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(2006)

"The World Upside Down : Boas, History, Evolutionism, and Science."

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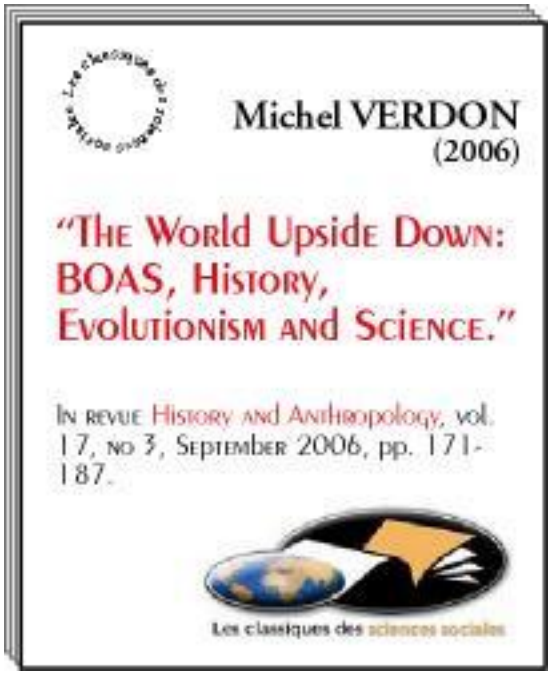

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Abstract

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A new wave of neo-Boasian anthropologists advocate retrieving Boas's sense of historicity. In his theoretical writings, and especially his early exchange with Mason and Powell in 1887, Boas linked history to Alexander von Humboldt's "cosmographical" method and to inductive science, accusing evolutionists of reasoning deductively on the basis of arbitrary classifications. Boas, on the contrary, would not classify but would consider the "individual phenomenon". Strangely enough, Boas's presentation of his scientific procedure has more or less been taken at face value, and I question this Boas-centric view of Boas. Examining Boas's theoretical statements, his onslaught against evolutionism and his ethnographic practice, I find the accusation of deductive reasoning against evolutionists totally polemical. Furthermore, I discover neither induction nor history or cosmography in his practice, but a Linnaean-type natural history. In brief, I uncover an inverse image of what Boas presented of himself, and no basis whatsoever for retrieving a historicity for contemporary anthropology.

Keywords : Boas ; History of Anthropology ; History ; Science ; Evolutionism

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Introduction

Retour à la table des matières

Some neo-Boasian anthropologists have recently advocated going back to Boas's anthropology to retrieve his sense of historicity (Bunzl, 2004 ; Orta, 2004), and even his fundamentally Darwinian thought (Lewis, 2001). Boas's understanding of history is intimately linked to his more encompassing views against evolutionists, and his powerful advocacy of induction. Elsewhere, while investigating his holism, I have examined some of his historical reconstructions, and found little historicity (Verdon n.d.) ; here, I deem it necessary to assess further his sense of history in a more abstract way, through his understanding of science.

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German-born, first trained in physics (PhD) and then geography in Germany, Franz Boas set out for Baffinland to study the Eskimos in 1883, and in 1886 started fieldwork on the northwest Pacific Coast ; this remained his key fieldwork to his death.

As early as 1887, he launched into powerful methodological critiques against evolutionists (1887a, 1887b, 1887c, 1887d) : they classified prematurely and argued in a deductive fashion, like physicists, Boas argued. Against such practices he presented himself as a thoroughly inductive scientist bent on retrieving the full history of individual phenomena, in the manner of Alexander von Humboldt's cosmography. Strangely enough, contemporary historians of Boas have not questioned this representation (save Wax 1956 ; Buettner-Janusch 1957 ; and White 1963, to my knowledge), giving rise to very "Boas-centric" interpretations of Boas and Boasian anthropology that hardly help evaluating his anthropology, and its relevance to contemporary anthropological practices bent on retrieving historicity.

I will thus study Boas's scientific thinking and its relationship to history by drawing mostly on his theoretical statements, as well as parts of his ethnographic practice ; as a social-cultural anthropologist, I will leave aside his important contributions to linguistics and physical anthropology. I will first examine his "inductive approach" and its

relationship to cosmography, and uncover very little induction. Turning to the question of evolutionism, I then appraise his allegation that it was deductive, and find the accusation mostly polemical. The question of sociocultural evolution evokes Darwin, especially since Lewis recently portrayed a thoroughly Darwinian Boas (2001). Stressing some of the fundamental flaws in Lewis's thesis, I end the article where Boas started, namely on the question of classifications, to re-evaluate the famous 1887 exchange with Mason and Powell on museum exhibits (1887b, 1887c, 1887d) ; I conclude that Boas's scientific thinking and practice belonged more to a taxonomic Linnaean-type natural history than anything else, contrary to the presentation of his scientific self.

In my perspective, borrowed from Foucault and Jacob, a Linnaean-type natural historian approaches phenomena through their *external* attributes (Foucault 1966 ; Jacob 1970). According to Foucault and Jacob, biology only surfaced in the nineteenth century with the concept of organization, which implied relating causally the various internal organs of animals, and Linnaean-type natural history completely lacked any notion of life and organization (the two being epistemologically linked) ; eighteenth-century natural historians perceived only living organisms grouped in species to be classified on the basis their visible structure, or external attributes (Foucault 1966 :144, 149, 156 ; Jacob 1970 : 37, 42, 54ff).

Boas, Mason, and Induction

Boas, Geography and Mason

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Boas first expressed his views on science in "The Study of Geography", claiming that science aims "to *deduce* laws from phenomena" (1887a : 640, italics added), and that the individual phenomena lose their relevance when the law has been "discovered". [1] Against this

[1] On the nineteenth century view that laws were "discovered", not "constructed" by the observer, see Buettner-Janusch, 1957.

method of physics, he advocated for the study of geography a method [173] inspired by history, whose aim is not to "discover general laws" but to study the phenomenon itself (640-641). About physicists, Boas went on to describe what scientists then understood as a process of induction : "All agree that the establishment of facts is the foundation and starting-point of science. The physicist compares a series of similar facts, from which he isolates the general phenomenon, which is common to all of them" (1887a : 641), which he surprisingly regarded as deduction, adding that "science has the one aim, to deduce laws from phenomena. The single phenomenon itself is insignificant : it is only valuable because it is an exemplification of a law" (642). To this he opposed Alexander von Humboldt's "cosmographical" method, which "considers every phenomenon as worthy of being studied for its own sake" (642), finally summarizing the two procedures as preferring "to recognize the individuality in the totality, or the totality in the individuality" (645, italics added), declaring the latter fundamentally inductive and historical.

In the same year, in an exchange with Mason, anthropologist and the curator of the National Museum in Washington, Boas falsely accused the latter of privileging inventions and espousing Morgan's supposition that "like causes produce like effects" (in fact, Mason had stated the exact opposite in a 1886 article). He further described Mason's method—"to compare the phenomena, and to draw conclusions by analogy" (Boas 1887c : 588)—as deduction, to which he contrasted his own method, namely "to study phenomena arising from a common psychical cause among all tribes and as influenced by their surroundings ; i.e., by tracing the full history of the single phenomenon", and to study "each ethnological phenomenon individually" (588, italics added), a method he described as inductive.

Boas thus portrayed Mason as an "atomistic" (opposite of holistic) deductivist privileging inventions. This established his main line of critique against evolutionists : they "deduced" laws on the basis of premature classifications, held a wrong theory of causality (like causes produce like effects), considering cultural phenomena outside the tribal whole to which they belong (which Mason did not systematically do—Mason 1886 ; Hinsley, 1981).

In "The Aims of Ethnology" (1889), Boas repeated the distinction between the two purposes in the study of "ethnological phenomena"

(1 : retracing the history of the single phenomenon—"single phenomenon", here, might refer to "a tribe" ; and 2 : ultimately to reach laws about sociocultural evolution). He insisted that they are not really distinct, since "the general law is expressed just as clearly in the individual phenomenon as the individual phenomenon is expressed in the general" (1889 : 68), hardly a self-evident statement. He clarified the matter in 1904, in "The History of Anthropology", contrasting his historical approach to the evolutionary one :

> The new historical view also came into conflict with the generalizing method of science... in which the discovery of general laws was considered the ultimate aim of investigation. According to this view, laws may be exemplified by individual events, which, however, lose their specific interest once the laws are discovered... This view is, of course, fundamentally opposed to the purely historical view. Here the laws of nature are recognized in each individual event, and the chief interest centers in the event as an incident of the picture of the world (26, italics added).

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Over close to 20 years (the article on geography was written in 1885—Bunzl 1996 : 55), Boas thus held remarkably coherent views. Indeed, if we add up these various statements, we get two parallel series of closely synonymous assertions :

1. *The method of science*

1887 : "deduces laws from phenomena" and the "single phenomenon" exemplifies a law ; is equivalent to "recogniz[ing] individuality in totality" ;

1889 : "the general law is expressed in the individual phenomenon" ;

1904 : laws are exemplified by individual events.

2. *The method of history/cosmography*

1887 : every individual phenomenon worthy of being studied for its own sake ; recognizing the totality in the individuality ;

1889 : the individual phenomenon is expressed in the general ;

1904 : "laws are recognized in each individual event", and "*the chief interest centers in the event as an incident of the picture of the world*" (26, italics added).

Boas and Induction

These statements call for some clarifications. In the nineteenth century, induction denoted the process whereby one inferred generalizations from a large body of data ; these generalizations yielded, or were read as, laws. The paradigmatic reference was Boyle's work, and his famous law on gases. Again, in the same period, deduction designated something akin to Descartes' physics, namely a *hypothetico-deductive* method whereby facts are derived, or "deduced", as corollaries from first principles ; or a method whereby, from the facts, one leaps imaginatively to some hypothesis, or some first principles, and verifies if the observed facts could be deduced as corollaries from that hypothesis, or those principles.

Against this background, where does Boas stand ? In the statement quoted above he showed that he grasped relatively well what inductive science meant, but misrepresented it as an instance of deduction, an idea he repeated throughout his career. Ironically, he drew this conclusion from an unfortunate use of synonyms. In this context, "to deduce" actually means "to infer" or "to derive" and does not, by any stretch of a philosopher's imagination, denote deduction as a scientific procedure typified by Descartes classical "hypothetico-deductive" method.

Furthermore, the procedure he mentions in his 1904 statement—the idea that the (Humboldtian) interest in the individual phenomenon

"centers in the event as an incident of the picture of the world" (1904 : 26)—has nothing to do with either induction or deduction but with the very Humboldtian idea that the microcosm (Humboldt's "biological province") displays the features of the macrocosm, reflects its laws, and that we can understand the whole from the study of a part only (Browne 1983 : 44, 46) ; this procedure is analogical, not inductive or historical (Browne 1983 : 52 ; Bachelard 1938).

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If we focus on Boas's ethnographic practice, what did he plan to do, and what did he actually achieve ? The list is impressively long, and I will confine myself to the projects that stand out. (1) He wished to capture the full history of "individual ethnological phenomena" [cultural fragments, or elements] by placing them back in as many of their "surroundings" as possible ; one of these surroundings was the very people among whom the individual phenomenon was found. (2) By putting together the various individual phenomena of the same people, he hoped to bring out their "style", or what could be translated as "the mental imprint left by the Volksgeist" (read : "genius of a people") to retrieve their meaning. (3) He also hoped to extract the laws of the development of the human mind as it manifested itself in the growth of cultures by reconstructing a vast series of histories of individual cultures, something he attempted to do (4) through the geographical mapping of sociocultural phenomena. Finally, he collected texts (5) as a means of finding how native peoples represented their own culture.

Of these elements of his research program, the cosmographical method (microcosm as displaying laws of the macrocosm) would neatly fit in projects (1), (2) and (5). Indeed, if individual sociocultural phenomena bear the imprint of a people's mental make-up, the microcosm could give us a picture of the macrocosm (culture) ; we would recognize the totality in the individuality. Texts (project 5) would also belong to this type of thinking. However, Boas never carried out any part of these "cosmographical" projects [2] ; they remained program-

[2] He nowhere "captured the full history of individual ethnological phenomena" ; he treated style in a "quantitative" manner, and collected texts but did not interpret them (Verdon n.d.).

matic. In addition, as I emphasized above, this type of reasoning is neither inductive nor deductive or historical, but analogical.

The third project (reconstructing the history of individual cultures to discern the laws of culture growth) consisted of at least two parts, namely the geographical mapping (a) from which to derive histories of individual cultures, and (b) in the hope ultimately to discern the laws of culture growth. The wish to discover the laws of sociocultural evolution through the reconstruction of a large set of histories of individual cultures was truly inductive. Boas never reconstructed the history of any individual culture, however, and the project also remained programmatic.

What about the parts of this vast research program that he did carry out, namely distributional studies and the historical reconstructions, not so much of individual, but of "regional cultures" (G. W. Stocking Jr's expression 1968a : 153 ; they are also referred to as "culture areas") ? I will deal with their historicity below. Were they inductive ? They could superficially appear so in that, from geographical distributions, Boas might be said to have inferred regional histories, and therefore proceeded inductively. This would strip induction of its specific meaning, however. Common usages are treacherous, and inference is no induction. Set against a definition of induction *as a scientific procedure*, Boas's regional histories were mere conjectures, or inferences based on trait distributions.

No doubt one or two specific studies, such as the one Lewis mentions on Alaskan needle-cases (Lewis 2001 ; Boas 1908 ; I could not find any other one), could be deemed inductive. From a vast collection of detailed facts, Boas generalized about the evolution of primitive art, but such studies in his ethnographic practice were the extremely rare exception rather than the rule.

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Where, then, does the image of an inductivist Boas stem from ? From his own statements no doubt, but equally through his critique of evolutionists as deductive thinkers, constructing premature classifications from the purely hypothetical premise of a unilinear evolution, and automatically classifying cultural products or societies on the basis of these aprioristic classifications.

Admittedly, all evolutionists shared teleological premises : that of proving a movement of ascent from the so-called primitives to their superior, "civilized" society. Such theses, needless to say, legitimated and fuelled colonial invasions, but this bears only indirectly on the issue I am here addressing, namely the validity of Boas's accusation about the evolutionists' allegedly deductive procedure. At this juncture, some clarifications are called for about evolutionism.

Some Considerations on Evolutionism

Evolutionary biologists usefully distinguish the "fact" of evolution from phylogenetic reconstructions, and from theories of evolution (Ruse 1973). Phylogenetic reconstructions are simply the sequence that biologists establish in the evolution of species (until recently mostly on the basis of morphological evidence but now ultimately tested against DNA) and tell nothing of evolution's mechanisms. A theory, in this context, is an explanatory model accounting for evolution by trying to identify its causes. Thus, the most famous book expounding the now accepted theory of evolution, Darwin's Origin of Species (1859), ignores phylogenetic reconstructions in its argumentation.

In the case of sociocultural evolution, I will plainly refer to "evolutionary reconstructions", or merely "reconstructions", and dissociate them from theory. Theories of sociocultural evolution were few. Most evolutionists (Comte and Tylor, for instance) simply fused evolutionary reconstructions and theories of evolution by inferring mankind's growing rationality from the increasing complexity of its creations. Others held a separate theory of evolution. Spencer, for one, located the roots of evolution in environmental changes that forced people to evolve new activities, and selected those who could adapt first ; they in turn decimated the earlier populations. Morgan explained evolution in terms of inventions in "arts of subsistence" that brought individuals more and better food, allowed demographic increase leading to outbreeding, creating better brains that thought up new inventions, and so on.

"Cultural evolutionists" also differed from "social evolutionists" (mostly Spencer, Durkheim and Morgan) ; the cultural evolutionists dealt with "knowledge, belief, art, morals, law, custom, and any other capabilities and habits acquired by man as a member of society" (Tylor 1871 : 1) and mostly did so in a disconnected (atomistic) way, reconstructing the evolutionary sequence of distinct customs or practices (games, beliefs, art, rituals) rather than whole societies.

As part of their teleological reasoning all evolutionists, social or cultural, described a unilinear evolution towards our allegedly superior type of society. Evolution had to be governed by laws, the roots of which cultural evolutionists located in the mind, portraying their evolutionary sequences in terms of a progress in rationality. To the *cultural* evolutionists, the axiom of human rationality, however feeble that rationality [177] was among primitives in their view, was a necessary premise for a positivistic program ; radically irrational cultural productions could hardly be governed bylaws, and could not exhibit any evolution, especially unilinear. Thus, cultural evolutionism stood or fell with human rationality, a crucial element to understand Boas's strategy.

Spencer, Durkheim and Morgan, on the other hand, essentially dealt with groups and activities, and how they differentiated over time (from homogeneous to heterogeneous social organization). Morgan dealt with institutions (the equivalent of groups), which evolved "in lock step", to use one of Stocking's expressions.

When it came to their evolutionary reconstructions, social and cultural evolutionists thus differed significantly. If not completely absent, the question of rationality was peripheral to the social evolutionists' work ; they were concerned with the increase in individualization and its consequences on social organization. Furthermore, their theoretical concerns left no room whatsoever for the questions of invention and diffusion, about which they did not write anything. On this question, Morgan stands midway. Although a social evolutionist, his theory of evolution led him to link evolutionary achievements to brainpower, and therefore to inventions and increases in rationality. To that extent, he is closer epistemologically to Tylor than to Spencer and Durkheim, and shall therefore be included for the remaining part of this discussion among the cultural evolutionists.

As is well known, evolutionism also called to mind a method, that of comparing artefacts or institutions regardless of time and space, on the basis of their assumed similarities. Once similarities were uncovered, some cultural evolutionists explained them by independent inventions. This implied that human beings were essentially inventive and, moreover, that "like causes produce like effects", an assumption mostly linked to Morgan and his disciples. In brief, it implied frequent independent inventions of the same ethnological phenomenon and treated invention as the most fundamental mechanism, not only of evolution but also of culture formation itself. For others, however (Mason, in fact—1886 : 250—and Tylor himself), inventions were the source of evolution, but diffusion was the predominant mechanism of culture formation : "Civilization is a plant much oftener propagated than developed" (Tylor 1871 : 53).

Boas condemned evolutionism, but which aspects ?

Boas and Evolutionism

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In 1887 Boas did not deny the fact of sociocultural evolution and, as a scientist, believed that it would eventually be possible to discover its laws. Nor did he completely reject all evolutionary reconstructions. As late as 1911, in The Mind of Primitive Man, he endorsed the view that primitives were ruled by custom, in contrast to more rational modern individuals increasingly capable of freeing themselves of its influence (Boas 1889 : 68, 69 ; 1911a : 210). Actually, he mostly discredited cultural evolutionists ; it was left to Lowie to achieve with social evolutionism what Boas had accomplished with cultural evolutionism (Lowie 1920). From the outset, however, Boas strongly, and rightly, opposed evolutionism on method (1898 : 108), and came to reject all their reconstructions (about religion, art, style, and so on).

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This accounts for part of his life-long strategy. Having supposed that all evolutionists made rationality the driving force of evolution, that they understood rationality to express itself through inventions, and that they further classified societies according to the double di-

mension of rationality and inventions, he countered evolutionism by assuming that humans are relatively uninventive and essentially nonrational, if not irrational, in most of their customs, save their techniques. [3] If so, then all evolutionary reconstructions based on mankind's increasing rationality collapsed.

He further exploded evolutionary reconstructions through his favorite method of dissociation, and this is where "history", not induction, creeps in. He repeatedly "dissociated" complex cultural phenomena, such as myths or rituals, and mapped the distribution of the various dissociated fragments. He regularly, if not systematically, found that their distributions did not overlap, and concluded that these phenomena were not "organic growths" (read : invented locally) but "accreted" phenomena ; in a word, they were pieced together, so to speak, through dissemination. This, more than anything else, was the methodology he persistently used to undermine evolutionary reconstructions ; I will deal with its alleged historicity in the conclusion. He applied the same method to classifications, evolutionary or not, and found them unfounded.

Furthermore, he undermined the evolutionists' "comparative method", confining his comparative endeavours to neighbouring societies. In all this, he never made use of any inductive procedure. But was he right in accusing evolutionists of deductive reasoning based on arbitrary classifications ? True, the evolutionists classified on the basis of relatively aprioristic classifications, but did they argue deductively ?

The problem all "ethnologists" faced, certainly in the latter half of the nineteenth century, was the question of sociocultural diversity and the concomitant fact that societies differed so thoroughly in their technological development. This could be explained variously. As Stocking showed, Tylor's evolutionism aimed to counter degenerationist and polygenist (creationist) theses (Stocking 1968b : 74-5). In the reigning framework of a Biblical chronology of 6,000 years, evolutionism was unthinkable ; only polygenism or degeneration (or envi-

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[3] This was the root of his famous theory of secondary explanations, according to which beliefs and customs have nonrational, if not irrational, roots. As I explained elsewhere (Verdon n.d.), Boas needed to invent this theory, which he started developing as early as 1891, to hit at the foundations of cultural evolutionary reconstructions (Boas 1891).

ronmental determinism) made sense. But the 30,000 years or so attributed to mankind after Boucher de Perthes' discoveries made an evolutionary narrative not only possible, but more plausible than if merely posited.

The German solution, which Boas adopted, was to invoke the action of various Volksgeister (sing. Volksgeist, the "genius, or spirit, of a people") ; there was sociocultural diversity because each "people" was endowed with a peculiar "genius" or 'spirit" (Geist) that stamped on it its cultural singularity. Despite its lack, and even rejection, of an evolutionary narrative, the Volkgeist tradition was neither relativistic nor enlightened, despite what a new historiography intimates. Indeed, most of those who explained sociocultural diversity in terms of Volksgeister—and this includes Virchow, Bastian and Steinthal, among many others, who directly inspired Boas—also worked within the dichotomous framework of Kultur- and Naturvolker ("culture peoples" and "nature peoples"). Zimmerman masterfully demonstrated how fundamentally racist such a dichotomy was (Zimmerman 2001 : 44, 50-52).

Boas evoked the Volkgeist but rejected the infamous Kultur—Naturvolker dichotomy, and dismissed most forms of racist narratives, in the wake of Waitz. [4] Why he did so [179] remains an open question ; the discrimination he suffered in Germany as a Jew might be part of the answer. From a methodological point of view, however, his rejection of racism and of evolutionary narratives was not based on induction, nor does it tell of deduction on the part of the evolutionists.

Both the evolutionists and German ethnologists attempted to account for sociocultural diversity. Most evolutionists (those Boas particularly targeted) sought an explanation in rationality ; the Germans, in some kind of occult, mystical entity. As I see it, the two brands of anthropology proceeded in similar manners. Like their German counterparts, the evolutionists did not think up their reconstructions ex nihilo ; like everyone else, they had to start from some "facts", and those

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[4] In fact, as early as 1859, Waitz was moving towards a cultural relativistic position which anticipated much of Boas's ; indeed, Lowie characterized Waitz's Anthropologic der Naturvolker as "a forerunner of Boas' The Mind of Primitive Man", whose arguments closely parallel those of Waitz (Lowie 1937 : 17).

facts were diverse societies displaying dissimilarities that could be construed as differences in complexity. They then inferred the law of increasing rationality from observed increases in complexity (whether or not they aimed to prove that complex societies were also superior in every respect) ; similarly, the Germans derived the existence of the Volksgeist from the similarities in style of a people's cultural productions and their dissimilarities from other peoples (whether or not they demoted some of these people to the level of cultureless, almost non-human, "nature peoples"). For the evolutionists, people differed because they were more or less rational ; for the Germans, because they were endowed with different geniuses. On what basis can either procedure be declared more inductive ? I fail to see any. In brief, to oppose an inductive German and Boasian ethnology to a late nineteenth-century deductive Anglo-American one, as Boas did and his commentators have repeated, appears to me a biased understanding of events, one that seems to accept Boas's indictments at face value. It wrongly opposes historicity on the one hand to some kind of pseudo-science on the other.

Furthermore, whether in nineteenth-century German ethnology or in Boas's anthropology, the *Volksgeist* was radically a-historical. Boas did not perceive it as the outcome of historical processes ; quite the contrary, he evoked it as some kind *archè*, a "homogenization principle" that gave randomly associated cultural fragments a cultural identity.

Boas and Darwin

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Although evolutionism brings to mind the name of Darwin, Boas's relationship to Darwin did not seem a controversial issue until very recently. In 1959, Kluckhohn and Prufer had re-emphasized how Bastian and Virchow—who strongly influenced Boas—more or less rejected Darwinism (18) ; they further remarked that Boas himself held that "Lamarck was still to be reckoned with" (22). As late as 1992, Stocking wrote that Boas was aware that his anthropology's "goal was essentially that of pre-evolutionary diffusionist ethnology" (Stocking 1992 : 122). In 2001, however, Lewis challenged common wisdom

and depicted a thoroughly Darwinian Boas. The claim should not be left unchallenged.

Boas did not reject Darwin, claims Lewis, "but the entirely different teleological perspective of Herbert Spencer and his followers" (Lewis 2001 : 382), [5] quoting Boas's 1887 exchange with Mason : "'It is only since the development of the evolutional theory that it became clear that the object of study is the individual, not abstractions from the [180] individual under observation' (Boas 1887b : 485)" (Lewis 2001 : 382), and the further Boasian claim that Darwin demonstrated that "the physiological and psychological state of an organism at a certain moment is a function of its whole history" (1887c : 589).

In fact, argues Lewis, Boas had almost anticipated Mayr's modern neo-Darwinian synthesis, stressing the uniqueness, the variability, the historical character and the randomness of sociocultural phenomena—the very features of biological phenomena in the modern neo-Darwinian synthesis (Mayr 1982). Furthermore, these very attributes also describe the social world as it appeared to the pragmatists (William James, John Dewey, and George Herbert Mead), who evoked Darwin's influence ; consequently, concludes Lewis, the Darwinian Boas was also a pragmatist.

How credible is this evidence ? First, the reference to Spencer confuses evolutionary theory and evolutionary reconstructions. Spencer's *theory* of evolution was nonteleological (no one could predict which way the environment would change) ; his unilinear evolutionary *reconstruction* was teleological but, as a social evolutionist, his intellectual heirs—among whom could be counted Durkheim—were not the main evolutionists that Boas inveighed against.

Second, Lewis quotes out of context. In the first quotation above Boas mentions "evolutional", not "Darwinian" theory (although he does later—1887c : 589), and the complete quotation tells a different story. Immediately following the above, Boas writes : "We have to study each ethnological specimen individually in its history and in its medium, and this is the important meaning of the "geographical province" which is so frequently emphasized by A. Bastian" (1887b : 485).

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[5] This, incidentally, flies in the face of some of Boas's classical statements accusing Darwinism of most of anthropology's ailments (1904 : 25-7 ; 1911a : 162).

From "evolutional theory" Boas leaps immediately to Bastian who, by no stretch of the imagination, could be transmuted into an evolutionist, let alone a Darwinian. The immediate reference to Bastian's "geographical province", and the emphasis on placing the specimen back in its surrounding, hark back to Alexander von Humboldt and the latter's claim that the plant could only be understood when placed back in its surroundings, in its "biological province". Von Humboldt's essay on *Cosmos* also had an "evolutional" element to it, and deeply impressed Boas. Darwin, on the contrary, denied any link between species and their surroundings, seeking on the contrary to prove widespread diffusion : "In considering the distribution of organic beings over the face of the globe, the first great fact which strikes us is, that *neither the similarity nor the dissimilarity of the inhabitants of various regions can be accounted for by their climatal and other physical conditions*" (1859 : 344, italics added).

Lewis next quotes Boas claiming that Darwin demonstrated that "the physiological and psychological state of an organism at a certain moment is a function of its whole history" (1887c : 589). Again, the message is quite different when one reads the complete paragraph, [6] and there is strictly nothing Darwinian about it. It amounts to saying that two organisms might look exactly the same, but differ radically because of their whole history. To my knowledge, Darwin never wrote anything even remotely suggesting anything like this. All this is but the tip of the iceberg, as Lewis goes on compounding inaccuracies, trying to substantiate his erroneous thesis. [7]

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6 'In the preceding sentence, Boas wrote : "Former events [...] leave their stamp on the present character of a people. I consider it one of the greatest achievements of Darwinism to have brought to light this fact [...]," (1887c : 589) and he follows with "that is, the character and future development of a biological or ethnological phenomenon is not expressed by its appearance, by the state in which it is (italics in text), but by its whole history. [...] The outward appearance of two phenomena maybe identical, yet their immanent qualities maybe altogether different : therefore arguments from analogies of the outward appearance [...] are deceptive." (1887c : 589)'.

7 Among others, he confuses at least four levels of reality : first, in the quotation mentioned above—and throughout the article—he mingles biology and evolutionary biology. Second, when appearing to deal specifically with evolutionary biology, he further muddles up three separate activities, namely (1) phylogenetic reconstructions and (2) evolutionary theory as such, which encom-

Overall, Boas's theory of culture formation and change does not manifest the slightest shred of Darwinism. Contingency, chance and randomness are indeed features of [181] Boas's cultures, and more specifically of the elements that cultures borrow. As a result, as many have noticed, Boas's cultures are random assemblages of traits, refashioned by the "genius of a people" (Spier 1931 ; Orta, 2004). Speciation for Darwin came from variations arising naturally, not from features borrowed from other species. He dealt with organisms that were not chance collages of mostly borrowed traits. Boas's ideas of randomness, indeterminacy, contingency and such like—which Lewis links to the pragmatists—first stemmed from thermodynamics, and one could more convincingly associate Boas to thermodynamics through the influence of von Helmholtz and mostly Clausius, whose lectures Boas so avidly wanted to follow that he moved to Bonn "for he could hardly hope for anything better anywhere" (Kluckhohn and Prufer 1959 : 7). Boas's historicism thus had nothing Darwinian about it, and was no more inductive than the evolutionists' reconstructions ; in the end, what was his scientific procedure ? This takes us back to the very heart of Boas's early confrontation with the evolutionists, what Stocking rightly identified as the crucial question of classifications.

Boas and Classifications

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In the final analysis, the image of an inductive Boas might ultimately rest on his rejection of all classifications. He did lash out at classifications, an onslaught that started in 1887 and culminated in the masterly *Mind of Primitive Man* and *The Handbook of American Indian Languages* (1911a, 1911b). What he dismissed as "premature classifications" were those that mingled various dimensions of reality such as race, culture and language (and led to identifying cultures with race, for instance) and yielded racist theses ; or those that did not dis-

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passes works on the "mechanisms" of evolution at (a) the population (population genetics) level and (b) the genetic level (mutations, and so on—specifically Mayr's type of work).

sociate phonetics from grammar and lexicons, and yielded questionable linguistic classifications (one of the *Handbook"s* themes). He rightly exploded all those classifications, but his procedure was here deconstruction, not induction or history. This is nonetheless the "methodological Boas" at his best, leaving all classifications in ruins— all but one, namely his own, as his 1887 exchanges with Mason and Powell illustrate.

Unlike most commentators (save Buettner-Janusch 1957), I find Boas's 1887 exchanges with Mason and Powell more than polemical. Most analysts have focused on Boas's statements, failing to place them back fully within the wider context of the exchange. In 1886 Mason had given the question of similarities in cultural phenomena in geographically distant areas an extremely serious treatment, concluding that diffusion played the dominant role in explaining similarities, and subscribing to an inductive method while arguing holistically (1886 :248-50). In a separate article, however, he wrote as museum curator, elucidating the ideas guiding his museum exhibits ; Boas ignored the nuances of the first article, and homed in on the second.

He began his first reply by claiming that Mason attempted "to classify human inventions and other ethnological phenomena in the light of biological specimens", something Mason did not do ; he merely mentioned it as one of the *many* possible ways to study and classify artefacts for museum exhibits. From this selective reading, Boas immediately concluded unjustifiably that "this method of research is founded on the hypothesis that a connection of some kind exists between ethnological phenomena of people widely apart" (1887b : 485). To prove his point he further misrepresented Mason's views, [182] ignoring the predominance Mason gave to diffusion, presenting him as an advocate of repeated inventions, and further stating unwarrantedly : "From this standpoint Professor Mason has arranged the ethnological collections of the national museum according to objects, not according to the tribes to whom they belong" (1887b ; 485, italics added).

He then embarked on his classical attack against evolutionism and the standard explanation of inventions (like causes produce like effects) to oppose his own theory of causality : unlike causes produce

like effects. [8] To Mason's alleged classification in terms of species and genus he opposed his famous claim : in ethnology, all is individuality, and the object of study is the individual phenomenon. From this, he argued that we must study individual phenomena (here, cultural fragments, or cultural elements) by putting them back in their surroundings, which, in Boas's view, meant "[arranging] them according to tribes" (1887b : 485). He was then literally calling for tribal "arrangements".

Mason politely replied that it is impossible to organize museum exhibits without prior classifications, which presuppose what he called some "classific concepts" ("categories of phenomena" : material culture, race, geographical areas, social organization, environment, and so on) (Mason 1887). He acknowledged yet again that objects can be classified in many different ways, and that museographers must select one way according to their museographic objectives (what they want their exhibits to "teach"). He went on with well-pondered reflections on classifications and inventions but, in his second reply Boas further turned a deaf hear to Mason to hammer his own message, emphasizing that we must place "ethnological phenomena" in their various "surroundings", and study "phenomena arising from a common psychical cause among all tribes and as influenced by their surroundings : i.e., by tracing the full history of the single phenomenon" (Boas 1887c : 588, italics added). Boas dubbed this the "inductive" method, accusing Mason of drawing "deductions" by analogy, and repeated that his historical-cosmographical method called for "tribal arrangements of museum specimens" (1887c : 588) although he later specified that the "arrangement" must be physical and ethnical, because such are the surroundings.

At this juncture Mason no longer replied because John Wesley Powell, head of the Bureau of American Ethnology and arguably America's leading anthropologist at the time, stepped in. Like Mason's true views, Powell's intelligent reply has not been given the full attention it deserves, save again by Buettner-Janusch (1957).

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[8] Boas's historians have failed to emphasize how arbitrary it was to single out this theory of causality as the only possible one. The move was utterly polemical, as a "balanced" view of causality would assume that like causes sometimes produce like effects, and like effects sometimes result from unlike causes.

Powell rightly emphasized that Boas had mingled two questions, namely the source of similarities between cultural phenomena in areas wide apart and museum classifications, stressing that he and Mason had adequately dealt with the first (Mason certainly had). Since Boas was really discussing museum classifications, Powell addressed this particular topic, insisting that every curator will privilege a classification of museum material, and that "No sound philophic scholar... will assert that his own system is complete and final, that any *classification or arrangement* is ultimate" (1887 : 612, italics added). He then followed with an insightful assessment of Boas's plan to arrange exhibits by tribe ; how could one ever do so, since tribes have moved, disappeared, coalesced, dispersed, especially after contact ?

Boas had further confused two other things, Powell remarked, namely tribal and ethnical arrangements, without specifying what an "ethnic arrangement" would be. [183] Powell could only conjecture, and asked himself if it was possible to group tribes by "ethnic" classifications. Here, ironically enough, he served Boas the very arguments that the latter later used in his writings (especially in *The Mind of Primitive Man*). How are we going to delineate "ethnic classifications" ? On the basis of races or other physiological attributes ? No such satisfactory classification exists (613). On the basis of language ? It does not delineate ethnic categories. The environment ? It would include radically different cultures. Overall, Powell sums up, the "arrangement [of museum exhibits] by tribes on ethnic characteristics of any kind is an impossibility" (Powell 1887 : 614).

Boas's reply to Powell was most disingenuous. Many museums, he claimed, and ones much larger than Mason's [read : Bastian's Berlin Museum], would have solved the problem Powell raised by "exhibiting a full set of a representative of an ethnical group, and [showing] slight peculiarities in small special sets" (Boas 1887d : 614), and this could be done "without making artificial classifications—only by grouping the tribes according to ethnic similarities. Such are not at all intended to be classifications... The principal difference between the plan advocated by Major Powell..., and that of other museums, is, that the latter exhibit the individual phenomenon, while the former make classifications that are not founded on the phenomenon, but in the mind of the student" (1887c : 614).

Yet, throughout his two long replies to Mason, Boas had repeatedly written of tribal or ethnic arrangements ; he had referred to Mason's classifications by objects as "arrangements" and, like Powell, used the two terms synonymously. [9] Without playing on words, museum "arrangements" of cultural artefacts are *groupings* of elements according to some preselected criterion or criteria, and are thus predicated on a *classification*, as Powell very well understood. What else could it be ? How could "things" arrange themselves, so to speak, without anyone imposing this arrangement ?

First, how could Boas delineate "tribes" or peoples ? He never answered the question. And how could he group them in "ethnic arrangements" ? He answered by intimating the existence of museums that had solved the problem by "exhibiting a full set of a representative of an ethnical group". This does not solve the problem, but supposes the problem solved ! And it would be solved, in the final analysis, because these ethnographers exhibit "the individual phenomenon", not some arbitrary classification ! What is here "the individual phenomenon" ? In his replies to Mason, the individual phenomenon explicitly referred to *individual* museum specimens. If so, the answer would not make sense. Here, the individual phenomenon must therefore refer to a tribe's culture ; then, how would exhibits of individual cultures lead to "ethnic arrangements" or "geographical provinces" without prior classifications, without ordering the data according to some criterion chosen by the museographer ? Boas proffered no answer in 1887, or ever after, but one might argue that "cultural areas" would have been his ultimate answer. Do they solve the problem ?

Boas carried out many distribution studies to reconstitute the history of regional cultures, and thereby delineated what could be called "cultural areas". It seems that he truly believed that sound classifications would "naturally" arise from the facts themselves, and that one ought to let the facts speak for themselves, with minimal intervention on the part of the observer (Harris 1968 : 406). We may thus surmise that he [184] believed his "geographical distributions of traits" and

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[9] For instance, he referred to Mason's "classifications by inventions" and technology (1887b : 589) and, in the same paragraph, rejected Mason's "arrangement" of ethnological collections according to technology (1887b : 588). Many more instances could be adduced.

"cultural areas" to have been precisely the type of classifications that spoke for themselves because they arose out of the "raw" facts. The facts were "out there", would "arrange" themselves naturally to an inductive mind, and would eventually reveal laws (Krupat 1990 : 138).

This calls for some comments. First, none of his distributional studies were possible without prior definitions (Mason's "classific concepts", such as myth, folklore, art, crests, family, marriage, clans, and so on). Second, he considered solved the identification of "tribes" or "peoples", still a notoriously vexatious problem : the Kwakiutl, the Coast Salish, or Bella Coola were all names pegged onto recognizable and geographically delineated entities, namely "tribes" or "peoples". [10] He thus operated as if he had solved the question of tribal classifications, for to name is to classify ; but he did not consider his tribal mappings as classifications, merely as "factual" arrangements ! Third, he superimposed his various mappings on these tribal *classifications* : a trait (the importance given to crests, for example), or a series of them, a style, or texts, are those of "the Kwakiutl", "the Bella Bella", or "the Bella Coola". These mappings regrouped tribes in "ethnic arrangements", or "ethnic classifications", namely "cultural areas" (or "regional cultures") so that the latter did not "arise out of the facts", but were arbitrarily imposed by the observer, as Steward demonstrated long ago (1955).

Conclusion : Boas and Historicity

In other words, Boas's "areas" (Bastian's "provinces") were his own taxonomies. [11] Despite all his claims to the contrary, he imposed on the facts an order based on the similarities or differences of socioocultural phenomena's *external* attributes. His "tribes" and "areas" supposed on his part a number of assumptions about the phenomena

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[10] As late as 1938, in Cultural and Natural Areas of North America, Kroeber stressed the radically unsolved problem of delineating cultural boundaries (1938 [1953] : 5) ; as the greatest specialist of cultural areas and their component cultures, he knew better than anyone else.

[11] Kroeber himself had written : "Culture area classifications were somewhat comparable to the pre-Darwinian taxonomies of the plant and animal kingdoms" (1962 : 16).

to be classified and the attributes selected, as Powell had lucidly argued (1887 : 614-16).

Interestingly, eighteenth-century natural historians understood their taxonomic efforts exactly as Boas understood his. Natural historians sought to discover a classification rooted in nature itself (Foucault 1966 : 152-3) ; "Retrouver l'ordre veritable qui existe dans la nature, voila le but de l'histoire naturelle" (Jacob 1970 : 60), an understanding which Alexander von Humboldt, natural historian rather than physicist, also shared (Bunzl 1996 : 39). But, as students of natural history also know, this order never "arises out" of nature itself but emanates from the natural historian's choice of external attributes.

Words are often deceptive, and there is nothing historical about natural history ; as Foucault and Jacob argued, it actually ruled out historicity. And so did Boas's "historical reconstructions". Boas's cultures were random assemblages of traits, theoretically styled, or patterned by the action of the *Volksgeist*. And his conjectured reconstructions were based on the assumption that external similarities of traits among neighbouring cultures result from diffusion, and expressed a connection in time, without any means of truly knowing where the trait appeared in the first place, how long ago, in which context, where it travelled first and why, and so on. In the end, such history boils down to movement in space, and this spatialization of time rules out true historicity. Like natural history, Boas's program of "culture history" was essentially a-historical, for [185] it lacked historical agents and any grasp of historical processes (save intermarriages or imitation—Wax 1956 ; White 1963).

This foray into questions of induction and deduction are necessary to set the record straight. In reality, much of the American historiography of American anthropology and Boas over the last 40-50 years has been Boas-centric, and led to distorting stereotypes and the very mythologization of Boas himself. True, evolutionism was vitiated by its racism (shared by all research programmes, save Waitz's, to a certain extent, and Boas's), its sociogenetic reconstructions and its orthogenesis. But evolutionists could not be faulted for premature classifications more than Boas, or for deductive reasoning. Everyone classified, everyone argued analogically and by *inference* ; none reasoned either inductively or deductively. Racism aside, the evolutionary narrative was conjectural history, as were Boas's reconstructions. All

these false contrasts conceal Boas's true achievements, such as comparing neighbouring societies rather than disparate ones, detached from their spatiotemporal coordinates (and fighting racism tooth and claw). In this he injected a healthy dose of *empiricism* into American anthropology, but the ethnographic and theoretical superstructures he built upon this empiricism have lost their heuristic value ; they certainly cannot stand as an inspiration for contemporary ethnographic practice and theorizing on the history of societies or cultures.

NOTES

Pour faciliter la consultation des notes en fin de textes, nous les avons toutes converties, dans cette édition numérique des Classiques des sciences sociales, en notes de bas de page. JMT.

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