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Accessing one-to-one correspondence: Still another paper about conservation

Rochel Gelman

When given experience designed to highlight the fact that the specific cardinal values of sets in one-to-one correspondence are the same (or different), three- and four-year-old children pass the standard number conservation tasks of equivalence and non-equivalence. The initial training involved set sizes of three and four items; the conservation transfer trials involved set sizes of eight and 10 items. Even where success was defined as being able to give a correct judgement *and* explanation on at least one task in the small-set range *and* one task in the large-set range, children did well. In Expt 1, 75 and 88 per cent of the three- and four-year-olds, respectively, met this criterion. In Expt 2 (run by an experimenter who was naïve to the hypotheses and results of Expt 1) 58 and 75 per cent of the respective age groups met the same criterion. It is concluded that the children must have accessed an available, albeit implicit, ability to use one-to-one correspondence. Otherwise they could not have passed the large set-size tasks which are beyond the range they count accurately. The paper ends with a defence of Rozin's (1976) accessing account of cognitive development.

Yes, the focus of this paper is on Piaget's number conservation task. Specifically, I confront seemingly contradictory results: preschool children do not conserve number. Yet, as revealed on the magic task (e.g. Gelman, 1977) they know that the operations of addition and subtraction alter set size whereas a change in the length of an array is number-irrelevant. I have *avoided* writing that the young children who pass the magic studies can conserve, this for the obvious reason that they would fail Piaget's task. Instead, I have granted them a *number-invariance scheme* wherein the operations of addition and subtraction are classified as number-relevant and others like displacement as number-irrelevant. Still, if they know that a length change is number-irrelevant how can they possibly fail to conserve?

I shall argue that preschoolers fail the number conservation task because they lack an *explicit* understanding of the principle of one-to-one correspondence. Despite an implicit understanding of one-to-one correspondence, this understanding is inaccessible under most circumstances. The proof of the argument is that there are some circumstances which yield successful performance on the Piagetian task. By successful, I mean that an explanation is given for a correct judgement – even on set sizes too large to permit accurate counting.

To begin, it helps to contrast the magic paradigm with the conservation task. In the former, young children are shown two sets of objects on separate trays placed side by side. Over a period of covering, shuffling and uncovering the displays to find 'the winner' (so designated by the experimenter), children come to expect that each tray will have a specific number of items, e.g. 'The winner has two; the loser has three'. Then unexpected variations, such as a rearrangement of the display or removal of items, are surreptitiously introduced. When children encounter the unexpected changes, they react with surprise, ask how the change came about and tell us whether the numbers have changed or not, e.g. 'Not four, only one-two-three; took one', 'Still three; just moved them'. As evident in these examples, there is a tendency for the children to use a counting algorithm to figure out answers.

In the magic experiment: the displays are placed side by side; the set sizes are small ($n \geqslant 5$); and counting behaviours are observed. In contrast, in the conservation experiment the two displays are one above the other: set sizes are seldom smaller than six; counting behaviours are seldom observed and, when they are, they are discouraged. In both tasks

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the child has to judge the effects of relevant and irrelevant transformations. A young child in the magic study can (and often does) do this by comparing the *specific* expected values with the ones encountered. There is no requirement that she use the principle of one-to-one correspondence. Indeed the magic task does not lend itself readily to this end since the displays are side by side. And since small sets are used, the young child's tendency to count single arrays serves as a reliable algorithm (Groen & Resnick, 1977; Gelman & Gallistel, 1978).

Piaget (1952) placed a premium on the ability to use one-to-one correspondence to judge numerical equivalence. He assumed that one-to-one correspondence is the psychologically primitive basis for a judgement of equivalence—a position that is congruent with many formal definitions of number (see Kline, 1972; and Greeno *et al.* (unpublished), for further details).

Gelman & Gallistel (1978) accepted Piaget's view that a child who conserves uses the operation of one-to-one correspondence. However, they maintained that the use and understanding of one-to-one correspondence is a later achievement, one which indexes the ability to reason about *non-specified* values. For Gelman & Gallistel, preschoolers' understanding of whether two sets are numerically equal rests on their ability to determine whether they yield the same particular cardinal value when counted. Hence, preschoolers are granted the ability to reason about specified values but not non-specified ones. Since, as Piaget pointed out, a true test of the use of one-to-one correspondence should rule out the tendency to count and thereby achieve a specific numerical representation, success on the standard conservation task should be beyond the very young child. (Parenthetically, it should be clear why Piaget insisted children not be allowed to count and small set sizes be avoided.)

A strong form of the Gelman & Gallistel hypothesis would have it that preschoolers are *never* able to reason about non-specified numerical values and thus should be unable to reason about equivalence judgements derived from an understanding of one-to-one correspondence. Since we first presented the argument in this strong form, I have come to question it.

Despite Bryant's critics (e.g. Katz & Beilin, 1976; Starkey, 1981), his conclusion that preschool children can and do use one-to-one correspondence to judge equivalence relations (Bryant, 1972, 1974) is probably correct. Brush (1972), Cooper *et al.* (1978), and Starkey (1978) all studied the preschooler's understanding of addition and subtraction with a method that first required the use of one-to-one correspondence. Children watched as an experimenter placed one object in each of two separate containers; then another pair in each, etc. In all three sets of experiments, some tasks involved set sizes beyond the counting range of their subjects. Even three-year-olds were able to indicate whether the containers had the same number of items. This done, the children then continued with a series of addition (and subtraction) trials with either one or both of the covered displays. For example, having established an equivalence relation, Cooper *et al.* (1978) placed another object in one container and then asked the child about the numerical relation. Since the three- and four-year-old children could participate in these experiments, they must have used one-to-one correspondence to judge equivalence and the subsequent effects of addition and subtraction. Still, there was no clear evidence that very young children could conserve—until Markman's (1979) results appeared.

Markman distinguishes between concepts that are organized as *classes* as opposed to *collections* (Markman & Siebert, 1976; Markman, 1979). The difference between concepts of *trees* and *forests* illustrates the distinction. Given a particular instance of a tree, one can answer whether or not it is a member of the class *trees*. However, given the same instance of the same tree, one cannot answer whether it is a member of a forest. There must be other trees nearby, i.e. the instance *tree* is a member of a forest only if it is in proximity with other trees. Markman contends that class terms focus attention on the particular members of a display, whereas collection terms focus attention on the overall characteristics of the display. She hypothesized that were young children induced to think of the rows in a conservation task as collections, their attention would be drawn to the emergent properties such as numerical value. Hence, she assessed the ability of four- and five-year-old children to conserve as a function of whether they were tested with the standard (class) or modified collection question. As an example, one group of children was asked the collection question, 'Does your army have as many as my army?' The other group was asked the standard question, 'Do you have as many soldiers as I do?' The exact same displays were used; except for the substitution of collection terms for class terms, the two conditions followed the standard conservation procedure.

Children in the class condition correctly answered about one and a half of four conservation trials. Children in the collection condition answered twice as many correctly. In addition they were able to give explanations. Given the explanation data, it is hard to deny that Markman's subjects conserved numbers. Presumably then, they accessed a quantitative use of the principle of one-to-one correspondence.

If a preschool child can access the principle of one-to-one correspondence, what might be the source of this ability? A clear candidate is the ability to count since counting involves the use of one kind of one-to-one correspondence: a unique count tag must be assigned to each and every object in an array. However, when counting a young child need not recognize the equivalence relation between the number of count tags and number of items in a display; after all only one set can be seen, the other is represented within the child. Thus, although the competence model for preschool counting grants the implicit use of one-to-one correspondence, it does not follow that the child has ready access to the knowledge (see Gelman, 1982; and Greeno *et al.*). A similar argument is made to distinguish between rule-governed sentence use and metalinguistic knowledge. A young child's ability to produce and understand language is rule-governed (e.g. Clark & Clark, 1977). Despite this, access to these rules on metalinguistic tasks will be beyond him for some time to come (e.g. Gleitman *et al.*, 1972).

How to make explicit the principle of one-to-one correspondence to children who can count but not conserve? We showed children pairs of rows of objects in one-to-one correspondence that represented either equal or unequal values. A child was first asked to count one of the rows and then indicate its cardinal number. Then she was asked to count the other row and then give its cardinal value. This done, she was asked if both rows had the same number. Note the judgement of equivalence was not solicited until the child had already achieved a specific cardinal value for both rows. This was done here and on other trials intentionally. We wanted to show the role of one-to-one correspondence in the definition of cardinal number by pointing out and making explicit the fact that displays which are (or are not) perceptually in one-to-one correspondence yield the same (or different) specific cardinal values when counted.

The proposed training had to be done with set sizes young children could count, i.e. around three to four items. In contrast the conservation post-tests had to include set sizes beyond the range young children count reliably. Should transfer occur, but be restricted to small set sizes, the findings would support the strong version of the Gelman & Gallistel hypothesis; should transfer also occur on large sets, the hypothesis would have to be revised.

Experiment 1

Children

Seventy-~~nine~~ children participated in the study. Three three-year-olds failed to follow instructions. The remaining 36 three- and 36 four-year-olds were randomly assigned to the experimental group ($n = 16$), the cardinal-once group 1 ($n = 12$) and the no cardinal group ($n = 8$). The mean age and range for the respective three-year-old groups were 42 months (37–47 months); 43 months (39–47 months) and 42 months (37–47 months). The comparable figures for the three four-year-old groups were 55 months (49–59 months); 54 months (49–58 months); and 54 months (50–59 months).

Children were in attendance at one of two day-care centres or one nursery school. They came from heterogeneous socio-economic and racial backgrounds. Overall the group tended towards a middle middle-class background.

Procedures and rationale

The design of the study included two age groups (three- and four-year-olds) and three training groups. Each group participated in a two-phase experiment which lasted 10–15 minutes. In the first phase, children were given the treatment for the group they were in. In the second phase all children were treated alike. They were tested on conservation of equals and non-equals in both the small and large set size ranges.

Since, in our experience, the children in the target age ranges from the schools involved almost never conserve, we did not run a control group of children who were simply given conservation tests during phase 1. It was presumed that the random assignment of children yielded comparable abilities across groups.

Phase 1

The experimental condition: Experience with two rows. The children were shown two rows of equal (4–4) or unequal numbers of items (4–3) placed horizontally and in one-to-one correspondence. Half the children were first shown two equal rows of equal lengths; half were first shown the two unequal rows of different lengths. Objects here and elsewhere were blue toy turtles measuring 2·5 cm in diameter.

A child was first asked to count the number of items in one of the rows. That row was then covered by the experimenter's hands and the child was asked 'How many are under my hands?' This was repeated for the remaining row. The child was then asked to judge whether the uncovered rows contained the 'same number or a different number' of items. All children answered these questions correctly.

Next, the child watched as the length of one row was transformed and then was asked to judge whether the specific number of items in that row had changed. For example, after lengthening or shortening one of the rows, the experimenter pointed to the altered (or unaltered row) and asked 'Are there still four (three) there?' If the child's response was 'Yes' a comparable question was asked for the remaining row. The child was then asked to judge whether the rows had the same number or a different number of items and to explain his judgement. The order of transformation types was randomized. Transformations on unequal trials yielded rows of the same length; equal trials involved rows of different lengths.*

One three-year-old responded 'No' to the initial question on the transformation trials. He was asked to count the array and judge again whether it still contained four items. Then he agreed to the numerosity of the array and the procedure continued as above.

Control conditions: There were two control conditions. In the first, children counted only one row and answered the 'how many' question. Children in the second condition were also asked to count one row; but they were not asked for the cardinal value. These groups are referred to as the *cardinal-once* and *no cardinal* controls.

Children in the cardinal-once control condition were shown single rows instead of pairs of rows.

* Gréco (1962) used a similar procedure with older children. However, he first solicited an initial equivalence judgement and then had children count one row. Also, it is not clear whether the same children had experience with both equivalence and non-equivalence relations.

They first counted one linear row (of three or four items); stated its cardinal value after the experimenter covered it; watched as that array was transformed (lengthened or shortened); and then judged whether the array still had three (or four) items. Each row was altered four times to provide a comparable number of trials across pretest conditions. The presentation order of set size was counterbalanced; that for transformation type was randomized.

In the cardinal-once condition, children had no experience with rows in one-to-one correspondence. Thus, unlike those in the experimental group, they had no opportunity to note that rows in one-to-one correspondence were assigned the same (different) cardinal values. Still, given that the pre-test could have set them to consider the specific value before and after a transformation, some transfer might occur.

The second control group—no cardinal—was asked to count single rows of three or four items for as many trials as the first control group. They were not, however, required to indicate the cardinal value rendered by a count. We expected the children in this group to behave as if they had no conservation-relevant experience. They received no explicit cardination experience and no equivalence experience. We anticipated they would fail to conserve, as three- and four-year-olds typically do.

Phase 2

Conservation tests. Conservation tests were administered immediately after phase 1. All children were given four conservation tasks representing two set sizes (large and small) and two types of number conservation (equivalence and non-equivalence). In these conditions two transformations (lengthening and shortening) were performed.

The small-set tasks used two rows of five toy turtles on the *equal* task and one row of five versus four turtles on the *unequal* task. The respective displays for the large set-size tasks were 10 and 10 and 10 versus eight.

The order of presentation of large and small set sizes was counterbalanced as was the order of conservation of equality and inequality tasks within a set-size range. The equal arrays were equal in length prior to the transformation and unequal in length after being transformed. The reverse was true for the non-equivalent arrays: before being transformed they were unequal in length and then equal in length after the transformation.

The conservation trials were run in the standard way older children are tested and *included requests for explanations.* Likewise, children were discouraged from counting. Tape-recordings were obtained here as well as during training.

Results and discussion

Training: Phase 1. As expected, children had no trouble counting the small set sizes. There were no errors in response to the initial request to count the number of items. In the experimental and cardinal-once conditions, requests of 'how many' were also answered correctly.

When experimental subjects initially saw the effect of transforming one of the displays they were simply asked whether 'that row still has four (or three)'. In the three-year-old group 50 per cent counted *before* they answered this question: the same result obtained for 4–4 and 4–3 displays. By the second transformation the respective figures were down to 31 and 19 per cent. The four-year-olds' tendency to recount before saying a transformation was irrelevant was not as striking. Thirty-eight and 25 per cent recounted on their first transformation; 31 and 13 per cent did so on their second transformation. Not too surprisingly, when these same children were asked why the two displays still had the same number, they made reference to their specific values and/or recounted again. There were only a few children who appealed to the irrelevance of the transformation or the absence of addition and subtraction.

Children in the cardinal-once group were less inclined to recount their single array when asked if it still had three (or four) after the first transformation. The respective three- and four-year-old figures were 25 and 6 per cent. The greater tendency to count on the part of experimental children presumably reflects an initial hesitation about the equivalence of set-size value when they first encountered a conflict between perceptual cues and numerical values of the two arrays.

Conservation: *Phase 2*. Table 1 presents the overall proportion of correct conservation judgements as a function of set sizes and condition. Three- and four-year-old children did very well; a full 68 and 74 per cent, respectively, were correct responses. Inspection of the table reveals no obvious effect of set size for the experimental group, i.e. the experimental children did about as well on the larger set sizes as on the smaller ones.

Table 1. Overall proportion correct judgements on conservation tasks in Expt 1

	Condition		
Values of displays and age group	Experimental ($n = 16$)	Cardinal-once ($n = 12$)	No cardinal ($n = 8$)
4 and 5			
3 yr	0·73	0·08	0·19
4 yr	0·75	0·58	0·19
5 and 5			
3 yr	0·69	0·13	0·06
4 yr	0·75	0·58	0·13
8 and 10			
3 yr	0·68	0·08	0·00
4 yr	0·66	0·29	0·13
10 and 10			
3 yr	0·75	0·08	0·00
4 yr	0·63	0·38	0·13
Overall			
3 yr	0·68	0·09	0·06
4 yr	0·74	0·46	0·15

Both control groups did poorly compared to the experimental one. In neither three-year-old control condition is there much evidence of conservation. More than half of the four-year-olds in the cardinal-once condition transferred on small set sizes. In contrast to their experimental counterparts they did not show much transfer on the larger set sizes.

Explanations of incorrect judgements were as expected. They made reference to irrelevant perceptual variables, contained fabrications or made no sense.

Explanations of correct conservation judgements were coded as *unacceptable* if a child counted *before* answering the same or different questions. Likewise, so were answers which stated the obvious (e.g. 'they both have a lot'). This left as *acceptable* explanations which included: references to irrelevant transformations (e.g. 'they just moved'); references to addition or subtraction ('you need to put another to make them the same'); references to the initial equality or inequality of number; appeals to one-to-one correspondence arguments: and counts that *followed* a correct judgement, e.g. 'They're still the same number because... well, you see there's 1, 2, 3, 4, 5 here and 5 there'.

Two independent raters coded the explanations. They agreed on 97 per cent of their judgements. The overall conditional probabilities that correct judgements were backed up by adequate explanations were 0·82 and 0·93 for the three-year-olds and four-year-olds, respectively. Given these high rates of explanations, the conclusion that children in the experimental group did conserve is on firm ground. When four-year-olds in the cardinal-once group gave correct judgements, they too tended to provide explanations

Table 2. Proportion of each type of explanation of correct judgements in Expt 1

Group	Explanation's focus				
	Addition–subtraction	Irrelevant transformation	1–1 Correspondence	Number	No explanation
Experimental: 3 yr	0·04	0·34	0·21	0·23	0·18
Experimental: 4 yr	0·04	0·53	0·09	0·24	0·07
Cardinal-once: 4 yr	0·10	0·20	0·15	0·35	0·20

although not quite as often as their experimental counterparts. The conditional probability was 0·77.

Table 2 shows a breakdown of the explanations which followed a correct judgement. Even if we exclude references to number, 63 and 67 per cent of the three- and four-year-olds gave adequate explanations. If we do the same for the four-year-olds in the cardinal-once control group, they again look worse than the experimental groups of either age: still 45 per cent of their explanations would qualify under this more stringent criterion.

Table 3 provides information regarding an individual child's tendency to conserve, i.e. give a correct response and explanation, on at least one of the tasks with small sets *and* one with large sets. Again, children in the experimental group look very good. Seventy-five and 88 per cent of the respective three- and four-year-olds pass this criterion. In contrast no three-year-olds in either control group do and a smaller percentage of the four-year-old cardinal-once controls do. Still a respectable level, 50 per cent of the four-year-olds in the first control group did pass this criterion.

In the analyses presented above, there is a slight tendency for the three-year-olds in the experimental group to do worse than the four-year-olds. This reflects a tendency for them to conserve less consistently. Whereas half of the four-year-olds in the experimental condition gave correct judgements on all of their trials, only 25 per cent of the three-year-olds did. The younger children were more shaky conservers, despite their ability to provide explanations.

In sum, three- and four-year-old children benefited from a brief session (5–7 min) wherein the emphasis was on their using specific cardinal values to make judgements of equivalence. Since the children transferred what they learned to small-item *and* large-item conservation tests and since they also provided explanations, two conclusions follow. First, they accessed an ability to conserve. Second, although preschoolers are biased toward reasoning about specific numerosities, they are not restricted to just this avenue. Under certain conditions, they can be induced to solve conservation tasks they would fail if they

Table 3. Percentage of children in Expt 1 who conserved with explanations on at least one small and one large set-size test

Condition	Age of children	
	3 years	4 years
Experimental	75	88
Cardinal-once	0	50
No cardinal	0	25

had to count. I was startled with the strength of the above effects and decided a replication study was needed to assess their stability.

Experiment 2

The experimenter (E.M.) who ran the first study was experienced in doing research with preschoolers, familiar with the hypotheses and very skilled at interviewing children in conservation tasks. Hence the decision to use a naïve experimenter (P.P.) in the second study; someone who did not know the hypotheses, was unaware of the initial results, and had very little experience with conservation tasks. This meant that we used someone who was also new at testing preschoolers since others who were experienced knew the hypotheses and results from Study 1.

This experiment was run as a replication of the first except: the experimenter obtained no explanations during training; and the sessions were shorter because, as revealed in transcripts, there was less time spent on obtaining explanations.

Children

Thirty three-year-olds and 30 four-year-olds participated in this study. They came from a comparable sample of children as above. There were 12 children in each experimental group; 12 in each of the cardinal-only groups and six in each of the no cardinal groups. The respective mean age and range of age for the three three-year-old groups were: 43·5 months (38–47 months); 43 months (36–47 months) and 45 months (42–47 months). The comparable figures for the four-year-old groups were 53 months (49–56 months); 53 months (48–59 months); and 52 months (49–54 months).

Results

In the main, the second study replicated the first. Strong results were obtained again, although the children in the experimental group in this study did not do quite as well. The overall proportions of correct conservation judgements were 0·63 and 0·79 for the three- and four-year-old subjects. In this study there was some effect of set size; still the experimental children did quite well on the large-item tasks. The proportions of correct response for the small and large tasks were 0·71 and 0·54, respectively. The comparable figures for four-year-olds were 0·88 and 0·71. These as well as the control groups' data are summarized in Table 4.

Children in the experimental groups were able to explain correct conservation judgements. Adequate explanations were provided on 0·45 and 0·76, respectively, of the three-year-olds' and four-year-olds' correct trials. Further, as in the first experiment, many of the experimental subjects conserved (with explanations) on at least one small set-size and one large set-size task. The respective figures are 58 and 75 per cent of the three- and

Table 4. Proportion correct conservation judgements in Expt 2

Set-size range and age	Condition: Experimental	Cardinal-once	No cardinal
Small			
3 yr	0·71	0·46	0·16
4 yr	0·81	0·46	0·04
Large			
3 yr	0·54	0·23	0·21
4 yr	0·71	0·08	0·04

four-year-old groups. The same was true for only 25 per cent of the four-year-olds in the cardinal-once condition and even fewer children in the three remaining control conditions.

In sum, levels of transfer performance were down somewhat from those obtained in the first study. Still, three-quarters of the four-year-olds and over half of the three-year-olds in the experimental condition did conserve on both large and small set-size tasks. I conclude that the training procedure reliably induces number conservation in preschool children.

General discussion

The training studies presented here do not support the strong form of the Gelman & Gallistel account of why preschoolers fail the standard number conservation task. Children in the experimental group were given experience designed to bring them to recognize that the specific cardinal values of two displays represented either the same or different number(s). The three- and four-year-old subjects were trained with small set sizes so as to ensure their being able to count and determine the cardinal values. When then tested on conservation tasks, three- and four-year-olds conserved on both small (4, 5) and large set sizes (8, 10). Since most children of this age cannot count accurately set sizes greater than four or five, the only way they could have conserved is on the basis of one-to-one correspondence. Hence it cannot be that preschoolers are never able to use this principle of numerical equivalence. Obviously, under certain circumstances even three-year-olds can.

I interpret the above transfer findings in terms of an accessing account of cognitive development. Rozin (1976) proposed that part of cognitive development involves an increasing ability to access the structures underlying early cognitive and perceptual abilities (cf. Fodor, 1972). What are early and possibly innate abilities, are used in restricted (or even just one) situation(s). But with development these are used in wider and wider settings because of a growing general ability to gain access to underlying competences. For Rozin, development also involves an increasing ability to access components of some of the structure used in specific settings and combine the accessed structures to serve a new ability. Consider the case of reading. I know of no claims that the ability to read is innate. Many have argued that a child has to access the phonetic speech-stream if he is to master the sound–sight correspondence rules (see Rozin & Gleitman, 1977, for a review). But the ability to do this develops relatively late, e.g. Liberman *et al.* (1977). In Rozin's terms, this is because the ability to produce speech only embeds an implicit ability to use the phonetic code. This ability is not an explicit one. With development, there is an increase in the ability to access the phonetic code and put it to work in the service of acquiring a new ability – to read.

In my accounts of counting (e.g. Gelman & Gallistel, 1978) the principle of one-to-one correspondence is an integral component of the counting scheme which is used in the service of counting. When I say that the above training results support an accessing account of development, I mean that the implicit ability to use one-to-one correspondence when counting was brought out and made explicit by the training procedure. Genevan reports of precocious number conservation lend support to the general idea that later developing number concepts often involve the accessing of implicit knowledge embedded in the structures which characterize early number concepts.

Piaget's initial treatment of number conservation focused on the child's understanding of transformations. More recently (e.g. 1975, 1977) he turned his attention to the conditions that a child must recognize before she can deal with transformations. She must first discover the correspondences between two states in order to make comparisons and this 'has to precede any transformations, any working of changes on these fixed states'. First, the child can determine correspondences without being able to apply the rules of transformations. Second, the use of transformations *relies* on the use of correspondences.

Finally, the child understands the system of transformations as it generally applies to quantity.

Following from Piaget's recent account, Inhelder *et al.* (1975) succeeded in producing 'precocious' number conservation. In their experiments four- and five-year-olds were shown displays in one-to-one correspondence and then a series of item removal and replacement transformations. For example, one item in an array was removed and the child was asked if the number of items in both rows was the same; then that item was put back into the array but at a different position and again the child was asked about equivalence. The idea in these experiments was to highlight the 'commutability' of items in a discrete set, i.e. that conservation holds when the act of adding an item at one point in space is undone by the taking out of an item from another point in space. Note that tasks like these require the child recognize that permuting the positions of items within set does not alter the number therein. This is akin to what a child does understand about counting, i.e. that the order of objects is irrelevant to the counting procedure (the order-irrelevance principle in Gelman & Gallistel's model). Inspection of the procedural details of the Inhelder *et al.* task makes it clear that children were also counting and using the resultant cardinal values. Hence the Inhelder *et al.* procedures could very well have worked because they made explicit, or pulled forward, the already implicit knowledge of one-to-one correspondence and cardinal number in the counting-principle structure.

I end up by focusing on access theories of development because they offer a potential setting within which to capture the picture of early cognitive development that I see emerging in the literature. I focus on Rozin's and not Fodor's position mainly because the former offers a potential account of whence new knowledge in development – a problem that was central to Piaget's objections to Fodor's and Chomsky's nativist positions (Piatelli-Palmarini, 1980).

As regards early cognitive development, it used to be commonplace to characterize preschool cognition in terms of how it was not like that in later years. White (1965) catalogued a variety of findings – from outside as well as from inside the Piagetian view – to support the idea that a major leap forward occurred between the ages of five and seven years. Given the subsequent research activities on the part of many, it is now possible to talk as well about the common abilities (e.g. Donaldson, 1978; Gelman, 1978). Under certain conditions, preschoolers can reveal competence in these and other domains. Indeed, it begins to look as though infants can be shown to have more competences in the perceptual and conceptual domains (e.g. Gibson, 1980; Cohen & Younger, 1981; Spelke, in press) than once presumed. One can even demonstrate that they are sensitive to numerical properties (e.g. Starkey *et al.*, 1980).

Despite the long list of what the old and young can both do, preschool children nevertheless are at a disadvantage. There are many occasions where their potential brilliance fails them. Theirs is a competence that is fragile, that can be on-again, off-again, that is used only in restricted settings, that does not generalize readily. In other words they have limited access to their competences. In retrospect, from the perspective of Rozin's version of accessing theory we have ended up with the characterization of preschool thought that we should have found. The theory leads one to expect pockets of competence which only reveal themselves under 'appropriate' circumstances.

If development involves, in part, increased access, the younger the child the more difficult it should be to show a capacity. Similarly, variations in tasks – even simple ones – should have more effect on the younger child than an older one. It should also take more to show a competence on their part. In this regard, the fact that it was only the four-year-olds in the cardinal-once control who conserved during the post-tests makes sense.

As we continue to show capacities at an earlier and earlier age, it is hard to resist the idea that there are foundations for cognitive development that are innate and specific to a particular domain. Although Piaget has allowed that the processes of assimilation, accommodation, and equilibration are innate, he has steadfastly held out against the idea that cognitive structures are innate. In a debate with Chomsky and others (Piatelli-Palmarini, 1980) on this matter Piaget defended his view that structures are constructed and not inherited. He said '...I am satisfied with just a functioning that is innate' (p. 157). Chomsky and Fodor argued that structure begets structure and it is logically necessary to assume a nativist position like theirs.

One of Piaget's reasons for resisting the nativist's arguments of Chomsky and Fodor was his concern for the problem of new knowledge. If we say that it is logically required that one can only know what one already is capable of knowing (to be sure in an unexpressed form), then Piaget's position that development brings new knowledge and more mental power is hard to defend. However, new abilities, i.e. other than those which are programmed in the genetic code, can develop from what is innately given. Recall the Rozin account of the ability to read, a new ability which surely brings with it new mental power. Rozin's notions about accessing by themselves do not constitute a solution to the developmental problem, i.e. how to account for new concepts within a nativist frame of reference. But it points to the form such a solution might take.

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Requests for reprints should be addressed to Rochel Gelman, University of Pennsylvania, 3813–15 Walnut Street, Philadelphia, PA 19104, USA.