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AUTHOR	Hollenbeck, Albert R.; Gewirtz, Jacob L.
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ABSTRACT

Two experiments explored the influence of neonate characteristics on maternal behavior. Experiment 1 investigated the influence of maternal attribution to the neonate of certain behavioral characteristics on maternal behavior in interaction. After videotaped mother-neonate pair interaction, neonates were removed for a nominal examination. Mothers were informed only that their neonates had been either alert or not alert in the exam. Mothers were then videotaped in interaction and afterwards informed that the neonate was now in the opposite state. Another interaction was then videotaped. Findings revealed that mothers of normally alert neonates jiggled and rocked their infants' bodies less than when they received minimal instruction. Mothers' pre- and post-delivery medication level was related to several behaviors in complex patterns. Experiment 2 compared maternal response patterns that occurred after mothers were instructed to follow gross themes with their neonates. A total of 52 mother and neonate pairs were observed under three videotape instruction conditions. Under "comfort instruction," mothers held neonates by the torso more, repositioned them less, and jiggled and rocked them more, than under minimal instruction. Race and parity determined maternal behaviors in complex patterns. Findings are discussed in terms of maternal perception of, and attachment to, neonates. (RH)

Attributions About, and Instructions on How to Treat, Their Neonates as Determinants of Mothers' Interaction Behavior

Albert R. Hollenbeck

American Association of Retired Persons

and

Jacob L. Gewirtz

Florida International University

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Abstract

Few data are available on the process whereby infants influence mothers. Two experiments explored how neonate characteristics can influence maternal behavior. Exp. 1 investigated how maternal behavior in interaction can be influenced by an attribution to the neonate of behavioral characteristics. Following a minimal instruction and videotaped mother-neonate pair ($N = 40$) interaction, neonates were removed for a nominal exam. Informed only that their neonates had been "alert,not alert" in that exam, mothers were videotaped in interaction and then informed that the neonate was now in the opposite state ("not alert,alert"), and again videotaped in interaction. Mothers of normally-"alert" neonates, *jiggled* and *rocked* their infants' bodies less than under minimal instruction. Within-subjects, mothers' *pre-* and *post-delivery medication* level related to several behaviors in complex patterns, and maternal *years of schooling* was related to several behaviors. Exp. 2 compared maternal response patterns after instructions to follow different gross interaction themes with their neonates. Mother-neonate pairs ($N = 52$) were observed under three videotape-instruction conditions minimal, to "love your baby," and to "comfort your baby". Under the comfort instruction, mothers *held* neonates more by the *torso*, *repositioned* them less, and *jiggled and rocked* their infant's bodies more, than under minimal instruction. Within-subjects, *race* and *parity* determined maternal behaviors in complex patterns. These findings are discussed in terms of maternal perception of, and attachment to, neonates.

Introduction

Attention has been drawn to gross conditions of contact between neonate and mother in the first hours and days postpartum as possible determinants of diverse maternal behaviors in interaction, including those that denote her attachment to the infant, then and in subsequent periods (Klaus, *et al.*, 1972; deChateau, 1976). Further, the infant's limited repertory of potentially-communicative responses in early life has the potential for influencing systematically (even training) maternal behaviors and thus provide a key to understanding the development of maternal attachment to the infant. Within the mother-child interaction process, these responses can facilitate an understanding of how the infant (a) acquires patterns of communication with its mother,caregiver, (b) learns to control the caregiving environment, and (c) acquires attachments to its caregiving parents (Gewirtz & Boyd, 1977). For these reasons, mother-infant interaction increasingly has become the arena of investigation in early infancy. At present, few hard data are available on the process whereby neonate and infant behavior influence maternal behavior.

Until recently, developmental theorists would have been interested primarily in the different parental behaviors exhibited by mothers and how these behaviors affect infant behaviors under a unidirectional model of mother-child interaction. However, such a model has been seen as too simple to explain the complex interactions that transpire between child and parent (e.g., Bell, 1968; Gewirtz, 1961). There has been an increasing emphasis on the role of the child's behavior in influencing the behavior of its caregivers. In this context, attention is given to conditions of contact between infant and mother in the first postpartum days, to understand the genesis of caregiver-infant interaction.

A number of publications have dealt with the area of social interactions (e.g., Cairns, 1979) and mother-infant interaction in particular (e.g., Gewirtz & Boyd, 1976, 1977; Osofsky & Connors, 1979). Of immediate relevance is work using measures of social interaction as indicators of normal and aberrant human development in early life. For example, studies using social interaction as an outcome measure have explored perinatal-risk and developmental-outcome determinants for preterm and fullterm infants (Bakeman & Brown, 1977), and the effects of different hospital experiences on maternal and infant behavior (Vietze & O'Connor, 1980). Other research has indicated direct evidence that drugs influence the behavior of both mother and neonate immediately post partum and up to one month afterwards (Hollenbeck, Gewirtz, Sebris, & Scanlon, 1984) and that gender cues influence nurse behavior to two-day-old neonates (Hollenbeck, Gewirtz, & Scanlon, in press). Thus, available evidence suggests that social interaction offers a potentially sensitive arena for measuring the functioning and social capacities of the human newborn.

A theoretical model has been proposed under diverse approaches that takes into account the multiple transactions between environmental forces, caregiver characteristics, and infant attributes as continuing and reciprocal contributions to the child's development (e.g., Gewirtz, 1961; Sameroff & Chandler, 1975). In this context, the continuing interactions of the infant with the environment, must be considered. This transactional developmental perspective makes it possible to view adult-child interactions in the larger context of diverse environmental variables and the history of the organism. With specific reference to parent-child interaction, this general model has been applied in several settings (see Vietze & Anderson, 1978, for a review).

In this project, maternal-behavior outcomes in interaction are investigated as a function of several types of benign attributions and instructions that could influence them. The benign manipulations used are directed primarily toward understanding how maternal behavior, in interaction with the infant's, can be influenced by instructional sets effected by labeling, situational factors, demographic factors and, particularly, by the infant's appearance and behavioral characteristics. Neonates were used because they manifest fewer interactive behaviors and are less familiar to their mothers, permitting more effective use of the instructional-set procedure and of an attribution procedure intended to stand for transient behavior characteristics. There exists currently minimal information on whether, and how, the child influences its parents and other caregivers. Therefore, the data collected here could fill a considerable void in our knowledge and, where anomalies are found in these interaction systems, could provide bases for remediation.

This study attempts to explore quasi-experimentally the effects of infant characteristics on maternal interactive behavior. Our working assumption is to let mothers demonstrate behaviorally *their* conception of "love" or "comfort," without imposing a constraining definition. The utility of this strategy is an empirical question. From social psychology, *attribution* theory would lead one to suspect that statements made by authority figures (psychologists in white coats in a hospital) in an appropriate context (the hospital setting following a nominal infant examination) should be sufficiently potent to generate measurable changes in maternal behavior.

Our plan was to explore experimentally how the infant's characteristics can influence the behavior of mothers. The underlying assumption involved is that, in its potential for inflecting the adult's behavior to the neonate, an instructional or labeling set given to a mother about particular neonate characteristics is systematically equivalent to the set the mother receives directly from such characteristics. Specifically, in one experiment particular transient behavioral characteristics were attributed to their neonates and, in a second experiment, mothers were instructed on how to interact with their neonates. In both studies, the resulting maternal behavior was observed in a standard interaction setting. Maternal behavior videotaped in interaction was to be scored subsequently by independent observers, blind to the instructional set given the mothers.

Experiment 1

Parents may be influenced by implicit messages communicated by persons having contact with their baby. Thus, the aim of this experiment was to investigate how mother-neonate interaction can be influenced by an attribution to the infant of particular behavioral characteristics. This was accomplished following a nominal examination of the neonate at which the parent was not present. A staff member remarked to the mother that, during the examination, the neonate appeared (a) "alert", or (b) "not-alert". Our interest was in the maternal behavior patterns in interaction in response to these attributions.

Method

Subjects. Subjects were 40 Caucasian mothers and their neonates recruited from the maternity services of two hospitals in the Washington, D.C., area.

Mean maternal age was 27.6 years (SD = 4.8), mean parity was .7 births (SD = .7, Range 0-2), mean years of education was 13.7 (SD = 2.1), and mean income was $26,300 ($SD$ = $9,800). Of the neonates, 20 were male and 20 female. At birth, mean neonate length was 51.8 cm (SD = .8), mean weight was 3380.0 gms (SD = 331.5). Twenty-four neonates were delivered epidurally and 16 by natural childbirth. All were full-term and healthy with no medical complications at birth or during the hospital stay. Any complications prior to assessment, for either mother or neonate, served as exclusionary criteria.

Design and Procedure. On observation days, project staff entered the nurseries and selected those neonates to be used that day. An optimal observation time was selected based on hospital staff needs and mothers' routines. Mothers were then contacted, the general study purposes explained, and informed consent obtained. Portable videotape equipment was set-up for taping in the mother's room. An assistant went to the nursery and checked that the neonate had been fed, diapered, and was aroused to an alert state. The neonate was brought to its mother in her room.

The induction in this study was used to investigate how mother-neonate interaction can be influenced by an attribution to the baby of particular behavioral characteristics. This was accomplished by a comment to the mother at the conclusion of a nominal examination of the neonate in the mother's absence. Before this exam, neonates had been presented to their mothers, each of whom had been asked to "get to know your baby for a short time" (*Know* condition). Following this 3-min. baseline observation, neonates were removed to another room for the nominal exam. After "exam," the baby was re-presented to the mother. For half the neonates the investigator remarked that during the exam the baby had seemed "alert"; for the other half, the remark was that the baby had seemed "not alert" [*Alert* and *Not-Alert* conditions]. Then the mother was asked to interact with her baby. After 3-mins. of interaction, the investigator stated that now the infant appeared to be "not as alert" or "more alert" (the opposite state of the original attribution based on the nominal exam). Attribution statements were made without considering the baby's actual state. Order of presentation of attributions was counterbalanced, half the mother's receiving each order. Mothers were debriefed by (a) allowing them to ask questions, and (b) explaining the importance of her not discussing any aspect of the study with anyone else in the hospital.

Videotape scoring. Twenty-eight maternal behaviors were scored from the videotapes by trained, independent observers. Table 1 lists the behaviors scored. Behaviors were scored present or not during each of 18 successive 10-sec. intervals, prompted by a tape-recorded audio cue. Observations began when the investigator on the videotape instructed the mother "to start." Observations terminated after 180-secs. This procedure yielded scores ranging from 0 to 18 for every behavior.

Two observers were trained independently to a percentage agreement >80% on each category, using a standard tape of mother-infant interaction. Two observers independently scored 21 of the 40 observation sessions; the remaining 19 sessions were each scored by single observers. Observers were blind as to which 21 tapes were scored twice. Percentage agreement scores for observer pairs for each of the 28 behavior categories ranged from 68 to 100. Overall agreement between independent observers across all behaviors and observations scored for reliability was 88%. Where double scoring of tapes occurred, the mean of the individual scores on each behavior for the two independent observers was used in all subsequent analyses. Six behavior categories were dropped because of low occurrence rates and/or poor observer agreement, leaving 22 categories to be employed in the analyses that follow.

Results

Analysis Plan. The analysis was conceptualized as an Order (2) by Attribution (3) repeated measures Multivariate Analysis of Variance (MANOVA). The MANOVA was used as a screening procedure to minimize the capitalization on chance in studies like this one which have many dependent variables and involve numerous group comparisons. Since this was an initial investigation of variables in this area, the MANOVA *alpha* level was set at $p < .10$

and subsequent univariate comparisons were made using an *alpha* of $p < .05$. Where marginal findings are reported, associated *p* values are given. Thus, when an overall MANOVA,MANCOVA test yielded results on a data subset, the univariate test information for the individual variables of the subset associated with the overall test result will be reported.

Order was a between-subjects factor while *Attribution* was a within-subjects factor. As no *Order* effect was found, the analysis was recomputed collapsing the *Order* factor and adding covariates hypothesized to impact on the study. Covariates were included in a theoretical ordering of most to least impact and tested, in stepwise fashion, for retention or exclusion in further analyses (Cohen & Cohen, 1983): (a) mother's *pre-delivery medication* by type and amount given, (b) *delivery anesthesia*, (c) *post-delivery medication* given to mother (see Hollenbeck, Gewirtz, Sebris, & Scanlon, 1984, for the scoring criteria on these three covariates), (d) mother's *education level* in years, (e) infant *gender*, (f) mother's *parity*, and (g) whether neonate was *bottle* or *breast fed*. The resulting Multivariate Analysis of Covariance (MANCOVA) and its significant findings are presented in Table 2 and will be discussed.

Finally, the residual error-correlation space was examined separately to detect relations for future intensive study among covariates and dependent variables. These findings are reported in the Appendix.

Contrast Main Effects. The *Know* vs. *Alert* contrast was the only MANOVA main effect detected. Examination of univariate analyses yielded one maternal behavior that differed and three that marginally differed. Mothers displayed more *jiggles body,rocks*, *holds,touch*, *pats*, and *looks away* to their infants during the baseline (*Get-to-Know*) observation than during the observation following the attribution that their infant was *Alert*. No differences were detected for the other observed dependent variables for this contrast. No additional MANOVA main effects were detected for the *Know* vs. *Not-Alert* or the *Alert* vs. *Not-Alert* contrasts.

Covariate Effects. In the MANCOVA, three reliable covariate effects were detected and, hence, controlled for in the study design. Under the *Grand Mean*: mothers receiving more *post-delivery medication looked away* ($r = .35$) more from their infants regardless of treatment condition. Under the *Know* vs. *Alert* contrast: the higher the mothers' *years of schooling*, the fewer times they *jiggle the body,rocked* ($r = -.35$), *patted* ($r = -.32$), and *looked away* ($r = -.34$) from and the more often they *held* ($r = .35$) their neonates. Similarly, the more *pre-delivery medication* mothers received, the fewer times they *held* their babies on their *laps* ($r = -.38$). An additional *pre-delivery medication* effect was noted for the *Alert* vs. *Not-Alert* contrast. the more *pre-delivery medication* mothers received, the fewer times they *held* their neonates *in burp position* ($r = -.66$) and *fed* ($r = -.39$) them and the more they *laughed* ($r = .38$), *held,touched* ($r = .35$), and *held in lap* ($r = .37$) their neonates. Finally, for the *Know* vs. *Not-Alert* contrast: mothers receiving more *post-delivery medication* demonstrated fewer *caregiving* behaviors ($r = -.31$).

Discussion

Since each observed covariate appeared under more than one manipulation besides the *Grand Mean*, the interpretations of covariate correlation with dependent variables is not straightforward. Furthermore, it should be recalled here that contrast effects are difference scores which may alter the keying of variables from contast-to-contrast, and thus impacting on the direction of the correlation. For these reasons, and the exploratory nature of the subsequent examination of the residual space, individual interpretations will not be offered. Instead, the impact of specific covariates on clusters of dependent variables will be discussed. All reported correlations are of moderate magnitude in the range from (+/-) .31 to .67.

Parity was related to four dependent variables: *torso*, *talks to*, *shoulders*, and *no contact*. The *no contact* relation with *parity* was observed under the *Grand Mean* and two of three contrasts suggesting a strong relation among parity and no contact independent of design factors. *Pre-delivery* *medication* was related to two positioning variables: *burp position* and *holds in lap* under two design elements. *Delivery anesthesia* was related to two infant position variables: *holds in lap* and *no contact* under two design elements. *Post-delivery medication* was related to five dependent variables: *laughs*, *kiss*, *no contact*, *looks at body*, and *looks away* under two design elements. *breast vs. bottle* feeding was related to *feeds* under one contrast and *caregiving* plus *vocalizing non-vocal sounos* under two design elements. Mothers' *years of schooling* was related to *jiggles body part* under two design elements. Finally, *delivery anesthesia* and *parity* were positively correlated.

When the covariates were controlled statistically it was found that mothers behaved differently to their neonates under the *Get-to-Know-Your-Baby* instruction than under the *Alert* attribution to the infant. Mothers under instruction exhibited more handling of their infants through touching, jiggles body,rocks, and patting; and more looking away than when the state of the infant was attributed. Although jiggles body,rocks was the only maternal behavior to differ at the conventional ($p < .05$) significance level. This implies that the mothers' behavior was inflected more by the demands of the instructional baseline situation than the attributed or actual state of the infant, except for one behavioral domain. Fewer jiggles body,rocks to an *Alert* neonate makes common sense for maternal behavior in this situation and is without need of further interpretation. The marginal results are also consistent with such a common-sense interpretation. When appropriate controls are utilized it is important to note the inflection of even a single maternal behavior in a reliable fashion when mothers respond to the attribution of an infant state characteristic by an authority figure.

Experiment 2

The aim of this experiment was to investigate and compare maternal response patterns when mothers are instructed to follow gross interaction themes with their neonates. Nurturance has been an important variable in descriptions of infant development, yet terms such as "love" and "comfort" are at best idiosyncratic to individual studies and at worst ambiguous as descriptors of adult behavior. We examined nurturance themes in a more direct fashion. The terms "love" and "comfort" were given to mothers as instructional sets on how to interact with her baby. As a within-subject control, mothers were also given an instructional set in which they were simply to "get-to-know" their infant. Our interest was in the maternal behavior patterns to which these terms reduce. Previous observations of adults and neonates have suggested to us that. compared to nonparents, parents respond differently to their own and stranger infants.

Method

Subjects. Subjects were 52 (26 Caucasian and 27 black) mothers and their neonates recruited from the maternity services of a hospital for women. Mean maternal age was 26.9 years (SD = 5.6), mean parity was .7 births (SD = .8, Range 0-3), mean years of education was 14.2 (SD = 2.5), and mean income was $25,400 ($SD$ = $15,800). Twenty-five male and 27 female neonates were used. Mean length at birth was 50.8 cm (SD = 2.5), mean weight was 3238.6 gms (SD = 411.4). Thirty-five neonates were delivered epidurally and 16 by natural childbirth. All neonates were full-term and healthy with no medical complications at birth or during the hospital stay. Any complications prior to assessment, for either mother or infant, were exclusionary criteria.

Design and procedure. On observation days, nurseries were entered and those infants to be used for the day determined by project staff. An optimal observation time was selected based on the routines of the nursing staff, hospital, and mothers. Mothers were then contacted, the general study purposes explained, and informed consent obtained. Portable videotape equipment was set-up for taping in the mother's room. An assistant went to the nursery and checked that the neonate had been fed, diapered, and was aroused to an alert state. The infant was brought to the mother in her room and the investigator stated the following:

"I would like you to interact with your baby. For the first 3-min. period, I will ask you to perform a simple task, for example, to play with your baby. For the second 3-min. period, I will ask you to show some characteristic to your child, for example, to show understanding or sympathy to your baby. The final 3-min. period will be just like the second, but I will ask you to show a different characteristic to your child. Please hold any questions until the end of the session. Feel free to do anytning you normally do with your baby during the taping. Now I am going to ask you to *get to know your baby* [*Know* condition] for 3-mins. Are you ready? You may *start.*" After the 3-min. segment was completed, the investigator said: "For this second period, I would like you to demonstrate *love* [*Love* condition] (or *comfort* [*Comfort* condition]) to your baby for 3-minutes. Are you ready? You may *start.*" After the second 3-min. segment was completed, the investigator said: "For the third and final period, I would like you to demonstrate *comfort* (or *love*) to your baby for 3-minutes. Are you ready? You may *start.*"

Following this observation the investigator explained again the purpose and procedures of the study. Mothers were debriefed by (a) allowing her to ask questions, and (b) explaining the importance of her not discussing any aspect of the study with anyone else in the hospital. Order of presentation of the attributions *love* and *comfort* was counterbalanced.

Videotape scoring. Twenty-eight maternal behaviors were scored from the videotapes by trained observers. Table 1 lists behaviors scored. Behaviors were scored present or not during each of 18 successive 10-sec. intervals prompted by a tape recorded audio cue. Observations began when the investigator on the videotape instructed the mother to "start." Observations terminated after 180-secs. This scoring procedure yielded scores ranging from 0 to 18 for any behavior.

Two observers were trained to a percentage agreement >80% on each category using a training tape. Both observers scored 25 of 52 observation sessions while the remaining 27 were divided randomly between the two observers. Observers were blind as to which tapes were scored twice. Percentage-agreement scores between observers for each behavior category ranged from 69% to 98%. Overall agreement across all behaviors and observations scored for reliability was 96%. Where double scoring of tapes occurred the mean of the individual scores on each behavior for the two independent observers was used in all subsequent analyses. Seven behavior categories were dropped because of low occurrence rates and,or poor observer agreement, leaving 21 categories to be employed in the analyses that follow.

Results

Analysis plan. The analysis was conceptualized as an Order (2) by Attribution (3) repeated measures Multivariate Analysis of Variance (MANOVA). MANOVA was employed here again as a screening procedure as described in Experiment 1. *Order* was the between-subjects factor while *Instruction* (*Know-Love-or-Comfort*) was the within-subjects factor. Covariates were included in a theoretical ordering of most to least impact and tested, in stepwise fashion, for retention or exclusion in further analyses (Cohen & Cohen, 1983): (a) mother's *pre-delivery medication* by type and amount given, (b) *delivery anesthesia*, (c) *post-delivery medication* given to mother (see Hollenbeck, Gewirtz, Sebris, & Scanlon, 1984, for the scoring criteria on these three covariates), (d) mother's *education level* in years, (e) infant *gender*, (f) mother's *parity*, and (g) whether neonate was *bottle* or *breast fed*. The resulting Multivariate Analysis of Covariance (MANCOVA) and its significant findings are presented in Table 3 and will be discussed. Because no *Order* effect was detected, the *Order* factor was dropped and the analysis was recomputed as a within-subjects design. Since this was again an initial investigation in this area the MANOVA *alpha* level was set at $p < .10$ and the univariate *alpha* level was set at $p < .05$. Where marginal findings are reported, associated p values are reported.

Finally, the residual error-correlation space was examined separately to detect relations for future intensive study among covariates and dependent variables. These findings are reported in the Appendix.

Contrast main effects. For the *Know* vs. *Comfort* contrast, a main effect was detected. Examination of univariate analyses yielded five maternal behaviors that differed. Mothers held their neonates by the *torso* more during the *Comfort* condition (M = 10.22) than during the *Know* condition (M = 6.32). In contrast, mothers used their *laps* more under the *Know* condition (M = 10.78) than under the *Comfort* condition (M = 5.98). Similarly, mothers *repositioned* their neonates more under the *Know* condition (M = 1.76) than under the *Comfort* condition (M = 1.47). Mothers did more *jiggles body,rocks* under the *Comfort* condition (M = 7.31) than under the *Know* condition (M = 3.53). Similarly, mothers held their neonates in *burp position* more under the *Comfort* condition (M = .72) than under the *Know* condition (M = .34). No differences were detected for the other observed dependent variables for this contrast. No additional MANOVA main effects were detected for the *Know* vs. *Love* or *Love* vs. *Comfort* contrasts.

Covariate effects. In the analysis of covariance, two covariate effects were detected and controlled for in the design. For the *Know* vs. *Comfort* contrast: relative to nonwhite mothers Caucasian mothers *repositioned* (r = -.35) more and used *burp position* (r = -.38) their neonates more in the *Comfort* condition than in the *Know* condition. For the Grand Mean: compared to Caucasian mothers, nonwhite mothers showed more *arms extended* (r = .39) and *feeds* (r = .37), but fewer *strokes* (r = -.47), *looks at body* (r = -.36), and *talks* (r = -.28) to their neonates regardless of treatment condition. For the *Grand Mean*: mothers with higher *parity*, *jiggled body,rocked* (r = .48) more and *laughed* (r = -.29) less often to, her neonate, regardless of treatment condition.

Discussion

Since each observed covariate appears under more than one design element besides the *Grand Mean*, the interpretations of correlations with dependent variables is not straightforward. Furthermore, it should be recalled here that contrast effects are difference scores which may alter the keying of variables from contrast-to-contrast and thus, impacting on the direction of the correlation. For these reasons, and the exploratory nature of the subsequent residual space examination, individual interpretations will not be offered. Instead, the impact of specific covariates on clusters of dependent variables will be discussed. All observed correlations are of moderate magnitude ranging from (+/-) .31 to .61.

Parity was related to five variables under two contrasts and the *Grand Mean*: *jiggles body,rocks*, *laughs*, *feeds*, *talks*, and *caregiving*. *Race* was related to seven variables under the four design elements: *arms extended*, *strokes*, *looks at body*, *talks*, *feeds*, *repositions*, and *burp position*. *Delivery anesthesia* was related to four variables under four design elements: *arms extended*, *jiggles body,rocks*, *strokes*, and *jiggles body part*. *Predelivery medication* was related to four variables under three design elements: *no contact*, *jiggles body part*, *looks at body*, and *kiss*. *Post-delivery medication* was related to two variables under two design elements: *looks at face* and *kiss*. *breast vs. bottle* feeding was related to seven variables under three of the four design elements: *arms extended*, *strokes*, *burp position*, *feeds*, *looks away*, *vocalize a nonvocal sound*, and *no contact*. Mothers' *years of schooling* was related to four variables under three of the four design elements: *arms extended*, *strokes*, *looks at body*, and *holds*. Infant *gender* was related to three variables under two design elements: *laughs*, *jiggles body part*, and *looks at body*. Finally, five of the covariates correlated among themselves independent of design elements: *parity* was related to *post-delivery medication*, *breast vs. bottle* feeding was related to both *race* and mothers' *years of schooling*.

When the control variates were controlled statistically it was found that mothers behaved differently, in a reliable fashion, to their neonates under the *Comfort-Your-Baby* instruction than under the *Get-to-Know-Your-Baby* instruction. Mothers did not behave differently, however, to their neonates under the *Show-Love-to-Your-Baby* instruction than under either the *Get-to-Know* instruction or the *Comfort-Your-Baby* instruction. Therefore, the only instructions that were effective in motivating mothers to treat their neonates differently were those of *Get-to-Know-Your-Baby* versus *Comfort-Your-Baby*.

The instructions to *Comfort-Your Baby* inflected maternal behavior in a systematic way. Mothers following this instruction held their infants differently (more by the torso and in a burp position and less in their laps), they repositioned their infants fewer times, and they jiggled,rocked their infants' bodies more than under the *Get-to-Know* instruction. These maternal behavioral differences are consistent with a common-sense definition of maternal comforting behaviors toward their infant. Again, as in Experiment 1, it is important here to note the systematic inflection of five maternal behaviors in a reliable fashion due to the simple instruction of an authority figure.

General Discussion

The findings presented here indicate that maternal behavior may be inflected by simple attributions to and instruction on how to interact toward their neonates. In Exp. 1, the instruction to mothers reflected an attributed infant state. Maternal behavior was altered, relative to a baseline behavior observation. Mothers in this attribution condition reduced their jiggles body,rocks in a reliable fashion. A common-sense interpretation of this behavior change is that alert infants need less stimulating intrusions by mothers for them to produce interesting stimuli. Similarly, in Exp. 2, five maternal behaviors were reliably detected when mothers were instructed to *Comfort* their babies, relative to a baseline maternal behavior observation. All of the observed behaviors could again be interpreted employing common-sense definitions of maternal behaviors expressed as comforting toward infants. What is striking about these data is that these are reliable group mean- differences rather than individual behavior patterns. It is also noteworthy that the behaviors elicited have a straightforward interpretation relative to the experimental manipulations. The implied manipulative power of an authority figure to influence behavior in a systematic way is, in one respect alarming, although not surprising (e.g., Milgram, 1969). Placed in a slightly different context these findings may be viewed as underscoring the importance of controlling for reactivity effects or unintentional demand characteristics in studies such as this that take place in complex environments (e.g., hospitals) where researchers often do not exercise any controls outside the procedures of their studies. Of equal concern is that typical experimental designs, even ones with multiple control groups, do not control for intrasession history (Campbell & Stanley, 1963) where the casual remark or unintended attribution by those in seeming authority may lead to the inflection of behaviors by those seeming lower in status as demonstrated here. It is incumbent upon investigators to rule out threats to internal and external validity through careful design, control, and procedural safeguards which need to include monitoring for unintentional "experimenter" effects. Studies that fail to report such safeguards should be viewed with caution.

The findings from Exps. 1 and 2 have implications for the recent work on maternal perceptions of their infants and for the ongoing debate over the hypothesis of maternal attachment to infants based on early contact. In a series of reports, Zeanah and his colleagues (Zeanah, Keener, & Vieira-Baker, 1987; Zeanah & Anders, 1987; Zeanah, Keener, & Anders, 1986a; 1986b) have invoked a theory of "stable mental representations" developed by mothers prior to birth to explain their observation that adolescent mothers' perceptions of several infant temperament characteristics were stable prenatally and at 4 months postnatally. Given the lack of infant behavioral stability in this period (e.g., Asch, Gleser, & Steichen, 1986) the pattern of findings reported by Zeanah and others is not surprising nor is their reference to an unobservable internal process as an explanation necessary. As suggested earlier, a behavioral analysis in a transactional framework that incorporates the present findings provides a more parsimonious explanation of the Zeanah observation. Mothers with limited experience with infants are, at birth, presented with highly variable neonates in a setting where infant state and infant characteristics are attributed by others (e.g., hospital staff, relatives, visitors) and "helpful" suggestions are offered as instructions by authority figures (e.g., physicians, nurses, relatives) to mothers. Mothers are thus responding to a variable stimulus (their baby), modifying behavior based on that stimulus and as suggested by others, and in response to these behavior modifications have specific behaviors come under operant control. Thus, it is not surprising to see a pattern of maternal behavior whereby infants who were less responsive during feeds were treated as unpredictable by their mothers who expected this both before and after birth. Their expectancy being rewarded by the infant's unresponsiveness and their own inconsistencies. The behavioral analysis can account for this outcome more simply than hypothesizing some supposed mental representation as an explanation.

Booth and Meltzoff (1984) have related maternal expectancy and birth experience to maternal attachment. In a retrospective study they found that mothers of one-month-old infants reported large discrepancies between expected childbirth experiences and reality. This effects was most noticeable among primiparous mothers, presumably the least experienced. Maternal attachment, as measured by questionnaire, was positively correlated to psychological outcomes, indicating that mothers who perceived the birth as difficult were less attached and mothers with an easier birth were more attached. The low correlation reported (r = .18) is not convincing of a strong effect here. As in the present study, the concept of "love" which was used as a question defining attachment by Booth and Meltzoff, may be to abstract to have meaning when defined in behavioral terms or expressed behaviorally. Booth and Meltzoff relied upon retrospective maternal reports of their feelings using such concepts as "love" to define attachment. Given the results reported here reliance on such gross terms, open to multiple definition by individuals, is unfounded.

The maternal attachment debate focuses on the hypothesis of whether early maternal-neonate contact following birth leads to a special "bond" between the pair with particular emphasis on the mother's "attachment" to the neonate. Myers (1984) has reviewed critically the available evidence for and against mother-infant bonding and concluded that evidence for the hypothesis generally does not support the idea that early and extended contact is crucial for bonding. Kennell and Klaus (1984), originators of the bonding hypothesis and its most persuasive supporters, argue in response to Myers' conclusion that slight alterations in hospital practice since hypothesis formulation account for some of the apparent inconsistencies in results across studies. They argue further that, "the complex factors involved in the bonding process cannot be considered in isolation" (p. 275).

The present findings from Exps. 1 and 2 relate to this debate in at least two ways. First, at a macro level of analysis, in terms of the experimental effect on maternal behavior of either attribution or instruction, these two studies provide evidence that maternal behavior was inflected in a systematic and reliable fashion by attribution and instruction. These findings could be interpreted to support either the contentions of Myers, that a maternal bonding effect is artifact due to the methodological oddities in the original Klaus *et al.* report, or to support Kennell and Klaus's contention that the complex factors surrounding the events of bonding must be considered to observe the bonding process in operation. We tend to agree with the former interpretation rather than the latter. The inflection of maternal behavior in the present two studies occurred in populations of mixed race, mixed parity, mixed maternal ages, generally middle to upper middle class groups, and mothers above average in educational attainment. In addition, these factors, where differences were present, were held constant statistically. Thus, given the observed inflections of maternal behaviors and the implemented statistical controls the effects seem just as imposing as "bonding" effects reported by Klaus *et al.* using black, primiparous women receiving free maternity services. It is not difficult to suppose that unintended (and uncontrolled) attributions and,or instructions might have had an even greater impact on such a naive group. Of course, this supposition remains an empirical question for future investigation.

Secondly, at a more micro level of analysis, in terms of the definition of attachment, Exp. 2 provides evidence that concepts such as "love" had an indefinite behavioral expression in this group of mothers. Since Klaus *et al.* do not behaviorally define maternal attachment but rather define it in terms of verbal reports of maternal affect, the findings of Exp. 2 suggest to us that mothers relate such abstract commodities as love, comfort, and attachment to a subset of familiar behaviors, common among women, to the expression of comforting behaviors toward infants. Thus, what may be happening in the "maternal-bonding" process, from our perspective, is not changes in maternal behavior to the infant, but changes in the verbal expression of rather gross and undefined feelings inflected by their hospital situation. Until a more systematic accumulation of maternal behavioral data addressing this complex of factors, the maternal-bonding hypothesis continues, but continues to rest upon an inconsistent data base with alternative explanations available.

Findings of these two experiments also point to the importance of controlling background factors in studies such as this that are conducted in complex natural settings (e.g., hospitals). Variables such as simple demographic factors (e.g., race, maternal education, parity) and more complex organismic factors (e.g., medications received prior to, during, and following delivery) accounted for significant variance impacting on a number of independent and dependent variables. The covariance procedure employed here allows statistical control for these background variables plus the opportunity to examine their impact on the dependent variables. Furthermore, covariates play an identifiable role in the residual space after controlling for experimental design factors. The examination of residual correlations (see Appendix) afforded the opportunity to identify potential relations among a number of independent and dependent variables as described under each experimental results section. These relations provide direction for future research.

The specific covariates identified as having impact in these two studies were consistent with commonsense interpretations. For Race, the pattern of moderate correlations is what might be expected for any dichotomized variable where some results were in one direction and some in the opposite direction. Findings for Parity were more meaningful than those for Race. Parity results indicated that the more children born to mothers, the more stimulation (e.g., jiggles body,rocks) and the less laughing by mothers. This might mean that experienced mothers are not afraid to stimulate their infants and are less enthralled by the mother-infant interactive experience. Finally, the higher the maternal education the more holding and less stimulating was done by mothers indicating perhaps a slight relation among belief systems (educational experiences) and behavior concerning intrusiveness with infants.

The pattern of covariate drug effects is more complex to interpret than other covariates. In both experiments the three drug variables; delivery anesthesia, pre-medications, and post-medications; were related to other variables in the residual space, again reinforcing the need to control for these variables. In the covariance analyses only the pre- and post-medication covariates were identified as impacting dependent variables in Exp. 1. In general, these findings are consistent with those reported by Hollenbeck, Gewirtz, Sebris, and Scanlon (1984) employing these same drug category definitions. The more maternal medication the less maternal behavior toward the infant. The exception in Experiment 1 was the pre-medication effect where more medication lead to more maternal laughter and more holding of the infant in the mother's lap. A similar reversal was reported by Hollenbeck, Gewirtz, Sebris, and Scanlon, where more post-medication lead to more maternal smiling. What may be happening is that additional medication serves to relax maternal inhibitions concerning the birth, the hospital stay, and,or maternal participation in the study. The absence of covariate drug effects in Exp. 2 may be a consequence of the powerful contrast main effects that were uncovered for maternal behavior. Since the drug covariates also appear significantly in the residual space their presence was measurable in the study.

Finally, we end on a note of caution for investigators and consumers of the literature on the impact of early life experiences with babies for parents. Method alone appears to determine what is found or not found in generic studies of this type. We have employed rather sophisticated techniques, both experimental and statistical, to control for what seem to be reasonable background factors and to guard against the uncontrolled intrusion of chance findings. Nevertheless, for as many consistencies found in maternal behavior patterns and their systematic influence by manipulation. there were also a number of puzzling contradictions. For instance, why did drug covariates vary from Exp. 1 to Exp. 2? The overall impact of drug exposure appeared to be similar to expectations. The samples were drawn from roughly the same population domains. Yet, here the mosaic of results was textured in a different way from Exp. 1, to Exp. 2, and to prior studies with similar samples. These types of findings lead us to ponder the development of a field of maternal-infant studies and so-called findings of maternal attachment where few benchmarks for human behavior have been established and the standards for scientific inquiry have been loosely defined.

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Appendix

Experiment 1

Residual Effects. The residual error correlation matrix provides an opportunity for the examination of hypothetical covariate impact on dependent variables while controlling for the manipulations of the study design elements. In this residual space, a number of relations were found beyond those described in the MANCOVA Analysis. The significant (at $p < .05$) error correlations with dependent variables for the different design elements are reported in Table 4, where its is seen that no residual effect was suffiently strong to be found under all design element manipulations. Under the *Grand Mean*, with all design factors present, residual effects for *parity, pre-delivery medication*, and *post-delivery medication* were observed. Under the *Know* vs. *Alert* contrast, effects for *pre-delivery medication, post-delivery medication, breast vs. bottle* feeding, and infant *gender* were observed. Under the *Know* vs. *Not-Alert* contrast, effects for *parity, delivery anesthesia, breast vs. bottle* feeding, infant *gender*, and mothers' *years of schooling* were observed. Under the *Alert* vs. *Not-Alert* contrast, effects were observed for *parity, delivery anesthesia, post-delivery medication*, and mothers' *years of schooling*.

Experiment 2

Residual Effects. The residual error correlation matrix provides an opportunity for the examination of hypothetical covariate impact on dependent variables while controlling for the manipulations of the study experimental design. In this residual space, a number of relations were observed beyond those described in the MANCOVA Analysis. The significant (at $p < .05$) error correlations with observed dependent variables for the different design elements are reported in Table 5.

Examination of Table 4 indicates that, as in Experiment 1, no residual effect was suffiently strong under all design element manipulations in relation to the same dependent variable. Under the *Grand Mean*, with all design factors present, residual effects for *parity, race, delivery anesthesia, pre-delivery medication, breast vs. bottle* feeding, and mothers' *years of schooling* were observed. Under the *Know* vs. *Love* contrast, effects for *parity, delivery anesthesia, post-delivery medication, breast vs. bottle* feeding, mothers' *years of schooling*, and infant *gender* were observed. Under the *Love* vs. *Comfort* contrast, effects for *race*, and *pre-delivery medication* were observed.

Table 1

Adult Behavior Codes and Definitions

1. Torso: Holds baby close to body or face at torso level, with either or both hands.
b2. Shoulder: Holds baby close to body with por-

tions of baby's head higher than adult's shoulder, with either or both arms.

a3. Arms extended: Holds baby with arms extended, but only if baby's body or body part is NOT in contact with adult.

ab4. Arms overhead: Holds baby at arms' length overhead.

5. Lap: Holds baby on lap or legs. May also be on upper abdomen in obese adult (functional lap).

6. No contact: Puts baby down, out of gross bodily contact with adult.

7. Reposition: Repositions baby while maintaining contact category. Must include shift of infant center of gravity or axis of body. Does not include shifting of extremities alone.

8. Jigs body,Rocks: Jiggles baby's whole body (rapid, jagged cycles within relatively short episodes), while adult's body does not move or moves minimally. Also score if adult rocks the baby with her body. In either case, the infant's whole body must be involved.

9. Holds,Touch: Score if adult fingertip touches infant without a distinct stroking motion, or if adult holds infant body part without moving it across the entire time block.

10. Pats: May use fingers, palms, or both, but must cycle on and off body at least twice.

11. Strokes: Fingers are used with active lateral movement.

12. Jiggles part: Score when adult jiggles infant's body part or when adult holds body part briefly then releases it. If body part is held and stroked with movement of the part, score jiggles, not stroke.

b13. Extend finger: Score when adult finger is extended for infant to grasp. Continue to score whenever the infant is grasping the finger.

14. Kiss: Score for kiss when lips or nuzzle with face.

ab15. Hug: Hugs or places cheek to infant's cheek while holding baby or baby's arms.

16. Burp position: Positions on shoulder or on lap and pats, fingers, or palms. Score only when the infant has been fed or there is verbal evidence of intention to evoke air bubble. Do not score lap or shoulder when burp position is scored.

17. Feeds: Adult provides baby with bottle or pacifier.

18. Caregiving: Combs, diapers, grooms, rearranges clothing or blankets.

19. Looks-face: Fixates on infant face.

20. Looks-body: Looks at baby other than at face. Includes back and side of infant head. May be assumed when hands are involved with infant's body.

21. Looks-away: Looks away from infant face or body.

22. Smiles: Naso-labial facial folds deepen, cheeks may move upwards,downwards, eyes may squint.

23. Talks: Talks to infant.

24. Vocalizations-nonverbal: Include clicking, clucking, cooing, and whistling.

25. Laugh: Do not score smile when laugh is scored.

ab26. Talks to another: The presence of another must have been clearly established. Eye contact with this other may be made while talking. Use of the third person while speaking of the baby suggests that this should be scored.

ab27. Visual stimulation with body part: Visually stimulates intentionally with body part. Includes nod of head and attempts to catch baby's attention with finger movement as intended visual stimulus. If head nod is to be scored, it must include lateral movement and,or vocalization to suggest intentionality of stimulation.

ab28. Visual stimulation with object: Visually stimulates intentionally with object, such as spectacles, bottle. Does not include "reflexive" rearranging of spectacles, etc.

Note: The same behavior sequence can require multiple scoring as, e.g., Extends finger for grasping and uses finger to stroke baby's skin.

aDeleted from analyses in Experiment 1 for low occurrence rates and,or poor observer agreement.

bDeleted from analyses in Experiment 2 for low occurrence rates and,or poor observer agreement.

Table 2

MANCOVA, and Univariate Analyses of Variance and Adjusted Means for the Get-to-Know vs. Alert Contrast Effect.

Source	F	df	p<
GRAND MEAN			
Post-delivery Medication	5.87	22,15	.001
ALERT VS. NOT-ALERT CONTRAST			
Pre-delivery Medication	3.27	22,16	.01
KNOW VS. NOT-ALERT CONTRAST			
Post-delivery Medication	2.11	22,15	.07
KNOW VS. ALERT CONTRAST			
	2.10	22,15	.08
Years of Schooling	2.17	22,17	.06
Pre-delivery Medication	2.00	22,16	.08

Univariate Source:	(MEANS) Know	Alert	F	d.f.	p<
jiggles body/rocks	5.63	4.65	6.60	1,36	.02
holds	2.55	2.48	3.85	1,36	.06
pats	4.45	2.70	3.63	1,36	.07
looks away	4.50	3.38	2.96	1,36	.10

Table 3

MANCOVA Results for Experiment 2.

Source	F	df	p<
GRAND MEAN			
Race	2.08	21,28	.05
Parity	2.13	21,28	.05
KNOW VS. COMFORT CONTRAST			
	2.14	21,27	.03
Race	2.19	21,27	.05
Univariate Source:			
holds by torso	7.40	1,47	.01
holds in lap	9.43	1,47	.01
repositions	8.04	1,47	.01
jiggles body/rocks	4.28	1,47	.05
burp position	4.48	1,47	.05

Table 4

Residual Correlations for Study Design Elements in Experiment 1 (df = 38, p < .05)

GRAND MEAN			
Parity with	*torso*	r =	-.34
	no contact	r =	.33
	talks	r =	.33
Pre-med. with	*burp posit.*	r =	.67
KNOW VS. ALERT CONTRAST			
Post-med. with	*looks body*	r =	.31
	laugh	r =	.37
Breast vs. Bottle with			
	holds	r =	-.38
Gender with	*caregive*	r =	.37
	vocal, nonv.	r =	-.36
KNOW VS. NOT-ALERT CONTRAST			
Parity with	*no contact*	r =	-.37
Anesthesia with	*laps*	r =	.41
	no contact	r =	-.44
Breast vs. Bottle with			
	holds	r =	.42
Gender with	*feeds*	r =	-.32
	caregive	r =	.31
	vocal, nonv.	r =	-.32
Years of Schooling with			
	jiggles part	r =	-.33
ALERT VS. NOT-ALERT CONTRAST			
Parity with	*shoulder*	r =	-.33
	no contact	r =	-.40
Anesthesia with	*laps*	r =	.32
	no contact	r =	-.61
Post-medication with	*no contact*	r =	-.37
	kiss	r =	.33
	looks away	r =	-.37
Years of Schooling with			
	jiggles part	r =	.38
COVARIATES			
Anesthesia with	*Parity*	r =	.32

Note. *Parity*, *Pre-medication*, *Anesthesia*, *Post-medication*, and *Years of Schooling* are scaled low to high (0,1,2,3,...,N); *Gender* is scaled (male = 1, female = 2); and feed mode is scaled (*Breast* = 1, *Bottle* = 2).

Table 5

Residual Correlations for Study Design Elements in Experiment 2 (df = 50, p < .05)

GRAND MEAN

Parity with	*jiggles body*	*r* =	.40
	laugh	*r* =	-.29
Race with	*arms extended*	*r* =	.39
	strokes	*r* =	-.47
	feed	*r* =	.37
	looks at body	*r* =	-.37
	talks	*r* =	-.28
Anesthesia with	*arms extended*	*r* =	-.34
	jiggles body	*r* =	-.31
	strokes	*r* =	.31
Pre-medication with	*no contact*	*r* =	.42
	jiggles part	*r* =	.36
Breast vs. Bottle with			
	arms extended	*r* =	.28
	strokes	*r* =	-.46
	burps	*r* =	.30
	feed	*r* =	.34
	looks away	*r* =	.31
Years of Schooling with			
	arms extended	*r* =	-.32
	strokes	*r* =	.36
KNOW VS. LOVE CONTRAST			
Parity with	*feed*	*r* =	.34
	talks	*r* =	.36
Anesthesia with	*arms extended*	*r* =	.39
Pre-medication with	*looks at body*	*r* =	.30
Post-medication with	*kiss*	*r* =	-.31
Breast vs. Bottle with			
	feed	*r* =	.36
	vocal nonvoc.	*r* =	-.32
Gender with	*laugh*	*r* =	-.28
Years of Schooling with			
	looks at body	*r* =	.29
KNOW VS. COMFORT CONTRAST			
Parity with	*caregive*	*r* =	.34
Race with	*reposition*	*r* =	-.35
	burps	*r* =	-.38
Anesthesia with	*jiggles part*	*r* =	-.31
Post-medication with	*kiss*	*r* =	-.31
	looks at face	*r* =	.31
Breast vs. Bottle with			
	no contact	*r* =	-.28
Gender with	*jiggles part*	*r* =	.29
	looks at body	*r* =	.30
Years of Schooling with			
	holds	*r* =	-.29
LOVE VS. COMFORT CONTRAST			
Race with	*arms extended*	*r* =	.32
Pre-medication with	*no contact*	*r* =	.46
	extends fing.	*r* =	.32
	kiss	*r* =	.41
COVARIATES			
Breast vs. Bottle with			
	Race	*r* =	.44
	Education	*r* =	-.61
Parity with	*Post-medicat.*	*r* =	.33

Note. *Parity*, *Pre-medication*, *Anesthesia*, *Post-medication*, and *Years of Schooling* are scaled low to high (0,1,2,3,...,N); *Race* is scaled (Caucasian = 1, nonwhite = 2); *Gender* is scaled (male = 1, female = 2); and feed mode is scaled (*Breast* = 1, *Bottle* = 2).

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