

## Boas, Kroeber, Lowie: American Anthropology Comes of Age

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Starting officially with President Thomas Jefferson's support for scholarly study of Native American peoples, a variety of American professionals and explorers labored for a century to build what became the new discipline of anthropology in the New World. Toward the end of the century, Franz Boas (*aka* 'Papa Franz') was assigned paternity of the new discipline, teaching first at Clark University and then establishing a Ph.D. program at Columbia University. The first graduate of Boas's department was A. L. Kroeber, chosen to set up a similar graduate program at Berkeley. Lowie, who joined Kroeber at Berkeley, came along just slightly later. Together, the three men were fundamental in shaping and developing American anthropology up to the 1930s.

In the 1940s Leslie White, in the American tradition of slaying the immigrant father, decided that anthropology up 'til then had been a mistake. He blamed a conspiratorial group which he called the 'Boasians' for a thirty-year delay in the development of the field. Ever after, while biologists "stood on the shoulders of giants," anthropologists credited their predecessors with the statement that they were standing on the bodies of pygmies. Certainly, the part of a dissertation labeled 'review of the literature' could be shortened considerably by repudiating all previous work, some of which had, in fact, been done by 'dead white men'. Marvin Harris himself, who like Leslie White, also founded a new and brighter anthropology, found that Morgan and Tylor were (surprise!) racists and that Boas, Lowie, and Kroeber were much-to-be-denigrated historical particularists (Harris 1968). Boas and most of his students displayed an unseemly concern for the facts, and it may be that Harris (like Herbert Spencer) was appalled by the specter of grand theories of unilinear evolutionism and materialism overthrown by facts unearthed by vulgar historical particularists.

In this article, I shall attempt to say some good things about Boas, Kroeber, and Lowie taking the view that the progress of a discipline generally requires that each new generation build upon foundations laid by previous generations. Perhaps we too "stand on the shoulders of giants." At any rate, a triumphal history is much better for advertising purposes than a history of repeated failures and outrages.

### *Franz Boas*

Boas was trained as a physicist and a geographer with a strong sense of the differences between laboratory sciences and field science. Scientific laws emerged from and applied to laboratory situations where variables could be carefully controlled. Like Einstein, Boas and his early students discounted the role of chance in the universe especially as it pertained to the scientific laboratory. Field data because of its unruly nature needed to be analyzed in terms of probabilities. Essentially, a field science or historical science involves making up stories and then choosing the story that best fits such facts as can be discovered. This process

would not furnish laws comparable to the first law of thermodynamics, but it would establish correlations (Lowie's word) that might have some predictive or explanatory value.

Older generations of anthropologists, lacking easy access to rail and steamship travel, were forced to rely upon the accounts of untrained missionaries and explorers for information about other peoples. Having access to relatively few and frequently inaccurate facts, nineteenth century anthropologists had the difficult task of explaining a reality that they could not see. At the beginning of the twentieth century, Boas could look out across the world and see that much of the subject matter of anthropology was disappearing – lost to history. At the same time, it was evident that existing theories and stories about humanity were wide of the mark. Some stories, used to justify colonialism, slavery, genocide, racism, and exploitative forms of communism, were not just wrong, but vibrantly evil.

Anthropology, viewed as the study of similarities and differences between groups of people, required detailed investigations of the historical origins and context of each similarity and difference. Boas's plan was to understand the historical development of each region of the world. After that, there would be plenty of time to draw up world wide evolutionary generalizations. It was this plan that was later called 'historical particularism'.

A few years later in England, Malinowski, despite the fact that he was a very sick man and often enraged by his informants, launched a similar attack against "conjectural history." He too wished for an anthropology based upon solid ethnography. Historians of anthropological theory pay little attention to adjectives, so it is no surprise that Malinowski went down in history as a hater of history, while Boas went down as a lover of history. At the time Malinowski wrote, many believed that all civilization originated in Egypt.

At the end of the nineteenth century, Boas provided a demonstration of his proposed methods of anthropological study through the publication of his study of the mythology of the Indians of the Northwest Coast of North America. At the time, many folklorists and anthropologists believed that human beings were basically not inventive. Cultural content was borrowed from the Egyptians, the folks out there in Atlantis, or perhaps point-eared space aliens. If a folk tale was found in Egypt and in Guatemala, then it was taken as proof that Egyptians had visited Guatemala. Following this kind of logic, there had been constructed a number of alarming and contradictory reconstructions of past human history, i.e. Smith (1911) and Rivers (1912). The fundamental assumption that only a few human beings are capable of having new ideas has alarming political implications.

In the United States, Daniel G. Brinton (1895 and 1896) attempted to counteract these sorts of assumptions by arguing that folktales were generally invented where they were found as a simple development or "unfolding" of the local culture. Each people ended up with the myths and stories that were appropriate to their location and stage of development. In this picture of strict unilinear evolution, everything is invented, nothing is borrowed. Here, there is a fundamental assumption that people cannot be educated, but must simply

develop at their own rate of speed. These two viewpoints, diffusion and independent invention, can both be used to justify the manipulation of the weak by the powerful. On the one hand, the ordinary person knows only what he is taught; on the other hand, he is incapable of being taught.

The year after Brinton presented his views on independent invention to the American Association for the Advancement of Science, Boas read a paper to that same organization effectively repudiating both extreme interpretations of independent invention and diffusion (Stocking 1968: 208-209). Boas approached the issue of diffusion vs. independent invention by collecting a great many Northwest coast myths and charting their distribution on the map. From the distribution it is plain that people make up stories and sometimes change the stories they hear from others. For the most part, plagiarism rules. Neighboring peoples tend to share the same myths, and this forces the conclusion that most stories are borrowed. Setting aside the Mayans, Aztecs, and other early civilizations, it can be argued that among the least creative cultures are the great urban and industrial civilizations that borrow everything from everywhere.

In consequence of the discovery of the relative predominance of diffusion, never seriously questioned, Boas and his students were labeled diffusionists. Their real interest, shown most clearly in the writings of Kroeber, was in explaining cultural growths and spreads using both diffusion and independent invention. The facts on the ground show that ordinary human beings prefer to learn things from their neighbors, but can quickly invent things on their own when necessary.

The debate between independent invention and diffusion is not really an anthropological debate, but a philosophical debate going back to a time when scientific study of human beings was considered superfluous. Most anthropologists, if they thought about it, may have been aware that the relative frequencies of invention and diffusion have a lot to do with the conditions under which people are living. Sometimes there is a lot of invention and sometimes there is a lot of diffusion. Looking back, then, it is hard to appreciate the importance of Boas's study of myths either as a demonstration of method or as a hard scientific finding. Mooney's deceptively simple explanation for revitalization movements and Tylor's seemingly endless demonstration of the "like working of men's minds" have a similar 'ho-hum' quality. On the other hand, the mark of many a great discovery is that people say, "I knew that all along."

One of the implications of extreme independent invention is that what gets invented has a great deal to do with biology. At the time Boas was writing, there was fear that the "Great Race" (Grant 1916) might be ruthlessly exterminated by intermixing with lesser races or perhaps simply by an absence of evolutionary challenges. In the United States, only a few years after the Civil War, the issue of relationships between the races was critical. Boas took measurements of people in different groups. In comparing the measurements (remember that he taught a course in statistics), he discovered that the differences *between* different groups of human beings are smaller than the differences *within* each group. In other words, Madame Butterfly might well share more genes with Puccini, than Puccini with the girl next door. Later research showing the actual distribution of real genes supports Boas's findings. From Neanderthal times onward and perhaps

before, interbreeding among human beings has been sufficient to prevent the establishment of racial types. There is no scientific or statistical way to define any human race. Still, racism is a long time dying.

Boas and his students, brought up in an earlier age, accepted the existence of human races at the same time that they participated in the overthrow of racism. They searched long and hard for accurate and scientific ways to define human races. Toward the middle of the twentieth century the concept of race was publicly rejected by the American Anthropological Association. As we speak, the forces of biological determinism again and again raise the specter of genetically determined behavior, and the United States census creates a *reductio ad absurdum* of racial categories.

Thomas Jefferson was upset when the French biologist, aptly named Buffon, declared that plants and animals from the New World were but pale reflections of the vastly superior plants and animals of the Old World. Discovering the origin of the peoples of the New World would put a final end to the argument. In the event, Boas organized a grand expedition to the Bering Straits area to test the hypothesis that there were cultural similarities between the peoples of Siberia and the peoples of the New World. If American Indians came, in fact, from Asia, they could not be considered to be an inferior creation made up especially for the New World and still referred to as *criaturas* (creatures, creations) in much of Latin America. Boas's hypothesis was stunningly confirmed. Since that day, the peopling of the Americas by way of the Bering straits has not been seriously questioned although alternative theories continue to be proposed.

Boas tested serious hypotheses about diffusion and independent invention, about race, and about the peopling of the Americas. These three lines of research cannot easily be put down as "historical particularism" or "antiquarianism." Certainly, they were as activist and political as any research could possibly be.

Still, Boas was concerned with data. An important part of the Boasian program for American anthropology was the collection of every possible bit of data available about the peoples of North America. Using the benefit of hindsight, various modern and postmodern anthropologists could have laid out a better scheme. All too often ethnographic information was collected from old people while their equally Native American children were ignored. Intent upon the task of 'salvage ethnography', anthropologists sometimes missed the important changes that were taking place in front of their eyes. Diffusion and independent invention are 'happening' processes, not just something that went on in the "good old days."

Armed with ever more perspicacious hindsight and a refined political sensitivity, critics of salvage ethnography have asked about the accuracy of the data collected. They have raised numerous questions about the ethics and morality of the process. On balance though, the program of salvage ethnography provided a framework where Native Americans, Euro-Americans and anthropologists could work together. The working together was not always harmonious, but the bottom line is that a great deal of information useful to all parties was salvaged. It is also a mistake to assume that salvage ethnography was not political. It was this activist science that converted Native Americans into

people with a history and gave their ways of life a kind of official sanction. The rise of anthropology went hand-in-hand with the decline of Indian boarding schools and the willful destruction of Native American culture.

*Kroeber*

During the 1930s, the Department of Anthropology in Berkeley was housed in a temporary building left over from the Spanish-American War. I remember waiting downstairs by the sarcophagus while my father carried out important duties on the second floor. I used to watch the smoke from Kroeber's pipe drift slowly down the staircase. Sometimes, my brother and I would be given the opportunity to pull on Kroeber's beard so that we could find out if it was real. It was. Much later, when I was at graduate school, Kroeber was retired and hard at work, so that we students rarely spoke to him, which was our mistake, I'm afraid.

After coming to Berkeley at the turn of the century, Kroeber threw himself into the task of completing his thousand page *Handbook of the Indians of California*. A big part of Kroeber's genius lay in his ability to lay out a program of work and get it done. After completion of the *Handbook* in 1917, graduate students of the Berkeley department were sent out into different parts of California to gather additional information in the form of 'Culture Element Distribution Lists' (known collectively as 'the CED') to be used to reconstruct the history and prehistory of California. Many of the students also completed deeper ethnographies of the people they studied. In 1948 when I entered the department, now located in a World War II temporary building, students in the pro-seminar were expected to complete a report based on an analysis of the CED.

The concept of culture element, like many of Kroeber's concepts, was sketched in, but not fully analyzed. Apparently, Kroeber wanted to get right to work and do the thinking later. Anyway, it turned out that some groups had thousands of culture elements and some very few, depending upon the diligence of the ethnographer. Of course, some cultural elements, "mass production," for example, are vastly different from others such as "table leg." Even so, as loyal students of the old school, many of us dream that this ambitious project will someday bear fruit. It is sad to think that it might be a costly, unfortunate mistake, but at least it is a more scientific mistake than the current conversations about 'memes' in evolutionary psychology.

The first edition of Kroeber's *Anthropology* (1923), lays out in a general way methods of reconstructing human history through the study of distributions of cultural elements variously conceived. It also foregrounds some classic errors in judgment. Kroeber, who was not alone, believed that the major task of anthropology was to determine the relative importance of biological and social factors in human behavior. This is the sort of debate that needs to be restricted to uninformed persons or, at best, to Lowie's "armchair philosophers." The task of anthropology is not to choose sides and debate the importance of different factors; it is rather to determine what the factors are and how they work together to produce human behavior.

Nature and nurture or heredity and environment, for example, are not in opposition to each other. Biological heredity, social heredity, and many aspects of the environment are all required for the production of a working human being or cultural system. From moment to moment the relative contribution of each causal variable changes in response to changing circumstances. For example, we inherit the ability to use language, but we still have to create it and learn it, a task that can only be fulfilled when nature and nurture walk hand in hand. One pities the biologists and social scientists who still carry forward ancient and irresolvable issues concerning which variable is the most important or which God is the highest. As the song says, "Some-time she go; sometime she doan go."

Genes and environments are different things, but we cannot draw so clear a distinction between messages and media. Genes are messages written on the molecules of DNA, and culture (seen as ideas or messages) is inscribed on the human brain (perhaps also on DNA-like molecules) or even on paper or clay tablets. Kroeber and Boas emphasized the cognitive or ideational aspect of culture as they considered the spread of ideas or cultural elements from one group to the next. From this standpoint, individuals and their cultures are simply the media across which the cultural messages pass.

By contrast, under the assumptions of unilineal evolution, the doctrine of independent invention requires the transformation of elementary ideas according to a specific ('divine') plan leading onward and upward toward civilization. Again, the individual is simply a medium across which the historical forces work their magic. Tylor made this same point when he demonstrated that both diffusion and independent invention require that people in different societies have minds that work alike. Therefore, the existence of either demonstrates the former. The Eskimo might have borrowed their patterns of kinship from the British or they might have worked them out on their own. Either way, to some extent, the Eskimo and the British occupy the same cognitive realm.

Boas, Lowie and Kroeber interpreted Tylor's statement as applying only to an evolutionary model in which one thing led to another and finally to civilization. As Leslie White pointed out, however, Tylor agreed with the proposition that both diffusion and independent invention were important. In general, Tylor and Boas even agreed as to the steps required to determine the origins of the elements found in particular societies. Attempts to lump Morgan and Tylor together make Tylor look less sophisticated than he was. Morgan thought he could lay out the entire "divine plan" for the evolution of humanity. Tylor was much more cautious -- almost Boasian.

At the turn of the century and for some years thereafter, anthropology was divided into three groups. Certainly, the majority of anthropologists placed their faith in pursuit of the false dichotomy separating extreme diffusionism and extreme inventionism. American anthropology as reformed by Boas was concerned to investigate relationships between invention and diffusion in terms of empirical investigation. Busy with all kinds of researches in linguistics and biological anthropology, Boas attempted relatively little in the way of historical

reconstruction beyond his studies of Northwest coast mythologies. Kroeber was more diligent and accomplished much more.

Kroeber defined a variety of types of cultural patterns (configurationalism) and the way in which they developed, changed and diffused (1944). Kroeber and Richardson's study of dress styles (1940), for example, traces the rise and fall of hemlines, *décolletage*, and other features of women's dress in the European tradition over a period of three hundred years. The paper develops and demonstrates the persistence and cyclical variation of stylistic patterns over time. One feels that probably Boas would have too -- that Kroeber and Richardson were moving too far and too fast. On the other hand, Kroeber and Richardson's study establishes the probability that stylistic patterns exist and that they are subject to regular variation. Kroeber's discussions and demonstrations of cultural growths and spreads anticipates Thomas Kuhn's work on paradigms. In particular, Kroeber's concept of pattern exhaustion remains exciting. Even today, we can see the frantic scurrying and innovating as some cherished pattern reaches the stage of pattern exhaustion.

Lamentably, just before the introduction of information theory, graph and network theory and computers -- things that might have made the study of geographical distributions worthwhile -- anthropologists abruptly lost interest in history and its reconstruction. The use of simple geographical distance was justifiably criticized, yet nobody came up with better ways of tracing distribution: for example, through networks of communication. The diagram of even the most primitive radio transmitter is more sophisticated and more useful as a depiction of a network than the ordinary diffusion diagram, yet there seems to have been little diffusion of improved models from other fields into the anthropology of diffusion. Collapse of the diffusionist pattern can also be attributed to problems in the definition of culture elements and in conceptualizing the role of geography as against other factors in the spread of ideas.

Kroeber's biggest and most criticized venture into grand theory was a paper designed to develop a distinction between organic and super-organic levels of analysis. Thinking of culture as communication, as ideas being transmitted, it is easy to make a distinction between what is communicated within the organism and what is communicated between organisms. Logically, groups of human beings, while different, are not that different from groups of cells. To be sure, cultures don't think, but neither do personalities. We do not, in fact, know what it is that thinks. Cultural systems share many properties with simpler sorts of organic beings even though they cannot fairly be called 'organisms'. At any event, Kroeber's characterization of human beings as passive transmitters of super-organic culture was not well received. No doubt, members of an academic culture characterized by individualism, entrepreneurship, and complete faith in the 'Great Man theory' have difficulty thinking about the super-organic. Outside of academe are members of a culture who regard their choices of food, clothing and housing as an individual matter, and who use conventional gestures and speech to deny the existence of culture.

Our cultural emphasis on individualism combined with a "Man bites dog" tendency to startle colleagues by pronouncing the non-existence of culture, may

have confused the issue completely. Levels of complexity, or, as Julian Steward (1949) would have it, "levels of integration" are everywhere. Whether one works with one level or another, or perhaps in the space between levels, is an analytical choice. One level is just as 'real' as another. Using a telephoto lens we see a tree. Using a wider lens, we see the forest. It is Reaganesque to argue that "if you have seen one person, you have seen them all." Still, you can learn a lot from the study of a single tree or a single person. In the real (cultural) world, there are only fifty-two Mondays in a year and the roster of great men must be gradually pruned over the years. Looking back, we see the forest and not the trees. Major inventions and the great men who invented them lose their importance. Who, today, cares about the inventor of cathode ray tubes? Who lavishes praise upon the inventor of the spear-thrower? As the individual fades away, the view from the super-organic appears brighter.

Planned by an expert, Kroeber's career reached its zenith in the few years following World War II. In 1948, there came the long-awaited second edition of Kroeber's *Anthropology*, a book that guaranteed its readers an easy pass on doctoral examinations, but hardly suitable for freshmen. A few years later came the summation of the first fifty or so years of academic anthropology -- Kroeber's encyclopedic *Anthropology Today* (1953).

Nobody else, except perhaps the far too modest Robert Lowie, had anything approaching Kroeber's encyclopedic knowledge. The movement of anthropology in the United States was away from 'salvage ethnography' and historical reconstructions toward active participation in cultural systems undergoing processes of modernization and acculturation. By mid-century, there wasn't much else other than sensational newspaper stories about various newly discovered remnants of the earliest forms of humanity. Psychological anthropology, following Boas and Kroeber's lead, was also becoming increasingly important in America despite an unfortunate tendency to degenerate into stereotypical pronouncements and categorizations about other cultures -- of which 'Apollonian' and 'Dionysian' (Benedict 1934) were among the least offensive. Both acculturation and psychological anthropology encouraged direct participation in ongoing social processes, and so, studies of specific communities began to replace studies of linguistic and ethnic tribes. The humanity of the people studied and of the anthropologist was increasingly foregrounded and the obscene goal of "social physics" receded deeper and deeper into the background.

#### *Lowie*

Malinowski tells us, "To the native, the construction of the canoe is the first link in the chain of the kula performances" (1922: 184). Lowie, who began his fieldwork in 1907 (but published later), writes, "When Strikes-at-Night was adopted into the Horse Dance, she did not at once accept the proffered eagle head and tail" (1935: 257). Malinowski observes the Trobriand Islander as an abstract 'native'. His activities are seen as parts of a typical performance and interpreted as a lofty and 'civilized' person. Lowie writes from inside the tipi portraying an individual engaged in idiosyncratic behavior. Lowie's historical particulars are often more concrete and more human than the dense descriptions of Malinowski. Malinowski and Lowie were not the first to write great

ethnographies, of course, but in their different spheres, they established the value of direct study of human beings.

Malinowski's importance lay, not so much in the fact that he wrote really good ethnographies, but in the fact that he gave some sort of self-conscious report of what he was doing and of his mental state. 'Real' anthropology was now accompanied by 'real' ethnography. Where Malinowski was inclined to spoil good ethnography with inane diagrams, Lowie began by making the reader aware of people like *Strikes-at-Night*. Always a gentleman, he politely introduces the reader to the folks. He fixes things, not statistically, but with an apt anecdote or mythological story.

Lowie was a city boy, who even in the late 1940s wore a coat and tie when participating in an archaeology dig. He must have been incomprehensible to Crow Indians -- at any rate, they seem initially to have ignored him. Finally, Lowie got hold of a piece of string and began to make 'cat's cradles'. The Crow had their own versions of these designs and so began an eager program of demonstration and education. In Lowie's ethnography, the dividing line between investigator and subject withers away (Lowie 1935). A man of intense modesty, Lowie appeared quite content to give the credit to Boas and Kroeber. But if Boas and Kroeber can be likened to two bullocks pulling the anthropological cart, Lowie was the drover. It was he who grasped the emerging nature of anthropology and it was he, who in the end, determined its direction. In the fields of mythology, art and psychology Boas was pre-eminent (1911 and 1927). In the fields of anthropological history, religion and social organization, Lowie made the larger contribution (1937, 1920 and 1948). Of course, Kroeber left few fields untouched, and whatever one's interests, there is always a seminal article by Kroeber somewhere in the background.

While not himself much engaged in the construction of theories of culture, which he considered a waste of time, Lowie took on the task of providing a theoretical basis for modern anthropology. His books on *Primitive Religion* (1922) and *Primitive Society* (1920) are major works setting up the categories of thought that most American anthropologists use today for the development of their understandings of religion and society. Kroeber and Boas remained in the 19th century, concerned with the long sweep of human history. The dearest goal of both men was the construction of a satisfactory theory of history, of human evolution. Even on the day of his death, according to Theodora Kroeber, he planned a new book about the emerging pan-western culture -- perhaps a higher stage of civilization. Thus, he may have seen as a kind of completion of the anthropological task the description of the next civilization.

Forty years earlier in *Primitive Society*, Lowie had questioned the values of progress and modernity, referring to 'civilization' as a "planless hodgepodge, a thing of shreds and patches" (1940: 441). For Lowie, there was no 'answer' in the sense of evolutionary theory, for "cultures develop mainly through the borrowings due to chance contact" (*Ibid.* 441). This was more true of industrialized and urban cultures than of any others. Perhaps the real anthropology that existed in earlier days was tucked away in the corners of Tylor's *Researches*, hidden in a chance remark in the writings of Baldwin and Spencer (1899), or

elaborately spelled out in Bogoras's unforgettable monograph on the Chukchee (1913).

The truth, the reality of anthropology, is that there are people out there, and those people are more like us than not. Just about everybody has some form of marriage and the function (usually not Lowie's word, although he considered himself a functionalist) of that marriage is the constitution of an economic unit having certain properties. *Primitive Society* deals with human behavior wherever it occurs. The topics are standard sociological topics; the family, the status of women, war, law, associations, clans and property. With regard to women, "it should be noted that the treatment of woman is one thing, her legal status another, her opportunities for public activity still another, while the character and extent of her labors belong again to a distinct category." This kind of analysis, which occurs on virtually every single page of *Primitive Society* offers a beacon that hopefully may still guide the students of women's status as well as students of other aspects of the human condition.

Lowie's style of thinking admits the entire human species to the categories of analysis and permits us to interrogate particular categories concerning their implications for humanity. Lowie's ethnography permitted the admission of human individuals and, at the same time, the study of living cultures and the living people in them. Thus anthropology moved from the study of "the old days," to the study of acculturation, psychology and cultural change in contemporary people. Margaret Mead (1928) could now study teenagers, even though in later years, as mature churchwomen, they might repudiate her findings.

Years ago, I wore an Otavalo poncho (brought from Ecuador by my parents) to Lowie's seminar. It was cold and rainy and the thick, all enveloping, woolen poncho seemed just right. He was the only person in the room who did not laugh ethnocentrically at my weird attire. Lowie's kindness ended, though, with the German examination. The department had decreed that I could not take my language examination in French, but must take it in German, a language I hardly knew. As I sat there looking at the unfamiliar text, I realized that it was a familiar piece by Metraux describing his journey down the Amazon. As I thought about things that might be observed upon the Amazon, the unfamiliar German words gradually made themselves known. I passed the examination more on the basis of ethnography than German. Later, for Lowie's seminar on European culture, I had the effrontery to submit a half-finished paper involving complicated statistics concerning temporal variation in Spanish typography. Nothing could have interested Robert Lowie less. On this paper, he wrote: "this small man with a small job to perform, sees it and does it; this large man, with a large job to perform dies ere he knows it." So much for myself, and perhaps for Boas and Kroeber as well.

It was Lowie who saw what needed to be done and did it.

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