

Stocking's Historiography of Influence

The 'Story of Boas', Gamio and Redfield at the Cross-'Road to Light'

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Abstract ■ This article is a critical assessment of George Stocking's historiography of influence and his genealogy of Boasian anthropology. Specifically, Stocking argues that Gamio and Redfield and the research projects and forms of anthropology associated with them were influenced by Boas and/or manifested and participated in, what his analyses constitute as, the Boasian school, tradition or paradigm. This contribution to the historiography of the field of anthropology has two objectives. First, the article provides a critique of the attribution of influence, in general, and of Boasian influence in relation to the anthropologies of Gamio and Redfield. Second, the article assesses Stocking's argument for the conceptual unity of the Boasian 'tradition' and 'paradigm', and suggests an alternative concept, that of disciplinary modality. The article concludes with a discussion of Lomnitz's assessment of the mutual interdependency yet studied blindness between the Mexican and Anglo traditions of North American anthropology.

Keywords ■ anthropology ■ archeology ■ Franz Boas ■ George Stocking ■ historiography ■ history of anthropology ■ Manuel Gamio ■ Robert Redfield

Although much of this [archaeological] work was carried on by men without doctorates in anthropology, the leading figures tended to be oriented toward *academic anthropology in its Boasian guise*. [Manuel] Gamio was a student of [Franz] Boas, who helped plan the work in the Valley of México, and [Nels] Nelson came to the Southwest from the Berkeley department. [Alfred] Kroeber and [Leslie] Spier were of course Boas students, and [Alfred V.] Kidder had studied under him. Whatever their feelings about Boas' politics, the Harvard archeologists all came from a department in which two of the three leading figures (Dixon and Alfred Tozzer) were in a broad sense Boasian. And as we have already noted, when Kidder took over the Carnegie Maya program in 1929, its focus was broadened to include contemporary ethnological work as well. (Stocking, 1992: 156–7)

Culture, Genuine and Spurious was a foundation document for the ethnographic sensibility of the 1920s and it is perhaps more than coincidental that the

authors of its three most problematic cases were all strongly influenced by Sapir. (Stocking, 1992: 290)

'The Road to Light' starts out toward Chicago rather than toward Mexico City. (Redfield, 1950: 153)

The historiography of influence and the genealogy of Boasian anthropology

This is a critical assessment of George Stocking's genealogy of Boasian anthropology. In his treatment of Manuel Gamio, Alfred V. Kidder and Robert Redfield, George Stocking has argued that these scholars and their forms of anthropology were influenced by Boas or participated in – what he analyzes in different essays as – the Boasian school, tradition, and/or paradigm. This article does not offer a re-writing or a revision of these histories, only an assessment of these claims. Thus, the analysis contributes a critique of the historiographic use of influence in the writing of the history of anthropology and a critique of Stocking's substantive and conceptual characterization of the Boasian tradition and paradigm of North American anthropology. The first section is a critical reappraisal of Boas's 'influence' on Kidder and Gamio as the intellectual source of stratigraphic methods in archeology and on Gamio as the source of the development of Mexican anthropology. The second section is a critical review of Stocking's thesis that Redfield was Boasian via the proxy of Edward Sapir. In the third section the analysis shifts to Stocking's arguments about traditions and paradigms to propose a different concept of disciplinary coherence of anthropology. The conclusion returns to Gamio, as well as Stocking, within a passage by Redfield that is used as an allegory about the exclusion and submersion of anthropologies by more visible and dominant traditions. This article provides further example of Lomnitz's (2001) assessment that the Mexican and Anglo traditions within North American anthropology are mutually interlinked, yet exhibit reciprocal blindnesses and silences about their enabling exchanges and interdependency.

Histories of stratigraphy and stratigraphy of history: Boas, Gamio, Kidder, Stocking

As is well known, Gamio went to Columbia in 1909 to study under Boas. He was later hired by Boas to do research within the International School of American Archaeology and Ethnology that Boas co-founded in 1910–11 (de la Peña, 1996). Under Boas's direction of the International School, Gamio began an archeology project at Teotihuacan that was the first use of stratigraphic methods in México. In the course of a decade, the study expanded into an integrated research project that incorporated ethnology and other

applied social sciences. By the time this work was accepted as his doctoral dissertation and published, Gamio was already two years into building the foundations of México's post-Revolutionary anthropology.

In his stories of Boas (e.g. Stocking, 1968, 1974, 1992), Stocking emphasizes the influence of teacher on student, not only to trace the genealogy of Boasian anthropology, but also to argue for a Boasian school, tradition and paradigm. Although the place of Mexican anthropology in this story is non-existent, Gamio's role is somewhat crucial if also minor: the influence of Boas over Gamio and other archeologists who first used stratigraphy allows Stocking to make the argument that the emergence of professional archeology, defined through the use of modern stratigraphic methods, was the result of Boasian influence and this, in turn, was a manifestation of the Boasian tradition and/or paradigm. This section has two objectives. First, the role of Boas in archeology is reconsidered through a review of the debates on who was the 'first' intellectual architect/originator of stratigraphy in the Americas. Second, the difference between the anthropologies of Boas and Gamio is synthesized to raise questions about the erasure of Mexican traditions from the historiography of North American anthropology.

First, given the nationalist Anglo American perspective of Boas as a dominating patriarch, one can understand Stocking's raising of Boas through the dismissal of Gamio as the primary intellectual author of stratigraphy. Likewise one can understand the Mexican historiography that gives significantly greater agency and intellectual authorship to Gamio both in terms of the development/application of the stratigraphic methods and in terms of the 'founding' of modern Mexican anthropology and its institutions. Interestingly, however, the historians of archeology position different persons in the role of 'inventor'; for example, the lionizing biographies of Alfred V. Kidder locate him as the 'big man' in the disciplinary narrative.

Beyond these nationalist or subject-driven histories that serve, implicitly or explicitly, to aggrandize certain protagonists, there is some substantive contention as to whom among Nels Nelson, Gamio, Boas, Kroeber/Spier and Alfred V. Kidder was or were the crucial archeologist/s and intellectual architect/s that 'first' applied and developed stratigraphic methods in the Americas (see Browman and Givens, 1996; Comas, 1960; Fitting, 1973; Garibay K., 1960; Givens, 1992: 29-76, 121-9; Stocking, 1992: 156-7; Strug, 1971; Woodbury, 1973: 20-5). On this point Stocking (1992) bypasses the question of 'origins' for that of 'influence' (or diffusion in horizontal and vertical forms) in order to argue that, regardless of which archeologist is 'the man' - and despite their political antagonisms - they 'are all in a broad sense Boasian' (see epigraph above, Stocking, 1992: 156-7).

Stocking therefore does not actually explicitly state, but only implies and asserts through assumption, that Boas is the crucial architect of

stratigraphy. Yet the argument, if it indeed is an argument, is circular: Stocking's assertions that students of Boas were Boasian (i.e. Gamio and Kidder) and that those who were not students (i.e. Dixon and Tozzer) nonetheless 'were in a broad sense Boasian' leads the reader to the conclusion that archeology, especially the 'Kidder directed Carnegie Maya program', was Boasian. This genealogy of influence, however, does not hold water. Logically, the causal relations of influence are only assertions and are not proven, perhaps cannot be. The explanatory value of the concept is zeroed out when we recognize that regardless of the substance of the 'influence', each of these intellectuals transformed 'teachings' or 'readings' into unique intellectual projects. Methodologically, influence is incoherent as a species of diffusion since it contravenes the historiographic principle of (historical) particularism to which Stocking subscribes.

Empirically, the genealogy of influence is also in error. Contra Stocking, Givens (1992: 9, 25, 47–52) notes that Kidder not only had some strong disregard for Boas and his ideas (as well as disdain for Gamio and his archeology),¹ but that his intellectual source for stratigraphy at Harvard was not Boas. Indeed, Givens states that Kidder took only one class with Boas and that Kidder's intellectual influences were the anthropologist Roland Dixon, the Egyptologist Reisner and the art historian Chase (cf. Woodbury, 1973: 20–5, 29–47). What makes Givens's argument (1992: 29–76, 121–9) persuasive is his differentiation of the actual methodological concepts and practices of stratigraphy as designed by Nelson, Kroeber and Spier, and Kidder (see also Browman and Givens, 1996). Patterson (1995: 48), without these technical distinctions, also identifies Reisner as Kidder's teacher of stratigraphy. He further explains that Max Uhle and Reisner were German trained archeologists financed by Phoebe Hearst through grants to the University of California at the end of the 19th century. This fact suggests, on the one hand, that Uhle was the first Americanist archeologist to use stratigraphy and, on the other hand, that Kroeber's source for using stratigraphy was not Boas at Columbia, as Stocking asserts, but Uhle at Berkeley.

Kehoe, without concern for the Boasian genealogy of anthropology, corroborates this idea. She states that, '[t]he German archeologist Max Uhle worked on shellmounds in San Francisco Bay (The Emeryville Shellmound, 1907), introducing state-of-the-art stratigraphic interpretations that bewildered A. L. Kroeber' (1998: 87). Stock (1938: 766, 772), however, notes that Uhle was supervised by John C. Merriam, who taught at Berkeley (early 1890s to 1920) before becoming the President of the Carnegie Institution of Washington (1920–38). Further, Merriam, who received his doctorate in 1893 at the University of Munich, had already used stratigraphic methods in paleontology-focused archeology in Bay-area middens, including the Emeryville Shellmound in the 1890s, some six years before Uhle worked there (1901–3). This suggests not only that Kroeber, as well as his archeological colleague Spier, did not learn stratigraphy from Boas

before he went to the University of California; but, more significantly, that not everything 'German' in US anthropology is Boasian or the result of Boas's insemination of ideas on American soil.

Indeed, if we take seriously Stock in his festschrift article on Merriam (1938: 766, 772), it may be that the 'originator' of the stratigraphic methods in the Americas is actually one of Boas's long-standing political enemies. Significantly, Merriam is a person thoroughly erased from anthropological histories as an intellectual pioneer. Stocking portrays him simply as Boas's nemesis and Browman and Givens only cite him as 'a geologist who was directing a research project' that 'recruited Uhle to participate in the excavations of the Emeryville shellmound' (1996: 82). The erasure of Merriam as an intellectual contributor to archeology is no doubt due to several factors: his proactive support of the Galton Society, Charles Davenport and eugenics; his non-anthropologist status; the attribution of his vision of anthropology to Kidder;² and, finally, his 'bad-guy' role in forcing the resignation of Boas from the National Research Council in 1919.

The confusion in finding the real and true originator of stratigraphy in the Americas points to the power of the 'great man' narrative style of writing the history of anthropology. However, the very idea of stratigraphy itself puts into question the 'influence' and 'great man' narratives, not only in Stocking's genealogy, but also in the 'insider' histories of archeology written from 'within' by archeologists (e.g. Kehoe, 1998; Patterson, 1995; Trigger, 1989; Willey and Sabloff, 1974). Instead of making Boas the patriarch, these 'insider' histories would rather identify their 'own' as primordial ancestors. Among the contenders, Uhle is most often regarded as the first pioneer. The collaborative work of Kroeber and Spier is often noted but then uniformly dismissed as a secondary development. There is always a quick mention of Reisner as Kidder's teacher, in an unpolemical but unequivocal dismissal of Stocking's privileging of Boas; in this way Reisner, too, is discarded as a 'first inventor' since he only taught in the USA and did not conduct research in the Americas. Thus the importance of Kidder is enlarged. These icons of archeology are privileged over any 'outsider' to both the discipline of archeology (such as Boas, Kroeber and Gamio) and the kin group of Anglo-Germanic North American scholars (such as Nels Nelson and Gamio).³

Outside of the two major North American choices of Kidder and Boas, which therefore implies consideration of Gamio, Nels Nelson is the most likely, non-American, non-German, contender in the Americanist historiography of archeology. This may mostly have to do with the fact that, according to Woodbury (1973: 43), 'Kidder gave full credit to N.C. Nelson for introducing the technique in his work in 1914 in the Galisteo Basin [near] Pecos'. 'But', Woodbury continues, 'at Pecos Kidder applied the technique to more complicated materials on a larger scale' (1973: 43). On this point of sources, genealogies, inventors and authority, listen to how Kehoe continues in the quote cited above:

Nels Nelson, who excavated the Bay shellmounds after Uhle without apparently absorbing the lesson of Uhle's method, then learned stratigraphic excavation from l'Abbé Breuil in Spain and introduced it, to much acclaim, in the Galisteo Basin of New Mexico in 1913. Alfred Kidder had learned the value of stratigraphic excavation from his Harvard professor, the Egyptologist Reisner, and selected Pecos to investigate . . . (1998: 87)

While a Frenchman is mentioned in these Anglo North American histories, the Mexican is not even considered: there is not a single reference to Gamio in either the works of Patterson or Kehoe.

Recently, Browman and Givens (1996) have sought to disentangle all these competing claims of to whom to attribute the origins of stratigraphy in the Americas. Although they do consider Gamio in a carefully argued section devoted to him, Browman and Givens ultimately also manifest a similar prejudicial logic and disregard for the Mexican as that evident in Stocking and the 'insider' histories of archeology. On the one hand, they review the substantial literature for the possible sources of Kidder's stratigraphy, as well as his interactions with Nelson and others; they conclude that Kidder creatively and independently developed his own unique stratigraphic methods based on his reworking of Reisner's teaching and knowledge of Nelson's methods (Browman and Givens, 1996: 85-7). On the other hand, they similarly review the much more entangled literature for the possible sources of Gamio's stratigraphic methods. Despite the lack of direct and clear evidence that Gamio 'learned' stratigraphy from any one person, they nonetheless conclude that 'because Boas oversaw the first implementation of the method by Gamio, at least a significant portion of the credit must go to Boas' (Browman and Givens, 1996: 91; cf. Godoy, 1977; Graham, 1961; Strug, 1971; Willey and Sabloff, 1974: 90, fn 2 p. 92).

Why is agency attributed to all the non-Mexican archeologists and passivity and mimicry attributed to the Mexican North American? The lack of evidence that anyone in particular 'taught' or 'influenced' him suggests instead that Gamio creatively pieced together teachings from Engerrand, Tozzer, Chavez, Holmes, Petrie, Boas, Seler, Saville – and maybe even indirectly from Uhle during his 1910 trip with Saville to the Andes – and then brilliantly synthesized and put into practice a method that none of these scholars themselves conceptualized or actualized. In these accounts Gamio, the Mexican, is not an agent and subject of history, only a passive object and receptor of influences and teachings of white males in positions of power in Anglo North American anthropology.

Darnell, from a Canadian vantage point, offers a refreshing option that turns, not to a German nor a European-trained archeologist, and especially not to an Anglo American, but 'south of the border.' In her assessment of the origins of the Boasian four-field approach she makes an oblique reference to this issue of stratigraphy:

Boas may have included archeology more because he thought that it ought to be there than because archeology before the Pecos classification [by A.V.

Kidder] in the late 1920s had much to offer to his overall interpretation of the history and psychology of the American Indian. His most lasting contribution to archeology may well be his encouragement of Manuel Gamio in Mexico. (Darnell, 2001: 322)

In other words, in Darnell's view the 'influence' here is not intellectual, nor causal, and not explanatory, but rather simply a matter of support.⁴ Influence in Darnell is thereby reduced from an analytical-explanatory category with intellectual substance to the point of being simply financial and moral encouragement. If this minimizes the importance of Boas, it is also ambiguous regarding Gamio, since Darnell only implicitly gives Gamio – a Mexican, after all! – credit for stratigraphy. Speculatively, this may be out of a greater unwillingness to give credit to any of the US-Anglo North Americans, that is, to Kidder or to Kroeber and Spier.

One way to 'resolve' this debate about historical origins would be to precisely trace, along the manner initiated by Givens, the different scientific values and applications of the diversity of methodologies and techniques that are all lumped together under the term 'stratigraphy'. Another way would be to track, as Bowman and Givens began to do, how the intellectual networks of scholars propagated, marginalized or eliminated certain ideas as well as certain authors in the development of an emergent 'modern' and 'professional' archeology. Having neither the training nor the goal in this article to pursue either path, I instead propose four points.

First, the question of the intellectual history regarding stratigraphy is still in debate and new perspectives that transcend nationalist myth, disciplinary justification and hero-biography need to be developed. Second, Stocking's arguments for the inclusion of archeology in a Boasian tradition or paradigm based on the assertion of its coherence as a theoretical-practical project forged by Boasian principles is clearly distorted. Third, there needs to be significantly more study of Gamio and Boas's relationship from 1908 to 1920⁵ to rigorously and ethnographically assess the substance and directions of 'influence'. Fourth, the notion of influence is itself the wrong way to go in asking about the history of archeology. The question of stratigraphy clearly points to transnational processes and dynamics of interdisciplinary exchange in the formation of nationalist 'brands' of science. Thus, in addition to the well-known need for histories of anthropology that account for the contextualization of ideas in social events, political economy of institutions, sociological power, factionalism and sex/gender-racial dynamics, we need to rigorously address the intrinsic transnationalism (and transcultural dynamics) of anthropology. We must break out of the nation/al as privileged assumption, methodological convenience and teleology in the writing of the histories of anthropological sciences.

The second question concerns the influence of Boas on Gamio more generally. Stocking does not discuss Mexican anthropology, but in this article it is necessary to briefly discuss Boas's and Gamio's contrasting

anthropologies. In part this is because Stocking's historiography of influence could lead some readers unfamiliar with México to an erroneous perception of this other North American tradition. Gamio, after all, is not really very Boasian, despite the easy characterization of a filial continuity of thought (see Darnell, 2001: 323; Gamio, 1942, 1959; Lomnitz, 2001; Moreno, 1960; Nahmad Sittón and Weaver, 1990; Olivera de Vásquez, 1965; Warman, 1970). It is true that Gamio introduced Boas's concepts of culture and cultural relativism into the Mexican context; both, however, were germinating seeds of a politics, ideology, practice and science of *Indigenismo* that had neither parallel nor homologue in Boas. Clear and dramatic divergences appear if one considers the two anthropologists' different conceptual and practical approaches to race, nation, anthropology, science – not to mention intellectual and institutional allies (see de la Peña, 1996; Godoy, 1977; Lomnitz, 2001: 256–8; Walsh, 2001; Weber, 2001).

Differences between Boas and Gamio began to develop at the end of the First World War and the Mexican Revolution. Gamio began to distance himself from Boas and the International School with the purpose, Guillermo de la Peña (1996) argues, of both retaining intellectual rights over the archeological material of the Teotihuacan excavations and navigating the anti-Boas/anti-foreigner politics of the revolutionary period. Yet there may be other reasons. Nothing has been published regarding Gamio's possible role as the source of Boas's information on anthropologist-spies, or even the possibility that he was involved in counter-intelligence; such activities might have added to the increasing distance between the two men.

Other obvious reasons may include Gamio's increasingly positive view of and support for eugenics, which he stylized within the ideologies of *mestizaje* (racial mixing) and *Indigenismo* as projects of national integration. Boas, as is well known, spent considerable energy in attacking eugenics, especially the ideas and policy proposals derived from Charles Davenport, the Eugenics Record Office and the Cold Spring Harbor Genetics Lab, all three of which were sponsored by the Carnegie Institution of Washington (CIW) (see Kevles, 1985; Stepan, 1991: 145–53; Walsh, 2001). Indeed, after becoming director of the new federal office of archeology, Gamio began to connect with the CIW on the basis of these shared interests in eugenics and archeology. After becoming friends with Sylvanus G. Morley, a Carnegie researcher and one of the archeologist-spies that Boas denounced in 1919 (see Castañeda n.d.a ; Patterson, 1995: 60; Sullivan, 1989), Gamio helped obtain the Mexican approvals by which the CIW inaugurated, in 1923, a second phase of its Maya research program. Thus, Gamio seems to have definitively distanced himself from Boas on personal, professional, political, institutional and intellectual levels. In turn, Boas abandoned a decade of work when he finally left Mexico and forever after turned a blind eye to Mexico and Latin America.

Another reason for contrasting Boas's and Gamio's anthropologies, along the lines just mentioned, is to anticipate the later discussion of Stocking's arguments about traditions and paradigms. In sum, whereas Boas fought to professionalize anthropology in the university as a science separate from the state and the politics of nationhood, Gamio created an anthropology that was directly tied to the state as the scientific agency and means of both 'good government' and nation-building (or, more precisely, 'national-modernity' building). These contrasting visions of the discipline can be characterized as 'university anthropology' and 'governmental anthropology'. Although both are forms of governmentality aimed at the public spheres of culture, the former seeks primary alliance/funding from the private sector and the latter from the state. University anthropology, for Boas, was a 'four-field approach' to the salvage anthropology of '*vanishing* Indian' cultures as a scientific problem of documentation. In contrast, Gamio's governmental anthropology was an 'integrated approach' to the applied anthropology of the '*persisting* Indian' race as a social problem of nation-building.

Thus, there is something of a family resemblance. But neither the similarity nor the divergence of these modes of anthropology can be explained or understood within a historiography based in influence that seeks to construct legacies and genealogies. The historical issues to do with the rise of North American anthropologies raised here demand diverse analyses that explore the political economy of institutions, the sociological bases as well as targets and objectives of power, and sex/gender and racial dynamics of institutions and intellectual communities. If, however, the contrastive similarities of Gamio/Boas were to be investigated as discursive practices and projects, then it would be worthwhile to bring Vasconcelos into the analysis as a third counterpoint: the latter's concept of 'cosmic race' (Vasconcelos, 1996; cf. Gamio, 1926; Kevles, 1985; Knight, 1990; Lomnitz, 2001; Stepan, 1991; Walsh, 2001; Weber, 2001) has crucial affinities and differences to both Boas and Gamio as all three are creatively contesting, yet engaging, Continental evolutionism, positivism and eugenics.⁶

Claudio Lomnitz (2001: 228–62), in an essay charting the history of Mexican anthropology, points to a curious relationship between the national anthropologies of Mexico and the USA. He characterizes this as a mutual exchange of ideas and interactions that is mostly marginalized and effaced in the public histories of self-representation by both participants. The writing of the histories of stratigraphy in Anglo-US North American archeology is exemplary of this elision of Mexicans, México and Mexican anthropology. Certainly the Stocking historiography of Boasian anthropology not only ignores the Mexican/Gamio contribution to US-Anglo anthropology, but marginalizes and erases the Mexican national tradition as a whole from consideration of North American anthropologies. Likewise, there is a similar disdain for discussing the 'influence' of US anthropology and anthropologists like Boas on Mexican North American

anthropologies, such as the governmental anthropology of Gamio. Certainly, Gamio's two terse two-page acknowledgements of Boas (1942, 1959) as the supervisor of his Teotihuacan project in a festschrift volume dedicated to Boas is 'loud' evidence of this disdain. Lomnitz is no doubt accurate in identifying this reciprocal effacement in the face of actual, substantive and sustained intellectual exchange and interaction. The myopic working relationship between Boas and Gamio may be a crucial and 'originary' moment for this institutional and disciplinary dynamic that Lomnitz identifies.

The retreat of Boas from Mexico, we may note, left open a space for forms and modalities of anthropology to continue to develop outside of the Boasian school, tradition, paradigm and disciplinary-institutional framework. These were transnational 'experiments' in anthropology in which non-Boasians, often concerned with political economy, engaged Mexican anthropology/ists. Both of these interlocutors are excluded from Stocking's genealogy of the Boasian paradigm.

There were other non-Marxian/materialist inter-traditional exchanges as well. For example, one major non-Boasian and yet heteronomous experiment in anthropology occurred in Yucatán. As early as 1916, Gamio recognized the Maya world (specifically the area of Quintana Roo) as one of three crucial limit-cases for the 'urgent nationalist work' of integration. Perhaps it was this vision that motivated him to pursue a relationship with Morley, Merriam and the Carnegie Institution in order to facilitate the establishment of a long-term research project at Chichén Itzá under the auspices of the Carnegie Institution of Washington. Although Gamio was not long in any governmental position to supervise this work, the CIW project was eventually expanded to include a pan-scientific program of research that had very definite parallels, as well as differences, to the modality of governmental anthropology that Gamio envisioned. Interestingly, Stocking has sought to write this Carnegie program of research into his story of Boas as part of the Boasian paradigm.

Boasian Chicago: 'Influence, genealogical or speculative?'

And as we have already noted, when Kidder took over the Carnegie Maya program in 1929, its focus was broadened to include contemporary ethnological work as well. (Stocking, 1992: 156-7)

As part of the organizational changes of humanist/social science research within the Carnegie Institution, the Chichén Project was augmented by a multitude of other scientific projects in medicine, botany, geology and geophysics, astronomy, climatology, history, anthropometry, physical anthropology, linguistics, ethnology and sociology, to mention just a few. Stocking's argument, which does not extend to much more than the implied assertion and assumption quoted in the epigraph, is that the

Carnegie Maya program also became Boasian due either to the fact that Kidder was influenced by Boas and therefore Kidder's research design of the CIW program manifested diagnostic Boasian traits by the sheer inclusion of ethnology, on the one hand, or that the inclusion of 'contemporary ethnology' transformed a narrowly defined archeological project into Boasian anthropology on the other hand. The above discussion argues against the idea that Kidder developed a Boasian archeology or a Boasian vision of anthropological research. This section therefore considers the second possibility, that is, that the Carnegie ethnology project directed by Robert Redfield was Boasian by either influence, confluence and fortuitous contingency, or by intellectual design. It needs be underlined that Stocking does not make an explicit argument on this and many points. He argues by assertion, inference and soft rhetoric of suggestion. Thus, in a different essay, Stocking argues that Redfield was influenced not by Boas per se but by Edward Sapir – one of Boas's most famous students and originator of a unique intellectual tradition in linguistics. Thus, due to the influence of Sapir on Redfield's work, specifically his dissertation project and first book, he participated in either a Boasian school or tradition. The significance of disclosing the inaccuracy of this set of ideas and assumptions is assessed at the end of the section and again in the conclusion to the article.

In his introduction to the Redfield–Tax correspondence, Rubenstein (1991: 5–8) states that Redfield was not a Boasian and that his thought developed outside of the Boasian tradition. What is of interest here is the brevity of the comment and the finality of its tone, which one may suspect is specifically addressed to Stocking. The relevant paragraph from Rubenstein can open up this issue for assessment:

[Fay-Cooper] Cole, who had trained under Boas at Columbia University, headed the anthropology section of the joint sociology and anthropology department at the University [of Chicago]. Other anthropology faculty members at the University, like Edward Sapir and Manuel Andrade, were also Boas students. Redfield, however, was most greatly impressed with the work of the Chicago sociologists who were developing an ambitious program of research on urban social life, and these scholars had the strongest influence on Redfield's training. Thus, unlike most of his anthropological contemporaries, Redfield was not a Boasian . . . (Rubenstein, 1991: 6)

Having asserted a distinct theoretical line of intellectual descent, Rubenstein then succinctly summarizes three specific differences of Redfield and Boasian anthropology. These are designated as methodological, not theoretical, differences:⁷

In doing this [Tepoztlan] research, he departed from his contemporaries in at least three important ways. First, his study of Mexican peasants was unique for its focus on contemporary life, rather than remembered culture. Second, the manner in which he conducted his study – residence among the people whom he studied, and learning their language – departed from the then more common pattern of brief fieldwork stays using the aid of an interpreter. Third,

he focused on a community embedded in a complex social and cultural context, rather than on a well-bounded, relatively isolated social group. (Rubenstein, 1991: 6-7)

Thus, Rubenstein flatly states that Redfield was not Boasian, despite being located among and partly trained by Boasians at Chicago (Fay-Cooper Cole, Edward Sapir and Manuel Andrade). In a formal sense, or in terms of the genealogy of academic kinship, Redfield is a second-generation student of Boas (that is, a student of a student of Boas) and, furthermore, by such reckoning he is 'doubly Boasian'. He not only took classes with Cole and Sapir at Chicago, but he was an informal 'student' of Gamio's, whose teacher was Boas. It was Gamio who both connected Redfield with Tepoztlan as a research site for his doctoral dissertation and inspired Redfield with a vision of a research project that articulated their shared interests. Significantly, however, Rubenstein's comment points out that neither proximal affiliation nor 'descent' necessarily entail influence, a genetic continuity, or unmodified transmission of ideas. Nonetheless, Stocking, in his assessment of the 'romantic' impulse of the cultural critique stylistics in 1920s ethnographies (1992: 276-341),⁸ gives reason to re-open the question of influence and the possible existence of a 'Boasian Redfield'.

Varieties of influence: not everything German is Boasian

In his essay that charts intellectual influence as the way to define the Boasian tradition and paradigm of anthropology, Stocking argues that:

Culture, Genuine and Spurious was a foundation document for the ethnographic sensibility of the 1920s and it is perhaps more than coincidental that the authors of its three most problematic cases were all strongly influenced by Sapir. (Stocking, 1992: 290)

Within the broader agenda of the essay, this influence by Sapir is evidence and proof of a Boasian school or tradition. The three ethnographies on which Stocking bases his arguments are Redfield's *Tepoztlan*, Benedict's *Patterns*, and Mead's *Coming of Age*. Why are these 'problematic cases'? Perhaps because each author initiated a powerful and unique intellectual contribution with their study. In the particular discussion of Redfield, Stocking does not support his thesis, which is not definitively or unambiguously argued. Stocking's broad argument is to chart the emergence of a Boasian tradition of fieldwork premised on a model of humanist/moral science that exists in dynamic tension with fieldwork based in natural/physical models of science.⁹

Within this agenda, Stocking's goal is to characterize the emergence of a 'second-generation' Boasian *school* of the 1920s, which in turn constitutes a *tradition* of humanist fieldwork along the lines of Mead, Benedict and Redfield. With regard to Redfield, then, the thesis is that: first, it is 'perhaps more than coincidental' that Redfield was 'strongly influenced' by Sapir

and/or that the ethnography *Tepoztlan* is 'strongly influenced' by Sapir's essay; and, second, therefore, that Redfield's ethnography (referring to his first book but also implying his style of anthropology) forms part of the Boasian a.k.a. Sapirian tradition of humanist ethnography cum social critique. The conflation of Boas and Sapir has concerned others and is therefore not discussed at length here; we only note that the same problem of the historiographic use of influence to create legacies and genealogies is at work.

Stocking, in the pages devoted to Redfield, attributes the most significant intellectual influence on Redfield to be German sociology; he also acknowledges this to have come through the teaching of Park and Burgess, but not Sapir or Cole. Boas, on the other hand, was not reading or being 'influenced by' German sociology. Despite his frequent trips to Europe, Boas was not reading, or not making much out of reading, the German (and French) sociologists that had constituted the modernist sociological imagination of the early 20th century – for example, Simmel, Tönnies, Weber, to say nothing of Durkheim, Marx and the early Frankfurt School. It seems that the Chicago-based Boasians (Cole, Sapir, Andrade) were also not engaging this sociological literature. Boas is simply not a sociologist, and he developed a social science – ethnology – in distinction to the German sociological tradition. In contrast, Redfield is indeed a sociologist who developed an anthropology out of a specific sociological segment of scholarship within the German tradition of moral/idiographic social science that is different, yet related to, and occasionally intersecting with, what has come to be identified as the Boasian school or tradition of anthropology.

Although it could be said that both belong to the broader intellectual tradition of German idiographic science, they do not share the same locations in it. Redfield, for example, more unambiguously 'belongs to' a German tradition of idiographic 'moral science'; that is, a social science based in assumptions of idiographic or historical particularism that focuses on the 'moral' (values, meanings, beliefs) as object of study. Thus, Redfield's ethnographic and early work is particularist in these two senses. The development of a 'scientific impulse' in Redfield's later work on acculturation and theorizing of the Great and Little Traditions of Civilization only approaches a nomothetic search for generalizable laws and ideal types in a Weberian frame; and it always retained focus on meaning and values. In contrast, Boas might be characterized as 'belonging to' a 'moral' idiographic science. In moving from his natural-physical science training, a 'nomothetic' model of science was not so much erased as relocated within, and in dynamic tension with, his idiographic approach to the historical/cultural particularism of meanings and values. Thus, Stocking has tried to trace the combined presence of 'romantic motivations' and 'scientific impulses' in Boas, beginning with the latter's article on geography (Boas, 1996; Stocking, 1974: especially 9–18, 1992: 330–41; see also Lewis's [2001, and commentaries] argument for Boas's use of Darwin).

Yet such categorizations do not specify sources for either intellectual, and tend to obscure critical differences and conflate issues in the use or critique of influence. The issue of sources is difficult here for the nuances at stake that can be lost due to easy simplifications. Thus, although Bunzl states with certainty that 'Dilthey directly influenced Boas, who on several occasions cites his work' (Bunzl, 1996: 27, 51), Kluckhohn and Prufer (1959) note the difficulty of disclosing influences on Boas since '[h]is bibliographies are generally sparse or devoted to documentation of concrete points rather than to making clear the sources of his basic philosophy of science or broad intellectual influences' (Kluckhohn and Prufer, 1959: 5). Thus, Stocking (1992: 122), in 1976 and prior to Bunzl's work, is unsure of the evidence on Boas's reading of Dilthey, but asserts that he must have been reading the hermeneuticist, among others of the German 'historicist tradition'. It is not clear what else Boas was reading in the emergent German moral social science, much less what use he made of it.

Stocking himself defines Redfield's intellectual sources as Park and Burgess. Through these Redfield is said to have incorporated ideas of American pragmatists such as Dewey and James, German sociologists such as Simmel, Tonnies, Sumner and Windelband (Stocking, 1992: 303, 305-6), and his contemporaries such as William Rivers, Clark Wissler and Sapir - but not Boas (1992: 305-6). As outlined in essays by Bunzl (1996), Massin (1996) and Liss (1996), the German tradition of counter-Enlightenment in which Boas was trained is defined by both Humboldts, Herder, Ranke, Ritter, Ratzel, Haeckle, Virchow, Fischer and Bastian. Despite this difference in specific lineage, both Boas and Redfield shared an appreciation of American Pragmatism and incorporated elements of it in their work (e.g. Lewis, 2001, and commentaries). What is clear in any case, regardless of shared 'influences' (rather, sources), is that both created an intellectual oeuvre that had a unique originality and seminal force that resulted in specific traditions; this fact makes typing either into a broader tradition a kind of distortion that is only merited for specific, limited analytical goals or insights.

'He influenced himself': influence and non-influence

Turning now to Stocking's more specific argument, we note that Stocking (1992: 302-3, 305-6) cites the earlier work of Godoy (1978). Godoy makes a close reading of Redfield's *Tepoztlan* proposal, its funding context and the research diary, in order to identify, among other points, the crucial importance of Gamio as a practical and intellectual guide for Redfield in the formulation of his research in México. Despite their ultimate and later divergences,¹⁰ it must be asked if this constitutes 'influence'? While acknowledging Gamio's role in 'suggesting' Tepoztlan as Redfield's field site (1992: 304), Stocking bypasses the issue of Redfield having been 'influenced' by anyone other than his teachers; Mexicans, such as Gamio and the other *Indigenista* intellectuals (e.g. Elena Landazuri, Diego Rivera and

Frances Toor) who 'introduced' him to México (Stocking, 1992: 302), are not mentioned as intellectual sources, only as logistical aides.

In a curious and revealing sentence, Stocking notes that '[o]bserving Gamio at work in the field, Redfield decided to become an anthropologist' (1992: 302). Gamio's active role in 'affecting' and 'influencing' Redfield is disrupted and dissolved. Stocking's choice of syntax, as well as analysis and historiographic explanation, does not attribute agency to Gamio, here or in relation to Boas. The meaning Stocking conveys is not that Gamio influenced Redfield, but rather that Redfield 'influenced' himself via the contemplation of the passivity of Gamio. Redfield has agency and the Mexican is a reflective object of thought. The point is not to assert the 'influence' of Gamio and/or other Mexicans over Redfield (nor Boas), which Godoy has already accomplished, but to point out the conceptual vacuity and its ideological usage in Stocking's historiography.

In contrast to Stocking, Godoy rather explicitly points to Redfield's synthetic theorizing of culture as an original invention interactively independent from Boasian anthropology. The exception of Boasian 'influence' that Godoy notes is not Sapir's work, but Wissler's 1923 book. Godoy discusses how Redfield influenced himself (as it were) with Wissler's ideas; his incorporation of Wissler was based on a synthetic framework based in the teachings of Park and Burgess and readings of Tönnies, Durkheim, Morgan, and Maine (cf. Redfield, 1941: x). Stocking, on the other hand, asserts that 'Redfield regarded Clark Wissler's *Man and Culture* as "the best piece of anthropological writing" he had read and *Tepoztlan* is clearly influenced by Wissler's concepts' (Stocking, 1992: 305). This influence of Wissler on Redfield manifested, according to Stocking, as the spatializing theories of cultural elements. This appeal to the reader by what is asserted as obvious is thereby converted into more evidence that Redfield is Boasian and part of the Boasian paradigm (Stocking 1992: 305-6; cf. Redfield, 1941). Yet the spatialization of culture was not to reach its full theoretical plenitude until the Folk-Urban Continuum nearly 20 years after *Tepoztlan*. Furthermore, in this first book, Redfield's spatialization of culture is only a moral mapping or mapping of moral judgment as both ethnographic description and analytical commentary. We might very well ask here, in the light of Boas's historical particularism that Stocking himself often champions, why is it that the differences between theoretically diverse concepts of the spatialization of culture – that is, concepts enmeshed in different if not also antagonistic theoretical projects – have no historical value for Stocking? The differences between such concepts must be reduced in order to create the continuity of legacy and genealogy as well as to eliminate from view the diversity of antagonistic contending conceptions of the spatialization of culture that also derive from German anthropologists.

Thus, we reach the crux of Stocking's assimilation of Redfield into a Boasian tradition. The argument comes to rest on the idea that Redfield exhibits a romantic cultural critique that is derived from Sapir's famous

manifesto. Yet, Stocking is slippery, as he refuses to state directly when he can instead lead the reader to his inference. His text therefore culminates in an ambiguous assertion that suggests his thesis and seems to offer supporting evidence in the form of the citing of another authority. Stocking argues that Park was influential, '[b]ut, Redfield showed a greater tendency than Park to romanticize the *gemeinschaftlich* organicism of the folk community, and the relation [of Redfield's spatial binary of folk/modernizing Indian] to Sapir's dichotomy of cultures was subsequently suggested by one of Redfield's students' (Stocking, 1992: 306; cf. Tumin, 1945). But what kind of 'relation'?

The quote by Redfield's student (i.e. Tumin) that immediately follows defines a *theoretical* relationship, not a relationship of 'influence'. It is stated in a theoretical 're-evaluation' (phrase from the subtitle of the essay) of Sapir's concept of genuine/spurious culture that seeks to rejuvenate Sapir's concept by explicitly linking it to Redfield's theory of folk cultures.¹¹ The relationship that Stocking quotes is one in which Tumin (and not Redfield) asserts that genuine culture is defined as folk and non-folk culture as spurious. This, however, would not be Redfield's location of a 'genuine culture' given the topography of cultural places that he theorizes as the Folk-Urban Continuum (1941): if this vision of Sapir's binary must be imposed on Redfield, it would be the politically independent 'tribal' Maya of Quintana Roo, studied by Alfonso Villa Rojas, that are 'genuine' in opposition to the spurious culture of urban modernity and not the folk society. For Sapir, it is not folk society – ambivalently caught between modernity and non-modernity, as described by Redfield – that is genuine, but authentic tribal culture exemplified, as even Stocking (1992: 290–2) himself argues, by the Pueblo Southwest.

As Stocking often reminds us, it is pertinent to remember Boas, especially here and especially one of his favorite precepts: similar facts can have different causes. Or, in the present context, a later identification of a similarity between two concepts (especially for purposes of theoretical synthesis) does not imply that the two concepts had the same origin or that one is causally derived from, inspired by or modeled on the other. If Redfield was reading Durkheim and other German sociologists who were focused on modernity, there is no immediately available reason to suspect that Redfield was somehow conspiring to hide Sapir as the true source of his cultural critique. Indeed, the coupling of the romanticist rejection of industrial capitalism with the modernist privileging of pre-modern social forms/peoples was not unique to Boas nor Boasian anthropology, but was pervasive in European thought; also it had diverse, if often interrelated, sources. The bottom line is that Stocking does not make the case for Sapir's influence on Redfield in terms of the establishment of a 1920s ethnographic sensibility. Instead, he effectively argues for the influence of Park's teaching as the means by which Redfield accessed a variety of theoretical work that he then synthesized in a brilliantly novel, if also problematic,

way.¹² If Redfield is therefore not influenced by Sapir in any manifestly meaningful way, he is also not influenced by Boas in any sense of the term.

'Academic anthropology in its Boasian guise': traditions, paradigms and modalities

I made a fairly self-conscious effort to apply the paradigm notion, worrying whether the social sciences were 'pre-paradigmatic' and to what extent they practiced 'normal science.' But I always regarded Kuhn's work as heuristic rather than definitive, and have been inclined to treat the idea of paradigms as a resonant metaphor, to be applied flexibly when it seemed to facilitate the understanding of particular historical episodes. . . . I have recently found it convenient to think in terms of 'paradigmatic traditions' . . . [since] it seemed that the paradigm notion, which in Kuhn's formulation emphasizes synchronic discontinuity, needed to be modified to allow for paradigm-like bodies of assumption that perdured through long periods of time. (Stocking, 1992: 344)

In bringing his stories of Boas together as a unified argument, Stocking argues that the 'Boasian school' developed into a kind of 'diachronic continuity' of assumptions which he variously conceptualizes as a tradition, paradigm or a paradigmatic tradition. The critique of this historiography so far has focused on the influence of Boas on three figures (Gamio, Kidder, Redfield), a kind of archeology (stratigraphy) and a kind of 'sociological' anthropology. The effort to dismantle the analytics of influence is both a vertical critique of diachronic continuity and horizontal critique of affinity and fictive kinship of thought. What are the implications of this reappraisal for the unities of traditions and paradigms thus created by Stocking's genealogical history?

Beneath the story of Boas

The critiques above have rejected the ideas of a Boasian archeology and a Boasian Redfield. But, what exactly are those things? These are slippery targets because Stocking himself is unclear about what he is stating about Redfield and archeology. If we consider only a 'Boasian Redfield' for the moment, we need to recognize, on the one hand, that the statements are based on a reductionist characterization of this complex anthropologist to the Tepoztlan field research and book; significantly, these were conducted under an SSRC grant in conjunction with the SSRC funding of Gamio's 1920s study of Mexican migration to the USA. Not only are Gamio's migration study and the Chan Kom 'Redfield' of the CIW not discussed, but the later Redfield, the comparative generalist of civilizational traditions, is not even considered as he falls outside Stocking's periodization. On the other hand, in rendering the Tepoztlan 'Redfield' emblematic, with a touch of 'Boasian'-styled acculturation studies, he elides explicit statements and leaves many points implicit or assumed; he uses a rhetoric that only indirectly argues his points.

In the essay on ethnographic sensibility (original date 1989), the substantive part of the argument is to define a Boasian school via a shared ethos of ethnographic fieldwork, that is, of cultural critique *avant la lettre*. This provided the foundation for Stocking to then advance a thesis about the existence of a Boasian tradition centered on culture, especially cultural determinisms and relativities, and a critical humanism in dynamic tension with a subordinated/submerged nomothetic-scientism. In both cases, the counter argument here is that Redfield represents an independent yet related mode of anthropology. Further, rather than Redfield being or becoming Boasian, it seems more plausible to argue the reverse: that the generations of Boas's students after the 1930s borrowed and adapted from Redfield to become – as it were! – Redfieldian. Stocking's 1976 essay on 'ideas and institutions' (1992: 114–77) as well as the 1985 'philanthropoids' essay (1992: 178–211) provide evidence for this in which genealogical inversion influences convert analytical into political alliance for funding.

Part of Stocking's essay charts the intellectual and institutional shift from a straightforward Boasian *ethnology* pursuing 'elements', 'process', distribution and diffusion to a *cultural anthropology* addressing structure, system, pattern, integration, psychology and acculturation (Stocking, 1992: 134–49). Further, the terminological replacement of *ethnology* with *cultural anthropology* – Stocking dates the completion of this process to 1945 (1992: 148) – entails, or is actually enabled and conditioned by, crucial disciplinary changes in the articulation of the subfields and the discipline as a whole to the university. There is a dramatic transformation of primary funding of university research to an emergent public sector of governmental philanthropy in which private capital is converted into nonprofit corporations and supplemented by state sponsorship. The Rockefeller funding of social sciences and its financing of the University of Chicago are exemplary. Stocking also notes, given his double focus on 'ideas and institutions', that this change occurs along with an uneven displacement or reframing across the four subfields of the thematic of 'the historical' in relation to culture as the unifying problematic that defines 'Boasian' anthropology (1992: 149).

On the one hand, there was both a continued and increased disciplinary specialization leading to the famously divided four fields and a consolidation of the discipline's realignment away from an explicitly dependent relationship on museums and the state. The latter entailed a struggle to position anthropology within the institutional setting of the university as a legitimate and real science among the 'hard' social if not also physical sciences. Comparative generalizing within at least a nominative scientific search for laws or policy-practical 'social engineering' was a basis for successful political insertion of anthropology within the then ever-growing private and governmental sectors of funding (Stocking, 1992: 178–211). Significantly, state funding includes, beyond explicit funding, major covert CIA co-funding of social research (see Price, 2000).¹³ In this

fluctuating context, Redfield was a key player. As Dean of Social Science at the particularly privileged University of Chicago, he actively participated with governmental and philanthropic agencies in the design of funding agendas for the social sciences and anthropology, from the days of the Hanover meetings of the SSCR to the post-Second World War negotiations of science in America.

Given his role as a powerful advocate in this area of funding agendas and being an intellectual force closer to Kroeber and Mead than to Wolf and Leslie White, it seems at first reasonable, but quite misleading, to insert Redfield into the genealogy of Boasian anthropology. It would be more accurate to argue for a wedding and mixing, if not a mutual 'acculturation', of Boasian and Redfieldian anthropologies (e.g. Redfield et al., 1936; cf. Stocking, 1992: 134–49). In other words, political affiliation and institutional alliance, based on rough intellectual affinity, are better descriptions of how Redfield, his anthropology, Boas's students, and their anthropologies articulated to each other in an era of increasing theoretical pluralization (1930s–70s) and in the face of multiplying materialist and militantly nomothetic theories of culture. As Stocking argued, in the aftermath of Boas's 1919 letter to *The Nation*, the Boasian 'school' was first of all a political faction that fought for a space of its own in the university against museum and government anthropologies that advocated racial theories. In mid-century, the intellectually diversifying Boasian heirs had to ally themselves with Redfield and Redfieldian sociological anthropology in a new disciplinary struggle that led to the consolidation of a 'Boasian' tradition of a four-field, university anthropology.

In such a case, it would be better to consider this 'Boasian' unity as a disciplinary modality, that is, a unified and integrated mode of constituting the discipline of anthropology within the institutional framework of the university and its cross-cutting articulations to the governmental and philanthropic private-sector funding agencies. It is not a tradition in the intellectual sense of that term, nor a paradigm in the sense of an intellectual-theoretical unity. It is a disciplinary modality constituted in real struggles for resources, and thus intellectual capital and privilege. If one considers a notion of paradigm that gives more significance to methodological strategies of investigation as the means of the articulation of theoretical discourses and research problematics, it would seem that Redfield, as Rubenstein notes above, was a unique contrast to the Boasian school; and, it is more accurate to say that the anthropological research undertaken by Boasians became more modeled on the 'sociological' ethnography first initiated by Redfield, than to say that Redfield adopted a Boasian modality of fieldwork. Indeed, Redfield's theorizing, especially after his second ethnography, the famous study of Chan Kom, had little to do with Boas, Sapir, or the Boasian intellectual framework. Further, this disciplinary modality in which Boas is attributed the ancestor role is well bounded as an historical moment. It could be dated from the inter-war period to the

beginning of the 1970s, when the theoretical vision of holistic science based in a happy dialectical and 'ecliptical'¹⁴ integration of the 'romantic motives' and the 'scientific impulse' began to fracture and eventually rupture into a new struggle over internal and external resources and the cultural capital of academia.

On the other hand, if Redfield and archeology were not and never Boasian, what does this imply for the idea of a Boasian tradition and paradigm? If the Boasians, in the contexts of their struggles over funding resources, symbolic capital of academia and the definition of legitimate university science, appropriated and incorporated Redfield and archeology into an intellectual and disciplinary alliance, respectively, what does this imply for the writing of the history of the field? Perhaps the historiographic genealogy of a Boasian paradigmatic tradition is best understood as a fictive genealogy coined in relation to these interdisciplinary struggles between divergent practical and theoretical visions of anthropology. Thus, the adjective 'Boasian' becomes less a sign of intellectual continuity in time or space and more a banner of political alliance within a specific disciplinary and institutional context. Further, Stocking's (1992: 344) 'paradigmatic tradition' of 'the classical period of cultural anthropology' seems less an intellectual unity than a specific modality of an academic discipline shaped by its particular location within a governmentalized public sphere of the university and foundation funding.

Beyond the story of Boas

The historiography of influence traces continuities of thought, primarily in the register of theories, concepts or approaches. It is on this basis that unities such as schools, traditions and paradigms are formulated and identified in the history of science. This is clearly the organizational schema by which Joan Vincent (1990) derives her typology of six theoretical traditions (cf. Stocking, 1992: 342–5). But it is also clearly the case with the idea of Kuhnian paradigms of normal science, in which the disagreements and discord are precisely the basis of a larger unity and coherence, as such debates require as their enabling condition not only consensus of pre-assumptions but shared frameworks of institutional power and communicative media. What is needed is an injection of sociological criteria, as per above, in the definition of the object in order to disclose other types of unities that are not represented in such monolithic and asociological analyses.

It is suggested that a notion of 'disciplinary modality' would be useful for specifying the distinctiveness of the academic anthropology that emerged in US universities from the 1920s on, based on government and philanthropic funding to study culture in a four-field framework of holism. This idea allows for the inspection of modes of anthropology in terms of sociological factors that are usually not considered in histories of science based on genealogies of ideas and legacies of influence.

The idea of metropolitan versus peripheral traditions that has been used to discuss national anthropologies seems to move in this direction (e.g. Gerholm and Hannerz, 1982; Lomnitz, 2001; Stocking, 1982). Yet the typology glosses over, if not eradicates, the specificity of internal alternatives and thus tends to homogenize variations and deviate strands in order to define the national tradition. This is especially clear in definitions and analyses of anthropologies as 'colonial-empire building', or 'nationalist/nation-building' and even as 'modernist' or 'postmodernist'. The national tradition therefore ends up conceptually close to a paradigm, even if it is understood now within an 'international' (versus 'transnational') framing of intellectual exchange, hierarchies of status, legacies of influence, funding strategies and formulation of the object-Other of study. Indeed, Stocking's (1992: 346–51) periodization of 'paradigmatic traditions' fluctuates between the use of theoretical continuity and national affiliation as criteria of coherency; in turn, these lose their force of differentiating unities when articulated to the one, hegemonic criterion that he employs as the telos of his analysis.

Ultimately for Stocking, the 'story of Boas' is the story of anthropology. Both are subsumed as subtexts or pretexts to the story of the continuity of the dynamic duo of scientific modalities (nomothetic/scientific impulse versus the idiographic/romantic motive). In turn, the focus on these theoretical 'motives' as theoretical legacies effectively obscures the enabling social conditions and political agendas that shape them into historically particular motivations of a different kind. That is, if the reader can excuse the teratological hybridization of Stocking and Foucault, these are 'motives to knowledge' based in 'motivations to power'. The historiography of influence that seeks, consciously or not, to cover over the specificity of the sociological, political, and economic conditions and agendas of theoretical traditions, manifests itself as yet another will to power via the production of specific kinds of knowledge and discourses. This suggests that the story of Boas needs to be re-told from the perspectives of various different critical genealogies.

Redfield and the Mexicans at the cross-'Roads to Light'

'The Road to Light' starts out toward Chicago rather than toward Mexico City. The changes in Chan Kom are in the direction of North American or cosmopolitan urbanized life rather than in the direction of Latin culture. . . . Apparently the spirit of this people is not favorable to the adoption of Latin manners or mores. . . . None of the aesthetic sensibility of Latin culture has found lodgement [sic] in the Chan Kom people. . . . The practicality, the exaltation of hard work, and the acquisitive rather than the expressive spirit – these qualities of the villager lead him away from Latin culture toward another, perhaps a predominating stream of world-wide expanding influence. Before progress came to Chan Kom, Chan Kom had a life-view of its own, not at all

Latin in nature, and Chan Kom has shaped the progress it has won in conformity with this ethos. (Redfield, 1950: 153–4)

This analysis has suggested that Redfield was less a part of the Boasian school/tradition than a part of a broader movement in which 'Boasian' ethnology married itself to 'Redfieldian' social anthropology in order to refortify a humanist anthropology within a four-fold disciplinary science. Likewise, Kidder's archaeological methodology of stratigraphy and the 'pan-scientific' approach to the culture history of culture areas does not so much originate in a Boasian paradigm or from Boas's school than become incorporated into it by the consolidation of a university-based four-fold discipline. Gamio's anthropology, in contrast, did emerge in part from a few basic ideas about culture and cultural relativism taken from Boas. Gamio, however, continued to develop an increasingly different mode of governmental anthropology that had less and less to do in its conceptions, agenda and methods with the university-based 'Boasian' ethnology and cultural anthropology. This divergence no doubt facilitates the marginalization, if not exclusion, of Gamio and Mexican anthropologies from the historiography of North American anthropology. In all three cases, the points of similarity, intersection, affiliation and mixing, however, are not based on a genealogy of influence nor a legacy of intellectual continuity from Boas or a 'Boasian school'.

The re-writing of Redfield, Kidder and Gamio into a Boasian tradition might simply and solely be an artifact of Stocking's historiographic framework – the story of Boas – that traces a genealogy to the father figure and, when recounting the descendants, adds ancestors where relationships 'did not' previously exist. Thus, in Stocking's story of Boas, Redfield's anthropology, just as much as Gamio's governmental science and Kidder's archeology, are modes and forms of anthropology whose specificity has been erased by both the dominant tradition of anthropology that marginalized or that cannibalized competitive alternatives and the historiography of the Boasian legacy that buried them in forgotten sediments. The historiographic traditions of the field either exclude (Gamio), marginalize or downplay (Kidder, Gamio), or efface (Redfield, Gamio) the specificity of the anthropologies that have not become emblematic and/or the mythic origins of the field and discipline. From this perspective, the analysis of the story of Boas suggests the need to find new concepts by which to historically recuperate and intellectually grasp the specificity of distinct modalities of anthropology.

In conclusion, then, the epigraph of this section can be discussed. Note that the Road to Light is not only double (being material and ideal), but duplicitous and forked. Godoy noted that Redfield became somewhat fed up with Cole's concerns about 'appearances' and what the SSRC might think if Redfield did not stay 'in the field' in Tepoztlan through the final summer (and time of resurgent violence) of the predefined research

period. It seems that here, in this quote, however, that Redfield found the *chiste* (the gist, the punchline) of catering to the (ideological) interests of one's funding agencies. While it is certainly true that Redfield expressed a 'cultural critique' – even of a romanticist bent – towards (Western) modernity and especially 'progress,' the message in his books on Chan Kom is that this village articulated, in a successful and laudatory manner, their cultural heritage with that of modern civilization. Further, he ironically attributes this to the Carnegie philanthropy which had such a positive effect in uplifting and 'modernizing' the Indians (see Castañeda, 1995 for details).

The text is not, however, simply addressed to the Carnegie Trustees and President. The passage is also addressed to Redfield's friend and colleague Manuel Gamio with a message of discernible ambivalence. On the one hand it confirms the reality of Yucatán as a *patria chica* or mini-'nation' of regional culture.¹⁵ In 1916 Gamio discussed the Yucatec Maya Indians (especially the tribal Cruzob Maya) as a case in which 'urgent nationalist work' needs to be conducted. On the other hand – or, is it simply the back of the same hand? – it is a (double) critique of Mexican nation-building (and of Gamio's vision of it) by defining the progress of the Indian as not only independent of the 'Latin culture' (that is, México/Mexican *mestizaje*), but as premised on a relationship to the USA, specifically to Chicago (!). The allusion to Chicago as the metropolis Ur-site of American civilization and universal modernity is, at least for those who of us who have not entered through the doors of the University of Chicago, a curious statement, no?

The reference to Chicago might be taken as an allusion to heavy Mexican migration, albeit from other parts of México, to that Midwestern city. And, thus, the message might be a statement in agreement with Gamio, that México's modernity is dependent upon 'a passage to/through' American modernity. But, Redfield states that the road starts out in that direction; he does not say that it ends there. The Maya are not migrating to Chicago, but heading in its direction to get to Chichén Itzá, an ancient Maya city excavated by Washington- and Harvard-based archeologists. Given the associations for Redfield of education, culture and progress, one might ask if, therefore, there are specific persons whom Redfield identifies as the Mexicans, Latins and/or Indians that have 'progressed' via a road to Chicago? Is he not referring obliquely to Gamio himself? And to Villa Rojas? As well as to Vasconcelos? All three are examples of Mexicans whose life trajectories brought them through the halls of the University of Chicago, if not also the Anthropology Department, for cultural and educational exchange. Regardless of this wholly speculative comment, Redfield does seem to be pointing ambivalently and ambiguously to large fissures in the forging of Mexican national modernity.

These fissures are, for Claudio Lomnitz (2001) – curiously yet another one of *those* Mexicans/Latins who speaks, coincidentally (fortuitously?)

unwittingly? prophetically?) from the navel 'of progress' (Chicago anthropology) via an (enabling?) passage through, not Chichén and Yucatán, but Tepoztlán¹⁶ – part of a reciprocal disavowal yet mutual exchange between the major *national* traditions of North American anthropologies. If Redfield's comments about the importance of a Mexican 'passage to Chicago'¹⁷ are oblique and ironic, we should note that at least Redfield, as well as Lomnitz 40 years later, is concerned with the interface between México/Mexicans and USA/Anglo North Americans. Yet, beginning two decades after Redfield and in the same anthropological halls 'of progress' at the University of Chicago, Stocking not only marginalizes Redfield, as well as Gamio and Kidder, but obscures these transnational issues in his historiography of a 'Boasian paradigm'.

This re-reading of the 'story of Boas' has sought to 'bring to light' some of these transnational dynamics between Anglo and Mexican anthropologies, as in the work of Lomnitz. The critique has had only the limited goal of changing and refocusing the available light, and thus identifying complications in the genealogy of the Boasian paradigm. The goal has not been to offer a new history or historiography, but to begin to dismantle the dominant interpretation of North American anthropologies as unitary, as derived from Boas, and as simply a uniform Anglo(-Germanic) American tradition.

Notes

This article was originally part of a paper delivered at a Presidential Session, 'The Legacy of Manuel Gamio', organized by Juan Vicente-Palerm for the 2001 Meetings of American Anthropology Association. The paper grew voluminous and was divided into four different yet related analyses for publication. The minor overlaps that exist are necessary for the coherence of the separate arguments. The author thanks Vicente-Palerm and co-panelists, especially Paul Sullivan and Roberto Melville. The friendships and intellectual aid of Juan Castillo Cocom, Lisa Breglia, Liana Chatzigeorgiou, Gabriella Vargas-Cetina, Igor Ayora-Dias, Carlos Bojórquez, Ana Luiza Izquierdo, Mario Ruz, Kay Warren, Oscar Barrera Nuñez and Shanon Sidell have been essential to the development of the ideas presented here. Ted Fischer, Ben Fallaw, Victor Munck, Kathy O'Connor, Anne Pyburn, Rick Wilk, Marvin Cohodas, David Price, Joy Logan, Geoff White and Claudio Lomnitz are thanked for their contributions to the ideas developed here. Carmen Morales and Mechthild Rutsch are especially thanked for providing me the opportunity to share this critique with the Seminar on the History of Anthropology in Mexico City. I thank the Fulbright CIES program for the Fulbright Garia Robles grant that supported the rewriting of this article and the COMEXUS staff for their friendship and support. I would like to acknowledge – in this context, note, without a wink – and dedicate the article to the influence of James Boon.

- 1 Givens does not give reason for this dislike. One might speculate about a racist attitude, which, however, would be ironic given Kidder's apparent distaste for eugenics and Gamio's approval of it. Professional competition

seems a clear factor, as is evident in a private statement made by Kidder to CIW (Carnegie Institution of Washington) President Merriam, who was a friend of Gamio's. Kidder suggested that Merriam reject Gamio's 1930 request for funding on the basis that 'I do not regard Dr. Gamio as a first-class archeologist, but he is probably better than any other native Mexican. . . . On purely scientific grounds, it would accordingly not seem to me desirable to back Dr. Gamio' (AVK to JCM, 'Memorandum for Dr. Merriam re: Gamio project', 12 June 1931; see also JCM to MG, 29 Oct. 1931; MG to JCM, 3 June 1931 includes a proposal, 'Project of Exploring an Aztec Cemetery in the Valley of México', Gamio File, Carnegie Archives, CIW).

- 2 In a separate work in progress (Castañeda n.d.b), I explore the specific forms and modes of anthropologies developed by Boas, Gamio, Kidder, Morley, Redfield and J.C. Merriam. Gamio and Merriam ultimately share greater affinities with each other and greater contrasts to Boas; this work further shows that Merriam has greater importance vis-a-vis the role given to Kidder in the conceptualization of the 'anthropological' research of the Carnegie Maya program.
- 3 I first became familiar with the work of Mechthild Rutsch (DEAS-INAH) after the second revision of this article; thus I have not been able to fully incorporate her important findings. She amply demonstrates that German anthropologists in Mexico were especially crucial to the intellectual development of Mexican anthropology, and that it developed along non-Boasian Germanic lines. This may also contribute to the fact that the historiography of the dominant Anglo-Germanic tradition in US anthropology has marginalized Mexican anthropology and has virtually erased the importance of other German scholars in the development of North American anthropologies.
- 4 Strug (1971), writing from Columbia University, details the possible sources of Gamio's use of stratigraphy and corroborates Darnell's view.
- 5 Gamio's New York/Columbia period, during which he connected with other Boas students and Latino intellectuals, seems crucial for his thinking and development. No studies of these issues in this 1908–1920 period have been made, in contrast to the growing study of Gamio's period of exile and migration research beginning in 1925 (e.g. Schmidt, 2000; Walsh, 2001; Weber, 2001).
- 6 Stepan's (1991) treatment of Gamio and Vasconcelos only whets the appetite. Walsh (2001) offers a focused study of Gamio's use of race, eugenics, acculturation and migration in relationship to the positions and practices of Boas, Vasconcelos, the NRC and the SSRC.
- 7 Citing Kroeber's (1931) review of Tepoztlan, Stocking suggests his recognition that Redfield's study was a departure from Boasian ethnology and thus does not fit into his paradigm except by rhetorical force: 'Redfield's community study of Tepoztlan was a "sociological" departure from traditional ethnographic approaches (Kroeber, 1931)' (Stocking, 1992: 282).
- 8 This essay is from the 1989 *History of Anthropology* volume on 'romantic motives'.
- 9 Stocking's broader goal was to lay out an historical context in which to assess the Freeman–Mead controversy. These broader aspects are not directly pertinent to the present analysis.
- 10 Godoy states that Redfield was impressed with Gamio's human relationships with his Indian workers and informants. Likewise, Rubenstein notes that while Redfield was known as a somewhat sober personality in the university setting, he had a warm and caring personality with his informants. Rubenstein quotes Villa Rojas (1979), who eulogizes Redfield for this aspect of his character (Rubenstein, 1991: 5–6). Whether or not Gamio is the 'cause' or determining

- 'influence' of this trait in Redfield, it seems that Redfield also sought to practice this ideal that he witnessed in Gamio.
- 11 The Tumin quote is: 'To the degree that a culture departs from its folk attributes, to that degree is it moving toward a condition of spuriousness' (1945: 199). Tumin, along with Tax and Gillian, were part of the Carnegie team of ethnographers working in Guatemala in the 1940s under Redfield. The Yucatán team in the 1930s consisted of Redfield, Asael T. Hansen, Alfonso Villa Rojas and Margaret Park Redfield. Sol Tax began working under Redfield as a Carnegie Researcher in 1934–5, which is also the year Redfield made his initial field trip to Guatemala. In 1937–8, M.J. Andrade's linguistic project moved from an 'independent' status to being administered by Redfield under the subsection 'Ethnological, Sociological, and Linguistic Research'. Sadly, Andrade died in 1941 and his research remained uncompleted, but it is archived at Chicago.
 - 12 If there is more direct and substantive evidence for 'influence', Stocking does not present it. Further, my search of the secondary literatures has not disclosed any attribution of dialogues or exchange between Redfield and Sapir (e.g. Darnell, 1990) or between Redfield and Kroeber (e.g. Kroeber, 1970). Although Redfield and Kidder certainly communicated on CIW matters, the secondary literature (Givens, 1992; Woodbury, 1973) makes clear this exchange was of a mundane, administrative nature.
 - 13 'We know from the Church Hearings that during the early 1960s "CIA funding was involved in nearly half the grants made by [agencies other than the Rockefeller, Ford and Carnegie] foundations during this period in the field of international activities"' (Price, 2000).
 - 14 James Boon (1982), making use of an obscure note by Marcel Mauss on pale and eclipsed moons, discusses the mobility, transmutability and perspective of hierarchy. With the shifting perspective of the temporality of orbits, the patterned relations of hierarchy become inverted, reverted, undone and reformed by the trajectory of bodies that enter into eclipses.
 - 15 Gamio's chapter actually discusses the area of Quintana Roo, which at that time was a federal territory, not a state. Historically and ideologically speaking it is 'part' of Yucatán. Further, the politically independent Maya Indians who continue to live there are 'refugees' and exiles from Yucatán.
 - 16 Lomnitz has conducted extensive ethnographic and historical research on Tepoztlan.
 - 17 The phrase alludes to E.M. Forster's *A Passage to India*, particularly the trip to the caves.

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