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The Concept of Waste in an Evolutionary Archaeology

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The concept of waste as an explanation for cultural elaboration was presented as an effort to demonstrate the power of evolutionary theory in archaeology. The notion itself, however, was originally conceived while teaching the prehistory of eastern North America. I review the historical roots of the idea and relate the waste notion to "competing" hypotheses appearing under the rubric of evolutionary theory. © 1999 Academic Press

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The editors asked me to relate the development of the concept of waste as it is used in this issue and so to contextualize the contribution of the other papers. I am able to do so because, unlike so many usages in archaeology (Lyman et al. 1997), the creation of the waste concept was a conscious, purposeful event, not a gradual sort of linguistic emergence. Like Kroeber's (1916) splendid little exegesis of seriation method, it is possible to say how and why the waste concept came into being and do so definitively. In constructing this account, it is hoped that the unending semantic debates that periodically plague other archaeological concepts [see Dunnell (in press) for an effort to unravel such discourse surrounding the term "style"] can be avoided. To a certain extent, this seems to have been the case, as the critical papers that have appeared (e.g., Boone 1997; Neiman 1998) have not been about what waste "really" is but about legitimate theoretical issues. This is not to concur with those critiques, and I return to them briefly later on, but simply to point out their constructive character.

I introduced the notion waste in a paper published in 1989 (Dunnell 1989). The idea, though it has dominated the influence of the paper to date, was offered there just as an illustration of how evolutionary theory might be able to solve persistent archaeological conundrums and as a way to show that the use of evolutionary theory was not "just another way to say the same old things," as one of my former colleagues used to say, but actually to lead to new insights. Waste was not the focus of the paper. Consequently, the idea was not developed fully or systematically.

Like most new ideas in my experience, the notion now identified as "waste" arose within the context of teaching, putting the lie to the dichotomization of research and teaching so widely used by administrators and legislatures unfamiliar with how the academic enterprise actually works. Although not published until 1989, I had been using the idea in the classroom for 10 years or so by then.[1,]* In this case the impetus lay neither in a theory course nor in a course that treated evolution, but in a course on North American, later eastern North American, archaeology. When I began teaching this course, the "new archae-

* See Notes section at end of article for all footnotes.

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ology" of the 1960s was well underway but in practice the culture historical paradigm was still very much in use (*sensu* Dunnell 1971, 1986). For the eastern United States a new text, *The Archaeology of the Americas, Volume 1, North America* (Willey 1966), had just been published and good, comprehensive, and well-written summaries of eastern archaeology had appeared (e.g., Griffin 1967). All were culture historical in outlook, save Caldwell's innovative and controversial *Trend and Tradition in Eastern North America* (1958),[2] and focused on chronology with modest stories about the contents of the boxes (phases, foci, cultures) thus created by periodization (Dunnell 1986). While the boxes were sound for the most part, i.e., they subtended contiguous blocks of time and space (e.g., Dunnell 1971; Lyman et al. 1997), the new archaeologists had raised questions about the validity of the stories told about the boxes, or lack thereof (e.g., Binford 1967; Taylor 1948; Watson, LeBlanc, and Redman 1971), as well as the mechanisms used to account for them (e.g., Binford 1968; Flannery 1967; Taylor 1948). In short, the appearance of the new archaeology had created something of a schism between theory and culture history (see Dunnell 1986 for a more detailed discussion). Not only did my students find the "what–when–where" stuff of culture history boring but, imbued with the ideals of the "revolution," so did I.

Explaining why things happened as they did was an obvious solution to both dilemmas. Thus began what has proved to be a lifelong quest for the tools that would make the realization of this goal possible. While my goals were strictly new archaeological in derivation, most especially the commitment to using science as the explanatory system, it did not take long for me to realize that the processualist approach had been sidetracked by its view of science and its concomitant insensitivity to issues of unit definition (language of observation). Further, despite the rhetoric, the new archaeology had not developed any means for relating the big picture that had been the bread and butter of the culture historians; the culture historians, for their part, not only failed to explain their constructions but seemed to lack interest in doing so. In short there were no ready models with which I could construct the kind of course I wanted to teach and, I suspected, would satisfy the student interest in "relevance."

My initial attempts fell along ecological lines, trying to find laws and/or generalizations in the mode of the new archaeology (e.g., Dunnell 1972), first as covering laws in the fashion of Watson et al. (1971), later using energy as a currency as in biology. The results were only partly gratifying. I could construct adaptive scenarios with ease but they failed on two counts—they could not be tested [and therefore did not meet my science criterion (Popper 1963)] and they could not account for change without relying upon an entirely reductive approach [i.e., environmental change (Kushner 1970)]. In retrospect, I began to reinvent the wheel; in this case the wheel turned out to be evolution. I, like virtually everyone else coming from an anthropological background, had taken "evolution" to be the kind of model forwarded by White (1949) and others (e.g., Sahlins and Service 1960; Binford 1968). Hence, too, my initial use of energy as the currency of discourse. But I early came to realize that cultural evolution, as it has since been named, suffered from the inability to subject its hypotheses to empirical testing and the inability to explain change. No matter how one tried, this tack always resulted in circularity. The conclusions were also the premises (Dunnell 1980, 1988). So my real discovery here was that there were two "evolutions," cultural and scientific, and that the latter was, as Blute (1979) put it, "an untried theory" in the social sciences. In making this transition I was much assisted by interaction with zoologist Richard Alexander and later R. C. Lewontin, as described elsewhere (Dunnell 1996). Reproductive success, not energy, became the currency and was measured in terms of individual fitness.

In the cultural evolution framework the Woodland "climax," marked by an elaborate mortuary florescence, was an enigma, being followed as it was by the prosaic Late Woodland cultures who decorated virtually nothing let alone supported a mortuary cult. So in the generally progressive development of culture assumed by cultural evolution, there was a serious bump, a "devolution," to be explained. The details of these efforts are treated elsewhere in this volume (Dunnell and Greenlee, this issue). For culture historians, however, there was no issue. Cultures were created and placed in chronological order. What one might say about the cultures beyond their time–space coordinates was largely descriptive convention unfettered by theoretical expectations. So as a teacher I was faced with ad hoc, untestable "explanations" à la cultural evolution or no explanation at all.

But on the face of it, scientific evolution was just as frustrated by the Woodland climax. Material elaborations such as represented by the Woodland mortuary complexes plainly had costs. Yet it was hard to see how they contributed to fitness, to reproductive success. Getting the "facts" straight was an important step. Critical examination of the data available at the time (e.g., Caldwell 1958; Cleland 1966; Dunnell 1967) failed to yield any evidence of Woodland maize-based agriculture, the supposed engine of the climax through the generation of a "surplus." Positing "horticulture" (e.g., Willey 1966), perhaps utilizing local plants, did little to remedy the bind created by those data. Furthermore, the discovery of villages, assumed to be the *sin qua non* of agricultural subsistence and settlement were, after 100 years of serious investigation, proving elusive at best. It has only been recently that the nature of the subsistence associated with the Woodland climax has become clear, at least in general outline (e.g., Bender et al. 1981; Prufer 1997). Progressive assumptions had created "facts" in their own image. Instead of villages, towns, or cities, Woodland settlement was dispersed. Instead of a specialized subsistence system focused on farming, subsistence was, as Caldwell (1958) and others (Cleland 1966; Dunnell 1967, 1972) had argued, more nearly like that of the Archaic precursors, a generalized (diffuse) system in which agriculture was but one technology producing a modest yield.

The essential issue thus boiled down to: Are there any conditions in a Darwinian understanding of evolution in which a decrease in reproductive success in the short run would enhance fitness? My answer was yes. There is *at least* one set of conditions under which an increase in fitness would obtain by diverting energy from reproduction. Since my focus was on explaining the appearance of the mortuary elaboration, I called the manifestation of this diversion of energy "waste," defining it as the use of energy for something other than reproduction (not survival, as developed below) (Dunnell 1989).[3]

The reasoning was simple. First, it was evident that virtually all higher animals spend time, often much time, engaging in activities that do not lead to greater fecundity (e.g., playing, sleeping) but that these activities are abandoned under stress. Engaging in nonreproductiove behavior thus has two effects: (1) it lowers population size directly through lower fecundity and (2) it provides a sink of "excess" time and resources that can be devoted to subsistence/reproduction under stressful conditions. Under normal conditions, individuals or populations that produce the largest number of young (both culturally and biologically) will pass on the codes (either genetic or cultural) for constructing new individuals or populations most successfully. This means that populations will tend to approach carrying capacity; how closely being a function of variability in the near-term carrying capacity; direct storage (income averaging), and indirect storage (waste, partly). One easily deduced consequence of this line of reasoning is that when environmental perturbations that adversely affect the carrying capacity for a particular set of people are on a large scale and unpredictable or have such a long period of recurrence as to render them so at the human scale, populations near carrying capacity would be catastrophically eliminated. Any populations with large amounts of waste would suddenly find themselves at a distinct advantage. They would have a smaller populations and thus lower resource requirements as well as a reservoir of time to intensify subsistence. Thus given variability in waste/reproductive energy allocations among individuals or populations of individuals, waste would be fixed in highly unpredictable and variable environments. In other conditions, though not necessarily all other conditions, individuals and/or populations maximizing number of offspring would most often become fixed at the expense of wasters.

A quick review of some relevant biological literature provides the grounding for the waste concept. The core of the hypothesis requires that nonreproductive uses of energy are at the expense of fitness. This idea underlies the so-called "disposable soma" hypothesis about the evolution of aging (e.g., Kirkwood 1977). Its applicability has been demonstrated not only for nonhuman species (e.g., Kirkwood and Rose 1990; Zwaan et al. 1995) but also recently in an ingenious analysis (Westendorp and Kirkwood 1998; see also Promislow 1998) for *Homo sapiens*. There are, of course, other noncompeting hypotheses to account for aging (e.g., "antagonistic pleiotropy" Promislow 1998), but the phylogentically widespread occurrence of sacrificing survival for reproduction shows the importance of energy allocation. The "disposable soma" hypothesis is straightforward: ". . . any investment in reproduction diverts resources away from the maintenance and repair of cells, with aging as a result" (Promislow 1998:719). The converse is the point here: any investment in maintenance and repair, i.e., nonreproductive activity, *necessarily* diverts resources away from reproduction, the central idea underlying the waste hypothesis.

The tests I proposed in 1989 required a link between environmental variability and reproduction. Resources link these variables in empirical populations. White (1993) has summarized much data to show that resource constraints, especially for subadults, are responsible for numbers of animals generally and has argued that it is a shortage of protein and nitrogen, rather than calories per se that is limiting. This is an empirical generalization to be sure, but it is clear that resources, either through environmental change, competition, or predation (Endler 1986; Hoffman and Parsons 1997; Strong et al. 1984), play the central role in population size and range boundaries. What the waste hypothesis does is provide a *mechanism* that explains some correlations between the occurrence of cultural elaborations identified as waste and variation in the environment. The "cause" of waste is natural selection acting in the usual fashion in somewhat unusual circumstances.

For archaeologists aspiring to science, one reason to prefer such an approach is the empirical testability of evolutionary hypotheses. It is important to realize, however, that evolution is stochastic, not deterministic; this has a large impact on test requirements. For example, one can deduce that waste is most likely to appear and attain relatively high levels at the edges of distributions in space and when environmental change results in a lower carrying capacity (Dunnell 1989). Resources determine boundaries (Hoffmann and Parsons 1997). Therefore, waste will be fixed more frequently and in higher amounts at peripheries where resources are less abundant and predictable by virtue of resource abundance itself and/or competition for resources. In similar fashion, an overall decrease in available resources will favor wasting populations and individuals, at least until such time as a new equilibrium has been established. If archaeology were essentialist, one might well think of falsification as a case which meets any of these conditions but in which there is no obvious waste or vice versa. But since evolution is probabilistic, it is the distribution of results, not single cases, that are needed to falsify a hypothesis. In practical terms, there is no guarantee that waste will appear when "needed," especially when it will have lowered the fitness of its transmitters in most if not all other situations. Human populations become extinct; individuals die without issue.

Further complications are introduced by the mode of description. To identify selection, an appropriate description takes the form of correlation. If there are, as there is every reason to suppose, other conditions under which waste enhances fitness, then poor correlations or no correlations at all would be found if waste fixation is treated as the independent variable and carrying capacity as the dependent variable. Likewise if carrying capacity were to be the independent variable and there is more than one way to fix waste, then correlations may be poor as well. In short, evolutionary explanations are not tested like one might test Boyle's Law. These are the complications that produce the iterative procedure of theory building/fact explaining that Lewontin (1974) used to characterize the biological enterprise and why evolutionary explanations are compelled to embed history.

This is a point where the arguments of Boone (1997) and Neiman (1998) seem to err. Their proposal that material elaboration is generated as "advertising" is seemingly presented as an *alternative* to the waste hypothesis, when, in point of fact, there is nothing about either hypothesis that precludes the other. In one case, advertising may be the explanation; in others it may be waste. In still others, both mechanisms, and others not yet detected, may be at work. While such advertising has been posited to explain such diverse phenomena as peacock feathers and deer antlers (e.g., Gould 1985:43) it has not been universally accepted because of the difficulty of testing it. Currently, at least, it is the archetype of the ad hoc functional argument against which Gould and Lewontin (1979) warned so strongly.

Another point raised by Neiman (1998: 286) is that the waste hypothesis implies "group selection." Group selection is no longer the bogey man it once was (Wilson 1973; Wilson and Sober 1994). Even so, there is nothing about waste that requires group selection. The scale at which selection (sometimes discussed as the scale of the individual) takes place is an empirical matter, to be determined case by case (e.g., Lewontin 1970; Vrba and Gould 1986). There is nothing about the waste notion that compels it to be a group property. It could just as easily be a property of individual organisms. Indeed, as Greenlee and I try to show elsewhere in this volume, the two can already be distinguished in the archaeological record. If there is continued access to skeletal materials (the only data we generally get on individuals) other techniques may provide even better information on how traits come to spread in populations (e.g., Sokal et al. 1991). But for the most part tests have to deal with *aggregate* data that constitute the vast bulk of the archaeological record (Binford 1981). Explaining aggregate data does not, however, imply that selection was operating at, or only at, a scale set by formation conditions of the archaeological record. Much archaeological explanation using evolution will have to explain population averages as the effects of selection and drift acting on individual organisms as well as groups of organisms of various scales.

The articles presented in this collection represent the normal development of science. Whereas my initial insight came in an empirical context, articles like Madsen et al. (this issue) tackle the issues raised by waste from a theoretical perspective. One consequence may be the broadening of the waste explanation beyond the context in which I first envisioned it to resource variability more generally and without the mechanism of catastrophic population collapse. Another, more certain, consequence is the deduction of new ways to test waste hypotheses, the implications for differing age structures of wasting/nonwasting populations (see also Hoffmann and Parsons 1997:36–37) being one. Indeed, Sterling's article (this issue) exploits this derivation in the context of monumental architecture in Egypt. The asymmetry of the appearance and spread of waste and its disappearance (Hoffmann and Parsons 1997:223–227) is another entirely independent set of tests only partly exploited here by Greenlee and me (this issue). Undoubtedly others will be invented.

I've often been asked why I did not publish the waste notion much earlier. Not only did I not rush to publish the idea, but, truthfully it never occurred to me to publish it at all. The argument is nothing not already contained in existing theory. The only novel elements were naming the manifestation of a particular kind of energy sink "waste" so as to link archaeological description with evolutionary theory and deducing some probabilistic tests of its utility. There is no "waste theory" or "waste model." New insights gained through evolutionary theory can be seductive in their own right, but one must be careful not to reify minor steps along the route to realizing the full potential of the theory.

The lesson in these articles is, however, that evolutionary hypotheses are empirically testable, do lead to novel results, and can be expected to produce cumulative knowledge as they have in other disciplines. There is a real danger that evolution will be coopted by social scientists as a means of preserving the traditional and highly ethnocentric view we take of human beings by "reinterpreting," conflating, and diluting evolution. The articles here show evolutionary theory at work and illustrate the novel insights and exciting achievements that result.

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NOTES

[1] I first began using this concept in graduate seminars, then in my undergraduate course "Eastern North America" around 1983. Consequently, its subsequent refinement owes much to the questions and probings of students rendered anonymous by the passage of time and failing memory, but that are nonetheless appreciated.

[2] I came to the University of Washington in 1967 and met Alex Krieger for the very first time then. Knowing my eastern interests, his first words to me were "Well, Dunnell, what do you think of Caldwell?" Both the tone and facial expression clearly pointed away from a positive response. His battles with Ford would not come up for weeks.

[3] This particular term seemed to capture the essence of the evolutionary idea of "waste" of reproductive capacity. Isbell (1978) had used "waste" in a similar way but without the evolutionary rationalization and probably chose the name for similar rea-

sons. Bill Dancey once related a story that bears retelling in this context. As an editor Dancey had noticed that one of his authors seemed to be reinventing notions that I had developed first in *Systematics in Prehistory* (Dunnell 1971) and suggested that the author read and cite the book. The response was "No—I did not read it and created my ideas without it, so I will not cite it," an amazing excuse for poor scholarship and not being familiar with the literature. While I do not recall that Isbell's idea had any role in my development of the waste hypothesis, I had certainly read the paper and that reading might well have suggested the terminology if not the idea.

REFERENCES CITED

Bender, M. M., D. A. Baerreis, and R. L. Steventon
1981 Further light on carbon isotopes and Hopewell agriculture. *American Antiquity* **46:** 346–353.

Binford, L. R.
1967 Smudge pits and hide-smoking: The use of analogy in archaeological reasoning. *American Antiquity* **32:**1–12.
1968 Some comments on historical versus processual archaeology. *Southwestern Journal of Anthropology* **24:**267–275.
1981 Behavioral archaeology and the Pompeii premise. *Journal of Anthropological Research* **37:**195–208.

Blute, M.
1979 Sociocultural evolution: An untried theory. *Behavioral Science* **24:**46–59.

Boone, J. L.
1997 *Wasteful behavior as status reinforcement.* Paper presented at the 62nd annual meeting of the Society for American Archaeology, March, 1997, Nashville.

Caldwell, J. R.
1958 *Trend and tradition in the prehistory of the eastern United States.* American Anthropological Association, Memoir 88, Menasha.

Cleland, C. E.
1966 *Prehistoric animal ecology and ethnozoology of the upper Great Lakes region.* Anthropological Papers of the Museum of Anthropology, University of Michigan, No. 29, Ann Arbor.

Dunnell, R. C.
1967 *Prehistory of Fishtrap Kentucky: Archaeological interpretation in marginal areas.* Dissertation, Department of Anthropology, Yale University. University Microfilms, Ann Arbor.
1971 *Systematics in prehistory*. Free Press, New York.
1972 *Prehistory of Fishtrap Kentucky*. Yale University, Publications in Anthropology, No. 75, New Haven.
1980 Evolutionary theory and archaeology. *Advances in Archaeological Method and Theory* **3**:35–98.
1986 Five decades of American archaeology. In *American archaeology: Past and future,* edited by D. J. Meltzer, D. Fowler, and J. A. Sabloff, pp. 23–49. Smithsonian Institution Press, Washington.
1988 The concept of progress in cultural evolution. In *Evolutionary progress?,* edited by M. H. Nitecki, pp. 169–194. Univeristy of Chicago Press, Chicago.
1989 Aspects of the application of evolutionary theory in archaeology. In *Archaeological thought in America,* edited by C. C. Lamberg-Karlovsky, pp. 35–49. Cambridge University Press, Cambridge.
1996 Foreword. In *Evolutionary archaeology. Theory and application,* edited by M. J. O'Brien, pp. vii–xii. University of Utah Press, Salt Lake City.
in press Style: Development of a concept. In *Style in oceania,* edited by M. W. Graves and E. E. Cochrane.

Endler, J.A.
1986 *Natural selection in the wild.* Princeton University Press, Princeton.

Flannery, K. V.
1967 Culture history v. culture process: A debate in American archaeology. *Scientific American* **217**(2):119–122.

Gould, S. J.
1985 *The flamingo's smile: Reflections in natural history.* Norton, New York.

Gould, S. J., and R. C. Lewontin
1979 The spandrels of San Marco and the Panglossian paradigm: A critique of the adaptationist programme. *Proceedings of the Royal Society, London (B)* **205:**581–598.

Griffin, J. B.
1967 Eastern North American archaeology: A summary. *Science* **156:**175–191.

Hoffmann, A. A., and P. A. Parsons
1997 *Extreme environmental change end Evolution.* Cambridge University Press, Cambridge.

Isbell, W. H.
1978 Environmental perturbations and the origin of the Andean state. In *Social archaeology: Beyond subsistence and dating,* edited by C. L. Redman, M. J. Berman, E. V. Curtin, W. T. Langhorn, Jr., N. M. Versaggi, and J. C. Wanser, pp. 303–327. Academic Press, New York.

Kirkwood, T. B. L.
1977 Evolution of ageing. *Nature* **270**:301–304.

Kirkwood, T. B. L., and M. R. Rose
1988 Survival sacrificed for reproduction. *Philosophical Transactions, Royal Society of London B* **332**:15–24.

Kroeber, A. L.
1916 *Zuni potsherds*. Anthropological Papers of the American Museum of Natural History 18(1), New York.

Kushner, G.
1970 A consideration of some processual designs for archaeology as anthropology. *American Antiquity* **35**:125–132.

Lewontin, R. C.
1970 The units of selection. *Annual Review of Ecology and Systematics* **1**:1–18.
1974 *The genetic basis of evolutionary change*. University of Columbia Press, New York.

Lyman, R. L., M. J. O'Brien, and R. C. Dunnell
1977 *The rise of culture history*. Plenum, New York.

Neiman, F. D.
1998 Conspicuous consumption as wasteful advertising: A Darwinian perspective on spatial patterns in Classic Maya terminal monument dates. In *Rediscovering Darwin: Evolutionary theory and archeological explanation*, edited by C. M. Barton and G. A. Clark, pp. 267–290. Archeological Papers of the American Anthropological Association, No. 7, Arlington.

Popper, K.
1963 *The poverty of historicism*. Routledge and Kegan Paul, London.

Promislow, Daniel E.L.
1998 Longevity and the barren aristocrat. *Nature* **396**:719–720.

Prufer, O. H.
1997 How to construct a model: A personal memoir. In *Ohio Hopewell community organization*, edited by W. S. Dancey and P. J. Pacheco, pp. 105–128. Kent State University Press, Kent.

Sahlins, M. D., and E. R. Service
1960 *Evolution and culture*. University of Michigan Press, Ann Arbor.

Sokal, R. R., N. L. Oden, and C. Wilson
1991 Genetic evidence for the spread of agriculture in Europe by demic diffusion. *Nature* **351**:143–145.

Strong, D. R., J. H. Lawton, and Sir Richard Southwood
1984 *Insects on plants, community patterns and mechanisms*. Harvard University Press, Cambridge.

Taylor, W. B.
1948 *A Study of archeology*. American Anthropological Association, Memoir No. 69, Menasha.

Vrba, E. S., and S. J. Gould
1986 The hierarchical expansion of sorting and selection: Sorting and selection cannot be equated. *Paleobiology* **12**:217–228.

Watson, P. J., S. A. LeBlanc, and C. L. Redman
1971 *Explanation in archeology: An explicitly scientific approach*. Columbia University Press, New York.

Westendorp, R. G. J., and T. B. L. Kirkwood
1998 Human longevity at the cost of reproductive success. *Nature* **396**:743–746.

White, L. A
1949 *The evolution of culture*. McGraw–Hill, New York.

White, T. C. R.
1993 *The inadequate environment: Nitrogen and the abundance of animals*. Springer-Verlag, Berlin.

Willey, G. R.
1966 *An introduction to American archaeology, volume 1, North America*. Prentice-Hall, Englewood Cliffs.

Wilson, D. S., and E. Sober
1994 Reintroducing group selection to the human behavioral sciences. *Brain and Behavioral Sciences* **17**:585–609.

Wilson, E. O.
1973 Group selection and its significance for ecology. *Bioscience* **23**:631–638.

Zwaan, B. J., R. Bijsma, and R. F. Hoekstra
1995 Direct selection of lifespan in *Drosophila melanogaster*. *Evolution* **49**:649–659.