

Lyman begins his review with the claim that this book is unsuited either for classroom or for use by archaeologists. Following this extravagant claim one expects to find a long list of well documented, egregious errors. Instead he provides only undocumented assertions such as: Read misinterprets Irving Rouse and Robert Dunnell and lacks conceptual clarity in his use of the terms attribute, variable, state and dimensions (but Lyman then goes on to summarize my method for forming groupings of artifacts using precisely the terms for which I supposedly lacked conceptual clarity), the book supposedly has illustrations that are “inaccurately labeled and unlabeled” and “numerous items published in the 1990’s are not cited.” (There are inadvertent omissions, including Lyman’s 2006 book, *Measuring Time with Artifacts*, as well as an article or two by Lyman or by Lyman and O’Brien published in the 1990’s). Whether these are egregious errors, minor shortcomings or misreadings by Lyman cannot be determined since no examples of the supposed errors are provided.

In the same vein, Lyman notes that on page 52 I have a 1960 quote from Rouse and I incorrectly state that it presages the work of Spaulding in 1953. I am guilty as charged. However, earlier on the same page I discussed how Rouse’s 1939 work “presages later attempts to objectify archaeological classification” (p. 52) and I (mistakenly) carried forward that idea and then applied it to the 1960 Rouse quotation. I leave it to the reader to decide if this creates so much confusion, as Lyman claims, that the book is unsuited for classroom use.

He states that my “characterization of taxonomic classification implies that attributes are weighted, types are asymmetrically defined, and types are defined by different numbers of attributes” and then adds the comment “none of these is a necessary feature of a taxonomy”, which only makes sense as a criticism if I had claimed that *all* taxonomic classifications are characterized in the quoted manner. Lyman seems to have in mind as a counter example a paradigmatic classification in which attributes are not weighted, types are symmetrically defined, and each type is defined by the same number of attributes. I included a paradigmatic classification as a special case of a taxonomy: “A paradigm can be considered to be a special case of a taxonomy in which no variable is excluded as one traces through a pathway in the taxonomy” (p. 310, n. 1), and so Lyman’s assumption that I exclude paradigmatic classifications from being part of taxonomic classifications is belied by what I wrote.

I made a two sentence summary of biological classification that is said to be “superficial” (not surprisingly for a two sentence summary) and out of date. But what I stated (“Biological classifications are based on homologous traits under the presumption that two synchronous species sharing a homologous trait must have had a single, ancestral species from which both are derived via natural selection”) is consistent with the characterization of homology and classification in a Special Issue of the journal *Biology and Philosophy* based on the 2006 meeting of the Philosophy and Developmental Biology Working Group. The editors of the Special Issue comment: “homologues are important morphological-developmental, taxonomic, and evolutionary units ... Perhaps the best known approach to homology is the ‘taxic’ approach associated with *mainstream work in contemporary systematics*.... The taxic approach uses points of resemblances between

organisms (shared character states) to diagnose their evolutionary relationships (Brigandt 2002, 2003; Donoghue 1992). These points of resemblance may reflect common ancestry (homology)” (Brigandt and Griffiths 2007:635). If I am out of date as Lyman claims, then mainstream, contemporary systematics in biology must also be out of date.

After presenting this list of supposedly egregious errors, Lyman goes on to discuss and critique the methods I presented in my book. He begins with the assertion: “In Read’s view, each artisan had a mental model of what a pot or projectile point should look like, and they approximated that model with each specimen they produced.” This statement does apply to James Deetz who I quoted, but I explicitly rejected Deetz’s notion of a mental template: “The idea of a transformation suggests a *dynamic aspect* at the conceptual level *that is lost* when the conceptual level is reduced to a *static construct* such as a ‘mental template,’ defined as ‘the idea of the proper form of an object [which] exists in the mind of the maker’ (Deetz 1967:45)” (p. 185, emphasis added). I go on to comment: “the ‘mental template’ metaphor is too stringent” (p. 185), a theme I pick up on later: “An artisan *does not mechanically produce* an artifact according to a mental template of trait-ideas” (p. 290, emphasis added). I define an artifact not by a mental model or template construct as one would suppose from Lyman’s comment, but in a very different manner: “Definition: *An artifact is a material object conceptualized by the members of a social group as belonging to a category that is part of the cultural repertoire for that group*” (p. 187, emphasis in the original). I leave it to the reader to determine how one goes from what I wrote to Lyman’s summary statement “In Read’s view, each artisan had a mental model ...”.

Lyman continues with the assertion: “Read believes that the complete set of artifacts comprising the piles [Lyman’s term] can be ‘seen’ as a statistically *representative* sample of a population (p. 154) in terms of the mean, mode, variants, deviants” (p. 112, emphasis added; perhaps what Lyman means by “variants, deviants” is variance and outliers). What I actually wrote is: “That an internally homogeneous/externally isolated group can be seen as a *statistical sample* from the members of a class of artifacts makes possible a way to provide class definitions from group characteristics” (p. 154, emphasis added). By adding the term “representative,” Lyman changed what I wrote into something that is obviously incorrect. There is no way one can assume that a group of artifacts, homogeneous or not, is a representative sample from an, as yet, unspecified population. Further, I used “seen as” to deliberately signal to the reader that conceptual work is needed to make the transition from groups of artifacts to samples in a statistical sense. The subsequent three pages and Figure 6.1 do that conceptual work.

In addition to changing the meaning of what I wrote, the added word *representative* is crucial for Lyman’s subsequent comments about my alleged failure to define what constitutes a population: “Read does not tell us how a population is defined” (p. 112), even though I wrote: “An *empirical population* is a well-defined collection of actual entities ... by well-defined is meant whether one has membership rule for the population that makes it evident if an entity belongs to that population” (p. 200-201, emphasis in the original). I then discuss problems with implementing this definition from an archaeological perspective on pages 200-201. I make a critical distinction between an empirical population and

“a population based not only on what was produced over a particular time period and at a specific location, but also on what could have been produced keeping fixed the conceptual and technological systems(s) used in the production of the artifacts in question” (p. 202). I spell out on pages 206-209 what is meant by this distinction between an empirical population and a population of “all possible artifacts,” with the latter consistent with the definition of a hypothetical infinite population by the eminent statistician R. Fisher: “[T]he values ... are interpreted as a random sample of a *hypothetical infinite population of such values as might have arisen in the same circumstances*” (1954:6-7, emphasis added). I go on to indicate why this distinction is necessary for making a transition from statistical parameters to normative values (when they occur) that may guide the production of artifacts.

Lyman continues with yet another alleged problem with representativeness: “he does not indicate how to determine if a sample is representative” (p. 112). However, I define and discuss this in detail on pp. 208-209: “By *representative* will be meant that estimates of population parameters such as μ and σ and based on the sample mean and the sample standard deviation are not biased ... random sampling of a population is one way to ensure representativeness” (p. 208, emphasis in the original). Whether a particular sample that one has in hand is representative or not depends on empirical details such as how the sample was obtained, but those details do not determine what is meant by a representative sample.

His following statement “what it [a sample] might be representative of (the population) is ambiguous” is simply an artifact of Lyman’s misrepresentation of what I wrote. There is no ambiguity: “If we can consider each artifact produced by an artisan to be a random sample of size $n = 1$ from the population of all possible artifacts that could be produced keeping fixed the production process, then a collection such as ‘all pottery objects made by potters sharing the same conceptualization about pottery form and size’ will be a random sample [i.e., representative] from the population of all pottery objects that could have been produced under the conditions stipulated in the sample definition” (p. 208). In this presentation I did not use standard statistical sampling definitions as we need, first of all, for our data analysis to be framed in accord with our understanding of past societies and artifact production and use, and secondly to employ statistical methods as a tool to achieve our research interests and goals. How we transform statistical concepts such as a population mean and a population standard deviation into archaeological concepts, rather than the reverse, is critical and non-trivial and to be worked out by archaeologists, not statisticians, as I discuss beginning on page 200.

Lyman’s concluding remarks continue the misrepresentation of what I wrote. For example, I wrote: “the presence of two normative widths [for the concave projectile points] implies that the settlement consisted of two lineages ... consistent with a society based on matrilineal lineages (Keesing 1975)” (p. 222) and I go on to note that this interpretation is in accord with what is known about the Chumash Indians with whom the village settlement may have been associated. For Lyman this conclusion is to be rejected because “there are three or four types” (when the convex points are included) and so according to him I must have made my conclusion on the basis of “ethnic and geographic propinquity

rather than projectile point type” despite what I wrote. What I based my conclusion on is stated explicitly. It is the responsibility of the reviewer to deal with what an author writes, not what the reviewer imagines the author has written.

Brigandt, I. and P. Griffiths. 2007. The importance of homology for biology and philosophy. *Biology and Philosophy* 22:633-641.