thesis of nucleosides and at least some amino acids. Likewise, an assignment of ribosomal proteins to the protogenote (*4*) suggests that the protogenotic ribosome was considerably more advanced than the first ribosome in the "breakthrough organism" (*1*) (the organism with the first encoded messenger RNA), which (by definition) did not incorporate translated proteins. Further reconstructions suggest that the breakthrough organism itself was metabolically sophisticated (*1*), which implies that it already transmitted genetic information intergenerationally with reasonable accuracy (although it probably translated this information imprecisely).

Thus, the limited information available at present suggests that the protogenote was in many ways similar at the molecular level to contemporary organisms. Notable exceptions may be the absence of certain metabolic pathways (for example, fatty acid synthesis) and a greater number of RNA enzymes catalyzing key metabolic steps (*1*). An organism with a large encoded repertoire of enzymes must replicate its genetic information with reasonable accuracy. It follows therefore that the "protogenote" was not a "progenote" (*2*).

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Quasicrystal Publications

When I see history rewritten in the newspaper, or even in popular science magazines, I try to ignore it. But now I read in *Science* (News & Comment, 2 Mar., p. 1020) that "Even before quasicrystals were actually observed, two maverick physicists . . . had suggested that these law-breaking structures might exist" and that "[s]tartlingly, it was only a brief period before a corresponding form of matter was actually observed. . . . On the basis of the NBS [National Bureau of Standards] observations Steinhardt and Levine declared that their theory had been vindicated."

Let us set the record straight. The NBS work (*1*) was published on 12 November 1984. The first paper by Steinhardt and Levine (*2*) on the subject was published on 24 December 1984, referencing the NBS paper several times.

The icosahedral phase was discovered at NBS in April of 1982. The earliest publication (by submission date) on icosahedral phases did not appear in print until June 1985. In this paper (*3*), Shechtman and Blech not only reported experimental results on the icosahedral phase but also proposed a structure of "randomly" packed icosahedra which maintained long-range orientation order through vertex or edge sharing. Their calculated diffraction pattern from the vertex-connected model showed a strong qualitative match to the experiment.

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Response: The underlying point of Gayle's letter—that Schechtman *et al.* deserve more recognition for their contribution to the quasicrystal field—is well taken. After all, if they had not discovered quasicrystals, the field might not exist. However, I am not sure why Gayle draws attention to the June 1985 paper in which Schechtman and Blech propose a model to explain their data. That model, a version of the icosahedral-glass model, has been rejected by most researchers, while the Penrose-tiling model still thrives. As for the precedence issue, Steinhardt told me that he and Levine began studying Penrose-tiling models in 1981 and that they gave invited talks and even applied for a patent related to this work well before the NBS group announced its discovery of quasicrystals. That is why Steinhardt and Levine were able to publish so soon after the NBS group did.—JOHN HORGAN