

SEP

## SPOTLIGHT

## WHO NEEDS (LOWER PALEOZOIC) BIOSTRATIGRAPHY?

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In the 1970s, comic genius George Carlin pushed the limits of censorship with a popular routine regarding the seven words you cannot say on television. Similarly, one can argue that geologists currently seeking grants from major funding sources, at least in the USA, are well advised to avoid using certain words or phrases in their proposals: regional, taxonomy, geologic mapping, and, perhaps the worst of the lot, biostratigraphy. This is quite unfortunate because, in reality, we have barely begun to tap the correlation potential of species range data within Cambrian and Ordovician strata, especially those provided by the rapid evolution of trilobites, conodonts, and graptolites through the early Paleozoic. Other groups have considerable biostratigraphic value in certain intervals and paleogeographic settings, but none rivals the aforementioned big three in their broad temporal and spatial range of utility. Over 95% of the lower Paleozoic system, series, and stage boundaries in current geologic time scales are zonal boundaries based on key trilobite, conodont, or graptolite species (Gradstein et al., 2004; Webby et al., 2004; Babcock and Peng, 2007). Consequently, the examples I provide to argue that the need for focused biostratigraphic research has never been greater, all involve use of the big three (especially trilobites, my specialty) in the Cambrian-Ordovician of Laurentian North America. A biostratigrapher specializing in a different taxonomic group, paleogeographic setting, or time interval, however, could probably make at least as strong a case for the untapped potential within his or her group.

If biostratigraphy is so important, then why has it fallen into such disfavor, as reflected in limited prospects for funding of purely biostratigraphic research and diminishing representation of this discipline in the geoscience faculty of academic institutions and government agencies? Flessa and Smith (1997) and Plotnick (2008) presented disconcerting data regarding the decline in the number of paleontologists joining geoscience faculties at North American colleges and universities. Within that downward trend lies an even more precipitous and alarming reduction in the number of dedicated biostratigraphers engaged in the description of new species and refinement of the chronostratigraphic framework for correlation within the lower Paleozoic. The situation is even worse in national geological surveys, where budget cuts and changes in institutional priorities have brought research on lower Paleozoic fossils and correlation nearly to a halt. Previously sizeable staffs of biostratigraphers at the U.S., Canadian, British, and Australian Geological Surveys have dwindled to a handful of individuals deprived of essential technical support and assigned various, nonpaleontological responsibilities that leave little time for the taxonomic and biostratigraphic work that needs to be done. This is not a global phenomenon, however. Support for taxonomic and biostratigraphic research has been plentiful in recent years in China and Korea, for example, and I have had the privilege of meeting and corresponding with some of the bright, energetic, young paleontologists emerging from healthy programs in those countries. There is no denying, however, that biostratigraphy is a discipline in

crisis in most countries and action is required to rectify the situation. The first step is to identify the factors contributing to the decline. Three factors that come to mind are discussed in the following paragraphs: (1) the misconception that a published biozonation is a completed work; (2) the fallacy that refined, nonpaleontological correlation methods have rendered biostratigraphy obsolete; and (3) a perceived loss of relevance for biostratigraphy in the minds of our fellow geoscientists.

Like a geologic map, a published biozonation and derivative chronostratigraphy (time scale) must be appreciated for what they are-works in progress. Owing to incomplete information, all are imperfect and can be corrected and greatly refined as new data are recovered from problematic areas or intervals. Those allocating limited resources, however, often are understandably reluctant to commit funds and personnel to produce a better map or zonation where one that is perceived as adequate already exists. Thus it is important that we emphasize the potential magnitude of improvement in both accuracy and precision of correlation. Reasonable estimates place the percentage of fossilized species that have been formally described at roughly 10%-15%, as opposed to the 85%-90% still awaiting discovery and description. With this number of species yet to be found, projected gains of one to two orders of magnitude in precision over that of current zonations are quite realistic. Most of the traditional genusbased trilobite zones established for the lower Paleozoic of Laurentia during the latter half of the twentieth century (e.g., Palmer, 1977; Stitt, 1983; Ross et al., 1997) were deposited over hundreds of thousands to one million years or more. In contrast, many of the species-based packages that we can now delineate span only tens of thousands or just a few thousand years. Such gains, however, require submeter-scale sampling, considerably narrowed species concepts (for good examples in trilobites, see Sundberg and McCollum, 2003; Webster, 2005; Adrain and Westrop, 2006), utilization of multiple fossil groups, and integration with nonpaleontological correlation methods. In a recently completed project, high-resolution biostratigraphic (trilobite and conodont) and carbon isotopic data from a dozen sections in Montana and Wyoming allowed us to trace four 1-1.5-m-thick parasequences in the uppermost Cambrian for over 125 km across the paleoshelf. Our goal was to test hypotheses that attribute extinctions at the base of the Ibexian Series (Ross et al., 1997) and top of the Ptychaspid Biomere (Taylor, 2006) to eustacy or other paleoceanographic events. Manuscripts detailing our conclusions are in preparation, and I do not wish to circumvent the peer review process by blurting them out here (but I will say that they are very interesting).

Although exciting advances have been made in nonpaleontological correlation methods in recent decades, most require, or at least benefit from, associated fossil control. Geochemical profiles, parasequence stacks and sequence boundaries, and magnetopolarity reversals, for example, certainly have enhanced our ability to subdivide and correlate lower Paleozoic sedimentary successions. They do so, however, by complementing biostratigraphy; they have not supplanted it. None of these tools can match the uniqueness of narrowly defined fossil morphospecies. On the other hand, once calibrated against a biostratigraphic time scale, they often assume the dominant role in extending correlation into areas where fossils are scarce. I tip my hat to my colleague Rob Ripperdan for providing carbon isotopic excursions to carry our correlations in the Lower Ordovician El Paso Group from the fossiliferous strata of southern New Mexico into the unyielding dolomites of the Franklin Mountains of Texas (Taylor et al., 2004). Such complementary integration will become increasingly important as narrower species concepts reduce not only the stratigraphic, but also the geographic range of the new species. It was, after all, the greater geographic range of utility that prompted biostratigraphers of the twentieth century to apply broad species concepts in the first place (Lochman-Balk and Wilson, 1958).

In an earlier Spotlight, Goodwin (2006) expressed concern over a disturbing ignorance on the part of other geoscientists in academe as to precisely what paleontologists in their departments do and what relevance it has for them. We all should share his concern and heed his call to educate our colleagues in this regard, a simple task for the biostratigrapher. Collaboration with traditional allies specializing in sedimentology, sequence stratigraphy, structural geology, and tectonics is easily arranged and goes a long way toward engendering a proper appreciation of what we can contribute. I speak from experience, having worked closely with two exceptional sedimentologists in Paul Myrow (Colorado College) and Tony Runkel (Minnesota Geological Survey), debunking myths regarding the early Paleozoic paleogeography of western Colorado, and the purported unsuitability of cratonic sequences like the Cambrian of the upper Mississippi Valley (UMV) for modern sequence analysis. We discovered that the emergent highlands shown along the Transcontinental Arch in Colorado in paleogeographic maps of the upper Cambrian and Lower Ordovician are merely artifacts of miscorrelation across that state (Myrow et al., 2003) and that the UMV inner-shelf succession is not nearly as incomplete nor as anomalous in sequence architecture as was generally hypothesized (Runkel, et al., 2007). In each case, a refined biostratigraphic framework enabled us to falsify widely accepted stratigraphic models built by outstanding geologists with less-precise time control (Gerhard, 1972; Sloss, 1996).

Greatly refined biostratigraphies also open the door for joint research with less-traditional colleagues, such as paleoceanographers and paleoclimatologists, as the time resolution achieved approaches that required for meaningful analysis of shorter-term climatic signals in deep time. Even such seemingly unlikely partners as hydrogeologists and environmental geologists are coming to appreciate the value of biostratigraphy in their efforts to understand controls on groundwater flow patterns and the distribution of karst terrain. For a splendid example, see Brezinski (2004), a thorough appraisal of the stratigraphic control on karst development in the Frederick Valley of Maryland. This paper illustrates the power of biostratigraphic data for clarifying complex stratigraphic and structural relationships in areas of extremely poor exposure. For an equally impressive example, see Ganis et al. (2001); in this work graptolites and conodonts from scattered outcrops form the basis of a greatly revised structural and tectonic model for the allochthonous strata of Pennsylvania's Hamburg Klippe. I cannot resist pointing out that the substantial gains in all the aforementioned regional studies came as a result of more detailed geologic mapping, expedited by refined biostratigraphy rooted in improved species-level taxonomy.

One more problem worth mentioning is the inherent incompatibility between the time-intensive nature of high-resolution biostratigraphy and the rapid return of results expected in the modern age of computerexpedited research. Technology has greatly accelerated some tasks, but the collection, preparation, and systematic evaluation of the fossils still require such immense amounts of time that the biostratigrapher is hard pressed to meet the new standards of productivity expected by research



John Taylor (at left in photo of field party in Wyoming) is primarily a biostratigrapher specializing in Cambrian and Ordovician trilobites. He is also a devotee of carbonate facies and depositional environments, a penchant dating back to his undergraduate research on the Silurian Keyser Limestone at Indiana University of Pennsylvania (IUP). At the University of Missouri, he was lured down-section into the lower Paleozoic by two world-class biostratigraphers, Jim Stitt and Ray Ethington. Remarkably, IUP was conducting a search for a paleontologist when John was finishing his Ph.D. at Mizzou, affording him the chance to return home to western Pennsylvania. More than a quarter century later, he is the senior member of IUP's (otherwise) strong and active geology faculty. Throughout his career, John has exploited his one redeeming quality, a pathological tenacity that enables him to extract trilobites from rocks others dismiss as barren, to establish partnerships with geologists who actually know what they are doing (e.g., Rob Ripperdan, to John's left, and Paul Myrow, to Rob's left ... well, not the best examples, but they are fun to work with). This collaboration has produced substantially revised Cambrian-Ordovician stratigraphies in the central Appalachians, upper Mississippi Valley, northern and southern Rocky Mountains, and southwestern USA. What John considers the most important product, however, is the experience provided geology undergraduates like Scott McCallum (on right) and Bryn Clark (to Scott's right).

institutions and funding agencies. Nonetheless, the immeasurable value of sound biostratigraphic data for testing the elaborate stratigraphic and paleogeographic models emerging from our computers justifies the required forbearance. Even such a landmark paper as Goldhammer et al. (1993), which earned the Best Paper Award in the Journal of Sedimentary Petrology for the groundbreaking work it represented in cyclo- and sequence stratigraphy, represents work in progress, and the correlations they propose must be treated as hypotheses for testing. I use that paper as an example here because additional biostratigraphic studies (Taylor et al., 1992; Loch, 2007) falsify many of the correlations they proposed between the rock units in Texas and Oklahoma and the Appalachians. This does not diminish the magnitude of the contribution made by Goldhammer et al. (1993); it merely reinforces my assertion that biostratigraphy remains an indispensable component of modern stratigraphic practice. If left uncorrected, such miscorrelations carry over into later syntheses, such as the finely resolved Paleozoic global sea-level curve recently presented by Haq and Schutter (2008), which proposes (again, for critical evaluation) more than 50 eustatic events for the early Paleozoic alone.

And let us not fail to consider the immense value of a refined time scale to the paleontological community itself as we scour the rock record for the oldest and youngest evidence of particular groups, behaviors, or conditions. Astonishingly, the success that my paleobotanist brother (Wilson A. Taylor) and his colleagues have had in tracking early land plants and their likely progenitors back into the early Paleozoic (e.g., Strother et al., 2004) has forced him to concede (although not in public or in writing, he insists) that my work to refine biostratigraphic resolution in the Cambrian and Ordovician has some merit. The greatly refined biostratigraphic framework currently taking shape will also serve us well as we seek greater insight into the spatial and temporal distribution of microbialites (specific types, as well as microbial reefs in general), the early history of vertebrates, the evolution of specific behaviors as reflected in the appearance of new and different ichnofaunas and ichnofabrics, and the list goes on.

We stand not in the twilight of biostratigraphy as a discipline, but at the dawn of an exciting new age of collaborative deployment of highresolution biostratigraphy and nonpaleontological correlation methods to create a chronostratigraphic framework of unprecedented precision. It will not be easy, and it will not happen quickly; however, the ultimate benefits are too great to allow haste or ignorance to prevent it from happening. It is truly an exciting time to be a biostratigrapher!

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