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Revisiting an Archean Impact Layer

Bruce M. Simonson, *et al.*

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Reforming Undergrad Biology Curriculum

IN HIS ARTICLE ON THE NEW NATIONAL Research Council (NRC) report on needed reforms in undergraduate biology education, Erik Stokstad ("Biology departments urged to bone up," News of the Week, 13 Sept., p. 1789) mentions some of the obstacles to effective curriculum reform—the immense inertia of the faculty and their reluctance to give up "their" subject. One of the primary drivers of these impediments was identified in the Editorial by Timothy Goldsmith in the same issue ("Why is a liberal education so elusive?", 13 Sept., p. 1769): Faculty are usually reluctant to teach outside their areas of expertise. From the perspective of curriculum reform, this combination can be deadly. It also leads to a curriculum whose composition is stochastic rather than planned, as courses are added or dropped as faculty arrive and leave. But at least for the first 2 or 3 years of undergraduate education, most biology faculty ought to be able to teach effectively in several broad areas—why do we insist that an upper-year high school teacher cover all areas but that only 1 or 2 years later, students must be taught in a specialist fashion?

The solution is obvious but very challenging: design a curriculum around goals rather than content and involve the faculty in teaching fundamental, cross-disciplinary courses and courses outside their area of expertise. This could be enormously stimulating! For many years in a biology department, I taught biostatistics, a course whose content cut aggressively across all discipline areas. The freedom from parochial, specialty-driven course content and the sheer joy of teaching something that was fundamentally and enduringly important enlivened and invigorated my teaching.

A curriculum designed on goals and cross-disciplinary content could be a lot slimmer than the obese, fact-filled, overlapping and often repetitive courses that constitute the typical biology curriculum. Such a lean curriculum would free up the time needed to involve undergraduates in real, meaningful research activity—a real benefit to both students and faculty.

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IT IS ENCOURAGING TO LEARN THAT BIOLOGY faculty recognize that "undergraduates [need] a better appreciation of the connections between biology and the physical sciences" ("Biology departments urged to bone up," E. Stokstad, News of the Week, 13 Sept., p. 1789) and that steps are being taken to improve the situation.

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Let me suggest a method established 30 years ago at the University of California, Irvine, that required two luncheon meetings to implement: one with David Brandt (chemistry) and myself (biology) and the other between William Parker (physics) and myself.

I asked these researchers and teachers to tell me what they teach in their beginning chemistry and physics courses: the gas laws, pH, oxidation and reduction, and kinetics and thermodynamics.

I then made it a point in my beginning cell biology course to correlate those subjects with my lectures on osmotic pressure; colligative properties and determining the molecular weight of proteins; the Henderson-Hasselbach principles of buffers; electron transfer reactions in the mitochondria; Michaelis-Menton enzyme kinetics; and the production and utilization of energy in metabolism.

As a result, the students grasped these concepts of cell biology more easily because they had already learned the basic chemistry and physics involved. They also recognized that chemistry and physics were necessary for a deeper understanding of biology and that those courses were not just requirements to take and then forget.

And the lunches were good, too.

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Discussing the Origin of Life

J. L. BADA AND A. LAZCANO ("SOME LIKE IT hot, but not the first biomolecules," Perspectives, 14 June, p. 1982) discuss, among other things, the pros and cons of low-temperature versus high-temperature (deep-sea hydrothermal) sites for the origin of life. They seem to have overlooked that the hydrothermal sites all have both high- and low-temperature areas within a few meters of one another and that the turbulence associated with the vents will ensure at least sporadic mixing of these environments.

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IN THEIR PERSPECTIVE "SOME LIKE IT HOT, but not the first biomolecules," J. L. Bada and A. Lazcano (14 June, p. 1982) state that for monomers to undergo polymerization in the early "prebiotic soup," concentration would have been necessary. Yet, although they cite the work of Oparin (*1*), they do not refer to his statements on coacervation. Coacervates could form in dilute solution and reaction with cations, or other insolubilizing moieties could then have formed enclosing membranes.

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Reference

1. A. Oparin, *The Origin of Life* (Macmillan, London, 1938).

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IN THE FIELD OF THE ORIGIN OF LIFE, SCIENTISTS are divided into segregated schools that do not even agree on the standards of scientific inquiry. Ordinarily, science is perceived as the difficult search for an ever-more-comprehensive, true explanation of the world. But in the words of J. L. Bada and A. Lazcano ("Some like it hot, but not the first biomolecules," Perspectives, 14 June, p. 1982), the research into the prebiotic soup theory of the origin of life aims "to construct a coherent narrative." This is a remarkable statement. The objective scientific principle of a search for the truth is replaced by the subjective aesthetic principle of a well-constructed story.

The search for truth is only possible as a community effort for which a critical rational discourse is a conditio sine qua non. This discourse is of value to the extent that the theory to be criticized and the references used for the criticism are not misrepresented. Bada and Lazcano address two theories on the origin of life: (i) a global heterotrophic origin of life in a cold prebiotic soup, in which organic compounds slowly accumulated over thousands or millions of years, eventually leading to the origin of evolution by the onset of nucleic acid replication, and (ii) my theory of a local chemo-autotrophic origin of life in hot volcanic exhalations by synthetic autocatalytic domino reactions of low molecular organic constituents on mineral surfaces of transition metal sulfides (*1*, *2*).

According to the first theory, the compounds accumulating in the prebiotic soup must be hydrolytically inactive. Otherwise, they could not accumulate so slowly. In the second theory, the organic compounds (e.g., organo-metal compounds, thioesters, keto acids, and active amino acid derivatives), which are constituents of the domino reactions, must be synthesized in an activated form and must undergo rapid subsequent conversion. A slow accumulation of such activated organic compounds under the hydrolyzing conditions of an aqueous solution is not possible. Therefore, these two theories are incompatible. Bada and Lazcano overlook that fact when they claim that the theory of a chemo-autotrophic origin of life "is not a new idea" but rather was anticipated in 1955 by M. Ycas (*3*). Ycas wrote, "Under the influence of the energy of light or electrical discharges, simple compounds (methane, ammonia, etc.) of the original atmosphere form a great variety of organic compounds in solution in the ocean… While in solution in the ocean, the organic compounds will interact, forming… a system of interlocking cycles… as one living thing, the metabolizing ocean. The further evolution of this system presumably led to the production of catalysts of a high molecular weight" (p. 715). It is clear from this quotation that Ycas's proposal is fully within the prebiotic soup theory. Therefore, it cannot anticipate the theory of a chemo-autotrophic origin of life, with which it is incompatible.

Bada and Lazcano go even further in stating that my theory of a chemo-autotrophic origin of life is "a component of the prebiotic soup theory" in the sense that its reactions "could have played an important role in enriching the prebiotic soup in molecules not readily synthesized by other abiotic reactions or derived from space." This shows what it means to strip the field down to mere story construction, controlled only by the need for narrative coherence. It seems that any reaction that comes along as a result of my theory or any other future theory will be added to the soup theory. In this vein, telling the story of a prebiotic soup becomes all-inclusive. True science, however, is exclusive, thriving on conflict and refutation and having content by what it forbids.

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3. M. Ycas, *Proc. Natl. Acad. Sci. U.S.A.* **41**, 714 (1955).

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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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Reference
1. D. R. Nelson, A. F. Trendall, W. Altermann, *Precambr. Res.* **97**, 165 (1999).

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

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rocks that, if subject to erosion, would have yielded age suites of zircons like those in the impact layers. Hence, the similar detrital zircon suites are not relevant at this stage to evaluating how close or distant these cratons were at the time of the impact. The similarity of stratigraphic sequences and ages provides much more substantial evidence that the Pilbara and Kaapvaal cratons may have been conjoined during the Archean (*2*, *3*).

Our study was designed only to determine the age and equivalence of the oldest impact layers in these areas. Future, more detailed U/Pb studies might support a single conjoined Archean landmass but would require examination of many hundreds of zircons from each impact layer, with the aim of identifying populations of grains in both areas that could have been sourced by rocks in only one area.

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Letters to the Editor

Letters (~300 words) discuss material published in *Science* in the previous 6 months or issues of general interest. They can be submitted by e-mail (science_letters@aaas.org), the Web (www.letter2science.org), or regular mail (1200 New York Ave., NW, Washington, DC 20005, USA). Letters are not acknowledged upon receipt, nor are authors generally consulted before publication. Whether published in full or in part, letters are subject to editing for clarity and space.

CORRECTIONS AND CLARIFICATIONS

EDITORS' CHOICE: "Snake vine and Munumbi Miller" (20 Sept., p. 1961). Gary Strobel was incorrectly identified as the discoverer of taxol. Strobel discovered fungal taxol. Taxol was discovered by M. Wall, M. C. Wani, and co-workers [M. C. Wani *et al.*, *J. Am. Chem. Soc.* **93**, 2325 (1971)].

BREVIA: "*BLM* heterozygosity and the risk of colorectal cancer" by S. B. Gruber *et al.* (20 Sept., p. 2013). The order of the authors was incorrect. The correct order is Stephen B. Gruber, Nathan A. Ellis, Karen K. Scott, Ronit Almog, Prema Kolachana, Joseph D. Bonner, Tomas Kirchhoff, Lynn P. Tomsho, Khedoudja Nafa, Heather Pierce, Marcelo Low, Jaya Satagopan, Hedy Rennert, Helen Huang, Joel K. Greenson, Joanna Groden, Beth Rapaport, Jinru Shia, Stephen Johnson, Peter K. Gregersen, Curtis C. Harris, Jeff Boyd, Gad Rennert, Kenneth Offit.

RESEARCH ARTICLES: "Super ENSO and global climate oscillations at millennial time scales" by L. Stott *et al.* (12 July, p. 222). The second sentence of the second paragraph on page 226, which reads, "At times of cooling at high latitudes, the tropical Pacific was experiencing either less-frequent or less-persistent El Niños" is incorrect. It should read, "At times of cooling at high latitudes, the tropical Pacific was experiencing either more-frequent or more-persistent El Niños."

SCIENCESCOPE: "Next up" (17 May, p. 1219). The new interim Under Secretary for Science of the Smithsonian was incorrectly identified as Ira Shapiro. His name is Irwin Shapiro.

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