

## From the armchair to the field and back again: C.C. Uhlenbeck's work on Blackfoot

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### Introduction

Like many of his colleagues, the Dutch linguist C.C. Uhlenbeck (1866-1951) spent most of his time in the comfort of his study, working on languages he knew from written sources. Around the turn of the 20<sup>th</sup> century, however, many non-European languages were ill-documented, so that linguists who wanted to study them were, even more than today, required to gather their own data in the field if they wanted reliable material.<sup>1</sup> In addition, a shift in focus from a more historical approach to a more synchronic approach to language meant that a different kind of data was sometimes needed. The historical approach was the traditional standard certainly in the early part of his career, but the synchronic approach, which has its own requirements, was gaining ground (Robins 1990: 180ff; see also Noordegraaf 2009). Which approach one favours determines which questions one asks. A historical linguist wants to know where a linguistic form or phenomenon comes from, what its origin is, and how it has developed over time; to such a scholar, to explain something is to understand its history. A synchronic linguist wants to know how a linguistic form or phenomenon relates to other aspects of the language and how it functions in the larger context of the language as a whole; to such a scholar, to explain something is to understand its functioning in relation to other elements in the linguistic system.

Uhlenbeck's personality and his early training made him more suitable for bookish work in the comfort of his own office, but he clearly felt that when the available records are unsatisfactory the linguist must go into the field to get reliable data. In addition, he felt strongly that the understanding of the nature of the Indo-European mother tongue – one of his major preoccupations throughout his career – should not just be based on careful historical reconstruction, but should also be checked against conclusions drawn from the study of other languages. One should not reconstruct a proto-language of a type which is not actually attested, therefore if one wants to propose a proto-language

of an unusual type one has to try and find an actual living language of that type to prove that it is at least plausible.

Toward the end of his career, in a programmatic paper in *American Anthropologist*, Uhlenbeck expresses his disdain for what he considers intellectual laziness on the part of comparative and historical linguists who do not look outside of their own narrow field: “some Indogermanists have neither the desire nor the courage to verify impressions which they have gathered from their own field of research by comparisons with the results of investigations in other, adjacent fields” (1937a:387-8).

In Uhlenbeck’s case, the “adjacent fields” had included, among others, the study of Basque, Eskimo/Greenlandic, and Algonquian. His work on Basque and Eskimo had investigated both typological parallels and the potential for actual historical connections with Indo-European, whether it be genetic or areal (see Bakker 2009a). The work on Algonquian was different in the sense that it was more strictly typological (Swiggers 1988). While Uhlenbeck did get involved in discussions about genetic groupings within Algonquian and in North America in general (see e.g. 1905a, 1905b, 1908, 1910, 1915, 1916a, 1916b, 1927a, 1927b, 1939a, 1948; see also Bakker 2009b), he never suggested that any North-American language could be genetically related to the languages of Eurasia (with the exception of Eskimo: 1934, 1937b, 1941, 1942; see also Genée 2003:158 and Van der Voort 2008).

The work on Algonquian was also different in that it was, with the exception of a short and unsuccessful attempt to gather original fieldwork data on Basque in 1903, the only occasion in Uhlenbeck’s long career in which he left the comfort and safety of his armchair to go into the field and gather his own data. Almost a century ago, doing fieldwork on a Indian reservation was no small feat, certainly not for a somewhat stiff academic such as Uhlenbeck who was accustomed to a fairly comfortable life and can hardly have imagined all the hardships which he would have to suffer in his quest for reliable data on the Blackfoot language. Nevertheless he went, not once but twice, and spent the summers of 1910 and 1911 on the Blackfoot reservation in Montana. In this paper I discuss Uhlenbeck’s work on Blackfoot and try to place it in the wider context of his professional development and the questions that occupied him throughout his career. I also discuss the resulting publications on the Blackfoot language and some of the reactions they drew from various scholars in the field.

### **From Eskimo to Algonquian to Blackfoot (1905-1910)**

Uhlenbeck entered the field of Algonquian studies in the early twentieth century in the way in which he generally tackled a new language family or subject area: he would read everything he could lay his hands on, often publishing a stream of (sometimes very brief) reviews of the works he read along the way, and then summarize what he had learned in an overview article, before working on more detailed aspects of his new topic. His publi-

cation record for the years 1905 through 1910 shows the shift from Eskimo to Algonquian and finally to Blackfoot.

The year 1905 marks Uhlenbeck's move from the study of Indo-European and Basque to the investigation of North-American languages. In that year he published for the first time on Eskimo: a small comparative paper about Uralic elements in Eskimo languages, which investigates possible connections between languages in northern Eurasia, Greenland, and North-America (1905a). In 1906/7 he published a small two part paper exclusively on Eskimo grammar (1906, 1907a), followed in 1907 by the ambitious comparative study *Ontwerp van eene vergelijkende vormleer der Eskimotalen* (1907b). During 1905-1907 we learn from his letters<sup>2</sup> to the German linguist and bascologist Hugo Schuchardt (1842-1927) that he has little time for anything other than the study of Eskimo and Greenlandic.

1908 then sees Uhlenbeck shift his attention to North-American languages south of the Arctic with the publication of a large overview article in *Anthropos*, entitled 'Die einheimischen Sprachen Nord-Amerikas bis zum Rio Grande' (1908, 1910a). This paper summarizes in brief form the entire state of knowledge about the then recognized 54 language families of North-America.

By 1909 his focus has narrowed more specifically to the Algonquian language family, as he publishes *Grammatical distinctions in Algonquian demonstrated especially in the Ojibway dialect* (1909, 1911a), followed in 1910 by the ambitious *Ontwerp van eene vergelijkende vormleer van eenige Algonkin-talen* (1910b).

In early 1909 this focus on Algonquian has already resulted in plans for a visit to North-America. In a letter to the bascologist Georges Lacombe (1879-1947) on 19 February 1909 he announces that he will be going to the United States from May to October 1910 for "études algonquines", and in March 1909 he writes to Hugo Schuchardt that he hardly works on Indo-European any more:

Ich fühle mich die letzte Zeit auch wenig indogermanistisch. Im Frühjahr 1910 werde ich einige Monate Urlaub bekommen um eine Reise zu den nord-amerikanischen Indianen zu unternehmen (zusammen mit meinem Schüler Herrn Cand. Phil. De Josselin de Jong). Das Jahr, welches der Reise vorangeht, werde ich hauptsächlich mit amerikanischer Ethnologie und Linguistik beschäftigt sein müssen. Bald hoffe ich Ihnen eine Abhandlung über das Algonkische schicken zu können.  
(Letter to Schuchardt, 4 March 1909)

It is not entirely clear when and why the decision was made to conduct original fieldwork. Perhaps the fact that his doctoral student J.P.B. de Josselin de Jong (1886-1964) was available to accompany him played a role. Certainly the data then available in print on Blackfoot were unsatisfactory, and if Uhlenbeck wanted to confirm his ideas about the structure of "primitive languages" (see Genee 2003) he needed reliable material. Why he

chose Blackfoot, rather than another Algonquian or North-American language, is also not known.

A few weeks later Uhlenbeck writes again to Schuchardt. This time he says that he no longer even enjoys Indo-European studies and is completely immersed in the study of North-American languages, even to the detriment of work on his beloved Basque:

Nur die Indogermanistik wird mir immer mehr antipathisch. Augenblicklich [?] muss ich das Baskische aber liegen lassen, weil die Erlernung der Algonkin-Sprachen mich ganz beansprucht. (Letter to Schuchardt, 22 March 1909)

### Into the field (1910-1911)

Preparations for the journey to Montana are well under way in early 1910. Blissfully unaware of local conditions and the special permission procedures that apply to travel to Indian country, Uhlenbeck writes on 14 February 1910 directly “To the Agent of the Blackfoot Reservation”, simply announcing their plans to visit the Blackfoot Reservation:

Dear Sir,

The signers of this letter, C.C. Uhlenbeck Dr. of Philology, Professor at the Leyden University, member of the Royal Academy of Sciences of Amsterdam, and J.P.B. de Josselin de Jong, Doctorandus of Philology at the Leyden University intend to have a stay of some months in Blackfoot reservation, Montana, with the purpose of living among the Indians and studying their manners, their superstitions<sup>3</sup> and their language. We shall arrive in the first half of May and we hope to stay among the Blackfeet till the beginning of October. Now we are taking the liberty to ask You<sup>4</sup> if You will be so kind to prepare the Indians and their chiefs in particular before our arrival and to tell them that we don’t want anything from them beyond the permission of partaking in their life. They may be sure that we have the utmost sympathy for their nationality and their language. And then if You would give us some information which might be useful to us on our arrival, we shall be greatly indebted to You. If You would kindly write to us, we pray You to address Your letter to the first of the signers, C.C. Uhlenbeck, Breestraat 53 Leyden Holland.

We remain Yours sincerely,

C.C. Uhlenbeck

J.P.B. de Josselin de Jong

The letter was answered by “Supt. & Spl. Disb. Agent Clarence A. Churchill”, who writes to Uhlenbeck on 4 March to explain that special permission is required to visit Indian reservations: “you are advised that by Section 2143, Revised Statutes of the United States, it is provided that: Foreigners are forbidden to go into the Indian country without a passport from the Department of the Interior, agent, or commanding officer of the nearest military post”; he is therefore forwarding Uhlenbeck’s letter to the Commissioner of Indian Affairs in Washington. In the accompanying letter to the Commissioner, Churchill writes: “I am of

the opinion that no ill consequences would result from the contemplated visit of these gentlemen and do not believe their presence on the reservation would be objected to by the Indians. I respectfully recommend that a passport be furnished or the necessary permission granted [...]". The forwarded letter was received at the Office of Indian Affairs on 9 March. On 26 March Commissioner R.G. Valentine writes back to Clarence Churchill "that the desired authority has been granted", and on the same day he writes to Uhlenbeck "that there seems to be no objection to your proposed visit to the reservation, and you are at liberty to do this, on condition, however, that the authority hereby given shall be revocable at any time in the discretion of the Indian Office [...]". Additional letters from Commissioner Valentine on 12 April to Arthur McFtridge, Superintendent of the Blackfoot Indian School in Browning, confirm his permission and ask him to "extend to Professor Uhlenbeck and Dr. Josselin de Jong every possible courtesy at the disposal of the Government", to "give them all practicable facilities for making the most of their stay and extend them every courtesy of the Government", and to "do everything to get them well started in their work."

The necessary permissions and arrangements are in place just in time and even recurring health problems do not deter Uhlenbeck, as he writes to Hugo Schuchardt:

Verehrter Herr Kollege, 22 April reis ich nach Amerika ab. Der von mir auserwählte Algonkin-stamm ist die Blackfoot-bande in Montana. Mit mir wird ein Schüler de Josselin de Jong einige Monate unter diesen Leuten verbringen. Erst gegen November werde ich zurückgekehrt sein. Für den Augenblick bin ich nicht gerade in den besten Verhältnissen: Influenza und Bronchitis haben mich etwas invalide gemacht. Die Blackfoot-Schamanen werden mich kurieren.

(Letter to Schuchardt, 22 March 1910)

After an arduous three-week journey by ship and train they arrive on 11 May (1911b:v). Uhlenbeck's name appears in the Visitors' Register of the Department of the Interior, U.S. Indian Service, on 21 June; his "Business on the Reservation" is given as "Studying the Peigan language". De Josselin de Jong's name appears in the Register on 28 June; he likewise gives his business as "Studying the language". They depart again on 15 August, after a stay of three months rather than the originally planned four.

It had been a productive period, as Uhlenbeck writes in September, just back from Montana, to Hugo Schuchardt:

Ich habe mehr als drei Monate täglich mit den südlichen Blackfoot-Indianen (Southern Peigan) verkehrt und ein sehr grosses grammatisches und ethnologisches Apparat mit nach Hause gebracht. Die Ausarbeitung wird einige Jahre erfordern. Ich bin gesund, aber ausserordentlich ermüdet.

(Letter to Schuchardt, 4 September 1910)

Uhlenbeck's field notes have unfortunately not been preserved, so we do not have detailed information on how he approached gathering data. However, from his publications and from his wife's diary of the second year (published in Eggermont-Molenaar 2005), we can conclude that he employed two main methods while he was on the reservation. First, he collected textual material. Informants would visit him in his tent and tell him stories, which he would transcribe and translate in long sessions mostly with Joseph Tatsey. He corrected these materials by reading them back to various people who visited their tent in the evenings. Some of the texts collected in 1910 he corrected once again the next year, as we can read from Willy Uhlenbeck's diary, by reading them back from the written versions to various visitors. People enjoyed these sessions and would come and ask Uhlenbeck to read them a story. This sometimes resulted in much hilarity, as the visitors found it very humorous that a white stranger from overseas could write down their language and then read it back to them in such a way that they were able to understand it. This feat becomes all the more amazing when one realizes that Blackfoot was very much an oral language at that time: no one wrote it, and the idea that it even *could* be written must have been nothing short of miraculous to many of the Indians, who, like many at that time, saw reading and writing as an exclusively western pursuit connected to European languages.

In addition to collecting text material, Uhlenbeck also conducted elicitation sessions with a group of young boys who would hang out around his tent and keep him company in the afternoons and evenings. In these sessions the goal was to gather lists of inflected verbs, nouns, demonstratives and other word classes, so that he would later be able to give complete paradigms in the grammar he was planning to write. He expresses his gratitude to these young informants in the Preface to *Original Blackfoot texts*:

During my stay in Blackfoot reservation, from the 11<sup>th</sup> of May till the 15<sup>th</sup> of August, I collected vast materials on Blackfoot grammar [...]. These grammatical materials were written down from the mouth of many people, mostly boys and young fellows, who were kind enough to allow me in their leisure-hours to interrogate them about intricate matters. With gratitude I remember the afternoons spent with some of the Mission boys, or in the evening in my tent, when surrounded by young Indians, I wrote down my notes, sitting on a trunk, by the light of a lantern. With gratitude I remember many of their faces and their names, though it may be scarcely possible to retain all of them in my memory. (Uhlenbeck 1911b:v)

His only complaint concerns the lack of assistance received from the local American authorities. It appears that, despite the letters written by Indian Commissioner Valentine, local Indian Agent Churchill and School Superintendent McFatridge did not go out of their way to help the two foreign professors:

Though I was recommended by the Dutch Government and by the Royal Academy of Amsterdam, the American authorities did not do very much to facilitate my

connections with the Indians, and my linguistic investigations among them. The more I appreciate the generous help, I received from the Reverend Fathers of the Holy Family Mission, the more I am grateful for the true friendship, which was shown to me by some educated members of the noble Peigan tribe. (1911b:v)

Plans for a second visit to Montana are already in place by December 1910. Jan de Josselin de Jong will not be going that year (he spends the summer of 1911 on the Red Lake Reservation in Minnesota to study Ojibwe), and therefore it has been decided that his wife Willy will accompany him this time, as Uhlenbeck writes to Schuchardt:

In Mai 1911 gehe ich wieder zu den Indianern Montanas. Meine Frau wird mich dann begleiten. Sie freut sich schon im Voraus, die tanzenden Schwarzfüße beim Sonnenfest zu sehen. (Letter to Schuchardt, 7 December 1910)

We know much more about the circumstances of the second field trip, because Uhlenbeck's wife Willy kept a detailed diary (edited and translated in Eggermont-Molenaar 2005). If we may assume that things were very much the same in 1911 as they had been in 1910, it is perhaps surprising that the second field trip took place at all. The physical circumstances were spartan. They lived in a tent besides Joe (Joseph) Tatsey's tipi, without a kitchen, bathroom or heating. Joe Tatsey's wife Annie cooked for them and initially also did their laundry, but Willy took that over after she decided it was too expensive.

The summer of 1911 was unusually cold and wet, and the diary is full of references to wet blankets, boots and clothes, water running into the tent, sand and dust making everything dirty, endless hard wind, "belly aches", and cold wet feet. In addition, there are various kinds of problems with the informants; and three deaths – the suicide of David Duvall, the collaborator of Clark Wissler, on July 10; and the deaths of Tatsey's mother of old age on 14 August, and, only two days later, of his daughter Josephine of consumption on 16 August – are certainly not conducive to good relations and a regular work schedule. We learn from Willy's diary that Uhlenbeck is frequently irritated about unfamiliar or unexpected behaviour of his informants that delays his work, and we see a reflection of his disappointment in the preface to the text collection produced after the 1911 work:

I conclude this preface with the sincere expression of my gratitude to the Indians, who have furthered my scientific purposes. Still it is a pity, that some well-informed and experienced men among the tribe were not disposed to impart their valuable knowledge, and that some others, who were willing to help me along, could not spend so many hours with me, as I should have liked and needed. (1912a:viii)

From the diary we may conclude that after the deaths of Tatsey's mother and daughter on 14 and 16 August the work schedule becomes even more erratic than before, with funerals that interfere and an understandable reluctance on the part of Joseph Tatsey to come and do linguistic work and on the part of his wife Annie to keep cooking for the

foreigners. The tone in the preface to the first text collection is very different with regard to the informants and expresses a much more sincere-sounding gratitude.



*Annie and Joe (Joseph) Tatsey, 1911; photo Willy Uhlenbeck-Melchior. Courtesy Mary Eggermont-Molenaar*



### Back to the armchair: Early results from the Blackfoot fieldwork (1911-1916)

Uhlenbeck must have spent most of his time after his return from the first fieldtrip organizing his materials. His first text collection is published within a year, in 1911 (1911b). *Original Blackfoot texts, from the Southern Peigans Blackfoot Reservation, Teton County, Montana; with the help of Joseph Tatsey* is a unique collection of stories, presented in a bilingual format; each page has two columns, with the Blackfoot text presented on the left and a close English translation on the right. The most important difference with other Blackfoot text collections is that Uhlenbeck – as well as his student De Josselin de Jong (1914) – gives the material in both Blackfoot and English. All other major text collections from around that time (Grinnell, Wissler & Duvall, McClintock, Maclean, Schultz) are in English only (Kehoe 2005:197). Besides a large number of traditional stories, it contains a text about clan names, some songs, and – in English only – genealogical and personal information about Bear Chief, one of his principal informants (and also the maker of some of the gifts they received; see Hovens 2009). The source of most of the material in this collection is Joseph Tatsey, the man with whom Uhlenbeck worked most of the time and who also was his host and interpreter.

*Original Blackfoot texts* was reviewed by Truman Michelson (1911), and announced with a brief paragraph by K. Th. Preuss (1912). Preuss praised the inclusion of the Blackfoot versions of the stories and the generic variety in the collection, but Michelson was rather critical of the way in which Uhlenbeck represented the phonetics of Blackfoot. His goal throughout seems to have been to provide a narrow phonetic version of what he heard, rather than analyze the sound system of Blackfoot. He never attempts a phonological analysis. Even variant pronunciations of the same word are carefully represented in his transcriptions, as Uhlenbeck explains in the introduction:

The attentive reader of these texts will soon be aware, that the same word in the same grammatical form is not always spelt the same way. There are many vacillations in the sounding of this language (e.g. in putting in, or omitting  $\chi$  and  $\chi'$  before explosives, and before  $s$ ), and I thought it better to express these vacillations in my way of spelling, than to efface them by an arbitrary uniform orthography.

(1911b:vii)

Michelson (1911) is very critical of this approach to the phonology of Blackfoot, and concludes “that these texts do not come up to the standard set by Boas, Sapir, Jones, Goddard and others”. He adds, however, that “considering his brief stay with the Piegans (three months), and that this was his first experience with any spoken American Indian language, Professor Uhlenbeck has accomplished much, – more than could have been expected under the circumstances” (1911:330).



*Bear Chief, Browning, Montana; ca. 1900-1905. Courtesy Glenbow Archives, Calgary (na-1461-43)*

Uhlenbeck reacts with some irritation to Michelson's review in the Preface to *A New Series of Blackfoot texts*:

I am well aware, that my system is capable of refinement and improvement, though I hardly believe, that some of the observations made by my reviewer in the "American Anthropologist" (N.S. Vol. XIII, pp. 326 sqq.) are absolutely correct. I admit, that a sharper line might be drawn between *a* and *α*, *e* and *i*, *o* (*õ*) and *u*, than has been done in my texts. But where I write *-ua* at the end of a word, the *-a* is a full-sounded vowel, and everybody, who knows something of Blackfoot as a spoken language, who has watched the Indians while talking among themselves, will confirm this statement. So *Nápiu* and *Nápiua* stand as equivalents by the side of each other (the shortest form *Nápi* has a different syntactical value). Nevertheless there may be hidden vowels in some other cases, which escaped my hearing. It is a well-known fact, every moment to be observed, that often only part of a word is pronounced clearly, while the rest of it is not even whispered, but only

indicated by articulation. I shall be glad, if my reviewer will be able some day to give us an accurate description of the Blackfoot phonetics. (1912a:vii)

None of Michelson's comments leads U to revise his transcription system or to attempt a systematic analysis of the phonology of Blackfoot: his way of writing Blackfoot is basically identical in both text collections, and even more than twenty-five years later in *Concise Blackfoot Grammar* (1938b) nothing has changed.

The second text collection, *A new series of Blackfoot texts* (1912a) is more than twice as large as the first one, and contains, in addition to traditional Napi stories and other myths, accounts of various aspects of Blackfoot life, events in the lives of his informants, and a very interesting section entitled 'Boys' experiences' (1912a:225-245), which records some of the stories told by Joseph Tatsey's young son John and his friend James Vielle about events in their lives. This book is again very positively reviewed by K. Th. Preuss (1913), in a short announcement that mostly comments on the ethnographic value of the stories. Walter Lehmann (1913) only gives a short summary of the content. The book is also the subject of a brief review by Clark Wissler (1912a), who remarks somewhat tartly that there is almost no new mythological material in the collection and that the non-mythological sections "deal with the best known phases of Plains culture and contain no important data not available in the writings of Grinnell, McClintock, *et al*" (1912:289). While he adds that "[t]his, however, should not be taken as disparaging the work of Professor Uhlenbeck, for it was obviously intended as a linguistic contribution", he completely downplays that what makes the collection unique is the fact that the texts are recorded in Blackfoot rather than only in English. None of the reviews comment on linguistic aspects of the collection or seem to see its bilingual presentation as the important breakthrough that it certainly was.

Further fieldwork trips are not planned. Whether this is because Uhlenbeck feels he has now gathered enough material or whether other factors play a role is not known. But whatever the reason, Uhlenbeck now throws almost all his energy into turning his data into publications on various aspects of Blackfoot language and culture. The first of these is the written version of the lecture 'Geslachts- en persoonsnamen der Peigans' (1912b; published in an English translation in Eggermont-Molenaar 2005:355-366). Based in part on the section 'Names of Clans' from *Original Blackfoot texts* (1911b:1-4), it treats a number of clan and person names and argues that Blackfoot clans are patrilineal and that Blackfoot marriage patterns are exogamous (i.e. one must find a marriage partner outside one's father's and mother's clan). Uhlenbeck also shows that many names are based on nicknames given by others as an insult. This is especially clear in such clan names as 'Not-laughers' (so named because they laughed a lot), 'Small-robbers', 'Bug-people' (a clan whose chief had syphilis), 'Skunks' (a clan whose members smelled bad because of syphilis), 'Lone-eaters' (a clan known for being stingy), 'Black-patch-people' (called after their dirty patched moccasins) (1911b:1-4). He speculates that many names are of recent origin.

Reactions from Robert Lowie (1911) and J.P.B. de Josselin de Jong (1912) focus on Uhlenbeck's analysis of Blackfoot marriage patterns. Lowie criticizes Uhlenbeck's interpretation of these patterns as clan-exogamous. De Josselin de Jong (1912:191-197; English translation in Eggermont-Molenaar 2005:367-373), without mentioning Lowie's critique, provides corroborating evidence for clan-exogamous marriage patterns by analyzing 109 marriages of Blackfoot people he knows. In the clan of the Fat-melters, who are known for inappropriately marrying within their own clan, 11 of 26 marriages (43%) are endogamous. In all the other 18 clans together, only 12 or 13 out of 83 marriages (15%) are endogamous. For De Josselin de Jong, this confirms that, while endogamous marriages do occur and the custom is no longer absolute, most marriages are indeed exogamous. Uhlenbeck reacts only with a one-paragraph note correcting a formulation in De Josselin de Jong's review (1912f).

Work on a grammar of Blackfoot is also under way but does not proceed as speedily as planned. In 1911, after the first fieldwork period, Uhlenbeck optimistically anticipated that he would be able to publish it very soon: "I collected vast materials on Blackfoot grammar, and if I am not thwarted by unforeseen circumstances, I shall publish them before the end of 1913" (1911b:v). After the second fieldwork trip he is more pessimistic, however: "[t]he publication of these texts may cause some delay in studying out and publishing my morphological materials" (1912a:vii-viii) and "the grammar may only be completed in about three years" (Eggermont-Molenaar 2005:355).

In the years that follow, Uhlenbeck publishes pieces which may be seen as building blocks for the grammar. A large article on noun morphology, *Flexion of substantives in Blackfoot* (1913a), humbly subtitled: *A preliminary sketch*, sees the light in 1913. By this time it is clear that the task of writing a grammar is altogether too overwhelming for Uhlenbeck. In 1912 he had suffered a major depressive period which had required a long hospitalization (see Eggermont-Molenaar 2009a) and was unable to envisage himself undertaking large projects. Not that 1912 was an unproductive year by any means: in addition to *A new Series* and 'Geslachts- en persoonsnamen' he publishes a small note on 'The origin of the Otter-lodge' in a festschrift for Vilhelm Thomsen (1912g), a short article about the name Uhlenbeck in *Nederland's Patriciaat*, and no less than 20 short reviews. However, his plans for the immediate future are now restricted to "some more monographs on chapters of Blackfoot morphology" and he has entirely given up on the idea of writing a grammar by himself, instead "leaving the troublesome task of writing a handbook on the whole subject to one of my students, Mr. G.J. Geers" (1913a:3). Ultimately, Geers will not continue his work on Blackfoot (see below) and Uhlenbeck will end up writing the grammar himself, but it will take him more than 25 years to complete.

Also in 1913 Uhlenbeck publishes a review of Clark Wissler's *Ceremonial bundles of the Blackfoot Indians* (1912b). A comparison of Uhlenbeck's review of Wissler's work with Wissler's earlier review of Uhlenbeck's second text collection (see above), shines an interesting light on the differences in viewpoint between the two scholars. Wissler

(1912a) dismisses Uhlenbeck's collection as containing nothing new from an ethnographic point of view, while allowing that Uhlenbeck's goals were linguistic rather than ethnographic. Uhlenbeck, on the other hand, begins by praising Wissler's book as a "very complete and careful monograph" (1913b:39) which "will be of great use to Americanists as well as to students of religion" (1913b:40), but then continues to give a long list of linguistic comments about the meanings and forms of individual words whose interpretation or transcription he does not agree with. It is clear that Wissler and Uhlenbeck have such diametrically opposed interests – Wissler in culture and Uhlenbeck in language – that neither of them will ever be satisfied with the other's work.

More morphological papers follow in 1914 (1914a-d). The output in 1915 and 1916 is largely restricted to reviews and short notes. In 1916 Uhlenbeck publishes the short paper 'Some Blackfoot song texts' (1916c). These texts are not based on his own work, but on audio recordings given to him by the famous ethnographer George Bird Grinnell (1849-1938). Uhlenbeck and his wife had met Grinnell and his wife in Browning, Montana in July 1911 and spent some days with them attending sun dance ceremonies (Eggermont-Molenaar 2005:74). Recording equipment (chiefly wax plates and cylinders) did exist in the early years of the 20<sup>th</sup> century, and was used sporadically to record mostly songs and music, but the audio recording of linguistic field data did not become common until after the middle of the 20<sup>th</sup> century (Nettl 1973). Uhlenbeck himself never made use of audio equipment, as far as I can see. But Grinnell had made these recordings as early as 1897, as Uhlenbeck mentions in the introduction to 'Some Blackfoot song texts':

On my return from Blackfoot reservation in 1911 I stayed a few days at the hospitable house of Dr. G.B. Grinnell, the distinguished author on the culture and folklore of the Plains tribes. Then he had the kindness to give me the following short song texts, recorded by him phonographically in 1897, with the permission to transcribe them, to translate them in my own way, and to publish them. (1916c:241)

Uhlenbeck had published some earlier songs in his own text collections (1911b:66-68; 1912a:210), so this was his third set of song texts.

### **Some theoretical work: "ethnopsychology"**

1917 then sees the publication of two major theoretical papers on North-American languages, which rely heavily on Blackfoot and other Algonquian material (1917a, b). Both appear in Dutch in *Verlagen en Mededeelingen der Koninklijke Akademie van Wetenschappen, Afdeling Letterkunde*, and deal with different aspects of the same issue which had fascinated Uhlenbeck for a long time: the similarities between person agreement morphology on verbs and nouns, which expresses agent and patient on the verb, and possessor on the noun. These papers are illustrative of what Uhlenbeck himself called the "ethno-psychological" approach to language. In these papers Uhlenbeck draws typological

parallels between Algonquian and Pre-Indo-European, which he thinks was a language much like a typical North-American language of the Algonquian type (Genee 2003:154-5).

The ethno-psychological approach to language attempts to discover typological generalizations among geographically and genetically distant languages and explain these commonalities through general psychological principles. Uhlenbeck clearly sees this type of explanation as the ultimate goal of linguistics: “Het is somtijds de taak van den grammaticus zijn toehoorders door dorre zandwoestijnen of zilte steppen te voeren, indien hij met hen de blinkende stad van het ethnopsychologisch inzicht wil bereiken [...]” (1917a: 208). The main conclusion of the paper ‘Het passieve karakter van het verbum transitivum of van het verbum actionis in talen van Noord-Amerika’ (1917a) is that in the large majority of languages of North America the transitive or active verb is felt to be passive rather than active, in such a way that ‘I eat it’ is really “felt” as meaning ‘it is eaten by you’ and that ‘I go’ is really felt as meaning ‘going is being done by me’ (1917a:197). Uhlenbeck bases these conclusions on the agreement patterns found in these languages, which would in modern terms be called “ergative” and “active”. He posits that this type of construction is in a sense more “primitive” than the Indo-European pattern; hence, he attempts to find traces of the “older” pattern in early Indo-European and to explain North-American languages with a more “European” type pattern as “more developed”:

[G]elijk in onzen taalstam de nominale en pronominale flexie nog sporen draagt van een ouderen toestand, waar [...] het verbum transitivum, zoo niet het geheele verbum actionis, passief in zijn opvatting was, zoo kunnen ook talen als het Klamath en Maidu een periode gekend hebben, die het transitieve verbum, of het handelingswerkwoord in het algemeen, passief concipieerde. (1917a:212)

This “passivity” is, for Uhlenbeck, connected to the ethno-psychological mentality of “the primitive language intuition”:

Voor het primitieve taalgevoel is de eigenlijke agens een verborgen macht. Deze werkt door middel van den schijnbaren agens, het primaire instrument, dat zelf weer gebruik kan maken van een secundair werktuig. Neem b.v. een zinnetje als “Den vogel doodt hij met een steen”. Een Blackfoot zal dit op de volgende wijze uitdrukken: “De vogel door-middel-van-wordt-gedood-door-hem een steen”. Hij die doodt is [...] inderdaad slechts de schijnbare agens, het primaire instrument, dat zelf door een verborgen macht wordt beheerscht. [...] Den grond van zulke verschijnselen moeten wij [...] niet zoeken in speciale levens-condities, maar in een zekere “primitieve” geestesgesteldheid. (1917a:214-215)

In conclusion:

[U]it de door mij besproken feiten blijkt duidelijk, dat voor een zekere fase van geestesontwikkeling de passieve constructie de voor-de-hand-liggende en natuur-

lijke is. In een geval als dat van het Baskisch heeft die constructie [...] de primitieve mentaliteit, waarin zij haar oorsprong had gevonden, tot in de dagen van intellectuele analyse overleefd. Zij is daar, niet anders dan b.v. op ons taalgebied het grammatisch geslacht, een niet meer begrepen herinnering aan een langvervlogen tijdperk van magisch-denkende voorouders. (1917a:216)

Uhlenbeck seems always to have remained convinced of the correctness of the ethno-psychological and historical conclusions he drew in these papers, given for instance their later translations into French (1922) and Russian (1950a, b) and his continued use of the key terms *casus energeticus* and *casus inertiae* in later work. The famous American linguist Edward Sapir (1884-1939), however, was decidedly less than impressed. He published rather harsh reviews of both articles in the first issue of *International Journal of American Linguistics (IJAL)* (Sapir 1917a, b; see also Genee 2005:187-8 and Swiggers 1988). He accuses Uhlenbeck of “ethnological speculation on the basis of linguistic data” (1917a:69) and “reasons of a speculatively psychological order” (1917b:78). To Uhlenbeck’s historical claim that the “passive” interpretations he finds in North-America are somehow more primitive and therefore older, Sapir is “not inclined to attach much importance. Such questions must be attacked morphologically and historically, not ethno-psychologically” (1917a:73). He disdainfully refers to “Uhlenbeck’s mystical theory of identification” and adds: “The less we operate with ‘primitive’ psychology, the better. Modern research is beginning to make it clear that the psychology of civilized man is primitive enough to explain the mental processes of savages” (1917b:79). One might be inclined to see in this last remark a reference to the ongoing Great War.

These reviews must have been painful to Uhlenbeck, not in the last place because they were published in the first issue of *IJAL*. In 1915 Franz Boas had invited Uhlenbeck to be on the editorial board of the new journal (letter of 4 October 1915), and Uhlenbeck had accepted (letter of 16 Oct. 1915). From correspondence between Uhlenbeck and Boas in the course of 1916 it is clear that Uhlenbeck had wanted to publish English versions of these papers in *IJAL*, but that Boas did not want to include them. In the course of 1916 Boas had apparently expressed reservations with regard to the first article in a letter to Uhlenbeck, which has unfortunately not been preserved, but whose existence we can derive from Uhlenbeck’s response of 13 May 1916. On 11 January 1917 Uhlenbeck offers Boas English translations of both articles for the first issue of *IJAL*, but Boas rebuffs him and says that instead, “if it is agreeable to you, I will ask Dr. Sapir to send me reviews of your articles” (Boas to Uhlenbeck, 8 Feb. 1917). And this is what happened.

If Uhlenbeck was upset, it does not show in the postcards he sends in 1918 thanking Boas for sending him copies of the first issue of *IJAL*. As far as I can see, he never reacted to Sapir’s criticism in print. In a letter to Sapir he merely says:

I regret that you differ so much from me, but I still hope that in the course of time the correctness of my general views will be corroborated by new facts. Though I

have a strong feeling of being right in the main, I allow, of course, that I may err in some details. At all events I am very grateful to you for the kind attention you have given to my theories. (Letter to Sapir, 18 Dec. 1917)

As Noordegraaf (2009) points out, Uhlenbeck remained convinced of the correctness of the ethno-psychological approach, and was critical of Sapir and other young linguists who failed to see things likewise. The exchange between Uhlenbeck and Sapir with regard to Uhlenbeck's papers may therefore be seen as resulting from fundamentally opposing views of the primary approach to the study of language.

### **The long road toward the *Magnum opus***

The year 1917 brings another disappointment for Uhlenbeck. On 18 October of that year his student Gerardus Johannes Geers (1891-1965) defended his dissertation *The Adverbial and Prepositional Prefixes in Blackfoot* (1917). The dissertation may be seen as a first attempt to begin sorting through Uhlenbeck's and de Josselin de Jong's text collections with a view to enlightening the verbal morphology, one of the tasks that certainly needed to be carried out in the process of writing a handbook. As we have seen above, five years earlier, in 1912, it had been Uhlenbeck's plan to leave the writing of a Blackfoot handbook to this promising student. It would seem, however, that Uhlenbeck was seriously disappointed by Geers' work. He wrote a harsh review of his own student's dissertation in *Museum* (1917c), in which he accused Geers of producing "beschouwingen die zich maar zelden boven het peil van die der door hem bestreden Amerikaanse geleerden verheffen" and pointedly warns: "Laten wij liever wachten, totdat het Blackfoot wat beter bekend en beschreven is, eer wij het als een sportterrein voor ons vernuft gaan gebuiken." He concludes, in an unusually personal attack on Geers: "In één woord: het werk van den heer Geers is een typische "eersteling". Hij zal nog veel aan zichzelf moeten veranderen, nog lang zich moeten toeleggen op zelfbeheersching en zelfbeperking, voordat hij het meesterschap bereikt, dat – naar wij reden hebben te verwachten – voor hem in de toekomst is weggelegd." The second part of the last sentence seems to be aimed at preserving Geers for the handbook project, despite Uhlenbeck's clear misgivings. But, perhaps not surprisingly, Geers did not continue his work on Blackfoot. Instead, he became a specialist on Spanish literature, working in Spain for a while and later as a lecturer and professor in Groningen.

It now remained for Uhlenbeck himself to continue work on the Blackfoot grammar, but the work slows down considerably. Three years later we see another large chapter, *A survey of the non-pronominal and non-formative affixes of the Blackfoot verb* (1920). Another three years later a one-page note is published in *IJAL* (1923), followed in 1925 by three more substantial papers: 'Some word comparisons between Blackfoot and other Algonquian languages' (1925a), 'Nieuwe woorden in het Blackfoot' (1925b), and a short note on 'Blackfoot imitá(ua), dog' (1925c). 'Nieuwe woorden' deals with the way in



which the Blackfoot language creates new words for new things. Some interesting examples are the literal translations of new words for buildings or objects, such as *church*, which literally means: 'we pray there'; *dining room*: 'we eat there'; *store*: 'we buy there'; *fork*: 'we eat with it'; *key*: 'we open with it'; *telephone*: 'we speak with it'; *clock*: 'we pass the time of day with it'. Also interesting are words indicating nationalities, such as *Frenchman*: 'real white man' (as opposed to Englishman); *Dutch* or *German*: 'not real (lost) white man'; *Canadian*: 'northerly white man'; *Americans*: 'those who have big knives'; *priest*: 'holy white man'; *bishop*: 'big holy white man' (19125c:2-8).

The English-language paper 'Some word-comparisons' (1925a) was published in *IJAL*, which gave it a wider audience. In it, Uhlenbeck proposes a large number of word and morpheme comparisons which, to a trained eye, are suggestive of the type of correspondences that would typically be used to prove genetic relationships between languages. However, Uhlenbeck simply presents them, without any indication as to how he intends them to be interpreted. He probably saw his correspondences merely as corroborating evidence for a genetic affiliation which was not in doubt, materials to be used for the further historical investigation of the Algonquian languages.

In the years 1927-1929 Uhlenbeck publishes a number of smaller papers on Blackfoot. 'De afwezigheid der datief-conceptie in het Blackfoot' (1927c) describes constructions with verbs which would typically have both a direct (accusative) and an indirect (dative) object in many European languages, but which show agreement with the indirect rather than the direct object in Blackfoot. Uhlenbeck uses the term *casus inertiae* for the indirect object. The paper appears in a rather obscure festschrift and seems mainly to have been an opportunity for Uhlenbeck to sort these constructions for his grammar. The short note 'Additional Blackfoot-Arapaho comparisons' (1927b), which continues 1925a, appears again in *IJAL*. The next year we see another descriptive festschrift-article in Dutch, 'Het emphatisch gebruik van relatief-pronominale uitgangen in het Blackfoot' (1928), followed by another two-page 'Blackfoot note[s]' in *IJAL* (1929).

A big project had been in the works, however, in these years in which Uhlenbeck did not publish much substantial work on Blackfoot. 1930 saw the first part of the Blackfoot dictionary that Uhlenbeck's student Robert H. van Gulik had prepared out of the thousands of filing cards that Uhlenbeck had collected in 1910 and 1911. This first part (Uhlenbeck and Van Gulik 1930) contains the English-to-Blackfoot portion of the dictionary. The second part, with the Blackfoot-to-English portion is published in 1934. As K.Th. Preuss mentions in his review in *Deutsche Literaturzeitung*, "die amerikanistische Wissenschaft hat allen Anlass, U. zu beglückwünschen, dass er in unermüdlicher 24jähriger Arbeit das mustergültig aufgenommene Material einer Algonkinsprache ebenso gründlich grammatisch und lexikalisch durchgearbeitet dem allgemeinen Studium zur Verfügung gestellt hat" (1935:2064). But he is later proven wrong when he states: "Mit diesem umfangreichen Lexikon, das dem kürzeren English-Blackfoot-Vokabular 4 Jahre später gefolgt is, erreicht die Auswertung der von Uhlenbeck und de Josselin de

Jong 1910/11 bei den südlichen Peigan in Montana gesammelten Texte ihr vorläufiges Ende" (2064).

Because after two more one-page notes in *IJAL* (1936a, b) and a two-page note in another festschrift (1938a), Uhlenbeck's real *magnum opus*, *A concise Blackfoot grammar*, finally appears in 1938, more than a quarter of a century after his fieldwork trips. This will be his last publication on Blackfoot. In 19 chapters Uhlenbeck treats almost the whole derivational and inflectional morphology of Blackfoot, with copious examples illustrating every single point. The book begins with a ten page chapter on phonology, followed by 17 chapters on morphology. A final chapter treats interjections. There is no chapter on syntax. The main division into phonology and morphology follows the standard organization of many grammars in those days, but the very small space devoted to sounds compared to words is unusual. The book can really not be called "concise", of course, although Uhlenbeck himself stresses in his Preface that "[t]his paper does not pretend to give more than the outlines of Blackfoot grammar" (1938b:i).

Robert van Gulik's assistance with the dictionaries had been so extensive that Uhlenbeck had made him co-author. The grammar only mentions himself as its author, but he did not finish the work without help. As he mentions in his Preface, Jan de Josselin de Jong corrected the proofs for the whole book, an enormous task for a grammar of this kind. As we know, Uhlenbeck was living in Lugano (Switzerland) by this time, and contacts with the publisher and printer in Amsterdam would have been cumbersome. De Josselin de Jong was professor at Leiden in those years and would have had easier access to both.

De Josselin de Jong, in a review in the Dutch journal *Museum* (1939), praises Uhlenbeck's careful supply of copious examples to illustrate every point he makes and emphasizes the fact that it is the result of many long years of work on the language. The grammar is also reviewed by C.F. Voegelin (1938), who calls it a "valuable document" (1938:726) and ends his review by saying that "[t]he author's purpose is accomplished: his work does indeed advance the day when a comparative grammar of the entire Algonquian stock will be possible" (727-8). But Voegelin is also critical. He accuses Uhlenbeck of in fact doing what he is always saying a linguist should never do: describe one language in terms of the structure and grammatical terminology of another: "the symmetry of Blackfoot word and stem classes is somewhat obscured by the introduction of Indo-European categories and metaphors from physics" (727). Voegelin does not give examples to clarify what he means, but one could speculate that by "Indo-European categories" he means the use of terms such as "subject" and the distinction between active and passive verb forms, and that by "metaphors from physics" he means things like Uhlenbeck's use of the terms "centrifugal" and "centripetal" for what we would now call "direct" and "inverse".

The bulk of Voegelin's criticisms, however, stem from Uhlenbeck's failure to benefit from the advances made in phonology in the years between the fieldwork and the publication of the grammar. This omission makes it hard to see the wood for the trees:

[A] dozen different symbols represent vowels where only three symbols are needed. When a linguist makes more distinctions than a language makes, there is naturally some duplication in the use of different symbols. [...] Now it would have been possible for the author to have said we have three vowel phonemes, with such and such phonetic characteristics of each [...]. In this way the main features of Blackfoot would be brought into clear relief and the marginal details subordinated.  
(1938:727)

Voegelin's comments echo the criticism expressed by Michelson in 1911 (see above). Uhlenbeck's work on the representation of Blackfoot sounds appears to have been hampered by a number of different circumstances. First, he seems to have had trouble correctly perceiving some of the phonetic contrasts that are important to the sound system of Blackfoot; this led to a number of errors and inconsistencies in his writing of the language, especially with respect to voiceless ("whispered") vowels, long vs. short sounds, and glottal stops. Second, he was unable or unwilling to keep up with the developments in phonological theory that took place in the first half of the 20<sup>th</sup> century (see e.g. Robins 1990:222-225), which could – and, as Voegelin implies, should – have led him to a better analysis of the Blackfoot sound system than he was ultimately able to provide. And finally, he was also hindered by that fact that he did not have audio recordings of his materials and therefore had to rely on the transcriptions he had made more than 25 years ago.

### Conclusion

The work on Blackfoot occupies a unique position in Uhlenbeck's career as a linguist. It was the only language he did fieldwork on, and the first and only time he attempted a full description of a living language by publishing not only some very significant text collections, but also both a dictionary and a grammar. The work on Blackfoot played an important role in the development of his ethno-psychological theories, not in the last place because it brought him in close contact with the speakers of the language. His presentation of the language was modern in the way in which he juxtaposed the Blackfoot and English versions of the collected materials and in the explicit recognition of the important role played by his interpreter and informants. However, his treatment of Blackfoot also exemplifies his failure to keep up with developments in phonological theory, resulting in an extremely detailed, but underanalyzed presentation of the language. As Voegelin puts it politely: "In Professor Uhlenbeck's work each detail is given equally loving attention, whether applying to a few words or to every word in the language" (1938:727).

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## NOTES

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<sup>2</sup> Correspondence is cited from the following sources: Azkue Biblioteka, Archive Lacombe, Spain (letters to Georges Lacombe), Universitätsbibliothek Graz, Schuchardt Archives (letters to Hugo Schuchardt), American Philosophical Society (letters to Franz Boas), Canadian Museum of Civilization, Ottawa (letters to Edward Sapir), Washington National Archives and Records (letters to and from Blackfeet Indian Agency, Montana). For a detailed description of Uhlenbeck's professional and personal correspondence see Eggermont-Molenaar 2009b.

<sup>3</sup> The use of the term "superstitions" may be offensive to many modern readers. From Uhlenbeck's other writings, however, it is clear that he did not mean anything degrading by its use. Uhlenbeck himself was not religious, and there are no indications in any of his or his wife's writings that he felt that Christianity was superior to other religious and spiritual customs. Uhlenbeck's English was less than perfectly fluent, and it is possible that he simply made an unfortunate choice of words.

<sup>4</sup> Uhlenbeck writes "You" with a capital, after the custom of writing the Dutch and German formal second person pronouns, *U* and *Sie*, with a capital.