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Cross-Modal Transfer of Shape is Difficult to Demonstrate in One-Month-Olds

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We tested 1-month-olds for cross-modal transfer of shape between touch and vision using a procedure described by Meltzoff and Borton, but including controls for side bias and stimulus preference. In Experiment 1 ($N = 48$), infants' looking times to smooth and nubby visual stimuli were not influenced by previous oral exposure to one of the shapes during the preceding 90 s, except for an effect on the first test trial in one group; this effect could have been due to limited cross-modal transfer, to Type 1 error, or to side bias, possibly interacting with a small stimulus preference. The failure of that effect to replicate in a group ($N = 16$) with less side bias (Experiment 2) suggests that it was not due to cross-modal transfer. Experiment 3 ($N = 32$), an exact replication of Meltzoff and Borton's experiment, also failed to yield evidence of cross-modal transfer. Overall, there is not good evidence that 1-month-olds can transfer information about these shapes from touch to vision. Future studies exploring the ability to transfer information about other shapes will be easier to interpret if they include controls for side bias and stimulus preference.

INTRODUCTION

Meltzoff and Borton (1979) reported that infants as young as 1 month of age can transfer information about the shape of objects from touch to vision; after being presented orally with a smooth or nubby pacifier, infants looked longer at a matching shape than at a novel shape with which it was paired. This is the first and only published evidence of cross-modal transfer of shape during early infancy. It changed the conceptualization of the development of cross-modal perception from accounts based on the development of separate sensory systems that are later interrelated (reviewed in Rose & Ruff, 1987) to accounts that begin with amodal perception at birth that is later refined to encompass arbitrary correspondences and nested relations (e.g., Bahrick, 1992). Meltzoff and Borton's result also contributed to the conceptualization of other aspects of development, including object perception (Mandler, 1990), concepts (Mandler, 1988), self-concept (Dowling, 1990), modularity (Marshall, 1984), language (Flege, 1988), motor skills (Thelen, 1995), autobiographical memory (Howe & Courage, 1997), and evaluations of psychoanalytic theories (Johnson, 1991; Zinkin, 1991).

Based on Meltzoff and Borton's original report, we began a series of studies on the specificity of the transfer and its development (Maurer, 1993). After a number of puzzling results, we decided to reexamine the original finding. Other studies have shown that young infants can transfer information about the material properties of objects (their rigidity; Gibson & Walker, 1984) or their movement (imitation of tongue protrusion; Meltzoff & Moore, 1977).[1] There has been, however, no independent demonstration of cross-modal transfer of information about the structure or shape of objects. In fact, there have been two published failures to find transfer of information about shape from oral touch to vision during the first month of life (Brown & Gottfried, 1986; Pêcheux, Lepecq, & Salzarulo, 1988).[2] Meltzoff

[1] Some authors argue that young infants' imitation of tongue protrusion, although a robust phenomenon, is not based on cross-modal transfer (Anisfeld, 1996; Jones, 1996).

[2] Using an operant procedure in which the infant's sucking controlled how long visual stimuli were presented, Kaye and Bower (1994) reported that newborns look longer at a shape that matches the pacifier they are sucking, than at a different shape, at least during the first presentations of those shapes, before the infants had had an opportunity to learn the nature of the contingency. This result is difficult to interpret, because the authors inferred looking time from the infant's sucking rather than measuring it directly. There is no a priori reason to assume that the infant was looking at the stimulus the entire time that his or her sucking controlled its presentation, nor that the relationship between the measured sucking and the actual looking time was the same for the two visual stimuli. The literature demonstrates that there are complex interactions between visual stimulation, attentiveness, arousal, and sucking (reviewed in Crook, 1979). Under many circumstances, there is an inverse relationship between visual stimulation/attentiveness and sucking (e.g., Haith, 1966; Mendelson, 1979): That is, when infants sucked longest in Kaye and Bower's procedure, they were *least* likely to have been looking. But under some circumstances, visual stimulation facilitates sucking (e.g., Mendelson & Haith, 1975). Thus, it is possible that the two visual stimuli affected newborns' ongoing sucking in different ways, and that the reported effect was not based on cross-modal perception. In any event, because they did not measure looking time, Kaye and Bower's report of significant cross-modal effects on looking time are difficult to interpret and we have ignored it in the text.

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and Borton's original report remains the key and often-cited evidence for cross-modal perception of shape in early infancy, and previous failures to replicate their findings have been largely ignored. This is illustrated by the many recent papers that cite the original study without mentioning the failures to replicate (Howe & Courage, 1997; Karmiloff-Smith, 1995; Mix, Levine, & Huttenlocher, 1997; Monk, Gunderson, Mechling, & Grant, 1996; Schmuckler, 1996).

The two studies that failed to find transfer from oral exploration to visual preferences used procedures similar to those of Meltzoff and Borton (1979). Brown and Gottfried (1986) tested 1-, 3-, and 5-month-olds with four different pairs of shapes including the nubby/smooth pair used by Meltzoff and Borton. Following 90 s of oral or tactile familiarization to each stimulus, 1-month-olds showed no visual preference for the familiar shape or a novel shape. Even the older infants showed no consistent transfer. Pêcheux et al. (1988) obtained a negative result in 1-month-olds tested following 90 s of exposure to a smooth or nubby pacifier. Their post-hoc analyses, however, have led some researchers to conclude that there was positive evidence of transfer. Pêcheux et al. divided the 16 infants into a group of 10 whose mouthing of the shape had not habituated during the 90 s, and a group of 6 whose mouthing had habituated. They then counted the number of infants in each group who looked longer at the matching shape during the test (a familiarity preference), and compared the result to the expectation that by chance half the infants would look at the matching shape and half at the novel shape. The results were marginally significant, $p = .055$, by a one-tailed test in the group that had not habituated, and were not significant in the group that had habituated. This result is very weak: It was not obtained from the group of infants as a whole, or from the group of infants who habituated (i.e., the group more likely to have information to transfer). Moreover, the result was only marginally significant, and then only by a one-tailed test, despite the absence of strong grounds on which to predict a novelty or familiarity preference: Meltzoff and Borton found a familiarity preference after 90 s of oral familiarization, whereas Gibson and Walker (1984) found a novelty preference after shorter exposure, which would be less likely to lead to habituation.

One possibility is that changes in procedure between the original and follow-up studies account for the different results, and that young infants demonstrate cross-modal transfer of shape under only some conditions. For example, Brown and Gottfried (1986) presented each infant with three pairs of stimuli, in addition to the smooth and nubby pair. The large number of stimuli presented within a short time may have made each stimulus harder for the infants to remember and to discriminate from the other stimuli. It also would have raised the overall level of stimulation, a manipulation known to alter the visual preferences of young infants (Karmel, Gardner, & Magnano, 1991; Turkewitz, Gardner, & Lewkowicz, 1984). In addition, the infants may have demonstrated transfer with the nubby/smooth pair, but the evidence for it could have been obscured by collapsing the data across all four pairs before the analyses. Pêcheux et al. (1988) might have found negative results because their stimuli had smaller nubs than those used by Meltzoff and Borton, and hence the smooth and nubby stimuli were less distinctive from each other. Their infants also were slightly younger than those tested by Meltzoff and Borton, and hence might not yet have developed the same cognitive skills.

Alternatively, the findings of Meltzoff and Borton may be spurious, and 1-month-olds may not in fact be able to transfer information about shape from touch to vision. Two features of the original design are troublesome: (1) There was no baseline group to assess infants' preference to look at one stimulus more than another, independent of any oral stimulation, and (2) there was no within-subject control for side bias. If infants have a natural visual preference for one of the shapes—say, the nubby stimulus—then Meltzoff and Borton's results may reflect an influence of oral stimulation in only one of the two groups—say the group that looked longer at the smooth visual stimulus following exposure to the smooth pacifier. Although such a finding in only one group may be evidence for cross-modal transfer of shape, it is important to establish whether 1-month-olds recognize the similarity between oral and visual shape only for a stimulus like the nipples and fingertips they have sucked on many times daily (the smooth stimulus) or whether they can also do so for an unfamiliar shape (the nubby stimulus); [cf. Bushnell's (1994) distinction between matching-by-recognition versus matching-by-analysis]. That can be determined only by comparing the visual preferences of infants in each experimental group to those of a baseline group that receives no oral stimulation.

Meltzoff and Borton attempted to control for side bias by presenting the familiar stimulus to half the infants on the left and to half the infants on the right. For a very large sample, this would be an adequate control. But in a small sample ($n = 16$ per group), it is possible for the familiar stimulus to appear by chance on the preferred side of more than half the infants. With such a small sample, the only adequate control for side bias is a procedure in which each infant is presented the familiar stimulus on both the left and right sides, so that the preference for a particular stimulus can be distinguished from the preference for a particular side (see Gibson & Walker, 1984, for a similar argument).

Because of their theoretical significance, it is important to establish whether or not the findings of Meltzoff and Borton are robust. Therefore, we attempted to replicate the findings by closely following their procedure while introducing controls for side bias and stimulus preferences. A preliminary report of Experiment 1 has been published previously (Maurer, 1993).

EXPERIMENT 1

In Experiment 1, as in Meltzoff and Borton (1979), infants first were given a nubby or a smooth pacifier to suck on for 90 s, then their looking times were measured as they viewed enlarged versions of the stimuli presented side by side. The exposure times, stimuli, and age range matched the original report. Unlike Meltzoff and Borton, we included a second test trial with the smooth and nubby visual stimuli reversed left-to-right in order to control for side biases. We also included a baseline group that received the visual test without prior oral experience in order to determine if infants have a natural preference to look at either the smooth or the nubby stimulus.

We also deviated from Meltzoff and Borton's procedure in at least two other ways. They faced the infants away from the visual stimuli during oral familiarization, then turned them toward the visual stimuli just before the test. To eliminate the possible disruption or extra stimulation arising from being turned around, we instead seated the infants facing the visual stimuli from the start, and occluded those stimuli by a blind until the visual test. Second, to keep infants from covering their eyes or putting their hands in their mouths—which might interfere with processing the shape of the pacifier and/or mimic the feel of the smooth pacifier—we swaddled the infants loosely in capes formed from pillowcases. A possible third modification is that we counterbalanced the side from which the pacifier was presented; we do not know whether Meltzoff and Borton did the same.

Method

Participants. Forty-eight 1-month-olds (M = 29.9 days old; *range* = 26–33) were randomly assigned to three groups of 16, with equal numbers of males and females in each group. In this and all subsequent experiments, all infants were between 38 and 42 weeks gestation, weighed at least 2,500 grams at birth, and had no known abnormalities. Participants were recruited from a subject pool of mothers who had volunteered their infants at birth for possible later study. An additional 13 infants were tested but were not included in the analysis because of equipment failure (n = 2), fussiness (n = 2), incomplete data (n = 3), or procedural error (n = 6).

Meltzoff and Borton used only a single observer to record looking times following oral familiarization. Because the purpose of our study was to examine the robustness of their findings, we used two observers and replaced an additional ten infants for whom the two observers disagreed greatly on an infant's looking preference (one observer recorded no looking on either trial; the proportion of time looking at the nubby stimulus differed by .30 or more across the two test trials or on each of the two trials; or the proportion of time looking at the nubby stimulus differed by .50 or more for the first test trial).

Stimuli. Both the visual and oral stimuli were replicas of those described by Meltzoff and Borton (1979). As we were unable to obtain the original stimuli, we followed the description given in the paper. Specifically, the tactile stimuli were spherical and nubby plastic pacifiers attached to a plastic cylindrical rod that projected through the base of a large rubber pacifier. The plastic used by Meltzoff and Borton is no longer manufactured, so we formed the pacifiers from a similar material (Thermal Bond-7204-4, AdChem Inc., Manchester, CT). The surface of the smooth pacifier was untextured and soft, but not malleable. The nubby pacifier was similar except for eight nubs evenly distributed across the surface. The spherical diameter of the pacifiers was 1.2 cm; the nubs were 2 mm in height and 3 mm in diameter, so that the total diameter of the nubby pacifiers was 1.6 cm.

As in Meltzoff and Borton, the visual stimuli were scaled 5:1 to the pacifiers. The spheres were constructed from Styrofoam balls, with the addition for the nubby stimulus, of protrusions constructed from wooden doweling and distributed across the surface of the sphere so as to give the same appearance as the nubby pacifier. The stimuli were painted orange and were suspended by black doweling, with four of the protrusions on the nubby stimulus facing the infant. The visual stimuli were mounted so that their fronts were 32 cm from the infant's eyes and their centers 28 cm apart (47°). At that distance, the spherical stimulus was 10.7° in diameter and the nubby stimulus was 14.25°. The luminance of both was approximately .86 log cd/m^2 against a background of approximately −.14 log cd/m^2.

Apparatus. The infant sat in an infant seat fitted with foam cubes to support the head, facing a black frame in which the visual stimuli were presented against a black background. A gray blind could be lowered in front of the frame to occlude the visual stimuli. On either side of the frame were peepholes for two observers, who could see the infant's eyes but not the visual stimuli. Above and behind the infant's head was an incandescent bulb, mounted so as to light the visual stimuli without casting any shadows. To either side of the infant were black curtains designed to prevent the infant from seeing the rest of the room. The observers used hand-held buttons connected to a Commodore PET 2001 Series computer to time the duration of the infant's looking time to each stimulus. The computer recorded looking times and signaled the end of each visual test trial by advancing a slide projector. This slide projector was used only to signal the ends of trials, and the projector bulb was not illuminated.

Procedure. An experimenter, who was not involved in testing the infant, positioned the visual stimuli and selected the pacifier before the procedure started. Two other experimenters, both blind to the positioning of the visual stimuli, tested the infant. The procedure was explained to the parent(s), who then signed a consent form. Infants were swaddled in a light green or light blue pillowcase, designed to prevent them from removing the pacifier from their mouths, from sucking on their hands, or from covering their eyes. They were then strapped securely into an infant seat and turned to face the gray blind, which concealed the visual stimuli. During the first 90 s, infants either: (1) sucked on a nubby pacifier (nubby group), (2) sucked on a smooth pacifier (smooth group), or (3) did not receive a pacifier (baseline group). The pacifier was held by one of the two observers. The holder did not interact with the infant once the procedure had begun and held the pacifier while standing to the side and slightly behind the infant, out of the visual field. For half the infants in each experimental group, the holder stood to the infant's left; for the other half, to the infant's right. As in Meltzoff and Borton (1979), the nubby pacifier was held so that four nubs pointed upward in the infant's mouth, leaving the other four to point downward. As in Meltzoff and Borton, the 90-s familiarization period was broken up into two phases. During the first phase, the room was lit by an overhead fluorescent bulb, but there was little visual stimulation for the infant to observe (i.e., only the gray blind and the black curtains). After 80 s, the overhead light was extinguished, leaving the room in darkness, and the blind was raised. Ten s later, the pacifier, if any, was removed from the infant's mouth, the holder moved behind an observation hole, and the visual stimuli were illuminated by the incandescent bulb behind the infant. The illumination during every phase of the procedure matched that described by Meltzoff and Borton.

The two observers timed the infant's looks to the left and right visual stimuli. The 20-s test trial began when at least one observer recorded a visual fixation on either stimulus, and continued until the sound of the projector advancing signaled the end of 20 s. The light illuminating the stimuli was then extinguished, leaving the room in darkness. One of the observers rotated the visual stimuli left-to-right without looking at, or feeling, them. The stimuli were then relit, and the second test trial began. The inter-trial interval was approximately 7 s. The familiar stimulus was placed on the left during the first test trial for half the infants in each group, and on the right for the other half.

Data analyses. In order to control for side bias while taking into account that an infant's total looking time toward the two stimuli might be longer during one test trial than the other, we converted the data from each trial into the proportion of time the infant looked at the nubby stimulus as a function of the time the infant looked at either stimulus. This proportion was the mean of those recorded by the two observers. We then used the mean proportion across the two test trials for each infant. To determine whether oral familiarization influenced infants' visual preferences, we used an analysis of variance (ANOVA) to compare looking times to the nubby stimulus in the baseline group and the groups that had sucked on the smooth or nubby pacifier. Cochran's tests indicated that homogeneity of variance was not violated. To determine whether there was an overall preference for one of the two visual stimuli, we also used a t test to compare the proportion of looking to the nubby visual stimulus to a chance level of .50. To test whether side bias affected the results, we calculated the correlation between the proportion of looking to the left on the first and second test trials, and counted the number of infants with a strong side bias (looking to the same side more than 80% of the time during both test trials).

Because our results differed from the original findings, we also conducted analyses modeled on the statistics reported by Meltzoff and Borton (1979). In order to do so, we analyzed the data from only the first test trial and from only the two experimental groups. As in Meltzoff and Borton, we collapsed the data from the two experimental groups and used (1) t tests to compare the proportion of looking to the matching shape to a chance level of .50, and (2) a binomial table to compare the number of infants looking longer at the matching shape to a chance level of 50%. Finally, to determine whether there might be an effect in only one of the two experimental groups, we conducted the analyses separately for each group. Because these analyses used data from only parts of a counterbalanced design, they would not ordinarily be considered legitimate. Nevertheless, we reasoned that they might point to possible reasons for the discrepancies in the literature that could be verified in subsequent experiments.

Results

For each group, there was good agreement between the two observers on the mean proportion of time infants had looked at the nubby stimulus across the two test trials: nubby group, $r(15) = .95$, $p < .001$; smooth group, $r(15) = .85$, $p < .001$; baseline group, $r(15) = .98$, $p < .001$.

The three groups did not differ significantly in the proportion of time spent looking at the nubby visual stimulus across the two test trials, $F(2, 45) = 1.34$, $p > .10$. As shown in Figure 1, 1-month-olds in all groups tended to look longer at the nubby visual stimulus than at the smooth visual stimulus; that is, proportion of looking time is greater than 50%, $t(47) = 2.67$, $p = .01$, two-tailed. This preference, however, was not altered by oral exposure before the test either to the matching nubby shape or to the nonmatching smooth shape. Although significant in the three groups combined, the preference for the nubby visual stimulus was not significant in the baseline group considered by itself, $t(15) = 0.28$, $p > .10$.

There also was significant evidence of side bias for individual infants: The proportion of time spent looking to the left on the first and second test trials were significantly correlated, $r(47) = .465$, $p < .001$. Half of the infants had a strong side bias (>80% on both test trials). Despite strong side biases in individual infants, there was no consistent side bias in the overall sample: The mean proportion of time spent looking to the left across both test trials was .54.

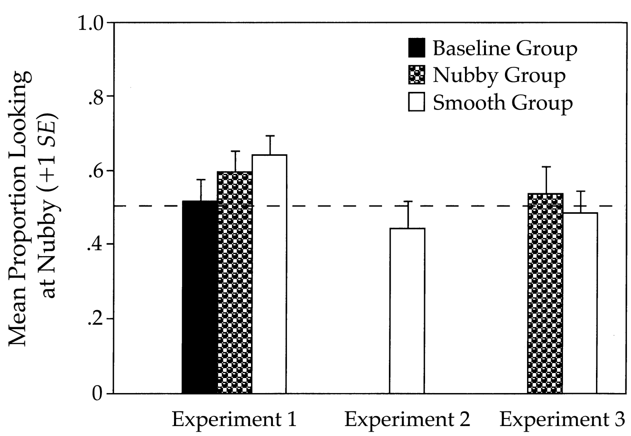

Figure 1 Mean proportion of looking time (+1 *SE*) directed toward the nubby visual stimulus across the two test trials following 90 s of sucking on a nubby pacifier (nubby group), smooth pacifier (smooth group), or of no oral stimulation (baseline group).

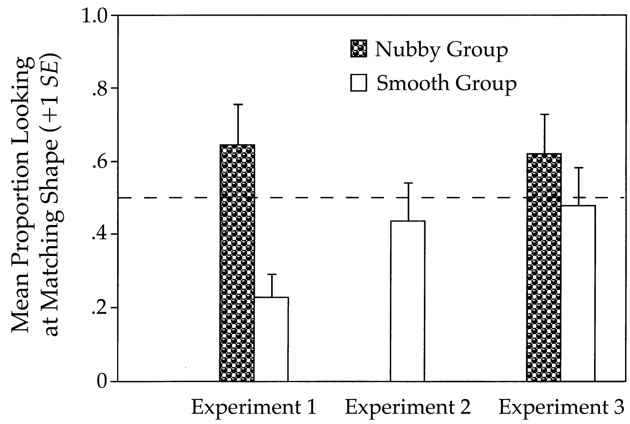

Figure 2 Mean proportion of looking time (+1 *SE*) during the first test trial directed toward the visual stimulus matching the pacifier (nubby/smooth) previously sucked on for 90 s.

Unlike the results reported by Meltzoff and Borton (1979), during the first test trial the two experimental groups did not show a preference for the shape they had sucked on (see Figure 2): They looked at it only 43.5% of the time, a result that is not significantly different from a chance level of 50%, $t(31) = -.88$, $p > .10$, two-tailed. Only 12 of the 32 1-month-olds looked longer at the shape matching the one they had sucked on, $p > .10$, two-tailed binomial test.[3] When we analyzed the data on looking times from the first test trial separately for the two groups, the results for the nubby group were not significantly different from 50%, $t(15) = 1.31$, $p > .10$, one- or two-tailed. Ten infants from the nubby group looked longer at the matching shape during the first test trial, whereas six looked longer at the nonmatching shape, a distribution that is not significantly different from 50-50. Infants in the smooth group, however, behaved differently: They looked at the matching stimulus only 22.6% of the time during the first test trial, a percentage significantly *less* than 50%, $t(15) = -4.30$, $p < .001$, two-tailed. Only 2 of the 16 infants in the smooth

[3] Based on a preliminary analysis, Maurer (1993, footnote 2) reported that 10 of the 32 infants looked longer at the matching shape. That analysis included infants for whom we subsequently discovered a procedural error. When those infants were replaced, the results are as reported in the text.

group looked longer at the matching stimulus on the first test trial, a number significantly *less* than 50%, $p < .01$, two-tailed binomial test. Note that the effect in the smooth group, although significant, is in the opposite direction from Meltzoff and Borton, who reported that the majority of infants (collapsed across the smooth and nubby groups) looked at the shape *matching* the one they had sucked on.

Discussion

Overall, there was little evidence for cross-modal transfer: Infants' distribution of looking between the smooth and nubby visual stimuli across the two test trials was not influenced by the stimuli they had just sucked on. Because of the left-to-right reversal of the visual stimuli between the two test trials, this analysis controlled for side bias and is likely to accurately represent the infants' preferences for the two visual stimuli following oral familiarization. When we considered the data from just the first test trial from one experimental group, however, there was a significant effect: Infants familiarized with the smooth pacifier looked longer at the *non*matching (nubby) shape. Although in the opposite direction from Meltzoff and Borton (1979) and true for only one of the two experimental groups, these data would nevertheless constitute evidence for cross-modal transfer for one shape (a smooth shape), if we can rule out the alternative explanation that the observed effect is an artifact of Type 1 error (from statistical fishing) or of an inadequate control for side bias and/or stimulus preference.

There was independent evidence that side bias affected the looking times of many infants: Half looked at the same side more than 80% of the time during both test trials, and overall the proportion of time spent looking to the same side on the two trials was highly correlated. There also was equivocal evidence of stimulus preference: In the three groups combined, but not the baseline group alone, infants looked significantly longer at the nubby visual stimulus than the smooth visual stimulus. A stronger preference could occur on a single test trial in any one group because of side bias, that is, the nubby stimulus could be placed by chance on the preferred side for more than half the infants in that group. The preference could disappear in trial 2 because the stimulus is placed on the opposite, nonpreferred side. There is independent evidence to support that explanation for the results on trial 1 from the smooth group: Of the infants who looked longer to the same side across both test trials, the nubby stimulus was placed on the preferred side during trial 1 for 80% of the 10 such infants in the smooth group, but only at a percentage near chance (55% and 50%, respectively) for the 11 such infants in the nubby group and 12 such infants in the baseline group.

Thus, it is possible that the smooth group's apparent preference for the nonmatching nubby shape on the first test trial, but not across the two test trials with the left-to-right reversal, may have resulted from side bias, possibly interacting with a small stimulus preference. A plausible interpretation, then, is that 1-month-olds do not show transfer of shape from touch to vision and that artifactual results can occur if the design does not include a left-to-right reversal as a control for side bias in *individual* infants. Meltzoff and Borton (1979) did report that there were no "significant differences for fixating right versus left side." Our data show, however, that the lack of side bias at the group level does not prove there was no side bias at the individual level: Overall, infants in Experiment 1 looked almost equally to the left and the right, even though half of the infants had such a strong side bias that they looked to the same side more than 80% of the time on both test trials.

Alternatively, it is also possible that (1) 1-month-olds show cross-modal transfer only for shapes that are similar to ones they have encountered before, and (2) the very presentation of the first test trial alters the infants' performance so that the effect is obliterated in the second test trial. Both propositions are plausible. First, as mentioned in the introduction, the smooth pacifier may have felt similar to nipples, breasts, knuckles, and/or fingertips that infants had in their mouths previously. That familiarity may have facilitated the formation of a schema during the 90 s of oral presentation, a facilitation that would not have occurred in the group exposed to the nubby pacifier. As well, because of the crudeness of 1-month-olds' visual perception, the smooth visual stimulus may have looked like breasts or fingers they had seen previously.

Second, although a within-subject left-to-right reversal of the test stimuli is necessary to control for side bias in a small sample, that procedure may obscure evidence for cross-modal transfer. Consider infants in the smooth group who looked preferentially at the nonmatching (nubby) stimulus during the first test trial. During the second test trial, they were given a choice of a smooth visual stimulus, which was familiar from oral familiarization, and a nubby visual stimulus, which was familiar from the first test trial. The effects of oral and visual familiarity may have cancelled each other out. Alternatively, the stimulation from the first test trial may have disrupted performance on the second test trial (e.g., Gardner & Karmel, 1994): It may have raised the overall level of stimulation sufficiently to move the infants' visual preference away from the more arousing nubby stimulus (i.e., the larger stimulus with more contour), so that during the second test trial they looked at it no longer than the smooth stimulus.

EXPERIMENT 2

In Experiment 2, we repeated the procedure for infants in the smooth group using a new group of 16 1-month-olds. If the effects from trial 1 of Experiment 1 were an artifact of side bias or Type 1 error, they would be unlikely to replicate in a second group of infants.

Method

Sixteen 1-month-olds (M = 30.5 days old; *range* = 27–33), were recruited in the same way as in Experiment 1. The stimuli, apparatus, and procedure were identical to those in Experiment 1 for the group familiarized with the smooth pacifier. No infants were excluded from the analyses.

Results

Interobserver reliability was .91, $p < .01$. There was less evidence of side bias than in Experiment 1: The correlation between the proportion of time spent looking to the left on the first and second test trials was .20 (cf. .47 in Experiment 1) and only 31% (cf. 50% in Experiment 1) of infants looked to the same side more than 80% of the time during each of the two test trials.

During the first test trial, infants looked 56.5% of the time at the nonmatching (nubby) visual stimulus and 43.5% of the time at the matching (smooth) visual stimulus, percentages that do not differ significantly from a chance level of 50%, $t(15) = .62$, $p > .10$. Of the 16 infants, 6 looked longer at the matching visual stimulus and 10 looked longer at the nonmatching nubby visual stimulus. The results were similar for the two test trials combined: Infants looked at the matching smooth stimulus 44.3% of the time, $t(15) = -.80$, $p > .10$, with an even split of 8 infants looking longer at it and 8 looking longer at the nonmatching nubby stimulus.

Discussion

In this group of 16 infants with less side bias than those tested in Experiment 1, there was no evidence of cross-modal transfer of information about the smooth shape, even when the analysis was restricted to the first test trial. Overall the results indicate that the one suggestion of cross-modal transfer of shape in Experiment 1 was an artifact of Type 1 error or side bias. They emphasize that spurious results can occur if there is not a second test trial with the stimuli reversed right-to-left, as was missing from all previous studies on cross-modal transfer of shape during the first month of life (Brown & Gottfried, 1986; Meltzoff & Borton, 1979; Pêcheux et al., 1988; see footnote 2, above). The reported negative results (Brown & Gottfried; Pêcheux et al.) may have arisen because side bias masked a real preference for the matching visual stimulus. The reported positive results in one or, less likely, both of Meltzoff and Borton's experiments may have resulted from testing a group of infants with strong individual side biases and serendipitously putting the matching stimulus on the preferred side for more than half the infants during the single visual test trial—as we did during the first test trial for the smooth group in Experiment 1.

Thus, the results of Meltzoff and Borton (1979) may be spurious. It is also possible, however, that procedural changes eradicated the original effect. Unlike previous studies finding no evidence of transfer for smooth and nubby shapes (Brown & Gottfried, 1986; Pêcheux et al., 1988), in Experiment 1 (and 2) we attempted to duplicate the original testing conditions of Meltzoff and Borton. In retrospect, however, we are aware of three differences: The fan from the slide projector that we used to signal the end of the trial added some white noise to the room, the infants wore pastel-colored capes, and an experimenter did not rotate the infant 180° and did not manually center his or her head just before the visual test. Although these changes would be unimportant to an adult, it is possible that they change the task for young infants. Note, however, that unlike Meltzoff and Borton, our design was based on a comparison of two experimental groups that sucked on a smooth or nubby pacifier, to a baseline group, which received no oral stimulation. As all three groups were exposed to the same white noise, cape, and lack of rotation, these aspects of the procedure are unlikely to have affected the comparison among groups. Nevertheless, given the importance of establishing whether or not the results reported by Meltzoff and Borton are robust, in Experiment 3, we eliminated these procedural differences and ran an exact replication.

EXPERIMENT 3

Experiment 3 was an exact replication of Meltzoff and Borton (1979), except that we included a second visual test trial with the stimuli reversed left-to-right. Unlike Experiment 1, there was no noise from a projector, infants did not wear capes, and there was no baseline comparison group. Also, as in the original study, infants faced away from the visual testing area while sucking on the pacifier and were rotated to face it just before the visual test.

Method

Participants. Thirty-two 1-month-olds (M = 29.8 days old; *range* = 26–33) were randomly assigned to two groups of 16. An additional 18 infants were tested but were not included in the analysis because of equipment failure ($n = 1$), procedural error ($n = 1$), fussiness ($n = 9$), a parent talking during the experiment ($n = 1$), or the two observers' recording of divergent looking times ($n = 6$; see Experiment 1 for criteria).

Stimuli, apparatus, and procedure. The apparatus and stimuli were the same as in Experiment 1. The procedure was the same with the following exceptions. The infant did not wear a cape and sat initially facing a blank white wall with his or her back to the visual stimuli. After the infant had sucked on the smooth or nubby pacifier for the initial 80 s, the overhead light was extinguished, leaving the room in darkness, and the infant was swivelled 180° to face the visual stimuli. Ten s later the pacifier was removed, the infant's head was centered if necessary, and the visual stimuli were illuminated. The end of each visual test trial was signaled by a red light above each peephole that was visible to the observer but not to the infant. Except for the inclusion of the second visual test trial and the use of two observers, the procedure matched exactly that described in Meltzoff and Borton (1979).

Results and Discussion

For each group, there was good agreement between the two observers on the mean proportion of time infants had looked at the nubby stimulus across the two test trials: nubby group, $r(15) = .96$, $p < .001$; smooth group, $r(15) = .91$, $p < .001$. There was less evidence of side bias for individual infants than in Experiment 1: The correlation between the proportion of time spent looking to the left on the first and second test trials was lower (.30) than in Experiment 1 (.47). Fewer infants (41%) than in Experiment 1 (50%) looked to the same side more than 80% of the time during each of the two test trials.

The analyses were the same as in Experiment 1, but all tests were one-tailed to facilitate comparing our results to Meltzoff and Borton (1979). As shown in Figure 1, the two groups did not differ significantly in proportion of time spent looking at the nubby visual stimulus across the two test trials, $t(30) = .57$, $p > .10$. Unlike Experiment 1, infants did not look significantly longer at the nubby visual stimulus across both test trials, $t(31) = .24$, $p > .10$.

As shown in Figure 2, during the first test trial, infants also did not show a preference for the shape they had sucked on: They looked at it only 54.9% of the time, a result that is not significantly different from a chance level of 50%, $t(31) = .66$, $p > .10$. Only 18 of the 32 infants looked longer at the matching shape, $p > .10$. During the first test trial, the nubby group looked at the matching stimulus 62.0% of the time and the smooth group looked at the matching stimulus 47.9% of the time. Neither of these percentages is significantly different from 50%, nubby group, $t(15) = 1.11$, $p > .10$; smooth group, $t(15) = -.20$, $p > .10$. Eight of the 16 infants in the smooth group and 10 of the 16 infants in the nubby group looked longer at the matching stimulus; neither of these distributions differs from 50-50, ps $> .10$.

Thus, even when we replicated exactly the methods described in Meltzoff and Borton (1979), we failed to find evidence for transfer from sucking on a smooth or nubby pacifier to visual versions of those stimuli.

GENERAL DISCUSSION

The analyses of these three experiments provided no evidence that 1-month-olds can transfer information about the shape of smooth and nubby stimuli between touch and vision. Thus, when we controlled for side bias in individual infants by examining the results from two test trials with a left-to-right reversal, in none of the three experiments were infants' looking preferences for smooth and nubby visual stimuli influenced by which of these stimuli they had sucked on previously. The one hint of an effect—on the first test trial in the group that had sucked on a smooth pacifier in Experiment 1—seems to have arisen from Type 1 error and/or side bias, possibly interacting with a small preference for the nubby visual stimulus: It was apparent only on the first test trial, and not across the left-to-right reversal; it was manifested as a preference for the nubby visual stimulus and hence was in the direction opposite to the preference reported by Meltzoff and Borton; and it did not replicate in independent groups of infants tested in Experiments 2 and 3.

Our results fit with previous failures to find transfer for these shapes at 1 month of age (Brown & Gottfried, 1986; Pêcheux et al., 1988) and call into question Meltzoff and Borton's (1979) claims. Because of the importance of those claims, we reexamined the results across our three experiments to be sure we had not missed or masked evidence of cross-modal transfer. One concern could be that in Experiment 1 an overall preference to look at the nubby stimulus longer than the smooth stimulus masked the demonstration of cross-modal transfer. (Meltzoff and Borton did not report their results in a form that allows one to tell whether the infants they tested showed an overall looking preference for the nubby stimulus.) In Experiment 1, infants looked at the nubby visual stimulus 58.4% of the time overall, a percentage that is significantly different from a chance level of 50%. The preference was not significant, however, in Experiments 2 and 3 and, nevertheless, there was no evidence of cross-modal transfer. Another concern is that the strong side biases we documented masked evidence of cross-modal transfer, demonstrating that 1-month-olds are influenced more by side than matching shape, but not necessarily that they cannot perceive the matching shape. Restricting the analyses, however, to infants who did not show a strong side bias and averaging across the five experimental groups we tested, the mean percentage of looking at the matching visual stimulus was 52.7% for the first test trial and 53.5% for both trials. Neither proportion differs from a chance value of 50%. A final concern is that individual infants might have shown preferences that were masked in the group averages. To the contrary, Table 1 supports the conclusions from the group statistics that infants' preferences were not influenced systematically by the relationship of the visual test stimulus to the shape they had just sucked on.

Although we have no basis for drawing conclusions about what other than cross-modal transfer might have created the effects reported by Meltzoff and Borton, our results do demonstrate the importance of controlling for side bias in studies of young infants, either by a left-to-right reversal of the test stimuli for every infant or by running such large groups that one can be confident that each stimulus being tested has been placed on the preferred side of half (no more and no less) of the sample. The necessary group size for the latter strategy is likely to vary with testing conditions and the age of the infants. What is clear from our results is that groups of 16 were not adequate to permit the use of a single test trial for 1-month-olds tested with these smooth and nubby stimuli. In the five groups we tested with pacifiers across these three experiments, between 9 and 11 of the 16 infants in each group looked longer at the same side during both test trials. The percentage of those infants for whom we serendipitously placed the nubby visual stimulus on the preferred side during the first test trial was near chance for two groups, but 67% to 80% for the other three groups.

Table 1 Number of Infants with Each Possible Looking Preference

	Preference for Matching Stimulus[a]	Preference for Novel Stimulus[b]	No Preference
Experiment 1	9	9	14
Experiment 2	5	6	5
Experiment 3	7	12	13

[a] Number of infants looking at the matching stimulus >55% of the time.

[b] Number of infants looking at the novel stimulus >55% of the time.

The results of this study indicate that one should be skeptical about 1-month-olds' ability to transfer information about the shape of smooth and nubby stimuli from touch to vision. They may be able to easily transfer information about other shapes or about other object properties. Indeed, studies that were designed to avoid artifacts from side bias have demonstrated that infants from birth to 1 month of age can transfer information about other properties, viz. information about intensity from vision to hearing (Lewkowicz & Turkewitz, 1980), information about synchrony from vision to hearing (Bahrick, 1996), and information about the rigidity of objects from touch (oral exploration) to vision (Gibson & Walker, 1984). Other than Meltzoff and Borton (1979), however, there has been no published demonstration of cross-modal perception of shape in infants younger than 3 to 4 months of age, and there are apparent limitations as late as 6 months of age (Rose, Gottfried, & Bridger, 1981; Streri, 1987; Streri & Pêcheux, 1986; see footnote 2, above).

The possibility that very young infants are able to match stimuli based on intensity or synchrony prior to being able to match based on more qualitative features such as shape is consistent with most theoretical accounts of the development of cross-modal transfer (Bahrick, 1992; Bushnell, 1994; Schneirla, 1965; Turkewitz, Gardner, & Lewkowicz, 1984), including our own hypothesis of neonatal synesthesia (Maurer & Mondloch, 1996). Bushnell, for example, suggests that matching will be facilitated by spatial proximity and/or shared temporal patterns. Thus, it would not be surprising if infants matched a bouncing sound with a synchronously-moving object or a malleable (as opposed to rigid) cylinder with a deformable (as opposed to rigidly-moving) rod. The visual and oral stimuli presented in our studies likely were not matched in intensity: The nubby stimulus is more intense visually, because it contains more contour (e.g., Karmel & Maisel, 1975), but it may not be more intense orally because less of it is in contact with the mouth. The oral and visual stimuli also did not share either a temporal pattern or spatial proximity: They were presented sequentially and did not move or bend.

Furthermore, infants may be captured by the most salient part of a stimulus; if the most salient feature differs across modalities, matching is difficult (Bushnell, 1994). It is possible that 1-month-olds were captured by the hardness of the pacifiers during oral exploration, rather than either texture or shape, making each of the visual stimuli equally familiar/novel. If the hardness is especially salient for them, they would show cross-modal transfer for rigidity (Gibson & Walker, 1984) but not for shape. They also may have attended to the size of the stimuli, which did not match across modalities.

Future research is needed to determine what types of cross-modal transfer are possible during early infancy, and if they include the cross-modal transfer of information about the shapes of objects. Those studies will be easier to interpret if they include a baseline group to assess initial preferences, and if they include two test trials with a left-to-right reversal. Only with these additional data will we be able to decide whether theoretical accounts of development should take cross-modal perception of shape as a starting point or as an emergent ability.

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