have, she reports as negative, and family life is generally described by others as mutually coercive. She also tends to exhibit most of the signs of clinical depression. Subjects were the archival records of 10 families who had been treated or were being treated at the Child Behavior Institute for conduct problems with their children. Data were gathered using the Systematic Observation Codes - Revision IIb (SOC-IIb) in eight one hour sessions for each mother. Data were reduced using autocorrelation and factor analysis. Results suggested this is a plausible methodology with the addition of some additional variable selection techniques. There was also some evidence from the exploration of a mother from each group that the two groups may differ as to who seemed to be driving the patterned interaction suggested by the factor analysis.

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CHAPTER 1

INTRODUCTION

Statement of Purpose

The state versus trait issue has been argued by almost every psychological researcher, directly or indirectly, since the beginning of recorded psychology. The controversy has taken many forms such as locus of control and "mentalism" versus "behaviorism". In reviewing the literature, an interesting shift was noted in the populations used by various researchers over the years. Early in the century, Freud, for example, was describing traits in a basically psychopathological group. Later, the state side of the argument argued against traits while studying college sophomores and appeared to generalize between the two diverse groups.

It is also interesting that although much has been done to try to study the problem reportedly behaviorally, it has frequently been approached from a linguistic position. Traits are hypothesized and then the underlying behaviors are inferred through some theoretical construction. This approach may be plagued by three basic problems: Judgment bias, construct bias, and semantic bias. What people do has played a relatively small part particularly in recent research as compared to what people say they do.

Coupled with the issue of subject choice and the role of actual behavior is the binary attitude frequently taken when studying the problem. Much of the research is filled with arguments apparently presupposing there can be only one or the other. Even the way the problem has been stated often suggests this. The state-trait dichotomy is likely, akin to most problems in psychology, some form of continuum.

What this research proposed was to develop a methodology suitable for a series of research projects studying the possible trait like behavior in insular mothers as they interact with their children. The insular mother, as described by Wahler, Leske, and Rogers (1979) and Wahler (1980b), is generally a single mother of low income who reports herself to be relatively isolated from social contact in her community. Of the few contacts she does have, she tends to rate them as generally negative. She enters treatment generally because of the oppositional behavior of one of her children and family life can be described by observers as mutually coercive. One other aspect of the insular mother is that they also tend to exhibit most of the signs of clinical depression.

The data gathered for analysis consisted of eight hours of home observations of the mother-child dyad coded according to the Systematic Observation Codes-Revision IIb. Subjects were 10 mother-child dyads in treatment at the Child Behavior Institute for the coercive behavior of the

child. Five of the mothers were considered poor trackers of the children's behavior and diagnosed as depressed. The other five mothers were considered good trackers of their children's behavior and were not depressed. Data were reduced using autocorrelation and factor analysis. Results suggested this is a plausible methodology with the inclusion of several variable screening techniques to rule out variables so infrequent so as to add little to the understanding of the factor matrix. There was also some suggestion of a difference between the two groups from the analysis of one mother from each group. The two seemed to differ along who seemed to be driving the patterned interaction suggested by the factor analysis. The good tracking mother who was not depressed seemed to be more active while the mother who did not track as well and was depressed seemed to be more reactive.

Review of the Literature

There are two issues within the argument that have particular consequence, historically, to the state-trait debate: The question of who will be the subjects and the question of how to define a trait. The importance of each issue to this argument stems from a consideration of the roots of this controversy. The trait theory was really in its glory at the turn of the century when Freud and the psychoanalytic school were just beginning to develop a

theory of personality development. The analysts described traits in terms of intrapsychic structures and the early drive theory. These intrapsychic structures and the dynamic balance between them were inferred from behaviors that seemed to cluster around certain themes and tended to be consistent across time and situations. Even before Freud, Emil Kraepelin was describing trait like behavior when classifying mental disorders. Kraepelin's traits were sets of behaviors which were typical of patients suffering similar problems; sets of behaviors consistent across both time and situation. Indeed, stripping away the theoretical jargon and looking at the behaviors which led to the theory, one can note that both Freud and Kraepelin described traits as behavioral clusters that seemed to be present across time and situation and those clusters defined the pathology by restricting one's freedom and not allowing the individual to act appropriately to situational cues.

In the early years of this controversy, researchers did not study many normal individuals. They were busy trying to establish treatment modalities for those who were suffering. This was also an era when there were few if any psychological theories of personality and the construction of such theories was just beginning. Already, the issues of subject and definition are present in the argument. The subjects of choice were those suffering from pathology. The definition arose from the observation and subsequent

assumption that these subjects were not responding to external cues therefore they must have been responding to internal cues. A fledgling theory makes an intuitive leap.

The current version of the state side of the question came into existence with the development of the behavioral school of thought in the 1940's and '50's. There was another notable change as that school began to develop. Behavioral researchers began using groups of "normal" (without diagnosable psychological dysfunction) individuals in exploring ideas about human development and responding. This could have major impact on the understanding of the trait-state controversy since we may have made a bad intuitive leap (that is, from a theory of personality based on the study of pathological subjects to its literal application to a "normal" population) from the early analytic construction of personality. The next logical question is why was that leap made.

In the '40's and especially in the '50's there was a great political push to focus on the positive. This was the golden age of the family and a prosperous nation. We had just successfully completed a devastating war in Europe and no one wanted to hear about the negative side of anything including psychological functioning. The idea was to understand the "normal" individual. Researchers, regardless of theoretical orientation, respond to the demands and fluctuations of the public for that is where the research

money is. It is reasonable to suggest that researchers from the analytically oriented school of thought sought to continue their work on behavioral consistency across time and situation, but given the political climate, couched the research using "normal" individuals. There likely was not time nor the resources to observe large samples of subjects waiting for the similarities to appear as there had been when observing pathological people who were generally hospitalized or coming regularly to the researcher for therapy. There was also a large, ready made, easily accessible population just waiting to be studied: The college sophomore. Hence, the leap was made from observing behavior and classifying it in a basically pathological population to trying to predict behavior in normal people from theories derived from the observation of those pathological people.

So far, only a brief overview of some important shifts in the research conceptualization of traits has been noted. The current level of controversy about this issue is somewhat more heated, but still suffers the same subject choice problem when considering its results.

The trait theory of personality, for the majority of this century, has defined a trait as a group of behaviors that describes some central theme and is stable over time and across situations. In the late '60's, there was a great deal of controversy over the trait theory as Mischel (1968)

challenged the prevailing notion of the stability of behavior across most situations. He suggested that the only areas demonstrating any stability across situation were cognitive functions and intelligence. These had some degree of stability with respect to time and situation. However, he emphasized that situational influences will effect performance on measures of these types of behavior. The areas he challenged the most were the areas of personality functioning and generalized attitudes. These were areas such as attitudes towards authority figures, moral behavior, and sexual identification. He stated that in those cases, situational influences lead to the discriminative differences in attitudes and behaviors.

The studies by Hartshorne and May (1928) and Hartshorne, May and Shuttleworth (1932) have been two of the frequently cited articles in defense of the state position. The former's findings on cheating and deceptive behaviors and the latter, a study on moral behavior, are seen as support for the notion that generalized attitudes and personality functioning are situation specific. Although in both studies there were high correlations between self report measures and paper and pencil tests administered in certain situations, when other measures were taken in other settings, the correlations dropped considerably. From these findings and similar findings in several other works, Mischel drew the conclusion that "the data on moral behavior

provide no support for the widespread psychodynamic belief in a unitary intrapsychic moral agency like the superego, or for a unitary trait entity of conscience or honesty" (1968, p. 26). The implied message is that there are no traits except for some cognitive behaviors that tend to act like traits.

This summary judgment poses a paradox. If people are highly situation specific, how is it we recognize people from setting to setting with some kind of continuity? Although the argument can be made that we interpret behavior patterns and visual cues that allow us to identify others even in highly different situations, intuitively, one would suspect that there would be something more. Even the idea that when we interact with another individual we act as a stimulus class that essentially creates the same situation for the other and hence we are seeing behavioral consistency over time and not across situation is not sufficient to satisfy one's intuitive belief in traits. Interestingly enough, the state position leaves an open door concerning that argument by stating that there is some evidence for the consistency of behavior over time and situation with the caveat that consistency across situation accounts for little of the variance in behavior. Perhaps that is what allows us the illusion of continuity.

In fact, Mischel (1982) takes up this argument in his review of the literature. He notes that the position that

one takes on the specificity versus consistency issue "profoundly affects one's view of personality and the strategies worth pursuing in the search for its nature and implications" (Mischel, 1982, p. 730). Compelling intuitive reasoning supports the notion of broad dispositions and cross situational consistency. Yet, the history of empirical research in this area has produced counter-intuitive results which suggests much less behavioral consistency. The research has led some behavioral scientists (e.g., Moos & Fuhr, 1982; Patterson, 1976; and Raush, 1977) to abandon trait ideas and seek finer grained methods for analyzing personality functioning. Mischel (1982) challenges, in particular, the most recent solutions to the dilemma: Epstein and O'Brien's (1985) reliability solution, Bem and Allen's (1974) template matching solution, and Bem and Funder's (1978) idiographic solution.

Epstein and O'Brien (1985) argue for the aggregate composites of measures to increase reliability coefficients. They contend that in the past, the reliability of single measures and one time samplings of behavior were low because of the measurement technique hence insinuating the lack of trait like behaviors. By aggregating (another term essentially for repeated measures) and using some basic psychometric techniques, Epstein feels that the patterns that structure behavior can be discerned. As support for

this position, the article reviews and reinterprets four classic studies and the results of Mischel and Peake's (1982) study.

In reviewing these studies, Epstein and O'Brien discussed how the use of basic psychometric methods would have increased the reliability of the studies and supported the trait position. Oddly enough, Epstein and O'Brien spend the first part of the paper citing past problems with the trait research specifically problems with definition.

"It was demonstrated that systematic biases in judgments, the constructs of raters, and the semantic relatedness of trait words were often more responsible for the relationships that were found than the actual consistency of the people who were rated (e.g. Chapman & Chapman, 1969; Jones & Nisbett, 1971; Ross, 1977; Shweder, 1975). Thus, the widespread belief in traits by psychologists and laymen, alike, could be dismissed as a folble of human thinking" (Epstein & O'Brien, 1982, p. 515).

Although their dismissal of the trait idea is a bit overstated, the criticisms by authors like Chapman and Chapman (1969) should be considered. Unfortunately, Epstein and O'Brien go on to state basic techniques for establishing traits that commit the same mistakes. Their first step is to create a list of items identified through theoretical construction. This one step violates all three of the

criticisms just quoted. Creating the list will, by definition, require the judgment of the raters. Using theory to determine relatedness requires that the raters have similar constructs used to judge the items. If the research has predetermined summary terms for behavior, then there will be obvious semantic problems. Epstein keeps promising a behaviorally based exploration of traits and ends up consistently approaching the task cognitively.

Coupled with this trait definition problem is a lack of newness in the aggregation solution. In earlier years, it was simply called repeated measures. Several different studies from the early era of the debate (Dudycha, 1936; Hartshorne & May, 1928; Newcomb, 1929) all employed repeated measures. Unfortunately, their findings failed to resolve the enduring debate. It can also be argued that the repeated measures or aggregation establishes temporal stability which is less of an issue than cross situational consistency. In noting the Epstein (1979) study, Mischel suggests that the reliability coefficients are evidence of temporal stability which has never really been a debated issue. However, he believes that the evidence for cross situational stability is less than impressive. Those behaviors cited in the Epstein (1979) research are not impressive because the high correlations even across settings are between behaviors that, by definition, are dependent on one another.

Bem and his colleagues (Bem & Allen, 1974; and Bem & Funder, 1978) put forward a trait theory of personality based on what they called the "idiographic," and "template matching" solutions. The "idiographic solution" (Bem & Allen, 1974) attempts to integrate the person and situation variables "by embracing . . . only those behaviors and situations which cohere for the individual". They then take that behavior and situation set and rationally deduce a label for it. The label is, again, one based in a literary understanding of the behavior labels (i.e. based on semantics) by the individual and does not allow the behaviors and situations to stand in covariation without further prejudice.

In Mischel's (1982) review of the literature on the debate, he reported the results of two replication studies of Bem and his colleagues work. The first assessed the idiographic solution searching for the traits of friendliness and conscientiousness. The idiographic solution involves allowing the subject to choose the behaviors from their repertoire that he/she believes best represent the trait being studied. Already there is a definition problem since the trait is defined by the semantic relatedness of behavior to the trait label, and the judgment of the relatedness is based on constructs over which the researchers have no control nor are they paying attention to its possible effects. Mischel (1982) designed

the study so as to assess both temporal and cross situational stability. As was expected, aggregation improved the reliability and increased the correlations for temporal stability. As mentioned earlier, Mischel does not take issue with temporal stability. The issue is cross situational stability. However, when behaviors were aggregated over occasions, there was no increase in such stability. He concludes that "although aggregation over occasions has the desirable effect of enhancing reliability, it does not provide a simple solution to the consistency paradox" (Mischel, 1982, p. 736). Mischel does not address the definition problem.

Bem and Funder's (1978) "template matching solution" proposes characterizing situations as sets of template-behavior pairs. A template is "a personality description of an idealized 'type' of person expected to behave in a particular way in that setting" (Bem & Funder, 1978, p.486). The probability of accurate prediction of behavior increases as the similarity between the individual characteristics and the associated template increases. While their results added support to a person-situation interactional position, they offer little to the understanding of traits.

Mischel's (1982) second replication assessed the template matching solution. Mischel's interpretation of Bem and Funder's definition was "to increase the predictive

power of trait information by matching the individual's personality with the personality of the situation defined by Q-sort ratings that supply 'portraits' of both the individual and the setting" (Mischel, 1982, p. 739). The predictive power is increased through the identification of situations that are functionally dissimilar but appear similar. Of all the research solutions that have attempted to support the trait position, it seems that this one is the one in which Mischel (1982) puts the most credence. Unfortunately, the results of his replication are not as impressive as the original study. Mischel (1982) notes the major drawback of this solution and all the solutions presented so far when he states:

"Though they conceptualized and described their efforts in the language of person-situation interactions and hoped to capture the personality of the delay [of gratification] situation, their method only described the correlates of performance in that situation (i.e., the ratings associated with duration of waiting time)" (Mischel, 1982, p. 743).

In other words, their results depicted the person's performance rather than aspects of the situation.

It is interesting to note that in almost all of the studies used to support or refute the trait position, the researchers used college students as subjects. Indeed, it appears that in all the recent research on the

person-situation debate the subjects have been college students. This may be an important variable that has not been considered as a source of variance in the past, but, perhaps, should be when trying to generalize the results to any population other than college students. One should keep in mind that individuals who are capable of attending colleges are generally those that have adapted successfully to societies demands. Although the number of individuals who attend colleges has grown by leaps and bounds over the past three decades, the totals are representative of only a small percentage of the population in the United States. In addition, it is most likely that those students who sign up for the majority of the studies and actually attend the research session are those who are motivated and genuinely interested in psychological research and one should begin to question the validity of generalizing the results of these studies to populations that may be less well adapted to society.

Before continuing with the idea of subjects, let us turn to the definition of trait and trait-like behavior. It seems that Epstein and O'Brien (1985) define a trait as a global term that describes some large area of human behavior. That large area is then "defined" by choosing items from the relevant domain and intercorrelating them to establish the measurement of a common construct. It appears that the definition of traits is basically a

literary-semantic one (a criticism alluded to above) in which the emphasis seems to be placed on the linguistic translation and logic implied by the linguistics that determine what a trait is and what behaviors should underpin the construct. Hence, we have developed "traits" of "altruism," "intelligence," and "conscientiousness" just to name a few. Even in the research that has developed the construct by looking at behavior first, one can see that the development of the construct is based on the names or summary terms we apply to the behaviors emitted (criticisms also noted above as construct and judgment bias). Again, although the intended emphasis is behavior, we have found ourselves reverting to literary-semantic understandings, and perhaps prejudices, to name the trait. Each of the traits listed above has a literary-semantic history that will bias any researcher trying to decide what kinds of behaviors would fall in that category. This appears to be a questionable way to look at what a trait is; however, historically, it is relatively easy to see how psychology has come to this position. Simply put, we have attempted to leap beyond the description of human behavior to attempt prediction prematurely.

Before moving on to the basic problems with the current version of the debate and the approach to be taken in this research, it is important to cover one other theorist's position on traits. George A. Kelly had an interesting

approach to understanding traits. His term, constructs, was geared toward understanding the categorization schemes an individual used to appraise and predict someone else's behavior in relation to one's own (Kelly, 1955). In developing his theory of constructs, Kelly, seemed to be evaluating the role types with which individual's categorized others. In his theory, people are thought to respond to others in terms of these constructs/roles. Someone might meet another and think, "She's just like my mother," and proceed to interact with the person expecting the person will respond accordingly with the role "mother" (Kelly, 1955). Although this way of approaching traits is somewhat different than the main stream argument, it is still useful in that Kelly saw that everyday people, like the psychologist-scientist, seek to create and test hypotheses about the behavior of others in order to have some control and understanding of their surroundings (Kelly, 1955; and Kelly, 1956/1969). He saw this need to understand and control as man's most overwhelming compulsion. Indeed, the fundamental postulate upon which he built his theory was: "A person's processes are psychologically channelized by the ways in which he anticipates events" (Kelly, 1955, p. 46). Kelly suggested:

"One's current acts and undertakings might have as much to do with the development of his personality as did the imprint of events with which he came in contact or

the insights he was able to conjure up with the help of his therapists" (1963/1969, p. 56).

In his subsequent travels gathering data in support of this theory, he came to the conclusion that, for example, a mother's description of her son's disruptive behavior was more a representation of the son's disruptive influence on the mother than a description of the son's situation. Perhaps more simply, Kelly saw people as describing the behavior of others in terms of their effect on the describer. He saw these descriptions as ways of construing one's self or another so as to control the world about one either by convincing the other to construe the world in the same way or to justify one's subsequent behavior (Kelly, 1958/1969). However, as one might have already noticed, the semantics can easily bog one down very quickly as one tries to analyze the interplay between two individual's constructs.

It seems Kelly responded to that problem of endless regression (akin to van den Berg's ideas on endless regression, 1961, p. 69) of who is influencing whom to construe what how by developing eleven corollaries to his fundamental postulate. Perhaps this was his attempt to influence others to construe interpersonal behavior in the same way. These corollaries are beyond the scope of this paper; however, Kelly's ideas are included here because, in a way, they are the semantic equivalent of the behavioral

hypotheses put forth here. These mothers have construed their children in such a way that the mothers will act towards their children in a set and characteristic way which may not be related necessarily to the child's behavior. The mother's description and actions to the child may be more representative of the child's disruptive influence on the mother's life and not so much a description of that child in his life situation. Further, the child will act in such a way as to conform to the mother's construction perhaps to develop a closeness that is not developing in other ways (somewhat akin to Kelly's commonality corollary, 1955, p.90 or Wahler's uncertainty hypothesis 1980a, that is, the child will act to reduce the uncertainty in the environment by engaging in behaviors which have predictable outcomes). However, this is getting ahead of the argument.

There are three aspects of the current state-trait controversy that need to be examined in detail. First, the use of college students as the subject base from which to determine the existence and function of trait like behavior. Second, the definition of what determines a trait (the linguistic predictor model versus the observation of behavior and post hoc clustering). Third, the attitude with which we approach the discussion of the issue. The literature abounds with statements which are of a binary attitude, either it is state or it is trait. If there is one thing that psychology as a science has established, it

is that very few human behaviors are the result of just one thing or just another. It may be even more than the interaction of person and situation as several theorists have proposed (e.g. Bem & Funder, 1978; and Pervin, 1983).

Given the historical aspects of the controversy, it is not unreasonable to propose that trait like behavior flows along a continuum. Likewise, state like behavior would also flow along a parallel continuum. The propensity with which an individual would rely on either state like to trait like behaving would covary with the degree of developmental insult (psychopathology). The term developmental insult is used here to mean any forces causing deviations from the theoretically normal process of development. (Although an argument could also be made for genetic and perhaps other factors causing deviations from the theoretically normal process of development, the emphasis, at least concerning the development of traits, is on how the individual reacts and adapts to that alteration in the developmental path.) The higher the level of developmental insult, the more likely it is that the individual will act from a trait like position and less from a state like position although both abilities would be present. The lower the level of developmental insult, the less likely it is that the individual will rely on trait like responding and the more they will respond to state like cues. In these latter individuals, traits would serve as internal structures which

would provide the basis from which they would interpret external stimuli and choose appropriate responses. These statements do not preclude positive traits. As has been stated by numerous developmentally based personality theorists (e.g. Freud, 1901; Erikson, 1963; Horney, 1939), no one progresses through the developmental sequela without some trauma and insult. It is the frequency of these trauma and how these issues are worked out that determines the level of resultant pathology. Hence, it is possible for the positive resolution of trauma and insult to result in a trait like cluster that is used and filtered through state like responding. To better understand this idea, let us turn to the definition of trait.

Earlier, I alluded to the idea that we had leaped beyond the information we had to attempt to predict and describe the behavior of a different population. I suggest that trait theorists heed the criticisms cited above and develop a behaviorally based assessment paradigm to determine the existence and function of trait like behavior. Although not stated in the terminology of the person-situation debate, several theorists (Kara & Wahler, 1977; Kazdin, 1982; Wahler, 1975; Wahler, Berland, & Coe, 1979; Wahler and Fox, 1980) have been developing seemingly parallel methods for approaching this problem.

Kazdin, while working on a behavioral description of symptom substitution, has described a phenomenon he calls

response covariation. "Response covariation means that two or more responses are related. When responses covary, a change in one response affects other responses" (Kazdin, 1982, p. 352). Wahler describes the same phenomenon as response clustering adding that when a response cluster is cued by a situation, the subsequent string of behavior will be determined more by the response-response chains than by situational cue-response chains. In other words, the probability of response A occurring in the next time segment will be dependent more on the preceding subject response than the preceding situational cue. This, then, leads to the definition of trait used here. A trait is a response cluster with relatively high intercorrelations among its component behaviors.

How then does the insular mother fit into this philosophical debate, one is likely asking. As has been noted above, the prior research points to an idea that traits will more likely be found in individuals suffering some diagnosable (by The Diagnostic and Statistical Manual of Mental Disorders, 3rd edition standards) psychopathology. Indeed, tightly interwoven response classes, traits, have been found in pathological children by several authors (e.g., Becker, Turner, & Sajwaj, 1978; Jackson & Calhoun, 1977; Nordquist, 1971; Wahler, Sperling, Thomas, Teeter, & Luper, 1970). To date and to the best of my knowledge,

there has been no study of the psychopathologically impaired adult and traits.

The insular mother, as described by Wahler, Leske, and Rogers (1979) and Wahler (1980b), is generally a single mother of low income who reports herself to be relatively isolated from social contact in her community. Of the few contacts she does have, she tends to rate them as generally negative. She enters treatment generally because of the oppositional behavior of one of her children and family life can be described by observers as mutually coercive. One other aspect of the insular mother is that they also tend to exhibit most of the signs of clinical depression.

This research was proposed to address each of the issues of subject choice, definition of trait, and attitude by looking at a sample of mothers who have entered treatment at the Child Behavior Institute with the presenting complaint being the presence of a coercive child. Utilizing a post hoc analysis of the behavior, it attempted to determine any clustering of behaviors indicative of traits. It was hypothesized there would be a continuum upon which the mother-child dyads would fall from the data. On one end of the continuum would be mothers who were diagnosably depressed, poor trackers of their child's behavior (mothers who scored high on Wahler's (1986) indiscriminate index), and demonstrate highly intercorrelated responses not consistently related to the behaviors of their child

(traits). These mothers are thought to be poor trackers because, as noted earlier, they are thought to possess strong patterns of response covariation without regard for the child's behavior. On the other end of the continuum would be mothers who were not diagnosably depressed, were better trackers of their child's behavior (mothers who scored low on Wahler's (1986) indiscriminate index), and who may have highly intercorrelated responses, but they would be consistent in relation to the behavior of the child. In other words, the child's behavior is thought to determine the mother's response covariation. The consistency would speak, then, not to a trait but to accurate state like responding. Therefore, since we are looking in the dyads, covariation patterns between mother and child behaviors should be diffuse in the indiscriminate mothers and tighter in the discriminate mothers. This sample speaks to the issue of subject choice by making the sample fit the definition of trait. When looking for fish, look in a stream, pond, or ocean; when looking for traits, look in a group of individuals who have a high likelihood of exhibiting pathological behavior, at least according to the definition of trait used here. With the current level of methodological sophistication, it was possible to infer, only, the response covariations of the mothers since they could not be separated from the dyadic interaction.

However, once the methodology is better developed, it is hoped one will be able to tease out this question.

Further support for the use of this population in studying traits is based in the dynamics underlying the coercive family process. Patterson (1974) argues that the a family caught in a coercive cycle will have a mother who is overly compliant with the demands of her child. Wahler (1980a) points out that in many families with coercion problems, the mother responds in an indiscriminate fashion. Wahler describes an indiscriminate mother as one whose responses have little to do with the child's behavior. The child, in turn, is likely to adapt by engaging in aversive behaviors which develop more certainty for the child in the mother's responding. Although responding inappropriately, Patterson's description of the compliant mother fits a state like position. She does so because in order to be compliant with her child's demands she must be tracking situational cues and responding to them. Conversely, Wahler's description of the indiscriminate mother fits the trait like position. She is not responding to her child's situational cues, but responding to some other stimulus. Wahler's research indicates that her behavior is triggered by response-response chains which fits the current definition of trait.

The idea of a good tracking mother and a bad tracking mother requires some elaboration. Good tracking simply

means the mother is observing and responding to the child's behavior. This would likely show up in the factor analysis as a strong correlation matrix with factors clearly showing a consistent relationship between parent and child behaviors if the range of behaviors is limited. It is also possible such a pair would engage in such a wide range of behaviors, so that the density of any one set of behaviors would be insufficient to allow for the development of a factor. The consistent relationship follows from what is expected of social interaction. That is, one would match the valence of the behavior with which one is being presented by the other. More concretely, if one is approached by another in a neutral manner, the consistent, expected response would be to return the approach in a neutral manner. Wahler's (1986) indiscriminate index measures the number of mismatches; the higher the number of mismatches, the more indiscriminate the mother is considered to be. Bad tracking, then, denotes a mother who's own behaviors bear little or no relationship to the child's behaviors. Thus, it is not untoward to suspect the likelihood of strong maternal response covariation patterns in these depressed, indiscriminate mothers. When looking for patterns of mother-child interaction via factor analysis of the dyad, one would expect diffuse dyadic patterns for the indiscriminate mother.

The search for these factors and their implication of state-trait phenomena will best be served through the fine

grained analysis of what people do in natural settings. Although the logical argument for these factors is good, in order to begin to establish the matrices from which the factors can be developed, several methodological issues must be addressed. So far, this piece of research suggests the issue of subject choice as a major variable in the pursuit of traits. Also of consequence is the methodology of choice and the limits of time series data. Indeed, methodological choice is probably the central issue at this point in the research series and thus the primary focus of this work.

Theoretical Methodological Issues

Given this proposal encompassed a huge, largely undeveloped territory, its aim was to develop a methodology with which to begin a series of research projects to address the issues stated above thus providing a basis for a continuum model of the state-trait dichotomy. The continuum proposes a pathological end where people would respond in a more rigidly trait like fashion. In other words, these individuals would demonstrate high inter-response covariations with little regard for environmental stimuli. On the other end, people would respond in a fashion that allowed for fluid transitions between state like and trait like behaviors. These people would demonstrate a responsiveness to environmental cues and also have typical ways of responding. Individuals in this range may have

lower covariation levels given the idea of fluid transitions between responding to the environment and responding to internal structures. That fluidity may be associated with a wider range of behaviors whose overall count would be too small to generate detectable covariation levels. This continuum idea cannot be addressed yet and it is quite speculative; however, as the methodology evolves, it would be an interesting idea to pursue. The main issue here is the exploration of a methodology suitable to deal with this question.

The definition of traits bases them as much as possible in behavior. The dyad's behavior was observed and coded via Wahler's Systematic Observation Codes - Revision IIb (SOC-IIb; Cerezo, Keesler, Wahler, & Dunn, 1986). The set of behaviors was analyzed to determine if any clusters/factors exist and the degree to which they are intercorrelated. The clusters would not be labeled with any summary term which may increase the literary-semantic bias. Rather, if a factor was identified, it would be referred to by its component behaviors. This may seem somewhat picky; however, every level of inference used to develop a new meta level label increases the chance of assumptive error as pointed out earlier in the ideas of construct and judgment bias. Although this method, roughly sketched here, attempts to make any cluster found as behavioral as possible, there will be some degree of semantic influence. This influence

will likely come at two levels. First, in the development of the coding system, summary terms describing behavior had to be used so that coders would reliably identify the same behaviors. Second, although reliability has been found to be moderately adequate with this system (Kappas for code composites are between .48 and .70), there likely will be some judgment and construct bias on the part of the actual observers, but these will be held to a minimum by the strictness of the code. Cerezo et al (1986), in the development of this coding system, have established the codes so that they are mutually exclusive. Therefore, any intercorrelations among the observed behaviors can be interpreted as a function of the observed individual and not an artifact of the coding system. What really separates this way of describing traits from the ways used previously is the fact that we are not setting out to discover any one "trait" but simply trying to describe behavior that is less than state like. In this way, we hope to eliminate as much of the literary-semantic, construct, and judgment biases that have been problematic in previous research.

To get to the point where one can even look to see if factors exist, two methodological issues must be addressed: 1) The choice of statistical model and 2) The effects of aggregated time intervals.

When Wahler (1975) set out to define response clusters in children's behaviors, he used Johnson's hierarchical

clustering technique (Johnson, 1967). He argued this technique provided "formal decision rules in reducing correlation matrices to a series of clusters. In essence, the extracted clusters were considered as possible response classes within the subjects' behavior repertoire" (Wahler, 1975, p. 31). Cluster analysis has the same goal as factor analysis: To summarize multiple variables with a few constructs. Whereas the goals are equivalent, cluster analysis has few unequivocal indices of the adequacy of the sample or the clusters developed. Therefore, it is difficult to discern whether the clusters were discovered or created by the statistical technique (Lambert & Azevedo, 1987). Factor analysis, however, has several indicators for the adequacy of sample and factors. Dziuban and Shirkey (1974) developed explicit rules for assessing the fit of a correlation matrix to the factor analytic model. Accordingly, a correlation matrix should have a high Kaiser-Myer-Olkin measure of sampling adequacy, a highly significant Bartlett's test of sphericity (a measure of the number of significant correlations within the matrix), high anti-image diagonal correlations, and large eigen values which indicate the degree of success of the variable reduction. The newer technique of hierarchical cluster analysis has not yet established comparable measures of the compatibility of the data to the statistical model (Everitt, 1980). It is for this reason factor analysis was chosen

over hierarchical cluster analysis. These measures of fitness were especially needed given the serial nature of the data; a problem to be discussed momentarily.

Since the above mentioned measures of adequacy will be used extensively in the discussion of the results, some brief definitions may be useful. "The Kaiser-Myer-Olkin (KMO) measure of sampling adequacy is an index for comparing the magnitudes of the observed correlation coefficients to the magnitude of the partial correlation coefficients" (Norusis, 1985, p. 129). The larger the KMO, the better the correlation matrix supports the assumptions of factor analysis. Kaiser (1974) indicates 0.5 as the absolute cut off score. Below that mark, the results are unacceptable. Although scores between 0.5 and 0.7 are adequate, the desired scores are above 0.7.

Bartlett's test of sphericity is a measure of the correlation matrix as an identity matrix. Given that an identity matrix is one in which the diagonals equal 1 and the off diagonals equal 0, the desired outcome is a matrix where the off diagonals are significantly different from 0. Such a situation will result in a large test statistic for sphericity and an associated small significance level (Norusis, 1985). Since the required significance level with this large a matrix is 0.01 and for ease of graphing, this variable was referred to as "1 - Bartlett's Sig".

Another indicator of the relationship strength among variables is the partial correlation coefficient. Given the existence of common factors in the matrix and the elimination of the linear effects of other variables, the partial correlation coefficients should be small. The anti-image correlation is the negative of the partial correlation coefficient. The diagonal of the anti-image correlation matrix should have a large proportion of correlations above 0.50 while the vast majority of correlations off the diagonal should be close to zero (Norusis, 1985). In the subsequent discussion, this measure will be referred to as "% of AIC Diagonals" and to be adequate, should be above 45% (.45 on the graphs) of the number of variables considered for each subject. The number of variables used will differ from mother-child dyad to mother-child dyad since each dyad did not engage in all possible behaviors nor did they necessarily engage in the same set of behaviors.

The strength of the factor and a measure of success for the analysis is the eigen value. The eigen value in factor analysis is the total variance explained by each factor in terms of the number of variables for which the factor can account. For example, in a correlation matrix of 14 variables where each variable accounted for an equal part of the variance (each variable = 7.14% of the variance), then a factor having an eigen value of 5.7 would account for 40.7%

of the variance or as much as 5.7 of the variables ($5.7 \times 7.14 = 40.7$).

Of course, most matrices will have more than one factor and several models have been developed for determining the optimum number of factors to use in a given matrix. The least restrictive model requires that a factor account for at least as much variance as one variable (i.e., the eigen value > 1.0). The problem with that approach is what good is a factor if it accounts for no more variance than one of the variables being used. Cattell (1966), while graphing the eigen values of factor analyses, noted a cliff like break between the large factors and the gentle slope of the remaining factors. He named this phenomena "scree" because of its resemblance to the rubble at the foot of a mountain after a land slide. He suggested the best number of factors to use for a given matrix would be equal to the number of factors still at the top with the dividing line being the point at which the scree begins.

Factor Analysis and Time Series Data

In general, psychology defines the "case" as the subject (the person or animal) being studied. The correlation matrix is comprised of the covariation of variables whose data points were gathered from separate subjects. Therefore, to say, within a correlation matrix of several variables, a correlation of height and weight was

high for a sample of people means one could predict with some degree of certainty, in that sample, an individual of above average height will also be of above average weight (above average being above the grand mean of that sample). Factor analysis, then, would summarize the multiple variables of the subjects being studied into a few factors (Lambert & Azevedo, 1987).

The present study, however, consists of time series data in which one individual is observed and measured multiple times over some fixed period. Thus, in time series, "cases" are not subjects, but time intervals. This presents the problem of independence between the "cases" required by the factor analytic algorithm. In this data set, there is likely to be some amount of serial dependence. Serial dependency is the idea that a response at time t is partly determined by the response at time $t-1$, $t-2$, and so on. To attempt to escape this problem, the data were statistically analyzed using lagged autocorrelations. This is a common Pearson correlation of the time series data of one of the variables against a lagged version of itself. Norusis (1985) defines lag n of a variable as "the value of the variable n cases before." In this study, a lagged correlation using LAG(MA,4) would produce a correlation matrix comparing the current case of Mother Approach Neutral with the fourth case before it. From this matrix, a "correlogram" can be generated giving a picture of that

behavior's time series serial dependency. The point at which the autocorrelation function drops below the level of significance would show the time interval required for statistical independence. To study this problem on every behavior present in each dyad would require slightly more than 800 computer programs. A random sample of 50 autocorrelations (5 behaviors from each mother) were conducted as a test of the extent of serial dependence among the variables. The autocorrelations were conducted out to 7.5 minutes, the limit of the hardware being used.

However, since behaviors are not usually instantaneous reactions between people, the time window used becomes crucial not only for serial dependence, but also in terms of what behaviors make sense in a correlation. Many correlations may be lost if one uses too narrow a time window. For instance, if one were studying the knee jerk response (where a physician, when testing reflexes, strikes the tendon below the knee cap and the leg jerks) and used a time interval of 0.001 seconds, one would find no correlation between being struck by the hammer and the jerking of the leg. However, if the time interval is widened to 1.0 second, the correlation is discovered. Conversely, if too wide a time window is used, say one hour, then too many behaviors may have occurred so as to obscure the correlation in static. Concomitantly, spurious correlations may be found (e.g., noting a high correlation

between nurses entering the examination room and the knee jerk response).

In this study, the eight hours of behavior collected over eight separate sessions were coded in fifteen second intervals as per SOC IIb convention. This presented two initial problems. The first was how does one, or even can one, study all eight sessions in one coherent package. Secondly, given there are 2560 possible time windows to study in an eight hour sample, how many time windows and which ones should be explored.

The first problem was difficult to address completely in this study. Given the one hour sessions were of insufficient density (too few total behaviors with which to develop a useful correlation matrix), it was necessary to take the eight sessions together for analysis. However, there existed the possibility of the lagged effects becoming distorted around the joints of the concatenated sessions. Each one hour session occurred several days apart. In lumping them together, the assumption is that each session is more alike than it is different and can therefore be taken as a whole. Further analysis in future studies will need to address specifically the extent of any distortion. cursory investigation here suggests the distortion is minimal since there seems to be some consistency in the range of behaviors emitted across sessions and preliminary inspection of factor scores suggest some stability. This

was suggested by the finding that the primary factor found in any given mother tended to be made up primarily by the same behaviors in both mother and child across time windows. That is, if behaviors A, B, C, D, and E were found in the primary factor at the first time window, at least three out of the five would show up in the primary factor in the subsequent time windows. This was a somewhat interesting finding; however, the weakness of the matrices really prevents any further interpretation of the factors generated by the analysis.

The second problem was how many windows to examine with factor analysis. This is a parallel problem to finding a window without serial dependence. Given the need to find an appropriate time interval where behavioral correlations were neither lost nor obscured, the fifteen second intervals were lagged to study 40 time windows between 15 second intervals to 10 minute intervals.

There was little theoretical basis for the forty windows chosen; the rationale was simply one of studying aggregates of fifteen seconds up to the limit of the computer hardware being used and the amount of data being used. This resulted in forty windows from 15 seconds to 10 minutes in increments of 15 seconds. It was also felt, as mentioned earlier, the narrower the time interval, the more direct an index of what took place is represented. Therefore the lagging began with the smallest time unit and moved up incrementally from there to the limit of the hardware, the data, and the researcher.

CHAPTER 2

METHODOLOGY

Subjects

Subjects were the archival records of 10 families who had been treated or were being treated at the Child Behavior Institute for conduct problems with their children. Each of the 10 families met the socioeconomic criteria that Wahler, Leske, and Rogers (1979) found associated with parent insularity. To qualify for inclusion in the study, each family must have had at least the mother in the household, a coercive child (mean age = 8.4) and completed the baseline phase of treatment including all testing and at least eight home observations. Home observations were done by undergraduates trained in the SOC-IIb system developed by Cerezo and colleagues (1986). The five mothers considered to be poor trackers also scored high on the Beck Depression Inventory ($\bar{X} = 20.6$) and can be considered in the clinically depressed range. Those considered to be good trackers scored lower on the Beck ($\bar{X} = 9.4$) below the cut off score for depression.

Procedure

From the eight protocols collected on each subject, mother and child responses were recorded for each 15 second

interval of observation. This yielded approximately 1920 intervals per subject; each of which could contain any combination of 13 possible maternal responses and/or 15 possible child responses. The initial interval of 15 seconds was chosen as the base time interval simply because it was the standard observation interval for the SOC-IIIb. It is also the shortest most direct index of what took place that is reliable according to Cerezo et al (1986). If either a maternal or a child response occurred more than once in any interval it was only counted once. The differentiation made was one of presence or absence within an interval, not degree to which a response was used. The reason for this differentiation had to do with the reliability of the coding system at this microlevel of analysis (Cerezo et al, 1986). From this matrix, behavior codes which did not occur were dropped and a factor analysis was performed to determine if the 28 possible maternal and child responses could be reduced to any response class or classes. In a parallel analysis, lagged autocorrelations were computed for a sample of behaviors left within each mother-child dyad to determine the extent of any possible serial dependencies. Lagged versions of a given behavior were created at all lags from 1 to 30 (7.5 minutes), the limit of the computer being used. Then ordinary Pearson correlations were computed among the lagged versions of a given behavior.

CHAPTER 3

RESULTS

It appears there are three basic questions to be answered here: 1) At what time interval(s), if any, is serial dependency not an issue, 2) At any time interval were the correlation matrices sufficiently robust for factor analysis, and 3) Were there any differences in matrix quality between mothers who were good or bad trackers.

To conduct autocorrelations on each behavior included in each factor analytic matrix for each mother-child dyad would require slightly more than 800 computer programs. A random sampling of 50 were done and they showed serial dependency to be a somewhat mercurial problem. One would hope the autocorrelation of the behavior would taper off, eventually, to a nonsignificant correlation coefficient and therefore give you a length of time at which the aggregated intervals would be independent of one another. The hypothesis suggesting this eventual independence stems from the idea your own earlier behavior would be less of a determinant of your current behavior than the current environment. Therefore, there should be less of a correlation between your current behavior and your behavior at some point in the recent past. However, some behaviors had a correlation coefficient that never appeared to

dissipate through to the last computed lag of seven and a half minutes. There was always some significant correlation between a behavior and its lagged version. The autocorrelations of some behaviors showed independence with a lag of only fifteen to thirty seconds. The autocorrelation of other behaviors seemed to cycle between being independent for some period then showing a slight to moderate autocorrelation. As noted earlier, lagging the variable means, in this study, comparing it to itself some set time before its current time value. Therefore a correlation at lag = 0 is $r = 1.0$ by definition since this is a correlation of something with itself. At lag = 1, the correlation is the variable with itself 15 seconds ago; at lag = 30, it is the behavior correlated with itself 7.5 minutes ago. For this series, $r = 0.075$ is nominally significant at 5% on a two tailed test.

Figure 1 shows a sample of four correlograms illustrating the four patterns suggested by eyeing the 50 autocorrelations conducted. It is included here only because it did appear these four patterns seemed to be representative of the autocorrelations conducted. The correlogram in the upper left corner of the figure demonstrates the pattern hoped for on all the behaviors. The coefficient tapers off below significance at lag 12 or 4 minutes, thus independence can be assumed at intervals of 4 minutes. The second correlogram (lower left) is an example

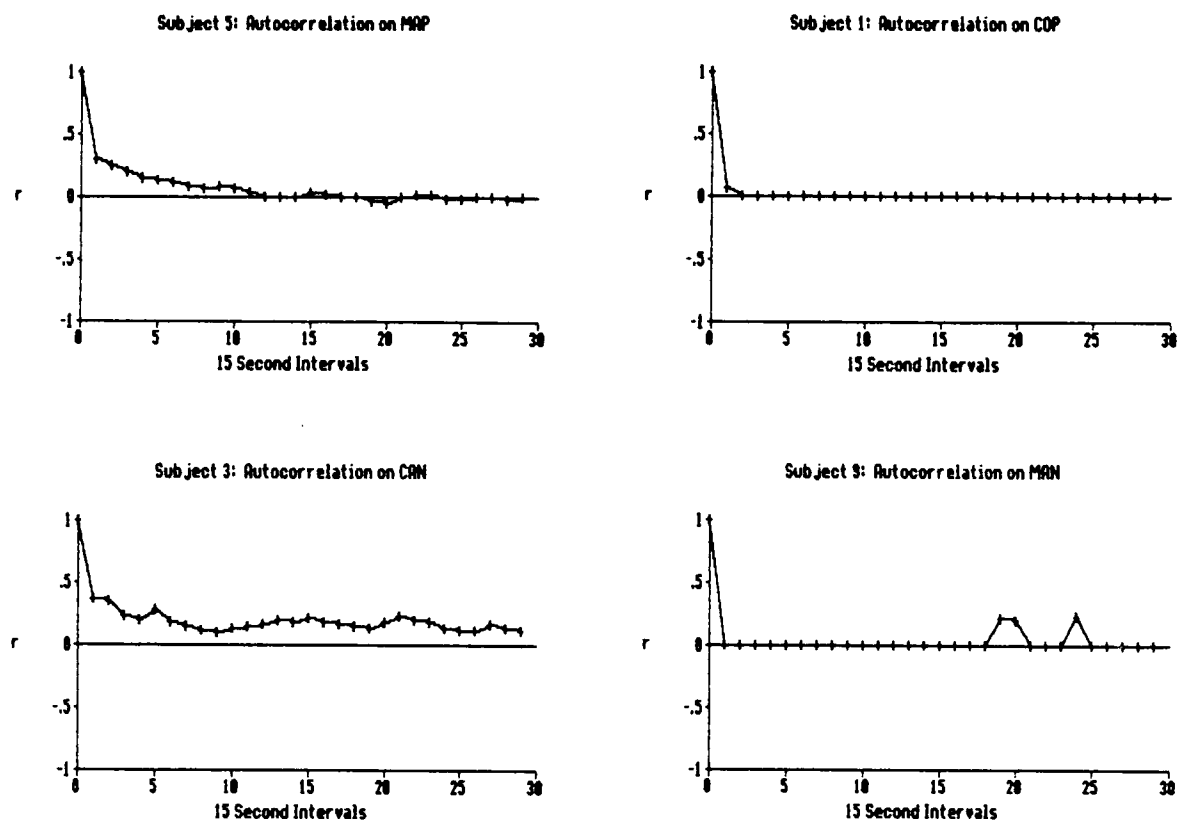


Figure 1. Examples of the four correlogram types found in the 50 autocorrelations sampled. Figure shows correlation coefficient for the behavior sampled at each lagged interval. Lagged intervals are labeled as total time rather than number of 15 second intervals intervening.

of an autocorrelation that never seems to dissipate. The correlation coefficient is significant out to the 7.5 minute limit. This is problematic since it suggests the behavior is, to some degree, dependent on the behavior occurring at some previous point in time. The correlogram in the upper right corner showed an autocorrelation which dissipated rapidly. The correlation at 15 seconds is not significant ($r = .055$, $\alpha > .05$). Serial dependence with this variable would not be a problem at all. The last correlogram shown, on the lower right, shows a curious effect in some of the autocorrelations. The coefficient drops to zero quickly but becomes significant again at 4.75 minutes and 6 minutes. One might hypothesize this behavior occurs for this mother in "bouts" like in boxing. It is possible, were we to carry the autocorrelations out to 30 minutes, these little spikes of behavior might show a 5 to 6 minute cyclic pattern.

In an attempt to graphically represent a composite of the autocorrelations, Figure 2 shows the mean autocorrelation coefficient for all fifty of the behaviors sampled at each lagged interval. As mentioned earlier, the first autocorrelation will always be 1 since it is the correlation of a behavior with itself at the same time interval. Although the mean coefficient does drop over time, it is a significant correlation at all intervals except one. It becomes just barely non-significant at the 6 minute mark (Mean $r = 0.0744$, $\alpha = 0.049$, standard

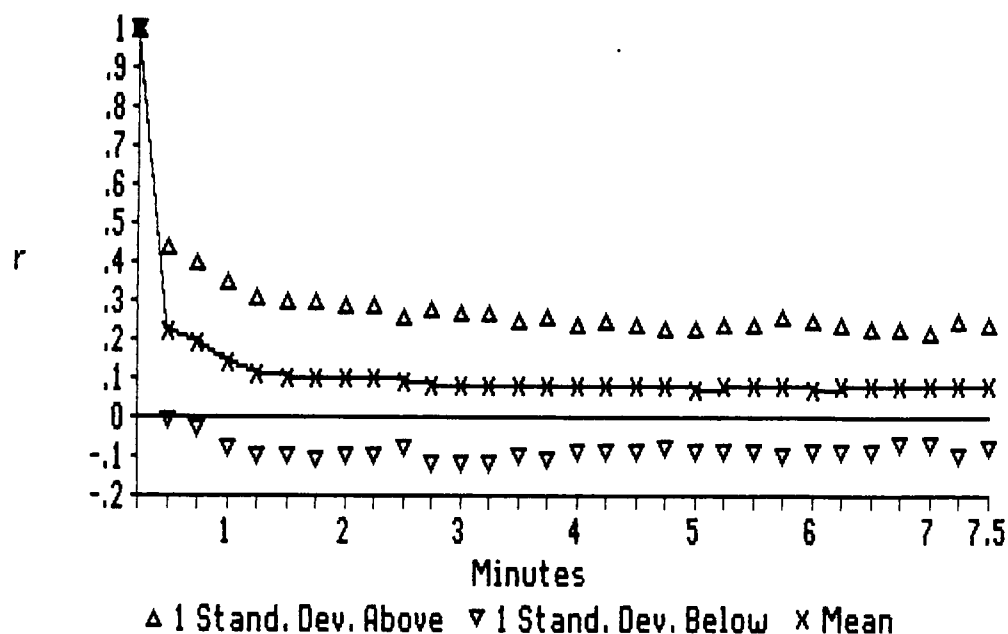


Figure 2. Correlogram of mean correlation coefficients for each lagged interval. Figure also shows one standard deviation above and below these means. Lagged intervals are labeled as total time rather than number of 15 second intervals intervening.

deviation = 0.169). The overall graph of the mean scores resembles correlogram 2 shown in Figure 1, the one whose correlation coefficient never seems to dissipate. This would suggest there is no time window across all of the mothers where serial dependency is not a problem using this method of detection and control. However, closer examination of the graph shows the correlations from about 3 minutes out to 7.5 minutes stay about the same between a correlation of 0.08 to 0.10. This, as noted earlier, is just over the mark for significance. It is possible serial dependency is not playing as large a role as currently thought; however, a more detailed examination is needed before a conclusion can be reached. Thus, the results of the autocorrelations did not clearly demonstrate one single window at which all the dyads could be compared satisfactorily.

Although not a perfect time interval for all behaviors nor all dyads, the lag of six minutes seemed to be one that could serve as a good comparison point between the mothers at this point. Where this lagged interval comes close to satisfying the requirements of serial dependency, unfortunately, none of the matrices at this point met factor analytic quality assurance measures mentioned earlier. Given the primary reason for using factor analysis was to have these criteria for legitimate factors, it makes little sense to describe the ten factors found at the six minute

lag which Norusis (1983) argues are artifacts of the mathematical model and hence have no validity. However, it is important to note what kind of matrices were developed at a time lag that mostly closely satisfied the problem of serial dependence. Therefore, what will be stressed is the quality measures of the factor analytic correlation matrix for each mother-child dyad at the six minute window.

To recap, briefly, what was intended by the concept of good and bad tracking mothers as stated earlier, good tracking simply means the mother was considered to be observant of her child's behavior and respond appropriately. Bad tracking refers to a mother who was considered to respond to her child in a way that bears little or no discernible relationship to the child's behavior. These categorizations were determined by score on the indiscriminate index developed by Wahler (1986) and mentioned earlier.

The basic factor analytic hypothesis for good tracking mothers was they will either produce a factor showing a consistent relationship between parent and child behaviors if the range of behaviors was limited or, more likely the range of behaviors would have been so wide, the density of any one set of behaviors would have been insufficient to develop a factor. The basic factor analytic hypothesis for bad tracking mothers was the production of a factor

predominated by maternal behaviors or as an inconsistent and diffuse relationship between parent and child behaviors.

As can be seen in table 1, there is little difference between the two groups in terms of the matrix quality of each respective subject. The KMO's are all below the minimum requirement of .50 which indicates the matrix is not well suited to factor reduction. Since the KMO is the primary measure of matrix suitability, a Spearman rank order correlation was conducted between rank on the indiscriminate index and rank on the KMO. Curiously, the correlation was very low (Spearman $r = 0.12$, $\alpha > .05$, two tailed). At first blush, this suggests a paradox since it was expected the two groups would differ in quality for factor analysis. However, given the problems already noted, it is most likely this is an artifact of a too inclusive variable choice paradigm. In this study, any behavior, even with a frequency count of 1 over the eight hours of observation, was included. It is likely with more refined variable choice rules like the ones outlined in the discussion section, the quality of the matrices will differ more.

Also shown in table 1, Bartlett's test of sphericity suggests that all the matrices are not identity matrices and have a significant correlation. It was hoped the percentage of anti-image correlation diagonals would be above 45% and as can be seen, only one of the subjects in the bad tracking group barely surpasses that mark. Interestingly enough,

Table 1

Comparison of indiscriminate index scores and factor analysis matrix quality scores for good versus bad tracking mothers at a lag of 6 minutes.

Tracking Group	Sub. #	Indiscriminate Index Score	KMO	1-Bartlett's Sig.	% AIC > .50	Eigen Value
Good	1	6.09	.34	.99	25	3.25
	5	1.44	.46	.99	22	2.65
	6	5.71	.40	.99	23	4.02
	7	16.16	.39	.99	27	3.70
	8	11.89	.41	.99	35	4.59
Bad	2	23.28	.48	.99	48	3.55
	3	20.75	.38	.99	23	5.29
	4	22.52	.36	.99	19	3.42
	9	29.51	.38	.99	32	3.46
	10	31.91	.47	.99	30	4.24

even though none of the matrices satisfy the requirements, the eigen values, a measure of the strength of the factors, are relatively high. If one were to go on the eigen value alone, one might draw many conclusions about the relationships between the behaviors comprising this factor when, indeed, the factor is no more real than a moon shadow. It is possible, however, with the data filtering suggestions made in the discussion, these matrices could be improved to fit the factor analytic model more closely. Interestingly enough, neither of the two dyads whose matrices came closest to satisfying the standards of factor analysis did so at six minutes. A short discussion of the two mothers whose matrices were satisfactory at these other time intervals may be enlightening.

A More Detailed Look at Factor Analysis

Two of the mothers, one a good tracker the other a bad tracker, developed matrices which met the minimum suitability criteria for factor analysis. Although the problem of serial dependency was a nagging question at the time intervals used, a brief analysis may serve as a comparison for more refined future studies. On the following figures, the X-axis begins with the originally observed fifteen second interval and ends with the aggregated 10 minute sections of the observation session. In order to avoid confusion, the X-axis was labeled in terms

of minutes instead of the lags mentioned throughout the paper. Each hash mark between minutes indicated an additional fifteen second interval has been added to the lagged unit correlated. When the symbol for "1 - Bartlett's Sig" is missing, it meant the value was mathematically undefined and the correlation matrix at that time interval cannot be used for factor analysis.

Subject 2 was a mother considered to be a bad tracker of her child's behavior and had an indiscriminate score of 23.28. In Figure 3, the data most closely approximate a matrix suitable for factor analysis at the 5.5 minute window. The KMO value is 0.5; 1-Bartlett's Sig. is 0.99 (Bartlett's Significance = 0.00001), and the % AIC Diagonals > .50 is 52%. Granted, these data only minimally meet the requirements, but it is interesting to note that at some window, they do meet the standards. The lower graph on figure 3 shows the eigen value of the strongest factor superimposed on the adequacy measures. At 5.5 minutes, the eigen value is 3.68 which means this factor accounts for as much of the variance as somewhat more than 3 of the primary variables. In this case, 25 variables were considered in the correlation matrix. Each variable alone would account for 4% of the variance which means Factor 1 could be used to account for about 14% of the variance. This is not a large slice, but a beginning.

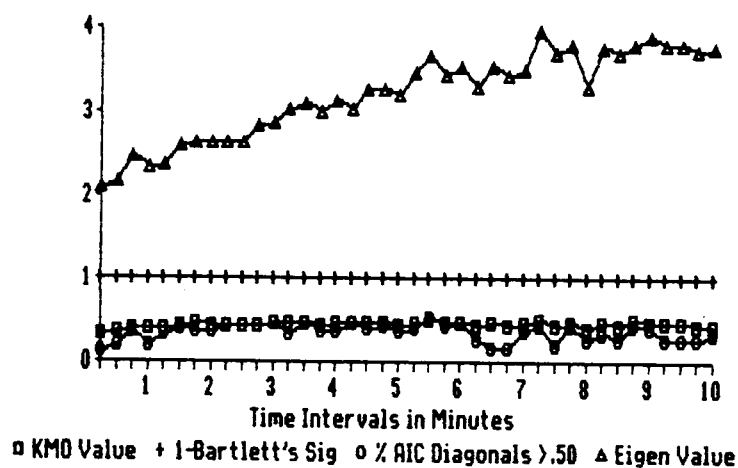
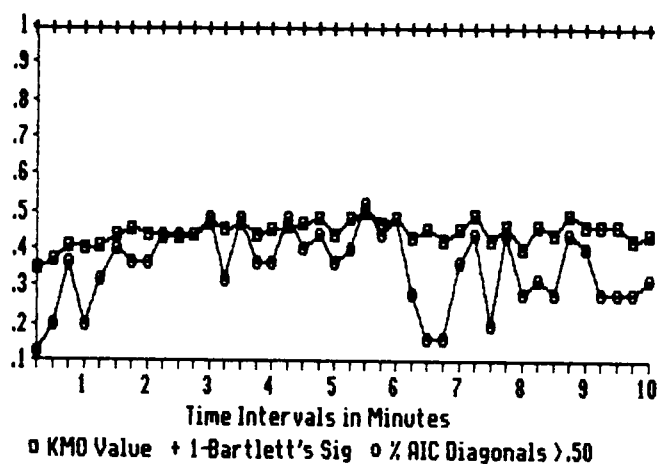


Figure 3. Top graph is the factor analytic matrix sampling adequacy measures across all 40 lagged intervals for subject 2, a bad tracking mother. Lagged intervals are labeled as total time rather than number of aggregated 15 second intervals. Bottom graph is the adequacy measures with eigen values superimposed.

The behavioral composition and associated factor loadings for the primary factor are shown in table 2. If a given observation session of subject 2 were high on factor 1, this would suggest the dyad is engaging in a series of behaviors in which the child is persistently giving instructions of various affective valences and the mother is primarily resisting the instructions, but occasionally, she complies. This particular set of data, suggests, then, that 14% of the time this dyad spends together may be taken up by the child attempting to instruct or request things from mother. In response, the mother is most likely to respond appropriately and oppose such instructions or requests.

Even though this mother was considered to be a poor tracker of her child's behavior, the primary factor discovered suggests she indeed is tracking and responding consistently to her child. Indeed, the most likely response, either positive or neutral opposition, to the child's instructions or demands is what would be expected from a highly functional mother, given the demands are unreasonable. This hypothesis is contraindicated by her indiscriminate index, a paradox. Another interpretation would be similar to Oliver Twist's plight in the Dickens novel where he requests more food only to be denied by an adult whose only set of responses is to deny whether positively, neutrally, or negatively. If opposition is this mother's only response set, then this apparent consistency

Table 2

Behavioral composition of primary factor found at Lag 22 (5.5 minutes) for Subject 2 whose dyad had a bad tracking mother. Primary factor's eigen value = 3.68.

Behavior	Factor Loading
Child Instruction Positive	0.89
Child Instruction Negative	0.80
Mother Opposition Neutral	0.78
Mother Opposition Positive	0.74
Mother Compliance Neutral	0.58
Child Instruction Neutral	0.41

may only be a screen for a trait like position. This suggests an alternative hypothesis that the child has adapted to a mother who tracks poorly by engaging in multiple instructions or demands of various valences and perhaps increasing frequency until the mother finally tunes in and complies. The apparent consistency being picked up may be driven by the child's need for certainty ala Wahler's (1980a) uncertainty hypothesis alluded to earlier. The opposite is, of course, also possible. The mother may be tracking well and opposing appropriately only to be worn down to complying.

It is also important to note the observation window had to be expanded to 5.5 minutes to discover a significant correlation matrix. This might suggest long delays between one actors behavior and the subsequent response. In turn, this suggests the possibility of slow, poor tracking on the mother's part. It is the mother who must be tracking since her behaviors are a reaction to the actions of the child (one cannot oppose without being instructed or commanded) and it takes a window of 5.5 minutes to discover the relationship with even minimal assurance (i.e., the measures of matrix quality).

Given the weakness of this correlation matrix, only this first factor could be accurately analyzed. However, if the matrix could be strengthened so as to allow for the interpretation of other factors, it is possible one could

tease out which interpretation fits better with the overall picture. However, this is sufficient to demonstrate how the factor could be analyzed and some of the considerations of the behaviors present.

Subject 8 had an indiscriminate index score of 11.89 and was considered to be a mother who tracks well. Shown in Figure 4, Bartlett's test of sphericity is calculable and significant only to 7.75 minutes beyond which the data are meaningless for factor analysis. Interestingly enough, at the 3 minute window, the data fit the factor analytic model better than any set of data at any time window studied. The hypothesis under consideration would suggest the factor would consist of both mother and child behaviors and there would be a strong consistent connection between the behaviors. At 3 minutes, the KMO is 0.55 which is minimally accepted by Kaiser (1974). The % of AIC Diagonals $> .50$ is 69%, significantly greater than the required 45%. The eigen value at 3 minutes, also shown in Figure 4, is 4.04. In this case, 26 variables were considered in the correlation matrix. Each variable alone would account for approximately 3.84% of the variance which means this factor could be used to account for about 15.5% of the variance.

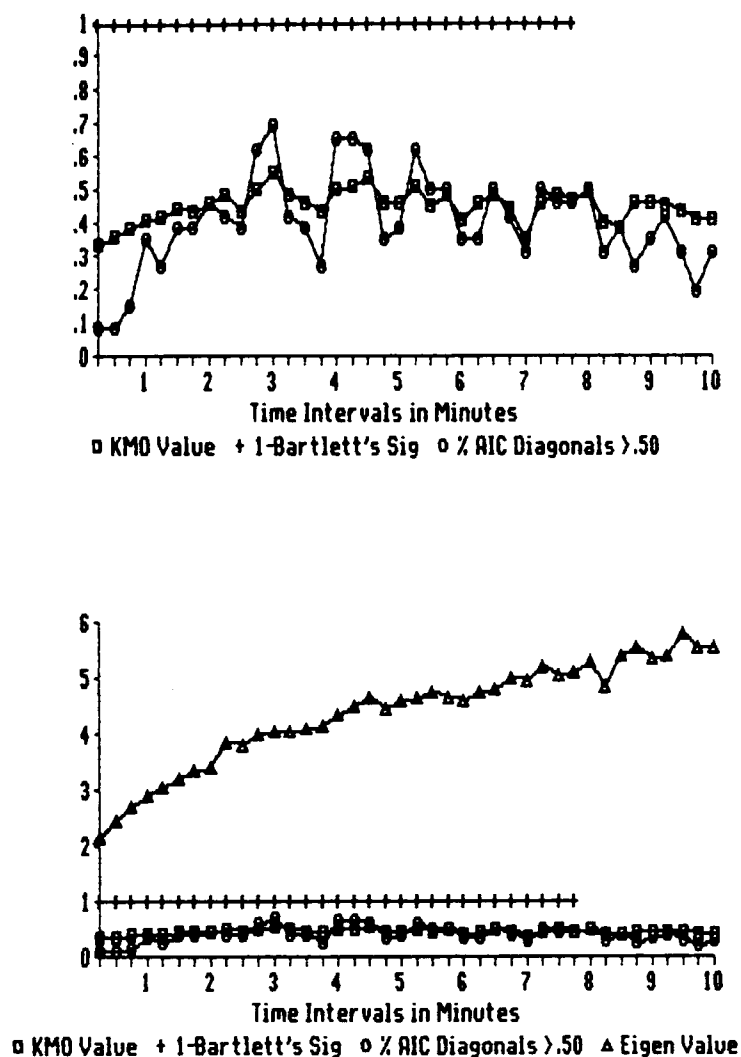


Figure 4. Factor analytic matrix sampling adequacy measures across all 40 lagged intervals for subject 8, a good tracking mother. Lagged intervals are labeled as total time rather than number of aggregated 15 second intervals. Bottom graph is the adequacy measures with eigen values superimposed.

Table 3 lists the behaviors comprising the primary factor for subject 8 and their respective factor loadings. As opposed to the factor found in subject 2, this factor was revealed at a window of 3 minutes. The shorter time may suggest a more vigilant posture on the mother's part to her child's behavior or a more rapid response from the child since in this case the mother is the instigator of the exchanges. Serial dependence may be a confounding variable at this small a window, but its contribution is not clear at this time. Two of the variables, Child Compliance Neutral and Mother Instruction Negative, were in the five sampled from this dyad for autocorrelation. Child Compliance Neutral had an autocorrelation whose significance dropped to zero with a time lag of only 30 seconds. However, Mother Instruction Negative showed the cyclic pattern illustrated in the lower right corner of Figure 1.

An interpretation of this factor suggests that for any given observation session high on factor 1 for this dyad, the mother will be attempting to instruct at times coercively. The child is most likely to oppose, but may in fact comply and that compliance may take the form of work. One important thing to note is the limited range of maternal behavior, essentially, only instruction, and that in this behavioral group, the mother is the actor while the child is the reactor. In the bad tracking mother, the child was the actor and the mother the reactor. If this relationship

Table 3

Behavioral composition of primary factor found at Lag 12 (3.0 minutes) for Subject 8 whose dyad had a good tracking mother. Primary factor's eigen value = 4.04.

Behavior	Factor Loading
Child Opposition Neutral	0.87
Mother Instruction Neutral	0.81
Child Compliance Neutral	0.79
Mother Instruction Negative	0.58
Child Work	0.56
Child Compliance Negative	0.52

holds up across a more robust sample, it may tell us something of the coercion traps in which these mothers find themselves.

The combination of the shorter time span required to find significant correlations and the more neutral stance of the dyad comprising subject 8 may suggest a more accurately tracking and responding mother as would be expected by her indiscriminate score. The longer time span required of the dyad comprising subject 2 may indicate a more idiosyncratic response style less attuned to the moment to moment behavior of parent and child and perhaps the child took charge of the interaction creating the appearance of consistency. Again, it should be stressed the exercise of interpreting these factors is more a demonstration of how the factors might be interpreted. Since the question of serial dependence is still out, neither correlation matrix was particularly strong and they only barely met the minimum criteria for suitability, these interpretations are tentative and speculative at best. Refinements of this rudimentary methodology, as enumerated below, applied to this data and to additional data currently being gathered undoubtedly will yield more robust matrices, stronger factors, an answer to serial dependency problems, and, hence, greater reliability in interpretation.

CHAPTER 4

DISCUSSION

This study set out to do three things: 1) Choose subjects whose characteristics are consistent with a theoretical model of traits, 2) Base the search for traits on observed behavior, and, mainly, 3) Begin the development of a suitable methodology. The first two seem to have been successfully met through the choice of dyads with mothers who fit criteria for depression and the use of a behavioral observation method, the SOC-IIb. The third, and undoubtedly most difficult, was, at least, initiated.

Although the factor analysis did not work as cleanly as expected, this study has made strides in establishing a workable protocol for the analysis of traits from behavioral data. The weakness of the matrices prevents intelligent speculation about the nature of the interactions between parent and child. However, as mentioned earlier, a post hoc review of the matrices and their weaknesses suggested several techniques for the improvement of the correlation matrix. One of the main problems was the sparseness of the data. Many of the behaviors would occur only a handful of times over the eight hours of observation while others occurred much more frequently. Unfortunately, this was not considered in this analysis and any behavior of any

frequency was included in the matrix. The only behaviors not included in any given matrix were those that did not occur at all. Lambert and Azevedo (1988), in research paralleling this dissertation, suggest an extra step in the analysis. This step would be a multivariate analysis of variance (MANOVA) to determine which variable's grand means differ significantly from zero. They showed that several of the variables occurred so infrequently, it was quite possible their true rate was zero plus random error. Using this technique to screen out non-productive variables will reduce and strengthen the matrix. Those variables may even have shown themselves in the autocorrelations as those variables whose correlation coefficients dropped to zero rapidly and remained there.

Another way of dealing with sparse variables that may focus the search for any underlying traits is to collapse the valence of the variables. In the SOC-IIlb coding system, all of the interactional behaviors are scored according to behavior and affective flavoring. Therefore, not only is there social approach by mother or child, but also positive affect associated with the social approach, or perhaps negative affect, or a neutral affect. The basic behavior is the same, though: Social approach. So the coding system breaks down one basic behavior into three categories. Instead of looking, initially, at all valences of a behavior, first run the factor analysis on the

summation of the behaviors across positive, neutral, and negative. If there is a factor suggesting a relationship between two or three of the behaviors, rerun the factor analysis on those behaviors with the valences expanded. One would still exclude behaviors that did not occur and those whose grand mean is not significantly different from zero. This approach would not replace the factor analysis of all variables appropriate for it, but would be an additional step to tease out, perhaps, a more detail sketch of the behavioral relationships.

The problem of serial dependency is a much more difficult one. Continued exploration of autocorrelation and a more detailed understanding of its implications may shed some light. Perhaps extending the time span for the autocorrelations will allow for a reduction in the problem. However, the jury is still out on this question.

Another pressing question is how to assess maternal traits while using dyadic data. Two answers suggest themselves immediately. First, reconstruct this piece of research and seek stronger matrices which may show a natural difference between the two groups. Hence, a way of teasing out the maternal contribution might become evident. For instance, if all the mothers follow the same pattern as the two described in detail, then we may find a difference between the groups in who is actor and who is reactor. Second, an interesting way of teasing out the maternal

contribution would be to conduct the same behavioral observations of the mother and other people such as a social services person, a maternal friend, and a maternal relative. After attempting to reduce those matrices, it would be interesting to see if bad tracking mothers remain reactors across situations and good mothers, actors. In this way, we would still have dyadic information, but it would address the issue of consistency across different situations. The major problem there would be to control for sparseness in the data since it is likely the situations with other people may feel somewhat contrived. Hence, the behaviors emitted may be of limited range. Perhaps giving each dyad a task for each behavioral observation session would increase the density of the data which, in turn, would strengthen the matrices perhaps solving two problems at once.

The important issue, however, is that in order to keep the exploration of traits at a behavioral level, one will have to put the subject in a situation in which they will have to behave. Further, since humans are social beings, the best milieu in which to observe them will be when they are interacting with others. Since there are already so many extraneous variables, a somewhat controlled naturalistic study seems appropriate. The idea of a trait being within an individual without an interpersonal manifestation seems very sterile and perhaps has been one of the problems with early questionnaire and observation of

solitary behavior oriented studies. The idea of trying to define a trait is difficult because it implies an internal structure, but it is a structure that has no constant presence. Its presence is only detectable in the individual's interactions with others. Indeed, the traits may manifest itself in the behavior of another toward the identified subject. The identified subject may act in such a way so as to enhance the probability others will act toward them in a specific way. This would be akin to the idea of projective identification, self-fulfilling prophecy, or Kelly's (1955) commonality corollary. It is possible, then, one might never pick out a trait without studying the dyad. However, until this study, there has been little work in the development of a methodology to try to tease out an individual's patterned behavior with others. One might suggest observing the dyad, but only analyzing the identified subject's behaviors; however, then one would lose the information about the other's stimulus class value. The question is, at best, intensely complicated. To begin to evaluate traits behaviorally, a workable methodology must first be developed.

Overall, this research has established some methodological guidelines for the possible study of traits from behavioral data. Even at this crude level of methodological sophistication, two matrices were sufficiently robust to yield factors. The factors found

suggest the possibility of a difference between good tracking and bad tracking mothers along the lines of active versus reactive. The establishment of this difference, however, will have to wait until more robust matrices can be developed. Nonetheless, it is a hopeful sign that perhaps with more refinement, answers may yet be found. Some techniques have been suggested for future attempts at repeated analysis of this and similar data which will undoubtedly produce more significant results. The major problem which lies ahead is serial dependency. However, the work currently being done looks promising and given the stringency of the adequacy criteria for factor analysis, it will not be long before reliable, valid factors will be uncovered which may add new data to the never dying debate about states and traits.

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VITA

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