Preface

Science is supposedly ultimately constrained by the nature of the physical world, meaning that changes in scientific methods and practice are supposed to be away from those with less utility and toward those that are more revealing, useful, and productive of insights into the nature of that world. In practice, however, science is no less susceptible to fads, culture shifts, and pendulum swings than any other realm of human endeavor. This is an especially important feature of science to keep in mind in the present climate of shrinking government funding (at least in proportion to the demand) and the resulting susceptibility of individual scientists and entire disciplines to being influenced by the changing priorities of funding agencies (even if, as such agencies maintain, those priorities come ultimately "from the community"). The present volume is in several important respects a testimonial to both the threats and opportunities that such scientific culture swings pose, both for the individual researcher and a wider field.

When scientific research in the Dominican Republic Neogene began more than a century ago, paleontology was an essentially descriptive discipline, focused mainly on finding, describing, and documenting the taxa represented in the fossil record, and (especially in invertebrate paleontology) on using these taxa for biostratigraphic correlation. Despite the successful integration of paleontology into the Modern Evolutionary Synthesis in the middle of the twentieth century (Simpson, 1944, 1953; Jepsen et al., 1949; Gould, 1983), the vast majority of paleontological research continued in this tradition, and most paleontological papers – including the fundamental works on the Dominican Neogene – were some version of "a new X from the Y of Z-land" (Gould, 1989:114).

The structure of paleontology, at least in the U.S., began to change in the late 1960s and early 1970s in association with at least three significant developments, each of which was to have significant influence on paleontological research in the Dominican Republic Neogene. The first was an increased interest in the ecology of fossil taxa (in addition to simply using fossils for paleoenvironmental reconstruction). There was a burst of research activity around this new slant on "paleoecology" as a new generation of paleontologists sought to interpret fossil assemblages by close comparison with living communities. Although by the early 1980s this research program had lost much of its focus, it did produce some innovative and lasting contributions, including attempts at documenting long-term patterns of biological communities in the shallow ocean (Allmon and Bottjer, 2001).

The second major development was the Deep Sea Drilling Project (DSDP) (see, e.g., Hsu, 1992; Corfield, 2001). This enormous (and well-funded) project was influential to paleontology in two significant ways. Scientifically it provided both abundant new data and a new temporal and (in many ways) intellectual framework for applying fossils to answering questions of Earth history, including climate, sea-level, temperature, and ocean circulation and nutrient status. Although it was concerned almost exclusively with microfossils, the DSDP clearly demonstrated the unique value of paleontology to reconstructing the biotic and abiotic environment in a modern high-tech scientific context. Methodologically, it also demonstrated – not least to paleontologists themselves – how paleontology could be an integral part of large-scale, multidisciplinary "big science".

The third development was the percolation of aspirations among the younger generation of paleontologists to contribute in substantive and unique ways not just to geology but to evolutionary biology. These stirrings led to what became known broadly as "paleobiology", a major subfield of which became devoted to the compilation of taxonomic data from the literature, a research program that came to be known as "quantitative" or "analytical" paleobiology (Gilinsky and Signor, 1991; Sepkoski, 2005). This and related research programs emphasized theoretic over descriptive approaches and new methods of analysis of existing systematic data from the fossil record as much or more than the acquisition of new data. It brought paleontology to the "high table" of evolutionary theory (Maynard Smith, 1984; Eldredge, 1995), and – intentionally or not – it diminished the status of traditional descriptive systematics for its own sake.

The lessons and implications of the first two of these developments – the DSDP and paleoecology – were not lost on the founders of the Dominican Republic Project (DRP). In the late 1970s this group concluded that land-based, macropaleontology could benefit from a DSDP-style, large-scale, international, multi-investigator approach to creating and compiling taxonomic, stratigraphic, and paleoecological data (Saunders et al., 1986; Jung, 1993). At the core of the new project were two main ideas. First was an emphasis on a rigorous stratigraphic and sampling protocol that would be used by all project participants. This would, the organizers thought, avoid many of the biases inherent in different investigator's styles of sampling, and would allow data from many researchers to be readily compiled and compared. Second was the decision to distribute sorted samples to systematic specialists around the world. This would, thought the project leaders, bring to bear a much more powerful set of specialists than would be possible with only one or a very few systematists.

With the benefit of almost 30 years of hindsight, several aspects of the DRP experiment are noteworthy. Most conspicuously, the common stratigraphic and sampling regimes were enormously valuable and used by almost all participants, and provided an excellent model in these respects for the subsequent Panama Paleontology Project (PPP; see Jackson et al., 1996; Collins and Coates, 1999). By comparison, the DRP systematics results were both more and less successful than one might have anticipated or hoped. Although it received significant funding provided by the Swiss National Science Foundation, the DRP never had the financial resources to support the work of the individual systematic researchers who volunteered to take on various taxonomic groups. This inevitably contributed to sometimes lengthy delays in, and sometimes total abandonment of, production of the individual systematic monographs. Although DRP coordinators and collections staff at the Natural History Museum in Basel tried to keep close track of the collections that had been sent out, some were never seen or heard from again. (This experience was not lost on the coordinators of the PPP, who explicitly chose not to distribute material to numerous independent specialists.) Finally, although the DRP organizers certainly envisioned that the data resulting from the project would almost certainly be used for research into broader paleobiological topics, they did not specify in advance what those topics would or should be. Although the DRP was enormously innovative in its approach to centralizing stratigraphy and sampling while decentralizing its systematics, it was, *as a project*, not particularly innovative in the applications of the data that resulted. It was, rather, left to individual researchers to use their or others' data to investigate whatever topic was of interest to them.

Which brings us to the third of those three critical 1970s-era developments in paleontology. As noted by Nehm and Budd in the present volume, many of the subsequent studies that used DRP data were of great significance for areas of paleobiology such as evolutionary tempo and mode and diversity, extinction and turnover. Yet these were not explicitly goals of the project at the outset. In other words, careful attention to making large, well-documented, and well-curated collections within a common, standardized, high-resolution stratigraphic framework made possible the fruitful application of the resulting data to larger theoretic questions. High-quality descriptive paleontology of the "traditional" sort permitted high-quality synthetic paleontology of the newer sort later.

Laudable though this outcome – and its copious illustration in the present volume – is, anyone who has written or reviewed an NSF proposal in the last 20 years knows that something is amiss here. It is almost impossible today to obtain funding for generation of basic systematic data without specifying beforehand to what larger (preferably pressing) theoretic use those data will be put. As an NSF program officer once put it to me, "there is an infinity of groups that need systematic revisions; we can only fund those that are interesting" because they can be used to address an "interesting" question. Thus the fundamental structure of the DRP, the success of which the present volume celebrates, would almost certainly not be fundable in this form by NSF or similar agencies today.

It has been frequently noted that paleontologists are a generally solitary lot, not especially well-suited to the large-scale collaboration and group-think often associated with "big science" projects. Historically, it is often observed, we have mostly pursued research that required relatively little infrastructure, aside from space to store our collections, a library, a microscope, and a means of travel. These attributes have been bemoaned as keeping paleontology out of the "big science" scene. We have, it is said, never "gotten our act together" and "gotten our share of the pie" the way the physicists, astronomers, or genomicists have. The difficulty of getting paleontologists to collaborate on one or a small number of larger topics or problems is highlighted by the multiplicity of national and international meetings and reports, most supported at least in part by NSF, that have attempted over the past couple of decades to chart a common, collaborative, "big-science" research agenda for paleontology (e.g., "Geobiology of Critical Intervals", Stanley et al., 1997; "Paleontology in the 21st Century", Lane et al., 2000; and most recently "Future Research Directions in Paleontology", FRDP, Bottjer, 2007).

It is noteworthy that the big collaborative projects in paleontology that *have* succeeded have been, in large part, not question-based, but (literally) data-based, such as the *Treatise on Invertebrate Paleontology* and the Paleobiology Database. In this context it is interesting that the recent FRDP report (Bottjer, 2007) includes as one of its five major objectives "Database and Museum Collection Development and Integration". The authors of the FRDP write: "Museum collections, databases and informatics are an integral part of the infrastructure of paleontology at present, and will continue to be so into the future. In order to be dynamic and useful resources, both require long-term support. Further, these two infrastructural resources are quite naturally complementary and interlinked. … Databases and museums undergird integrative multiuser research initiatives as well as individual projects. Being able to combine different datasets provides opportunities to ask new and more widely ranging questions in deep time studies. … Thus, both require long-term support and stability."

The present volume supports this objective and demonstrates the profound utility of well-coordinated data supported by carefully-collected and well-curated collections, and the editors have gone to considerable lengths to emphasize these themes. I suggest, however, that we might take this lesson even more seriously. As a discipline, paleontology might recognize, reiterate, and celebrate that "big paleontology" cannot be successfully modeled closely after "big physics" or "big astronomy" or "big molecular biology". Our major collaborations appear to be most fruitful in the coordination and assembling of large data sets, not necessarily in their interpretation around a narrow predetermined set of large or "important" questions. The actual generation of much of our data, especially systematics, and its application to questions about the history of the Earth and its life appear to require the dedicated attention of one or a very small number of individual researchers.

This does not make our science less than physics, astronomy, or genomics; it makes it different. It means that more projects like the DRP are needed – applied to both new field collections and existing museum collections (Jackson and Johnson, 2001; Allmon, 2005) – in order to generate and make available large quantities of new, high-quality systematic, stratigraphic, and paleoecologic data. It may be that the precise questions to which these data can be applied cannot now be specified. But that does not mean that the data are and will not be valuable. Indeed, many questions will not occur to us until the data are generated.

Finally, it should be noted that the DRP was and is a truly international, multi-institutional effort, involving museums, universities, and numerous individual researchers, including a number of Ph.D. students. The project was begun by Swiss paleontologists, and soon involved scientists from Tulane University, and eventually from dozens of other institutions around the world. In this context, I cannot help but note with pride (albeit more of the kind felt by the fan on the sidelines than of the player in the game) the prominent role that the Paleontological Research Institution (PRI) has played in this story since the early twentieth century. PRI's founder, Cornell professor Gilbert Harris, was the major advisor of Carlotta Maury, who conducted the first comprehensive overview of the macrofauna of the Cibao Valley, and published it in her landmark monograph (Maury, 1917a,b). Her collections remain today at PRI. When the DRP was started in the late 1970s, its architects chose PRI as the publisher of its systematic monographs in its journal, *Bulletins of American Paleontology*. To date, 22 such contributions have appeared, and more are in press and in preparation. With the retirement of Emily Vokes from Tulane in 1995 the large collections of Dominican fossils that she had assembled with her late husband Harold over more than three decades came to PRI. The involvement of a small museum in upstate New York in a project organized by a major European museum and a husband-and-wife academic team at a private university in Louisiana – now taken over by a new generation of researchers at an even more far-flung spectrum of institutions – is perhaps a fitting testament to how paleontology at its best (big, small, or otherwise) works.

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