Response

BORGESON SUGGESTS THAT NEIGHBORING high- and low-temperature areas around hydrothermal vents may have had some prebiotic significance. There are indeed temperature gradients associated with hydrothermal systems, and these arise from the mixing of hot vent waters with cold ambient seawater. As has been demonstrated elsewhere (*1*), organic compounds are rapidly decomposed at the elevated temperatures characteristic of hot vent waters. Minerals (such as pyrite) that form around vent discharges could have played a role in assisting in the synthesis of complex organic molecules from simple reagents (HCN, aldeyhdes/ketones, and so forth) present in seawater, but there were likely many environments on the primitive Earth besides hydrothermal vents where this could have occurred.

Matlin mentions that coacervates as imagined by Oparin might serve as laboratory models of precellular systems. Indeed, liposomes and micelles formed from abiotically synthesized amphiphilic molecules may have played an important role in the emergence of the first membrane-bound precellular systems (*2*).

As he has shown elsewhere (*3*), Wächtershäuser is fixated on what he considers proper scientific methodologies, especially in the context of the philosophy of Karl Popper. He considers our relatively modest attempt to describe the emergence of life, using an evolutionary narrative consistent with the possible prebiotic environments and the essential properties of living entities, as unpalatable. He does not mention that a core theme of his autotrophic theory is the appearance of pyrite-based "life" that consisted of only autocatalytic metabolic reaction networks in which no genetic information material was present. There is indeed some evidence that iron/nickel sulfide could have played an important catalytic role in the synthesis of organic molecules on early Earth, as Wächtershäuser has advocated. But the fact is, whether in solution in the entire ocean or associated with mineral surfaces, metabolism in whatever form is not life as we know it. As we emphasized in our Perspective, regardless of what Wächtershäuser may speculate, it is unlikely that life could have evolved into modern biochemistry in the absence of a genetic replication mechanism to ensure the stability, survival, and diversification of its basic components. The central tenet of Wächtershäuser's criticism is his belief that the prebiotic soup theory and his autotrophic reaction schemes are incompatible. However, it is hard to see why the results that have been achieved so far from experimental work that has been performed within the framework of his autotrophic theory cannot be quite easily accommodated into the prebiotic soup heterotrophic theory of the origin of life, given its open epistemological character.

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Another Form of Bias in Conservation Research

IN THEIR RECENT ANALYSIS OF CONSERVATION research literature, J. A. Clark and R. M. May ("Taxonomic bias in conservation research, Letters, 12 July, p. 191) show that vertebrates are grossly overrepresented in conservation research, whereas invertebrates are underrepresented and plants are adequately represented when compared with their prevalence in nature. The authors show disappointment in this trend because successful conservation requires the study of all groups of organisms. I completely agree, and for this reason, I in turn was disappointed in their analysis of the literature because they considered only plant and animal taxa, ignoring other groups, particularly microorganisms. Yet, there is increasing evidence within the published ecological literature that microbes can play important roles in the functioning of ecosystems and in the regulation of plant and animal populations and communities. To evaluate any existing bias against microbial taxa, I reviewed 5 years of issues (1997–2001) in three journals (*Conservation Biology*, *Biodiversity and Conservation*, and *Biodiversity and Distribution*). I found that microbes were rarely studied at all: fungi/lichens, 0.024 as a proportion of all articles; protists, 0.007; and bacteria/viruses, 0.006. These values are far lower than the proportion of articles considering plants or animal taxa, as reported by Clark and May, even though microbes may arguably represent the majority of the taxonomic diversity in natural ecosystems. It is clear from these data that conservation research is even more unbalanced than reported by Clark and May.

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Response

KLIRONOMOS MAKES A VALID AND IMPORTANT point. It is, however, a bit odd for him to be "disappointed" in our analysis. We did not explicitly include microorganisms in our analysis of the literature on conservation biology because, as Klironomos shows, such studies at present constitute a negligible fraction. We nevertheless agree that the paucity of literature in this area is not a good thing.

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Revisiting an Archean Impact Layer

G. R. BYERLY *ET AL.*'S REPORT, "AN ARCHEAN impact layer from the Pilbara and Kaapvaal cratons" (23 Aug., p. 1325), is an important addition to the growing literature on early Precambrian impact ejecta. Their zircon data provide compelling evidence that spherule layers in Australia and South Africa were formed simultaneously by a single impact about 3.47 billion years ago. The size and abundance of the spherules strongly suggest that they are part of a layer that was dispersed globally. We concur with Byerly *et al.*'s assessment that "zircons from both the South African and Australian layers are best interpreted as locally derived detritus" (p. 1326). However, the presence of two identical populations of unshocked zircons in both regions does not support a large separation distance between the Pilbara and Kaapvaal cratons at the time of impact. The two suites of zircon crystals are so similar that we believe they were eroded from the same source rocks, which implies that these strata were deposited close to one another in a global context. On the basis of stratigraphic and geochronologic similarities, various workers [discussed in (*1*)] have already argued that the Pilbara and Kaapvaal cratons formed in close proximity to one another. Byerly *et al.*'s data provide some of the strongest evidence yet in support of this theory. Their study demonstrates the potential for using impact spherule layers to constrain Archean paleogeographic reconstructions, as well as for high-precision time-stratigraphic correlation between Precambrian successions on different continents.

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Response

WE THANK SIMONSON AND HASSLER FOR their endorsement of our interpretations of the origin and ages of Archean impact layers in the Pilbara and Kaapvaal cratons. The question they raise concerning the distance between these two areas and the possibility of a conjoined Pilbara-Kaapvaal Craton at the time of impact was addressed in our original submission, but suggestions by editors and reviewers required its removal. We have demonstrated (*1*) that the spherule layers document impacts with energies appropriate for both global dispersal of impact materials and generation of large tsunamis. Identical detrital zircon suites in the impact layers would suggest proximity of these cratons only if potential source rocks for the zircons were present on only one of the cratons, which would presumably have been located closer to the impact site and served as a zircon source for both areas. This is not the case. Sampled areas on both cratons contain preimpact felsic volcanic