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# SIMILARITY OF PARENT AND INFANT VOCAL BEHAVIOR AS A DIMENSION OF REINFORCER QUALITY

by

MARIE P. PARNES

A dissertation submitted to the Graduate Faculty in Psychology

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1/18/00
Date

Claire L. Poulson, Ph.D.
Chair of Examining Committee

1/15/00
Date

Joseph Glick, Ph.D.
Executive Officer

Nancy Hemmes, Ph.D.

Bruce Brown, Ph.D.

Supervisory Committee

THE CITY UNIVERSITY OF NEW YORK

## Abstract

SIMILARITY OF PARENT AND INFANT VOCAL BEHAVIOR AS A DIMENSION OF REINFORCER QUALITY

by

Marie Parnes

Adviser: Professor Claire L. Poulson

The purpose of this experiment was to determine whether behavioral similarity between parent and infant vocalizations enhanced the quality of parental vocalizations as a reinforcer for infant vocalization rate. Parental vocalizations that matched infant vocalizations were systematically compared with parental non-matching vocalizations in a single-subject-reversal design. During the matching condition, the parent produced the same sounds as the infant immediately following their occurrence. During the non-matching condition, the parent produced different sounds immediately following the occurrence of infant vocalizations. The non-matching vocalizations for each trial were selected from those the infant emitted during the matching condition. For all infants, vocalization rates were systematically higher during the matching condition when compared with the lower rates that were observed during the non-matching condition. In addition to these changes in infant vocalization rates, response patterns suggestive of reinforcement functions were also observed for all infants. Differential patterns of response change were suggestive of reinforcement functions, rather than elicitative functions of stimulus similarity between parent and infant vocalizations. The results of this research support the conclusion that behavioral similarity between parent and infant vocalizations may be an enhancer of parental vocal reinforcers.

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## Introduction

Many theorists have examined the role of parental social reinforcement on early language learning in infants (e.g., Mowrer, 1960; Risley, 1977; Schumaker & Sherman, 1978). It has been proposed that parents not only attend to infant vocalizations in ways that increase their frequency, but also in ways that differentially reinforce successive approximations of their native language. It is highly unlikely, however, that in the natural environment, parents actually have the skills to shape infant vocalizations. What is required, then, is an explanation that could account for the transition from infant babbling to more adult-like sounds in the absence of direct shaping by parents.

Mowrer (1960) proposed that as infants babble and coo, and hear themselves make sounds similar to those of their parents, the similarity between infant and parent vocalizations acquires secondary reinforcing value, and serves to increase the future probability of adult-like sounds emitted by the infants. Mowrer (1960) called this the "autistic theory of imitation" because the infants are automatically reinforced when they produce sounds similar to those of the parents.

In addition to Mowrer, other theorists have asserted that automatic reinforcement helps to explain early language acquisition ( e.g., Skinner, 1957; Sundberg, Michael, Partington, & Sundberg, 1996). According to Skinner (1957), verbal behavior is maintained by environmental contingencies such that direct reinforcement, along with automatic reinforcement, may strengthen an infant's vocal behavior.

An automatic reinforcer is one that is produced by its response. Although the creation of an automatic reinforcer may be socially mediated, once established, it occurs

without social mediation. An automatic reinforcer is established through the pairing of a previously neutral stimulus with reinforcement such that the neutral stimulus acquires reinforcing value (Skinner, 1957). For example, singing results in the production of an auditory stimulus (i.e., a song) that may be paired with direct social reinforcement, such as praise. The pairing of the previously neutral auditory stimulus, a song, with social reinforcement, results in the song acquiring conditioned reinforcing value. Furthermore, the emission of the response (singing) that has topographical similarity to the previously neutral stimulus (song) may now have self-strengthening properties.

Empirical support for the establishment of automatic reinforcement and its role on the establishment of new vocal responses was investigated in research by Sundberg, Michael, Partington, and Sundberg (1996). Five children between the ages of 2-4 years served as subjects. One child was normally developing, whereas the other four children were diagnosed with speech and language delays, as well as other developmental disabilities. The purpose of the experiment was to ascertain whether new vocal responses could be established in the subjects' vocal repertoire by pairing vocalizations that were not previously emitted by the subjects, with established reinforcers. During the baseline condition, the experimenter recorded all vocalizations emitted by the subject. The experimenter did not prompt or reinforce the subject. During the pairing condition, target vocalizations were generated for each subject. Target vocalizations were defined as vocalizations not previously emitted by the subject. For each session, a familiar adult approached the subject, emitted the target vocalization, and immediately followed it by an established form of reinforcement (e.g., tickles, praise). The pairing procedure was repeated during a 1 to 2 minute period with approximately 15 pairings per minute. During

this condition, the subject did not receive any reinforcement by the experimenter. The post-pairing condition was the same as baseline, wherein no pairings of target vocalizations and established reinforcers occurred. The results showed that the subjects increased their emission of most target vocalizations, over baseline levels, during the post-pairing condition. The authors concluded that selected target vocalizations increased because the auditory stimuli produced by these responses functioned as conditioned reinforcers and automatically strengthened these responses. The authors also concluded that automatic reinforcement may play a role in language acquisition; but they did not investigate behavioral similarity as a kind of automatic reinforcer, and its relation to language acquisition.

Risley (1977) contends that automatic reinforcement alone may not be sufficient for initial language acquisition. He maintains that parents play a more active role in this process by providing direct reinforcement for some level of infant vocalizations. That is, while parents care for, or spend time with, their infants, they also provide primary reinforcers such as food, warmth, and comfort. On occasion, these primary reinforcers follow infant vocalizations. Eventually, parental reinforcement increases the overall frequency of vocalizations emitted by the infants. Furthermore, when parents deliver these primary reinforcers, they often vocalize to their infants. The pairing of previously neutral stimuli (i.e., parental vocalizations) with primary reinforcers such as food, warmth, and comfort, result in parental vocalizations acquiring secondary reinforcing value. Therefore, the combination of direct and automatic reinforcement (in the form of behavioral similarity) is most likely a major impetus in first language acquisition.

The findings of previous research have established that behavioral similarity has reinforcing properties for the behavior of children. That is, when children were imitated by an adult, they emitted higher levels of responding when compared to children who were not imitated (e.g., Miller & Morris, 1974; Gladstone & Cooley, 1975). Miller and Morris (1974), for example, explored the effects of being imitated with by an adult experimenter on preschoolers' performance on a marble dropping task. Children were presented with one of three holes in which to drop their marbles. The experimenter imitated a marble drop only if it occurred in the experimentally designated hole. Any marble drops in either of the other two holes resulted in the experimenter selecting the other non-imitated hole. As compared with subjects in a control group that were never imitated, subjects in the experimental group increased their choice of the hole that produced marble-dropping by the experimenter, and decreased their choice of the other holes.

In another study, Gladstone & Cooley (1975) established that behavioral similarity in the form of matching motor responses provided by an adult, functioned as a reinforcer for the motor behavior of four preschoolers. The children were allowed to operate any one of three noise-makers. If a child operated an experimentally designated noise-maker, the experimenter imitated the child by selecting the same noise-maker, thus producing behavioral similarity. If the child selected any of the other two noise-makers, the result was non-imitation by the experimenter. The children systematically increased their selection of the noise-maker that resulted in imitation by the experimenter.

These findings also apply to children with delayed development. In a study by Hallahan, Kauffman, Kneedler, Snell & Richards (1977), institutionalized children with mental retardation were divided into three groups. Each subject was randomly assigned to

one of three conditions: imitation, non-imitation, and modeling only. The subjects were given a three-choice task wherein the subjects and the experimenter took turns feeding colored plastic cookies to a "Cookie Monster." In the imitation condition, the experimenter imitated the subject's choice of cookie color, and in the non-imitation condition, the experimenter did not imitate the subject's choice of cookie color. Following imitation or non-imitation, the subject was given opportunities to imitate the experimenter. In the modeling only condition, the subject's choice always followed the experimenter's so that the subject always had the opportunity to imitate. The results indicated that the children in the imitation condition imitated significantly more than subjects in the non-imitation or modeling only groups.

Additionally, other research has indicated that being imitated may be either reinforcing or punishing to children with delayed development. For example, Kauffman LaFleur, Hallahan, & Chanes, (1975) used an ABAB single-case design to investigate the effects of adult imitation on two children with mental retardation in an attempt to decrease their inappropriate behavior. Exaggerated imitation alone, contingent upon one child's inappropriately fast eating, resulted in a systematic decrease from baseline levels. For the other child, however, exaggerated imitation combined with facial expressions of displeasure, contingent upon inappropriate vocalizations, resulted in a systematic increase over baseline levels. For one child, therefore, exaggerated imitation of inappropriate eating served to reduce that behavior; and for the other child, exaggerated imitation combined with the delivery of stern facial expressions of inappropriate vocalizations served to increase that behavior. It is worth mentioning that the two subjects were not

typically developing, and therefore, the results may not relate to normally developing children.

Finally, Parton & Fouts (1969) investigated the function of similarity using a color matching task. Using a computer-generated modified matching-to-sample procedure, children were presented with a standard stimulus key and then two comparison stimuli. One comparison stimulus was the same color as the standard, and the other comparison stimulus was a different color from the standard stimulus. The authors observed that children who could produce the same color or a dissimilar color from the standard panel color, maximized similarity by pressing the key associated with the same color. Thus, the authors concluded that similarity had reinforcing effects.

The previous studies manipulated similarity by having the experimenter imitate the behavior of the child. The findings of other research, however, also support that similarity can function as a conditioned reinforcer, but under circumstances wherein the child produces the similarity ( e.g., Baer & Deguchi, 1985; Poulson & Kymissis, 1994). For example, a study by Poulson and Kymissis (1994), that used a multiple-baseline design across subjects, demonstrated that preschool children will systematically increase an operant response (button pressing), over baseline levels, so they may have the opportunity to imitate the experimenter's vocal models. This occurred as long as some of the children's imitation of adult-modeled responses was reinforced. The authors concluded that similarity between the modeled responses of the experimenter and the imitative responses of the subjects may have acquired secondary reinforcement properties and may have maintained imitative responses that were never directly reinforced.

Most of the aforementioned studies have examined the effects of adult imitation on the behavior of children. A few studies, however, have experimentally assessed the effects of contingent-vocal imitation on infant behavior (i.e., Haugan & McIntire, 1972; Hursh & Sherman, 1973; Hamilton, 1977; Poulson & Nunes, 1988; & Pelaez, Otto, Paik & Gewirtz, 1999). Using a multiple-baseline design across subjects, Hursh and Sherman (1973) compared the combined effects of modeling, praise, and imitation on selected vocalizations in 15-20 month olds. The total package of modeling, praise, and imitation produced the highest frequencies of the selected target vocalizations over baseline frequencies, as compared to parental praise alone, and parental praise with imitation. Praise and praise with imitation were equally effective in producing an increase in the target vocalizations.

Three other studies explored the effects of vocal imitation alone (i.e., Hamilton, 1977; Haugan & McIntire, 1972; and Pelaez et al., 1999). In the first study, Hamilton (1977) used a between groups design to compare modeling, modeling and music, social praise, and vocal imitation by an adult experimenter on specific infant vocalizations. A within-groups analysis indicated that the infants in the vocal-imitation group increased their target vocalizations over baseline levels.

Haugen and McIntire (1972) compared the effects vocal imitation, tactile stimulation, and food on the vocalization rates of 3 to 6 month-old infants. Whereas all consequences increased vocalization rates over baseline levels, the group that received contingent vocal imitation produced the greatest increase in vocalization rates.

More recently, Pelaez and colleagues (1999) examined whether contingent imitation had reinforcing effects for infant vocal behavior. In this research, one group of

infants received contingent imitation from the mother. The other group received contingent motherese. Motherese is the altered language adopted by adults when speaking to young children that includes simple sentences, exaggerated intonation, and very clear pronunciation (e.g., Fernald & Kuhl, 1987). The results indicated that the highest rate of vocalizations occurred in the group that received contingent imitation from the mother. Nevertheless, Pelaez et al. (1999) compared only two kinds of vocal feedback, and she did not look at behavioral similarity in a broader sense. That is, the qualitative differences in the vocal feedback, rather than any differences in similarity, may have caused the differences in infant vocalization rate. Thus, to isolate whether similarity is the relevant variable, one might compare three or more similarity conditions. Such a comparison was undertaken by Smeets, Kauffman, and Kleinloog (1982).

In two experiments, Smeet et al.(1982) manipulated degrees and proportions of similarity using a motor-with-toy task with sixty first-and second-grade children. In the first experiment, the children were divided into five groups. Using a color sorting task that involved placing different colored discs on the nose of a toy seal, the experimenter produced varying degrees of similarity dependent upon the subjects' responses. The group that received the exact imitation condition produced significantly more imitation of the experimenter compared to the other experimental groups. Moreover, there were no significant differences between the other experimental groups. In the second experiment, the experimenter varied proportion or quantity of similarity across five groups of children. The group of children that received the greatest amount of similarity imitated more than the other groups of children that received lesser amounts of similarity. Furthermore, the other experimental groups did not differ significantly from each other. The authors

concluded that children increased their imitation of the experimenter only when they had been consistently and exactly imitated. One could not be sure, however, if behavioral similarity had a reinforcing function in either experiment, because there was no control for elicitation effects of the presumptive reinforcing stimuli.

Although empirical research supports the role of behavioral similarity as a reinforcer for the behavior of children and infants, one might be concerned if behavioral similarity in the form of parental imitation of infant behavior occurs naturally within daily parent-infant interactions. The findings of descriptive studies have indicated that during play time, mothers often imitate their infants (e.g., Hardy-Brown, Plomin, & Defries, 1981; Malatesta & Haviland, 1982; Moran, Krupka, Tutton, & Symons, 1987; Pawlby, 1977; Tulkin & Kagan, 1972). For example, Pawlby (1977) reported that each mother-infant pair produced a mean number of imitative sequences that had a range from 4.4 - 14.5 per 10-minute session. In addition, each individual pair engaged in imitative sequences for a mean time of 1.7 minutes, or 16% of each observation period. During that time when the mother and babies engaged in imitative sequences, mothers imitated their infants for a mean of 1.3 minutes, whereas infants imitated their mothers for a mean of .4 minutes. Vocal imitative sequences also occurred more frequently than motor and motor-with-toy imitative sequences. In another study, Malatesta & Haviland (1982) reported that 25% of all maternal changes of facial expression were contingent upon changes of expression made by the infant. Thirty-five percent of these contingent responses were imitative. The findings of research by Moran et al. (1987) also showed that mothers displayed an above-chance tendency to imitate the exaggerated lip and eye movements of their infants within one second of their onset. Furthermore, Hardy-Brown

et al. (1981), reported that two measures of maternal behavior, vocal-responsivity to infant vocalizations and vocal imitation, were positively correlated with overall communicative performance of the infant. Therefore, the findings of this research show that parent-infant imitation occurs during their interaction time.

The previously described research supports the hypothesis that behavioral similarity can function as a reinforcer for the behavior of children and infants. Nevertheless, systematic changes were not demonstrated for individual infants.

The purpose this research, therefore, was to investigate the extent to which vocal similarity produced by parental matching of infant vocalizations produced higher infant vocalization rates than vocal dissimilarity. A single-subject-reversal design was used to make these comparisons in 3 to 9 month-old infants. Infant vocalization was the dependent variable and parental matching and non-matching vocalizations was the independent variable.

It was expected that qualitatively different parental vocalizations ( i.e., similar and dissimilar) that are contingent upon infant vocalizations might produce systematic differences in acquisition patterns that would be suggestive of reinforcement functions. For example, a salient reinforcer might select infant vocalizations over other infant responses that might be occurring (Herrnstein, 1970). Under such conditions, one would expect a gradual, as opposed to an abrupt, increase in infant vocal behavior. Such a gradual acquisition curve suggests the operation of reinforcement, as opposed to discrimination or elicitation functions of similarity of parental vocalization. On the other hand, an abrupt acquisition may be more consistent with elicitation than with reinforcement functions because one would expect that each presentation of the eliciting

stimulus would trigger an infant vocalization, and therefore, produce an immediate increase.

Before the beginning of this study, research was conducted with a pilot subject to examine whether parental matching of infant vocalizations increased vocalization rates over those obtained during an operant-level baseline condition. A brief description of this research is contained in Appendix A. In addition, a paper that surveyed the infant learning research is contained in Appendix B.

## Method

### Subjects

Four typically developing infants, between the ages of 3 and 9 months, served as subjects. Their parents were recruited by word-of-mouth and through flyers posted on bulletin boards around the Queens College campus and within in the local community within a 5 mile radius.

Ellie was 4 months 9 days on the first day of the operant level baseline condition, and was 6 months 18 days on the date of her last experimental session. Joanne was 5 months 4 days on the first day of operant level, and 8 months 6 days on the date of her last session. Alex was 9 months 17 days on the first day of operant level, and 12 months 2 days on the date of his last session. The last subject, Ben, was 3 months 25 days on the first day of operant level, and  months  days on the date of is last session.

The experimenter administered the Bayley Scales of Infant Development, Mental Development Index (Bayley, 1969), to the four subjects. The results indicated that all four infants received scores in the average to above average range of development ( Mean = 100).

Setting and Apparatus

For three subjects, the study took place in the Queens College Infant Laboratory. The subjects sat behind a three-paneled plywood screen (61cm x 152 cm), with a window in the middle panel (30cm x 43 cm). The infant was placed on one side of the screen, and the parent sat on the other side and faced the infant. The mother determined the infant's preferred seating arrangement. Ellie initially sat in an infant car seat ( Even Flo Joy Ride Car Seat Carrier, Model # 2D31D1P1), and then switched to a walker (Graco Walker Model # 4436MT ) during her participation in the study. Alex was placed in a playpen (Kolcraft Model # 18342). One 28 -V incandescent bulb with yellow crystals was positioned on the upper left hand side of the window facing the parent 11cm from the window opening. Two independent observers activated the light by pressing foot switches on the floor. Frequency of infant vocalizations and parental vocal stimuli were recorded on two portable event recorders (S & K Products). Two video cameras (Panasonic WV-3260/8AF) were mounted on tripods (Vidcor); one faced the parent, and the other faced the infant. Each camera was hooked up to a special-effects generator (Panasonic WJ-3500) that allowed a vertically split image to be recorded from a television monitor (Panasonic CTJ—2062R) to a video cassette recorder (Panasonic AG1820).

For one subject, Joanne, the experiment was conducted in her home because she and her mother resided approximately 35 miles from Queens College. She and her mother sat across from each other on the living room floor in their home. Joanne sat in her saucer seat (Even Flo ExerSaucer Model # 605912). Two independent observers sat off to the side and recorded frequency of infant vocalizations and parental vocal stimuli on portable event recorders. Joanne's mother was signaled to provide the vocal stimuli by the

illumination of a 40-W soft pink incandescent light bulb that was attached by cables (Republic SuperFlex Type ETT) to two 6.5cm x 11cm switches (Leviton 120 V 15 Amp); one for each observer. The light was activated when both switches were depressed. A portable camcorder (Panasonic Model # AG450) and tripod (Vidcor) were used to record each session.

Procedure

The parents of Ellie, Alex, and Ben brought their infants to the Queens College Infant Laboratory at the Queens College campus. Sessions for Joanna were conducted in her home. All of the participating parents were mothers. All but one had completed at least four years of college, with one mother, a recent émigré from Russia, having obtained an M.D. in gynecology in her native country.

Upon arrival at the infant laboratory, the mother and infant sat in the reception area or at a table in the conference room. When the mother was confident that the infant was content ( i.e., dry, fed, and alert), they were brought into the experimental room. The mother placed the infant in his or her preferred seating arrangement, and then sat on the other side of the screen. The two observers sat on either side of the infant and did not talk nor make eye-contact with him or her. If the infant turned to look at either observer, the observer looked down or to the side to avoid the eye contact.

As previously mentioned, sessions for Joanna were conducted in her home. When Joanna was comfortable, her mother placed her in the saucer seat and then sat on the floor facing Joanna. Sessions were always conducted in the living room. The two observers sat approximately 5 feet away on the other side of the living room, and did not interact with Joanna while the sessions were being conducted.

One session consisted of two 6-minute intervals; and a minimum of three sessions were scheduled per week. Total participation time in the study for all subjects was between 2 and 3 months. A session began when the primary observer took her seat and prompted (usually a nod of the head) the mother to face her infant. A session ended after 12 minutes had elapsed, as indicated by a clock on the primary observer's event recorder. Sessions were terminated after 1 minute of crying, or at the parent's request. For example, a parent might have requested to terminate a session if the infant was fussy and was unable to be redirected or consoled.

The mothers were instructed to play with their baby using any toys that they wished. A variety of toys was available during all experimental conditions to keep the baby content. The mothers also were free to bring in toys from home to use during experimental sessions. Additionally, they were told that they could praise their infants for behavior other than vocalizing. Specifically, they were told that when the infant was quiet, they could praise their infant for "other" behavior. Social praise was defined as the delivery of statements that affirmed the particular behavior. Examples of such statements include: "I like how your sitting", "Good looking at mommy", and "Good playing with the toy." The purpose of the toys and the praise were to keep the baby content and to facilitate the interaction between mother and infant.

Response Definitions

An infant vocalization was defined as a voiced sound uttered by the infant. A pause of 1 second or longer defined the beginning of a new vocalization. The following sounds were excluded: coughing, breathing, sneezing, burping, and hiccoughing.

Any sounds judged to be crying produced the same contingencies as any other vocalizations; however, it was scored separately and was not included in the calculation of infant vocalization rate. Crying was defined as a shrill or an intense vocalization and possibly accompanied by a red face, furrowed brow, and/or sad expression. As stated previously, sessions were terminated if crying occurred for at least 1 minute.

Definitions of Matching and Non-Matching Parental Vocalizations

After all of the parental matching experimental sessions, the observers viewed the video tape and wrote down verbatim the vocalizations produced by the infants. These vocalizations were listed so they could be used by the parent during the non-matching experimental condition; that is, the vocal stimuli from the matching condition were yoked to the non-matching condition (refer to Table 1). Prior to the commencement of the first non-matching session, this list was presented to the mother. The experimenter reviewed the list with the parent to ensure that she was familiar with the vocalizations. During the experimental session, the parent held the list in her hand or placed it beside her. She was instructed to emit those vocal stimuli in the order that they appeared on the list. If the parent emitted all the vocalizations on the list before the end of the session, she was instructed to go back to the beginning.

Parental matching vocalizations were defined as the parent producing the same vocalizations as the infant's within 2 seconds of their occurrence. Parental non-matching vocalizations were defined as the parent producing different vocalizations within 2 seconds of the occurrence of infant vocalizations. As stated above, these were vocalizations previously emitted by the infant during the preceding matching condition.

Table 1

Vocalizations for Ellie, Joanne ,Alex, and Ben

| Ellie | Joanna | Alex | Ben |
|---|---|---|---|
| ah | AHHHHH... | ah | ah |
| ah-goo | ah-ah-ah... | ah-gah | ah-ah.. |
| ah-oo-ah-oo | ah-dah-dah | ah-nah | AH |
| ah-boo | eh | ah-mah | ah-ooo |
| ah-mah | eh-ay-ay-yah-yah | ah-yah | ah-uh... |
| aye-oo-ah | ah-uh-ah-uh-ah | co-co-co | eh |
| aye-uh... | ah-nah-nah-nah... | dah | hah |
| ah-broo-ah | ah-yah-yah-yah | dah-dah... | heh... |
| ah-woo | eh... | deh-deh | hah-mmm |
| ah-oooo | heh-heh-heh | dye-dye | HEH |
| bbrrrrroo-ah | aye-yo-yo-yo | eeeee..... | URRR |
| buh | ah-rah-rah-rah | eh | ow |
| eh-wah | ah-wah-dah | gah-gah | oo-dah-dah.. |
| eh-oooo | eh-nah | goo-goo | wah-wah |
| eh-yay | mmmmm | guh-guh | waaaaah |
| eh-uh | | | woo |

| Ellie | Joanna | Alex | Ben |
|---|---|---|---|
| eh-gah-gah | ay-Raspberry*-ah | ha-ha | |
| eh-oo-ah | ahhah | la-la-yah | |
| eh-buh | ah-mah | ma | |
| eh-ay | eh-Raspberry* | ma-ma-ma... | |
| eh-wah | mmmm-ah | nah | |
| ee-ay | ah | nah-nah... | |
| eh... | ah-wah | mmmmmmm | |
| gah.. | neh-neh | oh | |
| guh | eh-AHHH-eh-eh | oo-oo | |
| guh-aye | raspberry* | oo-uh | |
| oo-guh | | uh-ah | |
| oo-Raspberry*-ah | | uh-ah-woo | |
| uh-eh | | pee-pee-pee | |
| oo-ah-Raspberry* | | tee-tee-tee | |
| oo-eh-oo-eh | | raspberry* | |
| oooo | | | |
| oo-wah | | | |
| uh | | | |
| uh-eh | | | |

* = raspberry sound

Experimental Design

This experiment compared contingent matching and contingent non-matching of infant vocalizations in a single-subject-reversal ABCBC design. Experimental condition changes were made when the data were judged to be visually stable (Baer, Wolf, & Risley, 1968). As a supplement to visual inspection, behavior change was also evaluated with a statistical analysis procedure called the Mann-Wald test (Mann & Wald, 1943).

The Mann-Wald procedure is an example of an interrupted time-series analysis that uses a linear least-squares autoregressive model. It sequentially measures the same dependent variable over time; before and after some intervention. The dependent variable is composed of the observed data excluding the first four scores. The Mann-Wald procedure then provides individual t tests for slope and intercept parameters and their changes from before and after intervention. The advantage of this kind of procedure is that an unequal number of observations before and after intervention makes little difference in the results. Furthermore, this procedure can be used with smaller sample sizes (Gottman, 1981; Ramsey & Ramsey, 1997).

The chosen alpha level, the probability of making a Type I error or rejecting the null hypothesis when it is true, was set at .05. In general, the scientific community selects an alpha level of .05 as an acceptable probability of reporting a false result. Although making the alpha level more stringent (e.g., .01) decreases the probability of Type I error, it increases the probability of Type II error; that is, failing to reject the null hypothesis when it is false or missing an experimental effect (e.g., Hurlburt, 1994).

Experimental Conditions

The following are descriptions of each experimental condition:

(A) Operant level. Each subject began his/her participation in the study with an operant level condition. The purpose of this condition was to generate an operant level of infant vocalizations against which to compare the subsequent vocalization levels under different experimental conditions. The parent was instructed to play with the baby, but with the stipulation that she refrain from matching or praising infant vocalizations.

(B) Parental Matching Experimental Condition. During this condition, the parent was instructed to match all infant vocalizations immediately after their occurrence. For example, if the baby said "bah," the parent was to make that vocalization immediately.

(C ) Parental Non-Matching Experimental Condition. During this condition, the parent was instructed to make a dissimilar vocalization upon all infant vocalizations immediately after their occurrence. For example, if the baby said "booboo," the parent selected a different vocalization from the list of infant vocalizations, such as "gah." As previously stated, the vocal stimuli emitted by the infant during the matching condition were yoked to the non-matching condition.

Data Analysis

To obtain the infant vocalization rate per minute, the onset time of infant vocalization was recorded for each of the two 6-minute intervals per session. The rate was calculated by deriving the frequency of vocalizations and dividing by 6 (the duration of the interval). To obtain the measure of mean duration of infant vocalizations, the onset and offset times for each vocalization were recorded. The mean duration was calculated

by acquiring the difference between onset and offset times for each vocalization and dividing by frequency of vocalizations per interval.

As stated previously, parental matching vocalizations refer to the parent making the same vocalizations as the infant's within 2 seconds of their occurrence, whereas parental non-matching vocalizations refer to the parent making different vocalizations. Parental vocalization data were based on a yes/no measure to determine whether it was either a match or a non-match. To obtain a rate measure of parental matching and non-matching vocalizations, onset time of each parental matching or non-matching vocalization was recorded for each of the two 6-minute intervals per experimental session. Rate was calculated by taking the frequency of matching or non-matching vocalizations and dividing by 6 (the duration of the interval).

Interobserver Agreement

For all infants, interobserver agreement was obtained for 66% of 179 6-minute intervals. For Ellie, agreement was obtained for 76% of 49 intervals; for Joanna, it was obtained for 84% of 37 intervals; for Alex, it was obtained for 58% of 45 intervals; and for Ben, it was obtained for 50% of 48 intervals (see Table 2).

Interobserver agreement for infant vocalizations was calculated on a point-by-point basis by dividing the number of agreements by the sum of agreements and disagreements and multiplying by 100. The onset of vocalizations had to be recorded by both observers within 1 second to be counted as agreements. For all four infants, overall interobserver agreement for infant vocalizations per minute was 89%. Individual interobserver agreement scores for Ellie, Joanna, Alex, and Ben were 89%, 91%, 87%, and 89%, respectively (see Table 3).

Table 2

Percentage of Total Intervals for Which Interobserver Agreement was Obtained Across Experimental Conditions for all Infants

| | Percentage of Intervals | | | | |
|---|---|---|---|---|---|
| | Op Level | Matching | Non-Match | Matching | Non-Match |
| Ellie | 33% of 6 intervals | 66% of 15 intervals | 100% of 10 intervals | 72% of 11 intervals | 100% of 7 intervals |
| Joanna | 75% of 8 intervals | 100% of 10 intervals | 100% of 5 intervals | 75% of 8 intervals | 66% of 6 intervals |
| Alex | 50% of 8 intervals | 50% of 18 intervals | 88% of 8 intervals | 60% of 5 intervals | 50% of 6 intervals |
| Ben | 66% of 6 intervals | 43% of 14 intervals | 50% of 8 intervals | 43% of 14 intervals | 50% of 8 intervals |

Table 3

Interobserver Agreement Scores and the Number of Intervals for Which They Were Obtained for Infant Vocalization Rates across Experimental Conditions for all Infants

| | Interobserver Agreement Scores for Vocalization Rates per Number of Intervals | | | | |
|---|---|---|---|---|---|
| | Op Level | Matching | Non-Match | Matching | Non-Match |
| Ellie | 81% of 2 | 91% of 10 | 92% of 10 | 96% of 8 | 96% of 7 |
| | R=80-83% | R= 86-97% | R=80-100% | R=84-100% | R=88-100% |
| Joanne | 88% of 6 | 90% of 10 | 91% of 5 | 88% of 6 | 88% of 4 |
| | R=80-94% | R=80-96% | R=80-100% | R=85-95% | R=85-90% |
| Alex | 87% of 4 | 90% of 9 | 83% of 7 | 87% of 3 | 89% of 3 |
| | R=81-90% | R=80-98% | R=80-85% | R=82-90% | R=82-95% |
| Ben | 83% of 4 | 90% of 6 | 88% of 4 | 93% of 6 | 90% of 4 |
| | R=79-92% | R=85-92% | R=85-100% | R=83-100% | R=85-95% |

R = range

Additionally, interobserver agreement was obtained for duration of infant vocalizations. The onset and offset times of vocalizations had to be recorded by both observers within 1 second to be counted as agreements. Overall interobserver agreement for all infants was 87%. For Ellie, Joanne, Alex, and Ben it was 86%, 88%, 91%, and 85%, respectively (refer to Table 4 ).

Procedural Reliability

Procedural reliability measures were obtained for 67% of 179 consecutive 6-minute intervals. The number of agreements was divided by the sum of agreements and disagreements and then multiplied by 100. Onset of parental matching or non-matching vocalizations had to be scored by both observers within 1 second to be counted as agreements. For all infants, overall interobserver agreement was 89%. For Ellie, Joanna, Alex, and Ben it was 89%, 90%, 89%, and 87%, respectively (see table 5).

Agreement for correct delivery of the parental matching and non-matching vocalizations within was also obtained. As stated previously, correct delivery was defined as the parent producing the same or different sounds (depending on the experimental condition) within 2 seconds of their infant's vocalizations. Overall agreement for all infants was 88%. Agreement scores for Ellie, Joanne, Alex, and Ben were 88%, 86%, 86%, and 86%, respectively (see Table 6). In addition, individual agreement scores for the percentages of infant vocalizations followed by parental matching or non-matching vocalizations within 1 second are presented in Table 7.

Table 4

Interobserver Agreement Scores and the Number of Intervals for Which They Were Obtained for Mean Duration of Infant Vocalizations Across Experimental Conditions for all Infants

Interobserver Agreement Scores for Mean Duration per Number of Intervals

| | Op Level | Matching | Non-Match | Matching | Non-Match |
|---|---|---|---|---|---|
| Ellie | 80% of 2 | 91% of 10 | 91% of 10 | 96% of 8 | 96% of 7 |
| | R=79-83% | R= 85-97% | R=80-100% | R=84-100% | R=88-100% |
| Joanne | 88% of 6 | 88% of 10 | 89% of 5 | 88% of 6 | 88% of 4 |
| | R=80-94% | R=80-96% | R=80-100% | R=85-95% | R=85-90% |
| Alex | 87% of 4 | 91% of 9 | 83% of 7 | 86% of 3 | 88% of 3 |
| | R=81-90% | R=80-98% | R=80-85% | R=82-90% | R=82-95% |
| Ben | 80% of 4 | 88% of 6 | 88% of 4 | 93% of 6 | 90% of 4 |
| | R=79-92% | R=85-92% | R=85-100% | R=83-100% | R=85-95% |

R = range

Table 5

Interobserver Agreement Scores and the Number of Intervals for Which They Were Obtained for Rate of Parental Vocalizations Across Experimental Conditions for all Infants

| Interobserver Agreement for Parental Vocalizations Rates per Number of Intervals | | | | |
|---|---|---|---|---|
| | Matching | Non-Match | Matching | Non-Match |
| Ellie | 88% of 10 | 95% of 10 | 81% of 8 | 88% of 7 |
| | R= 85-97% | R=80-100% | R=84-100% | R=88-100% |
| Joanne | 85% of 10 | 86% of 5 | 89% of 6 | 88% of 4 |
| | R=80-96% | R=80-100% | R=85-95% | R=85-90% |
| Alex | 88% of 9 | 83% of 7 | 88% of 3 | 87% of 3 |
| | R=80-98% | R=80-85% | R=82-90% | R=82-95% |
| Ben | 84% of 6 | 87% of 4 | 92% of 6 | 91% of 4 |
| | R=85-92% | R=85-100% | R=83-100% | R=85-95% |

R= range

Table 6

Interobserver Agreement Scores and the Number of Intervals for Which They Were Obtained for Correct Delivery of Parental Vocalizations Within 2 Seconds Across Experimental Conditions for all Infants

| | Interobserver Agreement for Correct Delivery of Parental Vocalizations per Number of Intervals | | | |
|---|---|---|---|---|
| | Matching | Non-Match | Matching | Non-Match |
| Ellie | 81% of 10 | 88% of 10 | 88% of 8 | 88% of 7 |
| | R= 85-97% | R=80-100% | R=84-100% | R=88-100% |
| Joanna | 90% of 10 | 89% of 5 | 92% of 6 | 84% of 4 |
| | R=80-96% | R=80-100% | R=85-95% | R=84-90% |
| Alex | 85% of 9 | 81% of 7 | 89% of 3 | 82% of 3 |
| | R=80-98% | R=80-85% | R=82-90% | R=82-95% |
| Ben | 88% of 6 | 92% of 4 | 92% of 6 | 85% of 4 |
| | R=85-92% | R=85-100% | R=83-100% | R=85-95% |

R = range

Table 7

Interobserver Agreement Scores and the Number of Intervals for Which They Were Obtained for Correct Delivery of Parental Vocalizations Within 1 Second Across Experimental Conditions for all Infants

| | Interobserver Agreement for Correct Delivery of Parental Vocalizations Within 1 Second per Number of Intervals | | | |
|---|---|---|---|---|
| | Matching | Non-Match | Matching | Non-Match |
| Ellie | 80% of 10 | 88% of 10 | 87% of 8 | 88% of 7 |
| | R= 85-97% | R=80-100% | R=84-100% | R=88-100% |
| Joanna | 90% of 10 | 88% of 5 | 92% of 6 | 84% of 4 |
| | R=80-96% | R=80-100% | R=85-95% | R=82-90% |
| Alex | 82% of 9 | 81% of 7 | 88% of 3 | 82% of 3 |
| | R=80-98% | R=80-85% | R=82-90% | R=82-95% |
| Ben | 88% of 6 | 90% of 4 | 90% of 6 | 85% of 4 |
| | R=85-92% | R=85-100% | R=83-100% | R=85-95% |

R = range

## Results

For all infants, matching of infant vocalizations by the parent produced systematically higher vocalization rates when compared with non-matching of infant vocalizations. Additionally, these changes in vocalization rates across matching and non-matching experimental conditions were supported by the Mann-Wald time series analysis. That is, there were statistically significant differences in mean level and/or trend ($p<.05$) for all subjects across all experimental conditions. The individual results of the Mann-Wald procedure are presented in Tables 8 through 12. A more detailed description of the results obtained for one subject follows.

Figure 1 represents vocalizations per minute for consecutive 6-minute intervals for Ellie across operant level, matching, and non-matching experimental conditions. Infant vocalizations per minute are plotted on the ordinate and consecutive 6-minute intervals are plotted on the abscissa. The closed circles represent infant vocalization rate.

During operant level, data were variable, with high of 6 to a low of 2 vocalizations per minute. Within the first six intervals of the matching condition, the data increased to a high of 10 vocalizations per minute, with subsequent vocalization rates having a range between 5 and 9 vocalizations per minute by the 21$^{st}$ interval. During the non-matching condition, vocalization rates decreased. Between intervals 21 and 31, there was a low of 1 to a high of 4 vocalizations per minute. Upon the return to contingent matching, there was an immediate increase in vocalization rates from 1 for interval 31 to 6 for interval 32. Subsequent vocalization rates had a range from 2 to 7. During the second non-matching condition vocalization rates decreased from 6 for interval 42 and remained between 1 and 2 vocalizations per minute.

Table 8

Results of the MannWald Time Series Analysis for Differences in Mean Rates of Infant Vocalizations

| | Op.Lev-Match | | Match-NonM | | NonM-Match | | Match-NonM | |
|---|---|---|---|---|---|---|---|---|
| | M | M | M | M | M | M | M | M |
| Ellie | 5.5 | 8.7 * | 8.2 | 3.2 * | 3.1 | 5.1 * | 4.2 | 1.7 * |
| Joanna | 4.7 | 6.5 | 5.4 | 2.4 * | 1.7 | 3.4 | 5.2 | 2.99 * |
| Alex | 4.2 | 6.2 * | 3.6 | 2.1 * | 2.1 | 4.9 * | 6.4 | 3.6 * |
| Ben | 1.9 | 3.5 | 4.9 | 2.2 * | 3.2 | 5.4 * | 5.9 | 3.2 * |

* $p < .05$.

Table 9

Results of the MannWald Time Series Analysis for Differences in Trend for Infant Vocalization Rates

| | Op. Lev-Match Trend | Match-NonM Trend | NonM-Match Trend | Match-NonM Trend |
|---|---|---|---|---|
| Ellie | b=5.9 b=11.4 | b=10.1 b=7 | b=3.8 b=3.9 | b=3.2 b=3.3 |
| | m=-.04 m=-.16 | m=-.16 m=-.18 | m=-.15 m=-.09 | m=.19 m=-.11 |
| Joanna | b=6.4 b=-.92* | b=4.9 b=15.8 | b=6.2 b=-.98 | b=3.3 b=4.5 |
| | m=-.02m=.01* | m=.77 m=-.92* | m=-.77 m=1.2* | m=1.2 m=.09** |
| Alex | b=1.99 b=1.2 | b=3.2 b=1.5 | b=6.2 b=10.1* | b=8.6 b=19.7 * |
| | m=.27 m=.21 | m=.18 m=.08 | m=1.9 m=2.2 * | m=2.7m=-1.2** |
| Ben | b=8.8 b=.83 * | b=3.4 b=-.4 | b=2.4 b=1.8 | b=4.7 b=-3.3 |
| | m=-1.6m=.45* | m=.42 m=.20 | m=.36 m=.33 | m=.27 m=.40 |

* $p < .05$.

b = y intercept

m = slope

Table 10

Summary of the MannWald Time Series Results for Each Infant

| | Op. Lev-Match | Match-NonM | NonM-Match | Match-NonM |
|---|---|---|---|---|
| Ellie | M* | M* | M* | M* |
| Joanna | | M* | | M* |
| | T* | T* | T* | T* |
| Alex | M* | M* | M* | M* |
| | | | T* | T* |
| Ben | T* | M* | M* | M* |

* $p < .05$.

M = significant difference in mean level

T = significant difference in trend

Table 11

Results of the MannWald Time Series Analysis for Differences in Mean Duration of Infant Vocalizations

| | Op.Lev-Match | | Match-NonM | | NonM-Match | | Match-NonM | |
|---|---|---|---|---|---|---|---|---|
| | M | M | M | M | M | M | M | M |
| Ellie | .38 | .63 | 1.7 | 4.1* | 5.4 | 1.6* | 1.7 | 2.5 |
| Joanna | 2.3 | 3.1 | 3.1 | .80 | 1.9 | 2 | 2.3 | 3.9 |
| Alex | 1.1 | .92 | 1.5 | 2 | 1.7 | 1.3 | 1.3 | 1.7 |
| Ben | -.33 | .15 | 2 | 4.2 | 2.3 | 1.9 | 1.7 | 1.9 |

* $p < .05$.

Table 12

Results of the MannWald Time Series Analysis for Differences in Trend for Mean Duration of Infant Vocalizations

| | Op. Lev-Match Trend | Match-NonM Trend | NonM-Match Trend | Match-NonM Trend |
|---|---|---|---|---|
| Ellie | b=.76 b=.002 m=.004 m=.07 | b=.61 b=5.4 m=.15 m=.005 | b=6.4 b=1.2 m=-.25 m=.002 | b=1.9 b=4.9 m=.001 m=-.15 |
| Joanna | b=2 b=.8 m=.13 m=.22 | b=2.8 b=.94 m=.14 m=-.49 | b=4.5 b=-.32 m=-.67 m=-.58 | b=2.3 b=.45 m=.28 m=.6 |
| Alex | b=1.4 b=1.1 m=.004 m=.004 | b=-1.3 b=1.8 m=.001 m=.001 | b=1.5 b= 1.5 m=.004 m=.002 | b=1.3 b=.95 m=0 m=.007 |
| Ben | b=.009 b=-1.9 m=-.1 m=.18 | b=2 b=-9.5 m=.001 m=.84 | b=2.7 b=1.6 m=.002 m=.002 | b=1.7 b=.74 m=.001 m=.001 |

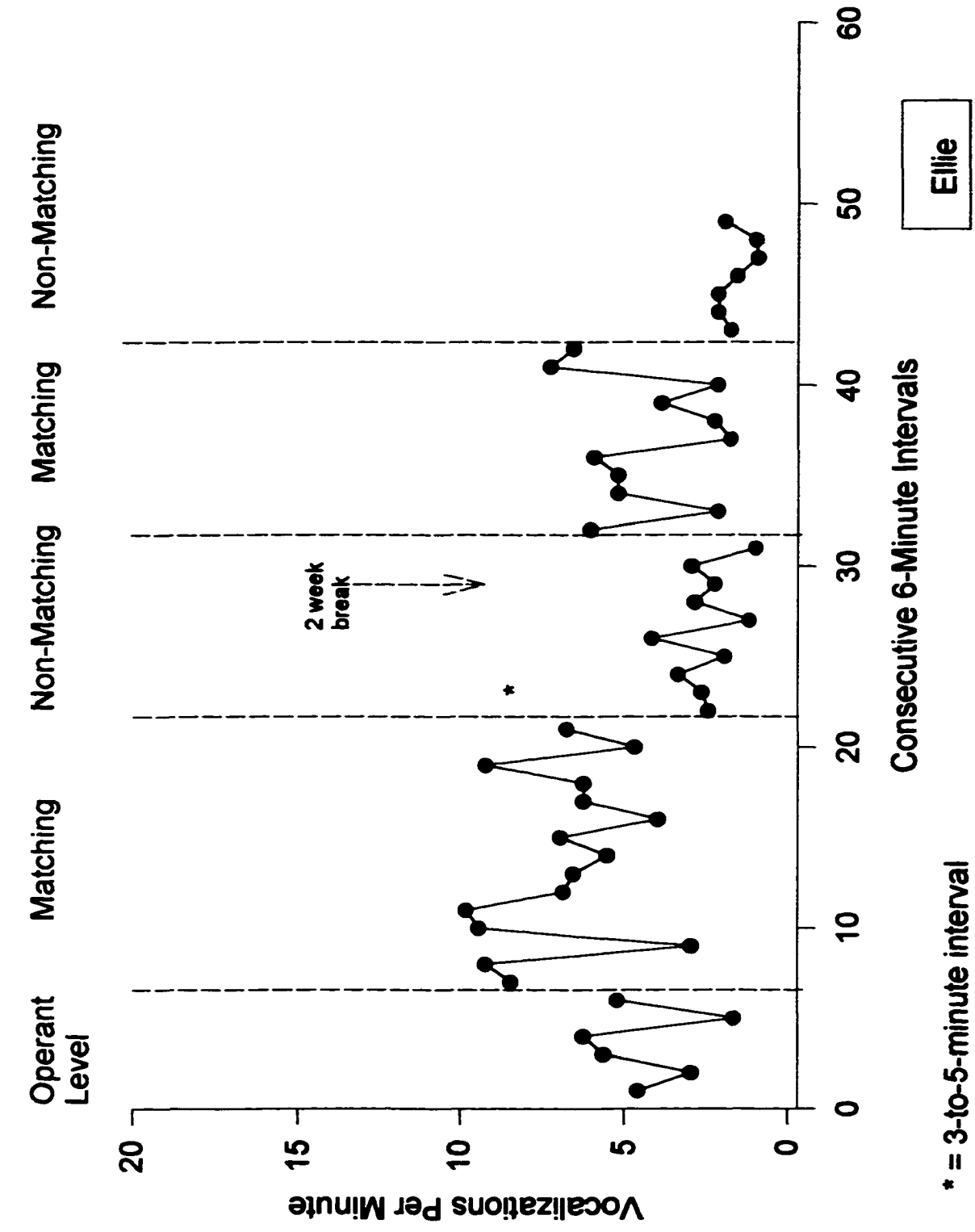
* $p < .05$.

b = y intercept

m = slope

## Figure Caption

Figure 1. **Vocalizations per minute for Ellie during operant level, parental matching, and parental non-matching experimental conditions for consecutive 6-minute intervals.**



Furthermore, these results for Ellie were supported by the Mann-Wald analysis. That is, there were significant differences in mean level across all experimental conditions.

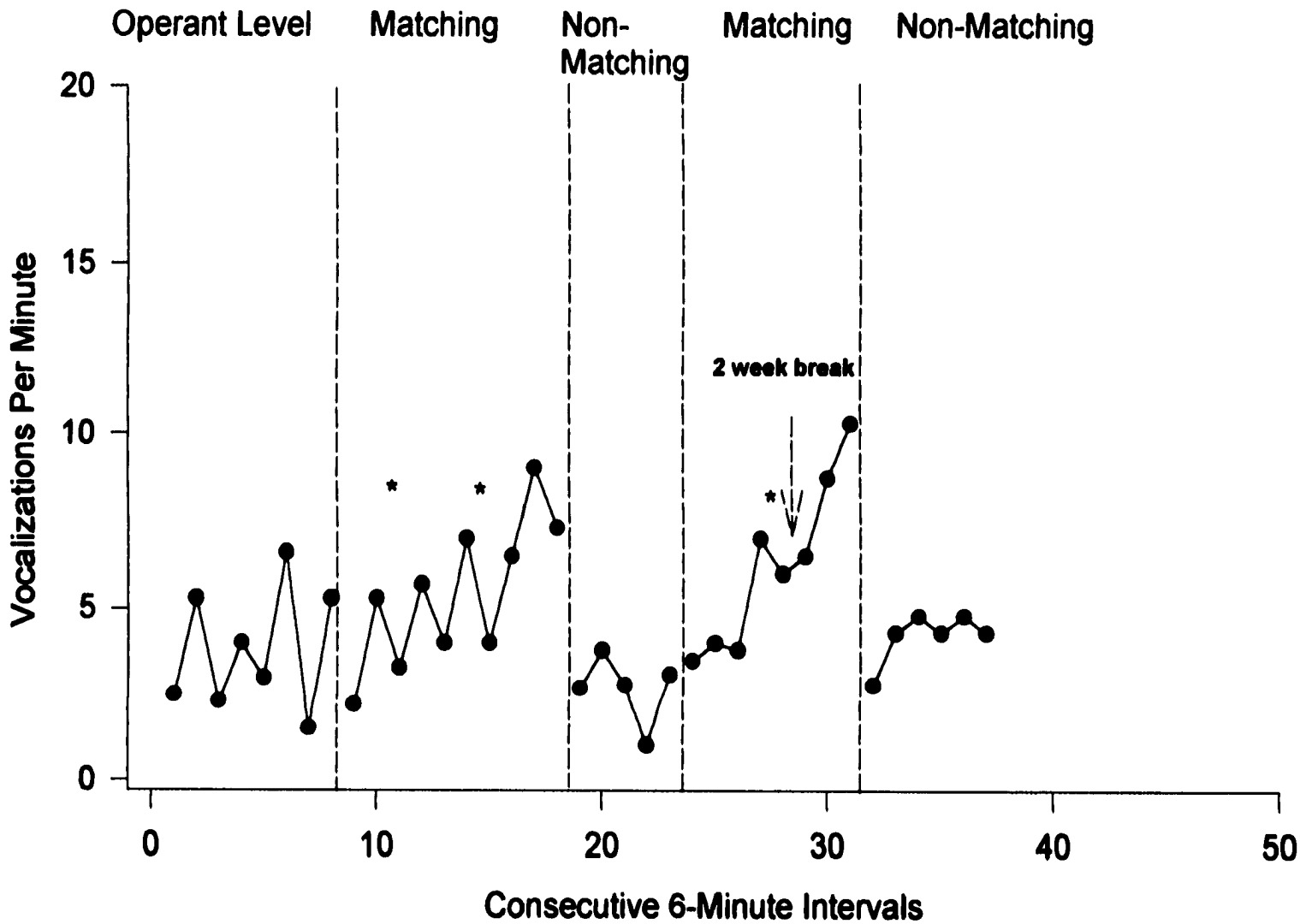
Figures 2, 3, and 4 represent vocalizations per minute for consecutive 6-minute intervals for Joanna, Alex, and Ben across operant level, parental matching, and parental non-matching experimental conditions. Alex's data are similar to those of Ellie in which significant differences were observed in mean level across all experimental conditions. Furthermore, with the exception of the comparison between operant level and matching, Ben's data also yielded significant differences in mean level. On the other hand, Joanna's data indicated significant differences in mean level only between matching and non-matching experimental conditions.

Therefore, although differences in mean levels of data can be observed for all infants, in which matching of infant vocalizations by the parent produced systematically higher vocalization rates when compared with non-matching of infant vocalizations, statistically significant differences were not observed across all experimental conditions for Joanna and Ben.

Additionally, particular acquisition patterns were observed in the data. In three of the four opportunities ( Joanna, Alex, and Ben), there was a gradual increase in vocalization rates between operant level to matching; and in four out of four opportunities ( for all infants), there was a gradual increase between non-matching to matching.

## Figure Caption

Figure 2. Vocalizations per minute for Joanna during operant level, parental matching, and parental non-matching experimental conditions for consecutive 6-minute intervals.

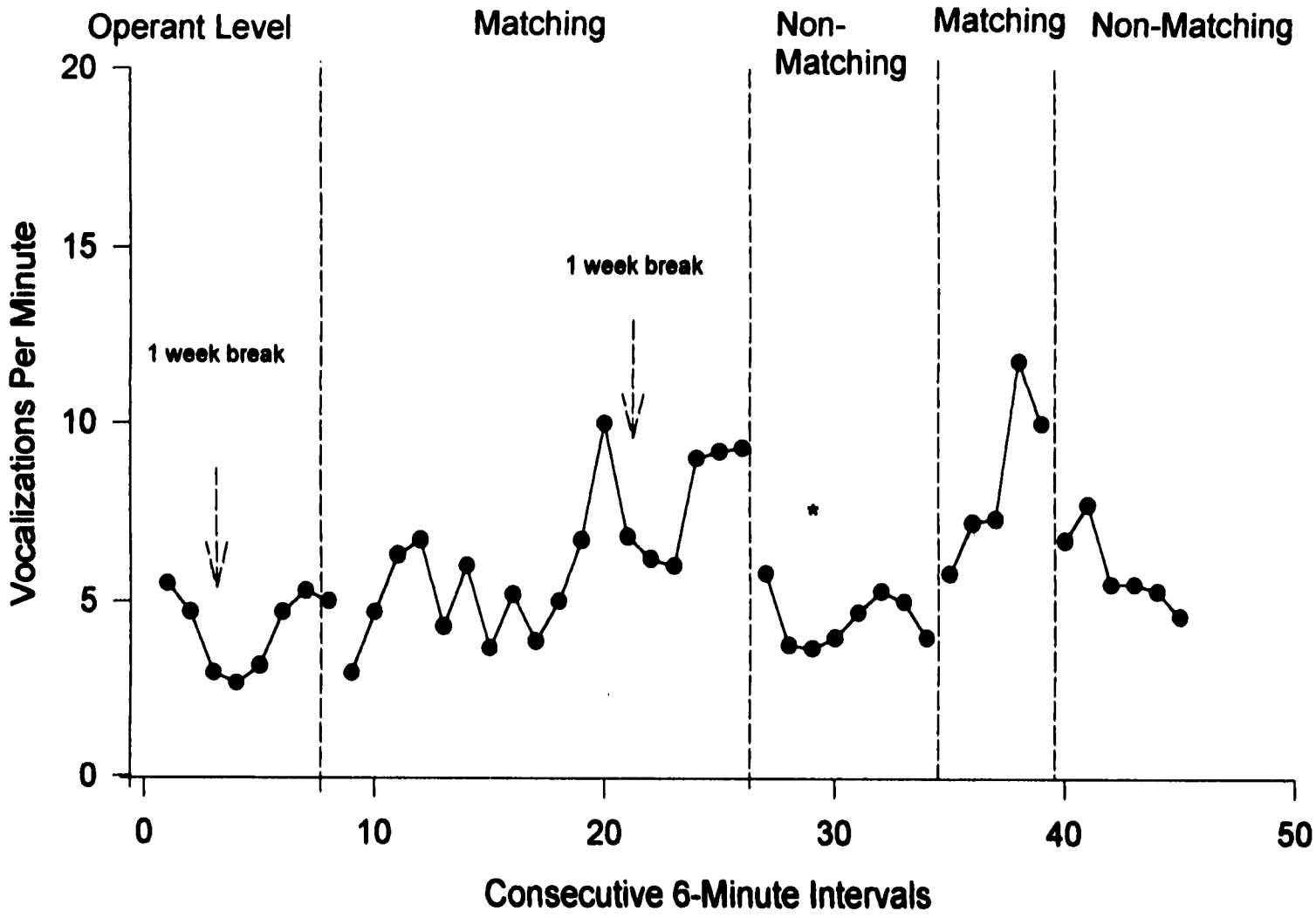



* = 3-to-5-minute interval

Joanna

## Figure Caption

Figure 3. Vocalizations per minute for Alex during operant level, parental matching, and parental non-matching experimental conditions for consecutive 6-minute intervals.

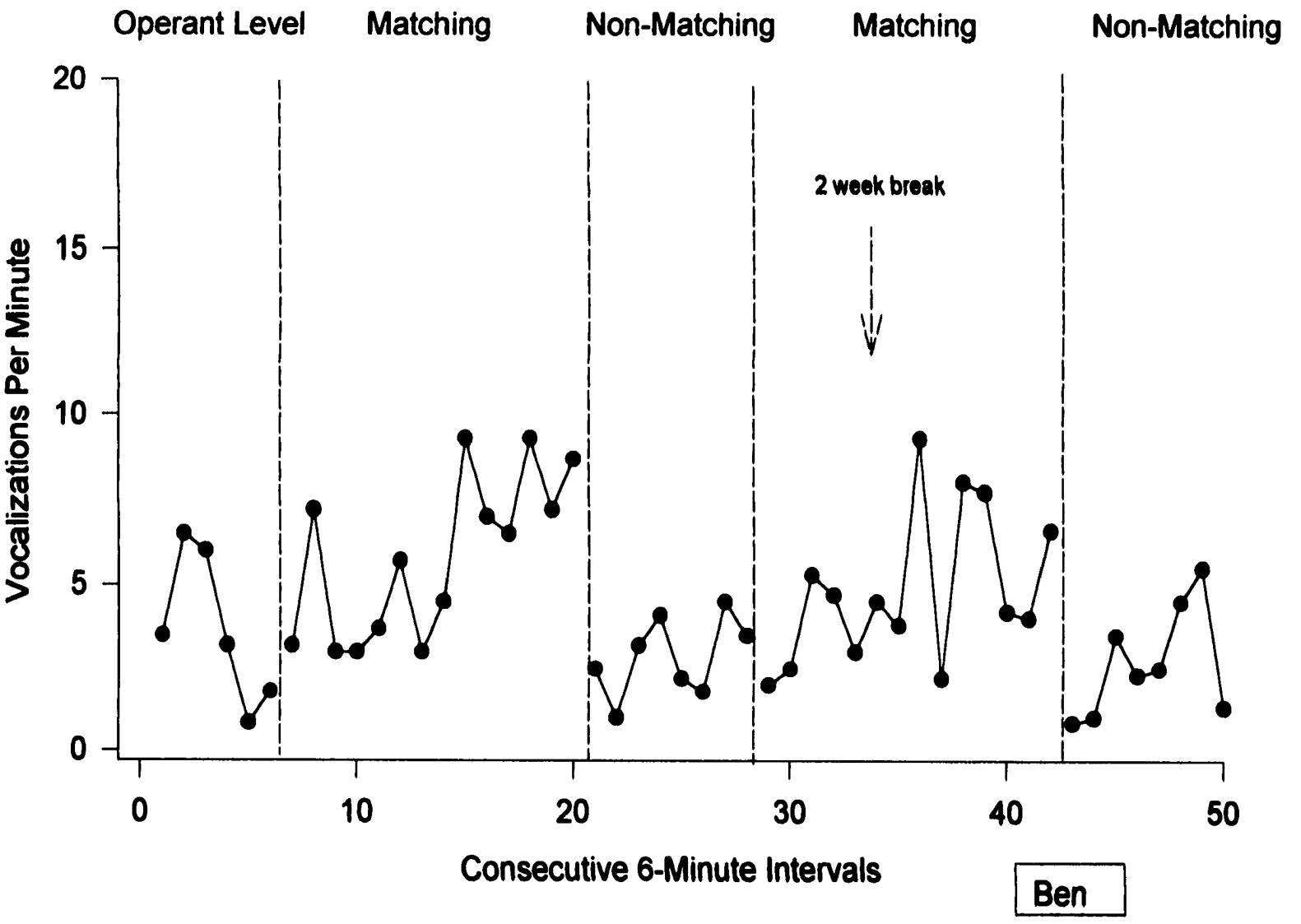



* = 3-to-5-minute interval

Alex

## Figure Caption

Figure 4. **Vocalizations per minute for Ben during operant level, parental matching, and parental non-matching experimental conditions for consecutive 6-minute intervals.**



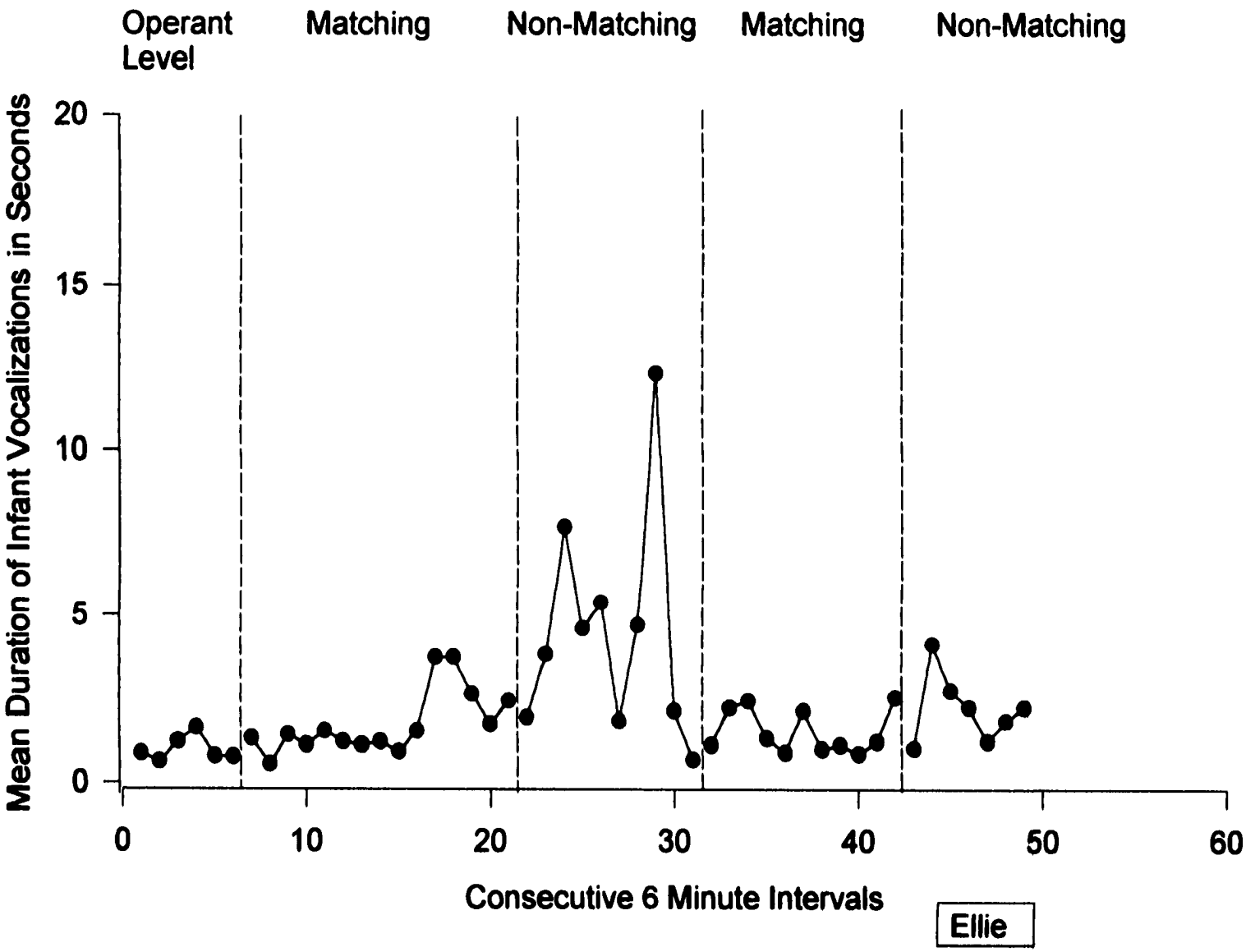
Because Ellie's data were more variable, it was harder to characterize them; however, her change could be considered gradual because almost half of her data points overlap with the matching condition. Nevertheless, only Joanna and Ben showed a significant difference in trend between operant level and matching; and furthermore, only Joanna and Alex showed a significant difference from non-matching to matching. In eight out of eight opportunities, a precipitous drop in vocalization rates from matching to non-matching conditions was observed. These results, however, were only statistically significant for Joanna. For Alex, there was a significant difference in trend for only one out of the two opportunities.

The duration data for Joanna, Alex, and Ben did not reveal any systematic differences in mean duration per 6-minute interval for parental matching and non-matching experimental conditions. Only Ellie's data showed a significant difference in the mean duration of infant vocalizations during the first matching condition and non-matching conditions, and from the first non-matching and matching conditions ( see Figures 5,6,7,8). However, there were no significant differences between operant level and the matching conditions, or from the last matching and non-matching conditions.

The mean duration of Ellie's vocalizations during all intervals of operant level and during the first ten intervals of the matching condition had a range between .5 to 1.5 seconds. The last five data points during the matching condition, however, increased to a high of 4 seconds and then stabilized to between 2 and 3 seconds. With the introduction

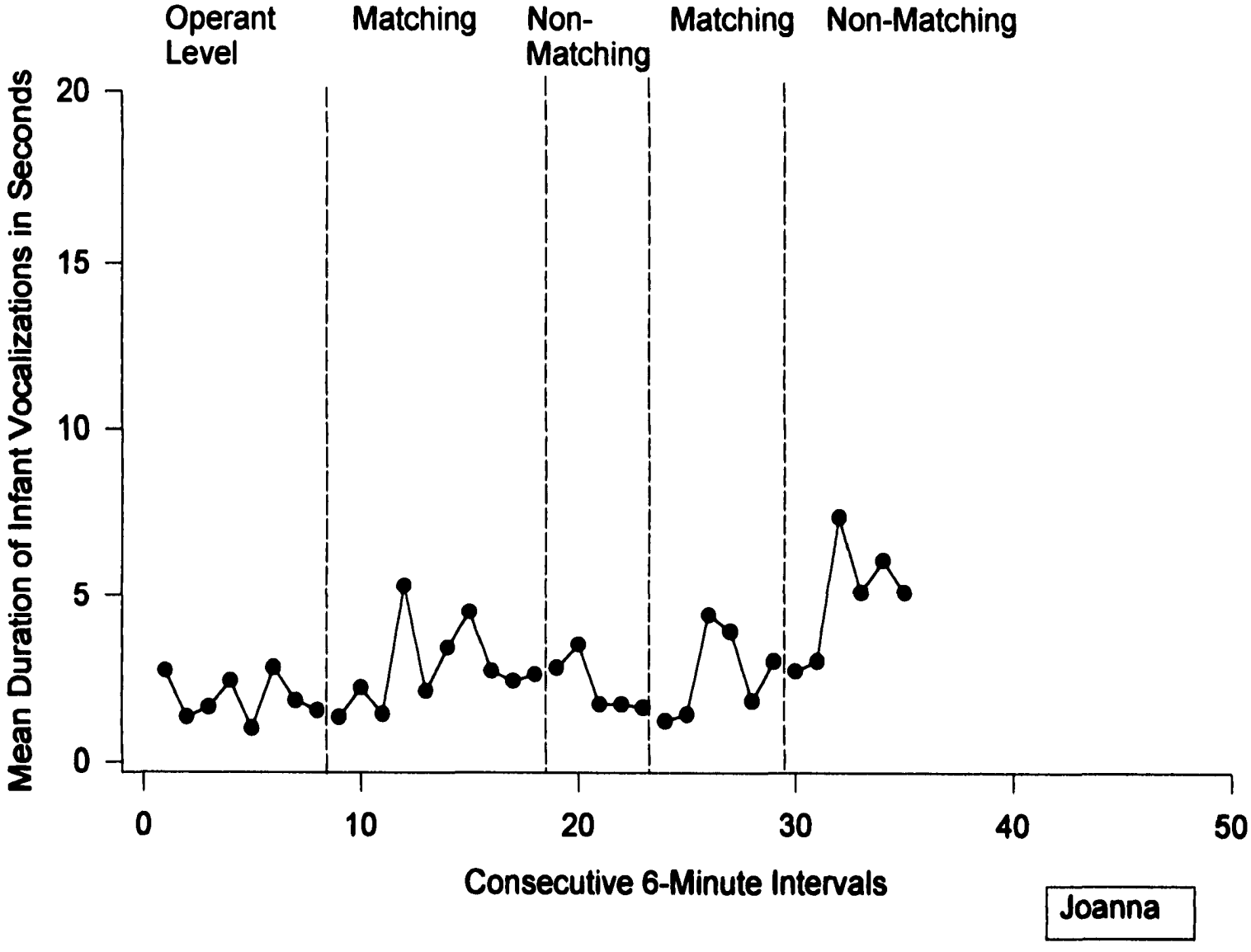
## Figure Caption

Figure 5. **Mean duration of infant vocalizations in seconds for Ellie during operant level, parental matching, and parental non-matching experimental conditions for consecutive 6-minute intervals.**



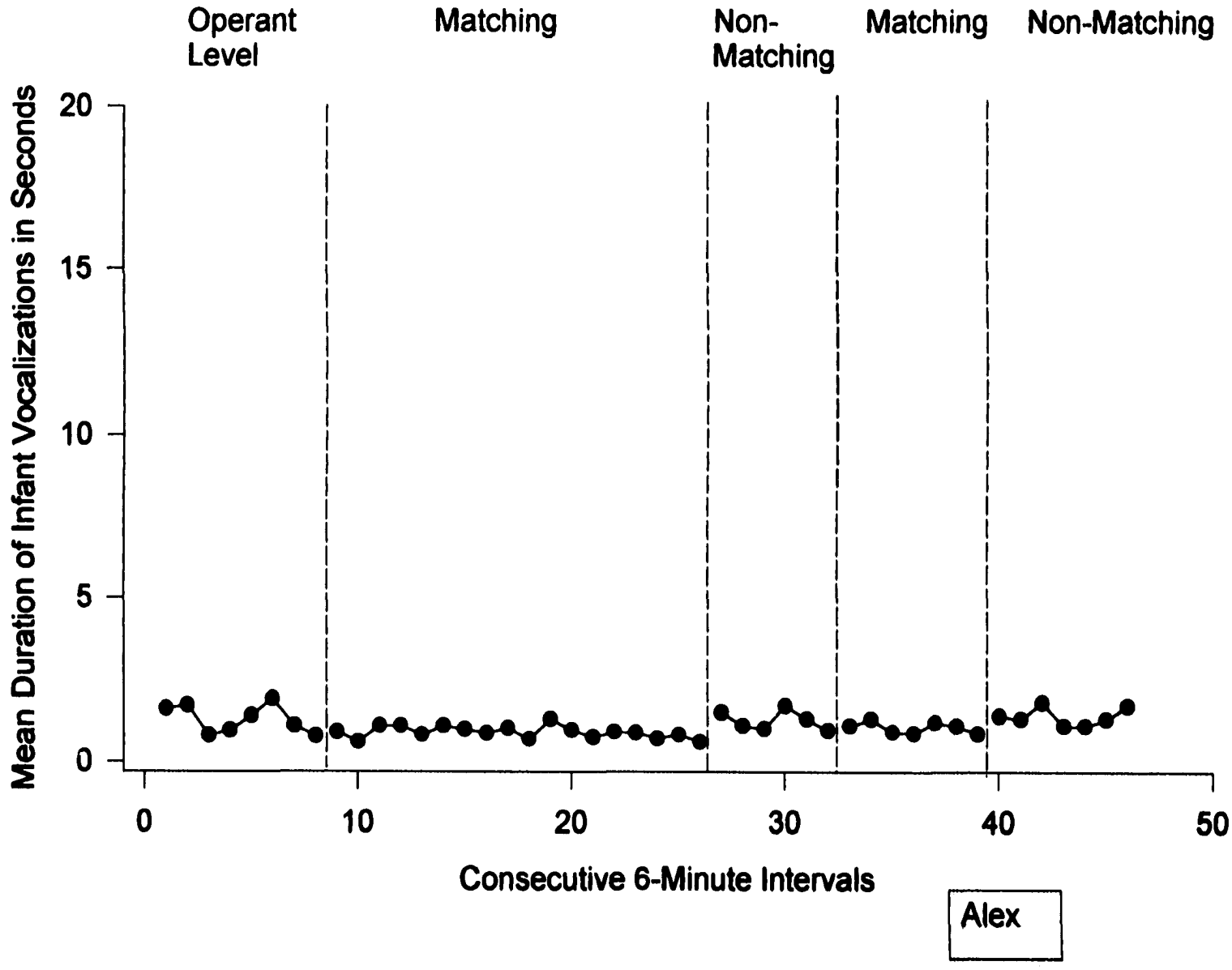
## Figure Caption

Figure 6. Mean duration of infant vocalizations in seconds for Joanna during operant level, parental matching, and parental non-matching experimental conditions for consecutive 6-minute intervals.



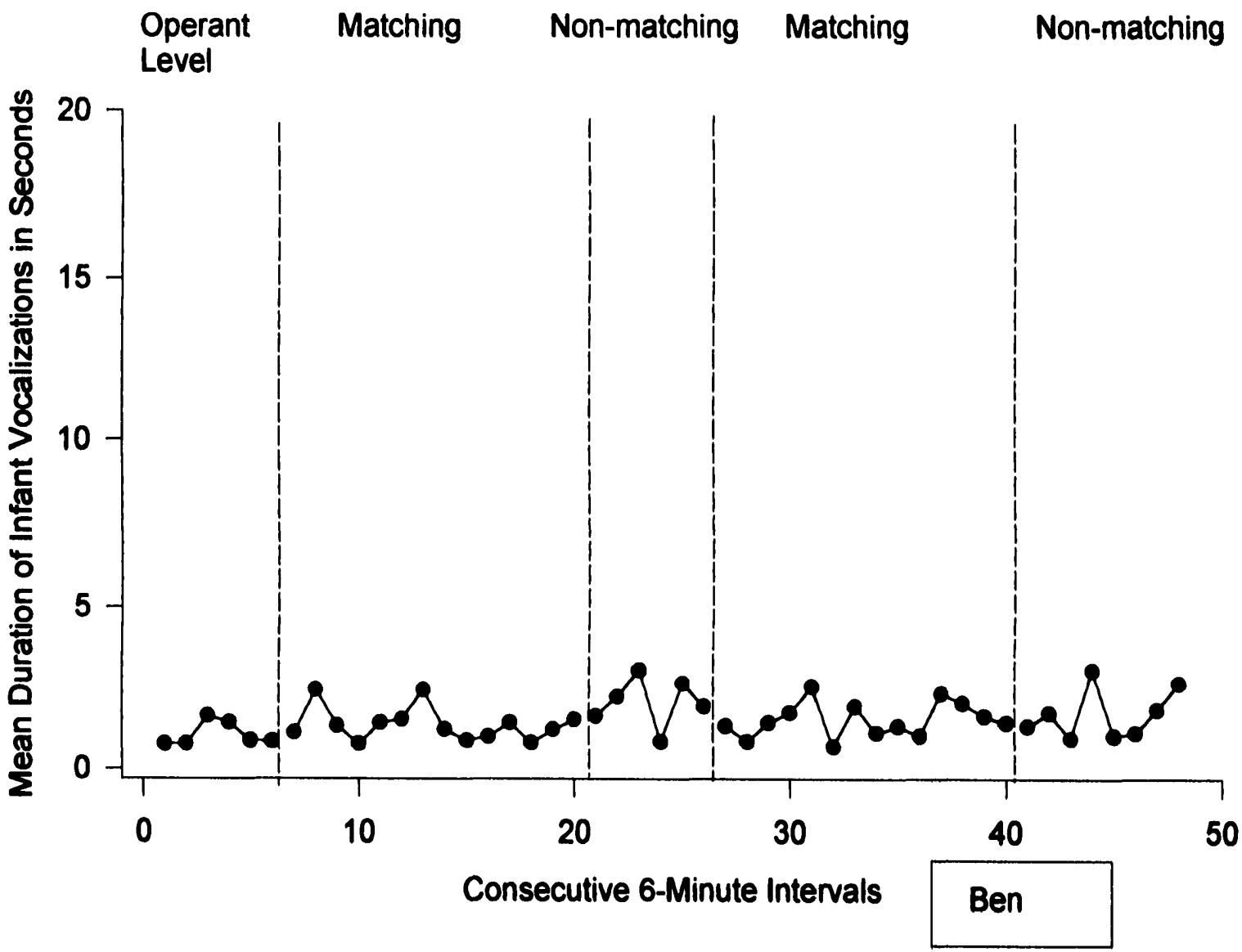
## Figure Caption

Figure 7. Mean duration of infant vocalizations in seconds for Alex during operant level, parental matching, and parental non-matching experimental conditions for consecutive 6-minute intervals.



## Figure Caption

Figure 8. Mean duration of infant vocalizations in seconds for Ben during operant level, parental matching, and parental non-matching experimental conditions for consecutive 6-minute intervals.



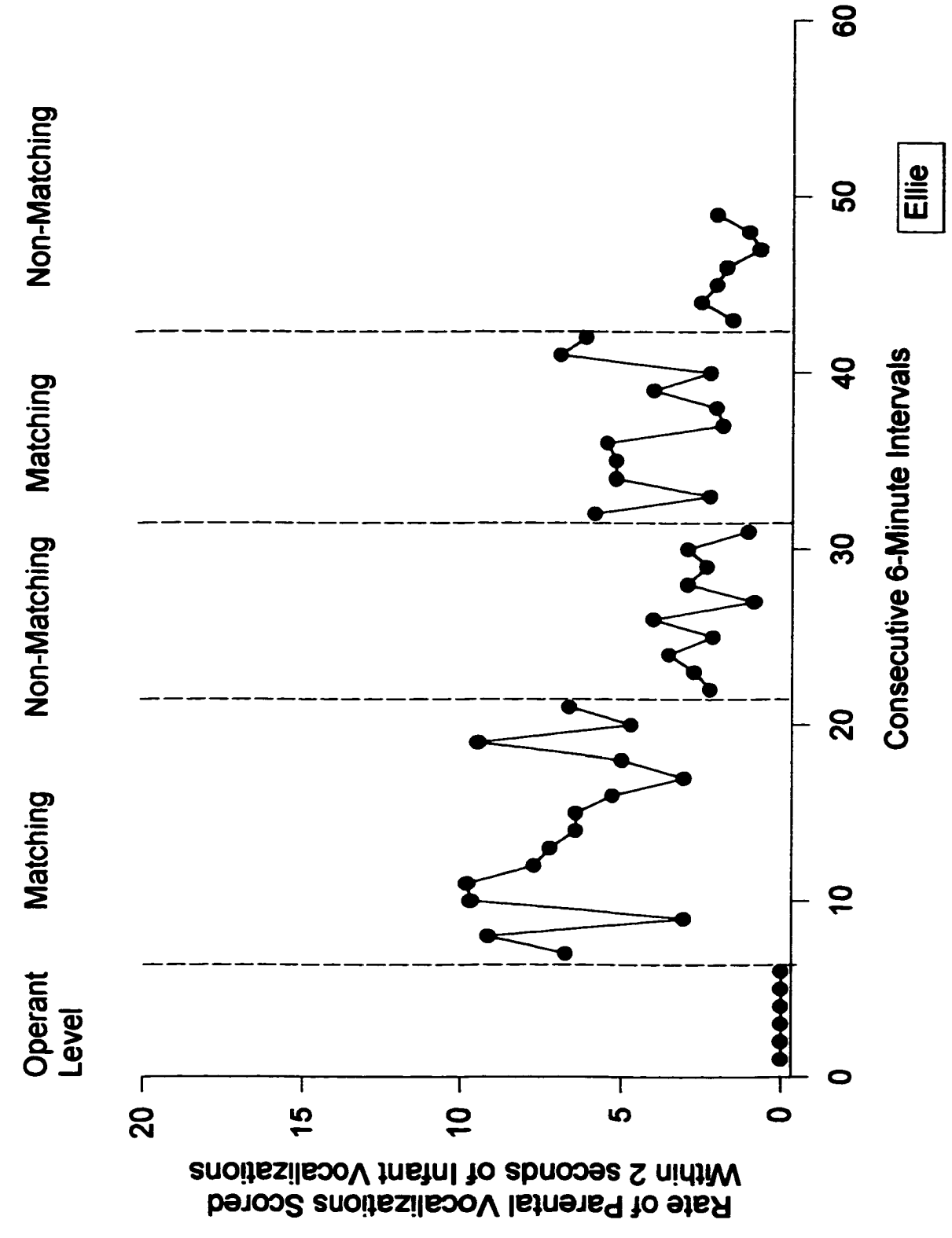
of the non-matching condition, the mean duration of infant vocalizations increased from 2 seconds for interval 22, to a high of 12 seconds for interval 29. Then the mean duration decreased to 1 second for interval 31. With the implementation of the second matching condition, the data were lower, with a range between 1 to 3 seconds. There were no significant differences in mean duration with the return to the non-matching condition.

Figures 9 through 12 indicate the rate of parental vocalizations scored as a match or a non-match within 2 seconds of the occurrence of infant vocalizations across experimental conditions. When one compares those figures with those representing infant vocalization rates, it becomes apparent that for each subject, infant and parent vocalizations are roughly equal.

To determine the extent to which infant vocalizations were followed by parental vocalizations within 2 seconds, a contingency analysis was performed. The percentages of infant vocalizations followed by parental vocalizations within 2 seconds and the percentages of parental vocalizations not preceded by infant vocalizations within 2 seconds are presented for each infant in Table 13. The percentages of infant vocalizations which were followed by a parental matching or non-matching vocalizations were systematically higher when compared with the percentages of parental vocalizations not preceded by infant vocalizations within 2 seconds. Thus, the experimental procedure was delivered as intended for each infant.

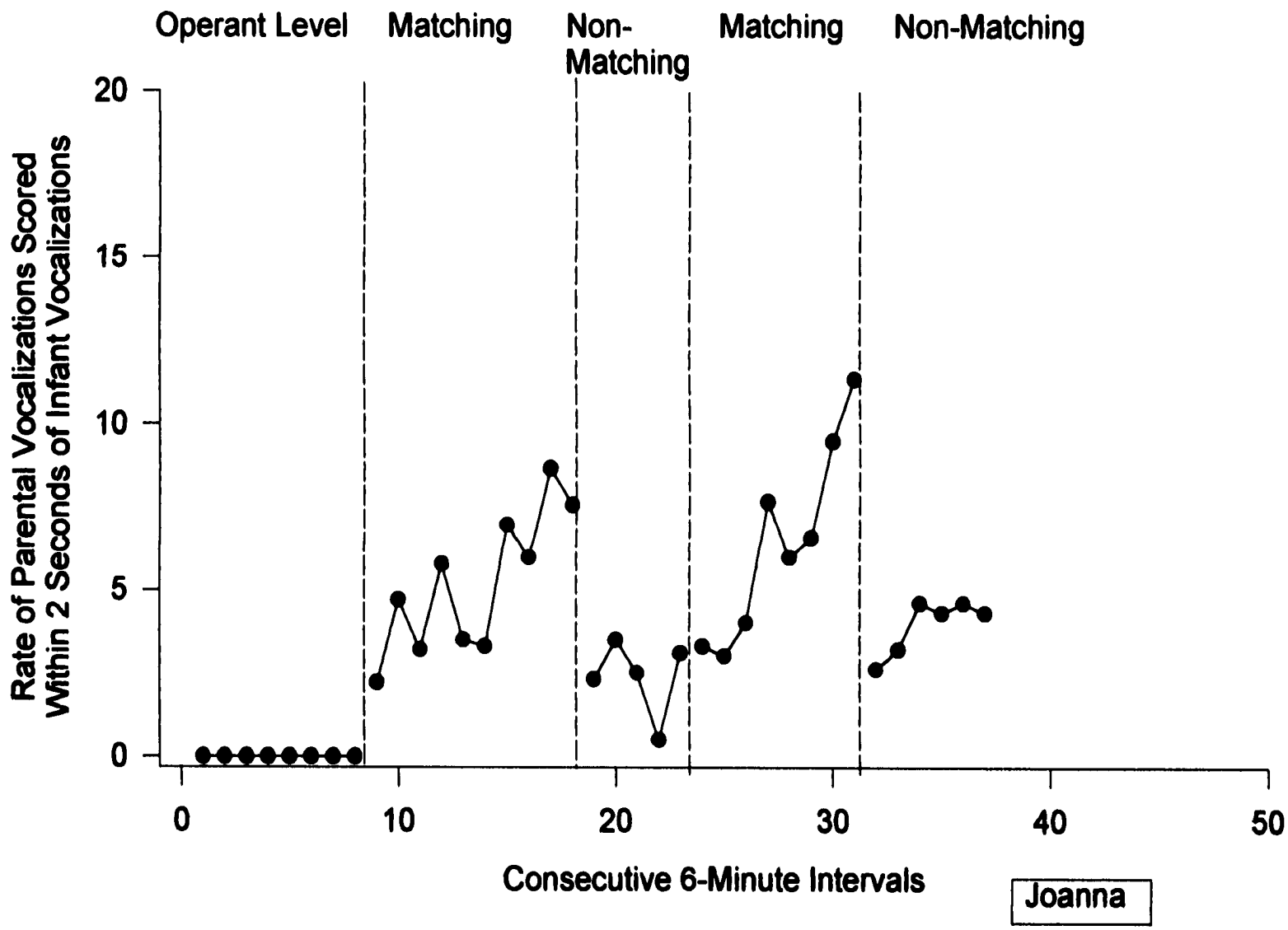
## Figure Caption

Figure 9. **Rate of parental vocalizations scored as a match or a non-match within 2 seconds of infant vocalizations for Ellie during operant level, parental matching, and parental non-matching experimental conditions for consecutive 6-minute intervals.**



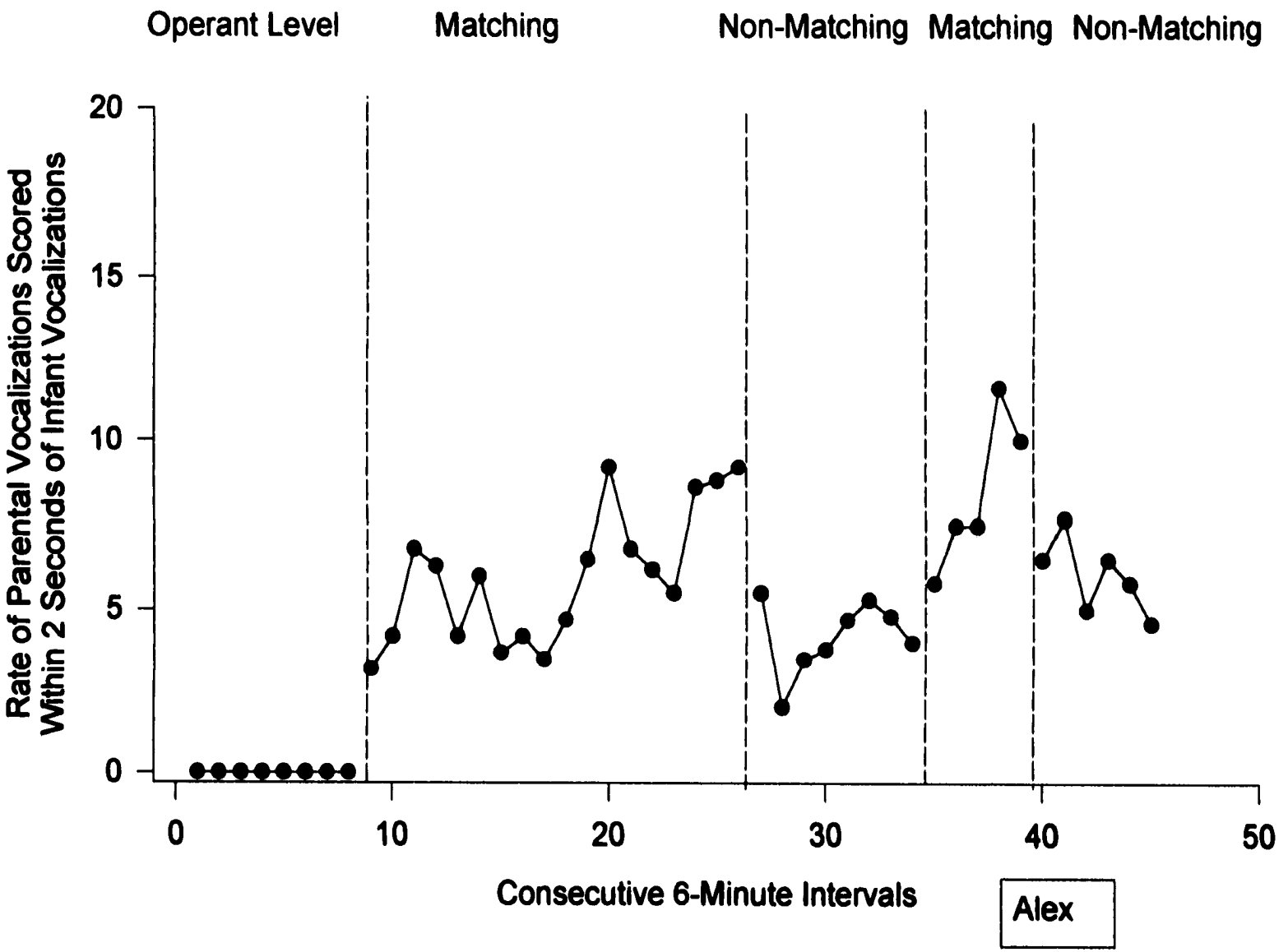
## Figure Caption

Figure 10. **Rate of parental vocalizations scored as a match or a non-match within 2 seconds of infant vocalizations for Joanna during operant level, parental matching, and parental non-matching experimental conditions for consecutive 6-minute intervals.**



## Figure Caption

Figure 11. Rate of parental vocalizations scored as a match or a non-match within 2 seconds of infant vocalizations for Alex during operant level, parental matching, and parental non-matching experimental conditions for consecutive 6-minute intervals.



## Figure Caption

Figure 12. **Rate of parental vocalizations scored as a match or a non-match within 2 seconds of infant vocalizations for Ben during operant level, parental matching, and parental non-matching experimental conditions for consecutive 6-minute intervals.**

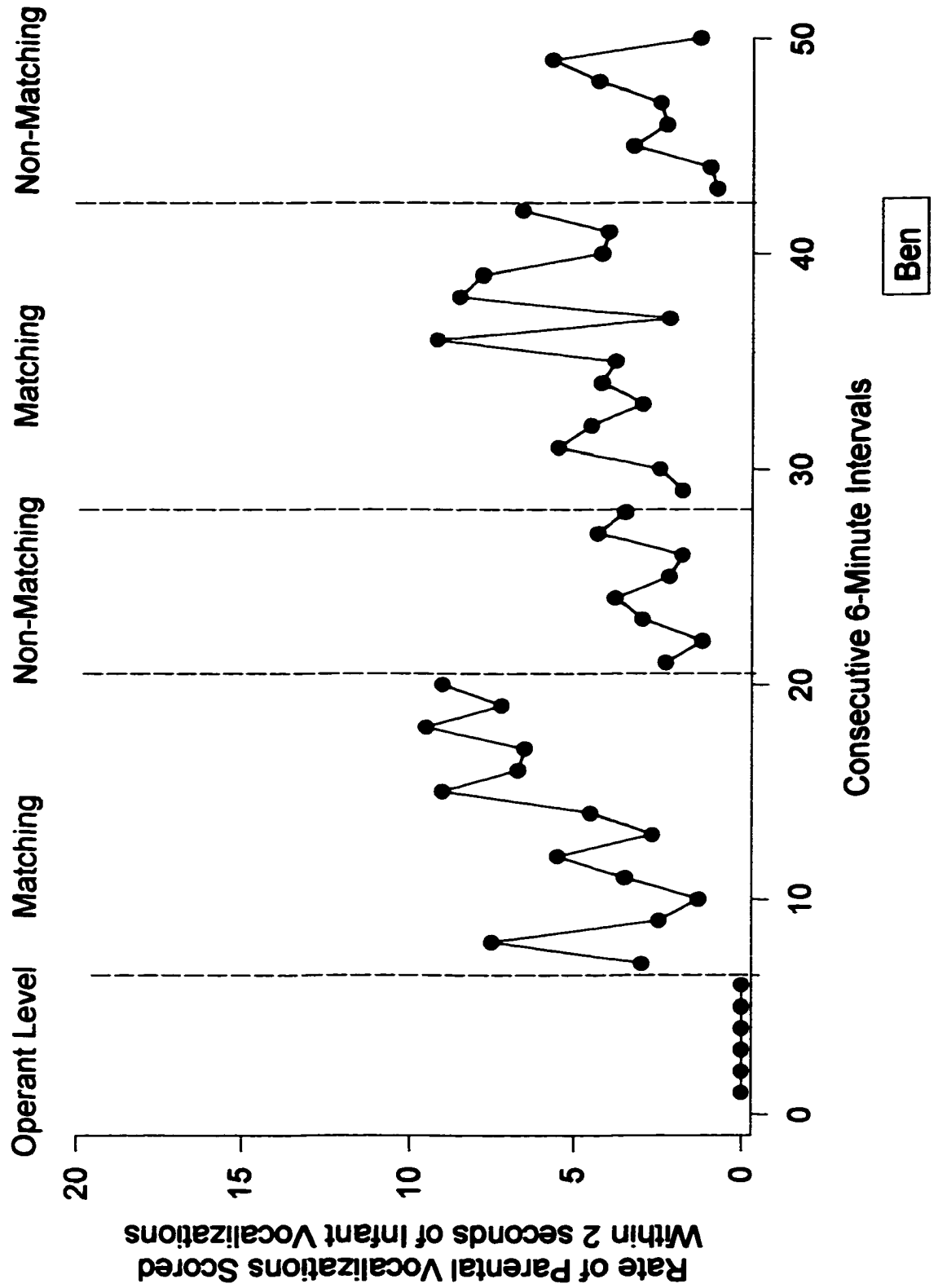


Table 13

Percentage of Infant Vocalizations Followed by Parental Matching or Parental Non-Matching Vocalizations Within 2 Seconds and the Percentage of Parental Vocalizations Not Preceded by Infant Vocalizations Within 2 Seconds Across Matching and Non-Matching Experimental Conditions

| | Contingency Analysis for each Experimental Condition | | | | | | | |
|---|---|---|---|---|---|---|---|---|
| | Matching | | Non-Matching | | Matching | | Non-Matching | |
| | * | ** | * | ** | * | ** | * | ** |
| | Infant Vocs | Parent Vocs | Infant Vocs | Parent Vocs | Infant Vocs | Parent Vocs | Infant Vocs | Parent Vocs |
| Ellie | 94% | 4% | 91% | 4% | 95% | 1% | 90% | 4% |
| Joanna | 95% | 3% | 88% | 6% | 91% | 2% | 90% | 3% |
| Alex | 92% | .2% | 93% | 1% | 97% | 0% | 93% | .5% |
| Ben | 93% | 3% | 92% | 5% | 93% | 2% | 91% | 4% |

* = percentage of infant vocalizations followed by parental matching or non-matching vocalizations within 2 seconds

** = percentage of parental matching or non-matching vocalizations not preceded by infant vocalizations within 2 seconds

Furthermore, when one compares the contingency data for matching and non-matching experimental conditions, there was no delay in the delivery of parental reinforcement. For the matching experimental conditions, the mean percentage of infant vocalizations followed by parental vocalizations within 2 seconds across infants was 94%. For the non-matching experimental conditions, the mean percentage was 91%.

Because it was observed that there was a sharp drop in infant vocalizations per minute from the end of the matching condition to the beginning of the non-matching condition in seven out of eight opportunities, it was important to further rule out any delay of parental responding during the non-matching condition. Therefore, the percentages of infant vocalizations followed by parental vocalizations within 1 second were also calculated (see Table 14). For the matching conditions, the mean percentage of infant vocalizations followed by parental vocalizations within 1 second was 92%. For the non-matching conditions, the mean percentage was 89%.

## Discussion

Because there was a systematic increase in infant vocalization rates with the introduction of parental vocal similarity, one may conclude that similarity caused the increase in infant vocalization rate. Furthermore, because of the systematic differences in acquisition patterns between the introduction and removal of the similarity condition, one may conclude that the similarity conditioned functioned as a reinforcement condition. That is, there was a gradual increase in infant vocalization rate with the introduction of parental vocal similarity and an abrupt decrease in infant vocalization rate with the removal of the similarity condition. As stated in the introduction, Herrnstein's (1970)

Table 14

Percentage of Infant Vocalizations Followed by Parental Matching or Parental Non-Matching Vocalizations Within 1 Second and the Percentage of Parental Vocalizations Not Preceded by Infant Vocalizations Within 1 Second Across Matching and Non-Matching Experimental Conditions

| | Contingency Analysis for Each Experimental Condition | | | | | | | |
|---|---|---|---|---|---|---|---|---|
| | Matching | | Non-Matching | | Matching | | Non-Matching | |
| | * | ** | * | ** | * | ** | * | ** |
| | Infant Vocs | Parent Vocs | Infant Vocs | Parent Vocs | Infant Vocs | Parent Vocs | Infant Vocs | Parent Vocs |
| Ellie | 94% | 4% | 89% | 7% | 95% | 1% | 90% | 5% |
| Joanna | 92% | 5% | 83% | 16% | 88% | 10% | 88% | 5% |
| Alex | 90% | 1% | 91% | 2% | 95% | 3% | 90% | 3% |
| Ben | 91% | 5% | 90% | 4% | 93% | 2% | 90% | 6% |

* = percentage of infant vocalizations followed by parental matching or non-matching vocalizations within 1 second

** = percentage of parental matching or non-matching vocalizations not preceded by infant vocalizations within 1 second

matching law predicts that a target response is influenced not only by the programmed reinforcers contingent upon it, but also by other response alternatives and their associated reinforcers in the environment. Thus, when the vocal matching is introduced, non-vocal infant may conclude that the similarity condition functioned as a reinforcement condition. That responses may be occurring as well. Eventually, during the matching condition, parental vocal similarity may have selected infant vocalizations over other responses. Thus, the increase of vocalization rate would have been expected to be gradual.

There is empirical support for Risley's contention that parental vocalizations can provide some level of direct reinforcement for infant vocalizations. For example, the results of descriptive research by authors such as Hardy-Brown et al. (1981) and Moran et al. (1987) corroborate Risley's theory. They reported that parents, in the course of their daily interactions, often speak to and imitate their infants. Therefore, the likelihood is greater that parental vocalizations would be paired with other reinforcers and acquire reinforcing value.

Risley's own research provides further support for the position that parents influence the rate of infant vocalizations and overall language development. The results of Hart and Risley's longitudinal research (1995) allowed them to conclude that parent-child vocal interaction affects the trajectory of the child's language acquisition. For example, one of their findings indicated that the amount of time that parents talk to their children is positively correlated with rate of vocabulary growth.

The results of the present study support findings reported by other researchers that behavioral similarity is more reinforcing than behavioral dissimilarity for children with normal development. For example, Miller and Morris (1974) and Gladstone and Cooley

(1975) demonstrated that behavioral similarity in the form of adult matching of motor responses functions as a reinforcer for the behavior of preschoolers. That is, when children are imitated by an adult, they exhibit higher levels of responding than non-imitation by an adult.

Results from the present study may also apply to children with developmental disabilities. For example, Hallahan et al. (1977) showed that adult imitation of the subjects' motor responses increased the children's imitative responses. However, contrary to the results of previous studies, research reported by Kauffman et al. (1975), demonstrated that behavioral similarity in the form of imitation of a child's inappropriately fast eating behavior resulted in a decrease in this behavior. Although this child was not of typical development, the study raises the question concerning the conditions under which behavioral similarity does and does not have a reinforcing function. It may be that children with developmental delays have different reinforcement histories when compared with typically developing children because, for example, similarity is not paired frequently with other reinforcers. Another possible explanation may be related to the nature of the response being matched. For example, imitation of inappropriate behavior may be more punishing than imitation of other kinds of behavior.

These present results also support the findings of previous infant studies in which imitation by an adult of infant vocalizations served to increase infant vocalizations (e.g., Hamilton, 1972; Haugan and McIntire, 1972; Pelaez, 1999). Nevertheless, in the infant studies above, systematic changes were not demonstrated for individual infants, but across groups of infants. In the present study, they were demonstrated within single-subject experimental designs.

In the present study, it might be assumed that similarity functioned as a reinforcer. Nevertheless, it is possible that the behavioral similarity produced during the parental matching condition elicited infant vocalizations. Similarly, under such conditions, the absence of behavioral similarity during the parental non-matching condition would have resulted in lower vocalization rates. Although there were systematic differences in vocalizations per minute across experimental conditions, one must be cautious in drawing the conclusion that behavioral similarity acquired reinforcer control over infant vocalizations. To determine whether this was the case, one could implement an experimental control procedure such as a noncontingent delivery of parental vocalizations. This procedure would keep the number of parental vocalizations constant, while disrupting the positive contingency between infant vocalizations and behavioral similarity.

Although it would have been highly desirable to implement a noncontingent procedure, it was not possible to do so with the subjects of this research. The mothers of Joanna, Alex, and Ben were unable to continue their participation because of other commitments that prevented them from coming to the laboratory regularly. As for Ellie, her vocalization rates during the last non-matching condition were already low; therefore, it would have been unlikely that the implementation of a noncontingent procedure would have produced even lower rates. Typically, with a noncontingent procedure, one would expect that vocalization rates would be lower than the rates obtained under matching and non-matching conditions. The reason is that under a noncontingent procedure, the putative reinforcers are presented in a response-independent fashion. The occurrence and the non-occurrence of the target response have an equal probability of being followed contingently by the putative reinforcer. To determine whether changes in response

strength are the result of the contingencies of reinforcement, it is essential to rule out the possibility that the same change in response strength occurred because of an elicitative or discriminative function of the presumptive reinforcing stimuli.

Nevertheless, the results of other research by Poulson (1983), Reeve, Reeve, Brown, Brown & Poulson (1992) and Reeve, Reeve, and Poulson (1993) have demonstrated that infant vocalizations can be operantly conditioned with social stimulation delivered by a parent. The researchers of those studies were able to draw that conclusion because of their use of appropriate experimental-control procedures that made it unlikely that infant vocalizations were being elicited by parental social stimulation. It seems reasonable, therefore, for one to assume that in this current research, parental matching vocalizations (a type of social reinforcer) delivered immediately after infant vocalizations, functioned as a conditioned reinforcer.

Non-matching was introduced only after that selection of vocal behavior was accomplished. Because infant vocalization rate had been continuously reinforced during the matching condition by the contingent production of parental vocal similarity, the introduction of the non-matching contingency might have been highly discriminable, as are many extinction conditions. Therefore, the non-matching condition might have produced clear discriminative stimuli that signaled the unavailability of parental vocal similarity. The discriminability of the non-matching condition following the matching condition may have produced an immediate decrease in responding. Because the procedure was not automated, however, it is always possible that some extraneous variable could have been operating.

The absence of an extinction burst may merely underscore the highly discriminable characteristics of the two procedures, given that the infant was already vocalizing at a high rate during the matching condition. The duration data for Ellie suggest that she experienced an extinction burst with the implementation of the first non-matching condition. Although rates of vocalization, the primary measure in this experiment, were not affected, Ellie produced a significant increase in mean duration of vocalizations during this non-matching condition when compared with the mean durations during the matching conditions. Nevertheless, there were no systematic differences in the pattern of mean durations of infant vocalizations for Ellie, Ben, or for the other subjects.

Additionally, there was an increase in infant vocalization rates over operant level for all infants when the similarity condition was introduced. This is also consistent with the functions of reinforcement, especially given the increase was gradual, rather than abrupt, for three of the four infants. A gradual increase suggests reinforcement functions, whereas an abrupt increase suggests elicitation.

These findings are consistent with Mowrer's (1960) theory that the similarity between infant and parent vocalizations has reinforcing value for infant vocal behavior. If similarity functioned as a reinforcer, then when an infant makes a similar sound to that of the parent, the similarity produced by the infant automatically reinforces more adult-like sounds in the absence of deliberate shaping by parents. For example, if an infant matches the parent's sound, then the next sound emitted by the parent may function as a discriminative stimulus for imitation of that sound. Once that sound is emitted by the infant, the similarity produced when the infant hears its own sound automatically reinforces that sound and thus increases the future probability of more adult-like sounds.

This process can be maintained for as long as some parental vocalizations are paired with primary reinforcers. This may be one of the variables that account for the acquisition of more adult-like sounds in infant babbling. Furthermore, Risley (1977) contends that automatic reinforcement is not sufficient in that account. Risley maintains that some level of direct reinforcement by parents for infant vocalizations is needed to produce a sufficiently high rate of infant vocalizations so that the relatively weaker variable of similarity can shape more adult-like infant vocalizations.

It would be desirable to replicate the present study with a variety of ratios of parental imitation per infant vocalizations. To be certain that similarity was the relevant variable, one might compare three or more experimental conditions that vary the amounts of parental matching on infant vocal behavior. If one could obtain sufficiently high vocalization rates, it would be possible to test whether greater similarity is more reinforcing than lesser similarity. For example, four similarity conditions might be compared: (a) 100% matching condition (parent matches 100% of infant vocalizations), (b) 75% matching condition (parent matches 75% of infant vocalizations), (c) 50% matching condition (parent matches 50% of infant vocalizations), and (d) 25% matching condition (parent matches 25% of infant vocalizations). If there were systematic differences in vocalization rates proportional to the value of the partial matching conditions, the argument would be strengthened that similarity was responsible for the differences in responding. As mentioned in the introduction, such a comparison was undertaken by Smeets, Kauffman, and Kleinloog (1982), in which the proportion of similarity was manipulated across groups of elementary school children. The group of children that received the greatest amount of similarity exhibited the highest levels of

motor responding. Thus, given that vocalization rates are sufficiently high, it would be reasonable to expect that this kind of experiment could be replicated with infants.

# Appendix A

## Description and Results of the Pilot Subject Research

The purpose of this research was to investigate the extent to which behavioral similarity functioned as a reinforcer for infant behavior. More specifically, the goal was to explore whether contingent matching of infant vocalizations by a parent increased infant vocalization rate.

## Method

### Subject

One typically developing infant, Becky, served as the pilot subject. Becky's mother learned of the research via word of mouth. Becky was 9 months 4 days on the first day of baseline, and was 11months 16 days on the date of her last session. The experimenter administered the Mental Development Index of the Bayley Scales of Infant Development and received an above average score (Mean = 100).

### Setting and Apparatus

Becky and her mother sat on either side of a three-paneled plywood screen (61cm x 152 cm), with a window in the middle panel (30cm x 43 cm). The window was covered with a Venetian Blind (76cmx43cm) with 2.5cm slats. Becky sat in an infant walker (Graco Walker Model # 4436MT ) during her participation in the study. One 28-V incandescent bulb with yellow crystals was positioned on the upper left hand side of the window facing the parent 11cm from the window opening. Two independent observers activated the light by pressing foot switches on the floor. Frequency of infant vocalizations and parental vocal stimuli were recorded on two portable event recorders

(S & K Products). The window blind opening and the activation of the signal light were automatically recorded with solenoid switches that depressed two keys on the primary event recorder. Two video cameras (Panasonic WV-3260/8AF) were mounted on tripods (Vidcor); one faced the parent, and the other faced the infant. Each camera was hooked up to a special-effects generator (Panasonic WJ-3500) that allowed a vertically split image to be recorded from a television monitor (Panasonic CTJ—2062R) to a video cassette recorder (Panasonic AG1820).

Procedure

The study took place in the Queens College Infant Laboratory. Upon arrival at the infant laboratory, the mother and infant sat in the reception area or at a table in the conference room. When the mother was confident that Becky was content ( i.e., dry, fed, and alert), she was brought into the experimental room. The mother placed Becky in her walker and then sat on the other side of the screen. The two observers sat on either side of the infant and did not talk nor make eye-contact with her. If Becky turned to look at either observer, the observer looked down or to the side to avoid the eye contact.

One session consisted of two 6-minute intervals; and a minimum of three sessions were scheduled per week. Total participation time in the study for Becky was 3 months. A session began when the primary observer took her seat and prompted (usually a nod of the head) the mother to face her infant. A green light located on the mother's side of the screen was activated when both foot switches were depressed by the experimenters. This green light served to signal the mother to raise the blind, wherein she delivered the vocal feedback immediately following an infant vocalization. An 80dB buzzer signaled the mother to lower the blind 5 seconds later after its opening. A session ended after 12

minutes had elapsed, as indicated by a clock on the primary observer's event recorder. Sessions were terminated after 1 minute of crying, or at the mother's request. For example, a the mother might have requested to terminate a session if the infant was fussy and was unable to be redirected or consoled.

The mother was informed that for all sessions, she could play with the baby using any toys that she wished. A variety of toys was available during all experimental conditions to keep the baby content.

Response Definitions

An infant vocalization was defined as a voiced sound uttered by the infant. A pause of 1 second or longer defined the beginning of a new vocalization. The following sounds were excluded: coughing, breathing, sneezing, burping, and hiccoughing. Any sounds judged to be crying produced the same contingencies as any other vocalizations. Crying was defined as a shrill or an intense vocalization and possibly accompanied by a red face, furrowed brow, and/or sad expression.

Definition of Parental Matching Vocalizations

Parental matching stimuli were defined as the parent producing the same vocal sounds as the infant within 2 seconds of their occurrence.

Experimental Design and Conditions

This experiment compared three experimental conditions in a single-subject - reversal ABACDC design. Experimental conditions were changed when the graphed data, were judged to be visually stable. The following are descriptions of each experimental condition.

(A) Operant level. Becky began her participation in the research with an operant level baseline condition. The purpose of this condition was to generate an operant level of infant vocalizations against which to compare subsequent vocalization levels under different experimental conditions. Becky's mother was instructed to play with Becky, but with the stipulation that she refrain from matching or praising any infant vocalizations. Toys were available for play at all times during this condition, as well as the other experimental conditions.

(B) Matching with window. When the shade was down, the mother was instructed to make the same sounds as the infant immediately after their occurrence. Thus, upon hearing the vocalization and observing the signal light, the mother raised the blind to match Becky's vocalization immediately. The mother closed the shade 5s later consequent to hearing the buzzer. Any infant vocalizations that occurred while the shade was open were not matched.

(C) Matching with open window. This condition was the same as the previous contingent matching condition, except that the window was kept open for the entire length of the session. All infant vocalizations were matched by the mother immediately after their occurrence.

(D) Noncontingent stimulation. A noncontingent experimental condition was conducted to determine whether infant vocal behavior was under reinforcer-contingency control. The density and pattern of stimulation that occurred during these sessions was identical to those that occurred during the contingent matching sessions. This was accomplished by presenting the same vocal stimuli, taken from matching sessions, during the noncontingent sessions. Moreover, the onset time of the vocal stimuli was yoked to

previous matching sessions. The mother was given a script that was to be followed when she observed the activation of the signal light.

Data Analysis

To obtain a vocalization rate per minute, onset of each infant vocalization was recorded for each of the two 6-minute intervals per session. Rate was calculated by taking frequency of vocalizations and dividing by 6 (the duration of the interval).

Interobserver Agreement

Interobserver agreement was obtained for 70% of 58 6-minute intervals. The onset of vocalizations had to be recorded by both observers within 1 second to be counted as agreements. Overall interobserver agreement for the onset of infant vocalizations was 84%. For baseline conditions, agreement was 84% for 10 out of 14 intervals. For the three matching conditions, agreement was 85% for 22 out of 36 intervals. Finally, for the noncontingent condition, agreement was 80% for 8 out of 10 intervals for the third matching condition

Procedural Reliability

Procedural reliability measures were obtained for 70% of the consecutive 6-minute. Scores were calculated by dividing agreements plus disagreements and then dividing by 100. Onset of parental matching vocalizations had to be scored by both observers within 1 second to be counted as agreements. Overall agreement for the onset of parental matching was 85%.

Agreement for correct delivery of the parental matching and non-matching vocalizations was also obtained. Correct delivery was defined as the parent producing the same sounds as the infants within 2 seconds of their occurrence. Matching stimuli that

were recorded by both observers within 1 second were counted as agreements. Overall agreement for correct delivery of the reinforcer was 88%.

Results and Discussion

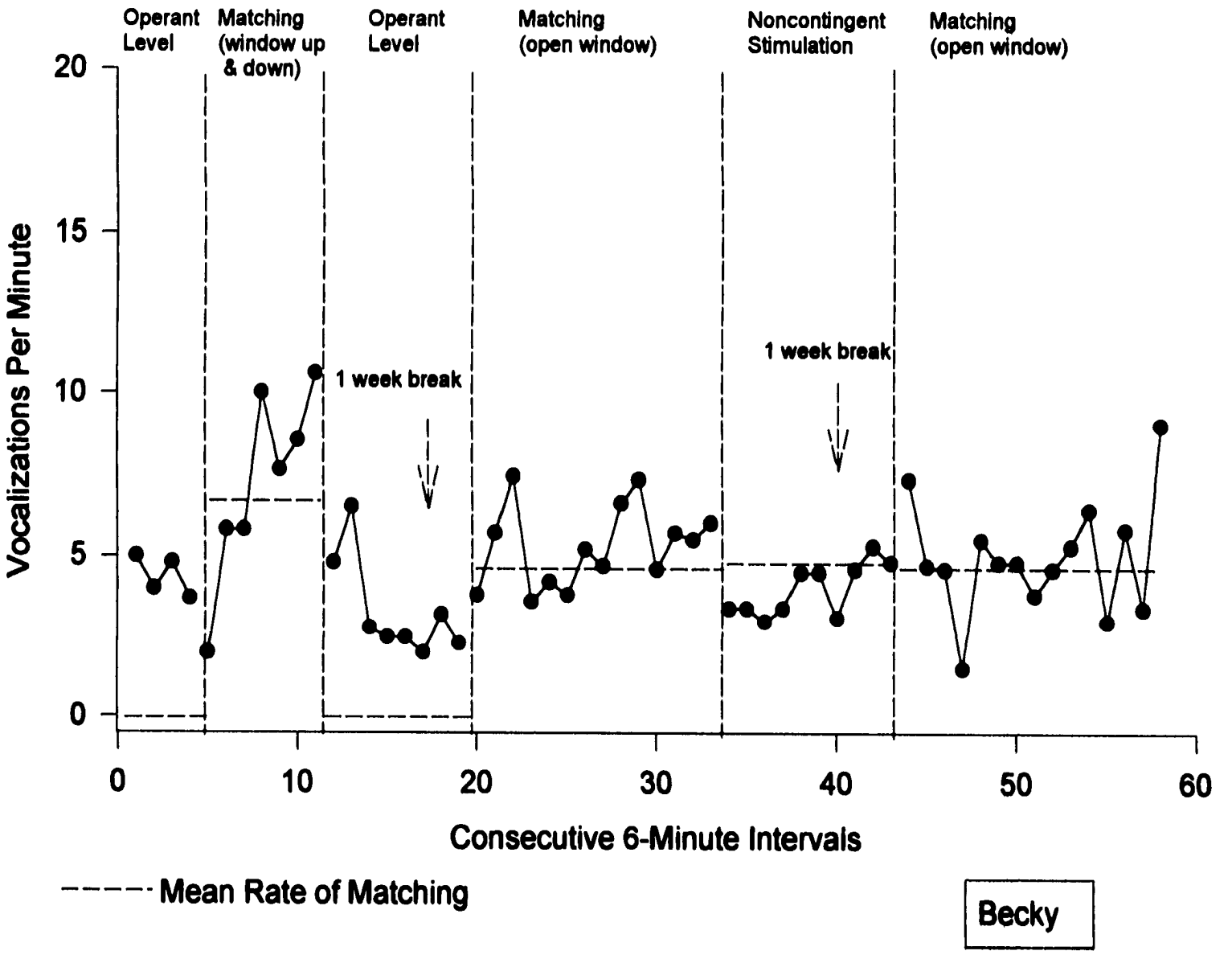
Figure A1 represents the number of infant vocalizations per minute for 58 6-minute intervals for all experimental conditions. Vocalizations per minute are plotted on the ordinate, and consecutive 6-minute intervals are plotted on the abscissa. The closed circles represent infant vocalization rate, and the horizontal line represents mean rate of matching stimuli.

During baseline, vocalization rates were stable and were approximately 5 vocalizations per minute. With the implementation of the first matching condition, vocalization rate initially decreased to 2 for interval 5 and then gradually increased over the next six intervals to a high of 10 vocalizations per minute. When baseline was introduced again, vocalization rates dropped to 5 and 7 for intervals 12 and 13, respectively. The data points for the next six intervals had a range between 2 and 3 vocalizations per minute.

At this point in the research, the principle investigator hypothesized that vocalization rates would conceivably be higher if the window shade was not in operation. That is, the vocalizations that occurred when the window shade was open were not reinforced. Only those vocalizations that occurred while the shade was down produced an opening of the blind and matching stimuli delivered by the parent. Therefore, only 73% of

## Appendix A Figure Caption

Figure Caption A1. **Vocalizations per minute for Becky during operant level baseline, matching, and non-contingent experimental control conditions. Mean rates of matching are shown by the horizontal lines**



infant vocalizations were reinforced within 2 seconds of their occurrence; and 2% of the matching stimuli were not preceded by infant vocalizations. Thus, for the implementation of the next three matching conditions, the shade was kept open, so a higher percentage of infant vocalizations would be followed by matching stimuli within 2 seconds or less.

When the second matching condition was resumed, the data were variable and had a range between 5 and 8 vocalizations per minute. Although the data were variable, there was a slight increasing trend. With the implementation of the noncontingent control condition, the vocalization rates were 3 for intervals 33 and 34, with the subsequent data points stabilizing at about 4 vocalizations per minute. There was a slight reduction in vocalization rates when compared with the second matching condition. With the implementation of the last matching condition, there was an increase from 4 vocalizations per minute for interval 43 to 7 for interval 44. The remaining data were variable, with a range from 2 to 9 vocalizations per minute. It is difficult to discern any differences in vocalization rates between the noncontingent condition and the last matching condition. Unfortunately, the baby was unable to participate because the mother was returning to college on a full time basis.

The differences between the first three experimental conditions were more pronounced when compared with the last three experimental conditions. Although 95% of vocalizations were reinforced within 2 seconds during the open window matching conditions ( and 3% of matching stimuli were not preceded by infant vocalizations), infant vocalizations did not increase as expected.

There may be two possible explanations. It is conceivable that the movement of the window shade may have acquired reinforcing properties. That is, the window shade

preceded the appearance and vocalization of the parent and, thus, became a conditoined reinforcer. This supports the contention of Mowrer (1977) and Risley (1960) such that the behavioral similarity between parent and infant vocalizations is probably a conditioned reinforcer, and therefore, weak. In this case, the addition of putative reinforcer ( i.e., the window shade), may have made the matching vocalization more potent. Additionally, the possibity exists that the infant may have produced vocalizations of higher durations during the last two matching conditions. An increase in duration of vocalizations would serve to bring down the overall frequency.

In conclusion, the similarity produced by parental matching of infant vocalizations increased infant vocalization rates when compared with the lower rates produced during operant level. Moreover, the vocalization rates during the noncontingent control condition were lower when compared with the last two matching conditions; even though the rates of matching stimuli were equal across those conditions. These data support the hypothesis that similarity produced by parental matching of infant vocalizations functions as a conditioned reinforcer.

# Appendix B

## An survey of the infant operant learning research.

## Abstract

THE DEMONSTRATION OF

LEARNING IN INFANT OPERANT RESEARCH

by

Marie Parnes

Advisor: Professor Claire L. Poulson

Numerous authors claimed to have demonstrated operant conditioning with different types of infant behavior. These authors concluded that the behavior of interest came under the control of the reinforcement contingencies. Nevertheless, in support of this claim, it must be demonstrated that behavior change is a function of the reinforcement procedure, and not a function of other stimulus arrangements. The infant literature from 1958-1997 was reviewed to determine the extent to which the putative reinforcement procedures actually met the definition of reinforcement. Based on this review, it can be concluded that only a small percentage of these studies demonstrated reinforcer-contingency control over infant behavior. The criteria for the demonstration of operant conditioning in infants are discussed.

## Introduction

Since the 1960's, there has been a proliferation of research exploring the operant conditioning of infant behavior. Operant investigations of many types of infant behavior have occurred. These studies used a variety of motor and vocal response measures such as head turning (e.g., Siqueland & Lipsitt, 1966), smiling (e.g., Brossard & DeCarie, 1968), sucking ( e.g., Sameroff, 1968), leg kicking (Fagen & Rovee, 1976), crying (e.g., Etzel & Gewirtz, 1967), and vocalizing (e.g., Poulson, 1983). The authors of these investigations concluded that the behavior of interest came under the control of the reinforcer. Prior to reaching such a conclusion, the authors must demonstrate that the changes in behavior are functionally related to changes in reinforcement, rather to changes in other stimulus arrangements. For example, social attention can elicit infant vocalizations (Bloom & Esposito, 1975), and in this situation, vocalizations are respondents. Vocalizations can also be emitted because social attention followed contingently after its occurrence (Poulson, 1983). In this case, vocalizations are operants. These types of learning processes must be differentiated by the researcher if conclusions concerning operant learning are to be drawn. This paper, therefore, is an analysis of the infant operant conditioning literature from 1958-1997 on the effects of positive reinforcement on different types of infant behavior. A similar critique was published by Poulson and Nunes (1988) on the infant-vocal-conditioning literature from 1957-1983.

To identify articles to be included in this review, a computer search was conducted on the PsycLIT (1997) database. The search terms used to produce abstracts were "Operant Conditioning and Human Infants." The search produced abstracts from journals dated from 1973 to 1997. A review of the references from those articles was used to obtain

older references prior to 1973. A review article from Hulsebus (1973) was especially useful.

Articles were included for analysis on the basis of three criteria: they consisted of original experimental research, they contained a reinforcement procedure, and they included infants with normal or delayed development between the chronological ages of 0-24 months. Articles were excluded from analysis if they did not have a comparison condition against which to compare positive reinforcement (e.g., Koch, 1967; Krulisova, 1973; Jones-Molfese, 1975), if they were review articles or exposition papers (e.g., Bloom, 1984; Lancioni, 1980; Poulson, 1984 ; & Poulson & Nunes, 1988; Poulson, Nunes & Warren, 1989, Pomerleau, Malcuit, Chamberland, Laurendeau, & Lamarre, 1992), if they were descriptions of apparatus (e.g., Rheingold, Stanley, & Cooley, 1962; Delucia, 1967; Bijou, 1980), or if they used subjects older than 2 years of age (e.g., Landau & Gewirtz, 1967; Weisberg & Tragakis, 1967). In total, of 145 articles initially obtained, 108 articles were selected for review on the basis of those three criteria. It is acknowledged that while this may not be exhaustive of the infant operant literature, it probably does contain most of the research.

The above procedures selected articles published in the following journals. Child development journals included: Cahiers de Psychologie Cognitive, Child Development, Developmental Psychology, Journal of Genetic Psychology, International Journal of Behavior and Development, Infant Learning and Behavior, Human Development, Journal of Applied Development, Journal of Child Psychology and Psychiatry, Journal of Experimental Child Psychology, and Merrill-Palmer Quarterly. The only journal related to the field of developmental disabilities was American Journal of Mental Retardation.

Journals that publish research in the area of speech and language were Journal of Child Language, Journal of Speech and Hearing Disorders, and Journal of Speech and Hearing Research. Nine journals that publish research in the area of general psychology were American Psychologist, British Journal of Psychology, Bulletin of Psychonomic Science, Journal of Comparative and Physiological Psychology, Journal of Behavior Therapy and Experimental Psychology, Journal of the Experimental Analysis of Behavior, Learning and Motivation, Perceptual and Motor Development, and Psychonomic Science. Pediatric journals were also included: Activitas Nervosa Superior, Acta Pediatrica, and Developmental Medicine and Child Neurology. Finally, one general scientific journal, Science, was included in the selection.

A table was constructed to facilitate the review of the literature. Because different studies used different experimental control procedures, they were sorted according to which of the experimental control procedures were compared with positive reinforcement. Information on the response, the reinforcer, and the experimental design also was presented for each study. Because much of the research on operant conditioning in general has been conducted with experimental-analysis-of-behavior methodology, and because much of that methodology involves single-case-experimental design and the presentation of single-subject data, it was also noted whether single-subject data from single-case-experimental design research were provided by each study.

Because of the massive amounts of data, it was possible that errors were made in the compilation of information. Therefore, interobserver agreement data were obtained on the accuracy of the tabled information. Ten articles were randomly selected from the pool of 108 articles, and assigned to a doctoral student. Interobserver agreement was 92%.

The purpose of this review was two-fold. First, the goal was to examine the extent to which control of behavior by contingencies of reinforcement had been established in the literature on infant conditioning. Second, the purpose was to discuss the necessity of the experimental-analysis-of-behavior methodology in infant operant research.

## The Pavlovian and Operant Research Paradigms

To facilitate a discussion of reinforcement control, it is first important to define key terms describing learning processes. Stimuli can have both eliciting and reinforcing functions. Before determining which function is operative in a given study, it may be helpful to review the two learning paradigms.

In the Pavlovian paradigm, conditioning occurs when different stimulus relations involving the pairing of conditioned and unconditioned stimuli come to control behavior. Environmental events that had previously no relation to a particular response could, through pairing, come to elicit a response. An unconditioned response may be elicited by an unconditioned stimulus. For example, food in the mouth elicits salivation. Classical conditioning occurs when a previously neutral stimulus is repeatedly paired with an unconditioned stimulus such that it becomes a conditioned stimulus. That is, after a number of pairings with the unconditioned stimulus, the conditioned stimulus comes to elicit the response (i.e., conditioned response). For example, pairing a previously neutral tone with food will eventually result in the tone alone eliciting salivation. In the Pavlovian paradigm, the relation is one of a stimulus eliciting a response that follows it. To demonstrate eliciting functions, stimulus presentation must increase response strength. If presentation and removal of that stimulus increases and decreases response strength systematically, then it can be concluded that the stimulus elicited responding.

By contrast, operant conditioning refers to a process in which the frequency of a response is modified by consequences that are contingent upon its occurrence. If the consequence of a response results in the increased probability of that response, that consequential stimulus is called a positive reinforcer. A stimulus is a negative reinforcer if its contingent removal results in the increased probability of the response that terminated it. There is also another relation, punishment, in which the consequence of the response decreases the future likelihood of that response.

To demonstrate operant conditioning, the case of positive reinforcement can be considered. For example, in the absence of satiation, the routine presentation of food reinforcers immediately following lever pressing will most likely increase the future probability of lever pressing. It is the contingent relation between the response and the reinforcer that increases response strength. Elicitation effects are ruled out by changing this contingency relation, but keeping the amount of stimulation constant. This could be accomplished by the use of a noncontingent control procedure and/or a DRO (differential reinforcement of other responding) procedure. If response strength decreases reliably for both noncontingent and DRO conditions, when compared with positive reinforcement, it can be concluded that the response is under the control of the reinforcer. The demonstration of differential responding with the implementation of different schedules of reinforcement is another way of showing reinforcer control. If patterns of responding conform to what is expected, then it can be concluded that the reinforcement contingencies are controlling behavior. For example, Weisberg and Fink (1966) examined fixed-ratio reinforcement schedule performance in infants and found that responding was characterized by a "break- and-run" pattern that is typical of such a schedule. In this

research, operant control was demonstrated because the differential signature patterns that characterize a fixed ratio schedules allows one to draw that conclusion. Extinction, or no stimulation, is yet another experimental-control condition against which to compare positive reinforcement. It does not, however, rule out elicitation effects because the increase in response strength during positive reinforcement can be attributed to the increased stimulation during that condition.

In summary, different stimulus relations govern Pavlovian and operant conditioning. In Pavlovian conditioning, the critical factor is the pairing, in close temporal contiguity, of two stimuli: the CS and the US. This pairing leads to a change such that the CS comes to elicit a response called the CR. Operant conditioning is defined by the relation between the response and the consequence that follows. If response strength reliably increases following contingent delivery of a particular consequence, then the consequence is a reinforcer.

## Research Design Issues in Infant Research

There are two kinds of research designs available to researchers in comparing positive reinforcement to various experimental-control conditions. They can make this comparison in a between-groups design, with one group receiving the experimental condition, and the other group receiving the control condition. Or, researchers can opt to use a within-subjects design, with each subject experiencing both experimental and control conditions. The type of design used by researchers is relevant in the review of the infant literature because it may influence the conclusions that can be drawn about any learning processes involved.

A between-groups design is traditionally taught to students of psychology. With the advent of statistical tests, large sample studies make experiments more powerful in detecting experimental effects. Large sample sizes of group designs also provide greater evidence for the generality of the relationship between the independent and dependent variables. Moreover, because subjects receive only one treatment variable, the experimenter can eliminate order of treatment effects, as well as eliminate concerns about the reversibility of treatment.

If one uses a between-groups design, however, one must be concerned with the effects of individual differences. Subject may have different learning histories, different rates of responding, and different preferences for particular reinforcers. These individual differences can affect the performances across groups. Typically, this is addressed by using of a large number of subjects and randomly assigning them to the experimental and control groups. The hope is that individual differences will be equally distributed across groups. One may, however, encounter difficulty in gathering a sufficient number of infants for random assignment to balance out individual differences. Therefore, another way to minimize the effects of individual differences is to match subjects on relevant variables, and then assign them systematically to different experimental and control groups. With respect to infants, however, it may not always be clear as to what that relevant variables might be. Furthermore, even if statistical analyses show significant differences across groups, it is possible that individual subjects reflect different learning processes. The putative reinforcer could have served as an elicitor for responding for some individuals, and as a reinforcer for others.

With the use of a within-subjects design, these concerns are diminished. Because each infant experiences every experimental condition, each serves as his or her own control. Therefore, the effects of individual differences in responding are minimized. A within-subjects design eliminates these potential confounds and, thus, may be more sensitive in detecting a functional relation between the independent and dependent variables. Furthermore, a within-subjects design potentially allows for the analysis of single-subject data across the implementation of both experimental conditions, and may allow one to determine the source of experimental control, either elicitative or reinforcing.

A potential constraint of a within-subjects analysis is that it does not control for order of treatment effects. That is, the fact that one treatment occurs first may affect the second treatment. In addition, there may be irreversibility of treatment effects. Nevertheless, the above mentioned concerns could be addressed with the use of a combination of single-subject and group design, with single subject analysis used for major independent variables, and secondary group controls for order of treatment effects.

Therefore, researchers must consider their research question and then weigh the advantages and disadvantages of a between-groups design and a within-subjects designs. If a researcher is primarily interested the performance of groups, and working with many subjects for a short period of time, then a nomothetic (determining averages of groups) approach is appropriate, If, however, a researcher is interested in the analysis of learning processes within individuals, then an idiographic approach (intensive long-term study of the individual) is appropriate (Kazdin, 1982). This could be accomplished with a single-subject design, or it could be imbedded in a within-subjects design.

Therefore, whereas the primary focus of this review is the level of experimental control obtained as a function of the experimental control procedures used, another focus is on the type of experimental design used by researchers.

## The Experimental Control Conditions

To determine whether changes in response strength are the result of the contingencies of reinforcement, it is essential to rule out the possibility that the same change in response strength occurred because of an elicitative or discriminative function of the presumptive reinforcing stimuli. To that end, it is important to examine the experimental control procedures that have been compared with reinforcement. Four types of control procedures are commonly used by researchers (Poulson & Nunes, 1983): (a) "no stimulation," (b) noncontingent reinforcement, (c) differential reinforcement of other responding (DRO), and (d) schedule control.

The first type of experimental control procedure to be discussed is the "no stimulation" procedure. During this procedure, no experimental stimuli are programmed. For example, Rheingold, Gewirtz, and Ross (1959) used no stimulation as a control procedure in an operant-vocal-conditioning experiment in a within-subjects A-B-A reversal design. During baseline sessions, the experimenter was present, but did not present social stimulation (i.e., a smile, a touch to the abdomen, and a "tsk tsk" sound) following any vocalizations. During conditioning sessions, the experimenter provided social stimulation immediately following each discrete vocalization. Extinction sessions were identical to baseline; the experimenter did not provide stimulation for vocalizations. Mean number of vocalizations increased over baseline measures with the introduction of the experimental condition, and decreased during the extinction phase. Although it

appears that the authors demonstrated operant learning, they did not rule out possible elicitation or discriminative effects of the putative reinforcing stimuli. The putative reinforcers could have elicited vocalizations from infants during treatment. Eliciting stimuli precede the response and cause it to occur as a reflex. At least initially, the number of responses elicited is roughly the same as the number of eliciting stimuli. Therefore an increase in the number of eliciting stimuli should produce a corresponding increase in the number of responses. Thus, in the Rheingold, Gewirtz, and Ross (1959) experiment, the increase in responding during conditioning could be the result of increased stimulation alone during the putative reinforcement condition.

To rule out the possibility that a mere increase in the amount of stimulation produced an increase response rate during the putative reinforcement condition, it is important to keep the amount of stimulation constant during the experimental and control conditions. During the control condition, the putative reinforcer could be presented independently of the target response. In this way, the amount of stimulation could be held constant between the control and the experimental conditions. If under these conditions there is a systematic increase in response strength during the putative reinforcement condition, then one may draw the conclusion that the contingency relation between the target response and the putative reinforcer caused that change. If responding should remain the same during both experimental and control conditions, one cannot conclude that reinforcement contingencies produced levels of responding above those which might be expected with elicitation alone. No stimulation, therefore, is not an effective control with which to compare positive reinforcement, because the increase in responding during "positive

reinforcement" could be the result of elicitation by the putative reinforcer, rather than the result of the contingency relationship.

The second type of experimental control procedure to be discussed is noncontingent stimulation. In a noncontingent procedure, the putative reinforcers are presented in a response-independent fashion. That is, they are presented with equal probability when the response occurs or does not occur. Such a procedure could have been of use to Rheingold et al. (1959). Use of a noncontingent schedule could have kept the amount of stimulation constant between the experimental and control conditions. Systematically higher response rates during the experimental condition could then have been attributed to reinforcement contingencies, rather than elicitation. While a noncontingent schedule is useful as a control procedure, one must guard against the possibility of an inequitable distribution of reinforcers following responding and not responding on the part of each infant. If the distribution of reinforcers is greater for responding than for not responding, response rates should remain elevated (e.g., Lattal, 1974). Thus, response rates during the control condition might be as high as those during the experimental condition. To ensure that the noncontingent schedule is properly implemented during the control condition, a conditional probability measure can be obtained by calculating: 1) the percentage of target responding followed by the putative reinforcing stimuli within N seconds, and 2) the percentage of putative reinforcers not preceded by target responding within N seconds. These measures would determine whether in fact there is a contingent response-reinforcer relation. If a positive contingency relation exists (e.g., during continuous reinforcement), the percentage of reinforcers for responding would be greater than for not responding. This should produce elevated response rates. Conversely, if a negative contingency exists

( i.e., DRO), the percentage of reinforcers for not responding should be greater than for responding . This should produce decreased response rates. If there is, however, an equal distribution of reinforcers for responding and for not responding, response rates should be lower than those obtained during positive reinforcement. Under these conditions, one has a truly noncontingent schedule.

To keep the amount of stimulation constant between experimental and control conditions, one method is to program a noncontingent schedule yoked to the experimental condition. First, for each experimental session, the time intervals at which the putative reinforcers were delivered are measured. Then as those same time intervals occur during the control condition, the putative reinforces are delivered. In this case, the noncontingent schedule is a yoked-variable-time schedule.

Whereas a yoking procedure can be helpful in keeping the amount of stimulation constant between experimental and control conditions, one must be aware of the potential problems . If one yokes within the same subject, as in Reeve, Reeve, and Poulson (1993), the subject must experience an experimental condition prior to the noncontingent procedure. If one, however, yokes across subjects, as did Bloom and Esposito (1975), the problem of individual differences appears (i.e., Church, 1964). That is, if experimental subjects respond significantly more than their matched control subjects, it could be due to individual differences in the effectiveness of the event as an elicitor of activity, and not because the event served as a reinforcer. Additionally, subjects may differ in terms of establishing operations such as deprivation and satiation, that may subsequently affect the potency of the reinforcer. Subjects may also differ in the initial probability of the target response. Therefore, these individual differences may produce differences between the

experimental group and its matched control group that are not attributable to differences in the response-reinforcer contingency relation.

The third type of experimental control procedure to be discussed is the differential reinforcement of other responding (DRO) schedule. Like a noncontingent schedule in which reinforcers are delivered on a regular schedule, a differential reinforcement of other responding (DRO) schedule consists of two procedures: 1) reinforcers are delivered on a regular schedule, and 2) delivery of reinforcers are withheld if the target response should occur. Thus, DRO schedules contain a negative contingency that it is expected to lower target response rates. There are two types of DRO procedures: time based and response based. In a time-based procedure, some minimum amount of time must have elapsed since the target response was emitted for the positive reinforcer to be delivered (Reuter, 1972). If the target response is emitted, the reinforcer is withheld for a particular unit of time. For example, in Reeve, Reeve, and Poulson (1993) delivery of the reinforcer occurred every 2 seconds as long as the infant did not vocalize. If the infant did vocalize, the reinforcer would be delayed by another 4 seconds.

During a response-based DRO, the subject must emit any response, other than the target response, to receive the reinforcer. If the target response should occur, then the reinforcer is not delivered. For example, in the case of vocal conditioning, the infant would receive the reinforcer for any response other than a target vocalization, such as smile. If a vocalization should be emitted, then the reinforcer would not be delivered until the other response is emitted.

The advantage of DRO over a noncontingent schedule is that it rules out the possibility of adventitious reinforcement of the target response. The advantage of DRO

over extinction as an experimental-control procedure is that density of reinforcement may be arranged to be comparable to positive reinforcement levels, thus controlling for elicitation effects of presumptive reinforcing stimuli. For example, Poulson (1983) provided a solution to yoking in her study when she compared continuous social reinforcement (CRF) with a differential reinforcement schedule (DRO) on infant vocalization rates. In this study, Poulson (1983) deliberately made the density of social stimulation higher during the DRO control condition than during CRF by selecting a low value (i.e., 2 sec) of the reinforcer-reinforcer interval. Vocalization rates were lower during DRO and higher during CRF. Her study successfully ruled out the effects of elicitation because stimulation occurred at higher rates during the control condition, as compared to the treatment condition, yet response rates remained lower during DRO than during CRF. If vocalizations were being elicited by social stimulation, then one would have expected the response rate during the DRO condition to be as high as those during continuous reinforcement. Because they weren't, Poulson (1983) could conclude that the contingencies of reinforcement controlled infant responding, and that elicitation did not.

In using a DRO schedule as a control procedure against which to compare positive reinforcement, it is imperative to have an observation-only baseline procedure to compare with the DRO procedure if one wants to be able to draw conclusions about the direction of behavior change. An observation-only baseline is an attempt to measure behavior as it occurs in the environment without the implementation of treatment. An observation-only baseline would be called extinction if it followed a reinforcement condition. To conclude, for example, that behavior increased during positive reinforcement, one would need to show that behavior did not simply decrease from some observation-only baseline.

Because there is a negative contingency between the response and the reinforcer during the DRO procedure, it is possible that DRO may suppress responding. Therefore, one would not know whether the increase in responding during the positive reinforcement condition was higher than responding that would occur during an observation-only baseline. Without an observation-only baseline, one would not know whether the positive reinforcement procedure produced rates higher than baseline, the same as baseline, or lower than baseline even though response levels during the experimental condition might be higher than the level of responding during the DRO condition.

The fourth type of experimental-control procedure is to show differential responding under different reinforcement schedules. Each reinforcement schedule is associated with a specific pattern of responding. Simple schedules of reinforcement can be classified into two types: ratio schedules and interval schedules. Ratio schedules specify the number of responses that must be emitted before the reinforcer can be delivered. Interval schedules specify the interval of time that must elapse before a response can be reinforced.

There are two types of ratio schedules: fixed and variable. In a fixed-ratio schedule, a fixed number of responses is reinforced. If an infant were reinforced for every 10 responses, this would be an example of an FR 10 schedule. A fixed-ratio schedule is generally characterized by a "break-and-run" pattern. After the reinforcer is delivered, there is a post-reinforcement pause in responding. That is, immediately after the reinforcer is delivered, responding ceases. The high and steady rate of responding that follows the post-reinforcement pause is called a ratio run. The length of the post-reinforcement pause is a function of the size of the ratio. A large ratio produces a long pause, and a short ratio generally produces a short pause; although a stable response rate

with no post-reinforcement pause may also be observed on small ratios. Another kind of ratio schedule is called a variable-ratio schedule. When this schedule is in effect, an average number of responses is required for the reinforcer to be delivered. For example, an infant may be required to emit 5 responses, then 20, and then 10 responses to receive the next reinforcer. Unlike the "break-and-run" pattern of an FR schedule, a VR schedule is generally characterized by high, steady response patterns. Additionally, a VR schedule may be characterized by frequent pauses if the ratio becomes too large.

There are also two kinds of interval schedules: fixed and variable. A fixed-interval schedule specifies a fixed amount of time that must elapse before a response can be reinforced. For example, in a FI 30-second schedule, 30 seconds must elapse and before a response can be reinforced. A fixed-interval schedule is characterized by slow to moderate response rates with a pause in responding immediately following reinforcer delivery. As the time for the delivery of the next reinforcer draws closer, the response rate accelerates. This is known as the fixed-interval scallop. Other investigators (e.g., Schneider, 1969), however, have observed a break-and-run pattern to fixed-interval responding. Schneider (1969) has proposed that both patterns may appear at different stages in the development of fixed-interval responding. That is, the scallop pattern may be characteristic of earlier performance, whereas the break-and-run pattern may appear after extended training. Additionally, Schneider (1969) has suggested that the break-and-run pattern applies better to shorter intervals and may not appear for FI's of 9 minutes or greater. The other interval schedule is the variable-interval schedule. A variable-interval schedule specifies a variable amount of time that must elapse for a response to be reinforced. For example, a VI 20-second schedule would require an average of 20

seconds to elapse before a response can be reinforced. A VI schedule typically generates a slow to moderate response rate that is constant and stable.

Because different schedules of reinforcement are associated with specific response patterns, any change in responding across different schedules could be caused by the different contingencies that are controlling behavior. For example, Lowe, Beasty, and Bentall (1983) compared the performance of three infants on various fixed-interval schedules. The topographies of responding were characteristic of fixed-interval schedules. That is, after reinforcement, there was a post-reinforcement pause, followed by an increase in responding toward the end of the interval. As mentioned previously, this is also known as "scalloping." If for example, infant responding was being elicited, the scalloping pattern would not be obtained. Rather, responding would remain constant, with no observed pauses or breaks. Thus, it is the differential appearance of signature patterns with the differential implementation of reinforcement schedules that allows one to conclude that behavior change was a function of the different contingencies in effect.

Using the criteria cited above, Poulson and Nunes (1983) reviewed that portion of the infant literature that consisted of vocal conditioning. Their review included the infant vocal conditioning studies between 1959 and 1983. Poulson and Nunes (1983) observed that 13 of the 15 studies in their review used no stimulation as an experimental-control condition against which to compare positive reinforcement. Three of those 13 studies also used noncontingent reinforcement, yet the authors did not show any behavior change between experimental and control conditions. One study reported differences in performance with use of a noncontingent procedure, but only in a between-groups comparison. Two studies were noted to use DRO against which to compare positive

reinforcement, and one of those same studies additionally used schedule control as an experimental control procedure.

Poulson and Nunes (1983) concluded that only two studies (i.e., Poulson 1983; Sheppard (1969) unequivocally demonstrated reinforcement-contingency control over infant vocalizations. That is, they used experimental-control procedures appropriate for eliminating elicitation as an explanation the for behavior change from control to experimental conditions. Poulson (1983) demonstrated reinforcement-contingency control using a DRO procedure against which to compare positive reinforcement, while Shepperd (1969) successfully demonstrated differential responding under a concurrent-response multiple schedule. The remaining studies either did not use an appropriate control procedure or did not show experimental control over infant vocalizations.

Based on the conclusions of Poulson and Nunes (1983), one might infer that there has been very little research on infant conditioning in which adequate experimental control procedures ruled out possible elicitation effects by presumptive reinforcing stimuli. It would be worth knowing whether the conclusions of Poulson and Nunes (1983), based on only 15 studies of vocal conditioning, apply to the rest of the infant conditioning literature.

The present review will extend the type of scrutiny provided by Poulson and Nunes (1983) to the rest of the infant conditioning literature regardless of the response conditioned. As described previously in the introduction, 108 studies were identified for this review. The primary focus was to determine whether reinforcement contingencies were responsible for behavior change, rather than other possible explanations, such as elicitation. This was accomplished by comparing the experimental and control conditions. The experimental condition in all studies was a contingent positive reinforcement

procedure. A variety of control conditions might have been used within a single study. Thus, a single article might be listed as reporting more than one type of control against which to compare positive reinforcement.

Table 1 shows the control conditions used in each of the studies. Columns one, two, and three identify the reference, the response type, and the reinforcer type, respectively, of a particular study. Columns four through eight designate the types of experimental control procedures used in the various studies. A within-subjects comparison of the relevant experimental and control conditions is designated by a "W," and a between-groups comparison of the relevant experimental and control conditions is designated by a "B." Finally, the last column notes whether experimental control was demonstrated within individual subjects. This usually reduces to the question of whether single-subject data across experimental and control conditions were considered and/or presented in drawing the conclusion about the function of the independent variable.

Table 2 provides the number of infant studies using a within-groups or between-groups design for comparing positive reinforcement and a control condition across four experimental-control conditions. It further provides the total number of infant studies using each control condition.

Because the primary focus of this review is the comparison between the positive reinforcement procedure and the relevant control condition, the following sections are arranged according to the type of control procedure used in particular studies. The four most commonly used control procedures are reviewed in the following order: (a) no stimulation, (b) noncontingent, (c) DRO, and (d) schedule control.

Table 1

Chronological listing of the infant operant literature from 1958-1997

| | | | Experimental Control Procedures | | | | |
|---|---|---|---|---|---|---|---|
| Reference | Response | Reinforcer | No Stimulation | Noncontingent Reinforcement | DRO | Schedule Control | SS Data |
| 1. Brackbill (1958) | smiles | social | W | | | | no |
| 2. Rheingold, Gewirtz, & Ross (1959) | vocalizations | social | W | | | | no |
| 3. Papousek (1961) | head turns | consumable | W | | | | no |
| 4. Smith & Smith (1962) | vocalizations & body orientation | revolution of playpen & audio-visual | | W | | | no |
| 5. Weisberg (1963) | vocalizations | social auditory | *W | B | | | no |
| 6. Simmons (1964) | panel presses | auditory | W | | | | yes |
| 7. Siqueland (1964) | head turns | consumable | W | | | | no |
| 8. Charlesworth (1966) | visual attending | social | W | | | | no |
| 9. Lipsitt, Pederson,& Delucia (1966) | panel presses | conjugate visual | W | | | | no |
| 10. Lipsitt et al. (1966) | sucking | consumable | W | | | | no |
| 11. Weisberg & Fink (1966) | lever presses | consumable | W | | | W | yes |
| 12. Weisberg & Simmons (1966) | touches to stimuli | consumable | | | | | yes |

| | | | | | | | |
|---|---|---|---|---|---|---|---|
| 13. Siqueland & Lipsitt (1966) | head turns | consumable | W | | | | no |
| 14. Caron (1967) | head turns | visual | W | | | | yes |
| 15. Etzel & Gewirtz (1967) | crying | social | W | | W | | yes |
| 16. Levison & Levison (1967) | head turns | visual | W | | | | no |
| 17. Lu (1967) | visual fixation | consumable | W baseline only | | | | no |
| 18. Wahler (1967) | smiles | social | | | W | | yes |
| 19. Brossard et al. (1967) | smiles | social | W | | | | no |
| 20. Friedlander (1968) | panel presses | audio-visual | | | | W concurrent | yes |
| 21. Leuba & Friedlander (1968) | panel presses | audio-visual | | | | W concurrent | yes |
| 22. Siqueland (1968) | head turns | consumable | W | | | | no |
| 23. Sameroff (1968) | sucking | consumable | W | | | | yes |
| 24. Todd & Palmer (1968) | vocalizations | social | W | | | | no |
| 25. Rovee & Rovee (1969) | leg kicks | conjugate visual | W | | | | no |
| 26. Routh (1969) | vocalizations | social | W | | | | no |
| 27. Sheppard (1969) | vocalizations | audio-visual tactile | | *W | *W | W | yes |
| 28. Wahler (1969) | vocalizations | social | | | W | | yes |
| 29. Watson (1969) | visual fixation | audio-visual | W | | | | no |
| 30. Weisberg (1969) | lever presses | consumable | W | | W | | yes |
| 31. Butterfield & Siperstein (1970) | sucking | auditory | B | W | W | | no |

| | | | | | | |
|---|---|---|---|---|---|---|
| 32. Schwartz, Rosenberg & Brackbill (1970) | vocalizations | social | W | | | no |
| 33. Eimas, Siqueland, Jusczyk, & Vigorito (1971) | high amplitude sucking | auditory | B baseline only | | | no |
| 34. McKenzie & Day (1971) | head turns | social | W baseline only | | | no |
| 35. McKenzie & Day (1971) | head turns | social | W baseline only | | | no |
| 36. McKinnon, Koepke, & Apland (1971) | panel presses | social | W | | | no |
| 37. Ramey & Ourth (1971) | vocalizations | social | W | | | no |
| 38. Zelazo (1971) | smiles | social | W baseline only | | | no |
| 39. Banikiotes, Montgomery & Banikiotes(1972) | vocalizations | social | W | | | no |
| 40. Brown (1972) | sucking | nipple type | W baseline only | | | no |
| 41. Clifton, Meyers, & Solomons (1972) | head turns | social | W baseline only | B | | no |
| 42. Clifton, Siqueland, & Lipsitt (1972) | head turns | consumable | W | | | no |
| 43. Haugan & McIntire (1972) | vocalizations | vocal imitation, consumable & social | W | | | no |
| 44. Hillman & Bruner (1972) | sucking | consumable | | | B FR or FI | no |

| | | | | | | | |
|---|---|---|---|---|---|---|---|
| 45. Kobre &Lipsitt (1972) | sucking | consumable | B | | | | no |
| 46. Millar & Schaffer (1972) | apparatus presses | audio-visual | W | | | | no |
| 47. Ramey, Hieger, & Klisz (1972) | vocalizations | synchronous visual | W | | | | yes |
| 48. Tomlinson-Keasey (1972) | vocalizations | audio-visual | W | | | | no |
| 49. Watson & Ramey (1972) | head turns | visual | B | B | | | no |
| 50. Hursh & Sherman (1973) | vocalizations | model, praise, imitation | W | | | | no |
| 51. Millar & Schaffer (1973) | apparatus touches & visual fixation | audio-visual | W | | | | no |
| 52. Brassell & Kaye (1973) | sucking | nipple type | W | | | | no |
| 53. Brown & Brown (1974) | vocalizations | social consumable | | | W | | yes |
| 54. Bloom & Esposito (1974) | vocalizations | social | W | B* | | | no |
| 55. Moore, Thompson & Thompson (1974) | auditory localization | social & visual | B | | | | no |
| 56. Vietze, Foster, & Friedman (1974) | head turns | visual | W | | | | no |
| 57. Wiegerink, Harris,Simeonson, & Pearson (1974) | vocalizations | social | W | | | | no |
| 58. Cyrulik-Jacobs, Shapira, & Jones (1975) | switch presses | auditory | | | | W concurrent | yes |

| | | | | | | | |
|---|---|---|---|---|---|---|---|
| 59. | Glavin & Moyer (1975) | crying | social | | | W | yes |
| 60. | Franks & Berg (1975) | sucking | conjugate visual | | W baseline only | | no |
| 61. | Ramey, Starr, Pallas, Whitten, & Reed (1975) | vocalizations | synchronous (social & visual) | W baseline only | | | no |
| 62. | Wormith et al.. (1975) | sucking | auditory | W baseline only | | | no |
| 63. | Crook (1976) | sucking & heart rate | consumable | W baseline only | | | no |
| 64. | Fagen & Kent Rovee (1976) | leg kicks & visual attention | conjugate visual | W | | | no |
| 65. | Kent Rovee & Fagen (1976) | leg kicks | conjugate visual | W | | | no |
| 66. | Millar (1976) | apparatus presses | social | W | | | no |
| 67. | Trehub (1976) | sucking | auditory | W baseline only | | | no |
| 68. | Cavanaugh & Davidson (1977) | panel presses | audio-visual | | W listed as extinction | | no |
| 69. | Finklestein & Ramey (1977) | arm pulls, panel presses, & vocalizations | audio-visual | W | B | | no |
| 70. | Hamilton (1977) | vocalizations | social | W | B | | no |
| 71. | Moore, Wilson, & Thompson (1977) | head turns | visual | B | | | no |
| 72. | McKirdy & Kent Rovee (1978) | leg kicks | conjugate auditory/ visual | W | | | no |

| | | | | | | | |
|---|---|---|---|---|---|---|---|
| 73. Rovee et al. (1978) | leg kicks | conjugate visual | W | | | | no |
| 74. Williams & Golenski (1978) | high amplitude sucking | auditory | W | B | | | no |
| 75. Millar & Watson, (1979) | arm pulls | audio-visual | W | | | | no |
| 76. Rovee-Collier & Capatides (1979) | leg kicks | conjugate visual | W | | | | yes |
| 77. Sullivan, Rovee-Collier et al. (1979) | footkicks | conjugate visual | W | | | | no |
| 78. Fagen (1980) | footkicks visual attention | visual | W | | | | no |
| 79. Rovee-Collier, Sullivan,Enright, &Fagen (1980) | leg kicks | conjugate visual | W | | | | no |
| 80. Fagen, Yengo, Rovee-Collier, & Enright (1981) | leg kicks | conjugate visual | W | | | | no |
| 81. Davis & Rovee-Collier (1981) | leg kicks | conjugate visual | W | | | | no |
| 82. Enright,Rovee-Collier, Fagen & Caniglia (1983) | leg kicks | conjugate visual | W | | | | no |
| 83. Fagen & Rovee-Collier (1983) | leg kicks | conjugate visual | W | | | | no |
| 84. Lowe, Beasty, & Bentall (1983) | apparatus touches | auditory food | | | | W various FI schedules | yes |
| 85. Poulson, (1983) | vocalizations | social | | | W | | yes |

| | | | | | | |
|---|---|---|---|---|---|---|
| 86. Gekowski, Fagen, & Pearlman (1984) | leg kicks | conjugate visual | W | | | no |
| 87. Dunst & Lingerfeld (1985) | leg kicks | conjugate audiovisual | W | W | | no |
| 88. Rovee-Collier et al. (1985) | leg kicks | conjugate visual | W | | | no |
| 89. Rovee-Collier, Patterson & Hayne (1985) | leg kicks | conjugate visual | W | | | no |
| 90. Fagen, Ohr, Singer, et al. (1985) | leg kicks | conjugate visual | W | | | no |
| 91. Mysliveck et al. (1987) | leg kicks | conjugate visual | W | | | no |
| 92. Hill, Borovsky, & et al. (1988) | leg kicks | conjugate visual | W | | | no |
| 93. Fagen, Ohr, Singer, & Klein (1989) | leg kicks | conjugate visual | W | | | no |
| 94. Rovee-Collier, Earley & Stafford(1989) | leg kicks | conjugate visual | W | | | no |
| 95. Wolan Sullivan & Lewis (1989) | arm pulls, smiling, & Vocalizations | audio-visual | W baseline only | B | | no |
| 96. Alessandri et al. (1990) | arm movements | audio-visual | W | | | no |
| 97. Boller et al. (1990) | leg kicks | conjugate visual | W | | | no |
| 98. Lewis et al. (1990) | arm movements | audio-visual | W | | | no |
| 99. Tyler & McKenzie (1990) | visual tracking | social | W baseline only | | | no |

| | | | | | | | |
|---|---|---|---|---|---|---|---|
| 100. Ohr & Fagen (1991) | leg kicks | conjugate visual | W | | | | no |
| 101. Wishart (1991) | leg kicks | conjugate visual | W | | | | yes |
| 102. Millar & Weir (1992) | apparatus touches | audio-visual | W | | | | no |
| 103. Reeve, Reeve, Brown, Brown, & Poulson (1992) | vocalizations | social | | | W | | yes |
| 104. Darcheville, Riviere & Wearden(1993) | apparatus touches | visual | | | | W various FI schedules | no |
| 105. Ohr & Fagen (1993) | leg kicks | conjugate visual | | | | | no |
| 106. Reeve, Reeve, & Poulson, (1993) | vocalizations | social | | W | W | | yes |
| 107. Thompson & Ingersoll (1993) | apparatus contacts | visual | B | | | | no |
| 108. Ohr & Fagen (1994) | arm movements | audio-visual | W | | | | no |

W = within-subject control
B = between subject control
* = no behavior change

Table 2

Number of infant studies using each of the four experimental control conditions against which to compare positive reinforcement

| | Experimental Control Conditions | | | | | | | |
|---|---|---|---|---|---|---|---|---|
| | No Stimulation | % | Noncontingent | % | DRO | % | Schedule Control | % |
| Within | 83 | 92% | 7 | 47% | 11 | 100% | 7 | 88% |
| Between | 7 | 8% | 8 | 53% | 0 | ------ | 1 | 12% |
| Total | 90 | 83% | 15 | 14% | 11 | 10 % | 8 | 7% |

Within = within-subject control
Between = between-subject control
Total = total number of infant studies

## No Stimulation as an Experimental-Control Procedure

In the review of the infant literature, 90, or 83% of 108 studies used no stimulation against which to compare positive reinforcement. Of those 90 studies, 8% used no stimulation in a between-groups comparison (e.g., Kobre & Lipsitt, 1972: and Thompson & Ingersoll, 1993), and 92% used no stimulation in a within-subjects comparison (e.g., Millar, 1976; McKirdy & Kent Rovee, 1978; and Ohr & Fagen, 1984).

This preponderance of within-subject comparisons suggests that infant researchers are sensitive to the issue of individual differences among their research subjects. This may reflect their awareness that different infants show preferences for different reinforcers, or even for differences in the quality of those presumptive reinforcing stimuli, such as tone of voice or facial expression. It may also reflect more practical issues such as the difficulty in acquiring a large number of subjects for between-groups research.

As previously stated, a no stimulation condition, whether used in a within-or-between subjects comparison with positive reinforcement, does not control for the possible effects of elicitation. The putative reinforcer could have elicited responding during the positive reinforcement procedure, and a subsequent decline in response strength during the no-stimulation control could have occurred because of the removal of the eliciting stimulus. The amounts of stimulation during the control condition must be equal to or higher than that of experimental condition to rule out elicitation effects. Under these conditions, if response strength decreases during the control condition, given equal to or higher amounts of stimulation, then the change in response strength must be because of changes in the reinforcement contingencies. Those studies that relied on no stimulation as the primary

experimental-control procedure did not rule out possible elicitation effects. Thus one may not conclude from those studies that operant conditioning of infant behavior occurred. Therefore, 83% of the studies that appear to demonstrate reinforcement-contingency control actually fail to do so because they do not demonstrate that it was the contingency relationship between the response and the presumptive reinforcer that was responsible for behavior change.

Twelve of the 90 studies, or 13%, used no stimulation in combination with other experimental control conditions (e.g., Dunst & Lingerfeld, 1985; Weisberg, 1969), against which to compare positive reinforcement. Perhaps those researchers recognized the insufficiency of no stimulation as a control procedure. The additional procedures they used will be mentioned in the following sections describing the other experimental control conditions.

In summary, the vast majority of the studies that used no stimulation as an experimental control procedure used a within-subjects analysis, while only 8% used a between-subjects analysis. Moreover, 12 studies used additional comparisons conditions against which to compare positive reinforcement.

### Noncontingent Stimulation as an Experimental-Control Procedure

As stated previously, a noncontingent schedule of stimulation can be used as an experimental-control procedure against which to compare positive reinforcement. In this type of schedule, the putative reinforcers are presented in a response-independent fashion. That is, the reinforcers are presented with equal probability when the response occurs or does not occur. This type of schedule is advantageous as an experimental-control procedure because it allows one to keep the amount of stimulation comparable across

experimental conditions. One must, however, guard against the possibility of an inequitable distribution of reinforcers following responding and not responding that can maintain high rates of responding. This can be addressed by obtaining a conditional probability measure.

Fifteen, or 14% of 108 studies, used a noncontingent-control procedure against which to compare positive reinforcement. Of those 15 studies, 8 or 53%, used noncontingent stimulation in a between-groups comparison (i.e., Bloom & Esposito, 1974; Clifton, Meyers, & Solomon, 1973; Finkelstein & Ramey, 1977; Hamilton, 1977; Watson & Ramey, 1972; Weisberg, 1963; Williams & Golenski, 1978; Wolan Sullivan & Lewis, 1989). The other seven studies, or 47%, used noncontingent stimulation in a within-subjects comparison (i.e., Butterfield & Siperstein, 1970; Cavanaugh & Davidson, 1977; Dunst & Lingerfeld, 1985; Franks & Berg, 1975; Reeve, Reeve & Poulson, 1993; Sheppard, 1969; Smith & Smith, 1962).

As stated above, 53% of the authors compared positive reinforcement with noncontingent stimulation in a between-groups design, whereas in the previous section, it was reported that only 8% of the authors used a between-groups design. One wonders why there was such a dramatic increase in the proportion of between-groups designs with the use of noncontingent schedules. Perhaps this choice was related to the logic of using noncontingent schedules of reinforcement as a control condition against which to compare contingent reinforcement. One wants the amount of stimulation in both conditions to be comparable, so that the contingency relation can emerge as the controlling variable. If, in addition to that concern, one wished to counterbalance the order of exposure to each condition, then a between-groups analysis would recommend itself. In a between-groups

comparison, half the subjects could be run with the positive-reinforcement condition first, so that a noncontingent procedure could be yoked to it. One would then retain a control for order effects of experiencing either condition first. Although that logic has appeal, an examination of the 15 studies using noncontingent-experimental conditions fails to support the use of that logic by the authors of those studies. When comparing the studies that used between-groups comparisons with those that used within-subjects comparisons, one finds no systematic differences in the number of studies that counterbalanced order effects. Furthermore, only four of the eight between-groups comparisons and only three of the seven within-groups comparisons exposed their subjects to the contingent schedule first. Moreover, the within-groups studies were as likely to use some type of yoking procedure between noncontingent and contingent procedures as the between-groups studies. Thus, the decision to use within or between-groups comparisons may have been related to other considerations that varied from study to study.

Regardless of the rationale for the selection of the experimental design, six studies reported significant differences in rates of responding between contingent and noncontingent conditions in a between-groups comparison (i.e., Clifton Meyers & Solomon, 1973; Finkelstein & Ramey, 1977; Watson & Ramey, 1972; Williams & Golenski, 1978; Weisberg, 1963; Wolan Sullivan & Lewis 1989), while two studies did not ( i.e., Bloom & Esposito, 1975; Hamilton, 1977).

Finkelstein & Ramey (1977) and Weisberg (1963) provide two examples of studies that compared positive reinforcement and noncontingent stimulation in a between-groups design. Finkelstein and Ramey (1977) compared contingent and noncontingent stimulation on response acquisition in 5-to-8 month-old infants. The amount of

stimulation received by the subjects in the noncontingent group was yoked to the stimulation received by the subjects in the contingent group. Subjects in the contingent group had higher rates of responding compared to the subjects in the noncontingent group. Finkelstein and Ramey (1977) concluded that contingent stimulation produced higher response rates when compared with noncontingent stimulation, and additionally, experience with contingent stimulation facilitated the learning of another response. Thus the subjects who received response-independent stimulation not only yielded lower response rates, but were slower to acquire a new response.

Whereas Finkelstein and Ramey (1977) used a yoking procedure to equate the amount of stimulation across groups, they might have improved their procedure by yoking the density and/or pattern of stimulation within subjects has. Individual differences between subjects can be greater than any differences between experimental conditions for a given subject. For example, yoking the reinforcement rate of a low-rate subject to a high-rate subject's noncontingent rate may not produce comparable densities of reinforcement between contingent and noncontingent conditions for high-rate infants. Therefore, Finkelstein and Ramey (1977) could have minimized the effects of individual differences by yoking the amounts of stimulation within subjects.

As with the case of Finkelstein and Ramey (1977), Weisberg (1963) also reported higher rates of vocalization for infants receiving contingent social stimulation than those receiving noncontingent social stimulation, although he did not use a yoking procedure to equate the amount of stimulation between groups. Nevertheless, he concluded that infant vocalization rates could be reinforced by social consequences.

As mentioned previously, there were two studies that did not report significant differences in behavior between positive reinforcement and noncontingent stimulation in a between-groups design. In the first study, Bloom & Esposito (1975) found that adult social stimulation increased vocalization rates equally in contingent and noncontingent groups. Based on the findings of their experiments, they concluded that the response-reinforcer contingency was not important in the facilitation of infant vocalizations, and, thus, that the putative reinforcing stimuli had an eliciting function. In addition, Hamilton (1977), in working with older infants, reported no significant differences in mean response rates of vowels, consonants, or whole words between noncontingent control and contingent-experimental groups. The experimenter programmed various kinds of social stimulation contingently upon infant responding. There were increases in mean vocalization rate for only one experimental group (modeling and social stimulation) as compared to the noncontingent-control group. Hamilton suggested that no differences occurred between most of the experimental groups and the noncontingent group because the total amount of adult stimulation may have been sufficient to produce satiation.

Six studies reported differences in response rates between contingent and noncontingent conditions in a within-subjects design ( i.e., Butterfield & Siperstein, 1970; Cavanaugh and Davidson 1977, Dunst & Lingerfeld, 1985; Franks & Berg, 1975; and Reeve et al., 1993; Smith & Smith, 1962).

Cavanaugh & Davidson (1977), Dunst & Lingerfeld, (1985), and Reeve et al. (1993) provide examples of three studies that reported higher response rates under contingent stimulation, as compared to noncontingent stimulation. Cavanaugh & Davidson (1977) reported these findings in 6-month-old infants. There were three groups of subjects. For

the first group, audio-visual stimulation was presented every 20 seconds during baseline and extinction. During the conditioning phase, panel presses were reinforced. The second group, which was yoked to the first group, received noncontingent stimulation throughout the entire study. The third group received the same procedure as the first group, but the light source was placed in another location. Within the first group, lower response rates were observed during the noncontingent phases, and higher response rates were observed during the experimental phase. A similar result was obtained for the third group. Cavanaugh and Davidson (1977) could have strengthened their experimental control by yoking the first group to their own response patterns. This would require, however, that those subjects receive a positive reinforcement condition prior to the noncontingent condition.

Moreover, Dunst and Lingerfeld (1985), using a conjugate reinforcement paradigm, found similar results in 2 to 3 month-old infants with an AABAA repeated-measures design. A mobile was suspended over the subject's crib, and was attached to the infant's leg by a cord. The mean rate of leg kicking was highest during conditioning, and lower during the noncontingent baseline and extinction phases. The authors did not incorporate a yoking procedure. If the authors, however, exposed some subjects to the positive-reinforcement condition first, then they could have used a yoked each noncontingent session to a positive reinforcement session.

Finally, Reeve, Reeve & Poulson (1993) observed reliable differences in responding when they explored the parameters of delayed social reinforcement on infant vocalization rate using a single-subject-repeated-reversal design, with 2 to 6 month-old infants. During Experiment I, unsignaled 3-second delayed reinforcement was compared with DRO and a

yoked noncontingent condition. In the noncontingent condition, the schedule of social reinforcement was determined by yoking each infant's no-contingency session to one of his/her 3-second delayed reinforcement sessions. This was accomplished by measuring the time intervals at which the putative reinforcers were delivered, and then delivering the putative reinforcers at those same time intervals during the noncontingent condition. Rates of vocalization were systematically higher during the experimental condition, and lower during the noncontingent control conditions. These results allow one to conclude that vocal behavior was under reinforcer-contingency control.

It is important to note that Reeve et al.(1993) is the only study that yoked stimulation between experimental and control conditions within subjects in a single-subject-experimental design.

Shepperd (1969), reported using a noncontingent procedure in a multiple schedule (FR3 NONCON) of reinforcement with a 3-month-old infant. Differential responding to the FR3 schedule was not established. The response rate during the noncontingent condition was comparable to that of the FR component. In this case, Sheppard concluded that the noncontingent schedule may have produced adventitious reinforcement, and, thus, it may have functioned as an intermittent reinforcement schedule.

In summary, forty-seven percent of the studies in this section compared positive reinforcement with noncontingent stimulation in a within-subjects design, and thus reduced the possible effects of individual differences in responding, whereas 53% used a between-groups design. The possible rationale for selecting a between-groups design was not evident here.

Although a noncontingent condition potentially controls for the effects of elicitation, one must ensure that the amount of stimulation is kept comparable. Usually this is accomplished by yoking a noncontingent schedule to the experimental condition. Thus of the 15 studies that used noncontingent stimulation, only three (i.e., Clifton & Meyers, 1973; Finkelstein & Ramey, 1977; Wolan Sullivan & Lewis, 1989; ) yoked the amount of stimulation between groups, and demonstrated behavior change. In contrast, only one study (Reeve et al., 1993) yoked the amount of stimulation within-subjects, and subsequently found reliable differences in responding.

## DRO as an Experimental-Control Condition

Differential reinforcement of other behavior, or DRO, is another experimental-control procedure against which one might compare positive reinforcement. Reinforcers are delivered, but never following emission of a target response. The reinforcer delivery may occur on a response-based or a time-based schedule. In a time-based schedule, some minimum amount of time must elapse since the emission of the target response before the positive reinforcer can be delivered. If the target response is emitted, the reinforcer is withheld for a particular interval of time. Thus, a time-based-DRO procedure contains two parameters. First, it specifies the minimum time intervals between reinforcer delivery. Second, it specifies the minimum time interval that must elapse following a target response for reinforcer delivery. During a response-based DRO procedure, the subject must emit a given response or a set of responses, other than the target response, during a specified temporal interval, to receive the reinforcer. If the target response should occur, then the reinforcer is not delivered. The advantage of DRO as a control condition is that it contains a contingency that prevents adventitious reinforcement of target responding, yet

this schedule allows one to keep the amounts of stimulation comparable across experimental and control conditions.

Eleven, or 10% of the 108 studies, used a DRO procedure against which to compare positive reinforcement (i.e., Brown & Brown, 1974; Butterfield & Siperstein, 1970; Etzel & Gewirtz, 1967; Glavin & Moyer, 1975; Poulson, 1983; & Reeve, Reeve, Brown, Brown, & Poulson, 1992; Reeve, Reeve, & Poulson, 1993; Sheppard, 1969; Wahler, 1967, 1969; Weisberg, 1969).

Of the eleven studies in which a DRO procedure was used compare positive reinforcement, 100% of them used a within-subjects analysis to make that comparison. This is consistent with the large percentage of studies (92%) that used a within-subjects design to compare positive reinforcement with no stimulation. This is in contrast, however, to the smaller percentage of studies (47%) that compared positive reinforcement with noncontingent stimulation in a within-subjects design. Furthermore, the studies that used a between-subjects comparison between positive reinforcement and noncontingent stimulation did not demonstrate the possible rationale for selecting this design. Nevertheless, of the 106 studies reviewed up to this point, the overwhelming majority (86%) used within-subjects design.

Because there is a negative contingency between the response and the reinforcer during the DRO procedure, this procedure typically decreases response strength. By comparing only positive reinforcement and DRO schedules, it cannot be determined whether DRO suppresses response rates over some baseline measure, or whether positive reinforcement increases rates over some baseline measure. Thus, one would need to incorporate some other baseline measure against which to compare positive reinforcement

or DRO. A baseline procedure might, for example, consist of a period of observation with no programmed consequences. This would allow one to determine whether positive reinforcement produced rates higher, the same, or lower than baseline, even though response levels under positive reinforcement might be higher than those during DRO.

Five studies used a response-based DRO procedure (i.e., Butterfield & Siperstein, 1970; Etzel & Gewirtz, 1967; Glavin & Moyer, 1975; Wahler, 1967, 1969). In contrast, the six studies used a time-based DRO procedure (i.e., Brown & Brown, 1974; Reeve et al. 1992; Reeve et al. 1993; Poulson, 1983; Sheppard, 1969; Weisberg, 1969).

Butterfield & Siperstein (1970) used a response-based DRO procedure when they compared reinforcement for sucking with reinforcement for not sucking in an ABCBC design. They found that median suck duration was higher during positive reinforcement for sucking when compared to a noncontingent baseline. In a repeated-reversal design, mean suck duration was systematically lower during the DRO procedure compared to those during positive reinforcement. Thus, Butterfield and Siperstein found a main effect for contingency and concluded that infants sucked reliably longer under the "during" (positive contingency) condition than under the "between" (DRO) condition. The authors, however, did not present any information regarding the amounts of stimulation presented during the experimental conditions.

In two studies, Wahler (1967,1969) used a response-based DRO procedure. In the first study, Wahler (1967) compared DRO with positive reinforcement in an ABA within-subjects-reversal design with two groups of 3 month-old infants. Smiling was the target response, and vocalizing and head turning were the "other" responses. For one group, mean frequency of smiling was higher during positive reinforcement when compared to

baseline; and during DRO, mean frequency of smiling decreased when compared to positive reinforcement. Wahler states that the amount of stimulation was comparable between positive reinforcement and DRO, although this was not documented. In addition, no single-subject data were presented.

In the second study, Wahler (1969) used DRO in a reversal design with an infant who was 3 weeks old at the beginning of the study. During baseline, the mother socially reinforced (i.e., talked, touched, smiled) two different vocalizations (e.g., "ba" & "ummm"). During the first DRO condition, the mother reinforced one vocalization, but not the other. During the second DRO condition, the mother switched the reinforcement contingencies by reinforcing the previously unreinforced vocalization and not reinforcing the previously reinforced vocalization. With the implementation of the first DRO condition, the frequencies of the unreinforced vocalization decreased from baseline. In contrast, the frequencies of the reinforced vocalization were comparable with those of baseline. Moreover, during the last DRO condition, when the reinforcement contingencies were switched, the frequencies of the previously reinforced vocalization decreased when its reinforcement was discontinued, whereas the frequencies of the previously unreinforced vocalization increased with the introduction of reinforcement. Wahler (1969) does not mention, however, whether the densities of reinforcement were kept comparable. Therefore, one does not know whether the changes in responding were the result of elicitation or reinforcement contingencies.

Etzel & Gewirtz (1967) also used a response-based DRO procedure in an ABA single-subject design to decrease crying by reinforcing smiling in one infant, and reinforcing smiling and eye contact in another infant. Crying and/or fussing always

remained on extinction. Although infant's rates of smiling increased and rates of crying decreased during DRO as compared to baseline, rates of the other responses did not always vary inversely. Moreover, during extinction ( i.e., smiling no longer reinforced) smiling did not systematically decrease for one subject, and crying did not systematically decrease for another subject. Although the authors met their goal of identifying procedures to decrease crying and increase smiling, they could not demonstrate that the responses were under reinforcer control. For example, when reinforcement of smiling was discontinued during extinction for the first subject, the data were not stable before the authors switched conditions. Thus, it would have been beneficial for the authors to run extinction for a longer period of time. If the authors had waited for the data to stabilize, they would have been in a better position to verify the effects of the independent variable on responding. Additionally, the authors did not mention whether the amount of stimulation was kept comparable across experimental conditions.

Finally, Glavin and Moyer (1975) used a response-based DRO to decrease crying in a single subject. In their study, they compared continuous reinforcement of crying with DRO, and found increased levels of crying during continous reinforcement and decreased levels of crying during DRO. Nevertheless, they did not repeat these observed changes in behavior in a reversal design. That is, they did not verify the behavior change by withdrawing the independent variable. Therefore, a functional relation between the independent and dependent variables was not established. That is, it cannot be verified that reinforcement contingencies were responsible for behavior change.

Five studies made use of a time-based DRO procedure (i.e., Brown & Brown, 1974; Poulson, 1983; Reeve et al. 1993; Reeve et al. 1992; and Weisberg, 1969). In the first

study, Poulson (1983), compared DRO and continuous reinforcement using a repeated-reversal-single-subject design in the conditioning of infant vocalization rate. During the DRO procedure, the infant received social reinforcement every 2 seconds as long as the infant did not vocalize. If the infant vocalized, social reinforcement was delayed for 4 seconds. Levels of stimulation during DRO were equal to or higher than during the experimental condition. Infants produced systematically higher rates of vocalizations during continuous reinforcement or CRF when compared to DRO. Poulson reported, however, that because a baseline measure was not included, it was not known whether CRF increased responding over DRO, whether CRF maintained some rate of responding and DRO decreased that response rate, or whether CRF increased responding over some base rate and DRO decreased that rate. Nevertheless, it can be concluded that infant vocalization rates were under the control of social reinforcement because behavior changed systematically with the implementation of the positive and the negative contingencies.

Two other studies using a time-based DRO procedure, focused on delay of reinforcement in infants (Reeve et al.,1992; Reeve et al.,1993). In the first delay study, those authors used the same time-based DRO as Poulson (1983) on the vocalization rates of infants in a single-subject-repeated-reversal design. Levels of social stimulation during DRO were equal to or higher to those during delayed reinforcement. Vocalization rates with delayed social reinforcement systematically increased above levels measured during DRO. Vocalization rates also systematically decreased during DRO and were comparable to baseline rates (Reeve et al. 1992).

In the second study by Reeve et al.(1993), positive reinforcement was compared with DRO and another control procedure, noncontingent stimulation. Social stimulation during DRO and noncontingent conditions were equal to or higher than those of delayed reinforcement. Infant vocalization rates systematically increased during the unsignaled delayed social reinforcement condition compared to DRO and to noncontingent conditions. In a second experiment, DRO was compared with a window-open baseline in an alternating treatments design. The window-open baseline was chosen instead of a window-closed baseline because, according to the authors, the infants tended to cry when seated in front of a closed blind. During the window-open baseline, the blind remained open throughout the 3-minute intervals. The mother was asked to touch and play with the infant through the opening. That comparison between window-open baseline and DRO yielded no differences between vocalization rates. Nevertheless, there were systematic increases in vocalization rates during the positive reinforcement condition when compared to DRO and to window-open baseline conditions. The rates of social stimulation during baseline were equal to or higher than those during delayed reinforcement. Therefore, both delay studies demonstrated reinforcer-contingency control because vocalization rates systematically increased and decreased with the introduction of delayed social reinforcement and DRO.

Brown & Brown (1974) appear to have used a time-based DRO procedure to demonstrate systematic behavior change in a single subject, using an ABAB reversal design on the vocalizations of a 17 month-old child. During the DRO procedure, reinforcers were delivered "periodically" throughout the session, but never for the occurrence of the target vocalizations. Target responding was systematically higher

during the positive-reinforcement condition when compared to baseline. In addition, the DRO condition systematically decreased target responding below those obtained during positive reinforcement. Brown and Brown concluded that the reinforcement procedure served to increase the percentage of accurate vocalizations, whereas the DRO procedure resulted in a decrease in the percentage of accurate vocalizations. There authors, however, did not present data regarding the amount of stimulation presented during the DRO schedule, nor did they specify the schedule parameters. As mentioned previously, it is desirable to keep the amount and patterns of stimulation comparable across experimental conditions because this controls for elicitation effects.

Weisberg (1969) appears to have used a time-based DRO procedure in an operant discrimination study with 15-25 year-old infants using multiple and chain schedules during discrimination training. During the DRO component, a clock was set to run for a prearranged time and continually reset itself whenever a lever press occurred during the S. Response rates during S- were generally lower than rates of responding during S+. During generalization testing, peak responding occurred during the former S+ value. Weisberg also reports that during the final stages of discrimination training, about 50 to 60 reinforcers were received during the time S+ and S- were presented. Though Weisberg does not specify the exact schedule parameters, he reports that the amount stimulation was roughly constant during the implementation of the different schedules. The fact that responding was lower during the DRO component, and higher during positive reinforcement, indicates that responding was under the stimulus control.

Only one study (Sheppard, 1969) did not report any behavior change with the use of a DRO procedure as a component in a multiple schedule of reinforcement. Sheppard (1969)

reported that his subject did not meet the response requirement of the DRO schedule, so it functioned as if it were an extinction schedule.

As mentioned previously, all of the studies used a within-subjects comparison to compare positive reinforcement and DRO experimental conditions. Five of the studies used a response-based DRO condition against which to compare positive reinforcement, and six studies used a time-based DRO condition against which to compare positive reinforcement. As mentioned previously, time-based DRO schedules contain two parameters. There is a reinforcer-reinforcer interval, and an response-reinforcer interval. Both parameters must be specified to program such a schedule. The three studies that implemented a time-based DRO schedule (Poulson, 1983; Reeve et al, 1992; Reeve et al, 1993) used the same schedule parameters. That is, the reinforcer was always delivered every 2 seconds. If the infant vocalized, the reinforcer was delayed for 4 seconds. These temporally different parameters, which contain a 2:1 ratio for the response-reinforcer and reinforcer-reinforcer intervals, were successful in gaining control over infant behavior. The remaining two studies that used a time-based DRO schedule ( Brown & Brown, 1974 ; Weisberg 1969), did not specify their schedule values. Therefore, if one does not know the schedule values, one can not be certain if the reinforcers were delivered as programmed.

A response-based DRO schedule also contains two parameters. That is, there are two responses that must be specified. First, there is the target response to be decreased by the negative contingency, and second, there is the other response to be increased by the positive contingency. Of the studies that used a response-based DRO schedule, four of

them specified the target response to be decreased by the negative contingency, and the other response to be increased by the positive contingency.

In summary, all eleven studies used a within-subjects analysis to compare positive reinforcement and DRO conditions, and thus minimized the possible effects of individual differences in infant responding. Although a DRO schedule can potentially control for elicitation effects, one must ensure that the schedule parameters are identified, and that the amount of stimulation during DRO is equal to or higher than those during positive reinforcement. Thus, only three studies (i.e., Poulson , 1983; Reeve et al., 1992; Reeve et al., 1993) specified schedule parameters, ensured that the amount of stimulation was kept comparable across experimental and control conditions, and demonstrated systematic behavior change.

## Schedule Control as an Experimental Control Procedure

The last method for discussion is the establishment of differential patterns of responding under different schedules of reinforcement. In the previous sections, positive reinforcement was compared with another experimental control condition. That is, no stimulation, noncontingent stimulation, or a DRO schedule might have been compared with positive reinforcement. In this section, any two positive-reinforcement schedules might have been compared. In addition to comparing overall levels of performance on different reinforcement schedules, one can find evidence of control by schedules of reinforcement from examining more fine-grained changes in responding (e.g., "scalloping" or "break and run") to be described below. If response patterns are typical of the reinforcement schedule in effect, then one might be more confident in drawing conclusions about the operation in effect.

In operant conditioning, one might follow an intermittent schedule with an extinction condition. If extinction produced an immediate reduction in responding following intermittent reinforcement, that would suggest that the response was being elicited. For example, in Pavlovian temporal conditioning, there would be an immediate reduction in responding under similar circumstances. On the other hand, if extinction produced a gradual reduction in responding following intermittent reinforcement, then one would be more likely to conclude that the response was operant.

As described in the introduction, there are two kinds of simple schedules: ratio and interval. Ratio schedules specify the number of responses that must be emitted before a reinforcer can be delivered. Interval schedules specify the time interval that must elapse before a response can be reinforced.

Each of these schedules can be fixed or variable. In a fixed-ratio schedule, a fixed number of responses is reinforced. On small ratios, a stable response rate develops, and there is no pause after reinforcement. On high ratios, however, this schedule is typically characterized by a pause in responding following reinforcer delivery, followed by a high and steady response rate. A fixed-interval schedule specifies the amount of time that must elapse before a response can be reinforced. This kind of schedule is generally characterized by slow to moderate response rates with a pause in responding immediately following reinforcer delivery. As time for the delivery of the next reinforcer comes closer, the response rate accelerates. After continued exposure to the FI schedule, performance sometimes changes to resemble a "break and run" pattern that is generally characterized by a FR schedule.

Ratio and interval schedules can also be variable. A variable-ratio schedule specifies the average number of responses that must occur before the reinforcer is delivered. This schedule is characterized by high, steady response rate, although ratios at a higher value may produce sharp pauses in responding. A variable-interval schedule specifies an average amount of time that must elapse for a response to be reinforced. This schedule typically generates a slow to moderate response rate that is constant and stable.

Research by Catania et al. (1977) illustrates the difference between VR and VI schedules. For one pigeon, a key-peck is reinforced on a VR schedule. The key-peck of the second pigeon is reinforced whenever the key-peck of the first pigeon is reinforced. In this way, the second pigeon is reinforced on a VI schedule. The reinforcement of the second pigeon does not depend on the number of its own pecks, but on how long it takes the first pigeon to produce the required number of responses. Thus, in this research, frequency and timing of reinforcement were held constant by using yoked-control experimental chamber. After exposure to this procedure, response rates of both pigeons stabilized to exhibit a constant rate of responding; although the response rates were higher on the VR schedule than the VI schedule. Moreover, whether the VR schedule was yoked to the VI schedule, or vice versa, the VR schedule always produced higher response rates than the VI schedule.

Eight of 108 studies, or 7%, used schedule control as an experimental-control procedure. One study (Hillman & Bruner, 1972) implemented different schedules in a between-groups comparison. Those other seven studies used schedule control in a within-groups comparison (i.e., Cyrulik, Shapira-Jacobs, Jones, 1975; Darcheville et al. 1993;

Friedlander, 1968, Leuba & Friedlander, 1968; Lowe, Beasty & Bentall, 1983; Weisberg & Fink, 1966; Sheppard, 1969).

Hillman and Bruner (1972) explored the sensitivity of sucking a nipple for milk by administering FR or FI schedules to different groups of 1-to-4 month-old infants. Although the authors reported that infants responded differentially to different schedules, there was no evidence in the records of anything approximating "scalloping" toward the end of the intervals in the FI schedule or toward the end of a number of sucks in the FR schedule. Thus, there was a failure to demonstrate reinforcer control because the authors did not find the typical signature patterns on the various FR and FI schedules.

In contrast, seven studies used schedule control in within-subjects designs. Four studies did not demonstrate reinforcement-contingency control, but three studies did show reinforcement effects when they obtained typical signature patterns of responding on various reinforcement schedules in individual infants. Of those showing reinforcer control, Lowe, Beasty & Bental (1983) reported the occurrence of differential motor responding with fixed-interval-reinforcement schedules in single subjects. Fixed-interval schedules ranging in value from 10 to 50 seconds were compared in two subjects, 9 and 10 months of age. The scalloping pattern, consisting of a pause after reinforcement, followed by an accelerated response rate, was characteristic for all schedule values.

Weisberg & Fink (1966) also obtained differential motor performance, but on fixed-ratio-reinforcement schedules in five 14-19 month-old infants. All subjects started with an FR value of 2 or 3, that was increased over sessions until it reached a final value of FR10. One subject had FR 15 as a final value. The break-and-run pattern was observed for all subjects; that is, there was a high and constant rate of responding, with a temporary zero

rate immediately following reinforcement. In addition, the mean postreinforcement pause of the subjects increased as a function of increasing FI value. The running rates of the subjects declined as a function of increasing FI value. When the subjects were then placed on extinction, and responding systematically decreased.

The last author, Sheppard (1969), successfully demonstrated differential responding by using a concurrent-response multiple schedule for vocalizations and leg kicks, in an infant 3 months of age. The rates of responding for both leg kicks and vocalizations were the highest in the presence of the discriminative stimulus associated with reinforcement, and lowest during the stimulus associated with extinction. Additionally, differential responding occurred when the discriminative stimulus was changed.

As stated previously, of the seven studies that had a within-subjects design, four did not demonstrate reinforcement-contingency control. Darcheville et al. (1993), explored the relationship between fixed-interval performance and self-control in 3-to-23 month-old-infants. All subjects received FI schedules. An animated cartoon served as the reinforcer. Subjects then received a choice procedure. The youngest subjects did not show the scalloping performance typical of fixed-interval schedules. Thus, the cartoon reinforcer could have elicited responding. Three other studies examined preference for different kinds of audio-visual feedback in a concurrent schedule with the use of a two-knobbed automated toy that provided stimulation when manipulated (i.e., Friedlander, 1968; Leuba & Friedlander, 1968; & Cyrulik-Jacobs, Shapira, & Jones, 1975). Conclusions cannot be drawn about reinforcing effects from these preference studies because there were no controls to determine whether contingency relations were involved in the preference. Differential elicitation may have occurred.

In summary, seven of eight studies that compared reinforcement schedules had a within-subjects design. Only one study used a between-groups design. Those seven studies that compared reinforcement schedules in a within-subjects design minimized the potential effects of individual differences in infant responding.

Furthermore, of the eight studies that used schedule control, only three studies (Lowe, et al, 1983; Sheppard, 1969; and Weisberg & Fink, 1966) demonstrated reinforcement-contingency control and eliminated the effects of elicitation. It was the differential appearance of the signature patterns with the differential implementation of reinforcement schedules that allows one to draw that conclusion.

## Summary

In the review of the literature, it was observed that in terms of experimental control conditions against which to compare positive reinforcement, a full 83% of the studies used no stimulation as an experimental control condition. In contrast, only 14% of studies used noncontingent stimulation; 10% of the studies used DRO, and 7% of the studies used schedule control.

Of the 90 studies that used no stimulation, 8% used no stimulation in a between-groups design. In contrast, 92% used no stimulation in a within-groups design. No stimulation does not control for possible elicitation effects of presumptive reinforcing stimuli. The putative reinforcer could have elicited responding during the positive reinforcement condition, and a reduction in response strength during the no stimulation condition could be attributed to the removal of the eliciting stimulus. The use of other experimental-control conditions, therefore, is preferable because they allow one to keep the amount of stimulation comparable across conditions. Moreover, the use of these other

experimental-control conditions helps one determine whether the reinforcer-contingency relation is the controlling variable.

Of the 15 studies that used noncontingent stimulation as an experimental-control procedure against which to compare positive reinforcement, eight used a between-groups design, and seven used a within-groups design. When one uses a noncontingent schedule, it is important to keep the amount of stimulation comparable across experimental conditions. This allows one to eliminate potential elicitation effects. This can be accomplished by yoking the noncontingent schedule to the positive reinforcement condition. In fact, out of 15 studies, only three (i.e., Clifton & Meyers, 1973; Finkelstein & Ramey, 1977; & Wolan Sullivan & Lewis; 1989) yoked the amount of stimulation between groups and demonstrated significant behavior change. In contrast, only one study yoked the amount of stimulation for each subject in a single-subject design, and demonstrated systematic behavior change across experimental conditions.

Of the 11 studies that used a DRO procedure against which to compare positive reinforcement, all used a within-subjects analysis. It is important that when one uses a DRO schedule, either response-based or time-based, that one specify the schedule parameters. Additionally, to minimize possible elicitation effects, one must ensure that the amount of stimulation is equal to or higher than that occurring during positive reinforcement. Of the studies that had a DRO procedure, only three (i.e., Poulson, 1983; Reeve et al., 1992; & Reeve et al., 1993) specified the schedule paramenters, and ensured that amount of stimulation during DRO was equal to or higher than the amount during positive reinforcement. These three studies also reported systematic changes in target responding across experimental and control conditions.

Finally, of the eight studies that compared differential-reinforcement-schedule control, one used a between-groups design, and seven used a within-subjects design. Only three studies (i.e., Lowe et al., 1983; Sheppard, 1969; & Weisberg & Fink, 1966), that happen to use a within-subjects design, reported the appearance of the typical signature patterns of responding with the differential implementation of reinforcement schedules. In these three studies, responding was under reinforcer control, and potential elicitation effects were ruled out.

Thus, only 10, or 9% of the 108 studies in this review used appropriate experimental-control procedures ( i.e., DRO, noncontingent, schedule control) that controlled for potential elicitation effects. These studies demonstrated behavior change across experimental conditions.

It is possible that some authors did not demonstrate reinforcer-contingency control because their primary hypothesis was not about the functional relation between behavior and the environment, but about other inferred internal variables. One author, Friedlander (1968), studied infant preference for different kinds of feedback. Friedlander (1968), however, did not specifically address the kinds of contingency relations between behavior and environment that define reinforcement. Thus, his studies, although suggestive and important, do not directly address the issues at hand. Another author, Darcheville et al. (1993), examined the correlation between FI schedule performance and self control. His primary focus was on internal variables such as "pause sensitivity," and "impulsiveness." This kind of question posits variables inside the organism as an explanation for behavior, rather than focusing directly on manipulable, environmental variables as causes of behavior. Impulsivity, and to some extent, preference, refer to hypothetical constructs

that are believed to cause behavior change in infants. Appeal to such hypothetical constructs can sometimes override the search for more directly measurable functional relations between behavior and environment that are responsible for the observed behavior.

Another focus of this review was to record the kind of experimental design used by researchers to make the comparison between positive reinforcement and the experimental-control condition. It was observed that 82% of the authors in this review used a within-subjects or a single-subject-experimental design to compare positive reinforcement and another experimental-control condition. In contrast, only 18% of the authors used a between-groups design to make that comparison. Furthermore, 7 of those 15 studies used noncontingent stimulation against which to compare positive reinforcement, and those authors did not provide a rationale for selecting such a design. In observing the pervasive use of within-subjects design, it can be hypothesized that the authors were aware of the benefits of using such a design over a between-groups design. Although benefits of between-groups design include statistical power, and the elimination order of treatment effects, one must be concerned about the effects of individual differences among subjects. Different infants may respond differentially to different reinforcers or for differences in the quality of those reinforcers. Individual differences in response rates can also affect the performances across groups. While this is addressed by randomly assigning large numbers of subjects across experimental groups, one can not be certain that these differences will be equally distributed across groups, or that one could gather a sufficiently large number of subjects for random assignment. A smaller number of infants could be used if they were matched on relevant variables and then randomly assigned to experimental groups. It is

not always clear, however, what these relevant variables might be. With the use of within-subjects design, the effects of individual differences are reduced because each infant serves as his or her own control by experiencing each experimental condition. Thus it appears that the majority of the authors in this review were aware of the benefits of within-subjects design for the study of infant behavior.

Although a within-subjects analysis may be valuable in the identification of a functional relation between the independent and dependent variables, because it reduces the effects of individual differences, it is not the only factor to consider. Therefore, it would be beneficial to consider methodologies, such as the experimental analysis of behavior, that facilitate the identification of such functional relations.

## Experimental Analysis of Behavior

Like any other natural science, behavior analysis is the science of behavior. Behavior analytic theory consists of laws and principles derived from the experimental analysis of behavior that describe the functional relations between behavior and environmental events. That is, classes of stimuli and responses are defined by their respective effects on one another (Skinner, 1935). The goals of the science of behavior are the same as the those of other natural sciences. Those goals involve prediction, control, and understanding of behavior. The first two goals, prediction and control, are inter-related. Control over experimental variables allows one to discover the functional relations between the independent and dependent variables. Given a certain amount of control, a researcher is then able to make predictions about what can happen under different experimental conditions. The results of such manipulations enhances the understanding about the

fundamental relations between the independent and dependent variables, and the conditions under which they would be observed (Schlinger, 1995).

Given these goals, many researchers agree that the science of behavior should meet a particular set of criteria (e.g., Sidman, 1960; Schlinger, 1995) First, it must have generality or inclusiveness. Generality refers to the range of conditions under which one observes the phenomena. This can include generality of subjects, of settings, of species, and experimental conditions. Second, a scientific theory of behavior must be specific enough to be testable. That is, behavior analytic theory must contain basic functional units that are observable and measurable. It is through repeated testing under different conditions that allows the discovery of scientific facts and the modification of theory. To the extent that theories rely heavily on hypothetical constructs, a greater proportion of the hypotheses they generate may be less directly testable than those generated by more strictly behavioral theories. Third, a science of behavior should be specific enough to make accurate predictions. It is through the testing of these predictions under a range of conditions that allows one to gauge the utility of a theory. Precise prediction specifies the independent and dependent variables and the functional relations between them. Fourth, behavior analytic theory should be able to generate new ideas for research, or generate useful applications. That is, behavioral theory should direct attention to new phenomena and stimulate further research. Fifth, behavior analytic theory should be parsimonious in that it explains a phenomenon with the fewest number of assumptions. This minimizes reliance on unobservable events that are potentially unmeasurable. Thus, for a science of behavior to have utility, it should be able to meet those criteria. Behavior analysis, as stated previously, contains the functional units of behavior that has allowed it to evolve

into of a strong scientific theory. Behavior analysis can explain already existing phenomena, guide research and theory, and offer a unified account of diverse phenomena (Schlinger, 1995).

What is required is an examination of the methodology that best facilitates the identification of functional relations between independent and dependent variables in infant learning research. Poulson and Nunes (1988) recommend six methodological considerations in designing or evaluating experimental analysis of infant operant conditioning studies. They are: (a) each subject should experience each primary experimental condition, (b) data should be presented graphically, (c) the primary analysis should be in terms of single-subject data, (d) visual stability of data should be obtained for each subject before experimental conditions are changed, or there should be some criterion for judging when learning has occurred, (e) the independent and dependent variables should both be measured and reported, and (f) the relations between the independent and dependent variables should show that the intended intervention procedure was implemented. The rationale for each of these considerations will now be discussed.

The first characteristic of an experimental analysis of infant behavior is that the primary analysis between experimental and control conditions should be done within each subject. This allows each subject to serve as his or her own control, which thereby minimizes the effects of individual differences in responding. A within-subjects comparison of experimental conditions, therefore, may be more powerful in detecting a functional relation between the independent and dependent variables.

The second characteristic is that data should be presented graphically across time or consecutive sessions. A visual display of the data can be presented, for example, with line

graphs, cumulative records, or bar graphs. By providing a graphic display of the data, acquisition curves, or pattern characteristics of various schedules of reinforcement, one can determine whether learning criteria have been met under one set of experimental conditions, before introducing the comparison condition.

The third characteristic is that the primary analysis should be in terms of single-subject data. When data are summarized for a group of infants, it may appear that all subjects experienced the same change in behavior. An analysis of the individual data, however, may reflect different learning processes, either elicitation or reinforcement. Thus, to see these distinctive patterns of responding, analysis of individual data may be necessary.

The fourth characteristic is that visual stability of the data should be obtained before switching experimental conditions. If one does not wait for data to stabilize, then it is difficult to verify the effects of the systematic introduction and withdrawal of the independent variable over behavior. That is, it would make it more difficult for one to conclude that behavior has changed, much less that any behavior change was because of the independent variable, rather than some other extraneous variable.

The fifth characteristic is that the independent and dependent variables both be reliably measured and reported. One wants to ensure that the dependent variable is reliably measured and displayed across all phases of the experiment. Additionally, one would want to ensure that the procedures programmed are presented as intended and, thus, its reliable measurement is also required. This measurement of the independent variable is a prerequisite to demonstrating that a relation between the independent and dependent variables is functional in producing reinforcement contingencies for behavior.

The sixth characteristic is that the relation between the independent and dependent variables should show that the intended intervention was implemented. One can program a procedure to occur, but it may function in another way. This happened in the case of Sheppard (1969), who programmed a DRO schedule, but reported that it functioned like an extinction schedule, because the infants did not pause long enough to receive the reinforcer. Thus it is important to know whether the intended procedure was, in fact, implemented.

As stated previously, using the above criteria, Poulson and Nunes judged that only 2 out of 15, or 13%, of infant-vocal-conditioning studies between 1959-1983 demonstrated reinforcement-contingency control. The authors of those two studies, Poulson (1983) and Shepperd (1969), used appropriate experimental-control procedures against which to compare positive reinforcement and thereby eliminated elicitation as an explanation for behavior change. Poulson (1983) compared continuous reinforcement with DRO, and Sheppard (1969) used a two-concurrent-response multiple schedule. These studies also met all of the criteria recommended by Poulson and Nunes (1988) for an experimental analysis of behavior.

Although only 13% of the studies reviewed by Poulson and Nunes (1988) demonstrated reinforcement-contingency control under their criteria, this low percentage is only relevant to the infant vocal conditioning literature. It would be interesting to know whether this percentage, would be higher if these criteria were applied to the rest of the infant conditioning literature.

The present review is an attempt to include the rest of the infant literature in a similar examination. As mentioned previously, of 145 articles initially obtained, 108 were

selected for this review on the basis of three criteria: they (a) consisted of an actual experiment, (b) included a reinforcement procedure, and (c ) used infants between the chronological ages of 0-24 months. When the Poulson and Nunes criteria were applied to these studies, only 6, or 6% of 108 studies, were identified as demonstrating reinforcement-contingency control. That is an even lower percentage than the 13% reported by Poulson and Nunes (1988). Of those studies considered to be entirely successful, one study, by Poulson (1983), used DRO as an experimental-control condition against which to compare positive reinforcement. Another study, Reeve, Reeve, Brown, Brown and Poulson (1992), used noncontingent stimulation as an experimental-control procedure against which to compare positive reinforcement. Reeve, Reeve, and Poulson (1993) compared positive reinforcement with both noncontingent and DRO conditions. Finally, three studies demonstrated schedule control over behavior: Lowe, Beasty, & Bental, (1983); Sheppard, (1969) Weisberg & Fink, (1969).

Other studies in this review used appropriate experimental-control procedures, but did not meet all the criteria recommended by Poulson and Nunes. For example, Dunst and Lingerfeld (1985) observed a decrease in behavior with the use of a noncontingent procedure, as compared with positive reinforcement, but they did not provide graphs of individual subject data. Additionally, phase changes were pre-determined, and not based on stability of responding. Cavanaugh and Davidson (1977) also compared positive reinforcement with noncontingent stimulation, but they did not present any single-subject data. As was the case with Dunst and Lingerfeld (1985), stability of responding was not considered when experimental conditions were changed. Wahler (1967) compared positive reinforcement with DRO in a reversal design, but he did not present any single-

subject data. Finally, Glavin & Moyer (1975) compared positive reinforcement and DRO in an AB design with a single subject, but did not repeat the comparisons between the A and B conditions, and, thus, they did not verify the reliability of the change in behavior. Thus, these studies mentioned above did not clearly demonstrate reinforcer control, based on the criteria of Poulson and Nunes (1988), because they did not implement the methodology that would allow one to conclude that contingencies of reinforcement were in effect.

Of the six characteristics of an experimental analysis of behavior as identified by Poulson and Nunes (1988), all are important in that they help facilitate the identification of the functional variables. While many studies in this review share one or two of these characteristics, they do not constitute a full experimental analysis of behavior because the source of experimental control was still not made explicit.

## The Role of Single-Subject Design in Infant Learning Research

A researcher should be able to demonstrate functional relations between the independent and dependent variables, whether using a within-groups design, a between groups design, or a single-subjects design. Whereas a group design has its advantages, it typically does not tease out individual differences in responding, and it is possible that each subject represents different cases of experimental control. For example, measures of central tendency for a group of infants will probably show a decrease in response rates with the implementation of a control procedure following a period of contingent reinforcement. The group data, however, may reflect different cases of experimental control within each group. Poulson and Nunes (1983) illustrate this point using hypothetical examples of infants under the two different kinds of control. In a situation

when the infant is under elicitative-stimulus control, responding during a DRO schedule, following contingent praise for vocalizations, would show high rates of vocalizations. In a situation when the infant is under reinforcing-stimulus control, responding during a DRO schedule, following contingent praise, would show low rates of vocalizations. To assess which control is operative in any situation, further analysis is required. One, for example, could use a single-subject-reversal design. This would involve re-introducing the subject to the positive reinforcement and the control conditions to see if there was a functional relation between the independent and dependent variables for a given subject and for each subject.

Although infant researchers frequently used within-groups experimental designs in which each subject does experience each of the experimental conditions, those authors did not present single-subject data. Typically, inferential statistics with repeated measures were used to detect differences within subjects across conditions. While measures of central tendency might provide information regarding group changes, it is possible that not all subjects in a group experienced a behavior change across experimental conditions. That is, it may produce what looks like the same change, but the behavior could reflect different learning processes, elicitation on the one hand or reinforcement on the other.

Traditionally, between-group design is taught to students in psychology and in the social sciences. It is believed that the use of a large sample size and the subsequent use of statistical analyses yield powerful experiments that are internally and externally valid. That is, if inferential statistics show significance across experimental groups, then two conclusions may be drawn. First, it may be concluded that there is a functional relationship between the independent and dependent variables. Second, it may be

concluded that the experimental effect is general to the target population. As mentioned above, the researcher, however, may encounter more difficulty in demonstrating a functional relationship between the independent and dependent variables because of the possibility of different cases of experimental control within a group that would go unnoticed in a typical group design. (Poulson & Nunes, 1988). Furthermore, group design methodology does not ordinarily allow for the analysis of the behavior in individual subjects and the antecedents and consequences that influence them. Single-subject methodology involves tracking individual response patterns across the implementation of different experimental conditions and, thus, allows one to directly determine the source of experimental control.

The recommendations for an experimental analysis of behavior as described by Poulson and Nunes (1988), are useful suggestions in conducting operant research in that they facilitate the identification of the type of control that is operative over behavior. Although it is recognized that not all research has to consist of entirely of single-subject design, it does seem important to have some experimental analysis of individual data to illustrate the learning process that is occurring. Single-subject design can be a useful tool for infant researchers because it can facilitate the identification of the functional relations between infant behavior and environmental consequences.

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