which he considers symbiosis and cellular evolution, it emerges that beginning in 1910 Pierantoni was talking about "hereditary physiological symbiosis" (a theory he invented) and that he found it for the first time in the scale insect *Icerya purchasi*. From hereditary symbiosis he argued for the symbiotic origin of the eukaryotic cell, a viewpoint similar to Ms. Margulis's serial endosymbiosis theory.

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Lynn Margulis and Michael F. Dolan reply: We are grateful for Tom Cavalier-Smith's criticism, as it sharpens our views. If symbiogenesis is defined as the appearance of new traits, organs or organisms resulting from protracted physical associations (symbiosis), then symbiogenesis gave rise to many more than five major new groups. Symbiogenetic partnerships may be either permanent, as with all of Mr. Cavalier-Smith's examples, or cyclical, as with the glands of the tropical plant *Gunnera* that function with the symbiotic participation of the cyanobacterium *Nostoc,* or the bacterial associations in the bioluminescent squid *Euprymna*. Of course, by that reckoning all lichens—more than 25,000 of them—evolved by symbiosis.

As we explained in our article, the presence of centriole-kinetosome DNA in *Chlamydomonas* and in akaryomastigonts of *Calonympha* is indeed still in question. Yet as incorrect as the theory of the origin of centriole-kinetosomes and their axonemes, or shafts, from free-swimming spirochetes may prove to be, it is still a great spur to research. So far the idea that intracellular motility originated by symbiogenesis has led to the following discoveries: cytoplasmic tubules that resemble microtubule-organizing centers in a spirochete *(Diplocalyx)* from the intestine of the termite *Cryptotermes cavifrons;* a spirochete, *Spirosymplokos deltaeiberi,* that is large, retractable, resistant, variable-diameter, free-living and "viviparous"; a tektin-like protein in *Spirochaeta halophila;* granules in the spirochetes *Spirosymplokos* and *Cristispira* with the same dimensions as those in the centrioles of the protist *Pseudotrichonympha;* new genera of termite and microbial-mat spirochetes, such as *Clevelandina* and *Mobilifilum;* and elaborate sites, both on the inside and the outside of protists to which spirochetes and other bacteria attach, that are analogous to centriole-kinetosomes.

The repeated dismissal of the spirochete theory has been less rewarding—but Lederberg's dictum is wise: imaginative speculation about evolution must be checked "by the most cautious criticism." Mr. Cavalier-Smith's excellent scientific comments are as invaluable as they are welcome. Of course we remain friends.

As for Umberto Pierantoni (1876–1959), yes, we revere his work. We are mostly familiar with his ideas on "hereditary physiological symbiosis" through his excellent general biology text that is still consulted in Latin America, especially Cuba (where our students in 1983 used it), and in Spain. In our article we covered little of the history of symbiogenesis, and so we omitted many early symbiogeneticists (including Umberto Pierantoni). For a more comprehensive treatment of the history of symbiosis research, see Jan Sapp's *Evolution by Association: A History of Symbiosis* (Oxford, 1994) and Liya Nikolaevna Khakhina's *Concepts of Symbiogenesis* (Yale, 1993). We welcome further correspondence from Riccardo Pierantoni.

WHAT HATH DROUGHT WROUGHT?

In his article "Desert Storm" [May/June 1996] Harvey Weiss has misrepresented the overall thesis of my article, "The Social Response to Environmental Change in Early Bronze Age Canaan" (*Journal of Anthropological Archaeology,* **14**:26–44, 1995). My point was that some societies adapt to severe environmental changes and others fail to do so. The interesting lessons to be learned here are not derived from a laundry list of towns that were abandoned; instead one ought to try to understand why some societies survived and others did not. What Mr. Weiss has neglected to mention is that even during the drought that began in 2200 B.C., major sites continued to be occupied by large populations. Examples include Tell Brak (Nagar) and Tell Mozan (Urkish), both in northern Syria.

His comment that "Palestinian water resources and topography ruled against the construction of irrigation canals" demonstrates a lack of understanding of the geography of both Palestine and Israel. In fact, there are many archaeological remains of classical-period check dams, canals and aqueducts that brought water from springs at high elevations to fields at lower elevations. Although it is true that there is little evidence for irrigation canals in the area during the Middle Bronze Age, any such evidence would have been buried under the more than three meters of alluvial sediment that filled the lowland valleys in the classical periods. Nevertheless, there are indications that cisterns and wells were cut into rock in the Middle Bronze Age, which made for a much more sophisticated water technology than there had been in the Early Bronze Age. To say that the climate became moist again after 1900 B.C. is inconsistent with much of the information gleaned from isotopic analyses, studies of geomorphology and data on the levels of lake water, which indicate that a generally dry period persisted at least until the classical periods in Israel (between the third century B.C. and mid-seventh century A.D.). Yet major cities existed in the Middle and Late Bronze ages of the Southern Levant. Somehow those societies managed to overcome the adversities and maintain sizable urban populations in semi-arid agricultural zones. Perhaps we who are now faced with the possibility of global climate change have something to learn from the ways ancient societies dealt with their environmental stresses.

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Harvey Weiss replies: Arlene Rosen obscures the fallacies of her simplistic argument. She states that I neglect to mention that major sites continued to be occupied by large populations. In 1993 and 1994 I discussed the epigraphic and archaeological documentation for the continued, but reduced, occupations of major sites across West Asia, including northern Mesopotamia, western Syria and Anatolia, as well as Afghanistan. Those occupations pose research questions, I noted, that must be addressed, as do the transfers of populations across such ecologically diverse regions as Mesopotamia and Turkmenistan.

Ms. Rosen cites Brak and Mozan in

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northern Syria as sites I fail to mention. In fact, I discussed those sites twice in 1993, and I have done so subsequently. Only one of the sites offers evidence about occupation during the Akkadian collapse and post-collapse. As Max Mallowan noted in 1947, Brak "shrunk to one-half its original area" following the Akkadian abandonment.

In the article Ms. Rosen cites at the beginning of her letter, she wrote that third- and second-millennium Egypt and Mesopotamia used canal-irrigation technology that was not adopted in the Palestine of the time. That absence, she maintained, demonstrated there were "cultural factors [in Palestine] selecting against this particular adaptive response."

I responded by noting that the hydrology of Palestine allows for neither Egyptian- nor Mesopotamian-style irrigation. Confronted with that criticism, Ms. Rosen now claims that the Hellenistic-Roman introduction into Palestine of check dams, canals and aqueducts somehow proves that Nile- and Euphrates-style irrigation technologies were available to third-millennium Palestinians but rejected by them. That, of course, implies that such choices were available in a region lacking a seasonal river flood deployable for agriculture.

Ms. Rosen also asserted that there is evidence for irrigation in the Middle Bronze Age and that such irrigation made town life possible during that still-harsh period. I pointed out that no such evidence existed for Middle Bronze Age irrigation in Palestine. Now Ms. Rosen states that though "little evidence" exists for irrigation canals, they are probably buried under the alluvial sediments of Palestine. Wrong again. Palestine has relatively little sedimentation, and has been subject to intensive archaeological investigation, but there is still no evidence of Middle Bronze Age irrigation.

To replace nonexistent canal irrigation Ms. Rosen requires a Middle Bronze Age technological adaptation to sustain her argument. Hence she argues for "cisterns and wells," a "much more sophisticated water technology than there had been in the Early Bronze Age." But are we to believe that Early Bronze Age peoples did not dig wells or cisterns? In fact, cisterns did occur in the Chalcolithic and Early Bronze ages in Palestine, but they held less than a hundred cubic meters of water; hence they would not have been able to sustain urbanized agricultural populations. Wells are documented as early as the sixth millennium and, of course, during the Early Bronze Age.

Climate during the Middle Bronze Age was wetter than it was during the preceding period of abrupt climate change, and it marks the return to "normal" conditions following the 2200–1900 B.C. event. Ms. Rosen contends that isotopic analyses, geomorphology and lake-level data indicate "a generally dry period" that "persisted at least until the classical periods in Israel." But the data now available for West Asia, from soil micromorphology and from the analysis of marine, lake and glacial cores, confirm the abrupt onset and end of what climatologists call aridification, although the problem of lake water residence still precludes an easy correlation between precipitation and post-drought refilling.

The qualities of the abrupt aridification between 2200 B.C. and 1900 B.C. are just beginning to be defined and quantified. Volcanic eruptions do not give rise to such events. The social adaptations to the climate change are visible archaeologically across the Old World.

With mistaken archaeological data and an outdated view of the abrupt climate change, Ms. Rosen sets, in traditional fashion, "collapse" and "adaptation" as opposite cultural responses. One anthropological lesson of the 2200 B.C. aridification is that collapse, in its varied forms, was a transregional social adaptation. Another lesson is that archaeological hypotheses must adapt to archaeological and climatic data, not vice versa. •

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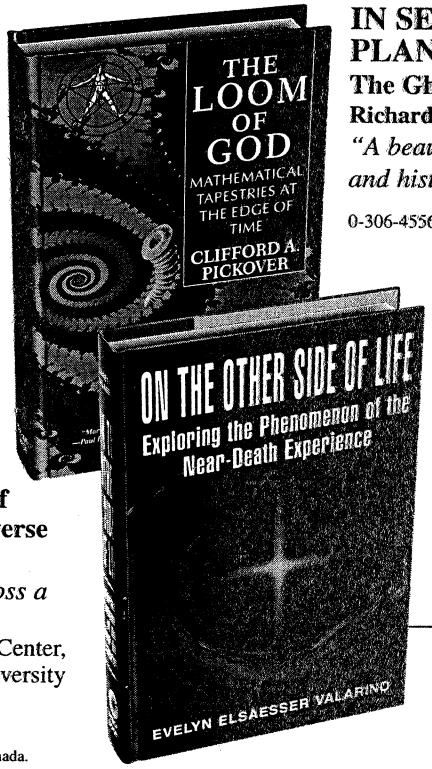

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