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The Concept of Equifinality in Taphonomy

R. Lee Lyman*

Department of Anthropology, 107 Swallow Hall, University of Missouri-Columbia Columbia, Missouri, 65211, USA

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The term "equifinality" was coined by Ludwig von Bertalanffy as he worked to develop general system theory. In 1949 he defined equifinality as reaching the "same final state from different initial states" in an open system, one capable of "exchanging materials with its environment." Taphonomists have typically defined equifinality as reaching the same final state from different initial conditions and in different ways, without consideration of whether a system was open or closed. Natural historical processes involving organic tissues comprise open systems. Whether two alternate taphonomic hypotheses can be distinguished or not can be construed as a problem of taphonomic equifinality, or it can be construed as a problem of statistical indistinguishability. For both, the epistemological problem reduces to one of classification. The production of much greater knowledge and understanding of the many reasons (cause–effect relations) why skeletal part frequencies vary has resulted from use of the equifinality concept because that concept demands innovative analyses of previously unimagined variables.

Keywords: CLASSIFICATION, CLOSED SYSTEM, EQUIFINALITY, LUDWIG VON BERTALANFFY, OPEN SYSTEM, QUANTIFICATION

Introduction

The term *equifinality* became commonplace in the literature on zooarchaeological taphonomy in the middle 1980s. The earliest use of the term *equifinality* in the general archaeological literature of which I am aware was by Frank Hole & Robert Heizer in 1969 who cited Ludwig von Bertalanffy (1956), who coined the term. Hole & Heizer (1969:376; see also Hole & Heizer, 1973:443, 1977:360) indicated two things. First, a culture is an open system "in the sense that matter [or energy] is introduced from the outside. [A culture is therefore] not completely determined by initial conditions." Second, Hole and Heizer noted that equifinality applied to open

* E-mail: lymanr@missouri.edu

systems but not to closed systems. Another early archaeological reference to equifinality was by C. C. Lamberg-Karlovsky (1970:112) who also cited von Bertalanffy (1956, 1962) and indicated that equifinality, "simply stated . . . means that any final state may be reached from different initial conditions and in different ways." Lamberg-Karlovsky gave no indication that the concept applied only to open systems. That set what may have been a dangerous precedent given subsequent use and a general failure to understand the nuances of the concept.

Since the 1970s, many zooarchaeological taphonomists have, at one time or another, used the term *equifinality* to characterize instances when they had detected a pattern in a variable such as skeletal part frequencies and they wanted to emphasize the fact that at that stage of analysis they could not decide which taphonomic process or processes created the pattern. In such an analytical context, the term equifinality signifies the necessity of additional intensive analysis to aid in making the decision, though few taphonomists have made this latter connotation of the term explicit. Most seem to have implicitly known this and gone on about their analyses, trying to determine how to analytically make the decision. My point is that, almost without exception, taphonomists fail to discuss at length what the concept of equifinality means epistemologically or ontologically. I am as guilty of this generality as any one else. In my book *Vertebrate Taphonomy*, for example, I (Lyman, 1994c:507) used *Webster's Third International Unabridged Dictionary* as the source of the definition of equifinality that I gave; that definition reads as follows: "the property of allowing or having the same effect or result from different events." The one exception to the general failure of taphonomists to discuss the concept of equifinality in detail of which I am aware is the inspiration for the following discussion.

Because the concept of equifinality does seem to be important in taphonomic research, I begin with a synopsis of von Bertalanffy's discussions of the concept. This establishes the meaning of equifinality intended by von Bertalanffy, and also reveals a critical yet previously unrecognized feature (at least by taphonomists) of the concept's definition. It also exposes several epistemological nuances of different perspectives on taphonomy. I then examine a recent critique of how the concept has allegedly been misused by zooarchaeologists. I show that in fact the concept has been used appropriately by zooarchaeologists, especially in light of the previously unrecognized feature. I conclude with a brief discussion of how the concept has served as a catalyst for much thoughtful and innovative research.

Have taphonomists misused the concept of equifinality?

Alan Rogers (2000a:721) recently argued that those who use the term equifinality to signify cases when two different processes produce "merely similar" results (e.g., Lyman, 1993:325, 1994c:258, 262–263) are wrong to use the term in this way. According to Rogers, those who use the term incorrectly are wrong because von Bertalanffy meant the term to signify those cases when two processes "yielded outcomes that were *identically* (not just approximately) the same" (Rogers, 2000a:721). Rogers (2000a:723, fn 3) indicates that von Bertalanffy held that "equifinality did not exist unless two processes led to the same final state," and von Bertalanffy's "proof proceeds by showing that [in closed systems] two outcomes cannot be literally identical. From this [von Bertalanffy] concludes that equifinality does not occur [in closed systems]" (bracketed phrases added by Rogers, personal communication, Nov. 11, 2004). Has the concept of equifinality as originally defined by von Bertalanffy in fact been used incorrectly by taphonomists? That is, has equifinality been used to characterize cases that do not actually fit comfortably within the original specifications or definition of the concept? To answer these questions, it is necessary to do three things. First, I summarize various definitions of equifinality that von Bertalanffy provided over the years. My list is far from exhaustive, but it need not be to demonstrate that von Bertalanffy did not deviate from his original definition over a span of twenty years. Second, I consider whether von Bertalanffy really meant identical or merely similar, and the epistemological distinction of these two terms with respect to statistical analysis and to taphonomy. Third, I review von Bertalanffy's distinction of open systems and closed systems, and argue that an organism represents an open system when viewed taphonomically.

Definitions

von Bertalanffy (1949:157) defined equifinality as reaching the "same final state . . . from different initial [states]," and indicated that it characterized an open system, or one that was capable of "exchanging materials with its environment." In 1950, he indicated that the term signified reaching a final state "from different initial conditions and in different ways" and noted that equifinality characterized open systems but not closed ones (von Bertalanffy, 1950:25). In 1956 von Bertalanffy (1956:4, 7) defined equifinality as reaching "the same final state . . . from different initial conditions and in different ways" and described it as "the tendency towards a characteristic final state from different initial states and in different ways, based upon dynamic interaction in an open system attaining a steady state." In 1962 von Bertalanffy (1962:12) referred to a study by botanist R. H. Whittaker (1953) which indicated that "similar climax formations [of plants] may develop from different initial vegetations." von Bertalanffy quoted with favor Whittaker's (1953:48) characterization of this as "a striking example of open-system equifinality."

In his 1968 book entitled *General System Theory* von Bertalanffy (p. 46) indicated that equifinality is meant to label "the tendency towards a characteristic final state from different initial states and in different ways, based upon dynamic interaction in an open system attaining a steady state." Elsewhere in that book he states that when "a steady state is reached in an open system . . . independent of the initial conditions, and determined only by the system parameters, [it] is called equifinality" (p. 142). He defines the term formally on page 40, where he notes that "in any closed system, the final state is unequivocally determined by the initial conditions," but in open systems "the same final state may be reached from different initial conditions and in different ways," and it is the latter that comprises equifinality. Thus, beginning in 1949 and extending through 1968, von Bertalanffy indicated that equifinality comprised the production of the "same" final state from different initial states, and that it characterized a (open) system that exchanged materials with its environment. There are two key parts to this conception of equifinality: sameness of results, and whether a system is open or closed. I next consider each in turn.

Identical or similar?

So far as I have been able to determine, von Bertalanffy never said anything about "identical" outcomes resulting from the operation of open systems, though obviously he did say equifinal results are the "same." This leaves unanswered the key question of how similar two phenomena must be to say that they are the "same" or identical rather than merely similar. Determination of how similar is similar enough to say phenomena are the same is a classification problem and so can be reduced to an epistemological issue that is easily dispensed with because no two phenomena are ever literally identical in terms of every conceivable attribute they might have (Lyman & O'Brien, 2002; O'Brien & Lyman, 2002). That they are identical in the pertinent attributes (or share a particular set of attributes) is what is of interest. Having a problem to solve specifies the "pertinent" attributes—the ones that should be used to classify phenomena such that like goes with like (or things that are the same are categorized as instances of the same kind or class of phenomena).

But even when using only pertinent attributes, how similar must two phenomena be in order to say that they are "identical" or "alike?" How many attributes must they share? One distal tibia may have slightly more tissue of the diaphyses associated than another distal tibia, or one set of tibiae may have more female or left tibiae than male or right tibiae than another set, and so on. To decide if the observations regarding amount of diaphysis, side of the animal that contributed the bone, or sex of the contributing animal are "pertinent" attributes for classifying skeletal specimens as "distal tibiae" depends on how the class "distal tibiae" is defined. Hopefully, it will have been defined with a particular problem in mind and thus the relevance of particular attributes to the definition will be explicit. In such cases the necessary and sufficient conditions for concluding that two phenomena are the same or identical (they are members of the same class or category of phenomena) are explicit. If the variable of interest is skeletal part frequencies, and distal tibiae is defined as the distal articular end of the tibia, then a piece of distal tibia diaphysis without any trace of the distal articular end should not be classified (and tallied) as an instance of distal tibia. (Whether of a male or female, whether the epiphysis and diaphysis are fused or not, and other attributes are likely irrelevant.) Of course, this does not mean that zooarchaeologists and taphonomists have always used such a definition of distal tibia when they use the term *distal tibia*, contrary to assertions to this effect by others (e.g., Marean *et al*., 2000). Perhaps pieces of distal diaphysis of tibiae were included, but this analytical nuance was not made explicit because the (typically inexplicit) definition of distal tibiae used was something like *any part or portion of the distal half of a tibia.* Such inexplicit definitions are not uncommon in zooarchaeology. For example, the term "MNE" is often used to signify the minimum number of distal humeri rather than the minimum number of the skeletal element termed humerus. And, "bone" is often used to mean any specimen of a vertebrate, whether bone, antler, horn, or tooth, or fragment thereof (Lyman, 1994b).

Terminological vagueness has been a continuing problem in zooarchaeology and taphonomy (Lyman 1994a, 1994c), and is especially relevant here. Rogers (personal communication, Oct. 12, 2004) founded his arguments on a statistical perspective of terms, and thus it is important to summarize that perspective. I avoid all but the most necessary technical jargon and attempt to keep the discussion on a commonsensical level. With respect to studying processes and interpreting their results using statistics, the analyst tests a null hypothesis. The null hypothesis presumes that there is no (statistically significant) difference between an observed value and a hypothetical value; the alternative hypothesis presumes that there is a difference. Rejection of a true null hypothesis is a type I error, and acceptance of a false null hypothesis is a type II error. When one tests a hypothesis, the significance level (symbolized by the Greek letter alpha, or α indicates the chance that one is willing to commit a type I error. The probability of committing a type II error (symbolized by beta, or β) can be calculated if the alternative hypothesis can be clearly specified. The "power" of a statistical test is given by the complement of β, or $1 - \beta$, and is the probability of rejecting the null hypothesis when it is in fact false and accepting the alternative hypothesis when it is correct. The best (most powerful) tests are those in which β is small and $1 - \beta$ is large. There are two ways to increase the power of a test while retaining a chosen significance level (α). Either increase the sample size, or if that is not possible, use a different statistical test. A larger sample size alters the probability distribution of outcomes, such as when comparing the difference between the means of two samples, that difference will be statistically significant if the samples are large but not if the samples are small (given how the statistical test is calculated). Choosing a different statistical test can increase (or decrease) the power of a test because different statistics test similar (not identifical) hypotheses but have different powers (given how power is calculated). Suppose that with current data and statistical methods we are unable to choose between two hypotheses (which seems to be correct, which seems to be incorrect). If an increase in the sample size might allow us to choose, then we confront a problem of statistical power. But if the two hypotheses have identical implications, then no increase in sample size will help us make a choice. We need a different statistical model and test. Statisticians call this an identifiability problem.

Rogers was concerned that zooarchaeologists tend to use the term equifinality to refer to problems both of statistical power and of identifiability. Sometimes, taphonomists cannot determine based on statistical analyses whether the frequencies of skeletal parts in an assemblage are the result of differential (density-mediated) destruction or differential (human-behavioral) transport (e.g., Lyman, 1985). Faced with such a case, Rogers's point was that the failure to discriminate which taphonomic process was responsible for the observed skeletal part frequencies could be the result of either weak statistical power (small samples) or the statistical test used. A taphonomist refers to the problem simply as one of equifinality without distinguishing whether the problem is one of statistical power or the test used. Thus, the source of Rogers's criticisms resides in a terminological distinction, and it highlights an arena where more research could be done—the arena of statistical methods (see next section). On a more general level, a statistical test indicates whether two phenomena (numerical values or sets thereof) are *statistically significantly* different; if not, the implication from a statistical perspective is that they are identical and belong in the same class or category of phenomena. This is what Rogers likely was driving at when he argued that von Bertalanffy was concerned with identical phenomena as resulting from different processes—they were statistically identical (or at least indistinguishable statistically).

It is unlikely that von Bertalanffy literally meant that two phenomena were "identical" in every attribute, but rather that they were members of the same class of phenomena. The last seems likely given that in two different publications he uses the Whittaker (1953) example of what von Bertalanffy referred to as "similar" instances of climax vegetation as a "striking example of equifinality" (von Bertalanffy, 1962:12, 1968:102). Surely von Bertalanffy knew that plant communities were never truly identical in all of the nearly infinite characteristics that an ecologist or botanist might choose to measure. And, note that von Bertalanffy said the climax formations were "similar," *not* identical. Thus in my view it is a red herring to claim that equifinality is misused when what is meant is that multiple sets of phenomena are merely similar or statistically indistinguishable. I agree with the implication of the analyses performed by Rogers (2000b) and others who have sought to explain frequencies of skeletal parts that statistical similarity is sufficient warrant to be concerned about equifinality and to proceed as if two or more processes or sets of initial conditions produced similar results. From a statistical perspective, they are identical (to some greater or lesser yet measurable degree). The challenge is of course to determine if they are identical from a taphonomic perspective, by which I mean it must be determined if similar taphonomic processes and agents created them. This is so if we are interested in the processes or agents themselves—identification of the processes or agents is the analytical goal—or if we are interested in knowing that the collection of bones is somehow biased with respect to a variable of interest.

Open or closed systems?

The fact that it applies only to open systems is the critical yet previously unrecognized (by zooarchaeological taphonomists) feature of equifinality mentioned earlier. Over twenty-years worth of definitions and discussions von Bertalanffy consistently pointed out that equifinality applies only to open systems and not to closed systems. The question therefore is: Is the taphonomy of, say, an animal carcass an open system or a closed system?

I think taphonomists would all agree that taphonomic agents and processes working on an assemblage of carcasses or bones constitutes an open system—a system that is capable of "exchanging materials with its environment" (von Bertalanffy, 1949:157). From a taphonomic perspective, each animal carcass (or bone) is an open system because carnivores, weather (moisture, sunlight), geological forces, and a host of other taphonomic agents and processes external to the carcass (or bone), because they operate independently of any particular carcass (or bone), affect and modify that carcass (or bone), removing parts of it, replacing parts of it, and so on. As various taphonomists over the years have noted, given how taphonomy works, it is precisely the recycling of animal (and plant) tissues into usable materials by various biological, chemical, and mechanical processes that renders it almost miraculous that there exist several hundred to many thousands of years old bones and teeth (and any other parts of all sorts of organisms) for zooarchaeologists and paleobiologists to study. It is miraculous because if the open system of taphonomic agents and processes worked perfectly efficiently, bones and teeth and shells and feathers and leaves and seeds and twigs would never be preserved; they would be recycled into nutrients that later processes would breakdown chemically and mechanically and/or later organisms would consume, digest, breakdown into constituent nutrients, and the like.

Summary

Rogers (2000a:723, fn 3) was the first to note the distinction between open systems and closed systems in the context of taphonomy. Rogers also argued that equifinality concerns *identical* results only, not merely similar results. I agree, but in a qualified way only. As noted above, Rogers emphasizes the distinction of "power" and "identifiability" with respect to statistical tests of similarity and difference. I prefer to emphasize that the difference between *identical* and *similar* is an epistemological issue concerning classification, a seldom-remarked aspect of science that, upon detailed exploration, often creates various analytical problems and unnecessary disagreements. Both emphases are valuable. From his perspective, Rogers also highlights another aspect of the concept of equifinality in taphonomic research.

Has the equifinality concept hindered or impeded research?

Rogers suggests that the taphonomic definition of equifinality as involving merely similar results produced by different processes is "dangerous" because it "distracts attention away from . . . the need for better statistical methods" and that as a result zooarchaeologists will assume a fatalistic attitude and not develop better methods (Rogers, 2000a:721, 722). Here Rogers (personal communication, Oct. 12, 2004) did not mean that taphonomists would completely stop developing or searching for better methods; instead he was concerned that the intensity of search and development would decrease, particularly with respect to statistical methods. In a sense he is correct about the latter. As he pointed out to me, many taphonomists still generate bivariate scatterplots of skeletal part frequencies plotted against each part's (usually, bone mineral) density, and calculate ordinal scale correlation coefficients between the two variables. These are precisely the procedures that were used twenty years ago (Lyman, 1984, 1985). There has been some statistical innovation (Beaver, 2004), but it has been minimal. I doubt that the low level of statistical innovation is a result of fatalism, but rather I suspect that it results from a lack of statistical sophistication on the part of many zooarchaeologists and taphonomists. If they knew more about statistics and its numerous and diverse methods as well as its underlying principles, I have little doubt that they would develop many new statistical methods (or at least more frequently use other existing tests). That this is likely is supported by the fact that taphonomists have produced various analytical innovations in other (nonstatistical) arenas with which they are much more familiar.

That there have been other sorts of analytical innovation is easy to show. First, it was in fact recognition of the problem of equifinality in the taphonomic sense that the absence or relative paucity of skeletal parts that are not only very susceptible to destruction but also those that are of the least nutritional value (Lyman, 1985) that led investigators to shift the quantitative unit of choice from generally poorly preserved taxonomically diagnostic long bone ends to generally much better preserved anatomical units. Stiner (e.g., 1991, 2002) choose anatomical portions such as proximal forelimb (scapula + humerus), distal forelimb (radius-ulna + metacarpal), proximal hindlimb (femur), neck, head, and other units. Her reasoning was that the densest part of each portion was the most likely to preserve and would provide an estimate of the abundance of all other parts comprising that portion, particularly those that were unlikely to preserve. Stiner's units are what we know as MNE (minimum number of [skeletal] element) values but for what are anatomically larger and more inclusive than a skeletal element or discrete anatomical unit (Lyman, 1994a, 1994b). Marean and colleagues (e.g., Marean & Spencer, 1991) chose to quantify not so taxonomically diagnostic long bone shafts specifically because they tend to preserve better than long bone ends. Their procedure is a labor intensive one involving refitting and computer generated maps of anatomically overlapping fragments (e.g., Marean *et al.,* 2000) based on the observation that long bone shaft fragments tend to preserve better than long bone articular ends.

Whether one sides with Stiner or with Marean with respect to preferred quantitative unit is irrelevant to my point here. What is important is that the innovative methods and insights of both Stiner and Marean grew directly from observations of equifinal results and these methods and insights are decidedly not the result of fatalism or fear of statistical methods. I personally wonder if Marean's method is really worth the time and effort because it is as yet unclear if his method actually produces interval scale data or merely more precise ordinal scale data. As of this writing, so far as I know, there are no experimental samples with known initial MNE values that have been analyzed using Marean's suggested protocol and that demonstrate that an interval scale of resolution has been reached. Once such experiments have been performed, no matter what their results, we will have gained yet another significant insight to taphonomy that ultimately results from use of the concept of equifinality.

A second point to make is that explicit awareness of equifinality prompted taphonomists to explore statistical methods for studying skeletal part frequencies in particular. Rogers's (2000b) efforts in this respect are exemplary; importantly, he is not alone in this effort (e.g., Beaver, 2004). I suggest that one reason there is no plethora of titles on this topic concerns the fact that skeletal part frequency data rendered as MNE data are typically at best ordinal scale. This prediction derives from the fact that with respect to measuring taxonomic abundances, the two measures of choice—number of identified specimens (NISP), and minimum number of individuals (MNI)—are at best ordinal scale (Grayson, 1984; Lyman, 1994a). MNE is a measure that shares properties of both NISP and MNI, and thus MNE would provide data somewhere between NISP and MNI in value were it used to estimate taxonomic abundances. Given how MNE is derived (Lyman, 1994b), and it is indeed a derived measure analogous to MNI but at the scale of skeletal element rather than at the scale of a complete skeleton, it is logical to suppose that the difference in MNE value for, say, femora of a taxon would at best provide a quantitative measure that was of ordinal scale relative to the MNE of tibiae for the taxon of concern.

Whether or not the preceding is true, and I suspect that it is true, the direction of requisite additional research is clear. We need to answer the question: Are MNE values at best ordinal scale, or are they (or can they ever be) interval scale? The research protocol of Marean and colleagues seems to implicitly assume that MNE values can ultimately be interval scale, given sufficient rigor in their derivation. Perhaps this is true, but the only way to know for sure, as indicated above, is to derive MNE values with experimentally broken skeletal parts. For example, one person takes a collection of, say, complete deer bones from several skeletons, variously breaks the bones after tallying the MNE per element, then gives a sample of the specimens to a zooarchaeologist who then derives MNE values. Results of those experiments will then demand detailed statistical analyses to ascertain if the "zooarchaeological" MNE values are indeed ordinal scale relative to the original MNE in the unfragmented collection, and if the MNE values might sometimes be only nominal scale and occasionally (though I suspect rarely if ever) even interval scale. Such an experiment may not comprise the "better statistical methods" sought by Rogers, but it certainly will inform us on the quantitative and statistical properties of what is presently one of the most frequently used (and contentious) quantitative units in zooarchaeology and taphonomy. And note that how MNE values are tallied concerns classification—how are fragments identified (to general skeletal element [humerus, tibia, etc.], to general portion of skeletal element [proximal humerus, distal tibia, etc.], or to exact portion of skeletal element [lateral half of proximal humerus with 3 cm of diaphysis beyond metaphysis]), and how are they summed or tallied (as general or specific categories of anatomical regions). Without explicitly placing it in the context of a classification problem, this is in part the critically important point that Marean and others have been making with respect to tallying diaphysis fragments.

Concept borrowing can be a good thing

It was in the context of attempting to discern the taphonomic causes of skeletal part frequencies that I began to appreciate the interpretive significance of equifinality. In 1984, I wrote that differences in structural density were an ultimate cause of variation in skeletal part frequencies, and I emphasized that detection of a correlation between density and frequency did "not permit the identification of specific taphonomic agents" (Lyman, 1984:294). The latter necessitated other sorts of research and the recording of other kinds of variables, the implication being that a taphonomist needed to establish various diagnostic or signature criteria in order to identify taphonomic processes responsible for the correlations between the structural density and the frequency of skeletal parts (Lyman, 1994c). In 1985, I explicitly identified some of those other variables, such as carnivore gnawing damage, in the context of finding that a particular pattern of skeletal part frequencies might be the result of density-mediated destruction or the result of differential transport (Lyman, 1985). Efforts to disentangle this category of equifinality were subsequently expended by many individuals, and some of these individuals have contributed to this issue of *Journal of Taphonomy*. Our knowledge of the past, of both taphonomic processes and their attendant signatures and of the behaviors of hominids and other taphonomic agents, has consequently increased considerably over the past decade (e.g., O'Connell *et al.*, 2002 and references therein). Because of this, in my view, the concept of equifinality has not been misused or come to signify loose analyses. Nor has it discouraged zooarchaeologists from developing more rigorous methods, including statistical ones.

Equifinality is recognized today as a serious problem by some processual archaeologists because they believe that a particular pattern in material remains "can be created by entirely different sets of causal processes" (Cunningham, 2003:392). Yet archaeologists in general have not explored the concept in depth and detail. Equifinality in the context of taphonomic research forces us to be thoughtful, careful, and diligent in our search for knowledge of the past. This is not a bad reputation for a concept that was borrowed from general system theory. I suggest it was a good thing to borrow because the concept applies explicitly to open systems, and the recycling of animal carcasses is indeed a process characteristic of an open ecosystem.

Acknowledgments

Alan Rogers's provocative comments served as the inspiration for this paper. I owe him because until I sought to refute his argument, I did not understand the distinction between open and closed systems. Alan's comments on an early draft prompted me to expand several aspects of the discussion and also provided a deeper appreciation of his (statistical) perspective. The exchange of emails between he and I was most gratifying despite our fundamental disagreement on several issues; all exchanges in science should be so pleasant and fruitful. I also owe Natalie Munro and Guy Bar-Oz for inviting me to participate in the 67th annual meeting of the Society for American Archaeology where a much less developed version of this paper was presented. Todd VanPool kindly checked the accuracy of my discussion of statistical equifinality.

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