wild squash, and the sizes of the later phytoliths overlapped the range of modern domesticated species (*11*). Other phytolith evidence from Vegas pointed to little or no moisture increase during this period, and paleoecological records from the region indicate that the period was probably the most stable climatic interval of the Holocene (*11, 16*). Further, a recent analysis of a Vegas phytolith sample from before 10,000 years B.P. (*12*) indicated that no change in squash phytolith size occurred between about 10,500 years B.P. and 9700 years B.P., when regional precipitation probably did increase as a result of the environmental changes that accompanied the close of the Pleistocene (*11*), Therefore, as with archaeological seed analysis (*17*), our data likely indicate that early squash domestication occurred.

Third, Rovner's assertion that phytoliths are difficult to identify at refined taxonomic levels is contradicted by a large body of empirical evidence accumulated in the past 15 years by investigators around the world who, for the first time, closely studied phytoliths in a wide sample of angiosperms (*1–10, 13–15, 18*). As with *Cucurbita,* three different sets of researchers agree that *Oryza* (rice) can be identified on the basis of the morphology of a single type of phytolith that occurs in reproductive organs (in this case, the glume) (*7, 19–21*), Our classifications have been validated by multivariate analysis, namely, multiple discriminant functions. In our studies of rice, the measurements were specifically taken to capture size and shape because, as is well known in taxonomy, these attributes together are often necessary for efficient classification (*20*). Since wild and domesticated *Oryza* can be distinguished in a randomly reserved test set from functions prepared from the training set (*7*), Rovner's objections that such classification is not yet possible are refuted by the empirical evidence. Our work with maize used frequencies of phytolith variants where the variants are defined by shape as well as by a size measurement; once again, our success in prediction is due to including both size and shape in the analysis (*18*).

Finally, Rovner's statement that early maize phytoliths from Ecuador are "*larger* than the size values presented for any and every modern reference maize tested" is contrary to the evidence. Rovner could be referring to one Valdivia sample that had a slightly increased fraction (by about 10%) of "extra-large size" phytoliths (those measuring from 20 to 25 micrometers in width) (*22*). However, when the broad size categories are converted into average mean widths, these phytoliths are smaller than many modern maize races (*18*),

Studies of agricultural origins demand the highest standards of research, and interested scholars from other disciplines deserve reliable information on this crucial transition in human prehistory.

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## A Small Misconception

Regarding "Carbon sink: A clue from Biosphere 2?" (R. L. Walford, *Science*'s Compass, 15 Jan., p. 330), I want to clear up a small misconception that has been widely repeated in the press (and also in the Perspective by J. E. Cohen and D. Tilman, 15 Nov. 1996, p. 1150), namely that "carbon dioxide [$CO_2$] was combining with the cement of the structure, carrying oxygen along with it to form calcium carbonate… hence the fall in oxygen concentration." The cement in Biosphere 2 did not cause the oxygen loss. It is true that the $CO_2$ reacted with the cement and that $CO_2$ contains oxygen atoms. But it is molecular oxygen ($O_2$) rather than oxygen atoms that was being lost from the air in Biosphere 2. The amount of oxygen atoms present in the water in Biosphere 2 is about 200 times more than the amount of oxygen atoms present as $O_2$, so the loss of oxygen atoms to the cement was insignificant. Rather, what caused the $O_2$ loss was the excess of organic matter in the soil, which supported an imbalance of $O_2$-consuming respiration over $O_2$-producing photosynthesis. The reaction of $CO_2$ with the cement only made it a little harder for us to find the true cause of $O_2$ loss, by scrubbing from the air the telltale product of respiration, $CO_2$.

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Walford correctly points out that concrete absorbs $CO_2$, but he does not point out that the $Ca(OH)_2$ responsible for this uptake was obtained by driving $CO_2$ off of limestone. Because some of the $CaO_2$ becomes silicate-bound and some remains unreacted, concrete manufacture is a net source rather than a net sink for $CO_2$. Further, the contribution of concrete manufacture to global $CO_2$ production is only about 0.2 gigaton of carbon (GiC), compared with 6.5 or so GiC produced by fossil fuel burning and to a continental sink of about 1.7 GiC (S. Fan *et al.*, Reports, 16 Oct., p. 442). Hence, even if limestone were slaked at one region and the concrete were used in another, the impact on the distribution of $CO_2$ in the global atmosphere would be negligible.

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## Green Revolutions

While we appreciate scientists' efforts to increase crop yields (C. Mann, "Crop scientists seek a new revolution," News Focus, 15 Jan., p. 310), it appears that we have not learned from mistakes of the past and that once again we have fallen victim to the old fallacy that science can alleviate the world's pain. The original "green revolution" focused solely on crop yields, while ignoring the subsequent ecological and sociological consequences. It also increased the dependence of developing nations on high-input agriculture (mechanization, pesticide, and fertilizer use)—a dependence that these nations could ill afford. This dependence in turn inflated the national debt of developing countries, contributed to rural displacement, increased poverty, and decreased overall crop biodiversity. At the time, science appeared to be solving world famine, but the real social and ecological ramifications had not been considered. Today, there is extensive literature questioning the basic premises of the green revolution and its impacts. Mann's article says little about such considerations. Instead, we are told once again that science will save us. But we have the opportunity and obligation to examine the potential impacts on our environment before we blindly engineer these high-yield marvels. Shouldn't we be able to learn from our past mistakes?

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Regarding Charles C. Mann's article "Genetic engineers aim to soup up crop photosynthesis" (News Focus, 15 Jan., p. 314), the development of techniques for manipulating chloroplast DNA in plants should have received more credit for renewing interest in altering the RuBisCO (ribulose-1,5-bisphosphate carboxylase-oxygenase) found in $C_3$ plants. With this advance, placing a foreign RuBisCO into plants was no longer a far-off dream. Furthermore, nature offers several enzymes besides the red algal RuBisCO that might be beneficial in $C_3$ crop plants.

While the discovery of high specificity in the red algal RuBisCO was unexpected, from the available data its high specificity seems to be associated with a considerable reduction in maximal turnover compared with the typical $C_3$ enzyme. Consequently, its introduction into plants may actually reduce net photosynthesis because both turnover and specificity determine the overall efficiency of the enzyme.

Using equations for RuBisCO kinetics and carbon dioxide ($CO_2$) release by photorespiration, we calculate that under current conditions net photosynthesis is more likely to be increased by replacement with a high-turnover RuBisCO enzyme, even if its specificity is somewhat lower. The benefit will be increased in the higher $CO_2$ environment expected in the next century. Suitable candidates are already known in the green algae and $C_4$ plants, where evolution of the enzyme has occurred in a high $CO_2$ environment.

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## A Dark Particle?

I write in connection with James Glanz's article "Has a dark particle come to light?" (News of the Week, 1 Jan., p. 13), where the intriguing results of the DAMA