OBITUARIES

ROALD FRYXELL: A REMEMBRANCE
1934–1974

Have you heard the one about the Fryxell wind-up doll? You wind it up, and it asks you why you wound it up . . . .

Somehow it is fitting that, six years to the day after Roald Fryxell was killed on May 18, 1974, in an auto accident at age 40, Mt. St. Helens sent yet another layer of volcanic ash to cover the Columbia Plateau and confound the geologists and archaeologists of the future. Although it is not particularly fitting that a remembrance of Fryx in *American Antiquity* should have to wait ten years after his death, perhaps an archaeologist, having learned from him about the movement of glaciers, can be forgiven for not rushing into print too quickly.

Since 1978, the Society for American Archaeology has presented the Fryxell Award to distinguished colleagues whose interdisciplinary approach to the study of archaeological questions does honor to the ideas and skills Roald Fryxell embodied in his research and teaching. Fryx himself applied his geological knowledge to archaeology by disentangling basalt, floodplain, and loess sequences in the Columbia Plateau; dating alluvial breccias in northern Mexico; and recording cave deposits in Spain, along with documenting the context of early North American occupations at Lind Coulee and at Marmes, in the Pacific Northwest.

His studies touched the future, too. He was instrumental in developing core sampling devices and analyzing lunar regolith samples returned by astronauts aboard Apollo 11 and five subsequent lunar explorations. The particulars of Fryx's early life and professional career, along with a selected bibliography of his published works, can be found in a lengthy obituary published by the Geological Society of America (Friends of Roald Fryxell, 1978).

To those of us who were his students in the Quaternary Studies Program at Washington State University, his chief contribution to our education was not field methods or scholarly papers or specific research results, but rather a way of thinking, of looking at data, and developing hypotheses. He taught us not only how to draw stratigraphic profiles, but *why.* He taught us that *how* you say something was at least as important as what you say. He taught us that *testing* competing hypotheses, not confirming your favorite one, is the foundation of scientific method, and that anomalies in data can lead to the most significant questions for future research. Most important, he taught us to look for, and sometimes to see, the "big picture," and to understand that bits of data have meaning only as they contribute to a broader understanding of the context of a research problem.

The significance of these lessons is driven home to all of us daily, whether we are in academia, government, or the private sector. It is Fryxell's *teaching,* and his passionate caring about the research process, his students, and science itself, that live after him, and on which the rest of this remembrance will focus.

For most of his teaching career at Washington State University Fryx only taught two classes: "Interpretation of Paleoenvironments," and "Physical Stratigraphy of Archaeological Sites." The first, in spite of its name, consisted of a set of readings in scientific method, a grueling nine-day field trip, and an individual student research project. The second consisted of intensive fieldwork at a specific locality, which changed from year to year. No one was admitted to either class without a full year of soils courses, including soil morphology.

The lectures were well-organized, neatly prepared on note cards, profusely illustrated with blackboard charts, graphs, and drawings (including the soon-legendary block diagram that described the development of the Columbia Plateau, drawn with four colors of chalk and both hands simultaneously, and always done the first evening of the field trip). Lectures introduced vocabulary, presented research results, and described field and laboratory techniques.

Discussions, though, challenged each student to answer the perpetual question, "Why?": Why must that be so? What if it were not so? What else must be true, or false, if that is true, or false? If this ash were 1,000 years older, or younger, what would happen to the rest of the sequence? How would a certain new bit of data change your interpretation? How can you find out? Why would you conduct those analyses, instead of some others? Why do you need those data, instead of some others? What data do you need to test that idea? What if those data show your idea to be wrong?

Discussion generated by such questions, and the groping for answers, consumed time in class, in vans on field trips, around campfires, over dinner, over wine, in the rain and snow, and in Helen and Roald Fryxell's living room. Being a student of Fryx's meant total immersion in data, and thinking about data, and thinking about how to think about data. having been a student of Fryx's means continuing to ask those sometimes unanswerable or otherwise difficult questions of oneself and one's research, and of one's colleagues and their research.

In short, he demonstrated—sometimes painfully—that science is a logical construct, that "facts" are tied together into a whole that is framed by *context,* and that both raw data and statistical or other manipulations of those data are meaningless without trying to understand that whole, including its context, and the nature of the connecting framework itself. Further, hypotheses are tools in the scientific endeavor, tools to be developed, tested, altered, tested again, and discarded if they are not helpful, or are false, or no longer guide research in meaningful directons.

If hypotheses are the tools of science, descriptive data are the raw materials of the scientific construct. Again and again, Fryx would assert that archaeology lacked not theory, but data. Read any archaeological monograph, he would say, and what do you find? Not *descriptions* of sediments, projectile points, local plants or animals or ethnography, but *interpretations* of "occupation levels," or "cultural phases," or "life zones," with chronology inferred from other interpretations of other data at another site, sometimes in another region altogether. Given that *all* data are biased by the observer's interests, skills, questions, and collection techniques, how scientific is it, really, to describe second- or third-level interpretations of those data as "facts" that "prove" your hypothesis? Fryx would ask, and we would ponder.

If his teaching methods were Socratic, his reading lists were positively Brobdingnagian. We were told to read Ernest Hemingway (to illustrate the need for, and power of, rewriting drafts until they say *exactly* what you mean); Hempel, and Popper, and Watson (to illustrate that "science" and "scientific method" rest on both rational and empirical research); Gilbert, and Davis, and Bretz (to illustrate that data and method are the foundations of theory and interpretation). And we learned map drafting, slide production, abstract-writing, and soil chemistry, along with ash identification, stratigraphic interpretation, and geological field sampling, all to make us better archaeologists.

One of the most important lessons Fryx taught, in a kind of backhanded way, was humility: the method of multiple working hypotheses, the careful examination of empirical data, the willingness to revise or discard an idea if the data do not support it, and the need to have all parts of an idea fit within a rational and empirical framework, all tend to make science and scientific research a humbling enterprise. As students, we of course developed marvelous ideas, which we sketched on cocktail napkins and with which we hoped to make our professional reputations. Fryx would always bring us back to earth, by asking about *data*: what do you need to know, to test that idea? He then would ask about other ideas: what else might account for that pattern? and after that, what else? How do you decide which hypothesis is better? How do you evaluate competing ideas, what data do you need, and how do you get them?

Sometimes the experience was not only humbling, but humiliating, and most of our "cocktail napkin" hypotheses died a natural and well-deserved death. Those that did not, of course, went on to guide research into alluvial chronologies of southeastern Washington (why are sites where they are?), or volcanic ash stratigraphy of "early man" sites (how can we say for certain which vent is the source?), or the physics of lithic technology (why do rocks break as they do, whether from human or other forces?). The constant interplay of empirical data with research methods and hypothesis development and testing never was simply an end in itself, but was a means to ask questions about past human lifeways by first trying to understand the natural environmental context in which those lifeways flourished.

Rather than being "add-ons" to archaeological research, such questions and their answers form the very *foundation* of our science. Only by comparing and contrasting data resulting from natural processes (e.g., soil-profile formation) with those we think may result from human action (e.g., midden development) can we provide a context for understanding human behavior. Only by understanding context can we begin to comprehend the nature of human endeavor, from the manufacture and use of tools to the place of human beings in the world, and all the other "big" questions.

As the Friends of Roald Fryxell predicted (1978:8), his untimely death has indeed had "impact beyond measure." In the past ten years, when funding for archaeology and numbers of archaeologists shot nearly uncontrollably upward, the scramble to recover threatened artifacts or publish new interpretations of areal prehistory has resulted in repeated failures both to conduct adequate contextual studies of archaeological sites, and to build theoretical frameworks on sound empirical data and traditional principles of scientific method.

Instead, professional archaeologists of all stripes conduct "surveys" that expect to locate significant archaeological sites in areas of high erosion, freeze-thaw, or active alluviation; many acres are surveyed, but few sites are found. Other well-known scholars infer major prehistoric subsistence changes attributed to "recent" invasion of a staple food plant, without conducting the necessary paleo-botanical or palynological studies to determine the actual distribution and abundance of that plant through time in the study area. And still other scholars seek to "prove" a single hypothesis by seeking only data that confirm that hypothesis. These failures to recognize the need for and uses of contextual studies, the mistaken understanding of the nature of scientific method, and the ultimate waste of time, money, and archaeological sites sacrificed to pursue these incompletely thought-out "research designs" should make us all shudder.

Finally, Fryx taught us that research data, and scientific method, and science itself are useful, and that their uses should be more widely communicated to the public. As usual, this lesson revolves around the question "Why?": Why do that study? Why dig that site? Why do it that way? Why do archaeology at all? Those of us who deal with grant sources, deans, decision-makers, clients, and the tax-paying public every day know that such questions are as common as they are valid. Answering them is vital to the continued health and vigor of our profession, and to the continued elucidation of the secrets of the past contained in our irreplaceable archaeological resources.

For ultimately, Fryx taught us that everything is connected to everything else: Columbia basalts to the moon, hypotheses to data, research questions to research methods, research answers to bill-paying publics, credibility to logic, quality of presentation to continued funding, environment to archaeology, science to the real world.

And so, Fryx, that's why we wound you up.

LESLIE E. WILDESEN