low-orange-brown colors of Jupiter with its reducing environment, a continuing reminder that optimum conditions for heteropolyamidine synthesis might well have existed when Earth was a young planet with an upper atmosphere rich in reduced carbon and nitrogen. As these cyanide polymers settled onto Earth's surface—land and sea—together with other products of atmospheric photochemistry, a proteinaceous matrix developed able to take part in and promote the chemistry leading to the emergence of life (*2, 8*).

A more widely accepted view of the origin of proteins starts with the prior synthesis of $\alpha$-amino acids from intermediates such as HCN oligomers or aminoacetonitriles (*9, 10*). How the thermodynamic barrier to spontaneous polymerization of these monomers is overcome, however, remains a question that generates much difference of opinion (*9*). With a "dilute soup" model in mind (*10*), Ferris has critically assessed our research and concluded that our new paradigm of innate protein structure is incorrect.

1) He quotes negative evidence for the presence of peptides in HCN products, but fails to mention the rigorous work of Draganic and Draganic (*11*) who combined a modified biuret procedure with infrared absorption spectroscopy to show the presence of peptide links and nitrile groups in products formed by ionizing radiation in aqueous cyanides. Subsequent hydrolysis yielded several $\alpha$-amino acids.

2) The point of our work on poly-$\alpha$-cyanoglycine was not to demonstrate HCN polymerization, but rather to show that HCN (and $H_2O$) could convert a homopolymer to a heteropolymer possessing protein side chains (*6*). Ferris proposes no alternative mechanism to account for side chain formation in HCN reactions.

3) It is conventionally assumed that HCN dimer is iminoacetonitrile. Since the HCN dimer has not yet been isolated we have suggested that other more reactive structures such as aminocyanocarbene or azacyclopropenylidenimine might actually be involved in HCN polymerization (*12*), particularly within clouds of hydrogen-bonded HCN molecules. The *N*-alkyl derivatives of iminoacetonitrile studied by Ferris (*13*) are so stable that they tell little about the nature of the elusive dimer of HCN.

4) Ferris presents no direct evidence that HCN oligomers are formed from diaminomaleonitrile, $(HCN)_4$. He reasons (*14*) that the steady state concentrations of $(HCN)_2$ and $(HCN)_3$ are exceedingly low in dilute aqueous solutions of HCN, as shown by the absence of exchange of $H^{13}CN$ with $(HCN)_4$ when incubated in alkali. However, $H^{13}CN$ would also be expected to yield some $(H^{13}CN)_4$, which apparently was not detected. If conditions did not favor $(H^{13}CN)_4$ formation, why should exchange of $H^{13}CN$ with $(HCN)_4$ take place? Indeed, both exchange and polymerization (as shown by the solution becoming black) evidently occurred in a parallel experiment carried out in DMSO. The aqueous experiments are clearly not conclusive and, in any case, are hardly relevant to our nonaqueous model. In our original studies (*3*), we were careful to remove $(HCN)_4$ from polymeric products before hydrolyzing them to $\alpha$-amino acids. Perdeuterated glycine obtained from HCN polymers could not therefore be due only to the presence of $(HCN)_4$ (*7*).

5) We agree with Ferris that the products of HCN photolysis—and, we would add, of spark discharge experiments—probably arise mainly from base-catalyzed reactions of HCN condensed on the walls of the reaction vessels. Such experiments may be significant, but not as models for atmospheric chemistry.

6) Regarding the components of carbonaceous chondrites, we continue to believe that the incorporation of carbon-bound deuterium in the glycine obtained by extraction of the Murchison meteorite with $D_2O$ is evidence for the presence of HCN polymers. Our current GC-MS studies (mass fragmentography) (*15*) lead us to question Lawless' concept of hydrogen-deuterium exchange brought about by "selective catalytic activity" of the meteorite (*16*).

Far from refuting the hypothesis that polyaminomalononitrile was the original ancestor of all proteins, it seems to us that the research of Ferris and his coworkers is too narrowly restricted to aqueous cyanide chemistry to have much bearing on the issues involved. We are encouraged to persist in our reinvestigation and reinterpretation of chemical evolution studies on the origin of proteins (*7, 8*).

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6 November 1978; revised 21 December 1978

## Infant Perception of Visually Presented Objects

Dodwell *et al.* (*1*) have published data indicating that human infants in the newborn period do not discriminate between real objects and pictures of objects. However, the experiment that produced the result is flawed.

Dodwell *et al.* used an experimental design in which they intended to present babies with an object and a representation of an object. They then intended to determine whether the babies reached as much for the one as the other. Unfortunately, rather than presenting the babies with a representation and an object, they presented two objects. To be sure, one of the objects was a photograph, but a photograph is an object; it has parallax variables around its edges. If one is to use a photograph to present representations, one must obliterate these object-specifying variables. This can be done either by using a very large photograph whose edges are out of the visual field or by presenting the photograph flush against a background (*2*).

Dodwell *et al.* thus presented the babies with two objects, one with a representation in its center, the other with another object in its center. It is not at all certain that the infant would see the representation or the small object (*3*); a demonstration that they could would itself be significant. In principle, Dodwell *et al.* could have determined this if they


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had observed whether the babies' reaches were aimed at the edges of the photograph as object or at the representation in its center and also whether the babies reached at the edges of the other background object or at the smaller object in its center. This would have been difficult enough, given the small size of the objects used in this study and the known inaccuracy of reaching in this period (*4*). However, a comparison of the rates and accuracies of Dodwell *et al.* with those of Bower [table 1 in (*1*)] shows that the former could not elicit the necessary behavior. The reason for this is undoubtedly their use of the two chairs. We ourselves have never been able to get any reaching from a young baby on either of those chairs, although Trevarthen, who designed the "specially designed" chair, has succeeded in eliciting the appropriate behavior (*5*). The study by Dodwell *et al.* thus used inappropriate stimuli and failed to elicit appropriate behavior. Although we have never claimed that it is easy to elicit neonatal reaching, it is possible (*6*).

We ourselves recently studied six babies between the ages of 18 and 26 days in the experiment Dodwell *et al.* intended. Two stimulus presentations were used. One was a red ball 5 cm in diameter with a small metal bell attached to its underside; the other was a pictorial representation of this on a card 20 cm by 26 cm. We presented either the real object or the pictorial representation of that object (the representation object) for four periods of 3 minutes each. Each object was presented once on each side, with order of presentation randomized. The length of presentation chosen is not equivalent to that chosen by Dodwell *et al.*, but is that which we have found to be both practical and efficient in eliciting neonatal reaching. The first reach may be slow to appear but is frequently followed by a burst of reaching; a shorter presentation period fails to exploit these characteristics of early reaching. We recorded all of the arm extensions (reaches) that could have touched object or representation object. For those reaches that succeeded in touching the object or representation object, we recorded whether object, representation, or representation edge was contacted.

If Dodwell *et al.* are right, the babies should reach for and contact the representation as often as the object. If our account of what would have happened in their study, had it been properly executed, is correct, the babies should reach only to the edges of the representation holder. If the babies could contact the real object sufficiently often, such a result could not be explained away as inaccurate reaching for the representation.

As Table 1 indicates, the babies contacted the edges of the representation object more than the representation itself ($t = 4.55$, $P < .01$), to such an extent that the two contacts with the representation are probably best seen as a reach aimed at an edge and missing. Reaches to the real object were more frequent and more successful ($t = 8.19$, $P < .001$) in accord with previous studies of the effect of the object size on frequency of reaching (*2*). It is thus clear that the real object was differentiated from the represented object.

Table 1. Mean number of three classes of reaching behavior of six infants.

| Gesture | Object | Representation object | |
|---|---|---|---|
| Reach (Successful or unsuccessful) | 15.16 | 4.33 | |
| | | *Representation* | *Edge of representation object* |
| Contact | 9.83 | .33 | 1.66 |
| Grasp | 2.63 | 0 | .83 |

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19 January 1977; revised 28 August 1978

Bower *et al.* (*1*) have suggested that our attempt to replicate an earlier study of Bower's on reaching in infants (*2*) was so flawed that the result does not refute his earlier claims. We made an honest attempt to repeat his experiment as closely as possible because, as we stated, its result goes against a long tradition of scientific research in child development, and, were he correct, the implications for our understanding of infancy would be great (*3*). The experimental conditions and procedures in Bower's original report (*2*) were so poorly specified as to make exact replication impossible. The "flaws" in our experiment occurred because, where information was lacking in his report, we specified reasonable procedures and conditions of our own. This was done only after a large amount of pilot research, as noted (*3*).

Our experiment was criticized on two grounds: (i) we seated the infants inappropriately and (ii) we used the wrong targets. In each case we will illustrate the difficulties of attempting to assess Bower's claims, both in terms of what was stated in his original report (*2*), and in terms of the new data given in (*1*) which purport to negate our findings.

In (*2*), Bower made casual reference to the way the babies were seated: "In studies of over 300 infants we have seen only two who would reach while lying flat on their back. It was necessary to prop the others at some angle to the horizontal. The ideal angle varies somewhat from baby to baby and can readily be discovered since when the baby is at that angle head and eyes turn freely and the arms are not used for support at all, and are thus available for reaching" (*2*, p. 17). In their technical comment, Bower *et al.* (*1*) explained how important it is *not* to put the baby in a seat, yet they still do not state how babies were accommodated in either the original experiment or their new one. In our report (*3*), we specifically mentioned pilot work on seating arrangements, in which infants were propped up or seated in a chair. The amount of reaching elicited was about the same in both conditions, and also when the infant was held in the lap. Despite trying all reasonable seating arrangements, including those implied but actually unspecified in (*2*), we have never observed anything even close to the very high average rate of reaching to objects (once every $4^1/_2$ seconds) that Bower reported. Such behavior would be observed only under extraordinary but unfortunately unknown circumstances. A difference in the seating arrangements is not likely to explain our failure to replicate.

As to the second criticism, that our targets were inappropriate, our pilot research showed that very young infants fuss when presented with a large surface close to the face. Bower did not specify the background used in (*2*), and in (*1*) remarked only that "... neither the writer nor the editor ... thought it necessary" [reference 2 in (*1*)] to make such a specification. Now he castigates us for using an inappropriate background, but it was one we settled on for good reason; a large background elicits inappropriate behavior that interferes with reaching. In his new experiment a background was used which is different from ours and, presumably, different from the one used in (*2*), although that is still uncertain. Why the original background was not specified, or why in (*1*) a background of different size from ours was used, when Bower *et al*. were trying to replicate aspects of our experiment, remains a mystery. Nevertheless, Bower *et al*. have a point; possibly our background was perceived by the infants as an object. In order to assess this possibility, we reanalyzed the videotapes of our second experiment to see if infants (i) made more reaches to the background edge than to its center, and (ii) made more reaches to the object than to the picture. To reach the center of the background in our experiment, reaches had to be made toward the midline; to reach the background edge, they had to be outside a line sagittal to the shoulder. Infants of this age do not normally manifest midline activity (*4*), so any reaches to the central target would strongly indicate visually triggered behavior. In fact our infants made more reaches to the background edge (57 percent) than to the center (43 percent), but the latter were not differentially distributed between object and picture, a result directly contradictory to that of Bower *et al*. (*1*).

Perhaps this contradiction is due to the apparent absence of a background in (*1*), when the real object was presented, in contrast to the settings reported in (*2*) and (*3*). On the other hand it may be because a "small metal bell was attached to its [the object's] underside" (*1*, p. 1138). Especially in view of recent reports of reliable orienting to sound in neonates (*5*) it is inappropriate to draw conclusions about visual recognition of objects from such an investigation.

In their new experiment (*1*) Bower *et al*. used a 3-minute observation period, "... that which we have found to be both practical and efficient in eliciting neonatal reaching. The first reach may be slow to appear but is frequently followed by a burst of reaching; a shorter presentation period fails to exploit these characteristics of early reaching" (*1*, p. 1138).. However, we used the period (2 minutes) used in Bower's original experiment, with which he earlier reported reaching. In the reanalysis of our videotapes (*4*), we detected no difference in the mean reaching rates between the first and second periods of observation under a given condition, nor any evidence for the type of delay followed by bursts of reaches which Bower *et al*. have now claimed to be typical. In addition, they have now suggested that 3 minutes is the best period of observation to use, yet have reported without comment an average rate of reaching to the object in the 3-minute period (approximately one reach per 24 seconds) which is about *six times slower* than the rate reported earlier for the 2-minute period (approximately one reach per $4^1/_2$ seconds).

The main conclusion to be drawn from this controversy is that it is vital to specify experimental conditions well enough to ensure that no similar dispute can recur. We are not alone in failing to replicate results from Bower's laboratory (*6*) and have here illustrated some of the reasons why this might be so. Since babies are highly variable in their behavior, investigators must at least specify their selection criteria, the proportion of subjects rejected, how state changes were manipulated or controlled, and what the range of individual variation in behavior was in the experiment, as well as give a full and clear description of how the investigation was conducted.

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5 July 1978; revised 2 November 1978

## Primate Olfactory Behavior

Goldfoot *et al*. (*1*) purportedly made three male rhesus monkeys permanently anosmic in order to test whether olfactory cues are necessary in the sexual attractiveness of females. However, the olfactory discrimination task described in their reference 6 does not confirm "that a completely anosmic condition had been achieved." It seems inappropriate to infer that since the animal fails to recognize anise-scented monkey chow it cannot possibly recognize olfactory cues indicative of female sexual status. Recognition of olfactory cues associated with the fertile phase of the ovarian cycle would be an adaptively significant response to a biologically important stimulus. If small areas of olfactory epithelium remained intact after the ablation procedure, one might expect the animal to be unable to recognize normally unimportant olfactory stimuli, such as anise. However, one might expect recognition of biologically significant olfactory cues indicative of the fertile phase of the ovarian cycle.

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23 March 1978

Goldfoot *et al*. (*1*) claim that intranasal application of cotton pledgets soaked in 10 percent formalin made their experimental rhesus monkeys permanently anosmic. Their evidence for anosmia was inability of the treated subjects to use the odor of anisole in a simple discrimination task. The conclusion that these subjects were anosmic may be unwarranted for several reasons. First, it has not been established that intranasal application of formalin will completely destroy nasal epithelial tissue or prevent the regeneration of olfactory receptor cells in areas of the olfactory epithelium coagulated by contact with formalin. There do exist detailed morphological and behavioral studies on effects of coagulation necrosis produced by intranasal syringing with zinc sulfate (*2*). Histological studies demonstrate that small pockets of olfactory epithelium are spared by the zinc sulfate treatment. This sparing may be due to air bubbles or mucus trapped in the ethmoturbinals. Regeneration of sensory cells occurs within about 10 days after treatment even after extensive irrigation of the nasal vault. Recovery of odor discrimination behavior occurs within 3 to 4 days after treatment. Treatment with formalin pledgets as described by Goldfoot *et al*. might be more effective than nasal irrigation with zinc sulfate, particularly if formalin vapors

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