comments

ON MATHEMATICAL MODELS OF THE CLASSIC MAYA COLLAPSE: THE CLASS CONFLICT HYPOTHESIS REEXAMINED

John W. G. Lowe

Recently, Hamblin and Pitcher (1980) have attempted to buttress the class conflict explanation of the Classic Maya collapse using a series of mathematical models. However, despite the exceptionally good agreement between these mathematical relations and the empirical data, the same cannot be said for the fit between the conceptual and mathematical models. The relations employed are very general, so much so as to often be isomorphic with very different processes. In one case at least, the same model appears to be consistent with several entirely distinct explanations of the collapse, and other mathematical readings of the monument data are by no means precluded. While these particular mathematical relationships fail to make a very strong case that the Classic Maya collapse was engendered primarily through peasant revolt and class conflict, which was very possibly Hamblin's and Pitcher's underlying aim, the attempt to cast explanations of the collapse into mathematical form points the way for the next generation of collapse models.

Hamblin and Pitcher (1980) in a recent article have generated a model of the Classic Maya collapse that makes quantitative predictions and achieves a spectacularly good fit with the data. Hitherto, the class conflict or peasant rebellion hypothesis has been a perennial favorite of Mayanists seeking to explain the demise of Classic Maya civilization in the southern Lowlands, having been espoused at one time by Kidder (1950), Altschuler (1958), Kaplan (1963), Erasmus (1965), and most recently by Thompson (1966, 1970) and Sharer (1977). Unfortunately, many of these rebellion theories have been rather vague, often couched in a kind of poetic ambiguity, and as a result, difficult to test.

Hamblin and Pitcher have translated the class conflict hypothesis into a formal, mathematical model that makes certain quantitative predictions. The major advantage of mathematical models is that implications flow more rigorously and unambiguously from initial assumptions. Thus, such models tend to be more falsifiable (Popper 1959, 1965), and it is only by making explanations of the Maya collapse more testable that we are going to be able to prune away the proliferation of hypotheses that have been and continue to be advanced.

THE DATA BASE

Hamblin and Pitcher are sociologists, not Mayanists, and are perhaps at their best at deriving implications both quantitative and qualitative from existing models. Their brief review of the literature is as incisive as anything written to date. A few mistakes, however, creep into their treatment of the monument data. Several sites are counted twice; both Benque Viejo and Xunantunich are listed, as is Hatzcab Ceel and Tzimin Kax, and likewise, Holactun and Xcalumkin. Chichen Itza's long count date should probably be more properly read as 10.2.9.1.9 (Thompson 1971) than 9.2.10.0.0. Also, Cerro de las Mesas is not a Classic Maya site.

More generally, one might question whether leaning so heavily on this particular facet of the archaeological data base is justified. Certainly, dated monuments are at best imperfect indicators of Classic Maya civilization, especially on the frontiers—Chontalpa, Belize, and the northern Maya Lowlands—where many important sites appear to lack such monuments completely. Also, even at sites with dated stelae, the monument record is sensitive to the intensity of exploration at a site; for example, at both Altar de Sacrificios and Tikal several more monuments were found in the course of multiseason excavation programs.

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A more substantive issue is whether the northern Lowlands should be included, i.e., does the failure to erect dated monuments after a certain date imply collapse there? Probably not; Coba was not abandoned just after 9.12.12.0.5; Oxkintok did not collapse shortly after 9.2.0.0.0, nor apparently did Uxmal at 10.3.0.0.0. And, if events at Santa Rosa Xtampak mirror the situation at the nearby site of Dzibilnocac (Nelson 1973), then a collapse there at 9.15.19.0.0 is most unlikely. Rather, it seems that the failure of the monument cult in the north resulted more from a subsidence of influence from the southern Maya Lowlands, than from local collapse.

The monument record itself is very fragmentary with somewhat less than one-half of the stelae discovered actually legible enough to read a date (Morley 1938). Then, too, there is the problem of distinguishing between dedicatory dates and references to events in the past or future. Differences among authorities abound; for instance, only some 83% of Morley's (1938) "certain" dates are accepted by Proskouriakoff (1950). Consequently, it could be argued that the fit of a model with monument data actually says very little.

But I would reject this view. The fact is, the monument record, for all its imperfections and distortions, constitutes one of the few comparable, quantitative indices, at once available for a large number of sites and providing very fine discrimination in time and space. An absolute chronology datable to a generation or less is rare in archaeology. In addition, few of the "monuments" are likely to be carried long distances; indeed, to my knowledge the only artifact dealt with by the authors that is readily transportable is a jade gorget from Tzibanche dated to 10.4.0.0.0 (A.D. 909). Another example would be a sherd of fine paste ware inscribed with a date of 9.18.9.4.4 from Palenque (Rands 1973), which for some reason the authors fail to utilize.

The dated monument record should, of course, be viewed with great caution as an absolute determinant of events at a particular ceremonial center. Nonetheless, many of the vagaries of discovery and preservation cancel themselves out when the total pattern of monuments in time and space is elicited, rather than particular site histories. For example, the errors mentioned above have no tangible effect on the overall trajectory of sites erecting monuments in the Maya Lowlands. Even the inclusion of sites from the northern Maya Lowlands is not decisive, as these constitute 10% of the total, and the waning of southern influence appears to parallel genuinely deteriorating conditions in the Peten and farther to the south. In addition, the monuments, involving as they do skills of literacy, high levels of stone carving and artistic ability, calendrics, astronomy, and mathematics, are particularly good indicators of occupational specialization and the presence of elite culture—in short, of civilization. To paraphrase what Winston Churchill said about democracy as a form of government, it is not that the monument data are such a perfect representation of the trajectory of Maya civilization, it is just that the alternatives are so much worse.

MODELING THE MAYA COLLAPSE

However, despite the viability of their basic approach and the undeniably excellent fit of the mathematical models to one of the most robust indices of Classic Maya civilization, Hamblin and Pitcher fail to establish the role of class conflict in the Classic Maya collapse. The qualitative evidence cited is amenable to other interpretation. For example, the murals of elite figures standing on, or otherwise subjugating nonelite figures that the authors believe represent the crushing of peasant rebellions, more probably represent intersite warfare and conquest with the defeated chiefs and nobles depicted "defrocked" and thoroughly humbled. Likewise, the "systematic mutilation of elite faces on Classic monuments" (Hamblin and Pitcher 1980:248) could have occurred at any time. Recently, a mural was uncovered in Quintana Roo and the faces were scratched out within 48 hours by natives who believed the countenances represented evil spirits. Alternatively, much of the defacement may simply relate to dynastic upheaval. The geographic pattern of the collapse, the movement from the periphery to the center, is more likely to represent encroaching external pressure than revolt beginning in the provinces. We see little evidence of a "Maya empire" in the Late Classic, and in the apparently decentralized political milieu of the time, the concept of a province seems misplaced.

Similar problems exist for the authors' three mathematical models. The first model is:

$$dY = pY \cdot dX$$

or equivalently, since we are really dealing with rates, increments per unit time, not differentials

$$dY/dt = pY \cdot dX/dt$$

Integrating both sides of the above equation with respect to dt produces:

$$Y = Y_0 e^{pX}$$

Here dY/dt represents the number of ceremonial centers dropping out of the stela-dated monument complex at time t, presumably due to peasant rebellion.

$$Y(t) = \int_0^t dY/dt \cdot dt$$

is equal to the cumulative number of ceremonial centers that had ceased to erect dated monuments by time t. Y_0 and p are constants, that is, parameters to be estimated from the data. The expression $dX/dt = N_t$ is equal to the number of sites still erecting monuments at time t, and

$$X(t) = \int_0^t dX/dt \cdot dt = \sum_{t=0}^{t} N_t$$

represents the cumulative number of sites with dated monuments. Here dX/dt or N_t is supposed to represent an escalation in aversive behavior on the part of the elite. Thus, it is possible to employ a relation the authors derive elsewhere, namely:

> The increment in the aversive behavior of the disadvantaged party (dY) is approximately a constant proportion (p) of the disadvantaged party's total aversive behavior (Y)—or experience accumulated since the beginning of the fight—times the increment in the aversive behavior of the advantaged party (dX), as specified in the following differential equation: $dY = pY \cdot dX$ or $dY/Y = p \cdot dX$ [Hamblin and Pitcher 1980:253–254].

The major difficulty with their model is readily apparent. It is not in the fit of the mathematical model to the empirical data; indeed, it is unusual for such a simple model to fit the data base so well. Rather, the problem here is the fit between the conceptual and mathematical models. The postulated identity between the accumulated number of centers with dated monuments and the total aversive behavior of the elite seems tenuous at best. Neither is it the case that all the apparent failures of ceremonial centers to remain within the dated monument complex is necessarily due to peasant rebellion. Other causes of site demise exist; for example, shifting economic fortunes and political conquest.

It is an overstatement to say, as the authors do, that "to the extent either of these assumptions is incorrect the data should deviate from the predicted pattern" (Hamblin and Pitcher 1980:254). What the authors have really established is not that escalating class conflict underlay the Maya collapse but that the monument data through time can be approximated rather accurately by the relation,

$$Y = Y_0 e^{pX}, \text{ or equivalently, } dY/dt = pY\, dX/dt,\ dX/dt = N_t.$$

This says very little per se, about class conflict. In fact, the same model can be used to support entirely different views of the collapse.

For instance, following Cowgill (1979) it is possible to assume that escalating warfare between ceremonial centers was the root cause of the collapse. It would seem reasonable that

$$dW_i/dt = kW,$$

that the incremental increase in the intensity of warfare at a site, i, is proportional to the intensity of warfare for all sites,

$$W = \sum_{i=1}^{N} W_i.$$

Therefore,

$$dW/dt = \sum_{i=1}^{N} dW_i/dt = \sum_{i=1}^{N} kW = kW \cdot N$$

Also, presumably the probability of failure for a site, p_f, is proportional to the total intensity of warfare:

$$p_f = aW.$$

But,

$$dY/dt = p_f \cdot N,$$

that is, the expected number of failures at time, *t*, is equal to the total number of sites times the probability each will fail. Therefore,

$$dY/dt = p_f \cdot N = aW \cdot N = a/k(kW \cdot N) = a/k \cdot dW/dt$$

Integrating both sides produces

$$Y = a/kW \text{ or } W = k/aY$$

if we assume $W = 0$ when $Y = 0$. Consequently,

$$dY/dt = aW \cdot N = a(k/a\ Y)N = kYN = kY \cdot dX/dt$$

This is of course exactly the same as Hamblin and Pitcher's equation (1) and fits the data just as well. Here Y is a proxy for the degree of escalation in intersite conflict. Also, precisely the same equation holds if we assume site collapse follows from an escalating competition in ceremonial construction (c = the total ceremonial construction is simply substituted for W above).

Alternatively, following Sanders (1973, 1977) and Deevey et al. (1979) one could argue that site failure resulted from ecological damage, and if the greater the cumulative damage, Y, the greater the failure rate, then

$$p_f = k \cdot Y$$

and

$$dY/dt = p_f \cdot N = kY \cdot N = kY \cdot dX/dt$$

as before.

But what of the other two models that Hamblin and Pitcher describe? These too, offer little direct support for the class conflict hypothesis. The second is a "collective learning model":

$$S = S_1A^n$$

Where S = the cumulative number of "successes," in this case remaining within the monument cult more than one hotun.

A = the cumulative number of attempts, or the number of failures plus successes. Here again the fit is apparently quite good with the parametric values $S_1 = .901$, $n = .906$. This equation simply says that the probability of founding a ceremonial center that lasted more than one hotun ($dS/dA = nS_1/A^{1-n} = .816/A^{.094}$) decreased slightly as the cumulative number of attempts increased.

But this relation is so general, that it appears to have little relation to class conflict, and the "collective learning model," $S = .901A^{.906}$ fails to specify exactly who is learning what. Precisely what processes decrease this rather arbitrarily defined success rate is never directly addressed by the mathematics.

This relationship between variables is an exceptionally general one (Hamblin et al. 1973) and is isomorphic to a number of very different processes ranging from saturation curves (e.g., the effect of increasing fertilizer on crop yields) to allometry (for example, the relation between molar size and body weight in apes). In this case, it is likely that the number of "successes" increased less rapidly than the number of attempts as a direct result of an escalating failure rate towards the close of the Late Classic. But this brings us to the third model.

The third, or dialectic model,

$$dX/dt = (r - X)\,ce^{kt}$$

where r, c, and k are constants, is apparently derived by means of mathematical error. Hamblin and Pitcher's equation (5),

$$dX/dt = kX \longrightarrow X = X_0e^{kt}$$

is differentiated to produce

$$dX/dt = k \cdot X_0e^{kt} = qe^{kt}$$

Thus $q = kX_0$ is treated as a constant, but in the next step, q is assumed to be a function of X, producing Hamblin and Pitcher's equation (7), $q = q_0 - cX$. If this is the case,

$$dX/dt = (q_0 - cX)\,e^{kt}$$

and

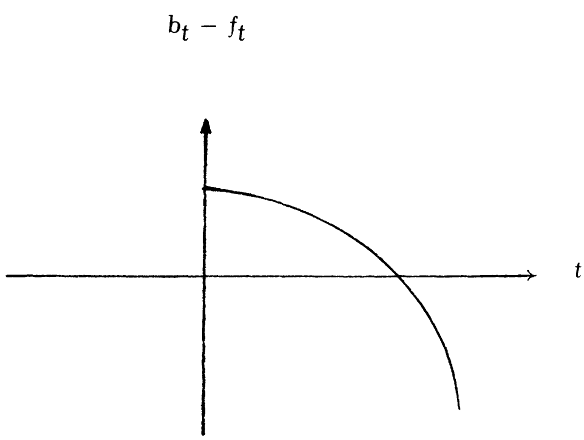
$$X \neq X_0e^{kt}, \; dX/dt \neq kX$$

If $k \cdot X_0$ is not equal to a constant, then their equation (5) cannot be correct, and if kX_0 is equal to a constant, equation (7) is incorrect.

So we are left with a relation, $dX/dt = (r - X)\,ce^{kt}$, whose meaning is somewhat obscure. However, remembering that $N_t = dX/dt$ and differentiating once with respect to t produces:

$$dN_t/dt = (k - ce^{kt})N_t$$

What the third model seems to say is that the birth rate minus the failure rate, $b_t - f_t$, is a function of time, such that:



As this difference becomes highly negative (the failure rate greatly exceeds the birth rate or the rate at which new sites enter the dated monument complex), the population of ceremonial centers rapidly falls to zero, and the collapse occurs.

Lastly, not only are different conceptual models compatible with the above mathematical treatment, but also other mathematical models are consistent with the monument data. For example, a very simple model with some interesting conceptual implications is:

$$b_t - f_t = b_o - f_o$$

equals a constant up to some collapse point, t_c, after which $b_t = b_o$, but f_t "takes off." Let dN_B/dt equal the rate at which new sites begin erecting dated monuments and dN_F/dt equals the rate at which sites cease erecting monuments, then:

$$dN_B/dt = b_o N_t$$

$$dN_F/dt = f_o N_t$$

or integrating both sides so as to fit cumulants

$$N_B = b_o X_t$$

$$N_F = f_o X_t.$$

Figure 1 demonstrates that the assumption b_t equals a constant does approximate the empirical data rather closely ($b_o = .039$, $r^2 = .994$). This is significant because it is possible to show that if the birth rate is approximately constant, then the authors' third model is inconsistent with their first two.

Likewise, Figure 2 suggests that prior to 9.16.0.0.0 (A.D. 751), f_t is very nearly a constant ($f_o = .012$) also, but after that time the probability of failure begins to skyrocket. If, prior to the "collapse point" (c. A.D. 750), $b - f = b_o - f_o$ equals a constant, then

$$dN/dt = (b_o - f_o) N \longrightarrow N = N_o e^{(b_o - f_o)t}$$

It is possible to demonstrate that the number of sites erecting dated monuments does indeed grow approximately exponentially up to that time ($r^2 = .971$) and the value of $b_o - f_o = .026$ (estimated by an SPSS nonlinear regression program) is in close agreement with the above estimated values (.039 − .012). The same data base as Hamblin and Pitcher used was employed except for the corrections mentioned above, including the exclusion of the eight northern sites.

This alternative model is certainly not complete. What is lacking is an explanation of events after the collapse point that presumably underlie the escalating failure rate of ceremonial centers. Yet, in several respects such a reading of the data seems preferable because it avoids several conceptual difficulties associated with the Hamblin and Pitcher thesis.

They must have assumed that communication was terribly slow, that in an area that witnessed the dissemination of calendric forms such as the lunar count from one end to the other in less than a generation (Teeple 1928), it took hundreds of years for rebellion to spread from one site to the next—not an impossible proposition, but certainly an improbable one.

Further, to Hamblin and Pitcher the collapse would appear to have been a predetermined, deterministic process, almost Spenglerian in nature, with the Maya society containing the seeds of its own destruction. Yet, this is difficult to comprehend. If revolutions were as destructive as to result in 90% reduction in total population, it is not at all clear why a behavior so clearly maladaptive could continue and perpetuate itself over a 500–600-year time span. There would have been ample time for the maladaptive, in fact, suicidal, consequences of a peasant revolt to become apparent to all concerned. And if revolutions did not produce the recorded depopulation,

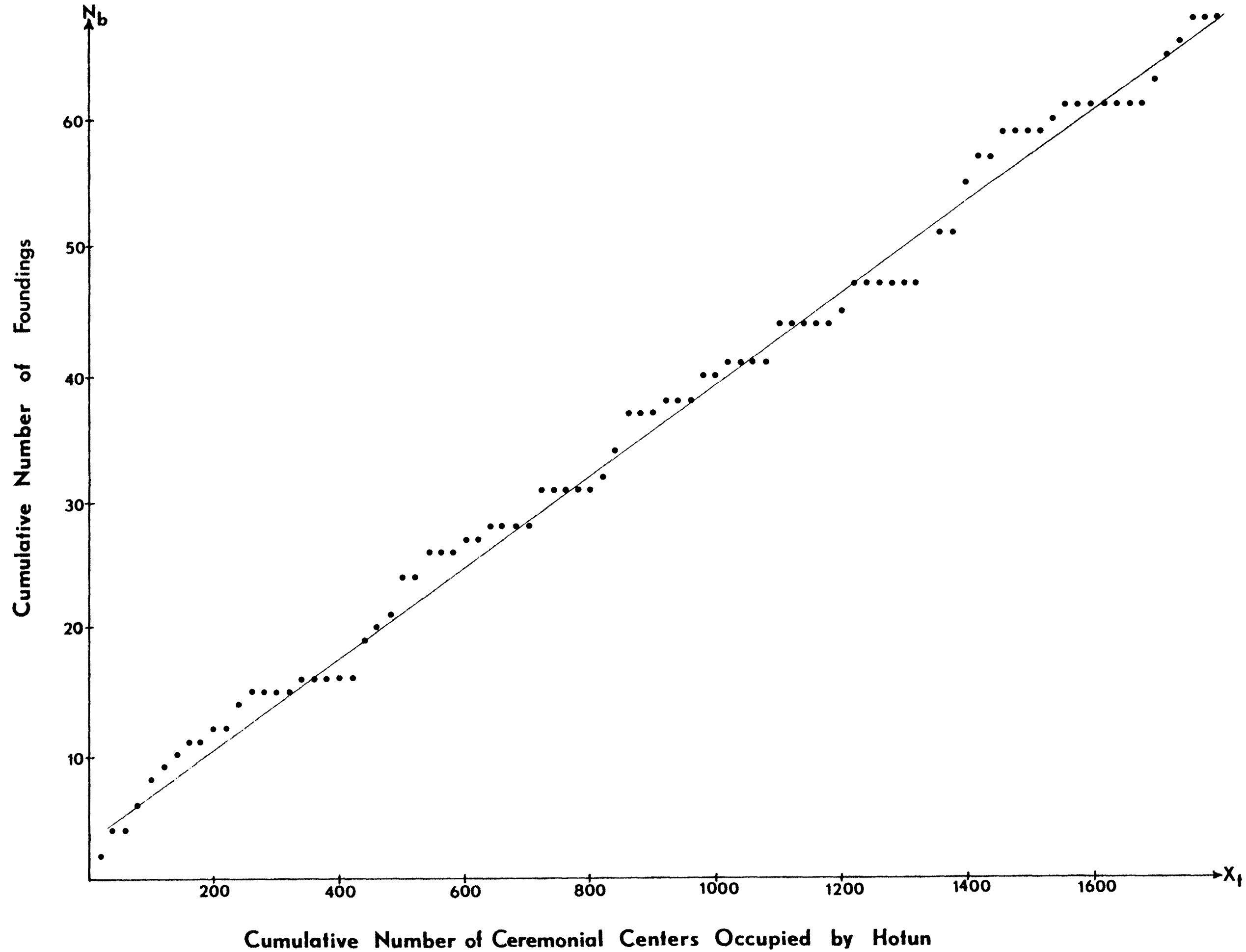


Figure 1. The cumulative number of foundings (of new ceremonial centers with dated monuments) versus the cumulative number of centers occupied, by hotun from 8.12.10.0.0 to 10.4.0.0.0.

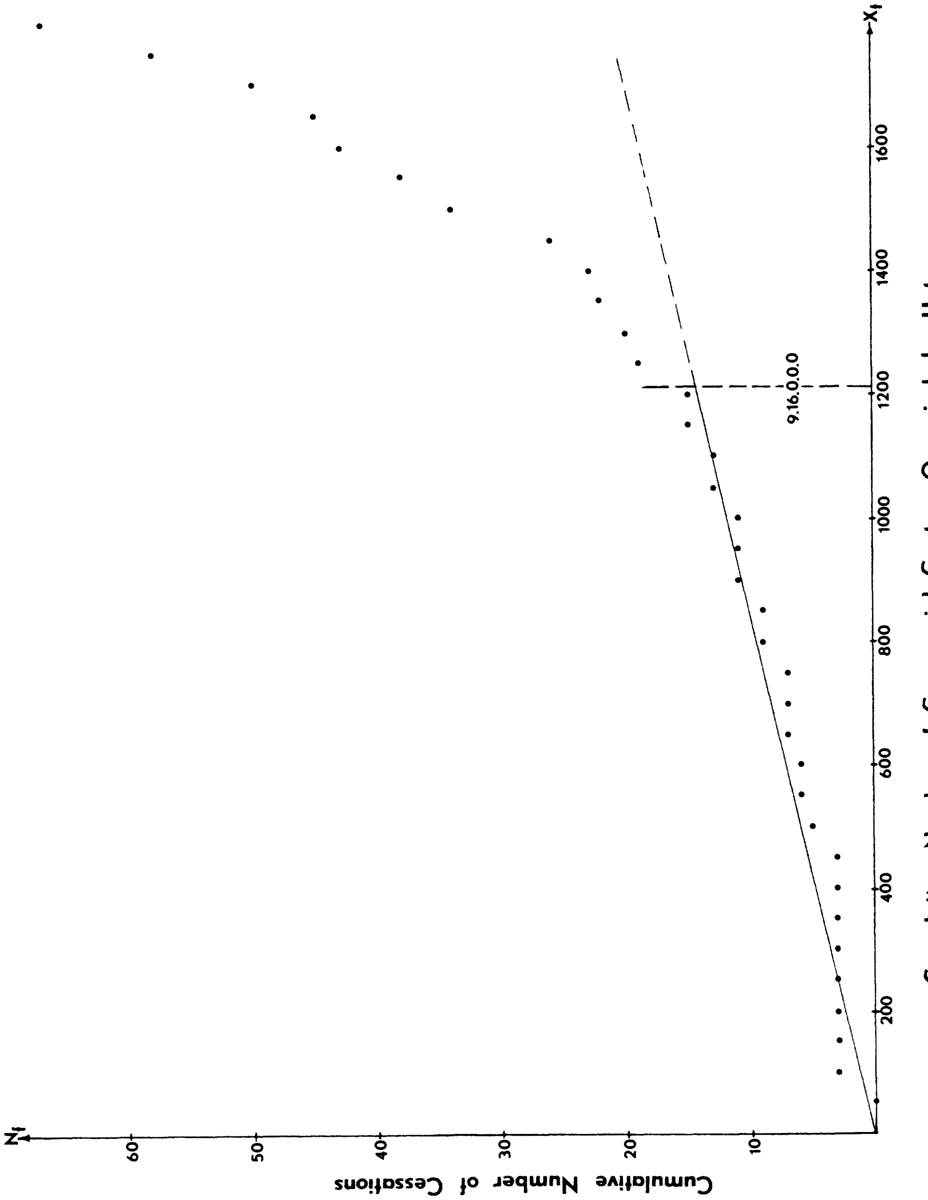


Figure 2. The cumulative number of cessations versus the cumulative number of centers occupied, by hotun from 8.12.10.0.0 to 10.4.0.0.0.

what did? It is, in short, very doubtful that the cataclysmic events of the collapse actually began in the Early Classic and simply gradually gained in intensity over more than half a millennium.

Presumably something propagated chaos and catastrophe between sites, beginning perhaps as early as A.D. 750. It is possible that what was communicated was rebellion. Certainly there are instances where systems of states appear to undergo epidemics of unrest and political upheaval: Europe in the early nineteenth century, coups d'état in Latin America and more recently in Africa. If these processes take on a consistent mathematical form through time (perhaps a Gompertz curve as Pitcher et al. [1978] suggest), then possibly this curve would conform to events in the southern Maya Lowlands in the late eighth and early ninth centuries. If so, then the case for the role of peasant rebellion in the Maya collapse is considerably strengthened.

It is in this sense that Hamblin and Pitcher's contribution to the Classic Maya collapse may be truly profound. As the authors note, "The excellent fits also eliminate alternative substantive interpretations of the data and processes unless they are demonstrated to predict the same functional relationships or unless the empirical patterns are shown to be better represented by other equations" (Hamblin and Pitcher 1980:259). From now on, it is to be hoped that new questions will be asked of any explanation. Can it be cast in mathematical form? Are the implications quantitatively consistent with known bodies of data? What new indices can be devised to test this model? Thus, while Hamblin and Pitcher fail to make as strong a case for the peasant rebellion hypothesis as a cursory examination of their mathematical modeling would suggest, in terms of an underlying method, their work is likely to prove a significant step down an increasingly productive avenue.

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1,000 YEARS OF NEW WORLD ARCHAEOLOGY

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Weather-stranded Norse explorer-refugees are reported in the medieval Icelandic Landnámabók *to have dug into an archaeological site in eastern Greenland about A.D. 981. Enough details are given to suggest that they investigated a Dorset Culture site. This 1,000-year-old account must be the oldest known archaeological report for the New World.*

The earliest recorded excavations in a New World archaeological site took place in East Greenland about 1,000 years ago. These diggings were made by Norse seamen-adventurers Rolf Rødsander and Styrbjorn, two of the predecessors and way-breakers for the more famous renowned Erik the Red and his sons in the exploration of the New World west of Iceland. The exact date of the dig is not known, but since Erik the Red's first trip to Greenland was 3 years before the Greenland colonization usually dated ca. A.D. 985 (Magnusson and Pálsson 1965), the exploration of Rødsander and Styrbjorn in east Greenland must have been a little earlier, in A.D. 981 or 982.

The story of the diggings of Rolf Rødsander and Styrbjorn was related in the *Saga of Snaebjorn Galti*, which regrettably is now lost. Fortunately, however, the outlines of the feat are preserved in the *Landnámabók*, the more or less official account of the settlement of Iceland. While some potentially important detail may be lacking, inclusion in the *Landnámabók* helps to confirm that the Icelanders considered the events as actually having taken place. The Viking and Icelandic sagas in general run the gamut from nearly pure romance to recording traditional oral history (Jansen 1972), but the *Landnámabók* was largely intended to elaborate what is essentially an enormous genealogy of the early settlers of Iceland. Although perhaps containing exaggeration, it was meant to be believed.

The account, as resumed on page 152 of the *Landnámabók* (Boyer 1973:47), goes like this:

Snaebjorn and his foster father Thorodd had gotten into legal difficulties regarding marital problems between his cousin Hallgerd and her husband Hallbjorn. Along with Thorodd's wife and some retainers, they fled Iceland in a ship, thus involving Rolf Rødsander and his boon companion Styrbjorn, since Rødsander was the co-owner, with Snaebjorn, of the ship.

They fled toward eastern Greenland to the offshore Gunnbjorn Skerries, the presence of which had been reported earlier, by ca. 900, by one Gunnbjorn Ulffson, another westward predecessor of Erik the Red. The Snaebjorn-Rolf Rødsander party found the skerries on Greenland's generally

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