drawn from industry, government, environmental groups, and academia. With this framework in place, it should be possible to evaluate the advantages and disadvantages of the various suggestions for an appropriate agency or agencies.This will consititute the last phase of the project.

The project is now just beginning; it will require inputs and cooperation from the national engineering, scientific, and technical communities if it is to succeed. The American Academy of Arts and Sciences welcomes your comments.

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"Progenote" or "Protogenote"?

The use of the word "progenote" to denote the most recent common ancestor of eubacteria, archaebacteria, and eukaryotes (*1*) (Research News, 3 Nov., 1989, p. 578) (an organism defined by its position in an evolutionary tree) was recently criticized by Carl Woese (Letters, 16 Feb., p. 789). Woese is, of course, correct in pointing out that "progenote" originally denoted an organism with inaccurate mechanisms for replicating and translating genetic information (*2*) (an organism defined by a set of putative biochemical properties). However, the word is now used (*3, 4*) as a simple cognate of "progenitor" (*5*); indeed, Woese himself seems to use the word in this way (*6*). This evolution of meaning is not surprising in view of the relative ease of defining ancient organisms by their positions in an evolutionary tree (constructed by comparisons of the sequence of ribosomal RNA molecules), and the relative difficulty of establishing their biochemical properties.

Nevertheless, we are sympathetic to Woese's effort to defend the original meaning of the word. Therefore, we wish to suggest that a custom in historical linguistics be adopted, where the prefix "proto" designates a language (or an organism) reconstructed with the use of rules of parsimony (7). Thus, the most recent common ancestor of archaebacteria, eubacteria, and eukaryotes is the "protogenote" because it contained the "protogenome." Likewise, "protoeubacterium," "protochordate," and "protoartiodactyl" denote the most recent common ancestors of eubacteria, chordates, and artiodactyls, respectively. The encoded macromolecules of each are reconstructed from the sequences of homologous macromolecules in their descendents. Such molecules are now for the first time available in the laboratory (*8*).

Woese also writes that it remains a "key unanswered evolutionary question" whether the "protogenote" was a "progenote." We agree that this question is key, but we also believe that it readily answered in its simplest form with the use of information now available. Many enzymes [for example, glyceraldehyde-3-phosphate dehydrogenase (*9*), enzymes involved in the biosynthesis of histidine and purines (*10*), and ribosomal proteins (*4*)] can now be reconstructed in the protogenote from the sequences of their descendents in all three kingdoms. Others might be assigned more weakly from the sequences of their descendents in eubacteria and eukaryotes alone. These protogenotic enzymes are not obviously either smaller or more limited than their modern counterparts.

As these enzymes could not have stood in metabolic isolation, the protogenote must have had other enzymes catalyzing at least a glycolytic path and pathways for the biosynthesis 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