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Introduction

Sometime between 1900 and 1920 anthropology emerged as an academic discipline in the United States. Each author who studies the period cites a different date; the choice seems in part to depend on which criterion is considered the most significant turning point.[1] (Among those most often mentioned are the systematic planning of research and some aspects of the establishment of an academic environment.)[2] The process was a long and complex one, and the lack of agreement on a specific year can be attributed to this fact. Suffice it to say that by 1920 the majority of the work necessary to establish anthropology as an academic profession had been completed.

Most of the research published concerning the development of anthropology has focused on the events immediately preceding and leading up to the new status rather than those postdating and resulting from it.[3] Yet it is clear that the 1920s can be viewed as a period of transition in the development of American anthropology and as such are well deserving of further study.

One key aspect of the professionalization of anthropology was the development of appropriate training, made available through various academic programs, for those who wished to become anthropologists.[4] The movement away from the "rather random endeavors of amateurs and self-trained men" (Steward 1973:v; see also Darnell 1998:70) and toward a systematic program of research undertaken by those Boas or his students had taught was certainly an important part of what occurred within the discipline between 1900 and 1920.[5] Curtis Hinsley suggests that by the beginning of the twentieth century self-trained amateurs were already "rapidly passing out of acceptance" (1981:264), and it is true that by the 1920s they played a much less significant role than they had previously, owing to the availability of those with more appropriate training. Yet even through the 1920s there were amateurs whose work was accepted by Boas and others of the time as legitimate and appropriate.

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Although anthropology could be called a profession by 1920, there were still only a small number of students available to do research. There was a crucial period, one rarely mentioned, from the time that the study of anthropology became available to students to the time that a sufficient group of students was trained and ready for work.[6] This period was the 1920s: a handful of fully trained individuals had academic jobs and were establishing new programs around the country, but the second generation of students had not had time to develop. The exact years are not the issue here, but rather the fact that this hiatus existed at all.

Moreover, the type of research Boas stressed at that time, ethnographic and linguistic salvage, called for a large amount of documentation.[7] For all its positive eventual uses, such as demonstrating the range of types of Native American languages, the contribution of linguistics to ethnological theory (Boas 1911b), and the refutation of evolutionary interpretations (Boas 1896), salvage work had the drawback of requiring a large number of individuals who were immediately available to do the research. Boas wanted the culture and language of all the Native American groups to be studied and recorded for posterity before they disappeared, and he needed researchers if his objectives were to be carried out.[8]

In addition to these negative reasons, a small number of less significant but positive reasons also dictated the use of amateurs. First, they often came supplied with an independent income and so could support at least their own investigations and sometimes those of others as well. Second, they often had more enthusiasm for their own specific interests than anyone else and would as a result work very hard in a narrow field.[9] Third, not being tied to an academic calendar, they were available on short notice and throughout the year.[10] And finally, on occasion they could be given sufficient informal training that their work could be considered at least adequate and sometimes as good as that done by those with more formal training (although they were rarely permitted to be theorists).[11]

This book examines anthropology in the 1920s through the life and work of one of the amateur scholars of the time, Jaime de Angulo. He is not regarded as one of the major figures of his day, yet there are good reasons for considering him an ideal subject. First, the period of his most

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extensive research, 1920–33, coincides almost perfectly with anthropology's decade of transition. Second, he provides a link between many of those who are generally recognized as the most important anthropologists of the day (Franz Boas, Alfred Kroeber, Edward Sapir, and others) and can be used as a focus for their interactions. Third, he himself personifies the period of transition, being one of the last of the amateurs whose work was accepted as valid. By the 1930s, because of the depression, there was an overabundance of academically trained anthropologists available to do research, and amateurs were viewed less favorably.

I use the term *anthropology* in the broad sense of "Boasian anthropology," which included not only what have come to be considered the traditional four fields of anthropology (cultural, physical, archaeological, and linguistic) but much of what is today the separate discipline of folklore. Cultural and linguistic anthropology, as well as folklore for its study of tradition and myths, are the important aspects here; the others had no relevance to de Angulo's work.[12]

Anthropology, linguistics, and folklore have separate histories, but in America they all came together under Boas. Thus, in the 1920s they were in large part practiced by the same individuals, primarily because a single subject matter was the focus of all three areas: the study of Native Americans. The salvage ethnography and linguistics that Boas stressed were aimed at recording the culture, language, and myths of the various Native American groups before they became completely acculturated. In the process of their work researchers recognized that there was a substantial overlap between what might otherwise have been considered separate disciplines (and what, in other places and at other times, functioned as separate disciplines).[13]

It is important to make explicit how cultural anthropology, linguistics, and folklore were related to one another through the study of Native Americans. The primary focus was most often ethnography, but good fieldwork required knowledge of the native language. This approach was a matter of principle with Boas and his followers: "The leaders of the first generation of academic anthropologists in the United States (Boas, Kroeber, Lowie, Radin, Sapir) believed in control of the native language as a matter of scholarly standards, if ethnography was to stand comparison with the disciplines studying classical, oriental and contemporary

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European cultures" (Hymes 1970:252).[14] The impetus for including the study of language as a necessary part of the study of culture did not originate with Boas; at least as early as John Wesley Powell (1877) it was cited as a goal, but for Boas and his immediate followers it was not merely one approach, it was doctrine (Noelke 1974:72). Folklore comes into the equation because one of the basic components of studying language, using the method established by Boas, was the acquisition of a large number of texts, and myths were the variety of text most often collected.[15]

How does de Angulo function as a key to understanding the 1920s in American anthropology? One way to answer this question is through the concepts of *social circle* and *invisible college*. A social circle is an amorphous group whose members have direct or indirect ties with many but not all of the other members; there is no formal leadership, and mutual interests are often more important than propinquity or ascribed status (Crane 1972; Kadushin 1966, 1968, 1976). The concept is useful as a way of describing the frequent shifts in group context de Angulo made. During the course of his life he belonged to several significant circles: Berkeley anthropologists led by Alfred Kroeber; Mexican linguists under Manuel Gamio; students of psychology under Carl Jung; the Taos and Big Sur artistic communities of D. H. Lawrence, Robinson Jeffers, and Henry Miller; and poets grouped around Ezra Pound such as Marianne Moore. He was never central to the existence of any of these various groups but active only on the fringes; all would have continued without his participation. Interestingly, these groups had no other common members; only de Angulo served to tie them together.

Although it is primarily de Angulo's contact with the social circles in anthropology that are of concern here, his membership in other groups is not irrelevant, for as George Stocking points out: "In various ways, Boasian anthropology of the 1920s was embedded in this intellectual context. The Greenwich Village of the New York *avant-garde* and the Southwest of D. H. Lawrence and Mabel Dodge Luhan were both important Boasian milieux; Boasians wrote poetry for the little magazines and articles for the liberal weeklies" (1976:33).

An *invisible college* is a larger structure than a social circle and can most simply be defined as a "communication network . . . that links groups of

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collaborators" (Crane 1972:35).[16] Boasian anthropology is the invisible college relevant to a consideration of de Angulo. Of the social circles in which de Angulo claimed membership, only those centered on Kroeber and Gamio belonged to this invisible college. Recognition of the existence of an invisible college in anthropology helps account for the various links de Angulo had with the leading researchers of his day. It also helps explain why these links should be viewed not only as several unique relationships to individuals but also as a method of meshing the anthropological tradition at several points simultaneously. Again, as with the social circles he joined, de Angulo was far from ever being a crucial member of Boasian anthropology. Since he was not the leader of any social circle, his membership in this invisible college was as an interested individual rather than as the focus of any particular research tradition.

As an amateur scholar, de Angulo was admitted only to the fringes of academia. He was given a university affiliation only once, when he taught two summer courses at the University of California, Berkeley in 1920. In later years others often assumed he was associated with Berkeley in some way, but Kroeber actively discouraged such incorrect assumptions. And since de Angulo never received formal training in anthropology, he had no certification in the field. The fact that he had an M.D. probably promoted his acceptance as a generally well educated man who would in the normal course of events not be expected to return to graduate school for further advanced training.

At the same time, de Angulo was extremely active within the limited role he was granted in the academic world. Ralph Beals, who entered the University of California, Berkeley's Department of Anthropology in 1922, remembers that "four factors were drilled into us as the mark of the professional: 1. subscription to the professional journals, attendance at professional meetings, and delivering papers when possible; 2. extensive reading in current and past literature; 3. field work; and 4. publication" (1979:6). By these criteria, de Angulo was essentially as much a professional as anyone else at the time. He joined the appropriate societies (the Linguistic Society of America from 1925 to 1932, for example). He went to at least one major international conference (in Hamburg, in 1930). He certainly read extensively in both the current and past literature, including the professional journals (although he did

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not subscribe to many, if any, of them). He conducted fieldwork with many Native American groups in California, as well as several groups in Mexico. He had a large number of articles accepted for publication in the major journals: *American Anthropologist* (AA), *Language*, *Journal of American Folklore* (JAF), *Anthropos*, and *International Journal of American Linguistics* (IJAL). He wrote book reviews for AA, at least some of which were on significant books (such as Malinowski's *Myth in Primitive Psychology*). In addition to Beals's list, de Angulo engaged in other activities that indicated his role in the profession. For example, his name periodically was included in discussions of what was happening in the field (in the *New International Year Book* for 1929, for instance, the section on anthropology mentions de Angulo's work on Achumawi, Atsugewi, Kalapuya, and Karok; Spier 1930:39). And whenever he was in New York, he stopped in at the Department of Anthropology at Columbia to visit with Boas and his students there (he knew at least Margaret Mead, Ruth Benedict, and Ruth Bunzel).[17]

In addition, de Angulo was an important figure in the informal community surrounding the Berkeley Department of Anthropology, much as well-known anthropologist Paul Radin was and in much the same way: "Paul Radin was around a good deal of the time, usually out of a job . . . the thing he liked best, and what he was best at, was just informal conversations with students, and he was wonderful as a person to have around; you met and talked with him and he was full of ideas" (Dillon 1977:56).[18] Since de Angulo was not a professor at Berkeley, he did not have students in the usual sense, but Kroeber used him informally as a resource for those who wanted to learn more about linguistics than Kroeber himself wanted to teach. One of the students he sent to de Angulo was Carl Voegelin, and it was apparently a successful match. Voegelin later said to Nancy Freeland, de Angulo's second wife, that de Angulo "had great potentialities as a teacher, and could inspire a young student as no one else in the field could do." In fact, Voegelin suggested of de Angulo that "with a feeling of security and a skillful stage manager, he could have had a marked success in the role of a member of a university faculty" and that "he thought it had been a great loss to Anthropology, that Kroeber had been too timid to sponsor Jaime" (Freeland to Gui de Angulo part 2, number 8, GDA).[19] Freeland did

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not agree with these comments, nor did Kroeber. Kroeber specifically commented on de Angulo's inability to function appropriately within a university structure (to Gamio, 18 May 1922, MAF) less than a year after he had given de Angulo a chance to teach anthropology courses at Berkeley. As a result of that experience, Kroeber decided that de Angulo would more appropriately fit into a research position; that would be a way to take advantage of his skills without forcing him into an inappropriate role. (It is worth pointing out that Kroeber did not have a full-time position available in the department for many years, so even if he had wanted to hire de Angulo, he would not have been able to do so.)

De Angulo was proud of his informal students and frequently mentioned them to Boas or Sapir. Comments such as the following appear scattered throughout his letters to them: "There was another man from the anthropological crowd, a young fellow, his name is *Strong*. Keep your eye on him. There is stuff there" (to Sapir, 7 September 1925, ES, his emphasis). And when he was working with Voegelin, he sent detailed reports of his progress to Boas, who followed de Angulo's advice about a summer project appropriate to Voegelin's current level of skill and interest (Boas to Kroeber, 3 May 1930, FB). Nancy Freeland refers to the large number of Berkeley anthropology students who "served their sejour d'honneur [stay of honor] on the hill [their home in Berkeley]" (to Gui de Angulo, part 2, number 8, GDA).

In many ways the Committee on Research in Native American Languages served as a substitute for a university affiliation for de Angulo (see Chapter 5). Through it he was granted a status he was not given anywhere else: that of a respected researcher whose work was not only appropriate but crucial to a major plan, that of salvage ethnography and salvage linguistics. He and a few other amateurs, such as Father Gabriel Morice, were included in the committee's ranks of professionals. Their work was funded along with that of the others, and published alongside theirs, despite the fact that the committee was concerned with maintaining a high standard of research. The committee needed de Angulo as much as he needed it, and both profited from the association. Although it was Sapir who originally suggested that de Angulo work under committee auspices, Boas continued to propose further projects.

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Boas used de Angulo a great deal; he was given more money than anyone else and worked on more different languages. One reason Boas chose de Angulo concerns the relationship between Boas and Kroeber, at least as far as the study of California Indian languages was concerned. They disagreed about the priorities of research in California from the time Kroeber first began work there. Boas felt that survey work was being overemphasized whereas Kroeber believed that it was essential to a preliminary understanding of the area. Boas thought it was more important to do thorough work on a single group at a time whereas Kroeber felt it was not the appropriate time to do detailed research on a single language (Darnell 1969:304–5). One way for Boas to ensure that the more detailed studies he wanted were actually undertaken was to use the committee as a way of funding research in California and to direct individuals to those specific projects he felt were important (Hymes 1961:26). This is exactly what he did with de Angulo. Kroeber didn't really take offense because he was by then busy doing other things; de Angulo was not so much in the way as filling a gap.[20]

On several occasions Kroeber requested that Boas give de Angulo assignments farther east—anywhere outside California; and if that was what Boas had wanted, de Angulo would have happily gone (as he did, in fact, go to both Washington state and Mexico when Boas requested it). But by keeping de Angulo in California, Boas was able to have more thorough linguistic studies completed of at least some languages. (De Angulo was not the only researcher funded by the committee who was asked to work on California languages. Hans-Jørgen Uldall also worked on many different languages there for the committee, and being European, he had no intrinsic interest in California over other states.)

It is not possible to prove whether or not Boas used de Angulo as a way to gain control over linguistic research in California. Boas never mentioned that he had such an objective when he overruled Kroeber's requests that de Angulo be funded elsewhere, but it seems extremely likely as well as reasonable. To a certain extent, Boas continued employing de Angulo because he met many of the previously stated characteristics of useful amateurs. He came supplied with an independent income (the majority was his wife's) and thus was able to pay for some of his own research. He was available on short notice and was willing to

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travel as necessary. His enthusiasm for the subject was admitted by all, even those who were not fond of his personality. He had more informal training than most amateurs, from the best teachers available (Boas himself, Kroeber, Sapir, and, much later, Uldall). As a result, his work was considered by many to be of high quality, even though it tended to be somewhat uneven. When all these factors were combined with the possibility of obtaining one more worker than he would otherwise have had, in an area where he felt a need for more intensive linguistic studies, it is no wonder that Boas kept funding de Angulo.

When de Angulo was offered the chance to teach at Berkeley in 1919, it was not seen as a problem that he had a degree in another subject, medicine, rather than in anthropology (a professional degree being seen as a more transferable credential then). When he was funded by the committee, this again was not seen as a reason to withhold funding (it was never mentioned in letters at the time discussing whether or not to support his work). Because of the slow development of anthropology, a large number of researchers at the time were in similar positions (Boas to Shuller, 19 June 1928, FB). Even by the 1930s, when more students were being formally trained, such training was generally an apprenticeship under one of the acknowledged leaders in the field (Joos 1958:37).

At the same time, funding de Angulo to do independent research no one else was available or willing to do was not the same thing as fully accepting him into the ranks of academia. De Angulo recognized that he was an outsider and often remarked on the fact in his letters. In 1925 he wrote, "It's going to be pretty hard to make an entrance into the academic world. . . . In New York, or here [Berkeley], they all like me and I am always admitted as a free-lance, but I will never get a real 'position'" (to Cary Fink, his first wife, 20 March 1925, GDA). A few months later he decided that a proposed trip to Europe would not be possible because "things are taking shape slowly, for me, in the matter of recognition in anthropology, and right now is an important time when to leave a thing unfinished would be bad" (to Fink, 2 May 1925, GDA).

In later years he became somewhat bitter about the limited degree of recognition he had achieved for all his efforts, perhaps expressed best in the following comment. He is discussing the songs he recorded on wax cylinders and the fact that he was unable to get the funding necessary

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to preserve the material he had collected: "The University [Berkeley] would not help me; took no interest; would not even give me enough money to have the records transcribed and made permanent on modern disks. Decent anthropologists dont associate with drunkards who go rolling in ditches with shamans" (de Angulo 1950a:360; the reference is not only to his interest in participating in every aspect of the life of the Native Americans he studied, including drinking and gambling, but even more to his reputation as an alcoholic who threw wild parties and refused to abide by convention). Even in 1932 and 1933, when he was at the peak of his acceptance, he wrote Boas at various times that he realized he was not "inside the game" (9 September 1932, FB) and that he felt isolated "pegging along doggedly by myself" (16 June 1933, FB): "It is hard to work here all alone. If I were in New York I could go and ask your advice and save myself a lot of trouble. So often I work hard on something only to find out in the end that I did it wrong and I have to do it all over again. But I suppose that cant be helped" (to Boas, 6 November 1932, FB). Notice that he did not mention Kroeber as a locally available person to whom he could turn with questions. By the 1930s they were not on good enough terms for that to have been a reasonable possibility. Thus, as Boas's long-distance worker, de Angulo often had to proceed with little guidance even when he wanted more. Although they exchanged frequent letters on various questions and problems, it was not the same as having someone available for lengthy discussions as the need arose (a role Kroeber was willing to fill only until 1923, for reasons described in Chapter 2).

At the same time that he wanted to become a part of the academic world, de Angulo had some feelings of ambivalence about the matter. He periodically wrote such comments as "I want to take some active part in the Indian Defense Association. I am not even a member! I am ashamed of myself! I must get out of my ivory tower and into the seething street, before it is too late!" (3 April 1925, CSG). This was written to John Collier about the practical problems of the Indians in California in 1925, about the same time that de Angulo wrote the statements previously quoted about how important it was to him to join academia. Ironically, he says here he must get out of the ivory tower that, on most occasions, he complained he had not yet been granted the right to enter.

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In addition to having equivocal feelings about joining academia, de Angulo did not really have the personality of an academic, anthropologist or otherwise. Kluckhohn has suggested, "The hallmark of the good anthropologist must be a curious mixture of passion and reserve" (1957:776–77). This combination permits one to become involved in the lives of those studied (the passion) yet maintain sufficient objectivity about what is learned to be able to analyze it at a later date (the reserve). De Angulo had a much larger dose of passion and much smaller dose of reserve than might be recommended, as illustrated by a passage from a letter he wrote in 1925 to Sapir:

> *There is one thing in one of your recent letters I want to talk about. You say that to Boas Science is "austere and impersonal." You know, that is just the thing that gets my goat. They have managed to take all the life out of Science. Why be ashamed of the joy and the exaltations that are the blood of knowledge? Why pretend that you have no emotions. In another century they will look aghast at the funereal aspect of our science. They will say: Those people were doing penance for something! Nine tenths of the scientific writing of today is such dull reading, and yet half the time the matter intended to be conveyed is interesting enough, only the author has been at such pains to revise his style and take all punch out of it, to reduce it all to a perfectly flat monotonous drone! All that is the penalty we paid for the overdevelopment of rationalism. We have driven our libido under ground. (14 May 1925, ES)*

Such passion is also amply demonstrated in Gui de Angulo's biography of her father, where his desire to be completely involved in every moment comes through: "We are enjoying life and love to the full. After that we will have no regrets to bite our hearts. We will have lived a full life" (to Fink, 10 July 1924, cited in Gui de Angulo 1995:243).

Accounting in part for this lack of academic reserve is the fact that de Angulo was also a novelist, something not unique but certainly uncommon. Three of his four novels were contemporaneous with the period of his most active involvement with the academic world, the 1920s. De Angulo saw the world through the eyes of an artist at least as often as the eyes of an academically trained researcher (perhaps partly because his

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formal training was in medicine rather than anthropology). This outlook was a significant factor in his tenuous connections with academia: he could never completely fit in because his most basic assumptions and approach were different from those of the professionals. At the same time, he wanted very badly to fit in, which meant that he was torn by dual loyalties to the Muse and to Boas. He wanted his work to be sufficiently "austere and impersonal" (as well as accurate) to meet the high standards of his long-distance mentor, Boas. Yet he also wanted his work to portray in a more personal way the thoughts and feelings of the Indians he met. These two goals were not usually compatible, and so he produced several varieties of writings in the attempt to find the best way to achieve them. Throughout the 1920s his writing generally fit into clear categories: fiction, ethnographies, and linguistic studies. It was not until the 1940s that he successfully achieved these separate goals in single works: *Old Time Stories* and "What Is Language?" (These works are considered in detail in Chapter 8.)

De Angulo's lack of reserve may not have been appreciated by his anthropology friends, but it certainly helped him enjoy fieldwork and gain the acceptance of those he studied. He joined wholeheartedly in whatever activities were occurring, including drinking, gambling, and curing ceremonies. At least with the Achumawi, if not with all other groups, he wanted to experience the life of his informants to the fullest extent they would permit. In fact, de Angulo "went native," which anthropologists are never supposed to do. As Ruth Behar explains: "But just how far do you let that other culture enmesh you? Our intellectual mission is deeply paradoxical: get the 'native point of view,' *pero por favor* [but please] without actually 'going native.' Our methodology, defined by the oxymoron 'participant observation,' is split at the root: act as a participant, but don't forget to keep your eyes open" (1996:5).[21]

This desire to fully participate in the everyday lives of his informants was most likely responsible for an important characteristic of de Angulo's ethnography. When it came time to write down what he had learned (speaking now only of the Achumawi, the one group for whom de Angulo wrote a substantial ethnography), he did not focus solely on reconstructing the past but looked at the present way of life as well. This

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approach was a significant departure from the standard anthropological practice at the time.

According to modern standards, de Angulo's fieldwork pattern might seem unusual, for as a rule (the Achumawi being the exception) he worked with a small number of informants in a large number of groups, never staying in one place very long. However, seen against the standards of the 1920s, his work was much more typical than might be supposed. Robert Lowie, for example, also worked with a small number of informants (usually old rather than young, as often was the case with de Angulo), in a large number of groups, frequently staying for only short periods of time before writing extensive manuscripts (at one point Lowie prepared 270 pages as the result of a seven-week trip; Murphy 1972:18–22). Lowie was not the only one whose fieldwork followed this pattern: Sapir worked with at least seventeen Native American languages, Boas with at least eighteen, and Kroeber with thirty-three.[22] Against this background the fact that de Angulo worked with thirty seems less astonishing. Of course, the reason these scholars and others were working with so many groups and their languages was the emphasis on salvage ethnography and linguistics and the concern that if the languages and cultures spoken and practiced by increasingly small numbers were not immediately documented, they would disappear unrecorded.[23]

At the same time that his fieldwork was in so many ways similar to that of the leading figures of his day, de Angulo chose a different emphasis from the usual one. The focus of Boasian anthropology was the aboriginal life. That is, rather than investigate and document the Native American groups as they were living at the time an anthropologist visited, he or she would attempt to reconstruct the life of the Indians prior to contact with white culture, what is generally termed the *ethnographic present*.[24] Thomas Buckley describes the "Kroeberian ethnographic present" as "locating the writer at the moment before colonial incursion" (1997:16), and Beals (1982) confirms this definition. At the insistence of Kroeber and Boas, de Angulo documented something of the prior lives of his informants, but he was most intrigued by their current lives, especially the interface with white culture. This is clearly the focus of his work "Indians in Overalls," which differed substantially from the ethnographic work of the time since in it de Angulo discussed only the modern-day

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existence of the Indians he knew. No doubt that was why he did not consider it one of his academic contributions. (All his academic contributions eventually found their way into either Boas's files or appropriate scholarly journals whereas "Indians in Overalls" was initially published in the *Hudson Review*, hardly a standard anthropological journal.)

One characteristic of de Angulo's that stood him in good stead throughout the years (and part of the reason he achieved as much recognition as he did) was his ability to meet and work with the major figures in a discipline. In anthropology he knew best and worked most with Kroeber, Lowie, Radin, and Boas, all of whom were important at the time and are still considered the most significant figures of their period. In linguistics he was in closest contact with Sapir but also worked with Boas, Kroeber, and Gamio, all again major figures. He had the same ability to immediately reach the acknowledged leader in a group in other endeavors as well: Jung in psychology and Pound in poetry.[25] Such approachability was still possible in the 1920s but would be much less likely to occur today, as access to both the academic world and invisible colleges has become more formalized and less available to those without formal training in a discipline.

In the case of folklore there was really no one to whom de Angulo could turn. There were two major strands in American folklore research in the 1920s and 1930s.[26] The first was influenced by Boas and his students at Columbia and depended on myths for its primary source of data. The second was influenced by Stith Thompson and others at Harvard and looked to ballads as well as narratives for its source material. Unlike Boas, those at Harvard used the Finnish method of reconstruction (which Boas disliked) and frequently worked through translations of texts (whereas Boas always advocated working with the original). Students of Boas used Native American data as their primary sources; students of Thompson used European, specifically British, material. There was in addition a minor third strand that later developed into ethnomusicology, which can be traced in large part to the influence of George Herzog, a student of Boas. De Angulo was a part of only the first and third of these research traditions, although in many ways the second has assumed greater prominence in folklore studies today.[27]

As a result of the influence of Boas, de Angulo published several times

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in JAF, which Boas edited from 1908 to 1924 and Benedict took over from 1924 to 1942. But he never considered himself a folklorist.[28] As he wrote in one article discussing Native American myths: "Not being a folklorist, I use my terms loosely. Folklorists will please correct" (de Angulo and Freeland 1928:250). In another, he remarked that his translations of myths were literal and thus intended for an audience of linguists rather than folklorists (de Angulo 1935:229). Nevertheless, de Angulo fit rather well into the tradition of folklore drawn from Boasian anthropology. Perhaps it was because he could not point to a specific person as the leading folklorist and one school of thought as the dominant one that de Angulo was so uncertain about whether or not his writings could be considered part of the field of folklore. Although Boas, as just noted, was the leader of one of the primary schools of thought within folklore, and de Angulo frequently corresponded with him, their letters never discussed subjects explicitly identified as folkloric (in the way they discussed linguistic problems, for example). De Angulo may not have considered himself to be a folklorist because none of his work was overtly labeled *folklore* by anyone he knew.[29]

De Angulo not only hesitated to identify himself as a folklorist; he was equally unsure about whether or not to call himself an anthropologist. The only title he felt comfortable in claiming was that of linguist. In the introduction to *Indian Tales*, for example, he calls himself a professional linguist, an amateur anthropologist, and a general philosopher (1953:4). For all his uncertainty, by the mid-1930s de Angulo had finally established at least a tenuous position in the academic worlds of anthropology and linguistics. But just as he was becoming successful in his efforts to be accepted by the professionals as one of their number, he took a giant step backward and reverted once again to the much lower status of an educated layman.

It would be simple to attribute de Angulo's return to the ranks of the amateurs solely to the car accident in 1933 in which his son was killed (Olmsted 1966 does just this), but it would not be accurate. The full explanation is much more complicated. One factor is certainly the car accident: injured physically and psychologically, he stopped all activity for a time. But he expressed no desire to give up what informal academic status he had achieved and did return to work, albeit at a slower pace, so

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