WE NEED BETTER CHRONOLOGIES: PROGRESS IN GETTING THEM

George L. Cowgill

Archaeologists have long celebrated their unique ability to deal with the long term. But we are increasingly recognizing that we can and must also understand more rapid events. They are critical for many of the questions we now consider most interesting, such as distinguishing between changes that unfolded gradually and those that happened suddenly. In considering interactions among regions, we need something better than cross-dating on the basis of resemblances in material objects. That is especially so when we would like to know where something first developed and the tempo of its spread to other regions.

As one particularly clear example of the beneficial effects of finer chronologies, Robert Santley (in Sanders et al. 1979:65–73) showed how shorter phases can drastically alter our picture of the ebb and flow of settlement history in a region. When Basin of Mexico sites in the Cuauhtitlan region are dated only in broad terms to the Middle and Late Formative periods, a rather placid picture of settlement change emerges. When finer subdivisions are made between early and late Middle Formative, and between early and late Late Formative, the story becomes very different, much livelier, and closer to lived experiences. This is not just crossing t's and dotting i's. It is more like using lenses of greater magnification to see phenomena previously invisible.

In a few special cases, high precision and accuracy is possible. In much of the Maya area Long Count dates are accurate to the very day, but they do not closely date everything we would like to know, and, for nearby areas, cross-datings are only approximate. In parts of the southwestern United States, absolute intervals of about a decade are possible, largely thanks to tree-ring dating. This has had a major impact on the questions that can be asked and answered. A tree-ring chronology going back to A.D. 771 has been established in the state of Queretaro, Mexico (Stahle et al. 2011). Eventually this method may be applicable to more regions and earlier times.

Meanwhile, intervals on the order of two or three centuries and an uncertainty range of a century or more exist in many regions of Latin America. We must and can do better than that. Even without tree rings, we should be able to obtain secure and uncontroversial intervals of a century or less. This could be achieved through a combination of relative dating by means of typology of ceramics and other materials, and through absolute dating by radiocarbon and perhaps other means. The papers in the special sections on "Rethinking Ceramic Chronologies" in this and the previous issue of *Latin American Antiquity* are good examples of progress toward that goal.

Inomata, Ortiz, Arroyo, and Robinson do an exemplary job of combining numerous radiocarbon determinations from good contexts in different sites with appropriate use of Bayesian statistical methods in order to make a very convincing case that the Middle and Late Preclassic portions of the Kaminaljuyú sequence should be dated about three centuries later than thought, with important implications throughout the entire Maya area and beyond.

Ball's paper is based on well-taken rethinking

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Latin American Antiquity 26(1), 2015, pp. 26–29
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DOI: 10.7183/1045-6635.26.1.26

https://doi.org/10.7183/1045-6635.26.1.26 Published online by Cambridge University Press

of the ceramic sequence at Becán, in the Maya Lowlands, without making any use of absolute dating techniques. It is a very welcome improvement over old "type-variety" and other assumptions.

Huster and Smith begin with complex massaging of abundant ceramic and stratigraphic data from Calixtlahuaca, in the Toluca Valley of Central Mexico. I am a little uneasy about some of their heuristic assumptions, but the results make good sense, and they are an important step forward in getting fine-grained chronologies for the Middle and Late Postclassic in various regions of Central Mexico. Their subsequent Bayesian analyses of a large number of radiocarbon determinations are exemplary and especially notable for their comparisons between treating durations of ceramic phases as independent and treating them as non-overlapping. In this case, suggestions of overlaps are slight, which is reassuring.

Vaughn, Eerkens, Lipo, Sakai, and Schreiber aptly point out that radiocarbon specimens usually do not have a direct association with the event one hopes to date. This is why it is so important to select radiocarbon specimens from really good contexts. Optically Stimulated Luminescence (OSL) readings, in contrast, directly pertain to the event one wishes to date. The authors are wisely cautious in noting that more specimens are desirable. I think OSL is still in a developmental stage, so that is also a reason for some caution. Nevertheless, the authors make a good case that the "Dawson seriation" developed in the 1950s for the Southern Nasca region of Peru is more complicated than originally thought and reflects regional as well as temporal differences. I add that Bayesian methods are just as applicable to OSL dates as to radiocarbon, and they should be applied when a reasonable number of measurements are available.

Korpisaari, Oinonen, and Chacama use new and old radiocarbon dates subjected to Bayesian methods, and some Thermo Luminescence (TL) dates, to make a very convincing case that the Cabusa phase in far northern Chile is largely post-Tiwanaku, as suggested earlier in publications they cite by Sutter and others (on the basis of bioarchaeology) and by Cassman (on the basis of textiles). The fact that some TL dates are too early adds to my feeling that more study of this technique is needed.

Some authors seem unduly concerned with efforts to specify exact beginning and ending dates for ceramic phases. I have no experience with applying Bayesian methods to chronology building, but I suspect that it is some consequence of the software used. Nonetheless, I assume that, in most cases, material culture did not change instantaneously, so what was made and in use in any one year, in any specific place, was only slightly different from what was there the year before or the year after. In such cases, beginning and end dates for phases are arbitrary constructs imposed by our inability to identify exactly what was there on a yearly basis. If so, it seems better to think of phase beginnings and ends as somewhat fuzzy concepts, rather than trying too hard to pin them down to exact dates. For example, if I estimate on the basis of radiocarbon and perhaps other evidence that a particular phase lasted from around A.D. 350 to 550, then what I am most sure of is that it was in full swing around A.D. 450, and what I am least sure of are its exact beginning and ending dates. In some publications, I have done away with horizontal lines between phases, putting phase labels at approximate midpoints of intervals, as I illustrate here in Figure 1 (see Cowgill 2007:264). I think that is the best way to do it. If one succeeds in identifying many short phases, it is not a warrant for adding horizontal lines—it just means the midpoints are closer together. But going lineless has made some colleagues uncomfortable, so in other cases I have inserted horizontal lines, even though I do not think they make much sense.

For the same reason, I am uncomfortable with the concept of "transitional" phases, unless they truly represent cases where change was exceptionally rapid. Indeed, one reason that very precise chronologies are so important is that they enable us to identify these unusual cases because they *point to some kind of drastic social change, the* arrival of influential newcomers, or some other noteworthy event.

Just what is this "Bayes" thing? Elementary statistics books often suggest that a good rule of thumb is that if you get a result that has less than one chance in 20 (5 percent) of happening just by chance, you should accept it as true. Conversely, if it has a greater chance you should dismiss it as just accidental. It is surprising and depressing to see how often people apply this rule blindly with-

https://doi.org/10.7183/1045-6635.26.1.26 Published online by Cambridge University Press

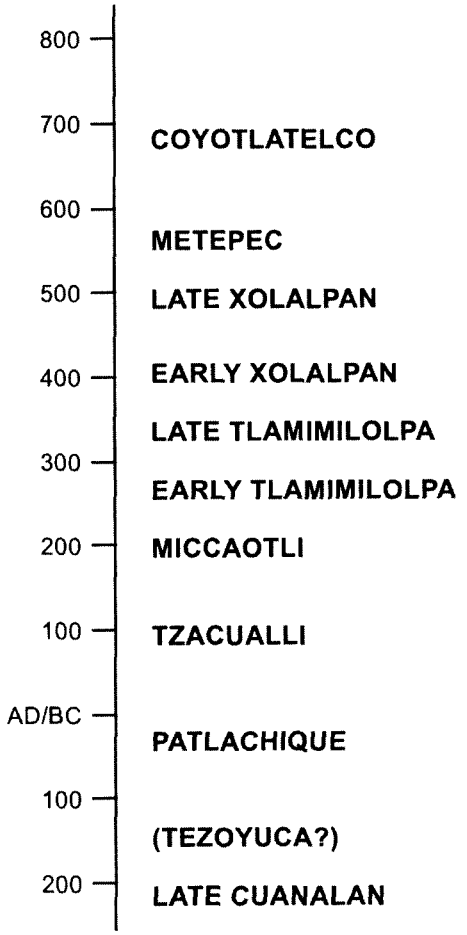


Figure 1. The ceramic chronology of Teotihuacan, without phase boundaries.

out thinking about it. Imagine an experiment in which one person, in a closed room and with no chance of cheating, draws cards from a well-shuffled deck. Suppose that a second person in another room guesses the cards correctly more often than would be expected five percent of the time by mere chance. That is, the "p" value is somewhat less than .05—less than one chance in 20 of happening by accident. How many people will really feel comfortable about concluding that this establishes extrasensory perception beyond reasonable doubt? I think most people are so skeptical of ESP that it will take a lot more than this to change their mind. Even if it were one chance in a thousand, many people would not be convinced. Thus, there is a common and sensible tendency to feel that an improbable result needs extra strong evidence in order to be taken seriously, no matter what the textbooks say. But most elementary texts leave you adrift in thinking about just how much an improbable finding should change your mind. If you do not accept in rote fashion what the textbook says, you are left changing your opinion on the basis of some vague intuition. This makes you what I like to call a "folk Bayesian."

What Bayes' theorem does is to enable you to make good use of sound statistical theory to go beyond this vagueness and get a much better-founded notion of how much your prior beliefs, in combination with the new evidence, should lead you to modify your beliefs. These revised beliefs are called "posterior."

I will not try to explain the math here. Many introductory statistics texts continue to ignore Bayesian methods or are even somewhat skeptical of them. The theorem itself is not controversial, but until recently many applications were too computationally demanding to be practical. Also, like any other technique, the methods can be misused, and they are not a sovereign remedy for all problems. For example, they are not very useful if one's prior information is too vague ("uninformative") or erroneous (invalid). Traditional methods are still useful for description and sometimes useful for inference.

In the special case of Bayesian approaches to archaeological chronology, it is often reasonable to assume that the proportion of intrusive later material in a layer is so small that it can be ignored, and prior information includes the assumption that nothing in a layer is later than anything in higher layers. That is helpful even if the possibility of a significant proportion of redeposited earlier material or heirlooms in a layer cannot be ruled out. Where redeposition can also be considered negligible, the prior information is even more informative.

Unfortunately, I know of no elementary book that describes the Bayesian approach, though they may exist. Bayesian statistics have a solid mathematical and logical foundation, but, like all other methods, they can be misused. Sharon McGrayne (2011) gives a lively journalistic account of their history and the personalities involved, worth reading for background, but a bit overenthusiastic and

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with none of the math needed to actually carry out Bayesian analyses. Bradley Efron (2013), himself a major player in the field, gives a more sober assessment, noting that, if priors are not very informative, Bayesian methods are not especially helpful, and more traditional methods still have their place. Alex Bayliss (2009) shows what is possible with really numerous measurements and has wise advice about radiocarbon dating in general. Robert D. Drennan (2009) is, in most respects, an excellent first semester text, but it does not mention Bayes. Stephen Shennan (1997:48), whose book is also excellent, finds the Bayesian approach attractive in philosophical terms but hard to apply practically. He does not describe Bayesian methods. Some applications really are not all that difficult, and, since 1997, growing computer power has made the approach far more practical. Iversen (1984) is somewhat advanced and only covers a narrow range of applications. Kruschke (2011) is not for beginners but covers a wide variety of applications, explains things very clearly, and provides lots of practical software.

Bayesian analyses of sets of radiocarbon dates have become popular because they can be done by computer packages whose inner workings may be quite mysterious to the user. But Bayesian methods are applicable to a far wider range of topics, and so far archaeologists have made little use of these possibilities. One exception is Robertson (1999), who used Bayesian methods to analyze small ceramic collections and identify spatial patterns of socioeconomic status in Teotihuacan. Ortman et al. (2007) carry these methods still further.

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https://doi.org/10.7183/1045-6635.26.1.26 Published online by Cambridge University Press