NEARLY PERFECT:
NOTES ON THE FAILURES OF SALVAGE LINGUISTICS

JEFFREY WAJSBERG

A DISSERTATION SUBMITTED TO
THE FACULTY OF GRADUATE STUDIES
IN PARTIAL FULFILLMENT OF THE REQUIREMENTS
FOR THE DEGREE OF DOCTOR OF PHILOSOPHY

GRADUATE PROGRAM IN SCIENCE AND TECHNOLOGY STUDIES
YORK UNIVERSITY
TORONTO, ONTARIO

July 2018

ABSTRACT

This dissertation examines the "salvage" era of American linguistics (c.1910–1940) and its focus on the extraction of knowledges and cultural artifacts from Indigenous groups whose civilizations were believed in peril. Through close readings of historical archives and published materials, I imbricate the history of these scientific collection practices through the interpretive frames of Science & Technology Studies (STS), deconstructive criticism, and postcolonial theory. I centre the project on the career of linguist-anthropologist Edward Sapir, seizing upon his belief that linguistics was "more nearly perfect" than other human sciences—that linguistic methods were more akin to those of the natural sciences or formal mathematics. I employ Sapir as the chief focalizer of my work to map the changing topography of the language sciences in North America over these pivotal decades of disciplinary formation. Failure, here, offers a heuristic device to interrogate the linear logics of science and success which buttress that desire for perfection. Both conceptually and historically, the dialectics of failure and success throw into relief the vicissitudes of fieldwork, the uncertainty of patronage relationships, and the untenable promise of salvage that characterized these years. Through this approach, I present linguistics instead as a kairotic science—from the Greek *kairos*, suggesting opportunity—not perfect, but situated vividly in the world, bound by space, identity, and time. I examine how linguists conducted their collection work through the extension of a scientific network (Chapter 1), their construction of a scientific identity to the gradual exclusion of amateurs and the reduction of informant contributions (Chapter 2), and the development of an experimental system within the temporalities of fieldwork (Chapter 3). My dissertation hence invites a critical intervention within the history linguistics to re-encounter the science's disregarded past and re-think its shared responsibility toward Indigenous communities in the present.

For my cat

I will always be waiting.

ACKNOWLEDGEMENTS

My work was generously funded by the Government of Canada through a SSHRC Joseph-Armand Bombardier CGS-Doctoral Award, which included a Michael Smith Foreign Study Supplement. I received additional support through a Provost Dissertation Scholarship from York University and an Andrew W. Mellon Foundation Fellowship from the American Philosophical Society.

I could not have accomplished this feat without my own network of supporters. I thank foremost my advisors Joan Steigerwald, Mike Pettit, and Sheila Embleton: your commitment, insight, and incision have enriched this project and contributed to my growth as an academic and intellectual. I was also influenced by other faculty members at York: Jamie Elwick, Edward Jones-Imhotep, Aryn Martin, and Natasha Myers. I owe a debt to Henry Cowles, Catherine Fountain, Judith Kaplan, Melissa Littlefield, Sylvia Nickerson, and Thomas Teo for their responses to my work in varying capacities, as well as to Barbara Dancygier and Margery Fee for their initial support in pursuing a Ph.D. I extend my gratitude to Joseph Errington and the Department of Anthropology at Yale University for hosting my first archival excursion; to Sylvie Laflamme, Jonathan Wise, and especially Benoit Thériault at the archives of the Canadian Museum of History; and to all the friendly, engaging, and always helpful archivists and staff at the American Philosophical Society, in particular Paul Sutherland.

I owe as much to my personal connections. I thank my friends and colleagues in the Department of Science and Technology Studies who shaped the research culture and social fabric of the program: Aadita, Aftab, Bretton, Callum, Cameron, Cath, Dorian, Drew, Ellie, Emily, Erin, Jason, Kasey, Kelly, Nanna, Travis, Tyler, Yana, and many others in the Greater Toronto Area. Special appreciation belongs to Angela Cope and Shayna Fox Lee, whose intelligence, conversation, and kindness have affected and inspired me beyond the scope of any degree. I am also grateful to the friends who put me up and put up with me at different stages of the research and writing process: Anna, Ingrid, Laurie, Marisa, Megan&Drew, Thelma, Wanheng, and others. Anita Law deserves singular mention for the innumerable calories and witty dialogue we shared over the years. Finally, I am thankful for the local businesses in Montréal that fed my body and morale. Shout-out to Encore Books, whose patient staff abided my loitering, and to John, Andrea, and of course AJ at Maté Latte and Kokkino café for fueling me with the requisite dose of caffeine. Thanks also to fellow café denizens Emilie Malame for emotional support and memes and the strident Dr. Eric Savoy for his unofficial guidance and the occasional *mot* juste.

I would not be here if not for my family: Denise, Leo, and Joseph. Much love and hope for the future.

TABLE OF CONTENTS

LIST OF FIGURES

~ Introduction ~

"Linguistics may be said to have begun its scientific career with the comparative study and reconstruction of the Indo-European languages. In the course of their detailed researches Indo-European linguists have gradually developed a technique which is probably more nearly perfect than that of any other science dealing with man's institutions."

- Edward Sapir, "The Status of Linguistics as a Science," 1929

Nearly Perfect

My dissertation addresses the professional development of a science of language in North America roughly between 1910 and 1940. In this period, American linguistics made great strides to obtain autonomy and recognition as a scientific discipline: linguists established scholarly networks, launched publication venues, communicated expertise, and assembled methodologies that were predominately their own. Through such labour and invention, linguistics was put on the map alongside the other, more secure social sciences of the time. Accounts of this disciplinary formation are often told through a lineage of great men: a progression that begins with Franz Boas, proceeds to Edward Sapir, and ends with Leonard Bloomfield (or perhaps Noam Chomsky). In my retelling of this story, I register the diverse set of factors that made a linguistic science possible: the informants, institutions, intuitions, inscriptions, and other inhabitants that contributed to the incipient field of study. Above all, I recognize the centrality of Indigeneity in this era of American linguistics. In these decades, scientific collection practices in anthropology and linguistics were motivated by the zeitgeist of "salvage" to document Indigenous languages and cultures: collectors made efforts to extract practices, knowledges, artifacts, and other ephemera from Indigenous civilizations that they believed were fated to disappear in the wake of Western expansion. The arc of empirical and epistemological success for linguistics was predicated on this prognostication of cultural failure. The story I tell here puts these narratives back into relation, knotting the rise of American linguistics to the Indigenous communities from whom they took linguistic data and without whom the science might not exist as it does today. As such, my dissertation is addressed to linguists interested in their disciplinary history, historians and science studies scholars attentive to the formation of a human science, and Indigenous groups investigating the entanglement of their languages with Western knowledge-taking practices.

The scope of my project coincides with the career of linguist-anthropologist Edward Sapir (1884–1939), a superlative scholar and categorical humanist. Neither biography nor intellectual history, the dissertation employs Sapir as its chief focalizer to map the changing topography of the language sciences in North America over these pivotal decades of disciplinary formation. Sapir's virtuosity spanned topics of language, culture, personality, aesthetics, and more, but his most notable contributions were to the science of linguistics: his scholarship and teachings shaped a uniquely Americanist linguistic tradition in the first half of the twentieth century. A Polish-born, Jewish emigrant, Sapir studied Germanic Philology in his B. A. (1904) and M. A. (1905) at Columbia University before earning a doctorate in Anthropology (1909) under the tutelage of Boas. The influence of Boas led him to study the Indigenous peoples of North America, a direction that would characterize much of his life's work. Sapir produced or supervised extensive grammars, dictionaries, and typologies of Indigenous languages, and he helped to found the prevailing techniques for training, collection, and analysis within this field of study. My dissertation follows Sapir from his first institutional appointment as Chief Ethnologist of the Anthropological Division for the Geological Survey of Canada (1910–1925); to his protégé Benjamin Lee Whorf, with whom he is frequently linked through the so-called "Sapir-Whorf hypothesis"; and finally to his last appointment at the "First Yale School" of Linguistics (1931–1939), where he trained a coterie of students focused on the study of Indigenous languages.

In the chapters that follow, I seize upon Sapir's belief, expressed in the epigraph, that linguistics was "more nearly perfect" than other human sciences—that their methods were more akin to those of the natural sciences or formal mathematics. The peculiar phrase, from which I derive my dissertation title, deserves brief comment. It occurred once elsewhere in Sapir's oeuvre, in a vastly different context: an article on the semantics of "polar gradation" of comparative terms. He drew on a case from the English language:

> "Perfect" is perhaps the best example of a polar term. . . . Through the habit of using polar terms only to indicate some measure of falling short of their proper significance they may finally take on a less than polar function. Thus, "perfect" comes to mean to some people, and to all people in certain contexts, merely "very good." This paves the way for the secondary grading of polar terms in a positive direction, e.g. "more perfect" and "most perfect." Logically such terms might be interpreted to mean "more nearly perfect" and "most nearly perfect" (conditioned superlative with polar goal); actually, that is psychologically, they denote rather "better" and "best" in an upper tract of "good." (1944/1963: 148)

The implication drawn from Sapir's usage, then, was that linguistics similarly belonged to the "upper tract" of the sciences: approaching the pole of perfection but, by degrees, failing to achieve it. Failure, in my work, offers a heuristic device to interrogate the linear logics of science and success which buttress the desire for perfection in linguistics and, I would suggest, in narratives of progress that inform any scientific discipline. Bruno Latour (1993) observes that a key feature of Western modernity is a belief in the emancipation of humanity and the domination of nature through science and technology. This form of progress entails a separation of knowledge from power that simultaneously excludes "premodern" civilizations—from whom the moderns nonetheless take—and divides nature and culture into discrete and incommensurable domains of specialization. A history of science trained on failure, by contrast, complicates the clean lines of modernity and engages the mess of social, political, and material entanglements that science obviates so mastery can be (nearly) attained.

The dissertation comprises three extended chapters that bring to the fore these themes of fallibility in the linguist's pursuit of scientific mastery. Both conceptually and historically, the dialectics of failure and success throw into relief the vicissitudes of fieldwork, the uncertainty of patronage relationships, and the untenable promise of salvage that characterized these years of American linguistics. Through this approach, I present linguistics instead as a kairotic science—from the Greek *kairos*, suggesting opportunity—not perfect, but situated vividly in the world, bound by space, identity, and time. I apply these categories of analysis, respectively, to the extension of Sapir's network of collection practices from his administrative centre at the Victoria Memorial Museum in Canada; to Sapir's apprentice Whorf and his efforts to construct a legible professional identity amid the emerging scientific culture of linguistics; and finally to the politics of temporal sovereignty that underwrote the descriptive work of his graduate program at Yale University. Below, I outline the contexts—historical and theoretical—that bear on my own efforts to situate these actors in the conditions that gave rise to their scientific activities.

❖ Historical Context: Salvage Linguistics.

The histories of two human sciences converge in the works I address: Americanist anthropology and European philology.[1] My study joins criticisms in Science & Technology Studies (STS) that consider the development of scientific disciplines through varied optics: through the lenses of institutions and interstices (Isaac 2012, Lenoir 1997), charisma and credibility (Clark 2006, Gieryn 1999), or epistemology and epideictic oratory (Knorr-Cetina 1999, Latour 1987), to name but a few influences upon my work. Within the history of linguistics, I situate my work in relation to others who have assessed the growth and development of schools of thought (Koerner 1999; Murray 1994, 1998), the science's professionalization in America (Alter 2005, Andresen 1990, Martin-Nielson 2011), the relationship of linguistics to anthropology (Darnell 1990, 1998, 2001), and the colonial legacy of the field (Errington 2008). I distinguish my project by foregrounding the co-construction of a scientific ethos for American linguistics in relation to the politics of collection, placing this historiography in conversation with scholars who have likewise examined the entanglements of human sciences and classification practices. These studies span the psy-disciplines (Igo 2007, Lemov 2005), race and anthropology (Conklin 2013, Fabian 2010, Teslow 2014, Zimmerman 2001), genomics (Reardon 2005, TallBear 2013), and medicine (Anderson 2006, 2008). Comparatively less work has considered the

---

[1] The term *Americanist* refers to a specialist in the study of "American Indians": the phrase in itself evokes the problematic history of Western classifications that postcolonial and decolonial scholarship calls into question. Sadiah Qureshi (2011), however, recognizes the irresolution of "appropriate" alternatives: "in Britain *Native American* is usually considered the least offensive term possible, while *Indian* is almost never used; however, in the United States the term has been reappropriated in some contexts" (287). In most instances, I follow the contemporary usage of *Indigenous* or *Native*, but I acknowledge that the referential uncertainty of these terms indexes the failure of settler-colonial language and its inadequacy to define First Peoples.

collection practices of linguistics—with some notable exceptions (Brain 1998, Fountain 2013, Kaplan 2013, 2017). My work asks how the science of language figures into these broader questions of race, culture, and difference, on the one hand, and of scientific authority, knowledge production, and empire on the other.

First, some background on the development of linguistics and anthropology is necessary. In America, the professionalization of anthropology occurred between 1879 and 1920, developing through multiple institutions and expanding scholarly networks. The Bureau of American Ethnology was founded in 1879 by John Wesley Powell, at a time when the study was rooted in local science societies of amateur anthropologists. From 1900 to 1920, anthropology began to shift from the government-sponsored Bureau to museum and university settings, although there remained continuity and cooperation—if uneasy—among these groups (Darnell 1998). Boas's school at Columbia served as a model for the academic program. The Boasian "four fields" approach gathered under one rubric the study of archaeology, physical anthropology, cultural ethnology, and linguistics.[2] All of Boas's students, including Sapir, had some training in each subfield. The four modules were held to be discrete but complementary axes for the study of human diversity: they could be investigated "independently or in combination" (Boas 1911/1963: 137), but no element was fully soluble into another.[3] Within this schema, linguistics held a privileged place as a supportive epistemology for ethnography, if not yet as an independent scientific domain:

[2] There was continual tension among the four subfields, which diversified into competing paradigms post-WWII (Darnell 1998: xi).
[3] Sara Eigen Figal (2008) examines how the categories of *race* and *culture* emerged in eighteenth-century natural histories and in the context of debates over colonialism and slavery. She connects the rise of hereditary and genealogical thinking about biological species with a language charged with the moral character of the family. Within lay and scientific discourses, the notion of racial difference came to limit or qualify membership in that family (i.e. the human species). George Stocking (1968) considers the extension of these categories in the history of anthropology from the early nineteenth century to the 1930s.

language was a phenomenon manifestly human and "the most plausible" means of classifying cultural variety (Darnell 1998: xii). In Sapir, however, the anthropological imperative for collection and classification met another tradition: comparative philology.

Nineteenth-century philologists in Europe famously succeeded in reconstructing a "dead" language: Proto–Indo-European (PIE), the common tongue from which most languages of Europe are believed to descend. PIE was not attested by any written records, but philologists derived its sound system from internal principles of language change through a technique known as the "comparative method."[4] Influenced by natural histories, these comparativists employed branching "tree" diagrams to model the genealogy of this language family, or linguistic "stock," with accumulated "sound laws" demarcating significant linguistic breaks in its history. These models quickly permeated public and scientific imaginaries, offering Europeans a means by which to envision their common history (Errington 2008) and scientists a useful site for interdisciplinary transfer (Alter 1999).[5] As Joseph Errington points out, however, while the circulation of these models offered Europeans a portrait of the vastness of human diversity and a deep history most of their citizens could share, their work also became a means by which linguistic difference could be appropriated for nationalist and imperialist narratives.[6] The results were used to

[4] The comparative method employs a systematic comparison of sound features between two or more related languages to derive properties of those languages' common ancestor.

[5] Max Müller's lecture on "Comparative Mythology" in 1858 introduced comparative philology to a broader English audience.

[6] Consider, as Christopher Hutton (1999) does, the propagandistic use of Indo-Germanic in Nazi Germany. Hutton challenges the accepted narrative that National Socialists "confused" the subjects of linguistics and race. Instead, he argues that linguistic thought was (and still is) central to the modeling of an ordered society—and that race sciences took their lead from the linguistics of the nineteenth century. His book thus seeks to reintegrate the work done by German linguists during the Third Reich into the broader history of Western linguistics, from which it has been bracketed off—and hence to destabilize the contemporary trend to see linguistics as ideologically neutral.

establish new hierarchies that placed Europe above its colonies (Hackert 2009). Correspondingly, in America, Daniel Brinton employed linguistic classifications to divide "primitive" from "civilized" cultural groups (Baker 1998, 2010).[7] By contrast to this ranking of cultures couched in evolutionary theory, Boasian anthropology was founded on the principle of historical particularism—the belief that each culture's development was relative to the historical experiences of that group—and its branch of linguistic inquiry was no different in that respect. The works of Sapir, Boas, and their colleagues sought to displace scientific racism and establish a positive and progressive reading of the world's linguistic, cultural, and physical diversity in their stead.[8]

In my work, I am interested in the extension of comparative linguistics to this American anthropological tradition in the early twentieth century—and its failures. Sapir, propelled by trust in the comparative method and confident in its potential success in America, similarly attempted to reconstruct a shared prehistory among Indigenous groups. However, where in Europe comparative philology had vast written corpora from which to draw, Americanists worked predominately on oral cultures. Linguists had to develop descriptive techniques for conducting fieldwork before venturing to make comparisons.[9] Collection and storage of linguistic data became the priority, and models of analysis that

---

[7] Brinton had difficulty funding his linguistic research and later turned to anthropology, particularly of racial classification. As Lee Baker (2010) observes, American anthropology in the nineteenth century was less established than the other big social sciences of economics or psychology (117), and anthropologists made their work relevant through the science of race.

[8] Tracy Teslow (2014) reminds us of anthropology's role in exhibiting racial formations to American publics. Though remembered now for their antiracism, Boas and the adherents to his doctrines did not think that race was an inherently problematic biological concept: rather, they believed a "good" racial science would invariably correct the bad.

[9] Indeed, one of the markers that linguists utilized to distinguish their work as science from the textual tradition of philology was their ability to account for the structure of oral languages, whether they be Indigenous or varieties of American English.

better suited field conditions soon predominated.[10] Other external factors arose to complicate this knowledge production. Despite the empirical certainty that his training in comparative linguistics conferred on him, Sapir was continually frustrated that historical methods failed to engender credibility among his peers in anthropology.[11] Moreover, practitioners interested in the scientific study of language were scattered across disparate departments (anthropology, literature, education) and had to meet and share their work under the auspices of other associations: the American Orientalist Society (1842), the American Philological Society (1868), or the Modern Language Association (1883).[12] The founding of the Linguistic Society of America (LSA) in 1924 and its journal *Language* in 1925 marked an important change in their situation. Linguists finally had a place of their own. At last, they had positioned themselves to reap the benefits of professionalization: salaried positions, scholarly networks, new outlets for publication, and recognition (however ambivalent) as a science. As Janet Martin-Nielsen (2011) reminds us, the other social sciences had already organized as professional scientific disciplines in America during the nineteenth century.[13] Linguistics was still newly organized, still vying for epistemological and financial independence, and still working to solidify its authority in the

---

[10] Janet Martin-Nielsen (2010) comments that anthropological linguistics before World War II was more concerned with the empirical practices of fieldwork than in theory; however, as the following chapters show, the interwar period was also a thriving period for the theorization of a science of language.

[11] The term *engender* is not used incidentally. Julia Falk (1999) recovers the presence and perspective of women in the founding of American linguistics, commenting on the barriers to entry they faced to become credible knowers in the emerging research culture.

[12] The American Philological Association faced similar straits in 1868: though European success signaled the possibility of a vocation in comparative philology, in America it had to negotiate an increasingly reductive definition of "science" (i.e. physical sciences), which excluded historical sciences (Alter 2005).

[13] Thomas Gieryn (1983) historicizes how, in the late 1940s, there was a move to integrate the "Big Five" social sciences—psychology, sociology, economics, anthropology, and political science—into what would later be the funding structure of the National Science Foundation (NSF), a debate which surfaced again in the 1960s, that time to prevent the social sciences from forming their own foundation. Linguistics failed to register within either debate, only incorporated into the NSF with the establishment of the NSF Linguistics Program in 1975.

scientific and public eye. My dissertation traces the itinerant path it took to obtain these markers of status.

I observe, too, how the production of scientific knowledge belongs to other contexts than the institutional and evokes more than the choices of maverick young men like Sapir. This recognition takes me out of the comfortable grooves of disciplinary history to consider how linguistic practices also fit into the troubled and troubling trajectories of the salvage paradigm. In the early half of the twentieth century, the zeitgeist of salvage guided the collection practices of much linguistic and anthropological work in America: researchers wanted to accumulate records (material, textual, archaeological) from Indigenous cultures that they deemed in peril of disappearing. James Clifford (1989) identifies the paradox at the heart of this motivation: it linked ethnic cultures symbolically to the past, valuing their authenticity as a product of distance from the "modern" and hence consigning these ways of life to loss. Despite the egalitarian leanings of these scholars, the institutional histories of anthropology and linguistics were nonetheless intertwined in the knowledge-gathering projects of settler-colonial governance. These forces were likewise eager to classify Indigenous peoples (racially and geographically) and collect their knowledges (cultural and linguistic), all while functioning to exclude, displace, or eradicate them. Salvage thus represents the greater context of failure that my dissertation seeks to address. The history of linguistics in this period formed a story of extraction on two fronts: linguists extricated themselves from other academic fields at the cost of extracting resources from Indigenous communities in the field to furnish their archives, contributing little to them in return.

❖ Theoretical Context: Failure.

In her (2010) essay on "Historiography's Contribution to Theoretical Linguistics," Julie Andresen argues that the historian is "uniquely positioned" to "leverage our understanding of the discipline's past in order to open a path to the discipline's future" (444). In contrast to E. F. K. Koerner (1999), who believes that historiography in linguistics should act as an "over-arching and unifying agent" (6), Andresen calls for a kind of performative history, where amidst descriptions of the past we might also stage encounters that transform the present. By engaging what the discipline has forgotten, we may alter linguists' orientation toward their methods of analysis and objects of study and encourage them to recognize the "partial perspective" of their work (Haraway 1988): to acknowledge that knowledge is the result of collective practice and thus represents the sedimented values of a research community (broadly conceived), some of which are known to the actors and some not. I do not perceive this partiality as an impediment to the study of language—as a circumstance to be remedied by unity—but rather, following the reflections of Julia Kristeva (1989), I understand the remediation of language to be a necessary component to its study, scientific or otherwise. It is my hope that by looking backward to its historical contingency that we might also find a way of moving forward—together, but not as one.

To this end, my dissertation foregrounds the under-arching and disunifying agent that lurks in the periphery of any performative act or partial perspective: failure. Failures, false starts, misfortunes, mistranslations, lapses in memory—these events (or non-events) characterize scientific projects and practices as much as their successes, if not more so. Failure, as a site of inquiry and analytic category, has long been operationalized within STS. It appears in the genre of the controversy study, where practitioners of the sociology of scientific knowledge (SSK) invoked a principle of symmetry to explain why competing scientific theories succeeded or failed (Bloor 1976/1991). Failure of human agency was central to the later uptake of symmetrical thinking in actor-network-theory (ANT), which criticized SSK for its reduction of science to human-governed rules and extended the symmetry principle to non-human actors (Callon and Latour 1992). Failure also has more allusive functions. It reveals the unreliability, uncertainty, or bias of technoscientific formations (Jones-Imhotep 2017, Martin 1991, Murphy 2006). It implicates the sciences through their role in the construction of social, material, or epistemological normativity and the exclusions therefrom (Anderson 2006, Canguilhem 1989, Fausto-Sterling 2000, Goffman 1963). Failure gets mobilized in the production of ignorance in experimental and cultural settings (Pettit 2013, Proctor & Schiebinger 2008). It also serves as grounds for the contestation or reconfiguration of scientific and technical expertise (Epstein 1996, Serlin 2004), or as a platform to amplify marginalized standpoints (Harding 2008). As Jack Halberstam (2011) writes in *The Queer Art of Failure*:

> The social worlds we inhabit, as so many thinkers have reminded us, are not inevitable, they were not always bound to turn out this way, and what's more, in the process of producing this reality, many other realities, fields of knowledge, and ways of being have been discarded and, to use Foucault's (2003) term, "disqualified." (147)

Failure, here, figures not as a recalcitrance toward success but its condition of possibility, the context of its undoing, and the space for its rethinking. Failure focuses attention on those for whom success forecloses, forgets, or otherwise does not fit. I therefore portray the dynamics of scientific failure and success not through the linear function of progress, as often they are presented, but as an open field of scrutiny and play.

The following chapters peruse the catalogue of failures that interleaved the growth of American linguistics, but there is one failure in particular worth addressing now. My attention to the development of an independent science of language in the twentieth century would be remiss without comment on the broader relationship of language and knowledge, as configured through and against this burgeoning scientific authority. As John Searle has put it, "Twentieth-century philosophy has been obsessed with language and meaning" (1995: 168). The linkage of language-knowledge has carried over into anthropology, history, literary criticism, psychoanalysis, and other social sciences and humanities, where to varying degrees language has been understood as the vehicle of human complexity. The "linguistic turn" has influenced STS, as well:[14] Kuhn (1962) cites linguistic relativity as an inspiration for his concept of scientific paradigms; linguistic rhetoric features strongly in Shapin and Schaffer's (1989) "literary technologies" and Latour's (1987) *Science in Action*; metaphor organizes work by Emily Martin (1991) and Evelyn Fox Keller (2002); deconstructive discourse analysis informs Donna Haraway (1989), Paul Edwards (1996), and Lily Kay (2000). This is to name only a few. In *Language Alone*, Geoffrey Harpham (2002) observes that language forms both the centre and supplement at once for humanist and antihumanist discourses: "*Language,*" he intones, "says it all" (7; emphasis in original). However, more recently, emphasis has shifted away

[14] Geoffrey Harpham (2002) identifies five such "turns" in the Western philosophical tradition: the turn to logical or ideal language; to ordinary or non-ideal language; to concepts refined in professional linguistics; to a postmodern notion of language as the essence of thought; and to an antihumanist dissolution of the subject within the primacy of language.

from language toward other complexities: material cultures, microbial life, political ecologies, big data. Many of the above scholars now discuss (not exclusively!) crittercams (Haraway 2008), "parliaments of things" (Latour 1993), climate data (Edwards 2010), and so on. If Wittgenstein (1953/2009) once conceived of language as an "ancient city," as "a maze of little streets and squares, of old and new houses, and of houses with additions from various periods; and this surrounded by a multitude of new boroughs with straight regular streets and uniform houses" (*Investigations*, no. 18), then perhaps, in the current academic-industrial complex, has this city filed for bankruptcy? Are its suburbs no longer in "good pleasure" (*Julius Caesar*, 2.1.295)? Has language—the nearly perfect tool of humanism and its critique—failed to live up to these grand expectations?

Provoked by this context, I take a moment to glance over the shoulder and consider how language might be otherwise conceived under the auspices of the "material turn." What the critique of the linguistic turn misses is the focus on the materiality of language in deconstruction: from the outset, Jacques Derrida (1976) differentiates speech from the technology of writing, a distinction between logocentric abstraction and the palimpsestic materiality of texts. Meanwhile, arbiters of the new materialism have in part solidified their collective identity by delimiting language or defining themselves against it. For instance, David Serlin (2004) contrasts his study of embodied technologies with "mere" metaphor (26). Michelle Murphy (2006) draws attention away from the citationality of chemical exposures ("Repetitions accumulated" [2]) to their materialization within office buildings. Annemarie Mol's (2002) argument for multiple ontologies reorients language (or "talk") such that it occupies a purely referential function (objects become clustered under the "same name"). Or, as Lorraine Daston (2004) announces, "Without things, we would stop talking" (9).[15] These texts reduce the interplay of language and materiality to questions of reference, of naming practices. They understand language as (always) already linked to human subjectivity, to a focus on interiority, or to an inquiry into a self-referential system (i.e. social constructivism). These utterances operate parallel to linguistics in constituting language as "uniquely" human, alike recanting the complexities of its materiality.[16] However, language embodies complex associations and histories, taking it in directions not intended by the user, and not determined by its referent in the world. Language is not alone, as Harpham's book title suggests, but an assemblage of human and nonhuman actors: language enrolls speakers, linguists, phonographs, typewriters, and databases; its vibrations are felt over wax, ink, paper, and air. At its most abstruse, then, my dissertation plays a game of cat's cradle amid the political and philosophical entanglements (the knots and *not*s) of language and matter, exploring and exposing how its divisions have become reified—even within STS. I seek to problematize the ontological status attributed to "Language" by examining the scientific practices that materialize and domesticate it. This line of inquiry is especially germane in the translation of linguistic methods and models to Indigenous languages, where the interaction of systems of knowledge *with*—rather than *as*—systems of language complicate the coherence of those frames.

[15] An exaggeration, to be sure, but perhaps a paradigmatic one.
[16] Claire Colebrook (2014) comments on the false dichotomy she perceives between deconstruction and new materialism. She recognizes the common strategy of new materialists to align deconstruction with the textual, but reminds us that the text, too, is a thing, in addition to marking things. Indeed, deconstruction begins with the "radically material concept of the trace" (135), and she speculates that its tools of analysis worked out on texts can be applied to those other things, as well. In particular, Colebrook highlights the ethical responsibility of deconstruction to declare that "any thing, matter or real is always given *as real to us*, with the 'us' also bound up with the processes of givenness that can never be mastered" (140; emphasis in original). In my work, I assess how the "givenness" of language applies to the politics of its "takenness" from Indigenous communities.

❖ Chapter Structure.

The choice of three long chapters reflects the major archives that I visited (and revisited) throughout the writing process whose scope formed the enabling constraints of my work: the Edward Sapir Papers at the Canadian Museum of History; the Benjamin Lee Whorf Papers at Yale University; and the Franz Boas, Mary Haas, and Charles Voegelin Papers at the American Philosophical Society. The different chapter structures represent and diffract the archival impetus of these knowledge sites; these choices reply (and re-ply), indirectly, to Ann Stoler's (2009) prompt to consider "archiving-as-process" and interrogate not only the archives' contents but the principles and practices that inform their structures (20). Stoler suggests that researchers should take an "ethnographic" rather than an "extractive" approach to archival work (47). In response, each of my chapters is anchored by a conceptual device that brings to the fore these archival forms. I employ different technologies of punctuation { **. . . — [ ]** } to structure my encounter with the archives, alluding to elements of their organization through form rather than reference. The ellipsis is used to convey communication across points in a network that composed much of Sapir's archives for the Geological Survey of Canada (Chapter 1). The dash is used to express the importance of identity formation that was integral to the personal and professional correspondence of Benjamin Lee Whorf (Chapter 2). The bracket is used to indicate the detailed attention to linguistic records in the archives of the First Yale School (Chapter 3). These marks also index the different stages of disciplinary development in linguistics on which my chapters concentrate: the scattered spaces of the ellipsis that anticipate disciplinary cohesion, the dash that forms a line between amateur and specialist, and the timely bracketing of the phoneme as an object of study exclusively linguistic.

Chapter 1: **"Curating Intimate Distance: Edward Sapir, Elliptical Space, and the Geological Survey of Canada."** My study begins with Sapir's first institutional appointment as Head of the Anthropological Division of the Geological Survey of Canada (1910–1925). At this juncture, linguistics had yet to gain its footing: it was a science in service, both to the Dominion Government and as a resource to the better-established discipline of anthropology. Despite Sapir's commitment to an egalitarian, anti-racist science, his Division was nonetheless in service also of an imperialist project that sought to displace, contain, or elide the First Nations, Inuit, and Métis peoples upon whose lands and knowledges the Survey depended. It was out of these conditions of estrangement and belonging, I argue, that Sapir cultivated a relation of *intimate distance* for himself and his disciples. Intimate distance—to draw near but keep apart—names an institutionalized affect that animated the network of collection practices through which Sapir sought to reach out across the contact zone and apprehend linguistic data from the intuitions of Indigenous informants. This structure of feeling characterized his inchoate yearning for disciplinarity within linguistics, allayed the persistent setbacks and unexpected encounters of fieldwork, and sustained the fantasy of completion characteristic of "salvage" ethnology.

Chapter 2: "'A Dash of Genius': Benjamin Lee Whorf, Scientific Belonging, and the Promise of Linguistics." Sapir is often remembered as the former half of the "Sapir-Whorf hypothesis," although there was never a collaboration between Benjamin Lee Whorf and his mentor. This chapter explodes the hyphenate and tracks Mr. Whorf—a self-professed hobbyist and fire inspector by trade—as he emerged both as an exemplar of and exception to a burgeoning disciplinarity within American linguistics. I employ the seeming paradox of the "amateur specialist" (Whorf was called so in in his *Times* obituary) to recapture a moment when linguistics had not yet established its professional identity and remained open to the contribution of amateurs. The chapter explores how the status of amateur afforded Whorf an unconventional perspective on the training structures, methodologies, and expected trajectories of a university researcher—foremost realized in his linguistic relativity hypothesis. Whorf, I show, sought belonging not only in the domain of the professional scientist, but as well in the role of scientific popularizer, envisioning for linguistics a moral imperative to inform and guide the public.

Chapter 3: "Bracketing Time: The First Yale School, a Study of Method." My final chapter expands focus to the group of linguists who studied under Sapir at Yale University, in particular Mary Haas, Morris Swadesh, and Charles Voegelin. From its inception upon Sapir's arrival at Yale to his death almost a decade later (1931–1939), this school served as a nexus for research on Indigenous languages and an emerging training centre for the scientific study of language—much of which unraveled soon after its leader's demise. This chapter puts into contrast the momentary collusions of institutional support with the group's interest in preserving timeless linguistic data: Sapir's students were eager to resume his work of documenting living languages and recovering dead ones. Here, I examine how these linguists trained their bodies and developed specialized writing systems to apprehend the languages of their Indigenous informants. These techniques and technologies for isolating and storing languages tie together colonial legacies and humanist ambitions; the continued vitality of their records also suggests an opportunity to re-think settler frames of reference within the archives of linguistics.

There are certain litanies we rehearse about failure until they appear to us as common sense: failures represent the stepping stones on the way to success; failures are what we discard when, finally, success is found. I argue to the contrary that failure is ever present: failure lingers under the surface of success, heralding its potential re-workings through new meanings, uses, or contexts. To this end, my dissertation considers the failures of salvage linguistics on three levels: the empirical failure that was the impossible task of assembling complete records on threatened languages, the institutional failure of this school of linguistics to ensure lasting support for their work, and the ethical failure of linguists toward the peoples they encountered in the field. In doing so, I reverse the polarity of Sapir's dictum that linguistics was a nearly perfect science. Where, for Sapir, the usage of *nearly perfect* indicates a falling short of, or a promise closely but not quite fulfilled, through the optics of failure I understand it as a source of different possibility: a call to elongate the space between science and success and, therein, re-orient the history of linguistics. The domain of the nearly perfect represents the *kairos* of my project: the opportunity to move away from the ideal of perfection and the linear narratives of scientific progress that accompany it. These narratives have their origin in modernist fantasies that relegate Indigenous peoples to the margins at best. Along these lines, the goals of my writing are to reflect on the legacy of salvage linguistics and to help foster a non-extractive model that recognizes language's materiality and hence the collective responsibility of linguistics toward Indigenous communities from whom they once took and now should be compelled to give back.

~ 1 ~

"... we will find ourselves drifting into the position of genteel spies for the Department of Indian Affairs. We cannot afford to be misunderstood by any Indians in Canada."
- Edward Sapir (to Marius Barbeau, 16 July 1920, *GSC*)

"The proximity of ethnography leads to a recognition of distance: this is a knowledge which withdraws from that which one has laboured to know."
- Sara Ahmed, *Strange Encounters*, 2000

Curating Intimate Distance:

Edward Sapir, Elliptical Space, and the Geological Survey of Canada

Introduction: On Intimate Distance.

Salvage linguistics often belonged to wider-ranging anthropological collection projects, especially in the early years of the twentieth century before linguistics gained its footing as an independent science in North America. One such example, the focus of this chapter, was Edward Sapir's position as Chief Ethnologist of the Anthropological Division for the Geological Survey of Canada (1910–1925). Sapir is remembered as the first linguist to employ native speakers' intuitions systematically to judge the adequacy of a grammar, a style of elicitation still in use today, and he developed this approach during his years in Ottawa while managing an anthropological research team and its associated museum. Despite Sapir's commitment to an egalitarian, anti-racist science, this participatory methodology was nonetheless borne out of conditions of extraction. His Division was a branch of a settler-colonial venture that participated in the segregation, coercion, and exploitation of the First Nations, Inuit, and Métis peoples. In *We Have Never Been Modern*, Bruno Latour (1993) praises the "ethnologist's detachment" (35) and extols their ability to construct a single narrative that incorporates nature and culture along the same plane. In this formulation, Latour flattens the dynamics of knowledge-power to a measure of scale and overlooks the historical role of anthropology in producing and perpetuating structural imbalances. This chapter demonstrates through linguistics, the disciplinary *attaché* of ethnology at the time, the techniques of distantiation that linguists and ethnologists alike employed in service of the state. In lieu of detachment, I examine how the "intimately distant" routines of Sapir and his scientific network contributed to the development of linguistic methodologies and their implementation in the field.

Sapir himself was "no museum man," decided Alfred Kroeber (to Putnam, 19 February 1908, *UCB*), commenting on his friend's early departure from his first official position at the University of California (1907–1908). There, Sapir had conducted fieldwork on Yana, Indigenous peoples of northern California, as part of a state survey based in the university. He left to defend his dissertation at Columbia University, earning a doctorate in Anthropology in 1908. Like Kroeber, Sapir trained under Franz Boas in the Americanist anthropological tradition, mastering its four subfields: cultural ethnology, physical anthropology, linguistics, and archaeology. However, not all areas of anthropology found equal distribution within the museum space, he would learn. Linguistics was included predominately as an aid, or "handmaiden" (Darnell 2009: 44), to ethnography. Textual records and rudimentary knowledge of language were integral for ethnographic research, but rarely understood as an end to itself. This subordination of linguistics was a sore subject for Sapir. Even at his second appointment at the University of Pennsylvania and its attached museum (1908–1910), where "American Indian linguistics" was on the curriculum, he was "immediately confronted with the museum attitude that collections were paramount" (Darnell 2009: 30), linguistic research secondary. To put it simply: the description of languages did not belong to the display culture of museum exhibits. Nonetheless, Sapir's commitment to linguistics and knowledge of its methods were unequaled among Boasians. In Sapir, the anthropological tradition met with the tenets of Germanic philology; and where philology usually concerned literary languages, Sapir's training under Boas inculcated in him an unceasing curiosity for the unwritten languages of North America. Even amidst other ethnographic work, he usually found time to puzzle out the details of their linguistic structure. Sapir was a "born linguist" (Boas to Kroeber, 24 May 1906, *UCB*), but what was a linguist born into before there was a professional science in America by that name? How did linguists extend themselves through spaces, networks, and lands that were not their own?

Sapir was no museum man, yet one he would remain for the next fifteen years. The American accepted an appointment as Chief Ethnologist of the newly created Anthropological Division of the Geological Survey of Canada from 1910 to 1925.[17] Director Reginald Brock requested Sapir at Boas's recommendation, for a position that, to the young Ph.D., must have seemed ideal. The scope of the Division was "pretty much what the man himself makes of it." Alongside museum work, its goal was to conduct "a thorough and scientific investigation of the native races of Canada, their distribution, languages, cultures, etc., etc., and to collect and preserve records of the same." To accomplish this task, the Survey both encouraged and provided generous support for fieldwork: "All expenses while

[17] Although Sapir worked in Canada for fifteen year, this chapter is more a history of American anthropology, for which the Survey was but a post north of the border. Sapir trained no students during his time there and consequently failed to leave a legacy in the Canadian-identified anthropology that developed through university programs later in the century (Harrison & Darnell 2006).

in the field and everything necessary for the proper carrying on of the work will be provided by the Department." As head of the Anthropological Division, Sapir was to help "organize, stimulate, encourage, and direct individual effort throughout the country" (Brock to Sapir, 3 June 1910, *GSC*). The Division was instituted with strong support from the British Association for the Advancement of Science (BAAS), the Royal Society of Canada, and "Canadians generally," Brock opined. A special vote passed in Parliament to approve Sapir's position as Chief Ethnologist (with a starting salary $2100 per annum), classifying full employees of the Division as civil servants. Linguistics was once again in service, but now to the Dominion Government in addition to the better-established discipline of anthropology.

For the first time in his academic career, Sapir had a secure position, and he was eager to make a name for himself in national and international contexts. Anthropology in Canada, up to that point a sparse collection of missionaries and amateurs, seemed a fertile ground for collecting data. Significant contributions could be made for a small cost, with smaller or one-man teams (Zaslow 1975: 289); long-range scientific objectives thus joined with short-term economic ones (297). However, after four years of intense productivity in the field and active collection work for the museum, the Division's funding was cut short by wartime budgets. Anthropology, which already represented less than 1% of the overall funding structure for the Geological Survey, was not considered strategically important during the war; compared to the 9% overall cuts to the Survey by 1918, the Division faced a 70% cutback (Vodden & Dyck 2006: 30), from which it would never recover. During Sapir's time there, the Division also endured the loss of personnel due to resignation, reassignment, or death; it was afflicted by a persistent lack of space to hold or display the collections that its staff had already assembled. Sapir's appointment was not the boon it had initially appeared.

But this story is not only about Sapir, his research team, and their collections. The Division, under the auspices of the Geological Survey of Canada, was not a purely scientific endeavour (if such a thing exists), nor did its successes and failures concern only its government-employed staff. Failure also served the settler government's framing of the "Indian Problem" (Dyck 1991): a failure for Indigenous groups to assimilate and abandon their "failing" cultural practices, for which government agents acted as coercive mediators.[18] The Survey, formed in 1842 under the guidance of William Edmund Logan to ascertain extricable resources, operated inextricably within these frames. In addition to being a training centre and base of support for scientists in Canada, it served the purpose of mapping resources and guiding prospectors, buoyed by a belief in industrial expansion and a large market for raw materials in Britain. The Geological Survey Act renewed the initiative in 1856, laying the foundation for the Survey's display hall (today, the Canadian Museum of History). Inspired by the Great Exhibition of 1851, the display hall allowed Canada to contribute to such international exhibitions (Vodden & Dyck 2006: 10); collection for the hall began in 1862, long before the Anthropological Division was established, with the aim of displaying Canada's achievements in every area of civilization to its peoples and the world. After 1867, the Survey became an integral organization for

[18] Noel Dyck (1991) accounts for the material, ideological, and legal underpinnings of the "Indian Problem": a belief—but really, as Dyck points out, an assertion—that the differences between Native and Euro-Canadian cultures must be resolved. The Indian Lands Act of 1860 cemented the administrative transfer from an imperial to a colonial government, from a trade relationship to one of settlement, and from a cooperative model to coercive tutelage (51). In this arrangement, "tutors" were tasked with saving Native peoples from stigma, vulnerability, and poverty, which were often the products of government regulation. The Indian Act of 1876 consolidated this policy of assimilation and extended powers to federal agents for its enforcement (52).

performing reconnaissance on the land of a newly confederated country; its mandate expanded to include exploration and resource appraisal, becoming part of the Department of the Interior in 1877. In 1907, control over the Survey shifted hands to the Department of Mines, effecting a change in its public image and research program (Zaslow 1975: 285). This shift coincided with increased attention to Indigenous civilizations in order to demonstrate the "early history of Canada" (279). In 1910, the Survey moved to the Victoria Memorial Museum, cementing the union of prospecting and representation: the Museum was projected to display the "infinite diversity" of Canada, which now included "human categories" alongside flora and fauna (278). The Survey therefore stood at the conjuncture of science, economics, environment, and government; it embodied a commitment to the past, the urgency of the present, and prospects for the future of a settler nation.

At the same time that the Survey worked to register the resources of a purportedly unified nation and the Museum to consolidate its cultural identity, the two initiatives functioned as branches of a colonialist project that sought to displace, contain, or elide the First Nations, Inuit, and Métis upon whose lands and knowledges that nation was based. In *The Transit of Empire*, Jodi Byrd (2011) argues that discourses of inclusion into multicultural liberal democracy erase Indigenous communities' prior claims to sovereignty, installing them as racial minorities already incorporated into the "implicit symbolic order" of the state (125), rather than enduring challenges to its borders. With its purported aim to integrate the First Peoples into Canadian history, the Museum embodied and emplaced the construction of the "Indian" as internal to the empire and evidence of its cultural diversity. Sapir may have organized the Anthropological Division along the lines of the Americanist tradition, with its commitment to collaboration, facilitation, and respect for the "Native point of view" (Darnell 2001: 12–20), responsive to all the tensions and ambiguity that entailed. However, as an extension of the Survey, the Division was nonetheless complicit in this colonial formation. Famously, Sapir refused for his team to act as "genteel spies for the Department of Indian Affairs," yet as scientists in service to the state, they had little recourse but to touch on the violences of New World colonization. Making the "strange" familiar is a common strategy of imperial projects, Sara Ahmed (2000) observes, and ethnography represents the "professionalization of strangerness," turning an "ontological lack" into an "epistemic privilege" (60). Ann Stoler (2009) puts it another way: "Colonial commissions reorganized knowledge, devising new ways of knowing while setting aside others" (29). The Anthropological Division, as a knowledge-taking branch of the Survey, was unavoidably entangled in the project of governmentality, despite the ire or disappointment of some of its staff or their efforts to the contrary.

However, the relationship of knower and who is known is not a one-sided and hierarchical arrangement. Postcolonial studies have demonstrated how the identities of the colonizer and colonized are bound together in intricate and unpredictable ways. Under this analysis, empire is not a "monolithic system of domination" projecting authority from its centres of power, but instead understood as a "network of power relations" diffused across a multitude of intimate encounters (Kaplan 2002: 14). Stoler (2006a) understands intimacy as another realm of colonial power, in addition to the rationalizing projects of the state: "Colonial authority depended on shaping appropriate and reasoned affect (where one's sympathies should lie), severing some intimate bonds and establishing others" (2), both in its officials and its colonized. Such "domains of the intimate" frequently included social policy, urban planning, and medical protocol (3). But as Stoler and her fellow essayists in *Haunted by Empire* show, there is no consensus on what constituted the intimate: the volume spans the intimacies implicated in juridical trials, labour histories, evangelical missions, intelligence testing, and more. Together, the authors explore "the intimate through and *beyond* the domestic and through and beyond the management of sex" (4; emphasis in original). These "affective histories" reveal a bio-politics of vulnerability (14), in which the intimacies of some bodies become exposed, regulated, drawn in, or cast aside. As I demonstrate below (in §5 especially), intimacy also ruptures modes of governmentality suggested in the discourses of bio-politics, revealing a two-way traffic between Indigenous groups and government agents, where the former's speech, silence, agency, and desire affected the course of these encounters.

Intimacy thus appears in many forms and guises and with varied functions. My chapter contributes to this discussion by naming and describing an affective commitment emergent within the Anthropological Division during the fifteen years of Sapir's tenure. The Geological Survey was such a rationalizing state project as Stoler describes, serving to remap and reclassify a land and its peoples from a distance; through the actions and sensibilities of the Division, especially, it was also an intimate project. The anthropologists under the Survey's employ had to strike a balance between acting as agents of a settler colonial government and Boasian ethnologists; they were lodged equally in the proximities of fieldwork and the remoteness of bureaucracy. Sapir, in particular, had to manage the representational space of the Victoria Memorial Museum against the actual space of Canada, coordinating a skeletal research team with a network of informants and other government actors. It was out of these conditions of estrangement and belonging, I argue, that Sapir and his colleagues cultivated a relation of *intimate distance* to help navigate the uncertainty of these spaces: intimate distance—to draw simultaneously near and apart—names an institutional affect that animated their network of collection practices. This structure of feeling deflected concerns over woefully diminishing budgets, allayed the persistent setbacks and unexpected encounters of fieldwork, and sustained the fantasy of completion characteristic of "salvage" ethnology.[19] It both produced and deferred vulnerability, embodying how the Division's humanistic sensibilities were (s)trained by the experiences and ambivalent demands of service. It also characterized how Indigenous communities negotiated the intimacies of their bodies, languages, cultures, and homes in relation to these strange and distant visitors.

For Sapir himself, intimate distance furthermore manifested in his being a linguist masquerading as a "museum man": it was a reaction to a stifling museum culture and it expressed an inchoate yearning for disciplinarity within linguistics. Language—itself an intimately distant phenomenon, at once internal and social, situated and personal—figured only incidentally into museum work. His team, moreover, was mostly untrained to ascertain linguistic data and his informants unprepared to give it. Yet from out of scenes of failure—including consistent obstructions, declining budgets, and fluctuating staff—Sapir ultimately derived some of his greatest contributions to the science of language, such as his comparative grammar of Na-Dene, his treatise on "Time Perspective," his six-unit classification of North American linguistic stocks, and his book *Language*. Failure for

[19] Raymond Williams (1977) terms "structures of feeling" to characterize social forms that "become social consciousness only when they are lived, actively, in real relationships, and moreover in relationships which are more than systematic exchanges between fixed units" (130). The expression differentiates "practical consciousness" from "official consciousness," capturing the tension between lived experience and received interpretation. These structures are often found "at the very edge of semantic availability" and seen only later more materially (134). In my usage, it refers to a feeling that permeated an institution (the Anthropological Division) but derived from interactions (between anthropologists and Indigenous informants) rather than institutionalized modes of intimacy.

linguistics to fit into the spatial or fiscal constraints of museum work, initially a marker of its uncertain epistemic grounds, later proved to be the condition of possibility for its success. Sapir's disenchantment with the Survey and growing differences with Boas over the place of linguistic research within anthropology marked this as a transitional period for the discipline, culminating in the establishment of the Linguistic Society of America the same year as Sapir's departure from Ottawa. Linguistics was a science born of the failures I will chronicle below.

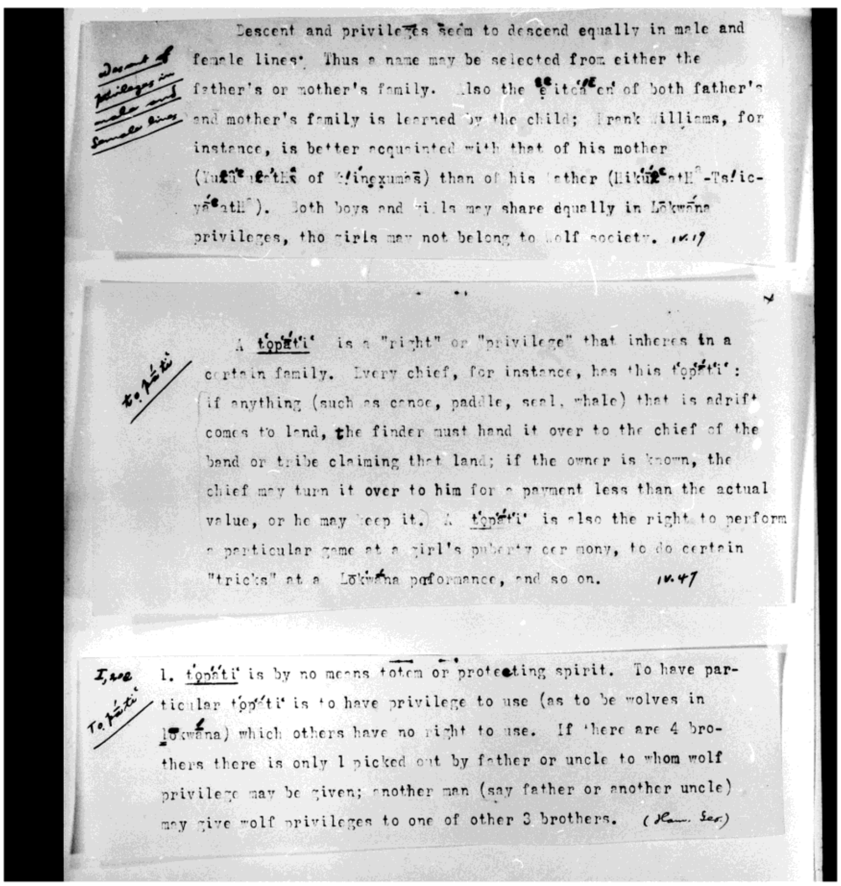
. . .

0. On Ellipsis.

This chapter maps scenes from the extension of a network that connected the vast expanse of Canadian territories to the Victoria Memorial Museum, where Sapir acted as chief administrative node in addition to being chief anthropologist, pursuing research between the Museum and territories. To this end, I trace the representational practices of his field journals, recirculating their structure as a conceptual anchor to organize my work. An excerpt of which, pictured below, comes from Sapir's notes on the Nootka, Indigenous peoples in British Columbia, whom he studied on behalf of the Survey in 1911 and again in 1913. These notes comprise enumerated sections (39 in total, in the Nootka journals), describing various dimensions of their culture ("Notes on Technology"; "Notes on Body Decoration"; "Notes on Linguistics"; etc.).[20] This example features a random selection of lexicography and demonstrates Sapir's process of constructing knowledge of their language

[20] Notes recollected after Sapir's death on behest of his son (Dell Hymes introduction to the materials). The Nootka material contains: "Ca. 1600 leaves, 19 notebooks of ca. 100p each, 5 notebooks of ca. 200p. each, and ca. 750 slips of personal and place names."

and culture. First-hand and retold experiences translated into hand-written notes were later retyped by Sapir or his stenographer; these typed notes were then separated by ellipses, cut up, reorganized, and reassembled; finally, they were written over again with Sapir's notes, corrections, directions, and diacritics. His journals form a bricolage, a palimpsest, and a hypertext. While the hypertext is a genre often linked to the "heterogeneous and fissured space" of computer-age informational networks (Hayles 1999: 28), here it corresponds to the elliptical space of Sapir's journals, a microcosm for the representational activities of the Victoria Memorial Museum and a trace of its researchers' efforts to absorb distant cultures into their archives.


Descent of privileges in male and female lines

Descent and privileges seem to descend equally in male and female lines. Thus a name may be selected from either the father's or mother's family. Also the [illegible] of both father's and mother's family is learned by the child; Frank Williams, for instance, is better acquainted with that of his mother ([illegible] of [illegible]) than of his father ([illegible]-Ts!ic-[illegible]). Both boys and girls may share equally in Lōkwāna privileges, tho girls may not belong to Wolf society. IV.19

tōpāti

A tōpāti is a "right" or "privilege" that inheres in a certain family. Every chief, for instance, has this tōpāti: if anything (such as canoe, paddle, seal, whale) that is adrift comes to land, the finder must hand it over to the chief of the band or tribe claiming that land; if the owner is known, the chief may turn it over to him for a payment less than the actual value, or he may keep it. A tōpāti is also the right to perform a particular game at a girl's puberty ceremony, to do certain "tricks" at a Lōkwāna performance, and so on. IV.47

I, we
Tōpāti

1. tōpāti is by no means totem or protecting spirit. To have particular tōpāti is to have privilege to use (as to be wolves in Lōkwāna) which others have no right to use. If there are 4 brothers there is only 1 picked out by father or uncle to whom wolf privilege may be given; another man (say father or another uncle) may give wolf privileges to one of other 3 brothers. (Ham. Sec.)

Figure 1. Fragment on Nootka Language – Sapir's Field Journal, 1913 (*GSC*)

The ellipsis typically marks a space of events within the semantic range of the knower that nonetheless goes unpronounced. The ellipsis exists to be filled in, whether implicitly by a shared situation uniting writer and reader or, in the case of Sapir's journals, actively by the collection practices of his research network. The ellipsis therein functions as context and co-text, eliding the contingency of this knowledge production and standing, for his readers (which included himself), in anticipation of its eventual completion. In *A Thousand Plateaus*, Gilles Deleuze and Félix Guattari (1987) argue that texts do not represent the world in a parallel symmetry, but are events that take place along the same plane and, as such, give way to perpendicular actions. There is no better example of this philosophy at work than in the activities of the Anthropological Division, wherein the management of representational and physical spaces met, knotted, and took flight in their extension of an information-gathering network across the heterogeneous and fissured space of early twentieth-century Canada. The Division at once represented the cultural topography of the country and contributed to its reshaping. The term *network* deserves some comment. Latour (1993) defines the network as an object of study "more supple than system, more historical than structure, more empirical than complexity" (3); within his model of analysis, differences between cultures are flattened into variations of scale and made comparable in their successful recruitment of human and nonhuman actors. However, in structuring my chapter stylistically after Sapir's journals, the solid lines of the network become instead dotted, elliptical, suggestive of the line without its assured definiteness. Elliptical space encompasses not only the marked or successful points of a network, but lingers on what is lost in between: what is absent, delayed, schematic; what is not yet reasoned out, not yet reducible to scale. The layers of remediation in Sapir's journal bring to mind Friedrich Kittler's (1990) concept of the "discourse network," which also serves as a model for my analysis. Kittler understands a discourse network as the "network of technologies and institutions that allow a given culture to select, store, and process relevant data" (369). In Kittler's analysis, media largely determines the situation of discourse. Like Deleuze and Guattari, Kittler conceives of a series of plateaus and breaks, though here corresponding to the changing medial ecology at the turn of the twentieth century. Around 1900, for the first time, "writing ceased to be synonymous with the serial storage of data" (229). Storage became a process instead of coordinating different technologies, such as the gramophone and the typewriter. For the Anthropological Division, storage was likewise not a matter of discourse alone, though writing did compose a good deal of anthropological work, and much of the colonial archive consisted of texts about language (Errington 2008: 3). The practices of the Division were material as well as discursive, embodying an analog assemblage of notes, artifacts, infrastructures, technologies, and environments, all of which interacted in tandem and at odds with each other: imperfectly, elliptically.

In Jacques Derrida's (1978a) essay on "Ellipsis," he remarks that writing signals the closing of the book and opening of the text. There is no true repetition: "Once the book is repeated, its identification with itself gathers an imperceptible difference" (295); each repetition exists in a context already shaped by a former iteration. My chapter reiterates the elliptical structure of Sapir's journals, collating a series of events in a similar manner that he abstracted Nootka society, but the repetition is not an act of identification with Sapir or an attempt to revive his insights without altering them. Like the punctuation mark, ellipsis repeats but does not bind; its points hang together in common cause but offer a space to think through what is not there, pairing presence and absence, pattern and noise. Sapir's notes are the organizing principle of this chapter but, as the ellipsis in its indeterminacy suggests, it is also a jumping-off point for different directions: a point of arrival, a point of departure, a point of return. Ellipsis, here, amounts not to the "elisions" of multicultural liberal democracy (Byrd 2011: xii), which Byrd seeks to unsettle with "cacophonous" disruptions, but represents the limits of the colonial archive. Ellipsis provides a space to consider not only the archival gaze, but what is implicit and unsaid within the archive's organization (Stoler 2009). Within elliptical space, meaning emerges from the triangulation of analyst, archive, and audience. Like Sapir's journal, my analysis can be cut up and read in multiple directions, chronologically or laterally. It is structured rhizomatically through a series of disjointed vignettes akin to plateaus, "tentacular" (Haraway 2016) rather than totalizing, yet "all the more total for being fragmented" (Deleuze & Guattari 1987: 6). The dotted lines (of articulation, of flight) emphasize the implicit and uncertain (the "…") along with the cacophonous (the "y + z + a"). My account combines narrative, analysis, and documentation into a travelogue that is neither a close nor a distant reading, but eschews depth in favour of surface area as an analytic model. The chapter follows an episodic structure, a survey befitting of the subject matter. Through the mark of the ellipsis, intimate distance thus comes to characterize my historical analysis as well. The ellipse curves around its foci in a manner that suggests the negative space of what has failed to register in the history of linguistics.

. . .

❖ Extension (into the Museum): 1910–11

1. **1910.** On Arrivals.

The Anthropological Division was founded on 1 September 1910. Edward Sapir was its only agent then; he spent the fall season in Alberni, British Columbia, studying the language and culture of two tribes of the Nootka: Tsishya'ath and Hopach'as'ath. Each member of the Geological Survey was required to submit an account of their yearly activities for inclusion in an annual summary report.[21] Sapir's (1910 Annual Summary Report, *GSC*) underscored how anthropology had been a secondary concern of the government until that point. Indirectly, the success of the Survey contributed to the impetus for his Division's creation: Canada, Sapir observed, was a country swiftly being settled, and settlement eradicated cultural materials unless they could be collected and preserved. Future Canadians, he opined, would be in danger of searching in vain for "authentic information" about Native peoples (7). The Anthropological Division had arrived to ensure this remembrance.

Sapir framed the goals of the Division in themes endogenous to Boasian ethnology, likening its institutional status with that of the Bureau of American Ethnology (BAE). In a statement published in *Science* (1911), he outlined the Division's first year of activity in an effort to publicize their research before an international scientific audience and entreat his colleagues' cooperation with this newly founded initiative. This statement emphasized how the national character of institutions such as his and the BAE belied the work at hand. Drawing "hard and fast lines" between Canada and the U.S., Alaska, and Greenland was "artificial," he insisted: the Wyandots of Oklahoma, for example, were "formerly Canadian

[21] Under the directorship of Deputy Minister McConnell, preparation of summary reports became the responsibility of the department chiefs in 1914, rather than of individual members (McConnell to Sapir, 17 October 1914, *GSC*). In 1917, the reports were again restructured, resulting in less detailed summaries.

tribes [that] moved far south well within the bounds of the United States." In such cases, "trespassing" was "logically necessary" (790). For Sapir, the same dotted lines that perforated national borders also frayed the edges of epistemic boundaries. The Boasian school, as I mentioned above, relied on differential evidence from archaeology, linguistics, physical anthropology, and cultural ethnology. Indeed, this comparative work was essential to the act of remembrance in the form of historical reconstruction: "not infrequently slim evidence for a point of reconstructed culture-history obtained from the study of one of these [units of analysis] may be strengthened and even reduced to certainty by evidence derived from a study of one of the other" (790). However, each subfield presented its own challenges within the new Canadian context. For cultural ethnology, Sapir identified key knowledge gaps across the five "cultural areas" of Canada (Eastern Woodlands, Arctic or Eskimo, Plains, Plateau-Mackenzie, and West Coast). For the other three Boasian subfields, the dearth of material was even greater, since less had been done in those areas "with regard to strict scientific method" (793). Of the extant grammatical and textual collections, Sapir was particularly critical: "a poor phonetical groundwork and a failure to grasp the traits of morphology from a purely objective standpoint vitiate the value of much of this material" (792). Ultimately, Sapir's aim was to introduce a new standard of systematicity within Canadian anthropology, premised on Boasian principles. Not only was this intention evident in his research plan, but the choice of scientific periodicals and museum exhibits as the major outlets for the Division circumvented the work of amateur ethnologists and collectors in Canada, whose own networks extended through private clubs and personal displays (Willmott 2006: 214). Sapir had arrived to establish scientific legitimacy and confer professional status upon the Division.

These arrivals signaled a shift in the history of Canadian anthropology between the eras of amateur and professional, museum-based work (Hancock 2006), coinciding with similar transformations in America. Government authority imbued the knowledge-taking practices of the Anthropological Division with scientific force that, along with greater mobility and access to resources, largely displaced the contribution of non-professionals. The placement of Indigenous peoples within this "regime of truth" followed a different route. In his final observation of the 1911 statement, Sapir rehearsed the logic of salvage:

> Now or never is the time in which to collect from the natives what is still available for study. In some cases a tribe has already practically given up, its aboriginal culture and what can be obtained is merely that which the older men still remember and care to impart. With the increasing material prosperity and industrial development of Canada[,] the demoralization or civilization of the Indians will be going on at an ever increasing rate. . . . What is lost now will never be recovered again. (793)

While Sapir understood Indigenous absorption into North American culture as an inevitability, with the anticipated "disappearance" of traditional knowledge forming an imperative for his work, as an agent of the government he was also entangled in the power structures driving those processes of assimilation. Incorporation thus figured both as threat and result, with no imaginable alternative.

James Clifford (1989) identifies this paradox at the core of the "salvage paradigm": the promise to keep cultural knowledge "alive," even when the knowers themselves were denied entrance to modern life and their ways of living consigned to loss. For ethnologists working within that paradigm, remoteness was a marker of authenticity: quite often, ideal informants were the elderly, those of low social class, or those otherwise estimated to have the least contact with Europeans (Nurse 2006: 56). But distance was a quality always already in peril: it could not be accessed without drawing its holders into intimate contact with the machinery of power. The establishment of the Anthropological Division signified an arrival, then, but it was as much a series of repetitions: of Boasian anthropology, of the institutional model of the BAE, of the domination of the Dominion Government, and of a salvage paradigm whose scientific authority was premised upon the anticipated failure of Indigenous civilizations.

. . .

2. **1911.** On the Museum.

Le Musée nationale. The National Museum of Canada. The Canadian Museum of Man. Le Musée nationale de l'homme. The Canadian Museum of Civilization. Le Musée canadien des civilisations. The Canadian Museum of History. Le Musée canadien de l'histoire. These shifting signifiers speak to a multitude of contested and overlapping trajectories. They also proclaim the Canadian government's attachment to the museum space as a site to narrate its history and orient visitors to its First Peoples for over a century. From the National Museum's inception in 1910, when the display hall for the Geological Survey moved from Montréal to Ottawa, to the present day, where its symbolic successor, the Museum of History, continues its legacy and also houses the archival material evoked in this chapter, the museum has served at once as historical site, representational plane, and archive. Astride this elliptical chain of signification, there is also a physical place, a point of anchorage, a set of coordinates—though even the location has not remained constant. Reference is unstable at both ends. For the duration of Sapir's tenure at the Anthropological

Division, that place was the Victoria Memorial Museum Building / l'Édifice commémoratif Victoria. Construction began in 1905; it was completed on 1 January 1911.

Figure 2. Exterior view of Victoria Memorial Museum, Parks Canada Agency / Agence Parcs Canada, 1987.

The scientific activity of the Division was imbued with force not only through government fiat but through this emplacement. In Canada, the founding of the National Museum signaled a newfound commitment to public science akin to the American model, which Sapir and his cohort sought to emulate. American anthropology of this era often contributed to spaces such as public museums or World's Fairs to ensure continued patronage and to reach diverse audiences. Participation in these exhibitions helped anthropologists to gain recognition and establish their professional identity (Parezo & Fowler 2007). Museums themselves served as sites to train students for fieldwork and instruct the future citizenry (Conklin 2013). Museum displays in particular were central both to the propagation and dispute of racial essentialism in America (Teslow 2014). Museums were moreover depositories where anthropologists could collect, preserve, and display objects of curiosity and, increasingly since the mid-nineteenth century, some even housed peoples who performed as living exhibits (Qureshi 2011). Museums were hence places to titillate and entertain, to invigorate Americans' belief in science and progress. They were centres of knowledge production and dissemination; they were nodes within broader discourses and disagreements on the subjects of race, culture, evolution, and difference. They were sites of colonial contact and regulation; they were sources of compassion and ambivalence. In short, as Susan Leigh Star and James Griesemer (1989) define, the museum exemplified a complex "institutional ecology": a space for negotiating multiple and often competing viewpoints and activities among an array of human and nonhuman actors.

Of primary importance for the official viewpoint was the establishment of "an interesting and instructive display in the Ethnological Hall" of the Victoria Memorial Museum, meant to "ensure public support" for the Survey (Brock to Sapir, 3 June 1910, *GSC*). The move to the Museum did not mean more space to operate, but more room for exhibition and safer storage (Zaslow 1975: 266). It became the central depository for the purchase and transport of new specimens. In 1911, the Division staff began unpacking and carefully sorting ethnological material according to cultural areas and tribes (1911 Annual Summary Report, *GSC)*, materials previously collected under the leadership of George Dawson. To allow for an ease of cataloguing, Sapir implemented a numbering system that corresponded to the five cultural areas he devised. The aim of the displays, in his mind, was a comprehensive depiction of Indigenous life: "The ideal tribal museum exhibit is not necessarily the one containing a large number of particularly beautiful specimens, but one in which a place is found for every aspect of native culture" (Sapir 1912: 64). In this way, Sapir and the Survey alike envisioned the museum as a "truth-spot." Thomas Gieryn (2002) argues that scientists imbue certain places as sources of legitimate knowledge, usually at the expense of the truthfulness of other locations. He understands this investment in truth-spots as a process that translates "place-saturated contingent claims," intimately bound to the local, into "place-less transcendent truths" (113). To paraphrase Latour: for Canada to become knowable, it must become a museum. The Victoria Museum was the planned centre for this (re)arrangement of truth claims about the country and its peoples, with the material-semiotic scene of the exhibition halls the focus of its public engagement.

The function of truth-spots takes on a different valence within the scope of the historical sciences, however. Not all science shares the "presumption of equivalence" that Gieryn describes, where credibility depends upon the standardization of research space and where mention of place (which he defines, in contrast to *space*, as a "unique spot in the universe") arouses doubt over the science's veracity (127). Indeed, within Boasian anthropology, place was a crucial organizational component and marker of authenticity. In Sapir's initial plan of research, for instance, he valued the remote Athabascan tribes of the Mackenzie Valley for their supposed marginality: "A thorough investigation of these tribes (Chippewyan, Slaves, Yellow Knives, Dog Ribs, Hare and Loucheux) is probably the greatest single need of ethnological research in Canada. Among these tribes, if anywhere in the dominion, we may expect to find the simplest and most fundamental forms of aboriginal American culture, granted that there is such a thing" (Sapir 1911: 792). Distance from urban centres was an indicator of an intimacy with the past that more proximal tribes would have lost from contact. Within the salvage configuration, traditional sites of knowledge were understood as places of truth that must simultaneously be disinvested with their capacity for truth in order to justify the Division's collection project. The truth-spot of the Victoria Museum depended less on place-less-ness, then, as it did on a re-placing: of amateurs, on the one hand, and of Indigenous autonomy over cultural knowledge on the other. The creation of such a "mimetic place" showed how theory was "embodied in the geography and architecture of the research institute itself" (Gieryn, 119), but the theory in question, rather than stripping sites of origin of their credibility, relied on their lingering trace. The Victoria Memorial Museum thus changed how truth was spatialized in Canada, both in the immediate scene of the exhibition halls and in the propagation of a geography of centres and peripheries. It brought some locations closer to truth, others farther, a process of deterritorializing one space and reterritorializing it as another.

. . .

3. **1911**. On the Team.

Sapir could not do all this work alone. With a location set, it needed to be filled with bodies: of actors, spectators, and artifacts. The Anthropological Division's second year saw the hiring of two more permanent staff: the archaeologist Harlan Smith, from the American Museum of Natural History, and assistant in anthropology Marius Barbeau, trained at Oxford. Together with Sapir, they represented three of the four Boasian subfields. (Francis Knowles was hired as the resident physical anthropologist in 1912 and received a permanent appointment in 1914.) The first order of business was two-fold: one, to encourage local support and attract temporary workers, as Sapir's (1912) appeal in

Queen's Quarterly, a specifically Canadian periodical, advocated; and two, to get the most out of the "rapid disintegration" of the tribes of Eastern Canada (Sapir 1912: 65), which occupied the staff for over a year and a half. Fieldwork during this period included: Iroquois and Algonquian of Ontario and Quebec (Sapir, Dr Alexander Goldenweiser of Columbia University); Wyandot of Lorette, QC, Windsor, ON, and Oklahoma (Barbeau); Micmac and Malecite of New Brunswick, Nova Scotia, and Prince Edward Island (Mr William Mechling of University of Pennsylvania and Dr Cyrus MacMillan, an English professor from McGill); and Inuit in the Arctic (Mr Vilhjalmur Stefansson). The Museum collection was already rich with specimens from the West Coast, but lacking in other areas; Sapir hoped to have "representative material" of them all within a few years (1912: 67).

A staff of three was too small for research, museum preparation, and administration, so temporary workers—a mix of amateurs and specialists, as the above roster shows—were hired on a rotating basis. Temporary staff were usually contracted for a field season or, afterwards, to complete manuscripts out of the field. Payments of salary for these workers, excluding research expenses, were half paid up front, the other half issued after research had been worked up. Renewals of contracts were ongoing and sometimes arduous negotiations, orchestrated between Sapir, Survey administration, and workers, and often vexing to all parties. Indeed, the terms of these limited contracts themselves led to confusion. In the case of young researchers, for example, a contract could include field expenses without a salary: "It is always assumed that a man is willing to go through a little trouble in the beginning in order to accumulate a little experience. There is, then, no allowance to be made for salary for your recent field work" (Sapir to Waugh, 20 February

1912, *GSC*).[22] More veteran scholars needed to be reminded that they would be prohibited from performing scientific work for other agencies while under the Survey's employ (Sapir to Radin, 17 August 1912, *GSC*). The truncated payment structure likewise weighed on Sapir himself, who was happy to recommend payment of a contract's balance even when a memoir did not include "absolutely all that is still coming to us"; he was eager to clear up "all our indebtedness" and, at a later date, hoped to pay salaries down in full: "as I think it is up to us to choose people who can be depended upon to have enough scientific interest in their work to write up the material" (Sapir to Mechling, 27 January 1916, *GSC*). Tensions between the ideal of enacting science and the practicalities of administering it would only multiply and continue to beleaguer Sapir throughout his time in Ottawa.

If Sapir had had his pick, and in some cases he did, he would have chosen a staff predominately of university-trained scholars who upheld similar values of "scientific interest." However, the emergence of anthropology within government-funded museums in Canada was insufficient to install a solid boundary between professional and amateur or to provide a corral of ready students (Waldrum & Downe 2006: 184). In the closing of his article for *Queen's Quarterly*, Sapir called attention to a need for university instruction in anthropology in Canada, claiming there was no better way for a new science to "take firm root" (1912: 68). Canada lacked a university program until the first department was founded in 1936 at University of Toronto, with hires at McGill and University of British Columbia beginning in the 1940s (Harrison and Darnell 2006: 4). Accordingly, many of the Division's permanent and temporary staff members were imported from, or at least trained

---

[22] Sapir regretted sending Waugh for one month's field work, and would not have had he known the latter had no permanent employment and was under some financial duress (20 February 1912, *GSC*). Waugh appreciated Sapir's offer of more time in the field (Waugh to Sapir, 1 March 1912, *GSC*).

in, the U.K. or the U.S. (where many studied under Boas). Although Sapir did not endorse the patriotism of national science, he recognized its pragmatism: "To make science a matter of nationality is, of course, the height of absurdity, but it is natural for a people to do its share of the scientific work being carried on within its own territories" (1912: 69). The conditions of university education in the country thus precipitated which bodies would be enrolled in the work of knowledge-production.

Partly due to this dearth of trained researchers in Canada, but also owing to the topography of the country, "boundary-work" between professional and amateur could only be partial.[23] The establishment of the Anthropological Division was not an abrupt transition of power, but as Nurse (2007) observes, it also initiated cooperation, especially in the domains of commerce and art. The Division's connection to the state, Nurse adds, did not always serve disciplinary autonomy, nor did the new centre disrupt local interests in anthropology or Indigenous cultures. Not all amateur anthropologists and collectors warmed to Sapir, either.[24] West Coast amateurs in particular received him as a foreign presence, preferring a "gradual and home-grown process of professionalization" over the Americanized brand Sapir represented (Darnell 2006: 141). On the other hand, not all amateurs were dismissed as poor imitations. In the same breath that Sapir urged a "higher standard" in North American linguistics akin to that of Germanic or Semitic linguistics, he complimented Father Adrien-Gabriel Morice for his grammars of the Carrier language

[23] Thomas Gieryn (1983) refers to the process of forging a demarcated scientific field as "boundary-work": "the attribution of selected characteristics to the institution of science (i.e., to its practitioners, methods, stock of knowledge, values and work organization) for purposes of constructing a social boundary that distinguishes some intellectual activities as 'non-science'" (782).

[24] The archaeologist Charles Hill-Tout was quite "sore" over one of Sapir's publications in *Science*: he "claims that I adopted a 'superior' and 'patronizing' attitude and that I have aroused dissatisfaction among several Canadian anthropologists. I presume he means himself. I had the bad taste to omit any reference to Hill-Tout in the paper" (Sapir to Teit, 15 March 1912, *GSC*).

(1911: 792–793). The enjambments of scientific professionalism and collaboration with amateurs speak to a slow rearrangement of bodies to serve the ideals of the National Museum, but also to a continued disjuncture. At no point did the Anthropological Division have a staff sufficient for the undertaking it proposed; it relied on a network of hobbyists, traders, adventurers, collectors, scientists, missionaries, stenographers, and government agents, with variegated ambitions and levels of expertise. Because Canada lacked the resources to study its diversity in full, the Division staff focused firstly on collection and secondly on publication and display (Zaslow 1975: 280), hopeful that later generations would resume the analysis they began. It was a utopic project, always deferring the conditions of its completion, with never enough hands to bring its goals into reach.

. . .

4. **1911**. On the Typewriter.

Within the history of American linguistics, the signature event of 1911 was the publication of Boas's *Handbook of American Indian Languages*. The *Handbook* argued that any dialect possessed a fixed sound system, distinct from (though sometimes co-occurring with) the axes of cultural or physical typology. Divided into two volumes, the book featured sketches from 14 language families of North America north of Mexico (based on John Powell's 1890 classification of 58 linguistic "stocks," which Sapir would later challenge).[25] Together, these sketches offered a standardized approach for classifying linguistic phenomena, focusing on three levels of analysis: "constituent phonetic elements," "groups of ideas expressed by

[25] Sapir (1921/2004) provided a gloss for his use of the term *linguistic stock* which, as my Introduction indicates, drew on the biological discourse of natural history: "All languages that are known to be genetically related, i.e., to be divergent forms of a single prototype, may be considered as constituting a 'linguistic stock.' There is nothing final about a linguistic stock. When we set it up, we merely say, in effect, that thus far we can go and no further. . . . The terms dialect, language, branch, stock . . . are purely relative terms" (125–126).

phonetic elements," and "methods of combining and modifying phonetic groups" (1911/1963: 28). Boas was less of a theoretician than Sapir, more invested in the practicalities of faithfully recording Indigenous languages.[26] Standardization helped to stabilize a scientific phenomenon at a distance, bringing the remote dream of cataloguing the hundreds of North American languages into reach. Another event, at a different node along this network, was no less important to realizing that dream: Sapir's requisition of an "Indian typewriter."

Figure 3. The Indian Typewriter (Catalogue #987.13.1, *GSC*)

The typewriter was an integral instrument for the replication of standard forms: its keyboard was modified specifically to transcribe Indigenous languages (chiefly of West

[26] Boas is frequently labeled "anti-theoretical" (e.g. Murray 1994: 62). Tracy Teslow (2014) argues that this interpretation was a posthumous dismissal by students (notably, Kroeber) in the 1940s and '50s, who were intensely interested in theory. Indeed, Boas postponed the inclusion of Sapir's account of Takelma for the second volume of the *Handbook* not because of the contents but the form of Sapir's analysis. As Boas told his pupil: "you have not adhered to the strictly analytic treatment that I wanted for the volume" (to Sapir, 31 May 1911, *GSC*). I deal with the topic of standardizing transcription styles; for more on the epistemological differences between Sapir and Boas, see Chapter 3.

Coast and Athabascan languages). Sapir commissioned the device upon his return from the field season, whereupon his office had need of "a special typewriter so constructed as to make it possible to typewrite Indian texts directly from notes to dictation. This would mean great saving of time in preparation of manuscript for publication and would also enable type-setters to print such material with less chance of error" (Sapir to Brock, 21 November 1911, *GSC*). The artifact in question (object "06-F100" in the Museum archives) was a manual typewriter, black, 25.5cm in height, 37cm in width and depth, and made of steel. It was produced by the Monarch Typewriter Company (the "Monarch Visible, number 3 model"), at a quote of $57. Sapir paid fastidious attention to the design of the levers in particular, supplying corrections to help capture the nuances of phonetic detail:

> 1. Instead of cedilla (ç) which comes on dead lever, there should be small hook coming below letter and turning to right instead of left (c˛).
> 2. Instead of superior w which has been substituted for cap. W there should be a small cap. w, that is, small w somewhat larger than ordinary w but not as large as cap. W. As it is, there are two superior w's provided for this machine, which is uneconomical.
> 3. There is no difference on this machine between small v and small cap. v. Small cap. v should be made somewhat larger than small v yet not as large as cap. v.
> 4. Instead of point above letter (ȯ) which has been put in as upper acute accent (ʻ) on dead lever, there should be point below letter (ụ).
>
> (Sapir to Brock, 2 February 1912, *GSC*; symbols not exact)

Within phonetics, related sounds could comprise disparate physical events (there were always minute differences between any repetition, an insight prefiguring poststructuralist thought), yet still fall within a range that made them linguistically the same. Sapir's concern over capitalization and diacritics demonstrated fidelity to the reproduction of phonetic differences in the process of transcription, useful for analysis later.[27] Sapir made efforts to ensure the typewriter and its symbols could articulate the widest possible range salient to Indigenous languages and, accordingly, his own perception.[28] Latour (1987) underscores the role of inscription devices in the replication of scientific knowledge; he defines a *machine* in this context not as a singular entity but as "a machination, a stratagem, a kind of cunning, where borrowed forces keep one another in check so none can fly apart from the group" (129). The Indian typewriter performed such a role for the Anthropological Division: attunements of ear and eye etched into the ethnologists' notebooks were, through the clack of fingers and keys, the coordination of levers and ink, made more solid and sedimented as knowledge.

The typewriter not only propagated standards but was one itself. Sapir's contemporaries sought to create similar devices: "I am buying a typewriter on the installment plan—the same one you saw here—and have just had some new characters and diacritical marks added for phonetic work" (Mason to Sapir, 9 June 1914, *GSC*). Its reproducibility exemplified how a science endeavoured to recreate the conditions of its

---

[27] The conventions, at the time, were to use small caps to indicate voiceless forms of consonants ordinarily voiced (lateral, trill, nasal continuants). In the 1920s and '30s, a "phonemic" mode of transcription began to proliferate and, for some, supplant the phonetic mode. Phonemic transcription did not include exhaustive details, since contrastive sounds could be predicted from the phonological rules of a dialect. It would prove to be another point of difference between Sapir and Boas; the latter felt phonemic forms could be inferred from phonetic transcription but not the reverse (cf. Anderson 1985: 205–207). I elaborate in Chapter 3.

[28] As his later student Mary Haas once reflected, regarding the old phonetic notation: "Sapir, I think, clearly loved every one of those apostrophes. He is very, very precise with that" (1976: 380, *MHP*).

success elsewhere. Its handiness, so to speak, was a matter of form as well as function. In Kittler's (1999) analysis, mechanical writing "deprives the hand of its rank in the realm of the written word":

> Therefore, when writing was withdrawn from the origin of its essence, i.e., from the hand, and was transferred to the machine, a transformation occurred[.] . . . It is no accident that the invention of the printing press coincides with the inception of the modern period. The word-signs become type, and the writing stroke disappears. The type is "set," the set becomes "pressed." The mechanism of setting and pressing and "printing" is the preliminary form of the typewriter. In the typewriter we find the irruption of the mechanism in the realm of the word. (199)[29]

The typewriter also gave rise to the profession of the typist, which I explore in §12 below.

Canonically, the typewriter formed an inscription technology that was spatially fixed.[30] At odds with the supposed static arrangement of keys, however, the Indian typewriter was not a static technology. Upon submitting his manuscript on "Abnormal Types of Speech in Nootka" (1915), Sapir remarked that the "paper will probably require making several new matrices for characters used in writing Indian words" (to Brock, 16 January 1913, *GSC*).[31] Typesetting for an ever-growing record of novel linguistic data had

---

[29] Both Kittler's and Latour's arguments draw on Elizabeth Eisenstein's (1979) account of Europe's transformation through the technology of the printing press.
[30] Katharine Hayles (1999) contrasts the typewriter with digital information networks, where the word is produced as image, "fluid and changeable" (26).
[31] This museum memoir presented data from Sapir's 1910 research trip to the Alberni canal, Vancouver Island; his informant then was the young chief Dan Watts, of the Nootka; further data was gathered in 1913–1914 by Alex Thomas, of a different tribe in the same region. Sapir's analysis of the Nootka language focused on features that marked characteristics of the person addressed or spoken of. Whereas features that distinguish social rank/status are linguistically widespread, in Nootka "consonantal play" indicated "abnormal" physical properties: e.g. unusually fat or short, physically defective, left-handed, circumcised.

to be a fluid practice. It gave rise to reformulation and repair: “I am arranging to have my Indian typewriter thoroughly overhauled, so that unclear and smudgy characters can be repaired” (Sapir to Boas, 12 December 1922, *GSC*). Interaction with its object of study thus transformed the science’s epistemic tools. Wiebe Bijker (1995) conceives of technologies as the frame upon which the interaction of multiple groups hinges. Certainty, the Indian typewriter was an event both in the emerging field of linguistics and in the extension of a discourse network on behalf of the Canadian government. In addition to embodying the principles of phonetic transcription, the typewriter materialized a power structure that favoured the Dominion: written over oral; settler over native; “modern” over “primitive.” A technology employed to extend reach, it also constrained range of motion, reconfiguring who could make assertions on behalf of language and where and how. These designs neither determined the device nor its use, however; it was not a tool of perfect replication, nor was it perfectly replicable. The typewriter was mutable. As an office tool, it changed many hands; as a representational platform, it was changed in turn by hands that would never touch it.

. . .

5. **1911.** On Native Speakers.

An assemblage therefore of purpose, place, person, and technology, the Anthropological Division was also a nexus of cultural encounters. Mary Louise Pratt (1991) defines the “contact zone” as a space where once geographically distal cultures “meet, clash, and grapple with each other, often in contexts of highly asymmetrical relations of power, such as colonialism, slavery, or their aftermaths as they are lived out in many parts of the world today” (34). The Division’s pursuit of scientific knowledge was no exception, traversing these zones and generating new ones. Kapil Raj (2007) brings an analysis of the intercultural contact zone into the domain of science studies; he understands the mobility of scientific knowledges not as a function of “emanations from a pre-existing center” (7), in which non-European cultures form “passive reservoirs of data” (3), but emerging instead through “complex processes of accommodation and negotiation” with local actors (9). His work attends to the importance of the field in prompting these encounters: a shift in localities from the “socially homogenous enclosed spaces” of the laboratory to the “open air” settings of map-making, linguistics, and administrative science (14).[32] In 1911, the Anthropological Division consolidated its centre in Ottawa but it also entered the field, and in so doing entered into similar themes of contact that Raj outlines: the work of translation, calibration, and interpersonal trust, all which follow from science’s change of venue (224), constrained and made possible the Division’s efforts. Translation particularly concerns me here. Sapir was the first to employ a native speaker’s intuitions “to test the adequacy of a grammar” (Darnell 2009: 34), a tactic still in use today, but the native speakers in question were also Native, and their participation occurred within the space of the contact zone.

Ethnologists were sensitive to the projection of categories in their translation work. Boas observed how the Eurocentric categories that substantiated linguistic analysis (such as case, gender, tense) were frequently troubled by Indigenous languages, and he endorsed taking a comparativist stance in the assessment of their phonetics, vocabulary, and grammar (1911/1963: 28–35). Sapir’s technique for drawing out a speaker’s intuitions

[32] On the “open air” sciences, Raj borrows from and complicates the work of Michel Callon: “Callon, who coined the term ‘recherche de plein air’ to designate knowledge practices that necessarily involve negotiations between specialists and other heterogeneous groups in their very making and certification. These practices, as Callon stresses, are fundamentally different from ‘field’ sciences where practitioners simply take the world outside the confines of the laboratory to be an inanimate space for collecting data, which is then centralized and processed in the secluded calm of the laboratory” (2007: 14).

supported this objective. A later exchange with Father Morice demonstrated how he went about using a speaker's own knowledge about language as an organizing principle:[33]

> I find any arrangement of linguistic material that proceeds from the standpoint of the reader's language irksome. As a scientific student of language I am always primarily interested in the native viewpoint. Hence alphabetical lists of elements from the standpoint of English seem to me rather beside the point. In a practical grammar, however, it may sometimes be advisable to follow the reverse method.
>
> (Sapir to Morice, 21 January 1918, *GSC*)

Sapir was determined to maintain the standpoint of his informants, but the act of translation was a displacement into scientific discourse nonetheless. Within ethnology, Ahmed (2000) writes, "The ethnographer creates and destroys at the same time in the very accumulation of documents . . . the need to resolve foreignness is set against a need to preserve it" (59). Linguistics held an indispensable role, in that regard. The study of language, unlike other dimensions of ethnology such as social structure or religion, had the advantage that its categories remained unconscious (Boas, 59), untrammeled by folk rationalization. The linguist was language's only witness. Linguists, whose work fell within the laboratory of the mind,[34] were inured to external circumstance; their method was the most intimately distant, their object the least affected by exposure to the open air. The

[33] Sapir elaborated on the differences between a practical and technical grammar: "A practical grammar has, of course, to proceed more slowly and carefully and to take less for granted on the part of the reader than a technical work intended for linguistic experts who can be trusted to take better care of themselves and who, moreover, need not master the endless details that are necessary for practical use" (to Morice, 21 Janaury 1918, *GSC*).

[34] Or "under his hat," as Benjamin Lee Whorf later put it. See Chapter 2.

solvency of linguistics as a science exceeded the violence of translation or the paradoxes of salvage. Or so they believed.

Though the aim of abstracting a linguistic pattern from physical encounters entailed their forgetting, the conditions that gave rise to translation were, for Sapir and his fellow staff, confronted on a routine basis. The mobility of science required more than the intuitions of N/native speakers. Paul Radin, a close friend of Sapir's and fellow Boasian, started work with the Survey in 1912 to study the language and social organization of the Ojibwe. Radin was hired on as a fieldworker, but completed his translation work in Ottawa, which Sapir had to explain to administration: "It seemed advantageous, in order to have him get his large mass of Indian manuscript text translated without interruption, for Dr. Radin to work with his interpreter at some distance from Sarnia" (to Marshall, 22 April 1912, *GSC*). Transporting informants to a separate research headquarters was convenient to isolate their language, but it was not always possible to consult them in a controlled environment, or to induce their cooperation when such conditions availed: "This winter I have the services of a half-caste lad about 15 years old and am trying to collect a little of the folklore. It is very difficult to induce them to speak of it, even when they are alone in my tent" (Jenness to Sapir, 26 December 1915, *GSC*). The ideal informant, indeed, required less coaxing: they were already fluent both in English and in their traditional language and could, with some effort instructing them in phonetic transcription, function as a relay for data collection. Regna Darnell (2009) notes: "Sapir preferred informants whose English was sufficient for accurate translation and who themselves had intuition for the structure of their native language. This ability was crucial in teaching Indians to write their language and record additional texts" (74). Staff recognized the importance of these interpreters, naming them in publications and expressing admiration in private: "It rather frightens me to think of doing non-material ethnology without an English speaking interpreter" (Sapir to Smith, 6 July 1920, *GSC*). Ethnology depended upon these intermediaries, but where their intuitions were valued, the conditions of their labour were sometimes taken for granted.

The Survey infrastructure upon which the Division operated was not always as conducive to informants as its staff were appreciative. When Sapir returned to the field in 1913 to resume study of the Nootka, he enlisted the aid of two interpreters, Frank Williams and Alex Thomas:[35] "I took considerable pains to get them personally interested in their old customs, many of which are now falling into disuse, and succeeded in teaching them to write their own language phonetically." Sapir could obtain material from them "at very much less than field costs," at a rate of 50 cents per standard size page, which included payment for "time consumed by the informant in dictating the text, time consumed by the interpreter in writing it down phonetically, time consumed by him in going over the text and putting in interlinear English translation" (Sapir to Brock, 20 March 1914, *GSC*).[36] The general rates of labour in British Columbia, he noted, were far greater than in the east; the Division provided ruled paper and Government envelopes that required no stamp (Sapir to Thomas, 19 March 1914, *GSC*). Correspondence between Sapir and Thomas reveal that payment for these elicitations was continually delayed, causing dissatisfaction (Thomas to Sapir, 28 August 1914, *GSC*, among other examples). Delays could last almost two months (Thomas to Sapir, 21 January 1916, *GSC*), occasioning Sapir to write to the treasurer to

[35] Sapir was "very eager that some of the younger Indians who can write English and who are in close touch with the old men that best know the old time customs should send me accounts from time to time of various things that I ask about" (Sapir to Thomas, 14 October 1911, GSC).
[36] Interlinear translation was itself a genre of distancing. It represented texts through up to four levels of progression: traditional spelling (in the original language); phonetic transcription; word-for-word translation; free translation (in English usually).

prompt payment (Sapir to Marshall, 24 January 1916, *GSC*). The relationship proved always to be an asymmetrical one. When Thomas sought to arrange a higher pay scale, having found an informant who "cannot part" with a whaling account ('osimtc) except for a greater sum (to Sapir, 3 January 1916, *GSC*), Sapir refused: "arranging for an extra scale of payment for material like the whaling 'osimtc . . . creates a very bad precedent for other kinds of information"; the treasury department, moreover, would not understand why one type of information would cost more than another: "It would not look business-like, and might get them so disgusted with our work that they would refuse further support of it" (to Thomas, 27 January 1916, *GSC*). Sapir was also in a position to critique Thomas's handwriting; he noted it was getting worse, perhaps due to the number of texts Thomas was producing, and thus providing fewer words per page, which at a rate of 50 cents Sapir deemed unfair (to Thomas, 18 February 1916, *GSC*). In "The Economics of Linguistic Exchanges," Pierre Bourdieu (1977) argues that the "objectivism" of the language sciences relies on a relation of symbolic power between the speaker and analyst, one which brackets the social conditions that gave rise to the encounter. Here, we see how infrastructure prefigured the linguistic structure these agents hoped to ascertain, in the first place by the vital presence of globalized English, a legacy of imperialism, and secondly by the uneven economies of translation.

. . .

- **Extension (into the Field): 1912–1913**

6. **1912.** On Long-Distance Dependency.

Sapir relished the chance to do fieldwork, and over the course of his tenure with the Survey had a handful of opportunities, large and small, for the pleasure of working *en plein air*.

More often the case, his employment as Chief Ethnologist meant he had to take a step back and assume a supervisory role, which he pursued with equal attention and care. In 1912, Sapir did some collecting himself, but for the most part remained at the Division's headquarters occupied by managerial and museum work, while his staff pursued their research afield: Barbeau continued his study of the language and culture of the Wyandot in Oklahoma; Meckling on the Malecite of New Brunswick; Radin on the Ojibwe of Ontario; James Teit on the Tahtlan of Telegraph Creek, BC; Goldenweiser worked on the language, culture, and social organization of the Iroquois, while Waugh studied their technology and Knowles began obtaining physical data, including skeletal remains (1912 Annual Summary Report, *GSC*). Sapir was an active yet magnanimous supervisor, who believed in letting his agents find their own path and focused himself on relating their researches to the larger goals of the Division (Darnell 2009: 66), though he guarded linguistics "jealously" (69). He corresponded with his staff in the field often, and his letters reveal in what ways he guided them, particularly on matters linguistic, from afar.

Calibrating the four subdisciplines (archaeology, physical anthropology, cultural ethnology, and linguistics) was one task Sapir performed in this role: differences among potentially overlapping specializations had to be teased apart to ensure the smooth operations, and mutual respect, of his team. For example, Goldenweiser's and Waugh's research converged on the subject of Iroquois medicine, so Sapir advised Goldenweiser to focus on the ritualistic elements and Waugh on the material (Sapir to Goldenweiser, 12 April 1912, *GSC*). Sometimes these approaches diverged as well, both in manners of collection and varieties of data. Archaeologist Harlan Smith remarked on the dissimilarities between his specialty and cultural ethnology: "Barbeau is back with the fat and short hair indicative of what he has been through. I envy you ethnologists the ease with which you can find material to work upon. You don't have to walk weary miles without finding anything" (Smith to Sapir, 6 November 1913, *GSC*). Goldenweiser confided his opinion that ethnology and physical anthropology did "not mix very well" (Waugh to Sapir, 10 February 1912, *GSC*). To Sapir's surprise, what constituted good linguistic evidence did not necessarily translate into useful ethnological material: "I must confess that I considered myself as having been somewhat taken in [by Mohawk texts], though even so we have a probably reliable mass of text material that would prove valuable for purely linguistic purposes" (Sapir to Goldenweiser, 11 October 1912, *GSC*). Having for years negotiated such trials of interdisciplinarity, Sapir later wrote, "Any one person knows comparatively little, but is almost certain to know something that hardly anyone else has heard of" (to Mason, 29 November 1917, *GSC*). This was the philosophy he imbibed as Chief Ethnologist.

Nonetheless, on the subject of linguistics, there was none who could know more than Sapir, and it was no secret. Linguistics training in North America was still relatively scarce, even among anthropologists, and operated largely within an apprenticeship model and through private networks of expertise. As Mason noted: "I should not be attempting serious linguistic work on the same scale as you or Harrington with the little training that you gave me and knowing nothing of Indo-European" (to Sapir, 22 September 1912, *GSC*). Ethnologists acquired limited linguistic facility, enough to record texts rather than to abstract grammars: "I do not think I ever will be competent enough in linguistics to write on I[roquois] grammar" except "in order to be able to give intelligent translations and, wherever necessary, analysis of the text" (Goldenweiser to Sapir, 15 April 1912, *GSC*). By contrast, Sapir's mastery of linguistics was intimidating: "I am a little bit afraid to talk linguistics with you after your last two letters on this subject, because you sit all over me for minor mistakes even when, as in the case of the Tanoan material I sent you, I told you that it consisted of stray notes thrown together in a haphazard way" (Radin to Sapir, 27 June 1915, *GSC*). Yet some staff members saw it as an occasion to improve their ability on the subject. As Waugh noted: "I wish to avail myself of every opportunity to advance in this line, as I note the intimate bearing it has upon the work in hand" (to Sapir, 4 September 1912, *GSC*). For such ambitions, Sapir was happy to lend his support and recommend readings.

Nonetheless, he maintained a sharp distinction between the rudimentary linguistic work ethnologists accomplished and the more specialized philological training he employed:

> I am rather surprised to learn that you expect to devote yourself mainly to linguistics on your third trip. Do you mean to intend to undertake a thorough-going study from the point of view of a philologist, or that you wish merely to obtain enough of an insight into Iroquois grammar to enable you to handle your ethnological text material properly?
>
> (Sapir to Goldenweiser, 28 March 1912, *GSC*).

> It will naturally not be either necessary or possible for you to go into details on any grammatical point. The main stock of your linguistic data would have to be lexical and the grammatical data would merely be supplementary by way of more sharply defining the linguistic boundaries already presumably fixed on the basis of the phonetic peculiarities revealed in the vocabularies. . . . [A]nything like a thoroughgoing study of the Athabascan dialects would

> have to be undertaken by a linguistic specialist and would require an extended period of time with each tribe visited.
>
> (Sapir to Teit, 16 January 1913, *GSC*)

For many linguistic problems, only Sapir's own ear would do: pitch accents in Athabascan, for instance, were material of "a rather delicate phonetic nature" that Sapir "must secure [him]self" (Sapir to Teit, 17 November 1920, *GSC*). On other subjects he showed great humility, but on linguistics he knew he had no equal within the Anthropological Division.

Beleaguered as he was in Ottawa, however, Sapir was prevented from exercising his fine-tuned ear on every occasion. He learned, instead, to employ his network of fieldworkers as extensions of his knowledge-taking apparatus. Radin, who described himself as a "lay-philologist" (to Sapir, 27 March 1912, *GSC*), became one such tool. Sapir coached him to distinguish the place of articulation of vowels in Ojibwe: "The second variety of vowels pronounced way back in the mouth may be velarized or uvularized vowels, i.e. vowels pronounced with resonance due to position of the back of the mouth and tongue approximating to the q̣ position. Thalbitzer describes such a se[quence?] of uvularized vowels for Eskimo" (to Radin, 22 March 1912, *GSC*). Sapir's comparative training granted him remarkable confidence in his own intuition,[37] which he broadcast to his workers in the field. He urged Radin to better discriminate between "surd" and "sonant" (voiceless and voiced) stops and, in preparation of the final report, continued to draw out precise phonetic details: "If you hear arrested stops as distinct from long consonants, they should, of course, be separately denoted" and could, for the benefit of the printer, be

[37] Reading through Boas's report on Siouan in the *Handbook*, Sapir was alarmed by the number of misprints, remarking that the phonetics did not "chime" with his own; admittedly, he was "very likely" wrong, since he had never heard Dakota (to Radin, 7 February 1912, *GSC*).

represented by means of "superior perpendicular lines" followed by "p, t, and k" (to Radin, 5 June 1913, *GSC*). This work of long-distance coaching was calibration of a different sort, but as Radin once put it, "sometimes even a phonetician is a bad prophet, as witness" (to Sapir, 22 January 1913, *GSC*). Sapir trusted the perceptions of some agents better than others, but even at a distance, he trusted his own analysis most of all.[38]

Barbeau was another example of an agent whose senses became enrolled as an extension of Sapir's. Initially, his interest in the Wyandot language served only ethnological purposes. An episode with one of his informants, Kate Johnson, demonstrated the practice of shifting linguistic registers during elicitation. He had begun recording a story with her, in English, when upon realizing its ethnological importance "changed [his] plan and decided to get it directly in Wyandot" (Barbeau to Sapir, 3 November 1911, *GSC*). The repetition of such encounters induced a shift in epistemological registers, and Barbeau became more devoted to the study of the language itself. At first, Sapir gently discouraged his eagerness:

> I am glad to learn that you are carefully reviewing your names and texts for the purposes of getting more accurate linguistic analyses, and I am not surprised to hear that this work proceeds rather slowly. I am somewhat skeptical, however, of whether you ought to concentrate more on Wyandot linguistics than is really necessary to enable you to understand your text material for ethnological purposes. A thoroughgoing study of an Iroquois

[38] Sapir described his own process: "As to writing up grammars, I may say that I consider it dangerous to rely on one's general knowledge, no matter how fresh it may be in one's mind. The only plan that seems to me to be worth a moment's consideration is to make careful collectanea under various heads, phonological and morphological. In this way the material boils itself down to systematic shape, and the actual writing is hardly more than putting facts and ideas into connected form that are already worked out for you inductively. . . . As for my own linguistic work, I do not intend to publish anything that I call a complete grammar except in a slow and pedantic manner" (to Radin, 2 April 1913, *GSC*).

> language is no joke and would, in itself, require many months of uninterrupted labor.
>
> (Sapir to Barbeau, 7 May 1912, *GSC*)

Barbeau persisted nonetheless, already impressed by his own progress: “Although I have but begun the handling of my already bulky material I have been surprised at the number of things I could understand after a short while, that I had not noticed before” (Barbeau to Sapir, 15 May 1912, *GSC*). Sapir was less impressed, and in his critique of Barbeau’s report on Wyandot phonetics, reiterated to him at length the difference between “intermediate” unaspirated stops and voiceless aspirated stops: “All Iroquois dialects that I have ever heard have these two series of stopped consonants, and it would be decidedly disturbing to find that Wyandot was quite different in this fundamental respect” (Sapir to Barbeau, 22 May 1912, *GSC*). He suspected that the problem lay in the Barbeau’s inability to hear the difference, rather than its being an exceptional property of this dialect, and recommended he listen again and compare with Boas’s cases. A linguist’s ear needed to be sensitive to the production of speech sounds that were non-distinctive in their native language.

Barbeau was frustrated: “I have for the 3rd time the other day gathered all my faculties and tried, with only that end in view, to detect three series of dentals instead of the two I have so far recorded; . . . discarded the hope of ever finding more than two series of dentals in Wy., both of which are aspirated as in English, one not being vocalized” (to Sapir, 13 June 1912, *GSC*). After another month of effort, he was successful in finding the equivalent in Wyandot of what Sapir described in Oneida and other Iroquoian dialects. The solution to the problem was unexpected and simple. The seeming irregularity that perplexed Sapir and Barbeau could be explained through language change: in Oneida, words with aspiration had undergone a process of reduction at some point in the language's history, wherein the dropping out of vowel resulted in alternation between voiced and voiceless aspirated stops. Wyandot had the same "same series of aspirated sounds" but "in such a way that you cannot call them that name" (Barbeau to Sapir, 16 July 1912, *GSC*). Sapir approved (to Barbeau, 20 July 1912, *GSC*), and he began to rate Barbeau's abilities more highly.[39] Sapir encouraged him to investigate Wyandot structure further, proclaiming it "indispensable linguistic data" (1912 Summary Report: 456), and later helping Barbeau to work up the material for publication as a Museum memoir (1915). Years afterwards, Barbeau remained a proven accessory: "I should be much obliged if you could test out at least one point. I have a strong suspicion that the Athabascan languages have pitch accent. If you could determine this point for Carrier it would be a most interesting and important thing for me. The difference in pitch would probably be one merely high and low" (Sapir to Barbeau, 16 July 1920, *GSC*); "There are so few people that can distinguish pitches adequately that it would seems a pity to lose this opportunity to get some independent evidence" (to Barbeau, 10 August 1920, *GSC*). As the Indian typewriter was a technology for the extension of reach, so too were Sapir's agents thus an extension of his sensorium, enabling him to elicit data even in his absence, to hear through their ears, and to speculate on the mouths of informants whom he'd never met. The linguist's subjectivity was at once "distributed" and "centred" (Mialet 2012), intimate and distant, but it did not operate with the predicted harmony of a closed system. These moving parts had to be carefully calibrated, and the spaces in between held the capacity for imperfection

[39] "I trust that ultimately you will be able to work out the grammatical rules regarding pronominal class that is followed by noun-verb complexes. . . . I must say that I am very much gratified with the enthusiasm with which you have taken up Wyandot linguistics" (Sapir to Barbeau, 29 July 1912, *GSC*).

and surprise. At the same time, this elliptical space attenuated the intimacy of the encounter: it allowed Sapir to reach out across the contact zone and apprehend linguistic data from the intuitions of Indigenous informants, reducing them to distant tongues.

. . .

7. **1912.** On Ticklish Data.

The micro-transactions of power between agents and informants were contingent on a foundation of interpersonal trust, not always realized on both ends. Especially at the outset of their work, the staff met with a persistent non-recognition of their scientific authority. As Barbeau disclosed plaintively to Sapir early on: the elders, whom he considered the "most primitive of the Hyandots, from whom I expected most, have partly failed, so far, to help me"— taking any opportunity to "escape" (1 October 1911, *GSC*). Sapir's reply indicated that Barbeau was learning a lesson every ethnologist did: "When you want them the most, then they discover that they are 'very busy'"; instead of contributing to Western scientific work, they "hang around the store and gossip" (5 October 1911, *GSC*).[40] Their informants' ability to refuse cooperation worried Sapir more upon the hiring of a physical anthropologist, Knowles, who was to join Goldenweiser and Waugh at the Six Nations Reserve at Grand River. Sapir recognized the delicacy required of those encounters:

> The work of measuring Indians is naturally a somewhat more ticklish proceeding than ethnological or linguistic work and may need a good deal of explaining to Indians. I hope that you will reassure them as to the character of the work whenever you have the opportunity to do so. It may not be a bad

[40] Eventually, Sapir coached his staff members to work around the rhythms of their informants: "It is sometimes difficult to get good interpreters on the West Coast during the Summer, when so many of the Indians of British Columbia are fishing or working on canneries" (Sapir to McIlwraith, 19 August 1921, *GSC*).

> idea, though I should be somewhat cautious in expressing myself, to imply that these measurements are intended to show that the Iroquois are the finest type of Indian.
>
> (Sapir to Waugh, 9 April 1912, *GSC*)

On this subject he wrote to Chief John Gibson of the Seneca people the same day, presenting the case for Knowles and stimulating the ticklish membrane of trust.

Gibson had previously cooperated with Survey members in procuring specimens for the Museum. Neither Goldenweiser nor Waugh, who studied respectively the social organization and material culture of the Iroquois, had "paid attention to the Indian race itself," Sapir told him. Sapir's letter contained an appeal on behalf of Knowles for the collection of biometric data, urging Gibson to encourage cooperation amongst his people.[41] He was careful to distinguish this research as a scientific and not a political enterprise:[42]

> Please do not imagine that this work is anything like a census for the Department of Indian Affairs. It is simply part of the work that we are doing in the study of the natives of Canada. We hope by means of these measurements to learn more than we now know about the Indian race. At the same time, we shall be able to find out how the Iroquois Indians compare

---

[41] Specifically, Knowles was to "get a very clear idea of the Indian type, including hair, eyes, skin color, shape of head, face, length of limbs, height of body, and weight" (Sapir to Gibson, 9 April 1912, *GSC*).

[42] The relationship of Boasian anthropology and race is a complex and thorny one. Teslow (2014) observes how the category disappeared from a mid-century anthropology that retreated from a positivist articulation of race and, reflexively, from their memory of its propagation under Boas. Physical anthropology, however, was committed to accounting for the variability of bodies across cultures. Boasians believed that racism was rooted in individual racial prejudice (15); in their view, it could be corrected by a "good" racial science, one which rejected "universal stages" of human development and understood race instead to be a historically contingent and environmentally conditioned phenomenon. Sapir was conscious of the negative connotations of what, to him, was a positive science of race and made efforts to maintain distance between them. See §15 for the collision of these premises.

> with other tribes in America. No doubt they will be found to be physically superior in several respects to other tribes. I should be personally thankful to you for any help that you could give Mr. Knowles and for whatever you can do to make the Indians of the Reserve understand the character of his work.
>
> (Sapir to Gibson, 9 April 1912, *GSC*)

Gibson received the proposition congenially; was "only to[o] pleased" to assist Knowles (to Sapir, 19 April 1912, *GSC*). Sapir's flattery, so cautiously expressed, hit its mark. What's more, it suggested the intimacy of courting evidence. In the *Handbook*, Boas recommended that "intimate friendships" formed the basis of superior ethnography (1911/1963: 51), and the best kind what informants themselves committed to print. At the same time, the very bonds that enabled this work could impede the register of authenticity they hoped to cultivate: the "authentic" was a function of distance from the West that could, inversely, only be obtained through intimate contact with its agents. Often, it was ethnologists who patrolled these boundaries. Nurse (2006) reveals how ethnologists often took an active role in discerning what were "authentic" elements of Indigenous culture from those derived from contact; within these encounters, "effective ethnographic research required discrimination" (54), since even to informants the origins were unsure. It was accepted practice, then, to mislead (Boas 1911/1963: 50), misdirect, or in this case flatter subjects into yielding data that was uncontaminated by "secondary reasoning and . . . re-interpretations" (Boas, 56). In Sapir's words: "The aboriginal element should always be carefully peeled out" (to Waugh, 3 October 1911, *GSC*). Ethnologists strove to be respectful both of person and custom, as Sapir's letter to Gibson shows, but the salvage paradigm, with the authority of the Canadian government behind it, licensed them to make exceptions.[43]

As Sapir's letter also suggests, peeling occurred in practice and practitioner alike: ethnologists had to draw out ticklish data out the conditions of contact that gave rise to the encounter; in the same place, they felt the need to disambiguate their research interests from the government that authorized their work's scientific force. Whether the latter was included to persuade his informant or himself remains unclear. Sapir was sincerely "taken aback" by Gibson's "sudden death" on 1 November 1912 (to Goldenweiser, 6 November 1912, *GSC*). Disappointed that the obituary notice made no mention of the late Chief's involvement with the Survey, he made efforts to commemorate his friend, who would be "immortalized in the pages of our Journal" (Sapir to Goldenwesier, 12 March 1913, *GSC*). Sapir wanted to convey the authenticity of their relationship, and perhaps it was so, the intimacy between the two men genuine. But it was also an intimacy across contact zones and unavoidably a distant one.

. . .

8. **1913**. On a Glacial Pace.

The Canadian Arctic Expedition launched in 1913, the largest and most ambitious scientific undertaking in Canada to that date. It continued the work begun by Vilhjalmur Stefansson in 1911, who organized and led the enterprise. The Expedition was not directly under the auspices of the Survey, but organized under a special grant from Parliament (Sapir to Jenness, 18 April 1913, *GSC*). Stefansson had originally secured funding from two American

---

[43] Barbeau, for example, went about spying on a ceremony to which he was denied access (Nurse 2006: 59). Leslie Dawn (2006) provides a more thorough account of how Barbeau later used his knowledge and authority to confirm a discourse of Indigenous disappearance.

institutions (the National Geographic Society, in Washington D.C., and the American Museum of Natural History; $22,500 from each). On his return to Ottawa, he also gained support from the Survey and Prime Minister Robert Borden. The latter, however, was concerned over issues of sovereignty within a predominately American-funded expedition and decided, ultimately, for Canada to fund the entire project (Jenness and Jenness 1991: xxx). It henceforth became the *Canadian* Arctic Expedition, and any new lands or resources discovered would belong to the Dominion Government. The internationalism of science proved secondary to nationalist and economic interests. Discontent arose early among the staff members, who objected to two clauses in their contracts regarding communication to and from the Expedition, insisted upon by Director Brock (xxiv).[44] In addition, all notes were to be turned over, a condition which prompted threats of resignation. The disagreement passed and the crew set sail aboard the *Karluk*,[45] with command over the exhibition divided between a Northern and Southern Party as a result (the former led by Stefannson, the latter by Rudolph Anderson). The Northern Party intended to explore new territory deep in the Arctic; the Southern to catalogue the flora, fauna, geography and peoples of the Mackenzie River delta, with two ethnologists on staff: Diamond Jenness and Henri Beuchat.[46] Misgivings lingered toward their leadership upon departure from Esquimalt, Vancouver Island, on 17 June 1913. Their hopes were not all that would sink.

[44] One, that no news could be given out except through official reports from Survey headquarters, and two, that all mail from Expedition members had to be forwarded through Ottawa (and presumably vetted).
[45] The *Karluk* "was a California-built brigantine, launched in 1884, and used initially in the Alaskan salmon trade. In 1892 it was bought to operate in the newly opened whaling grounds near Herschel Island (northwestern Canada) and made 14 voyages into the Arctic before lying idle from 1911 to 1913" (Jenness and Jenness 1991: xxxii).
[46] The two were to be paid the lowest salary among the scientific staff ($500); only the cook had one lower ($480). Stefansson, in place of payment, negotiated exclusive publication and lecture rights to the material (xxxi).

On 12 August 1913, the *Karluk* was firmly trapped in the ice northeast of Flaxman Island, off the coast of Alaska. It was "not powerful enough to force its way any farther . . . and this time it did not escape. Thereafter it drifted with the ice, first eastward for several days until it lay north of Camden Bay, then slowly westward until ultimately, in mid-January 1914, it was crushed by the ice and sank" (xli). Nearly half the crew members perished, among them Beuchat, whose health was already diminished (xliii), and who died trying to reach land. Along with the loss of life was the loss of ethnographic equipment, as Jenness informed Sapir: "All my instruments and many of the ethnological books were left on the *Karluk*; the rest were at Collinson Port—in either case equally inaccessible during the winter" (19 October 1914, *GSC*). Julie Cruikshank (2005) chronicles how glaciers have long been the site of contact between Indigenous and European worldviews; insofar as the Western division between the categories of nature and culture has prevailed, she urges the reader to revisit local ontologies that understand the human and natural worlds as responsive to each other. In this episode, the Expedition was induced to respond; the transit of the Western knowledge-taking machine delayed. The environment was not a passive scene on which contact zones were set; Canada was itself an actor, at times yielding, at others ferocious. Indeed, as the diaries of Jenness narrate, waiting became not the absence of possibility but its primary spatio-temporal condition: in waiting—for game to hunt, for snowstorms to abate, for supplies to restock, for spare moments to elicit stories and other linguistic material, for dull evenings to commit all this to record—Jenness formed the basis of his ethnography. In this way, the rhythms of the Arctic and its peoples shaped the scientific research that sought to abstract knowledge from them.

The initial inclusion of two ethnologists within the Expedition was to study the Inuit peoples of the Coronation Gulf, whom Stefansson had named the "Copper Eskimo" (Jenness and Jenness 1991: xxxi). Their main goal was "the collection of a full ethnographic material, based on study and observation among the Eskimos of the Arctic region," concentrating on the non-material side of culture, since their technology had already been studied (Sapir to Jenness, 6 March 1913, *GSC*). Sapir had recommended the more experienced Beuchat perform the linguistic work, while Jenness undertook the anthropometric collection (to Jenness, 19 June 1913, *GSC*). With the former's passing, it fell all to Jenness, who became another example of ethnologist-turned-apparatus.[47] However, he was greatly disadvantaged by the loss of equipment aboard the *Karluk*:

> This was a very severe handicap, more especially in dealing with the Eskimos of this region, who have lost many of their ancient customs under the influence of the whites, and with whom therefore observation alone does not yield very profitable results. The only alternative method is through the language, but unfortunately the Eskimo language is an extremely difficult one, both structurally and phonetically. My interpreter was only in his 16th year, and his knowledge of English was confined to the ordinary conversation current among a few white men in a very peculiar and limited environment.
>
> (Jenness to Sapir, 19 October 1914, *GSC*)

[47] Sapir: "I had not written to you in regard to linguistic matters, as I had imagined perhaps mistakenly that your training and interests had not been along those lines, and I am therefore doubly pleased to find that you expect to pay attention to this aspect of the work. Of course, the very best sort of ethnological material that you can get would be texts obtained from dictation" (to Jenness, 7 May 1913, *GSC*). Jenness made excellent progress in recording texts and vocabulary: "spoken words which he has had repeatedly reproduced before the natives so that he could get the text letter-perfect and translated for comparison with other Eskimo dialects" (Sapir to Jenness, 29 July 1915, *GSC*).

Ethnology required specialized techniques, instruments, and the cooperation of informants. As Nurse (2006) observes, Native peoples sometimes interpreted this situation as a cultural exchange, a perception either abided or discouraged; to ethnologists, whose primary interest was cultural memory, informants were "repositories of the past" (59). Within the salvage paradigm, remoteness promised a greater affinity with the past and retention of traditional knowledge, but these expectations were routinely dashed.

Within his diaries, Jenness was persistently troubled by the ostensible loss of ancestral knowledge. At other moments, he reflected on these lacunae as a mode of agency:

> The [Cape] Halkett people were laughing about it while Brick was present—it was a popular story which Stefansson had recited to them as an example of one of their charms. He had asked them to tell him any more they knew, for he would write them down and they would always be remembered—otherwise they would soon be forgotten. At the time they sat quiet and said nothing, but laughed heartily when Stef[ansson] had gone. Such is the fate I fancy of many an ethnologist—more often than is supposed.
>
> (Jenness and Jenness 1991: 122)

In *Unspoken*, Cheryl Glenn (2004) analyses how Indigenous students employ silence tactically in the classroom to disrupt structures of power; here, as well, silence punctuates the rhythms of data collection, rendering them elliptical.[48] The people, like the land, resisted assimilation into the categories of Western knowledge. The story of the Arctic Expedition was as much, therefore, one of the failure of scientists to inhabit the space of

---

[48] Audra Simpson (2014) argues for an ethnography of "refusal" that moves anthropology away from the "documentary" mode and its reliance on antiquated categories of difference that derive from colonial contact.

Northern Canada as it was the purported failure of Indigenous peoples to adapt their culture to the changing times. Yet it was also a story of adaptation on both sides, and the disinclination of ethnologists to notice and account for this parity reflected the biased teleology of salvage that coloured their perception. This ideology confined First Nations, Inuit, and Métis to vessels of the past, displacing their shared present and imaginable future. In many ways, waiting still characterizes the relations between the settler state and Indigenous peoples; for others, waiting upon this elliptical curve has ended and they are idle no more.

. . .

9. **1913**. On the Hall.

The Museum was not static while its agents were out collecting data and specimens in the field, though as a repository of these materials it was, certainly, dedicated to stasis. In 1913, the first exhibition hall opened for the public. According to the 1913 Annual Summary Report, the Division of Anthropology "select[ed] such material as seemed most calculated to give the public a general idea of the culture of the more important tribes of Canada," with the remainder carefully stored with the aim of future exhibition (Sapir et al. 1913: 358, *GSC*). These materials were all numbered and organized according to Sapir's five cultural areas. However, exhibits representing only three of those areas were to be installed during 1913 (the Eastern Woodlands, Arctic, and West Coast); materials from the Plains and Plateau-Mackenzie, coming into the Museum in increasing quantity, had to be stored due to lack of exhibition space (Sapir to Brock, 17 January 1913, *GSC*). The task of categorizing, cataloguing, and storing these materials was a substantial one: in 1912 alone, over 1500 specimens were added to the collection, by gift or purchase; the following year, over 1300.

Caring for these collections posed a challenge, as well, and meant responding to another, no less assertive, environment: the literal ecology of the institute. Here, Sapir encountered the mundane materiality of salvage: “Owing to the continuous dry heat in the hall, a large number of specimens are bound to deteriorate,” he observed, recommending a particular level of moisture be maintained through two or three small tanks of water kept in a suspended canoe, “an out of sight moisture distributer,” replenished regularly by the caretaker (to Brock, 19 November 1912, *GSC*). To avoid further atrophy, he wanted the temperature in the ethnological hall as close to the freezing point as possible, leaving instructions for the engineer to turn off all the heating coils except one (to Brock, 8 February 1913, *GSC*). Initially, museum work was hindered by the lack of a full-time curatorial assistant, but from 1913 to 1919 Frederick Waugh arrested his fieldwork and was hired on as preparator to manage and maintain the growing collection of ethnological specimens.[49] Negotiating the needs of this space demanded continual attention, both at the everyday and managerial levels.

The necessity of the Museum as a truth-spot, so clear to its scientific staff, was not always shared to the same degree by administration. On more than one occasion, Sapir appealed to the Deputy Minister of Mines on behalf of the urgent need for a second exhibition hall to fulfill the objectives of the Museum. Lack of space was a persistent complaint throughout his administrative correspondence.[50] In reply to a letter from the Acting Deputy Minister calling for information on the Division’s activities, he emphasized

[49] Sapir advocated on behalf of Waugh, who in his view was not merely a clerk but remained a member of the “scientific staff,” and whose beginning salary should be $1600 rather than $1200 (Sapir to McConnell, 17 September 1914, *GSC*). Within anthropology, museum work was scientific work.
[50] Early on, Sapir once protested the lack of a “Yale lock and key” for the basement room devoted to ethnological work; the room was accessible to anyone, and it soon “developed into an informal coat room, repository of whiskey bottles, and so forth” (Sapir to Brock, 29 February 1912, *GSC*).

museum work as one of its three major undertakings, alongside field research and publication. The Museum had a special role in mediating knowledge claims for a broader public: "Of more direct appeal perhaps to the general public than our publications are the museum exhibits, which are planned to give in readily intelligible and palatable form some of the more elementary or obvious results of our researches." He reiterated the Museum's requirement of "enlarging its facilities" and claiming a second hall, though even that, he foresaw, was a modest proposal: "Properly speaking, the Division of Anthropology should have a complete floor, embracing four large exhibition halls, assigned it for its exhibitions. As our collections grow we will find that even the two halls now asked for will be quite inadequate" (to McConnell, 2 October 1914, *GSC*). He was not alone in these concerns. The principal officers of the Geological Survey branch involved with the Museum, which included Sapir,[51] formed a committee that "drew formal attention to this condition which at that time threated to paralyse any effort looking toward the realization of a Museum in keeping with the dignity and rapid development of the Dominion." The Museum staff had to share the Victoria Memorial Building with the Drafting and Topographical staffs, which caused "congestion" in the basement and on the second (office) floor; the third floor, the only one suited for "lighter laboratory work, study and storage rooms absolutely necessary for the requirements of the Museum," was used instead as drafting room. The crisis of space already threatened "a cessation of activities":

[51] The other signatories were: Lawrence M Lambe, Vertebrate Paleontology; E. M. Kindle, Invertebrate Paleontology; J. M. Macoun, Assistant Botanist and Naturalist; Robt. A. A. Johnston, Mineralogist; Harlan I. Smith, Archeologist; L. D. Burling, Assistant Invertebrate Paleontologist; W. J. Wilson, Assistant Paleontologist; P. A. Taverner, Naturalist and Curator; Francis H. S. Knowles, Physical Anthropologist.

> The exhibition halls four years after the occupation of the building are with one exception still devoid of the necessary exhibition cases, and it has been necessary to resort to other devices on which to place exhibits. One of these halls . . . has for some time been used as a freight shed and as a storage room for tents other field equipment as well as a lot of other materials which for lack of a more suitable definition may be classed as rubbish.
>
> (Sapir et al. to McConnell, 16 October 1914, *GSC*)

The Museum was a site for mediating scientific truths, but before the staff could reach their intended audience, they had to persuade another public of their superior claims to its use.

The officers couched their Memorandum in patriotic language, perhaps hoping to frame their appeal in terms consistent with bureaucratic interests:

> In modern times every national capital of any importance has its collections of objects pertaining to the various arts and sciences. They are possessions in which the people take national pride. . . . The great museums of London, Paris, Berlin, Vienna, Petrograd, Dresden, Munich, Birmingham, Calcutta, Tokio, Washington, New York, Chicago and a host of other cities of the world may be cited as instances of this kind. The ordinary visitor to the halls of any of these institutions views only one of the phases of their activities—the exhibition phase. He is probably wholly unaware of the patient and painstaking care often involving weeks and even years of unremitting labour before many of the things which he sees may be satisfactorily presented to view.
>
> (Sapir et al. to McConnell, 16 October 1914, *GSC*)

The Museum's predecessors, among them John Macoun, likewise struggled to convince the government officials of the worth of such an institution; many politicians did not believe in science for its own sake, but saw it as an investment to promote the material resources of the country (Waiser 1989), one on which they could demand returns (55). The Museum was ultimately recognized by Parliament, but not in the way Sapir and his fellow scientific staff had hoped. The Hall of Canadian Anthropology was closed to the public from 1916 until 1921, when the Victoria Memorial Museum became a temporary base of operations for the Senate after a fire destroyed the Centre Block of Parliament. The "Indian Hall" had been the only part of the original museum plan completed to that date, and the specimens curated for exhibition were "liable to deteriorate" outside their present conditions (Sapir to McConnell, 3 January 1918, *GSC*). The place of science under the patronage of the Canadian Government was far from static, and it was likewise liable to deteriorate during wartime years, as we will see.

. . .

❖ Retraction (the War): 1914–1918

10. **1914**. On Linguists, Fire, and Dangerous Intimacies.

The year began with a fire. "Mr. Tavener's house burnt up yesterday" (Smith to Sapir, 15 January 1914, *GSC*), and with it Sapir's apartments there. He was away on a five-month research trip to Alberni, Vancouver Island, studying the Nootka, doing the work he relished. The fire less deterred Sapir than it did accent his growing disenchantment with Ottawa. Sapir chaffed under his administrative responsibilities and recognized that the Survey held little regard for his field of study, a concern which would become more pronounced when wartime budgets ravaged support for his Division. Not two weeks prior, Boas contacted

Sapir about the latter's rumoured desire to leave his appointment as Chief Ethnologist for a position with an emphasis purely on research. For Boas, the decision was clear: "I believe that any step of this kind would be the mistake of your life" (Boas to Sapir, 2 January 1914, *GSC*: 1). Purity of research focus was not an option; he insisted Sapir continue to invest in the space of the museum, for there were no better alternatives.

Boas's letter provides insight into the relationship between anthropology and museum culture in this era, in addition to his investment in his (former) students. Museum work in the United States, he underscored, was far from the promised land Sapir imagined: "The fundamental difficulty that you would find everywhere is that all purely scientific work, particularly the work in which you are interested, would have to be done as a side issue, and that the essential interest of the museum is not exploration, but the exhibit, and ordinarily the popular exhibit" (1–2). A different museum role would neither relieve Sapir of administrative duties nor be more "sympathetic" to his work: "At present you are to a very great extent your own master" (2); "in any museum where you are under a curator, your freedom will be ever so much restricted[;] . . . no university position could give you the opportunities that you have at present time" (3). Certainly, Boas had his own interest in maintaining his pupil, so carefully placed, in Ottawa. It added to "the growing number of North American museums organized according to Boasian principles" (Darnell 2009: 63), and Boas doubted whether Sapir's successor would maintain that course. He worried that the continuity of Sapir's research would be disrupted and incline instead toward the popular exhibit: over four years, Sapir had "gone straight ahead according to what seemed to me sane and safe scientific principles, without yielding to the clamor of premature popularization, which is the bane of our science" (3). He recommended Sapir separate his personal and professional feelings: "I should consider it a misfortune for anthropology if you were to give up, because the organization of your work entails a certain amount of work that is irksome to you" (3). He did not want Sapir chasing this dream, since "no position in existence" would satisfy it: "In short, unless you are in a financial position to free yourself entirely from the conditions of remunerative positions, you cannot get rid of a certain amount of work that you dislike" (4). Sapir evidently relented, his dream of a pure research position deferred, but his disillusionment went unabated, and the onslaught of cutbacks and failed ventures over the ensuing years only fanned the flames.[52]

Sapir did endeavour to find other outlets. Though the absence of a university training centre contributed to anthropology's late development in Canada (Harrison and Darnell 2006: 4), it was not for Sapir's lack of effort. Later in 1914, he proposed a series of public lectures to encourage "university publics" to take an interest in anthropological work (Sapir to Falconer, 19 June 1914, *GSC*). The heads of McGill, University of Toronto, and Queen's were favourable to the idea, with Toronto's president Robert Falconer's being the most receptive (Sapir to McConnell, 13 October 1914, *GSC*). The series would have comprised the following seven lectures, with an option to combine the fourth and fifth into one ("Primitive Cultures"):

1. What is Anthropology? –Dr. E. Sapir
2. Early Man (lantern slides) – F. H. S. Knowles
3. Primitive Industries (lantern slides) – H. I. Smith
4. Primitive Society – C. M. Barbeau

[52] As Sapir feared, during wartime the Deputy Minister of Mines was "very eager to eliminate everything that [wa]s not absolutely indispensable for the work of the Survey" (Sapir to Mechling, 8 April 1916, *GSC*).

5. Primitive Religion – C. M. Barbeau
6. The languages of Primitive Peoples – Dr. E. Sapir
7. The Archaeology of Canada (lantern slides) – H. I. Smith

(Sapir to Falconer, 2 October 1914, *GSC*)

Sapir was willing to lecture for no salary, but in the end, neither Toronto nor the Survey could fund the $150 in travel expenses (Falconer to Sapir, 9 October 1914; Sapir to Falconer, 14 October 1914, *GSC*), and the plans for the lecture series went up in smoke. Neither the museum nor the university system in Canada was at this moment amenable to the extension of Sapir's network.

Sapir was not alone in feeling the diminishing returns of his profession. Indeed, it was a sentiment shared amongst other students of Boas, including Radin:

> I just received your letter and it made me think of the fact that we anthropologists, i.e. the four Semites who graduated under Boas, are either an unusual aggregation of men or a self-centered set who insist upon giving in to their intellectual whims whenever the spirit prompts them. I am inveighing against the tyranny of modern science which insists that you do original research and hard work, when it is so much better for your soul and your mind to lie on your back and gaze into a New Mexican sky, walk into mountains or, still better, read history of Greek and Latin, while Lowie until recently wanted to write and read philosophy and Goldie wanted to read books like Levy-Bruehl and Durkheim. Now come you with your composing and delight in modern literature!

(Radin to Sapir, 19 February 1916, *GSC*)

Radin ruminated over the remote possibility of writing an interpretive study of Winnebago or Ojibwe: "Whether such an undertaking would be of any permanent scientific value to the world I do not know, and I do not much care, but I know that it would be of permanent value to me and satisfy certain aesthetic cravings of mine that field-work threated for a time to dull." In contrast to the restrictive demands of professionalization, Radin appreciated the virtuosity of a wandering mind, a sentiment which Sapir shared. Disturbed by World War I and distraught over his wife Florence's illness, Sapir had turned to aesthetics; he dabbled in music and poetry to cathect his feelings of unbelonging and express his dissent toward militarism. Even after the war, this dissatisfaction remained. Sapir came under fire for a lecture he delivered for the Independent Labour Party in 1919, about which the *Ottawa Journal* ran the headline "A Preacher of Class War" (see Darnell 2009: 165–167). As a Polish-born, New York intellectual and Jew, Sapir was ever an outsider among the political class in Ottawa; his efforts to establish ties to the university and nation resisted, he could only mitigate the social and intellectual isolation he felt there, in pursuit of the scientific life.

. . .

11. **1915**. On Advocacy.

Diminishing returns extended to the realm of service, as well. During his trip to Alberni, Sapir was shocked by the government's neglect of the Natives' health. The Indian Act of 1876 established Native peoples as wards of the Canadian state, conferring upon the government the authority to regulate and manage their existence. Sapir appealed to Duncan Campbell Scott, then the Deputy Superintendent of the Department of Indian Affairs (DIA), to draw attention to the mediocre care of the doctor entrusted to their community. Sapir held no personal grudge against this man personally, who in his estimation was above average, but it did "not seem to [him] that the Indians [were] getting anything like the requisite attention in medical matters." Tuberculosis in particular afflicted this community, and Sapir judged they needed "active preventative superintendence" in order to curb its effects, especially on childhood mortality. Sapir was deeply affected by the death of the five-year-old sister of one of his informants; the doctor had arrived only on the fifth day after he was called, to confirm his prognosis over the phone that the case of tuberculosis was fatal from the beginning. The tribe rarely thought to summon the doctor, except in extreme cases, and due to his salary and the difficulty of the trip he seldom made visits.[53] For Sapir, these conditions were not a matter for debate: "It seems to me to be a matter merely of adequate service" (to Scott, 19 March 1914, *GSC*). This episode was not the only instance of Sapir's advocacy. Two months later, he contacted Scott again on behalf of the Iroquois at the Six Nations Reserve, concerning eleven treaty belts believed stolen from the Reserve. Following information from Dr. Frank Speck at the University of Pennsylvania, Sapir was convinced they were on exhibition there, under the private collection of George Heye, a trustee of the University Museum (Darnell 2009: 56). Having compared two sets of wampum belts, he was prepared to make an affidavit on their identity, and he hoped Scott would assist in the return of these belts to their rightful own, with Speck's role to remain confidential, given his connection to the university (to Scott, 16 May 1914, *GSC*). Sapir therefore took on the role as ally and advocate, a role particularly seen in the challenge to the Potlatch Ban he organized in 1915.

[53] The Department of Indian Affairs increasingly obstructed efforts of Indigenous communities to form their own associations or circumvent the Department's administrative control. One of these restrictions prohibited bands from hiring their own doctors or lawyers (Dyck 1991: 93).

The Potlatch Ban, which lasted from 1885 to 1951, prohibited the gift-giving ceremony practiced by Indigenous nations in the Pacific Northwest; the law was a means of administering colonial rule and disrupting non-European cultural practices, belonging to the legislative and moral regulation set into motion by the Indian Act.[54] Nayan Shah (2006) underscores how judicial trials often prove to be "flashpoints" for colonial regulation of intimacies, where intimacy becomes a legal category in addition to a personal one.[55] In this matter, Sapir was caught between the two registers. While he was living among the Nootka out west, they asked him to draft a petition against the renewed enforcement of this law, Sapir's having gained the people's trust: "you have seen all our customs performed and fully understand them" (Thomas to Sapir, 4 December 1914, *GSC*). He consulted Boas concerning the vigor with which the "old more or less dead letter potlatch law was being applied," with the hope that, through their intervention, it could be "applied with more discrimination." Sapir was planning a "semi popular bulletin" whose main purpose would be "making it clear to whites generally that the potlatch can not be summarily condemned even from the white man's standpoint, and that at any rate the abolition of it would mean the inflicting of a great deal of unnecessary hardship on the Indians, and could only assist in a general demoralization" (Sapir to Boas, 10 February 1915, *GSC*). He wrote to Boas, who had previously condemned the ban in 1897, and to others familiar with West Coast ethnology (John Swanton at the Bureau of American Ethnology; Charles Hill-Tout and Charles Newcombe in British Columbia) who could attest to "the harm and injustice that

---

[54] Potlatches were important for the redistribution of resources within tribes. The ban represented a challenge to Indigenous communalism, promoting liberal possessive individualism instead (Dyck 1991: 88).
[55] Under the purview of the 1876 Indian Act, for example, Natives could become enfranchised and cease to be "Indian" in the juridical sense (Mawani 2009: Chapter 5). The Act gave federal administration jurisdiction over who would count as "Indian" under the eyes of the law.

would be done to the Indians by a wholesale abolition of [the potlatch]" (to Newcombe, 10 February 1915, *GSC*). Sapir hoped that "a mass of expert opinion on the subject would carry weight with the Deputy Superintendent General [Scott]" (*ibid*). His petitions fell on deaf ears; scientific expertise, ultimately, could do little to sway the government's discriminatory policies. Sapir, again, found his sensibilities as ethnologist at odds with his allegiance as civil servant.

. . .

12. **1916.** On Standardization and the Stenographer.

The typewriter was not the only instrument of standardization. A report on the "Phonetic Transcription of Indian Languages" was published by the Smithsonian in 1916, following discussions that occurred at various meetings of the American Anthropological Association between 1913 and 1915. The report was an extension of the *Handbook*, providing directions for amateur linguists to help them contribute to data collection on endangered languages (Darnell 2009: 88). Boas was chair of the committee responsible for the report, which included Sapir, Kroeber, and Pliny Goddard. Its aim was to resolve inconsistent use of phonetic systems and establish a set mapping of sound to symbol in American ethnology. The first section described the possible vowel and consonant sounds produced by the speech organs through their different places and manners of articulation, recommending a series of characters, drawn from the Latin and Greek alphabets, to represent them. The second section provided an in-depth account of the system of diacritical marks and accents needed to supplement the transcription of American languages (for instance grave, acute, circumflex, or inverted circumflex accents for pitch). The object of the first section was to offer a "comparatively simple system of transcription adapted to the ordinary purposes of recording and printing texts" (2), with the latter included more for the benefit of specialists, whose hope was that one day "all of the extant North American languages may be discussed and compared" (2). However, for the ethnologist's task of compiling large volumes of texts, the more thorough mode of transcription was "expensive and often impracticable": "Such an elaborate system proves too complicated for students who are less thoroughly trained in phonetics and therefore less discriminating in their perceptions of sounds" (2). The goal here was to obtain large samples of vocabulary, from which later lexicographic work could be undertaken. As with the typewriter, though, efforts to replicate this standard were marked with differences.

Initially, Sapir agreed that some consensus on transcription practices was in order, though he was less doctrinaire about it than Boas, opting to allow a measure of space for the "judgement of the investigator" (to Boas, 9 December 1912, *GSC*). Sapir did not believe a universal standard was integral, so long as analyses were clear and internally consistent: "Phonetic symbols are not fetiches [*sic*]. They are merely symbols of sounds and when adequately explained in any set of texts or grammatical work, as they should always be, I do not see why there should be the least trouble in passing from one system to another" (to Mason, 5 March 1913, *GSC*). Furthermore, he considered an authoritative statement on the phonetics of American languages premature given the early stage of their knowledge-gathering project and the "interdependence of theory and orthography" (Darnell 2009: 90). Building consensus among the committee members, therefore, proved to be a challenge in and of itself. Sapir was satisfied with the second draft of the report, which Boas and Goddard also accepted, but Kroeber found his work too meticulous: "the report as it stands will be probably intelligible to and usable by only three or four anthropologists in the country besides yourself" (Kroeber to Sapir, 13 June 1914, *GSC*). Radin's correspondence with Sapir around this time reveals the fault lines beginning to form between ethnologists and linguistic specialists:

> [Nels Nelson] tells me that Boas, Goddard and Kroeber, especially the two latter are kicking strongly at your phonetic report because it is too minute and would demand too many changes. They make me sick. They wish to be phoneticians and object to the one essential thing about the subject, namely the recording of all those peculiarities of sounds that are not of a personal nature. My advice would be, not to pay any attention to them, for after all Americanists must realize now that they should cater to those scholars who are inevitably going to be drawn to the subject of American phonetics and linguistics within the next generation, and who are not unnaturally going to demand the same standards, to which they are accustomed in Indo-European philology. Am I right?
>
> (Radin to Sapir, 28 July 1914, *GSC*)

The report was a junction (and disjunction) in shifting and adapting standards from a text-based philological tradition to the field, in addition to signaling the growing territoriality between linguists and ethnologists. Ultimately, Sapir grew tired of the disputes the document evoked, satisfied with his own contribution, and a third draft of the report was approved for publication by the other three authors (Darnell 2009: 90–91).

Even after its approval, the report remained a volatile implement. Sapir proposed that the Survey make use of it as a guide for their publications, recommending copies be made and distributed to their anthropology mailing list at a cost of $75–100 for 750–1000 prints; or, per Boas's recommendation, they might cooperate with Columbia University and the Bureau of American Ethnology to furnish another 2000 copies (Sapir to McInnes, 10 October 1916, *GSC*). The expenditure was "not only highly advisable but necessary," he insisted (Sapir to McInnes, 13 November 1916, *GSC*), but nonetheless, they would have to settle for a paltry 100 copies, which was "better than nothing" (Sapir to Boas, 5 December 1916, *GSC*). The costs of extending a standard were never final, nor were its guidelines followed with fidelity. Given the fluid practice of calibrating speech sounds to phonetic script, the report was no substitute for linguistic training, and its recommendations were subject to *ad hoc* modification. Mason, in response to Sapir's review of his work on the Salinan language ("you took me over my head on the first line"), expressed his dismay at these inconsistencies: "How in the hell do you ever expect to have a uniform system of orthography adopted if all the signatory formulators refuse to adopt and use half of their own recommendations?" (Mason to Sapir, 26 July 1916, *GSC*). Of course, Mason himself wasted no time making his own adjustments:

> With regard to my other orthographical problems, in most cases I have followed your advice, but have adopted some other deviations which will probably make you shake your fists and tear your hair. . . . The one point where we have departed radically from the committee's recommendations is in regard to the open and close vowels. It seems foolish, in a language where at least 90% of the vowels are open to restrict the use of the ordinary roman characters to the close quality and use the greek characters for by far the greater number of cases merely because that is the system used in discussions of phonetics. We are therefore using the ordinary roman symbols

> for the open quality, and the greek vowels, which are probably less than 5% of the total, for the close.
>
> (Mason to Sapir, 14 September 1916, *GSC*)

Systematicity was the ambition that drove the committee to introduce the report, though the standard they sought to instill depended not on maxims alone; the directions had to be followed, and when new data presented a challenge or simply when it was inconvenient or costly to reproduce, that standard could be flouted.

The standard depended also on the typewriter, whose presence was a pre-condition to the belief that the report's directions were reproducible. The artifact brought into reach the correlate goal of the standard: to render the languages of North America more familiar yet to maintain their strangeness. The typewriter's materiality, I've mentioned, was not fixed within this arrangement, and neither were the hands that employed it. Along with the proliferation of the typewriter, Kittler (1999) reminds us, was the advent of the typist: where men wove the figurative tissue of the text, women wove the literal (186). While male ethnologists debated the finer points of orthography, it often fell to female stenographers to perform the act of transcription. The Anthropological Division was no exception, having hired on two stenographers as clerical staff in 1913: Frances Bleakney and Ariel McConnell. Where McConnell aided the archaeologists Smith and William Wintemberg, Bleakney was assigned to assist in the preparation of ethnological and linguistic material and hence worked closely with Sapir. Not long after the publication of the phonetic report, Sapir petitioned Director McInnes to promote her to the appropriate position and pay scale, in recognition of her many accolades (McConnell's promotion having gone through before the war). Bleakney had passed first on the civil service examination (class II B), "outdistancing male and female competitors by a comfortable margin"; her work transcribing texts on the "Indian typewriter," with its distinct keyboard, was moreover "of a highly technical nature—in [his] opinion, probably the most severely technical of any undertaken by the Survey's clerical staff" (Sapir to McInnes, 11 October 1916, *GSC*).[56] Bleakney was frequently overworked, such that Sapir had to request a stenographer from another division, "Miss" Holcombe, be transferred to break up the workload and assist Waugh (to McInnes, 17 July 1917, *GSC*). A lack of stenographic aid continued to hamper the scientific work of the office during the war: with McConnell otherwise occupied with the archaeologists, Bleakney alone was responsible for assisting the four other permanent staff of the Division (Barbeau, Knowles, Waugh, and Sapir), leaving an excess backlog of office work; Sapir suggested another stenographer be hired under Bleakney's supervision, due to her position's "unusually technical nature" and given that she was likely to resign from it soon (to McInnes, 20 November 1918, *GSC*). In *Queer Phenomenology*, Ahmed (2006) interrogates how the philosophical tradition relies on the writing table and the gendered division of work spaces invested in such platforms and then forgotten in publication; she argues however that, in moments of failed extension or "non-use" (49), subjects can become conscious of these relations and forge new understandings of objects. The Division

---

[56] Full responsibilities were outlined in the Stenographic Section of the Clerical Division:

> 1. Section undertakes dictation and typing of all letters, and the typing of all manuscript copy, required by Anthropological Division.
> 2. Dictation and typing of all letters and the typing of minor manuscript copy (less than 5 pages of typewritten manuscript—foolscap size), undertaken without Requisition.
> 3. Manuscript of Memoirs and other work of similar nature accepted for copying by the Head Stenographer on presentation of a Requisition signed by the author and countersigned by his Chief of Division.
> 4. All requests for stenographic assistance must be made to Head Stenographer.
> 5. HS will provide a Stenographer or Clerk, on presentation of Authorized Requisition, for work done in office requiring clerical assistance.
>
> (Sapir to Brock, 27 July 1914, *GSC*)

experienced such a failure of extension during this period of wartime cutbacks, and through its disorientation other relations come into view. The typewriter emerged as a tool both for the reproduction of a standard as for reproducing a gendered division of office work, but from the failure of these expected operations grew a recognition of the stenographer's important work, upon which the Division relied. The labour of these stenographers, whose diligent hands are omitted from histories of linguistics in favour of the phonetician's deft ears, were nonetheless instrumental in bringing their objectives closer to fruition.

. . .

13. **1916**. On the Transit of Time Perspective.

From the beginning of his time with the Survey, Sapir was occupied with comparative schemes. He early on presented new data on the linguistic relationship between Kwakiutl and Nootka (1910 Annual Summary Report, *GSC*), but the effort of reconstructing an unattested language akin to Proto–Indo-European was beyond the scope of one man, even one as talented as Sapir. In the nineteenth century, comparative philology succeeded in part because Indo-European already possessed vast corpora of written records. In North America, the task of linguistic reconstruction required first the migration of orature into analyzable text—a consolidated effort on the part of this network of scholars, fieldworkers, informants, and stenographers, with their tools and standards—which would take years. Sapir was invested in the extension of comparative linguistics to American languages, and he shared his eagerness to reconstruct a distant prehistory of Indigenous peoples with his fellow ethnologists, but the latter were not always amenable to these linguistic methods. Sapir observed that, where Boas's interests in ethnology were predominately historical, his interest in linguistics was the opposite: "merely descriptive or 'psychological,' hardly at all historical or reconstructive" (to Radin, 3 March 1913, *GSC*). Between 1913 and 1920, comparative work consumed Sapir's attention (Darnell 2009: 108), culminating in his sketch of the proto-language "Na-Dene" (1915), his instructional essay on "Time Perspective" (1916), and his reduction of North American language families to six super-stocks in "A Bird's Eye View of American Languages North of Mexico" (1921).[57] The wartime slump, coupled with the loss of the Victoria Memorial Museum Building to Parliament, delayed the lateral movements of the Division's collection and display activities; these conditions also encouraged Sapir's attention to drift to the linguistic material gathered, resuming his speculations and taking lines of flight into the deep past.

In addition to the five geographic areas he instituted, Sapir wanted to develop a relative chronology based on linguistic, cultural, and archaeological evidence for the transit of the First Peoples of North America. Buoyed by Kroeber's announcement of two large language families in California (Dixon & Kroeber 1913), and its becoming "daily clear that there is an Uto-Aztecan unity as sure as Indo-Germanic,"[58] Sapir tasked his colleagues to prove more relationships between "so-called independent linguistic stocks" (to Radin, 3 March 1913, *GSC*); he called for a reduction of Powell's taxonomy or, as he once called it, the "slaughter of linguistic families" (to Radin, 18 July 1913, *GSC*). Powell's classification of 58 stocks was insufficient for the work of linguistic reconstruction, but it had its supporters among Boasians and at the Bureau of American Ethnology (Darnell 2009: 115). Sapir was

[57] "A Bird's Eye View" was a one-page summary based on the talk he delivered at the American Association for the Advancement of Science in 1920. A full version appeared in *Encyclopaedia Britannica* (1929a).
[58] Sapir's "reconstruction of Proto–Uto-Aztecan . . . in 1913 was the first systematic application of Indo-European methods to a North American language family" (Darnell 2009: 112).

sensible to this resistance, recognizing that Boas opposed the establishment of "larger linguistic groups of a genetic character" and preferred, in accordance with Powell, to believe in an "almost unlimited number of distinct stocks" (to Radin, 10 June 1913, *GSC*). Sapir postulated that more Indigenous languages were related than he could ever credibly demonstrate: "Language is conservative, of course, but conservatism is only a matter of degree, not an absolute fact" (to Radin, 10 June 1913, *GSC*). However, knowing the burden of proof was on their end, Sapir proceeded carefully in his hypotheses and advised his collaborators to be similarly circumspect.[59] When Radin, who was studying the languages of Mexico, presented evidence of a relationship between Otomi and Mixtec-Zapotec based solely on comparative vocabularies, Sapir urged him to look further, exploring their morphological characteristics as well: "While remotely related languages may have diverged very considerably in morphological respects, still, if they are related at all, fundamental points of comparison can generally be found"; Sapir himself was "reluctant to embark on lexical comparisons before [he had] justified this procedure to [him]self by ascertaining the presence of such fundamental morphological similarities, even if they are rather vague in character"(to Radin, 18 July 1913, *GSC*). Sapir's own research on the possible linguistic affiliation among Athabascan, Haida, and Tlingit, thence considered independent stocks, followed from these principles. His (1915) article introducing "Na-

[59] For example, Sapir was cautious about introducing his evidence that the languages spoken by the Yurok and Wishosk [Wiyot], in California, were related to the distant Algonquian: "I have found some astonishing points of contact, lexically and morphologically, between these, particularly Wishosk, and another well known linguistic stock, which I shall not mention at present because I am certain to be laughed at for my pains. . . . Suffice it to say that I am either on the wildest goose chase I have ever indulged in, or else about to land something decidedly revolutionary" (to Speck, 18 July 1913, *GSC*). He confided more in Radin, in a letter sent off the same day: "Please treat this information as strictly confidential, as, should it turn out that I have been hasty, I will present a rather sorry picture" (to Radin, 18 July 1913, *GSC*). The hypothesis would put him into conflict with his contemporary Truman Michelson, a comparativist at the Bureau of American Ethnology.

Dene" presented detailed evidence of their likely genetic relationship based on systematic comparisons of their morphology, vocabulary, and phonology.[60]

The realization of the Na-Dene proto-language was not a momentary insight, but the work of years. As with other ventures, Sapir employed his fieldworkers as extensions of his apparatus and drew on published material from his network of scholars (for example Boas's work on Athabascan, for which the latter would disapprove). The process of gathering evidence began with early efforts to classify Athabascan tribes for the Survey: "my primary idea was to base this classification on linguistic evidence as deduced from fairly extensive vocabularies and test grammatical questions, and on information obtained by direct enquiry as to the exact tribal boundaries of the various groups of Indians you might come in contact with or hear about" (Sapir to Teit, 21 December 1912, *GSC*). The potential affiliation, however, remained on Sapir's mind: "I have been rummaging around of late in one or two problems of linguistic relationships. You already know that I have pretty substantial evidence to show that Athabascan, Haida, and Tlingit are genetically related" (Sapir to Speck, 18 July 1913, *GSC*). During his 1914 fieldwork, Sapir found a moment to investigate the matter for himself, putting his fine-tuned ear to work where his proxies had failed him: "I was very proud to learn that Tlingit proves as you [Radin] say to have a well-marked development of tones, as in the hour or so that I spent with Shortridge I succeeded in determining this matter as a problem to be worked out. It seems astonishing

---

[60] Sapir tried to preserve the Native point-of-view with this name: "The name that I have chosen for the stock, Na-dene, may be justified by reference to no. 51 of the comparative vocabulary. 'Dene,' in various dialectic forms, is a wide-spread Athabaskan term for 'person, people'; the element *-*ne* (*-*n*, *-*ŋ*) which forms part of it is an old stem for 'person, people' which, as suffix or prefix, is frequently used in Athabaskan in that sense. It is cognate with H[aida] *na* 'to dwell; house' and T[lingit] *na* 'people.' The compound term 'Na-dene' thus designates by means of native stems the speakers of the three languages concerned, besides continuing the use of the old term Dene for the Athabaskan branch of the stock" (1915: 558).

that Swanton could ever have missed it. . . . It is somewhat irritating to find that so much of our linguistic work has to be done over again all the time" (to Radin, 28 November 1914, *GSC*). Confident in the features of these languages, Sapir more readily asserted a potential genealogy of the Na-Dene family:

> Tlingit and Athabascan are related in far more than general morphological respects, as I shall show in a rather elaborate paper that I am now preparing. It seems clear to me now that Haida, though undoubtedly related to both Tlingit and Athabascan, stands rather on a side, having developed special peculiarities of its own. In spite of this, however, I believe that it comes rather nearer the original phonetic system characteristic of the mother tongue of the three groups than either Tlingit or Athabascan.
>
> (Sapir to Radin, 28 November 1914, *GSC*)

Linguistic reconstruction thus served as a tool to read the movements of people across time and space, here suggesting the historical divergence of Na-Dene language groups. These techniques were yet rarefied within a North American ethnological context—indeed, their veracity was mistrusted by anthropologists like Boas who lacked training in Indo-European methods, and so Sapir decided to commit these principles of historical reconstruction to print in his paper on "Time Perspective" in 1916.[61]

[61] Ironically, the science of language dedicated to the tracing of movement confronted its own crisis of translation. Sapir complained of the "slovenly manner" of the French reprint of "Time Perspective": it was "perfectly clear to me as I read along that violence was being done by the translator to the genius of the French language and, more important than this, that the meaning of the original version had in many instances been completely missed" (Sapir to McConnell, 19 July 1917, *GSC*). However, it was impossible within their budget to hire a permanent translating staff (McConnell to Sapir, 2 August 1917, *GSC*).

Reconstruction was not limited to the purview of linguistics but was a mode of inquiry that incorporated an array of differential evidence. In "Time Perspective," Sapir sought to codify its variegated forms in addition to eking out a place for linguistic reconstruction as an indispensable tool of historical science.[62] Here, Sapir rejected the inclination within "folk psychology" to treat Indigenous peoples as a blank canvass on which to project generalized statements about human nature, or to see them as "less encumbered by secondary or untypical developments" (1), merely because they lacked the written documentation of their Western counterparts. The question, for anthropologists, became how to derive a relative chronology from the jumble of historical information they assembled; an absolute chronology, with certain dates, was unlikely, given the constraints of the material (3). For them, it was therefore a problem of translating a "two-dimensional photographic picture of reality into the three-dimensional picture which lies back of it" (2). Was it even "possible to read time perspective into the flat surface of American culture?" In Sapir's view, the history of Native cultures should not be envisioned as static in the way the metaphor of the photograph suggested; it was comparable, rather, to a "long-exposure star chart," where "immensities of space are indeed reduced to a flat, but in which the extent and direction of movement of nearer bodies, the planets, are betrayed by short lines" (8). The "nearer bodies," in this case, manifested in the different methodologies invoked to draw these lines.

[62] Sapir forwarded a draft to Clark Wissler, Curator of Anthropology at the American Museum of Natural History, to get a sense of its reception: "I am particularly impressed with the linguistic part of your paper which seems to me the strongest and most important contribution. . . . Personally, I should very much like to see linguistic studies demonstrate their usefulness in the solution of more general problems. As it stands now, it is extremely difficult to convince an outsider that linguistic work leads to anything outside of its own special problems" (Wissler to Sapir, 9 February 1916, *GSC*).

Sapir gathered evidence into two categories: the direct, which overtly suggested temporal relations, and the indirect, from which temporal sequence might be inferred (5). Direct included documentary evidence from the past 400 years of contact, Native testimony through tribal history regarding the relative sequence of events, or stratified archaeological relics (strictly speaking inferential, but reliable enough to be considered direct, he noted). Indirect evidence was drawn from the other three subfields of Boasian analysis—physical anthropology, cultural ethnology, and linguistics—as well as geographic distribution, and each was accompanied by Sapir's cautions. Physical evidence, for example via skeletal remains, could evoke population density and, with corollary proof, suggest the length of stay in a given region. Ethnological evidence depended on the notion of "cultural seriation": simpler cultural elements that belonged to several tribes were often interpreted as having a greater age than the more complex (for example, a simple carved totem versus more elaborate poles). Additionally, the number of associations accumulated around cultural elements could also help determine their relative age (for example, the more entrenched an association, likely the older the cultural element in question). Attention to geographic distribution traced the diffusion of cultural elements with more accuracy, but geographic contiguity itself (for instance, the five cultural areas of Canada) was descriptive and not necessarily historical in character. Finally, linguistic evidence could help to date cultural elements through comparative grammar and vocabulary, especially place names, and the geography of linguistic stocks. Sapir contended that language was less susceptible to the looping effects that influenced ethnological data, since language was a "more unified" complex than culture, lacking the "disturbing force of rationalization" which "distorts" the latter (52). Since language change occurred at a slower pace than cultural and was less likely to enter into the "field of consciousness," it proved to be the more reliable: language was the "most perfectly self-contained" human system (53). Reflecting later on the outcomes of historical science, Sapir wrote: "whereas in natural sciences, . . . the value of phenomena lies in that they lead to general concepts or 'laws', in the historical sciences 'laws' have whatever value they possess because they proceed from phenomena." Historical science dealt with "more or less arbitrarily selected subject matter" so the "causal nexus of things can not be rigidly followed out" (to Lowie, 23 July 1917, *GSC*). In "Time Perspective," Sapir presented a kaleidoscopic approach for determining these laws, but linguistic reconstruction, in his mind, was the methodology best equipped to mitigate circumstantial factors and bring close these distant movements. Linguistics was a historical science in its discovery of the "laws" of language change, but its methods were more akin to those of the natural sciences.

Despite the care and clarity through which he presented his thoughts, Sapir's dream of popularizing historical linguistic methods was deeply unpopular among some of his colleagues. Radin had proved to be a trusted confidante and sounding board on this subject, but that trust would reach its limits when Radin's overzealous efforts would compromise Sapir's campaign and ultimately provoke his ire. From the outset, Sapir had to check his friend's enthusiasm; the latter had early on held the belief that the languages of Mexico were all related, which would have been "the most epoch-making generalization yet made in American linguistics" if it could be proved true (Sapir to Radin, 28 November 1914, *GSC*). Sapir had his doubts and presented contrary evidence on the matter, but Radin was yet determined to make his mark: "As you surmised, I take the greatest pride in my introduction and the speculative-historical method used. While I fully realize the danger inherent in such a method, I think that when developed in connection with certain concrete data as I have done, it is likely to be more suggestive and of greater value than general theoretical discussions a la Goldenweiser and Lowie" (Radin to Sapir, 23 December 1914, *GSC*). In 1919, Radin published a paper that proposed all Indigenous languages of North America were related through twelve linguistic stocks, a premature declaration based on dodgy evidence that endangered Sapir's assiduous work (but probably led to his clarification of six super-stocks in 1921).[63]

This misstep coincided with a period of growing dissent between Sapir and Boas on the subject of genetic relationships and the place of linguistic reconstruction therein. Not long before Radin's controversial publication, Sapir was previewing his evidence for the relationship between Tsimshian and Chinook, two Indigenous languages of the Pacific Northwest (to Boas, 19 July 1918, *GSC*). The presentation fell on deaf ears, for Boas was immediately resistant to the hypothesis, believing a diffusionist interpretation just as likely as the "assumption of genetic differentiation," in which they descended from the same stock (to Sapir, 22 July 1918, *GSC*).[64] Sapir aired his frustrations in a letter to Boas:

> Linguistic differentiation takes place in time, not in space, and there is no end of opportunity for all kinds of very special dialectic developments. . . . I must confess that I have always had a feeling that you entirely overdo psychological peculiarities in different languages as presenting insuperable obstacles to genetic theories, and that, on the other hand, you are not

[63] For more on the "Radian fiasco," see Darnell (2009: 118–121).

[64] For more on the difference between diffusionist and genetic approaches, see Darnell (2001: 51–61). In essence, Boas's training in geography placed an emphasis on spatial distribution (with such mechanisms as diffusion and migration) in order to reconstruct temporal lines, whereas Sapir's philological training was the inverse, preferring to establish genetic relationships through the suggestion of time depth.

> specially impressed by the reality of the differentiating processes, phonetic and grammatical, that have so greatly operated in linguistic history all over the world.
>
> (Sapir to Boas, 26 July 1918, *GSC*)

The disagreement reached its apogee when Boas singled out Sapir, Kroeber, Dixon, and "particularly Radin" in an article for *American Anthropologist* (1920), expressing his renewed skepticism toward linguistic reconstruction. About that article, Sapir reflected: "his whole approach is so different from mine and from that of the vast majority of linguistic students that the attempt to argue about the theoretical basis can only result in mutual irritation. . . . His wholesale use of the idea of diffusion must also strike anyone that has had real experience with the brass tacks of linguistic history as rather absurd" (to Lowie, 15 February 1921, *GSC*). Sapir continued to speculate upon historical linguistics. Of note was his dizzying dream of finding comparisons between Na-Dene and Indo-Chinese: "evidence accumulates so fast that it is hard to sit down and give an idea" (Sapir to Kroeber, 21 October 1921, *ALK*). However, with the battleground so thorny, and his allies' contribution so unreliable, it became a dream he kept to himself, out of reach of those he worried would misapprehend it.

. . .

14. **1918.** On Economies of Authenticity.

With their wartime budget meted out so frugally and the display hall still absorbed by legislature, the Anthropological Division had to economize, in various ways, to endure these privations. At first, there was an economy of space: the floor adjacent to the Museum drafting room had to be partitioned into offices, which cut into storage facilities (Sapir to

McConnell, 25 January 1915, *GSC*). These conditions only worsened over time: "We are getting terribly cramped for room in our anthropological hall, which, owing to the [Centre Block] fire, now includes exhibition space, offices, and storage both for our own department and for one or two others" (Sapir to Speck, 23 November 1916, *GSC*). Following this constriction was a litany of unfilled requisitions: among them, five sections of bookcases ("badly needed"), one paper cutter ("badly needed"), a telephone in the Hall of Canadian Anthropology ("badly needed"), two steel filing cabinets, one steel cabinet with six draws for filing photographs, needed as soon as possible (Sapir to McInnes, 10 November 1916, *GSC*). The stringent allowance of paper was particularly significant, requiring the Division to find other venues to promote their work. Sapir reached out to Clark Wissler, at the American Museum of Natural History, to print their anthropological research: "The war has cut right and left into all expenditures of a non-economic nature" (Sapir to Wissler, 25 January 1918, *GSC*). Sapir had secured consent from the Deputy Minister to transfer publication of scientific manuscripts to other institutions, provided the Survey received proper credit. The reply was negative: "I fear we are not much better off, for the cost of printing has risen more than fifty percent which alone cuts down our output very materially" (Wissler to Sapir, 28 January 1918, *GSC*). Anthropology everywhere was made to retrench.

Museum activities did not come to a complete halt in Ottawa. Besides truncated collection work, the staff provided a special exhibit on Indigenous handicrafts for the Central Canadian Exhibition (1917 Annual Summary Report, *GSC*), an annual fair which began in 1888. This project inspired Sapir to associate the Division with a more lucrative, and hence impactful, branch of the government. Late in 1917, he proposed to Duncan

Campbell Scott that the Department of Indian Affairs might encourage Native industries within Canada, putting an emphasis on their disappearing handicrafts. The endeavour would dovetail with the interests of anthropological research, maintaining a line of traditional knowledge among younger generations: progressive contact with European culture diminished interest in "what is relatively crude or barbaric in their culture, but also for what is of distinct merit and worthy of preservation," Sapir asserted. He conceived that schools could be run under the direction of elderly men and women from the tribes: "nothing would tickle an old basket maker more than to receive an impression that an order had gone out from Government headquarters to some school in the neighborhood to the effect that she was to teach the girls of her own tribe the details of an art that she loves and knows so well." It would be a practical endeavour and "heartening from a moral standpoint," which he hoped Scott would not dismiss as "mere sentimentality," and also contribute to a "distinctively Canadian industrial growth." There was already a large market for these wares, and Sapir saw no reason why handicrafts could not be "rescued" from the "category of mere tourists' curios and raised to that of industrially and aesthetically desirable objects" (to Scott, 20 December, 1917, *GSC*). Smith had already taken an interest in the pedagogical and commercial aspects of anthropological work.

In their comparison of U.S. and Canadian commodity culture, Kathy M'Closkey and Kevin Manuel (2006) examine efforts to commercialize Indigenous handicrafts across the twentieth century, arguing that this work of inclusion precipitated efforts to assimilate First Peoples into a nationalist project. They locate such "commercial othering" (227)[65]

[65] What Sarah Ahmed (2000) calls "stranger fetishism": "consumer culture is one site in which becoming other is offered to Western subjects through the commodity form ('stranger fetishism'). Such commodities are assumed *to contain* the difference of stranger culture" (125; emphasis in original).

within Canada's emerging tourism industry which began after the First World War and flourished mid-century. Certainly, Sapir's proposal anticipated these ventures, but economies had always been crucial to the work of the Anthropological Division. In addition to paid informants, their research depended on a circuit of production and exchange of cultural artifacts. Chief Gibson's initial attachment to the Survey had been the collection of Iroquois cultural specimens: he had found "a real one piece pine canoe" for one or two people, painted red, and a hundred-year-old canoe with a crack at the bottom that could be fixed (Gibson to Sapir, 18 September 1911, *GSC*), for which Sapir was happy to remunerate. Out of these transactions, authenticity soon became subject to the laws of supply and demand: "it is very hard to get the old relics but we bought some old ones and some made by order new" (Gibson to Sapir, 26 September 1911, *GSC*). The Division was satisfied to commission what it could not find, within an economy that transmuted the new into the old. Sapir sought to acquire other such new old-fashioned wares: for example, "an old-type Nootka canoe" for $30.00 (to McConnell, 18 December 1914, *GSC*), which was preferable to new canoes that had undesirable modern features such as nails and oarlocks. As objects of science, these specimens held "no commercial value" (Sapir to Speck, 23 July 1913, *GSC*),[66] yet their circulation relied nonetheless upon an assignation of a price forgotten except on the accountant's ledger. The salvage paradigm depended on the attribution of authenticity through intimate relations with the distant past. However, where intimacy failed, economy filled the breach.

Discussion of the handicrafts initiative intensified in 1918. Scott forwarded Sapir a letter from Thomas Deasy, an Indian Agent in British Columbia, who wanted to take charge

---

[66] This statement was made in the context of delivering artifacts across customs.

of the business, provided it received the DIA's support: "If the Department decides to assist Indians to manufacture basketry, totem poles, leather and fur work, and provides them means of continuing work in winter months, I would be pleased to have charge of this work." Deasy advised that a large city like Vancouver or Toronto would make an ideal site. The business itself required an investment of $5000, which would cover 500 baskets, 200 pieces of black slate carvings, and the purchase of Reverend G.H. Haley's collection of "curios"—all of which would be sold at a profit, Deasy assured. He compared the prospects of such an industry to the situation in Alaska, which benefited from similar merchandising, especially during the summer months from tourism along the Pacific Coast. Deasy openly endorsed their exploitative model, citing how dealers in Alaska paid the Haida little for totem poles and basketry but made significant revenue. He concluded: "A store, of the kind proposed, would also be a 'museum'" (Deasy to Scott [cc: Sapir], 4 May 1918, *GSC*). Sapir disagreed, perceiving that Deasy was more invested in the "disposition of private collections" than in the "encouragement of native industries": "I should not imagine, however, judging from the general tone of his letter, that he would be as good a man for the general management of this work as Mr. Teit" (to Scott, 18 May 1918, *GSC*). The latter of whom was a more "suitable and energetic man" (Sapir to Teit, 21 March 1918, *GSC*), knowledgeable about handicrafts and popular with the local population. The handicrafts venture appeared to flounder, taken up again only in 1927 when Barbeau co-conducted another exhibit (M'Closkey & Manuel, 231). Sapir's efforts to combine science and industry approached realization, but the two never met.[67] Scientific integrity proved to have too poor of an exchange rate.

[67] In the meantime, Smith spearheaded a less ambitious project of creating prints based on Indigenous design:

. . .

❖ Retraction (the Denouement): 1919–1924

15. **1920**. On Genteel Spies.

Economy was not the only factor torqueing the work of the Division. Throughout his appointment in Ottawa, Sapir had to negotiate his scientific identity within the margins of civil service, leveraging a belief in the purity of scientific research against the realities of being employed as a state scientist. The providence of the Geological Survey both enabled and constrained what he could and could not say: he had to be reminded, for instance, that "public addresses by officers of the Department" should meet the approval of the Minister of Mines first (McInnes to Sapir, 21 March 1918, *GSC*). While attached to the Dominion Government, Sapir was also put into contact with a culture stranger than any he would encounter in the field: bureaucracy. The dissonance resounded most powerfully in 1920, when a scientific monograph that Barbeau had prepared on the Hurons of Lorette, Quebec, was pulled into House politics, precipitating a crisis in the Division members' status as civil servants and their self-perception as scientists.

This controversy encompassed not what was said, but how it was repeated. On 23 June 1920, in a debate over assimilationist policy in the House of Commons, Barbeau's report was quoted out of context in support of an argument for "compulsory

---

> Smith tried to prove that we could help pay the Canadian war debt by commercially utilizing prehistoric Indian designs. As a result of much discussion and voluminous correspondence he has prepared an album of prehistoric Canadian designs which is intended to serve as an artistic and industrial stimulus to Canadian designers and manufacturers. This album will probably be the first publication of our anthropological series to break our long silence.
> (Sapir to Mechling, 15 July 1919, *GSC*)

The work was later published as a Museum Bulletin entitled "An Album of Prehistoric Canadian Art" (Smith 1923), after wartime restrictions had abated. In Sapir's preface, he spoke to the potential of this collection for "invigorating" Western art, which had "so frequently been degraded to lifeless clichés" (lii). Scientists were to blame, he intimated, for neglecting the industrial potential of this art in favour of its scientific virtues.

enfranchisement." Sapir heard from Teit that "this evidence had been quoted in the House as typical of Indian reserves in general" when it applied strictly to the situation in Lorette. Though Sapir could only find one passage cited in the official transcripts, and it did "not go as far as [he] had thought," the impression it "undoubtedly" created "could not but be prejudicial to the Indians." [68] He continued: "I understand from Teit that there were other, more misleading references to your report, but he has not been able to find them for me as yet. Under the circumstances I do not think that I shall make a formal protest to either the Director of the Geological Survey or Mr. Scott himself" (Sapir to Barbeau, 16 July 1920, *GSC*). Even though Sapir declined to lodge an official complaint, the consequences of this breach of trust reverberated in unofficial circuits:

> All the same I wish once more to make it perfectly clear that there are to be no communications touching Indian affairs sent to the Department of Indian Affairs without the consent of the proper authorities within the Geological Survey, nor is any money to be accepted from the Department of Indian Affairs except with the express permission of these same authorities. I hate to

---

[68] Sapir attached a copy of the transcript for Barbeau. John Harold is the speaker:

> 'Although in most respects the Lorette half-breeds have been Europeanized, the fact that they do not enjoy the rights and duties of citizenship in many cases dwarfs their moral sense and feeling of responsibility. An undue prolongation of such tutelage leads to mendacity and other vices. Many of the best Lorette people, besides, chafe under the restrictions and humiliation resulting from their being officially treated as "savages."
>
> Those gifted with initiative, who want to start in business, find themselves hampered by their legal status. As long as a Huron lives exclusively on the reserve he has no existence in the eyes of banking and business concerns; for he is exactly in the position of persons under age. The bank manager, Notaire Cyrille Renaud, said to us: "Although they may be reliable and have money or property, they are unexistent when they live on the reserve."
>
> According to many statements, real property on the reserve is reduced to about one-third of less of its normal value, if compared with property situated in the immediate vicinity. As on the reserve, the owner may sell only to another member of the band, and as there is very little demand for more property, the price of purchase is very low.'
>
> (Barbeau qtd. in House of Commons Debate, 23 June 1920 [Sapir to Barbeau, 16 July 1920, *GSC*])

> make this rule explicit, but I am afraid that if we do not follow it very literally, we will find ourselves drifting into the position of genteel spies for the Department of Indian Affairs. We cannot afford to be misunderstood by any Indians in Canada.
>
> (Sapir to Barbeau, 16 July 1920, *GSC*)

The threat of perceived collusion, or indeed the actual occurrence of it, shook Sapir's faith in the office; he was straining against the seams of civility.[69] The same department whose alliance he'd solicited had turned against him. Sapir's hope of disentangling scientific research from the political structure upon which it depended, a balancing act requiring careful spacing, had abruptly collapsed.

. . .

16. **1921**. On the Book *Language*

Sapir was not an ardent proponent of standardization. He acquired knowledge of linguistics and anthropology within an apprenticeship model and, as we've seen, communicated his expertise through similarly private networks, leaving open a space for his pupils to strike out their own path. Kroeber chastised him for this very attitude: "The decadence of linguistics is largely your own fault. You're an individualist and haven't built up a school. Do something general in character" (to Sapir, 4 November 1917, *GSC*). Amidst

[69] Despite misgivings between their departments and other philosophical disagreements, Scott and Sapir maintained a civil correspondence, each belonging to the belletristic milieu of the gentleman-scholar: on various occasions, Sapir passed manuscripts of his poetry to Scott, which he read with pleasure (Scott to Sapir, 15 April 1918; 7 May 1919; 12 January 1922, *GSC*). Sapir also commented on an early production of the Carroll Aikins play *The God of Gods*, at Scott's behest; Sapir complimented the play for its literary merits, but was highly critical of its haphazard presentation of Indigeneity: "the proper thing to do is not to trim the play to a supposed Indian background but to throw all cultural realism ruthlessly to the winds. Mr. Aikins will not succeed, without much thankless labor, in making his play Indian; if he tries too hard, he will spoil his conception. What he should do is to remove every word and reference that points to a specific primitive background" (Sapir to Scott, 22 January 1922, *GSC*).

the growing disparity between himself and other Boasians on subjects linguistic, Sapir perhaps began to recognize the truth of Kroeber's assertion and the value of a disciplinary base, even while he remained committed to an interdisciplinary synthesis that characterized much of his work, at this and later stages of his career. He reflected on the importance of having fieldworkers trained specifically in linguistics:

> Secondly, you are quite right not to want to burden the ethnologists with linguistic work on the side, except, of course, where they work in out of the way places from which data are sparse. The majority of men not specially trained in language do poorly at this kind of work. Almost invariably the really essential things are missed. A good linguist can find out more, along certain lines, in five hours' honest work than the average ethnologist in six months of weary questioning.
>
> (Sapir to Wissler, 3 October 1920, *GSC*)[70]

Without belabouring the cliché: a good linguist was hard to find. Sapir set out to rectify this dearth and disseminate the methods of analysis he had cultivated and purveyed through

[70] The earlier part of this paragraph elucidates Sapir's view on the role of fieldwork within linguistics:

> My own experience in such matters is that an overhauling at first hand of work done by others is apt to be unexpectedly profitable. (I have only had a few hours all told at Haida, for instance, but you would be surprised to know how much of fundamental importance to Na-dene was revealed in those hours. There is, after all, no substitute for direct impressions in linguistics. It is like art. In both fields one may talk a deal around the subject and, failing direct contact with the source, go far afield.) However, such rapid field reconnaissance would not entail too intensive work at many points. It would give point of view, perspective, vantage point from which to evaluate what is already more or less adequately recorded [on Polynesian linguistics].
>
> (Sapir to Wissler, 3 October 1920, *GSC*)

private networks, culminating in the publication of *Language* in 1921.[71] It would serve as a crucial "boundary object,"[72] negotiating the place of linguistics in relation to anthropology.

The book belonged to a series of works that emerged over the 1920s to codify the principles of Boasian anthropology: "*Language* was immediately recognized as a Boasian paradigm statement, with its definition of language as a variable human resource inherent in the scope of anthropology, the training of anthropologists, and the commitment to record texts" (Darnell 2009: 96). However, Sapir's focus was on introducing linguistics as a distinct tool of analysis, irrespective of its value to the other modules of Boasian analysis. He wanted the book "to serve as a stimulus for the more fundamental study of a neglected field" (iv). He presented the subject matter both for students of the discipline and for a general audience, an "outside public" who might otherwise dismiss language as an unscientific topic (iii). As a general audience book, it represented the culmination of his time in Ottawa as well as the vehicle of his escape from the diminishing returns of that post, a popular work that would make him appealing to the American university system as an instructor as well as a researcher (Darnell 2009: 105). To this end, he limited the use of technical terminology, omitted diacritics, and drew examples mainly from English, occasionally supplemented by other Indo-European and Indigenous languages. Throughout the book, Sapir resumed the argument introduced in "Time Perspective": namely, that the value of language as an object of study depended "chiefly on the unconscious and unrationalized nature of linguistic structure" (iii). It was this criteria which distinguished

[71] Darnell (2009: 96–106) provides a superior account of the book's composition and immediate reception.
[72] Star and Griesemer (1989) define boundary objects as "both plastic enough to adapt to local needs and the constraints of the several parties employing them, yet robust enough to maintain a common identity across site" (393). My use of the term emphasizes their ability to satisfy the needs of multiple groups.

the study of language as a field of inquiry, rendering it more primary than other methodologies within anthropology: "It is doubtful if any other cultural asset of man, be it the art of drilling for fire or of chipping stone, may lay claim to a greater age. I am inclined to believe that [language] antedated even the lowliest developments of material culture, that these developments, in fact, were not strictly possible until language, the tool of significant expression, had itself taken shape" (17). Speech required society (1), he maintained, yet linguistic systems themselves were recalcitrant to external force: "Language is probably the most self-contained, the most massively resistant of all social phenomena. It is easier to kill it off than to disintegrate its individual form" (170). In the same gesture, therefore, that his book realized the Boasian paradigm, Sapir began to demarcate the independence of linguistics therefrom.

Language proceeded in Sapir's programmatic style. It commenced with a review of ideas on the nature of language, stressing that speech is an acquired, non-instinctive, and social phenomenon, in contradistinction to theories which place its origin strictly in the physiological realm (akin to the development of walking), or somehow arising from guttural (interjections) or imitative (onomatopoeic) forms, both of which he argued are conventionalized despite appearances to the contrary. From there, the chapters unfolded from a discussion of the smallest unit of language (the individual speech sound) to the progressively larger (sound systems, the structure of words, the typology of disparate languages). With these basic principles of linguistics established, the latter chapters expounded on topics from historical linguistics: drift (the *locus classicus* of this concept), the discovery of phonetic laws, and the various mechanisms by which languages influence one another. The final chapters mapped out a field of potential correspondences between language and other areas of human life, making similar cuts around language, race, and culture that Boas made in his introduction to the *Handbook* in 1911. Bookending this text were Sapir's reflections on language and thought, his first ruminations over what would become the basis of linguistic relativity for his protégé Whorf. Sapir conceived of language as a "pre-rational . . . prepared road or groove" for thought (10–11), though the two were not "co-terminous" (10); at once an object of scientific and aesthetic interest, for Sapir language thus embodied the "collective art" of a culture (180), whose shared experiences became sedimented in grammatical processes over time. Although Sapir was agnostic to any theory of psychology at this moment (Darnell 2009: 99), his speculations on language and mind seemed to court psychology as another disciplinary fit and certainly prefigured his later work on language, culture, and personality.

Sapir's book was equal parts empirical and speculative. The concept of linguistic drift was perhaps his most conjectural and, for the purposes of my reading, also the most evocative. In its articulation he indirectly assailed the geographic focus of Boas's diffusionist approach to language change:

> But language is not merely something that is spread out in space, as it were—a series of reflections in individual minds of one and the same timeless picture. Language moves down time in a current of its own making. It has a drift. If there were no breaking up of a language into dialects, if each language continued as a firm, self-contained unity, it would still be constantly moving away from any assignable norm, developing new features unceasingly and gradually transforming itself into a language so different from its starting point as to be in effect a new language. New dialects arise not because of the

> mere fact of individual variation but because two or more groups of individuals have become sufficiently disconnected to drift apart, or independently, instead of together. (123)

So it was for language change—the secret work of "psychic undercurrents" transpiring over generations (151)—and so, on another and more immediate scale, might it be said of scientific change. The communities of practice that represented the four pillars of the Boasian synthesis would likewise split off and grow disconnected over time. Thomas Gieryn (1999) uses the image of a map to account for how epistemic authority is decided; he presents this development in agonistic terms, as a matter of conflict and resolution, with skirmishes occurring along the outermost borders of a stable centre: "The cultural space of science is a vessel of authority, but what it holds inside can only be known after the contest ends, when trust and credibility have been located here and not there. . . . The spaces in and around the edges of science are perpetually contested terrain" (15).[73] When Sapir wrote *Language*, it was within the context of such epistemic boundary-work—an effort to put linguistics on the map, as it were. However, the arrival of *Language* did not index a climate of contest—few, then or now, challenged Sapir's incredible facility with language—but one of drift.[74] For the science of language, the interior of this vessel was more akin to a sieve: boundaries were established less through the solid lines of fallings-out than in the falling-through-the-cracks between dotted lines, in the elliptical space of a crumbling truth-spot, in the wake of drifting linguists.

[73] Gieryn (1999) emphasizes that the location of "contests of credibility," vital for guarding the boundaries of science, is less the vaunted space of the laboratory or the austere corridors of the ivory tower; rather, "authority is decided downstream" (27), in "courtrooms, boardrooms, and living rooms." A crucial component of the epistemic authority of the sciences is thus its uptake, and at stake are the privileges of a confluence of power and knowledge that make such claims to authority possible.
[74] Few but not all: Pliny Goddard (1920) notably disputed Sapir's Na-Dene hypothesis.

. . .

17. **1922**. On the Museum and Its Publics.

Efforts for the Anthropological Division to establish a university curriculum ended in 1914 and never resumed during Sapir's time there. Other outreach programs persisted in the meantime, including participation in private clubs and associations such as the Ontario Historical Society (Sapir to Goldenweiser, 1 March 1913, *GSC*) and the Logan Club, which helped "to make the scientific activities of the Survey better appreciated by the local public" (Sapir to McConnell, 24 July 1917, *GSC*). These venues grew in importance with publication halted and the display hall occupied by Parliament, such that in 1920 the Division organized their own Anthropology Club of Ottawa, with Sapir as President and Smith and Secretary (Sapir to Scott, 16 February 1920, *GSC*). Even with the War concluded, austerity measures continued to strangle the activities of the Museum:

> I do not think I am exaggerating if I say that the Museum is less of an actual thing now than in the fall of 1910 when I joined the Survey. While Mr. Brock was Director everything was done to make the museum staff feel that it was getting somewhere, but since his departure things have been going from bad to worse. The war is a convenient excuse, no doubt, but those who know the works from the inside understand perfectly well that it is not likely that things would be very much better for us if there had never been a war.
>
> (Sapir to Lighthall, 10 April 1920, *GSC*)

In addition to the paucity of research, publication, and display, there were no new permanent hires.[75] The Division was having difficulties keeping their extant staff in "proper status" (Sapir to McIlwraith, 13 July 1921, *GSC*), and it was becoming clear that the Division would never recapture the optimism and productivity of its first four years of work.[76]

The situation had worsened such that staff formed a Museum Committee, with Sapir as chair, to petition the Minister's office to obtain full control over the Victoria Memorial Museum Building and become a separate branch of the Department of Mines, coordinate with the Survey but with a separate budget. Sapir beseeched William Lighthall, a Canadian historian and member of the Royal Society of Canada, to put forward a resolution to the Royal Society for their support, asking in plaintive tones: "Please remember always that we really have all the makings of a first class museum. . . . What we are suffering from is an acute case of wet blanket. Can you help remove the wet blanket?" (to Lighthall, 10 April 1920, *GSC*). The Royal Society agreed to ratify the proposal for the establishment of an independent Canadian National Museum and presented the resolution to the proper authorities. In 1921, the Victoria Memorial Museum indeed became a separate division, with William McInnes put in charge (1921 Annual Summary Report, *GSC*). This restructuring made the Museum and Survey, on paper, separate entities—but they still

[75] On publication, Sapir reported: "This failure to continue the Anthropological Series of memoirs and bulletins that was well under way before the war is due not to lack of material but to the present policy of rigid economy in publication expenditures. This policy, if continued in its present form, threatens to render all but useless the work of the Division of Anthropology except insofar as the department allows its anthropological manuscripts to be published by other institutions" (1921 Annual Summary Report: 21).
[76] Conditions were equally poor south of the border: "Conditions here are not by any means encouraging. Thus [sic?] Museum is rather restricting than expanding, and I fear that even as good a man as Lowie is not going to stay there. My department has been cut down to myself alone and the New School for Social Research is dropping more and more theoretical subjects. I think the outlook is not at all good at the present time." (Boas to Sapir, 28 September 1920, *GSC*).

shared much the same staff and facilities and, most importantly, a bottom line (Zaslow 1975: 354). The new Director of the Geological Survey, W.H. Collins, was a geologist and did not consider the Museum integral to the Survey's budget, leaving it ultimately "strained for funds" under his management (357).[77] There was, nonetheless, a return to form. Smith, Wintemberg, Barbeau, and Waugh resumed fieldwork that summer, and Sapir occupied the majority of the year with linguistic research (1921 Annual Summary Report).

By 1921, Parliament had also departed from the Museum, and the building was once again the home of the Anthropological Division, excepting one hall now belonging to the National Gallery. A lack of suitable cases remained an impediment to establishing a permanent ethnological exhibition, but visitors were not impeded and returned in great numbers. A public lecture series was set in motion, with one series for adults and another for children. The series was supported by the Publicity Bureau of the Department of Trade and Commerce, which provided the operator and suitable film (1921 Annual Summary Report). A clipping from the *Ottawa Citizen* on 7 March 1921 indicated the success of public education program (Darnell 2009: 84). The series expanded in 1922, with lectures on various scientific topics in natural history, anthropology, and paleontology, including: hunting dinosaurs; asbestos or fire-proof cotton; animal life of the Pacific Coast islands; water power or white coal; Indians of the plains (taught by Jenness); modes of crossing Canada; the glacial age; pioneer days of British Columbia (by Barbeau); our Selkirk mountains and their precious metals; "Down the Mackenzie River to Their Oil Fields"; Northern Ontario's natural resources; "My Summer in the Norway of Canada" (Smith); and boring deep wells for valuable minerals. The Museum was receiving visitors (and

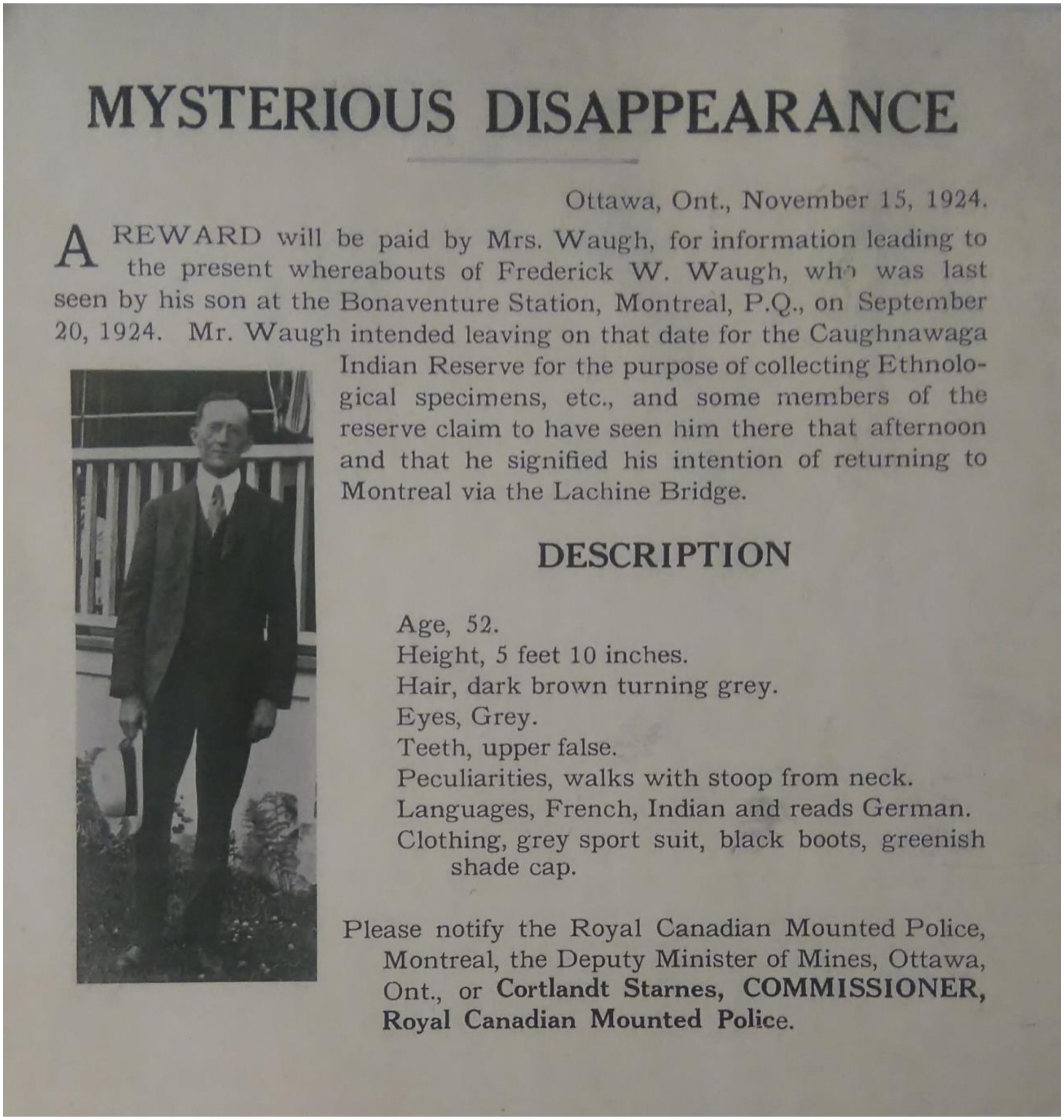
[77] Collins conceded, retrospectively, that the two entities should have been autonomous of each other.

appreciation) from around the world, McInnes reported, giving new purpose to the accumulation of collections: the educational value of the institution became a measure of "the cultural standing and progressive spirit of the country" (1922 Annual Summary Report, *GSC*). Linguistics still had little place in the display culture of the Museum or this new series of talks, and Sapir, though participating in some lectures, continued to work up linguistic material for publication in international journals, returning to form, as it were, elsewhere.

. . .

18. **1924**. On Corporeality.

In contrast to the textual work of philology, the task of fieldwork exposed bodies to the vicissitudes of uncertain spaces. The physical and psychological toll was a cause for concern and also a badge of pride. Half in jest, Sapir once worried that his friend Radin had "become a prey to [his] scientific zeal by getting [him]self murdered in some out-of-the-way corner" (to Radin, 7 December 1912, *GSC*). Research environments were active agents—often tricksters, as §8 showed—in the work of the Anthropological Division, and they affected some agents more than others. Knowles, for one, had "another nervous breakdown" and needed to take a leave of absence (Sapir to Mechling, 15 July 1919, *GSC*); the following year, he tendered his resignation to the Geological Survey, per the advice of a nerve specialist he was seeing in Dublin, who "advise[d] [him] not to return to Canada on account of the severity of the climate and the fact that [he] ha[d] been so constantly in bad health there. . . . in Canada there would be the possibility of recurrence" (qtd. in McInnes to Sapir, 1 June 1920, *GSC*). There were few men qualified to take up Knowles's position, most of whom were already employed at American institutions (Sapir to McInnes, 2 June 1920, *GSC*), and so the Division lost its physical anthropologist. In 1924, Sapir's prophecy was fulfilled. The Division lost another member when Waugh suddenly disappeared.[78]



MYSTERIOUS DISAPPEARANCE

Ottawa, Ont., November 15, 1924.

A REWARD will be paid by Mrs. Waugh, for information leading to the present whereabouts of Frederick W. Waugh, who was last seen by his son at the Bonaventure Station, Montreal, P.Q., on September 20, 1924. Mr. Waugh intended leaving on that date for the Caughnawaga Indian Reserve for the purpose of collecting Ethnological specimens, etc., and some members of the reserve claim to have seen him there that afternoon and that he signified his intention of returning to Montreal via the Lachine Bridge.

DESCRIPTION

Age, 52.
Height, 5 feet 10 inches.
Hair, dark brown turning grey.
Eyes, Grey.
Teeth, upper false.
Peculiarities, walks with stoop from neck.
Languages, French, Indian and reads German.
Clothing, grey sport suit, black boots, greenish shade cap.

Please notify the Royal Canadian Mounted Police, Montreal, the Deputy Minister of Mines, Ottawa, Ont., or **Cortlandt Starnes, COMMISSIONER, Royal Canadian Mounted Police.**

Figure 4. Waugh Missing (Sapir to Waugh, 15 November 1924, *GSC*)

[78] His last written words: "P.S. Please send me about 4 more notebooks, the kind that open at the side" (Waugh to Sapir, 18 July 1924, *GSC*).

In the fall of that year, Sapir sent a telegraph to Waugh, inquiring with concern after the abrupt end of his colleague's correspondence (9 October 1924, *GSC*). With no reply still, an investigation began. The initial hypothesis was that Waugh had arrived ill at the Caughnawaga [Kahnawake] Reserve outside Montréal, in the outlying parts, without officials being aware of it (Memorandum on Disappearance of Mr. F. W. Waugh, 5 November 1924, *GSC*). Jenness and Richard Waugh, his son, left for Montréal to meet with the RCMP soon after; they found evidence that Waugh had indeed left for Kahnawake on September 20th with the intention of purchasing specimens, his suitcase and pack-bag having been taken from the Check-Room of Bonaventure Station in Montréal. As the notice for information circulated by the RCMP indicated, Waugh had perhaps tried to cross the Lachine Railway Bridge on his return and, presumably, there met his end. His whereabouts remained forever unknown; the man disappeared without a trace. However, his ethnological notes, photographs, and specimens from that summer spent with the Montagnais [Innu] on Sept-Îles, Quebec, were forwarded to Ottawa shortly before his disappearance (1925 Annual Summary Report, *GSC*). The man had yielded to corporeality, but his contribution to scientific research survived him.

This chain of events recalls Rebecca Herzig's (2005) formulation in *Suffering for Science*. Here, she describes the valorization of volitional suffering among scientific practitioners arising in late nineteenth-century America: researchers who submitted themselves to conditions of privation, asceticism, and self-experimentation for an "imagined, undying body of science" (7). It was a paradoxical mixture of power and vulnerability that followed along racialized, gendered, and economic lines, with only certain (white, masculine, educated) bodies valued for their suffering; the same hardships endured by local informants, for example, demonstrated “native robustness” but served no higher purpose (80). In the case of the Anthropological Division, suffering made no such discernments; it was distributed evenly through the extremities of the Division’s network of personal and professional contacts. After Beuchat’s death when the *Karluk* sank, Sapir discovered that he had been entailing part of his salary for his ill mother; since Beuchat perished in service to the Survey, Sapir assumed that the full amount ($1500) would be paid out to her, and he rallied for the woman to receive a pension, as it was the duty of the Dominion Government, he believed, to provide for dependents of those lost in the pursuit of “scientific services” (to McConnell, 22 September 1914; 10 November 1914, *GSC*). Later, Sapir also hoped to commute the sentences of two Inuit men condemned to death and another to ten years in prison. He wrote to Jenness how these sentences were “rather unfair and absurd” and beseeched him to intervene: “inasmuch as you know more about the Eskimos and their practical psychology than anyone else in Ottawa, it would probably be a very advisable thing for you to write a pretty strong letter of protest to Department of Justice” (to Jenness, 25 October 1923, *GSC*). Jenness, however, saw no ground for a reprieve: “I am not at all sure in my own mind that the sentence ought not to be carried out. True, one of the prisoners is a young man; true also that the Copper Eskimos have only recently come within the jurisdiction of civilized laws. The fact remains, nevertheless, that murder has always been more or less rampant among them, for trivial causes, and means must be found to stop it” (to Sapir, 1 November 1923, *GSC*). For Sapir personally, the death of Florence Sapir, his wife, had the deepest impact. She had long ailed since arriving in Ottawa and, in 1923 and 1924, had twice to drain a lung abscess; infection set in the final time, and a week afterwards she died (Darnell 2009: 135).[79] Sapir had kept his personal and professional lives separate—"I think few of us realized how long she had been ill or knew that she had suffered so much" (Smith to Sapir, 24 April 1924, *GSC*)—but these were feelings he could not defer: "it will probably take some time before I can do scientific work with anything like relish" (Sapir to Boas, 19 May 1924, *GSC*). Sapir had capitulated much to this space, for the sake of an undying body of science, but it was the passing of nearer bodies that affected him the most intimately.

. . .

Conclusion. 1925. On Departures.

Sapir left Ottawa on 23 September 1925, having accepted a position as Associate Professor of Anthropology at the University of Chicago. His departure was also a return: to the United States, to the academy, to the dream of research. For fifteen years, he had extended himself through the space assigned to him—Canada and the Geological Survey—but the setting did not fit. Sapir was replaced by Jenness as Chief Ethnologist for the Survey's Anthropological Division, whose focus was on making the Museum appeal nationally, not globally; because Sapir left no students and ergo no legacy behind in Canadian Anthropology (Darnell 2009: 191), the extension of the Boas's paradigm was, as he predicted, discontinued. The same year saw Sapir's increasing involvement in the newly founded Linguistic Society of America; there, he needed no longer to justify the primary assumptions of his field but found common ground with those who shared his scientific identity (for more, see Chapter 2), enjoying the pleasures of disciplinarity. One of his final acts at the Museum, he published "Sound Patterns in Language" (1925) in the LSA's journal *Language*, one of the

[79] Darnell (2009: 132–137) provides a fuller account of Florence's illnesses through their varying stages.

first articulations of the phoneme in North America, securing an object of study exclusively linguistic (for more, see Chapter 3). This newfound liberty was not independence—Sapir was still a part of Anthropology at Chicago and later cross-appointed in both Anthropology and Linguistics at Yale—but an autonomy founded on intimate distance: Sapir remained involved with anthropology but increasingly identified as a linguist in his career.

Within this affective history set out in elliptical space, intimate distance manifested at different levels within the information-gathering network emplaced in the Victoria Memorial Museum: it traversed personal, institutional, professional, and epistemological grounds, negotiating the space between informants and ethnologists, between government agents and administration, between linguistics and anthropology, and more. Intimate distance was a way of managing what was in and out of reach, and in that way, it is perhaps a characteristic of any network. For the Anthropological Division, a relation of intimate distance negotiated the fears, setbacks, and contradictions that piled up alongside the data and artifacts brought in by this network of collectors, integral for balancing the compassionate and humanistic principles of Boasian anthropology inside the increasingly dehumanizing bureaucracy of the Survey. It helped Sapir and his colleagues to reach out into a space that was not theirs and, when they retracted their network from these lands, to defer the conditions of vulnerability that the salvage imperative continued to enact on the Indigenous informants from whom they extracted resources under the name of science. In the following chapters, I explore how intimate distance resurfaced in Sapir's controversial disciple, the "amateur specialist" Benjamin Lee Whorf (Chapter 2), and in the epistemic tool that helped distinguish linguistics from anthropology, the phoneme (Chapter 3).

. . .

~ 2 ~

"However steadfast one's commitment to truth, there is no avoiding the noise."
—Lauren Berlant, *Cruel Optimism*, 2011

"Once in a blue moon a man comes along—was not only a pioneer in linguistics. He was a pioneer as a human being—the most notorious of con men—an amateur specialist—eminently worth rereading and pondering in these poststructuralist times—the nonsense that he unwittingly helped to foster is completely out of control—largely self-made, and with a dash of genius—for exercise, he enjoyed walking—often to be misunderstood and his theories denigrated on the basis of superficial readings—wrong, all wrong!—it is so easy for individuals or organizations of a somewhat crackpot nature . . . with all sorts of contexts added . . . do not know where it goes or how it is used . . ."[80]

A Dash of Genius:

Benjamin Lee Whorf, Scientific Belonging, and the Promise of Linguistics

Introduction: Whorf—and the Noise.

Edward Sapir arrived at Yale University in 1931 and promptly established his school of linguistics. There, he attracted new and former students—many from his intervening years at Chicago (1925–1931)—and, with their help, resumed the project of salvage linguistics. Together, they developed important concepts, practices, and training regimens for the science of language in America (see Chapter 3). Among his students, none was so quizzical as Benjamin Lee Whorf. A fire insurance inspector and talented amateur linguist, Whorf would, in the span of his short career (c.1928–1941), make his way to the centre of Americanist linguistics and become one of its most memorable—or most notorious—

[80] The epigraph—a composite of quotations about Whorf and his legacy (the last by the man himself)—announces his uncertain position in the history of linguistics. Chase (1956: v); Lakoff (1987: 330); Deutscher (2010: 21); *New York Times* obituary (1941); Schultz (1990: 5); Pullum (1991: 161); Sapir to Kroeber (30 April 1930); Carroll (1956: 22); Lee (1996: 14); Pinker (1994: 57); Whorf to Fassett (24 May 1940, *BWP*).

members. Whorf belonged to the tail-end of a long tradition of amateur contributions to linguistics and anthropology, but he is remembered today as either a maverick individualist or an abject failure. Rather than use the "dash of genius" to remark on Whorf's exceptionalism, I employ the dash and Whorf to signal the dividing line that formed between amateur and specialist in linguistics during his lifetime. Where Chapter 1 showed how Sapir relied on a vast network of quasi-professionals to fulfill the knowledge-taking aims of the Geological Survey of Canada, this chapter considers through Whorf how that elliptical space solidified into a genealogical line of university researchers: linguists. Here, I analyze how the scientific identity of the "linguist" was institutionalized and became increasingly the possession of a white, male, middle-class, university-educated worker. This process of professionalization gradually excluded amateurs, on the one hand, and created circumscribed roles for Indigenous informants on the other.[81] Whorf's intellectual journey—as both a central and peripheral member of the institutions where these inchoate identity categories were worked out—reveals a calibration of the linguist's subject position and also its limits.

More than likely, if you have heard of Whorf at all, it has been as the second half of the so-called "Sapir-Whorf hypothesis," though there was never a collaboration between Whorf and his mentor, as such.[82] The hypothesis suggests that the structure of the language we speak in some way influences the way we think or perceive—that we are guided, in our

---

[81] My thinking is influenced by Aileen Moreton-Robinson (2015) who, in the context of Australian Aboriginal racial politics, argues that ethno-national identity categories are white possessions premised on the dispossession of Indigenous lands. I maintain that the American linguist's scientific identity followed a similar route through the capture of Indigenous languages.

[82] Harry Hoijer appears to have coined the hyphenate at his presentation for the conference on Language in Culture, University of Chicago, March 23–27, 1953. Though contemporaries Ernst Carrier, Leo Weisgerber, and John Trier reached similar conclusions about language and thought in Europe, Hoijer contends that Sapir and Whorf took on special significance for their interest in American Indian linguistics (1954b: 93).

perception or recollection, by the routes that are most familiar to us through language. The history of this idea, broadly conceived, extends back at least to the seventeenth century, with efforts to employ linguistic difference to characterize and delineate modes of reasoning endemic to the major national languages of Europe (Leavitt 2010), as well as to eighteenth-century German thinkers Johann Herder and Wilhelm von Humboldt (Koerner 1992). Variations upon the concept, stressing lexical categories such as colour terms, arose in nineteenth-century philology and ethnology. For instance, William Gladstone, a classicist, was struck by Homer's systematic description of a "wine-dark" sea and in turn drew conclusions about the limits of Ancient Greek colour perception (Deutscher 2010); later, Hugo Magnus developed an evolutionary sequence for the development of human vision based on analogous results (Schöntag & Schäfer-Prieß 2007). Despite a longstanding intellectual lineage, in the twentieth century the theory became anchored to Sapir and Whorf.[83] Since then, it has proven to be one of the most enduring or virulent topics in linguistics, depending on one's perspective: tested and debated (Hoijer 1954), reviewed and reconceived (Lakoff 1987, Slobin 1996, Lucy 1997, Gentner & Goldin-Meadow 2003), debunked (Pullum 1991; Pinker 1994, 2007), retested (Boroditsky 2001, Gilbert et al. 2008), rebunked (Casasanto 2008), and redebunked (McWhorter 2014).

The attribution to Sapir and Whorf derives from a series of writings over the better part of four decades (Sapir 1921, 1924, 1927, 1929b, 1931; Whorf 1937, 1939, 1940a, 1940b, 1941a, 1941b), including posthumous collections of Whorf's published works and

[83] On occasion, the hyphenate is expanded to include Dorothy Lee, a student of Kroeber and Lowie's in California, who also worked on the subjects of language and worldview (Murray 1998: 24–25).

unpublished fragments and manuscripts (1952, 1956).[84] Sapir's devotion to the idea was secondary: though he encouraged Whorf, in his own works Sapir grew more interested in culture and personality, and employed the relationship of language and experience as a means to that end. Whorf, by contrast, was deeply invested in the concept. He came to an early instantiation of linguistic relativity (what he called "oligosynthesis") independently, before he ever met Sapir, while studying the morphology of the Uto-Aztecan languages Nahuatl and Piman (1928a, 1928b, *BWP*).[85] He made efforts to pitch the idea to funding agencies and veteran scholars, to circulate it in letters and at conferences. Though he never articulated the hypothesis in full, fragments of his thought process appear scattered across these formal and informal settings.[86] It was not until over a decade after his death that these stray thoughts coalesced in an edited collection, assembled by friend and colleague Harry Hoijer.[87] The *Collected Papers on Metalinguistics* (1952) republished three of his most widely distributed articles and became the basis for the 1953 conference on "Language in Culture." Supported in part by a grant from the Ford Foundation, the conference served as a nexus of interdisciplinary exchange between anthropologists, psychologists, linguistics, and philosophers.[88] The conference proceedings were published in reduced form the following year (Hoijer 1954a). These proceedings show that Whorf's

---

[84] I cite the majority of Whorf's published works from John B. Carroll's (1956) edited collection, but, for the purposes of clarity, I reference their original year of publication.
[85] He was no doubt influenced by his mentor's work before then, as his (1939/1956) paper in Sapir's memorial volume indicates.
[86] Penny Lee's (1996) study remains the most thorough extrapolation of Whorf's ideas.
[87] John B. Carroll, once a research assistant of Whorf's, centralized his mentor's published and privately circulated works in a second collection (1956). He framed it as the book Whorf had "hoped to write" had he lived (Carroll 1956: 23). Whorf had in mind "a text of moderate length on language and linguistics [to serve] as an intellectual tool with special reference to science and technology" (to Miller, 13 November 1940, *BWP*).
[88] Jamie Cohen-Cole (2014) draws attention to the vogue of interdisciplinarity in the research culture of Cold War America, as well as to the role of funding agencies such as the Ford Foundation in encouraging these platforms.

ideas met with immediate qualification and scrutiny. From the outset of "the Whorfian vogue" (Murray 1994: 190–197), his peers, successors, and competitors were eager to narrow, contain, select, restrict, and diminish his suggestions—and they debated, more often than not, what he actually said. As they do now.

Why has controversy lingered for nearly a century over the incomplete hypothesis of a minor linguist? Why has this selection of Whorf's research occluded our memory of his translation work, his archaeology, or his accomplishments as a descriptive phonologist? And what of Sapir, whose contribution to American linguistics was far greater, who yet gets relegated to an afterthought in this configuration?[89] Why has Whorf generated so much noise—with so little to communicate? A glance at some responses to his legacy elucidates these questions. Recent criticisms of the Whorfian hypothesis are usually twofold: on the one hand, refusals of the strong form ("linguistic determinism") as impossible and the weaker form as "banal" (Devitt & Sterelny 1987: 178); and, on the other, challenges to Whorf's evidence, for instance claiming that he misrepresented Hopi conceptions of time and propagated a myth about "Eskimo" words for snow.[90] The latter especially are often accompanied by *ad hominem* attacks, centred on Whorf's "amateur" status: Geoffrey Pullum, for one, calls him a "weekend language-fancier" (1991: 163); and John McWhorter remarks that Whorf was a "fire inspector by day" and not an authentic "card-carrying

[89] Murray (1994: 175–176) speculates why Sapir's legacy retreated so swiftly after his death: his scattered students, the inability to standardize his "genius," the neo-Bloomfieldian turn against historical linguistics, and the international expansion after World War II turning attention away from North American languages.

[90] Both claims are arguably the result of misinterpretations. Whorf himself described the tense-aspect system of Hopi at length in another work (1936a). Concerning the "Eskimo Vocabulary Hoax," Chicoki and Kilarski (2010) evaluate the literature on it, past and present, and show that the prominence of this minor example represents the transmission of a misconception by writers whose own interest in the language was "instrumental and opportunistic" (371), rather than based on a studied reading of the original texts.

linguist" (2014: xii).[91] McWhorter (2008) even assails Whorf's personal charisma to reduce his contributions: "Whorf was also a mesmerizing speaker, and a looker to boot" (141).[92]

Figure 5. B.L.W., P.Y.T. (Obituary Clippings, 1944, *BWP*)

These invectives against Whorf himself suggest how the controversy has crossed over, in his critics' minds, from the domain of "bad science" to that perhaps of pseudoscience. As Michael Gordin (2012) distinguishes, bad science can be recognized and dismissed for

[91] Whorf was indeed a member of the Linguistic Society of America and other professional associations throughout his life; whether they carried cards or not, or do so now, I am not sure.
[92] William Clark (2006) traces how particular forms of charisma, tied to disinterested academic labour and imbricated through such technologies as seminar papers, doctoral dissertations, and library catalogues, came to prominence alongside the emergence of the research university in the nineteenth century.

being "substandard," but the epithet of *pseudoscience* designates something more menacing: a challenge to "the authority of science, science's access to resources, or some other broader social trend" (2)—or, to put it differently, the threat of failure. For Whorf's detractors, it appears not only necessary to dismantle the hypothesis empirically (a subject of debate, at least) but also to disparage Whorf, as though his presence within the field was in itself objectionable and a threat. Whatever the reason, the message is clear: Benjamin Lee Whorf, you do not belong.

Rather than dwell on the truth conditions of linguistic relativity (or the lack thereof), a circular debate which resurfaces periodically almost by rote, in this chapter I seek to diffract the semiotics of Whorfianism and engage the manifold ways we might otherwise remember a figure such as Whorf: as a prodigy, a failure, a genius, an amateur, as the whipping boy of cultural relativism—or as all these personages at once. As Steven Shapin (2010) argues, the character of the knower often contributes to the credibility or truthfulness of their knowledge claims. Here, I will show Whorf as a historical actor and as a chimerical figure within the collective memory of the early development of the language sciences who, perhaps because of his categorical volatility, also features as a recurring threat to the cultural and epistemic authority the discipline has since accrued and now guards.[93] As part of my broader endeavour to explode the Sapir-Whorf hyphenate and explore the research culture surrounding it, I focus on Whorf's emergence as a practitioner of linguistics in the 1920s and '30s, amid the discipline's incipient professionalization as a

---

[93] It is the threat, conceivably, of historical memory at all for any field swayed by the attractions of scientific presentism. As Julia Falk (2003: 140) observes, the most insistent upon the autonomy of linguistics were the least likely to engage in historicism; by this token, historiography functions as a destabilizing vector to that closure.

human science in North America, inaugurated by the 1924 founding of the Linguistic Society of America (LSA). As a practitioner of linguistics, Whorf kept his own intimate distance: where for Sapir the relation evoked working through a scientific network from his headquarters in Ottawa, for Whorf it meant proportioning himself—his values, interests, and commitments—within the confines of an emerging academic discipline. How did Whorf negotiate his scholarly life against his full-time employment as a fire insurance inspector? What about linguistics captured his imagination? Did his unusual trajectory challenge expectations for a scientist at the time? Were his ambitions so different? Could they enable us to think about the different publics (institutional as well as "lay" audiences) available to early-twentieth-century linguistics?

At the same time that I pose these questions endogenous to Whorf's archive, I also employ him as a figure to rehearse and rethink the interests of my own fields of study. Literature in STS and in the historiography of linguistics has offered ways of disrupting the singularity of the genius, tracing the roots and routes of their knowledge claims and revealing the networks which fostered the conditions for their aptitudes to thrive. These approaches challenge the hagiographic function of "Whig" histories that set scientific geniuses apart and draw them into strict lineages. The scientific "persona," in particular, has emerged as a category for mediating the individual and the social, in contrast to the lionization that occurs in many biographical genres: the persona instead represents "a cultural identity that simultaneously shapes the individual in body and mind and creates a collective with a shared and recognizable physiology" (Daston & Sibum 2003: 2). To put Whorf in contrast with Sapir or the other academics at the First Yale School, I employ the category of persona to think through the tensions of Whorf's personal eccentricities and his desire to belong to a greater scientific culture. Two personae appeared out of Whorf's life: one, the "amateur specialist," a title he never claimed; another, the public intellectual, an identity he never obtained. This chapter divides into two sections that follow Whorf's various attempts to assume a legible persona within disciplinary frameworks and the public sphere, mapping these efforts respectively onto the categories of the scientific self and the scientific popularizer.

Where discussions of the scientific genius are often success stories—whose protagonists' achievements are characterized by triumphs of intellect, rhetoric, or charisma, and whose tribulations resolve into fixed or stable identities—the categories of scientific self and popularizer also lend themselves to narratives of inconclusiveness, irresolution, multiplicity, and failure (e.g. Galison 1987, Secord 2000). I situate my analysis in this latter camp, foregrounding the noise, the friction, and the failures that inhere in Whorf's contingent and almost tragicomic efforts to construct an authentic scientific identity.[94] Through the heuristic of failure, I interrogate and deconstruct the logics of science and success that undergird such concepts as genius, expertise, and disciplinarity. The volatility of Whorf and of his legacy, I will argue, show how failure is a constitutive—rather than temporary or detrimental—condition of scientific knowledge-making, as it was for Sapir in Ottawa. This condition is particularly salient when the character of that science is still in formation and yet to be decided: before success delimits what is emergent, schematic, and indeterminate about it.

[94] An instructive example is Jan Golinski's (2011) articulation of British chemist Humphry Davy's "experimental self." Davy's own subjectivity was the topic of enduring scrutiny, both in the experiments with nitrous oxide he performed on his own body and later in his efforts to translate those personal sensations into credible scientific evidence. Through Davy, Golinski observes how credibility relied on established witnessing practices and how creative individuality was fostered by disciplinary matrixes. The failure to adhere to either meant that one would, like Davy, often be left to flounder.

To this end, I employ the em dash as a conceptual device to guide my study. The hyphen has long served as a point of anchorage for the Sapir-Whorf hypothesis. As a tool, the hyphen has the potential to join "opposites in a metonymic tension" that yet maintains the separate identity of each part (Hayles 1999: 114). While the dash, an elongated hyphen, functions similarly—to emphasize—but to set apart—as it does in a list of successful and successive "geniuses," the dash also invites transgression, digression, and suspension of the status quo. Here, it serves to widen the space between Sapir and Whorf, opening up the claustrophobic debates surrounding them to examine instead the conditions of the latter's ambivalent search for belonging. Following loosely from Thomas Rickert's (2013) conceptualization of the "ambient," I understand belonging in similar terms, as that which recedes from identity, but which nevertheless supplements it. By shifting attention to belonging over identity as a category of analysis, I employ Whorf as a sound level meter to gauge the ambient noise out of which he and his linguistic relativity principle emerged: the social, intellectual, and affective waves that shaped and constrained his words, thoughts, and actions. Belonging captures the intimate desires that animated Whorf's quest for recognition in the scientific and public spheres—desires that remained, in part due to his own partitioning, at a distance. Rather than follow the route most familiar and deposit Whorf in a hyphenated relationship to Sapir, or to strike him through with a dash entirely, I de/re-arrange those lines into a web, pulling out four cords—Whorf as hobbyist, *bricoleur*, unifier, and luminary—to provide a sense of where and how he sought to belong. While I do this work to retrieve a more complex portrayal of Whorf, I observe nonetheless how the categories of identity through which he tried to re-invent himself remained consistent with the project of salvage and the belongings that it limited.

❖ Whorf—an Amateur Specialist.

Named so in his *Times* obituary, Benjamin Lee Whorf was memorialized as "an amateur specialist"—an enigma, an anomaly, a contradiction in terms. That he was something of an oddity, even to his contemporaries, was no secret. His linguistic ability was long the subject of commentary and curiosity: as Kroeber remarked, with regard to his ability to perceive patterns in language, Whorf had "imagination, . . . and a genuine insight and a touch of genius" (Hoijer 1954a: 231). His career, moreover, did not follow the expected trajectory of a university researcher like Sapir. Whorf was a full-time fire insurance inspector with a bachelor's in chemical engineering, through whose own dogged persistence and self-discipline made his way into the inner circle of Americanist linguistics. A self-professed hobbyist (Whorf to Boas, 9 April 1928, *BWP*), Whorf was yet unlike the nineteenth-century adventurers, citizen scientists, or "amateur rationalists" (Darnell 2001: 9) who contributed data for armchair analysts.[95] From about 1928, when he undertook his first research excursion to study the Uto-Aztecan language Nahuatl, through his later appointment in 1936 as Honorary Fellow in Anthropology at Yale, to his sudden rise in notoriety a year before his death in 1941, Whorf occupied a median point between scientist and amateur at a time when these categories were in flux and heavily debated. During Whorf's lifetime, American anthropology underwent a process of professionalization under the direction of Franz Boas, migrating from the auspices of the Bureau of American Ethnology (established 1879) to the university system between 1900 and 1920 (Darnell 2001); linguistics, often the disciplinary *attaché* of cultural ethnology within the Boasian paradigm, followed suit shortly afterwards, in part due to the contributions of Sapir and his school at Yale.

[95] For a more probing analysis of "armchair" anthropology, see Efram Sera-Shriar (2014).

However, the fields of anthropology and linguistics at this time were still open to the contribution of amateurs.[96] There was as yet an insufficient number of university-trained candidates to undertake the vast project of salvage linguistics and ethnology in America, and amateurs like Whorf were often independently wealthy, held enthusiasm for the work, and received comparable training to their academic peers under an apprenticeship model with the masters of the field.[97]

Whorf hence remains a useful figure for gauging the promise of linguistics at this juncture and, I will show, his amateur status also bears on what Lorraine Daston and Peter Galison (2007) have termed the "scientific self." Arising in the nineteenth century alongside the advent of research universities, the scientific self denotes a co-construction of disciplinarity and subjectivity, moulding practices of observation to uphold the tenets of objectivity.[98] Daston and Galison (2007) reject broad technological or ideological transformations as the cause for the widespread application of different modes of objectivity; instead, they view the training of subjectivity, both within disciplinary structures and popular imaginaries about the scientist, as an indispensable component of these rise and falls. My next chapter examines how American linguistics began to ossify its standards of training and practice, a disciplinary process which did and did not fit Daston and Galison's model of objectivity. Here, I examine how Whorf's subjectivity—as a "self-made" linguist (Sapir to Kroeber, 30 April 1936, *BWP*)—represented an aporia within such

---

[96] Wendy Leeds-Hurwitz (2004) provides a stimulating account of a similar figure, Jaime de Angulo, a Spanish physician who became an "interested individual" (5) active within American anthropological and linguistic circles particularly between 1920 and 1933.
[97] Indeed, before the establishment of professional academic anthropology in the nineteenth century, practitioners were typically self-taught and self-identified (Darnell 1998a: 12).
[98] Daston and Galison present three dominant eras of objectivity (truth-to-nature, mechanical objectivity, trained judgement) and three scientific selves to match them (the sage, the worker, the expert).

developments. How did he negotiate the different interests of his academic career and his regular employment? This section brings into focus the vicissitudes of balancing profession and calling, highlighting the positionality of the amateur specialist against the rapidly solidifying professional identity of the "linguistic self." Whorf's subjectivity was not delimited by the requirements of objectivity alone, but encompassed the leisureliness of the hobbyist and the playfulness of the *bricoleur*—traits which dashed the austerity of objectivity through his whimsical operations. Below, I trace Whorf's intersection with the newly professionalized discipline and its training structures and, in so doing, shift focus from a notion of the self as an achievable end, or direction of torque, to that of scientific belonging, a term which more aptly captures the latticework of desires, longing for community, and cross-identifications that marked Whorf's crossing through linguistics.

a. The Linguist in Training—the Hobbyist in Transit.

From the beginning of his scholarly ambitions, Whorf owed much to an array of supporters: Herbert Spinden and Alfred Tozzer, both anthropologists, for their early assistance and connections (1927–1928); the Indigenous informants and Mexican locals who enabled his initial research trips (1930); and Sapir's Linguistic Group for accepting him as a colleague and friend, despite an ostensive absence of qualifications. However, his *Times* obituary cast his career in a different light:

> Here is another instance of the amateur, who, by focusing his spare energies on some subject of fascination to him, rose above the humdrum of a daily chore, opened his eyes to new horizons and took pride in making a deposit in the growing fund of human understanding. (29 July 1941)

It presented his course of study as a kind of intellectual bootstrapping, a celebration of the hobbyist. Whorf's own (early) description of his training in linguistics reinforced this narrative, to an extent. His university had not offered courses in philology, but as he professed in a statement of his studies on Semitic languages: "many people have shown a desire to study and work in two entirely independent fields," that the "stress laid on original research at 'Tech' [M.I.T.] has stimulated me in the other field," and "of linguistics, I have taught myself" (to Torrey, 30 January 1928, *BWP*).[99] In a letter to Edgar Sturtevant, organizer of the Linguistic Institutes at Yale, he underscored: "my self-training has been rigid . . . and my work carefully handled and up to a good standard" (20 July 1928, *BWP*). What was it about linguistics that inspired this confidence? His unpublished manuscripts reveal that this was not the first of his forays into other sciences. His early writings spanned the subjects of zoology, physics, and evolutionary biology, as well as topics on science and religion.[100] Linguistics was only the most receptive. Were his talents, then, more suited to the study of language? Or was the discipline of linguistics itself, only recently becoming professionalized, more porous and open to the contributions of adventurers, collectors, university scientists, missionaries, government agents—and hobbyists? Were its methods, not yet standardized, more flexible?

The appellative of "amateur" need not carry negative connotations; for Whorf, it did not.[101] Rather, the status of amateur was a source of negotiation and possibility. Ever-

---

[99] Whorf came across the subject of philology in high school, studying Latin and becoming impressed by its inflectional system (30 January 1928, *BWP*). He was also struck by how different Semitic languages were from others he encountered (French, German), and he resumed study of Hebrew at the Hartford Theological Seminary.

[100] "Why I Have Discarded Evolution" (1925, *BWP*) was sent to biologist Thomas Hunt Morgan, who read eight or nine pages of it before chewing him out.

[101] The category of "amateur" was porous and applied to an array of actors doing social science in this era. Whorf was unlike the public intellectuals that Russell Jacoby (1987) chronicles, often "coffee house

industrious, Whorf began his linguistic studies before he came into contact with the Yale school. On his own initiative, he embarked on a series of modest research projects to study the language and civilization of Ancient Aztec and later Maya, relying on locally accessible manuscripts and reproductions to craft translations of hieroglyphics. Most of these efforts took place in Hartford, Connecticut, where Whorf was employed as a fire insurance inspector, but he found opportunities to advance his studies elsewhere: "He worked not only in the Watkinson Library, but also at any library he could profitably visit on his numerous business trips away from Hartford" (Carroll 1956:10). In the meantime, Whorf conferred with Spinden, at the Peabody Museum, and Tozzer, an anthropologist at Harvard; these two were early supporters of Whorf's research and would later recommend him for a Social Science Research Council (SSRC) grant. They brought his work to the attention of Boas (Spinden to Whorf, 6 January 1928, *BWP*), encouraging him to present a paper on Aztec linguistics and Toltec history at the International Congress of Americanists in New York City, in 1928. It was through this network of contacts, institutions, and scholarly associations that Whorf became increasingly immersed in the conversations of the field. The same year, he began networking at the first summer Linguistic Institute (LI), though he could only manage to attend public meetings during a leave from work (Whorf to Sturtevant, 20 July 1928, *BWP*). The "exigencies of employment" continued to interfere with his ambitions, rendering it impossible for him to stay in New Haven that year or the next (to Sturtevant, 8 July 1929, *BWP*). Nonetheless, throughout his later research trips and his temporary academic appointment at Yale, Whorf maintained his full-time position as

intellectuals" writing in "little magazines" oriented toward social issues, or the female social scientists prohibited from academic employment due to anti-nepotism rules who nonetheless forged research careers (e.g. Eleanor Gibson).

fire prevention inspector, endeavouring to work his scholarly career around it. The amateur specialist at once belonged to the academic community and poured beyond the edges of it.

The summer Institutes were crucial sites where the training regimens for the "linguistic self" were developed and debated by central figures in the field—and, though Whorf only skirted across them, the early LIs were also important to assess how he navigated these disciplinary currents. For a description of the first two Institutes that took place at Yale University (1928–1929), I draw on Martin Joos's (1986) characterization of them, in his madcap and often anecdotal *Notes on the Development of the Linguistic Society of America*.[102] The LSA founded the LIs upon deeming educational opportunities inadequate to support the association's two major aims: the "spreading of knowledge of linguistics, and the recruiting and training of linguistic scholars" (Hill 1964: 1). This mission statement stood in contrast to the lay opinion that a society of linguists must have wanted to "improve" language somehow (e.g. through spelling-reform or devising an international auxiliary language). At the time, the "notion of investigating a language without trying to change it . . . was never contemplated seriously by well-educated knowledgeable persons" (Joos, 17). Here, I am interested in how this endeavour was intertwined with the research culture that reinforced it. The two Yale Institutes were supported in large part by a subvention from the Carnegie Corporation. Lasting six to eight weeks, along with the December Annual Meetings the LIs helped to frame the academic

[102] The editors stress the circumstantial nature of the evidence of this text, in their Foreword: "This document is *not* a history of the Linguistic Society of America and should not be read as one. It is a set of notes and observations on the early career of the Society, made from Martin Joos's always very special and sometimes highly personal stance. Several informed readers of the unpublished document have spotted what they consider to be serious factual errors. But this is not a history; it is only source-materials for one."

year. At the first LI there were 39 courses offered in total, by 24 instructors and with 45 registrants, ranging in topic from field methods in linguistic anthropology to comparative Germanic philology. The classes themselves ran often at a ratio of two students to one professor—or, at the second, at a ratio of one-to-one.[103] Roland Kent, Secretary of the LSA at the time, considered the narrow difference between instructors and students "a failure" (Kent qtd. in Joos, 28). Still, he lauded the unofficial meeting spaces the LIs encouraged: "Not the least valuable of the activities of the Institute was the gathering of small informal groups to discuss linguistic problems quite apart from the times and places of the scheduled courses; and in these, from time to time, virtually every one participated" (Kent qtd. in Joos, 28). It was in these informal settings that the scientific culture of American linguistics began to take shape.

Joos offers an artful description of the atmosphere and social structure of the first such gathering, worth quoting at length for its richness of detail:

> We pause to remind readers that [Kent's] Report was written during the long since forgotten Prohibition period and was for the eyes of not simply our own Linguistic Society members and their associates but also for stiffly starched members of the Yale Corporation. Any printed allusion however discreet or veiled, to the accessibility of 'drink' within or near the sacred precincts could spoil the chances for a 1929 Institute in New Haven. Actually, the hard drinkers were a vanishingly small minority, and 'drugs' were foreign to the scene. Not *cannabis indica* but *nicotiana tobacum* was lavishly

[103] At the second LI, the ratio was one to one (26 instructors and an equal number of registrants). Joos imagines that speculation on the stock market before the Crash and the "unbearably stodgy" New Haven summer contributed to the second LI's diminished attendance (29).

> employed to saturate the air with its incense of good fellowship wherever those 'small informal groups' held their all-hours gatherings, notably just after the break-up of every Public Lecture Discussion period and serving to continue the discussion without restraint and within face-to-face groups; non-smokers were few, and many of 'the girls' had become smokers simply in self-defense. The per-capita ingestion of the forbidden ethanol, suitably flavored and discussed along with linguistic topics, in close sequence or simultaneously, in the course of any one gathering of the sort in which 'virtually every one participated' was what probably would now be called 'two drinks'—nothing to worry a spouse or a parent. The footnote to the Bloch Obituary, *Language 43.6* (1967), belongs to a quite different sort of milieu, Chicago's Near North Side, which corresponded fairly well to Greenwich Village in lower Manhattan, that haunt of sin. New Haven was different. Did none of the Faculty ever visit spots where one went for 'serious drinking' such as were numerous within a few minutes' walk from Harkness Recitation Hall? Not many, surely, because they were noisy and crowded and too dimly lighted for phonetic discussions accompanied by suitable diagrams and demonstrations. (28)

To put it in other words: these were good linguists, law-abiding citizens, eager to court the favour of their university patrons and—like any good linguist—they had oral fixations. Their regimens for training the body fit within the broader regulations on the body-politic.

Joos goes on to add: "That 1928 Institute inaugurated nearly the whole inner form and a good many of the traditional details for the Society's warm seasons generally" (28).

Amidst the din and the thick miasma of a New Haven speakeasy, then, emerged an "interstitial academy," as Joel Isaac (2012) has defined: an informal cluster of clubs, seminars, and subcultures operating alongside but apart from traditional departments. Isaac identifies the research practices and pedagogies at the interstices of the academy as integral to the cohesion and authority of "normal science." The barriers to entering any such academy are never distributed evenly, and these sessions were no exception. Despite support from Yale and the Carnegie Corporation, the organizer, Sturtevant, had to assume a fair deal of the financial burden and hidden labour of the early LIs, before they moved to the City College of New York in 1930: the first LIs were thus founded on the "tacit assumption that only a man with Family Money could assume the burdens of a Yale Professor's social duties" (Joos, 27). Moreover, we notice the makings of a homosocial atmosphere in the "incense of good fellowship" Joos demarcates, where female attendees—"the girls"—had to intrude within a male-coded arena of these informal gatherings and no doubt risk magnified social stigma attached to the indiscreet libations.[104] Frederick Newmeyer (1996) and Julia Falk (1999) each discusses how the move toward professionalization in linguistics was gendered, in effect pushing out women, whose backgrounds were in less technical fields such as language instruction. Indeed, women, who had numbered nearly a third of the membership of the first LI, occupied significantly fewer places in the second (Joos, 32). This interstitial academy, at the same time that it generated cohesion and consolidated a training centre, also enabled gatekeeping at the threshold of scientific belonging along lines of class and gender.

---

[104] Peiss (1986) examines the expansion and commercialization of leisure at the turn of the century through the eyes of young working-class women, who gained access to social clubs and other male-coded spaces.

At first, Whorf's circumstance posed a challenge for him to obtain the forms of (cultural) capital necessary to undertake academic inquiry—to join the club, as it were. His diaries from this period reveal how he travelled to the LIs in the morning and returned home at night, at least once stopping at Waltnut Beach—"water good, but low tide" (23 July 1939, *BWP*)—so he could not partake completely in this interstitial academy nor the New Haven nightlife. His SSRC application offers another example. It was originally not for the Grant-in-Aid he later received, but for a year-long research fellowship. He once confided that he was worried about competing against "men with PhD degrees" (to Mason, 6 December 1928, *BWP*). The bulk of his research, Whorf mentioned in the application, would have to be undertaken during a five-to-six weeks leave granted by his employer, pointing out—with what now we might call naïveté—that he must take care of his dependents, foremost (1928 SSRC Application, *BWP*).[105] The response was a setback. The SSRC secretary informed Whorf that he must dedicate himself "full time to the project at hand" (17 December 1928, *BWP*), but recommended he apply for the shorter-term grant instead. The part-time commitments of the amateur specialist limited his access to scholarly resources.

However, where the "exigencies of employment" stifled certain opportunities early on, toward the end of his career Whorf's independent employment became an advantage. His steady position at the Hartford Fire Insurance Company, close to Yale, made him an "ideal homebody" to operate as a hub for letter-writing among Sapir's students, when their mentor's health began to fail (Darnell 1997: 557). In a circular letter sent out to the members of the Committee on a Society for American Indian Linguistics (Boas & Sapir to

[105] Whorf did receive the leave, upon accepting the Grant-in-Aid.

Whorf et al., 8 February 1937, *BWP*), asking about the future of the field, Whorf was the only one not to acknowledge the bleak job prospects and fleeting institutional support as a predominate concern, focusing instead on matters of publication (for instance, on preserving raw data vs. analysed material) and fieldwork (standardizing orthography). Once a source of trepidation, he now appeared to exult in being the only plain "mister" among doctors in Sapir's school at Yale (c.1938, *BWP*).[106] The security of his employment inured him to the disappointing job market afflicting the other members of the Yale group, many of whom had salaries tied to external sources, like the Institute of Human Relations, which reverted back to the university upon Sapir's death (Darnell 1998b: 367). Despite offers of more permanent appointments late in his life, Whorf "consistently refused them, saying that his business situation afforded him a more comfortable living and a freer opportunity to develop his intellectual interests in his own way" (Carroll 1956: 5). His position as amateur specialist was far from a source of derision; it was, in the least, double-edged. In times of financial straits, Whorf's amateur status afforded him economic freedom—it was an advantage rather than a liability.

This positionality also provided him a different orientation toward the work itself. Because Whorf did not derive his chief source of income from teaching and research, like his colleagues, it gave his mind—and pen—leeway to wander. Besides the nonlinear reasoning that yielded his linguistic relativity principle, which I will pursue below, he also published and presented at many non-academic venues. At the Hartford Theosophical Association, for instance, he gave a public talk entitled "The Magic of Language" (27 Oct

[106] Another mister—Ephraim Cross, who had attended the first LI— contacted Whorf in an attempt to recruit him as an ally to challenge the "democratization" of the society (Cross to Whorf, 31 January 1939, *BWP*). Whorf surmised there was no problem at all.

1940, *BWP*), which concerned what he called the "transcendental word magic" of Egypt and India. Likewise, his ruminations on metaphysics appeared in the journal *Main Currents in Modern Thought*, where he espoused his desire for a diverse "brotherhood of thought" (Whorf 1941c: 15), based on a relativist understanding of language. Unlike the disinterested professional Max Weber argued for in "Science as a Vocation" (1919), a scientist free from utilitarian demands and partisan politics, Whorf was more akin to the Victorian "man of science" before him (White 2003), eager to expound on subjects general and particular, publishing equally on specialist and spiritualist topics. Whorf had professional aspirations, to be sure, but science remained for him a hobby, something done at his leisure. In this way, he prefigures Michael Lynch's (2009) response to Weber in "Science as a Vacation." Lynch argues that scholarship requires "a vacation from more common modes of engagement": "the academic vocation demands more than mental detachment or disinterest[;] it requires institutional distance and temporal leisure from political and economic arenas, with their pressing demands for useful or acceptable answers" (106). Lynch stresses how this leisure time should be a "temporary refuge that affords the opportunity to see, or do, something different" (114), but for Whorf, perhaps, that longing for elsewhere was the persistent condition of his scholarly life.

Linguistics was, for him, a source of enchantment and amusement—it suggested that which lay beyond the commonplace; it was freeing and fun. This attitude emerged in his impassioned appeal to Irving Pescoe, at M.I.T., over the education of his fellow "Tech boys." They, like him, might have "vague dreams" of building bridges or becoming policemen: "Yet the very fact of having this constructional kind of career-image may indicate a mind interested in structure and design above the ordinary, and fitted to make an exceptional man" (16 April 1939, *BWP*). Whorf wanted these boys exposed to the social sciences and humanities, so the "more sophisticated kind of magic" he associated with the search for underlying truths would not "evaporate" with "the increasing detachment of the maturing, economically pressed man." Over the twentieth century, Shapin (2010) observes, "modern scientists" became distinct from "priests": "Their expertises [were] not fungible—either one of technical expertise into another form or technical expertise into moral authority" (391). Nonetheless, what made linguistics a worthwhile transaction for Whorf—worthy of investing his own time, energy, and financial resources—was, indeed, the science's fungibility: the potential for an interest in language and structure to be transposable to other domains of life. Whorf's pursuit of this emancipatory ideal, however, belied the conditions that constrained who might otherwise obtain it. The gendering of many of the above expressions—*man* of science, *brotherhood* of thought, fellow Tech *boys*—was not innocent or incidental. Nor was the deeply gendered body imagined to possess the curiosity and alacrity Whorf described;[107] nor the homosocial culture of the early Linguistic Institutes. Those who could buy into the ideal Whorf endorsed were those who could afford the economic and social costs.

Simultaneously a foundational and outlying member of the linguistics community, the amateur specialist—equal parts homebody, travelling businessman, scholar, and hobbyist—strew an unlikely trail across the developing boundaries of a science in its formative years, belonging to each of these categories yet reducible to no single one. The image below, a hotel card etched with Whorf's translation work, gathered together these

[107] Elizabeth Grosz (1994) analyzes how the ideal of the disembodied and unencumbered mind is constructed on the basis of male corporeality and proposes a turn to the experiences of women for alternative models.

incongruous parts. Whorf was indeed a "card-carrying linguist," but of a different sort than McWhorter imagined.[108]

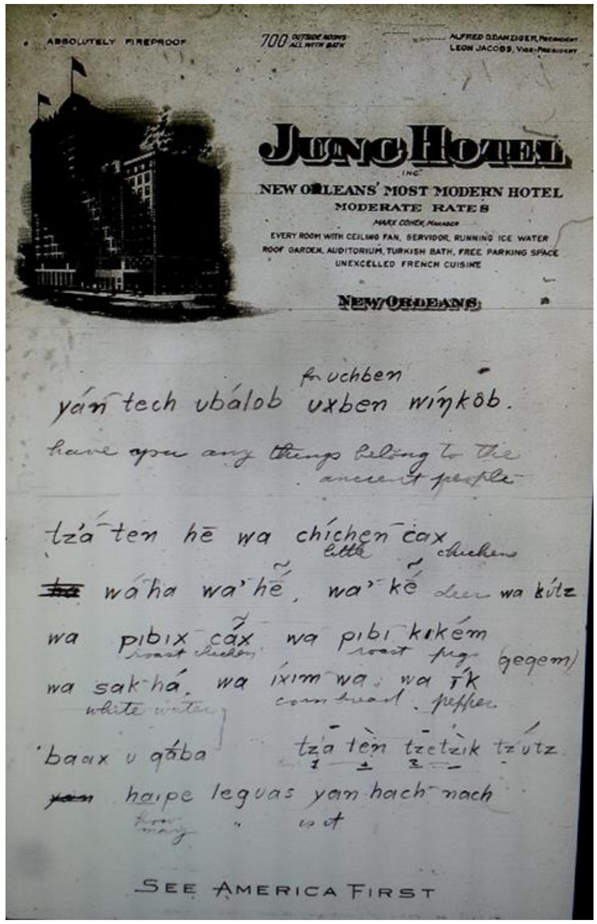


Figure 6. Carte Hôtel (Unpublished Documents, c.1929, *BWP*)

In her riveting study of the "Cartes Postales" genre in turn-of-the-century Paris, Noami Schor (1992) makes reference to the "*plaisir de la carte*": "Part visual (these cards are very beautiful), part cognitive (these cards contain a great deal of information), the *plaisir de la carte* is in large part nostalgic . . . a fragment of past Parisian life" (237). By contrast, the hotel card was visually blunt and utilitarian. The tag —"ABSOLUTELY FIREPROOF"—was a source, perhaps, of some mirth for Whorf: a chuckle for the fire safety inspector, his day job

[108] Hans-Jörg Rheinberger (2010) identifies notetaking as an important site for examining the "redimensionalization" of data and not simply a neutral container for its transportation.

and hobby converging at a congenial moment. If Whorf felt attachment to the card beyond as a repository for scrap notes, it would be as a keepsake, likely taken in transit from his first field site. The card showed how Whorf taught himself to be a linguist—but it also revealed some ways in which he was already trained. The material surface and the written text were intimately bound: the hotel, as a location, was a refuge or liminal space for the business class—it located Whorf in the social coordinates of American, male, and middle-class. The card encapsulated the trafficking of his mind and body, his career and curiosity, the hobbyist and the professional. It also suggested who might have fallen by the wayside—those who could not have been an amateur or a specialist. Leisure time, vacation days, access to resources and social connections, mobility—all these forces which converged to make Whorf's unlikely career possible—were privileges of the class to which he belonged and this card marked. Because he possessed the class mobility—literally—to cross the boundaries of amateur and specialist, Whorf was able to satisfy his longing to belong to a scientific community. Belonging as a category of analysis thus accommodates those, like Whorf, who lacked an easy identification with science and success—but it also suggests the barriers for entry of those who were denied the opportunity to belong at all.

b. An Engineer—among Bricoleurs.

Whorf's identification with linguistics was always partial: he absorbed its methodologies but maintained an independence from its institutional structures. His ambivalence toward disciplinarity brings to mind Paul Feyerabend's (1993) commentary on science education: "An essential part of the training that makes facts appear consists in the attempt to inhibit intuitions that might lead to a blurring of boundaries" (11).[109] In order to make its object of study tenable to scientific inquiry and amenable to academic curricula, linguistics has had to split language into more manageable parts, bracketing off elements that are less tenable to its tools of analysis. Linguists are not engineers, using their expertise to devise new languages.[110] They are *bricoleurs*—or more accurately *bricollecteurs*, given the scope of Americanist work—working within the enabling constraints of their "experimental systems" (Rheinberger 1997), reshuffling data and playing with evidence to devise new understandings of language.[111] During Whorf's lifetime, semantics—the branch of linguistic inquiry concerned with meaning—was one such boundary: semantic categories lacked the methodological rigour of phonetic or grammatical analysis, so they were largely set aside. In the previous section, I considered how Whorf subtended the expected trajectory of a social scientist, recalling a time of amateur contributions to linguistics and anthropology. Here, I examine how the perspective of a chemical engineer among *bricoleurs* enabled him to make lateral movements across systems—and speculate how such movements have contributed to his contentious reputation today.[112] Whorf saw possibilities not only within the limits of an experimental system—he was as an accomplished descriptive linguist—but

[109] Feyerabend recommends an unspooling of disciplinarity, an anarchic model revolving around experiment and play: as in the case of childhood development, "the initial playful activity is an essential prerequisite of the final act of understanding. . . . The process itself is not guided by a well-defined programme, for it contains the conditions for the realization of all possible programmes" (17).
[110] Contemporaneous initiatives like the International Auxiliary Language Association and Esperanto movement complicate this statement.
[111] I treat Hans-Jörg Rheinberger's idea of the "experimental system," along with and in relation to the neologism of *bricollecteur*, with closer attention in the next chapter.
[112] Derrida (1978b) remarks on the slippage between these categories: "The engineer, whom Lévi-Strauss opposes to the *bricoleur*, should be the one to construct the totality of his language, syntax, and lexicon. In this sense the engineer is a myth. A subject who would supposedly be the absolute origin of his own discourse and would supposedly construct it 'out of nothing' . . . The notion of the engineer who had supposedly broken with all forms of *bricolage* is therefore a theological idea; and since Lévi-Strauss tells us elsewhere that *bricolage* is mythopoetic, the odds are that the engineer is a myth produced by the *bricoleur*" (360).

also astride them: through his theory of linguistic relativity, he built on connections between linguistics and psychology and blurred the boundaries between the study of grammar and meaning. Whorf was not "against method," as Feyerabend would commend, but he did smudge it. In this section, I address the confluence and dissonance between Whorf's subjectivity and the ideals of objectivity structuring the science of language, showing how Whorf's status as the amateur specialist—a boundary *subject*, a desiring subject—enabled him to occupy the "linguistic" self and stretch its contours to their limits.

Within their configuration of objectivity, Daston and Galison emphasize the eye, or visual culture, as its locus, and emphasize how its image is materialized through the genre of the scientific atlas and routinized in the training practices and habits of scientists. While Whorf's contemporaries in linguistics employed atlases themselves—indeed, one of the first actions of the LSA was to assemble a Dialect Atlas of North American English, after similar projects took place in Europe[113]—the primary mode of their visual expression was phonetic transcription, which had yet to become sufficiently standardized.[114] As the above description suggests, linguists learned to employ a differently embodied way of knowing, relying more on the ear than the eye, and it is this *aural* objectivity that I want to stress (and revisit in the next chapter): to be a good linguist, you had to listen. Linguists, and phonologists foremost, trained their ears in a manner similar to the epistemic virtues of "trained judgement" that Daston and Galison describe. The phonologist was an active interpreter who learned to abstract from the acoustic signal of speakers an underlying

---

[113] The venture culminated with diminished scope in the first volume of the *Linguistic Atlas of New England* (Kurath et al. 1939).
[114] As I showed in Chapter 1, standardizing a phonetic alphabet for North American languages was a topic of debate among Sapir, Boas, Kroeber, and Goddard. Chapter 3 demonstrates how standardization continued to occupy George Trager and Bernard Bloch for much of the late 1930s.

phonemic inventory of their dialect. Linguists, like the experts Daston and Galison describe, had specialized training that enabled them to employ their own bodies as experimental apparatuses to apprehend patterns of sound; only they could cut through the noise to apprehend the signal, as I will demonstrate in the next chapter.

Whorf's passage from visual observation to aural apprehension characterized his entrance into the evidentiary framework of American linguistics. His early works on language demonstrated a strong visual emphasis: his interest in Semitic languages, for example, which he would have seen written but sparsely heard; or his study of hieroglyphics, in which he was the first to posit the term *grapheme* to describe and explain the visual likeness of Aztec and Maya hieroglyphics (1931). This emphasis would give way, over the course of his career in linguistics, to the study of sound systems or, much later, to the more abstract semantic category of "cryptotype" (discussed below). His shift from visual to aural evidence followed the transition within American linguistics itself from an interest in written to oral cultures.[115] A concentration on recording and archiving Indigenous languages set the American school apart from the philological tradition, which was based on the examination of literary evidence and, as such, focused on "high" culture. Unlike the languages of Europe, which already possessed rich corpora of written records, Indigenous languages belonged to predominately oral cultures and posed a different set of challenges to linguistic methodologies. The task of obtaining data to conduct linguistic research in the Americas also began to overlap with the conditions of doing field science. Questions arose over how to standardize transcription practices across this varied terrain;

[115] In the latter half of the 1930s, linguists reoriented themselves again toward the textual, developing writing systems to mitigate the conditions of fieldwork and better capture the phoneme. See Chapter 3.

new technologies, such as the phonograph (Brady 1999), complicated accepted practices that permitted linguists to employ their own bodies as instruments to apprehend phonetic data. The amateur specialist intersected with these important debates within the history of American linguistics over what model of objectivity—and hence what linguistic self—would best befit this situation. The different stages of Whorf's relativity hypothesis mark his degree of boundedness—or belonged-ness—to these methodological constraints.

Whorf's initial interest in philological work was to uncover the origins of human speech. He believed that the structure of the Semitic language family held the secret to the "possible original common basis for all languages" (to William Rich, 1928, *BWP*). While studying the morphology of Ancient Hebrew, he claimed to have discovered a "new scientific frontier" that would "throw light on the human mind" (to Schapiro, 6 December 1927, *BWP*): a set of relationships that united hundreds of Hebrew root words which, until then, had been considered unrelated.[116] He named this relationship "binary grouping" ("The Inner Nature of the Hebrew Language," *n.d.*, *BWP*): a "binary group" is "a group of Semitic roots . . . having in common a certain sequence of two consonants, containing all the roots with this sequence in one language, and having these roots with but few exceptions allocated to a few certain kinds of meaning" (8). In Ancient Hebrew, Whorf found 180 such groupings, which he believed to be the building blocks of meaning in Semitic languages—and, potentially, all Western languages.

Whorf recognized from the outset, however, that the scholarly and scientific world was "not ready" to receive such a contribution from "an unknown amateur" (to William Rich, 1924, *BWP*). In the intervening years before his SSRC-funded fieldwork, he had also

[116] Today, this is often called root-and-pattern, or nonconcatenative, morphology.

begun to study the Uto-Aztecan languages of Mexico, having learned of a collection of grammars and texts at Watkinson Library in 1926; in these, Whorf noticed a similar morphological phenomenon as in Ancient Hebrew. Through his work on Uto-Aztecan languages, he refined this theorem of binary grouping into the principle of "oligosynthesis," meaning "built out of a few parts":

> a name for that type of language structure in which all or nearly all the vocabulary may be reduced to a very small number of roots or significant elements, irrespective of whether those roots or elements are to be regarded as original, standing anterior to the language as we know it, or as never having had an independent existence, theirs being an implicit existence as parts in words that may have always been undissociated wholes. ("Notes on the Oligosynthetic Comparison of Nahuatl and Piman," 1928, *BWP*)

Whorf believed that these Indigenous languages would help him reveal "the primitive underlying basis of all speech"—hence "laying the foundations of a new science" and establishing his "authority in the field unshakeably" (to William Rich, 1927, *BWP*). In one of his first encounters with a Boasian anthropologist Paul Radin, however, Whorf's notion that these Indigenous cultures were "primitive" was promptly disabused (to William Rich, 1928, *BWP*). Whorf's ambitions—to find a common link among the languages of the world but also to establish his reputation as an expert—led him to linger at the threshold between hobbyist and academic, and to try to meet the moral and epistemic commitments of this scholarly discipline.

Nonetheless, in his early works, Whorf continued to amalgamate disciplinary frameworks, drawing correlations between linguistic structures and those of chemical engineering that were more familiar to him. Of Aztec, he described oligosynthesis in what amounted to a periodic table of meaning: 35 elementary units that he claimed represented "a map or plan of an actual realm of ideas." He expanded on this analogy between linguistic and chemical elements elsewhere:

> The isolation and study of roots and their manner of combining opens a certain new psychological field in which we can study the realm of ideas, hitherto an uncharted mystery . . . Almost as the chemist in his laboratory decomposes the substances of nature into their basic elements and observes the laws by which these few elements combine to yield this manifold variety of substances." (to Morley, 24 January 1928, *BWP*)

While Whorf did not imagine linguistic elements to have the same ontological substance of chemical elements, they possessed enough similarities to be put them into productive tension: "oligosynthetic languages seem almost like laboratories in which we might learn to analyze ideas almost in the manner of a chemist" ("Investigations in Aztec Linguistics," 1928, *BWP*). As Lily Kay (2000) defines, the "chimera" serves to neutralize the opposition between ontology and analogy (318): her definition underscores how knowledge about the world is subject to an interchange between nature and culture rather than the distilment of one from the other.

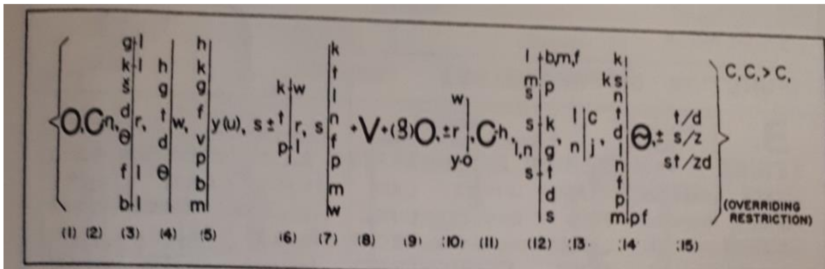


Figure 12. Structural formula of the monosyllabic word in English (standard mid-western American). The formula can be simplified by special symbols for certain groups of letters, but this simplification would make it harder to explain. The simplest possible formula for a monosyllabic word is C + V, and some languages actually conform to this. Polynesian has the next most simple formula, O, C + V. Contrast this with the intricacy of English word structure, as shown above.

Figure 7. Chem(er)ical Equation (Whorf 1940b/1956: 223)[117]

Whorf's interest in the communicability of abstract relations anticipated the structural objectivity of the Vienna Circle, whom he would later engage directly (see §2d). As Daston and Galison (2007) characterize it, structural objectivity indicated a "longing for a common world, and one that can be communicated, not just experienced" (301). Where the virtues of structural objectivity seek to flatten differences between the sciences through a common language of logic, the amateur specialist drew strength from their chimerical potential and delighted in their surprising percolations when straining one disciplinary logic through another. Never fully enrolled in the project of disciplinarity, Whorf was less confined by its constraining logics. Belonging to a greater scientific community—where disciplines as varied as chemistry and linguistics could produce unexpected cross-pollinations—was a recurring motif in Whorf's opus.

As his career matured, access to more linguistic data and the allure of mastery within the field prompted Whorf to draw sharper boundaries around his object of study. Whorf's descriptive work on Indigenous languages was shaped by his informal training

[117] "I relish an occasional chemical simile" (1941/1956: 236), Whorf would later declare.

with Sapir in the early 1930s, and the quality of his grammars was recognized both by Sapir and Boas. He published in the major journals of American linguistics and anthropology: for example, a comparative grammar of Uto-Aztecan in *American Anthropologist* (1935) and a grammatical analysis of Hopi verbs in *Language* (1938). The empirical certainty that linguistics offered figured strongly in Whorf's discourse.[118] His command of linguistic methods solidifying, Whorf also began making broader and bolder claims about his ability to find linkages between structure and meaning in the Indigenous languages he studied. In Maya, he observed, "the whole vocabulary is pervaded with relations between phonetic form and ideational content" (1930, *BWP*). In his report on Hopi, he likewise observed that "systemic symbolism" was typical (1936a/1956: 56); morphological aspect in Hopi verbs, for example, was an "illustration of how language produces an organization of experience" (55). Whorf's theory promised a mode of analysis that could discern the essential atoms of meaning from oligosynthetic languages, but he did not envision those atoms as a constant pattern, an unchanging tableau between world and linguistic system as structural objectivity would later entail. He described the development of Hopi—following the influence of Sapir's "thought-grooves" (1921/2004: 180)—as a process of sedimentation: "this Uto-Aztecan dialect has moulded, worn, and rubbed it into its present shape" ("The Hopi Language," 1935: 55, *BWP*). Languages inducted and ossified knowledge about the world in slow time over generations. In this way, they resembled experimental systems themselves. Language, Whorf believed, enacted in a "cruder but also

[118] He expressed his desire, for instance, to "light up the darkness, the thick darkness of the language, and thereby much of the thought, the culture, and the outlook upon life of a given community"; linguistics was the "glass" through which "true shapes" would appear, which had "hitherto been . . . the inscrutable blank of invisible and bodiless thought" (1936c/1956: 73). Through linguistic empiricism, Whorf perhaps re-staged the racialized semiotics of the "discourse of extinction" (Brantlinger 2003).

in a broader and more versatile way the same thing science does" (1936a: 55). Linguistics, as the science of language, operated as a tool—the master signifier or Rosetta Stone—to draw these systems together and make them commensurable. For Whorf, language was also "the great symbolism from which other symbolisms take their cue" ("On Psychology," *n.d.*/1956: 42), and linguistics gave form—as well as normativity—to these intangibles, where the dominant psy-disciplines could not.[119]

Mastery over linguistics thus offered Whorf a means to fulfill an early fantasy of his, of retrieving a kind of Tower of Babel—a quest for "the universal language, to which the various specific languages give entrance" (to English, 12 July 1927, *BWP*). Through the science of language, Whorf found a sufficiently communicable means to make claims about interiority with the clarity of an observable science. His desire for a universalism, however, was complicated in his efforts to apply his principle of oligosynthesis to more familiar languages. He developed a theory of "crypotypes" to supplement his analysis and extend it to less morphologically complex Indo-European languages where the interchange between morphology and semantics was less overt. A cryptotype was a hidden "complex" of relationships that operated as an invisible "central exchange" for meaning within a language—for instance, the category of gender in English, which undergirded the nominal system of the language but was not present grammatically except in the alternation of pronouns. The concept of the cryptotype revealed a "submerged, subtle, and elusive meaning, corresponding to no actual word, yet shown by linguistic analysis to be

[119] Whorf was especially critical of contemporary psychology and turned to "linguistic psychology" instead to connect experience with interiority (Whorf to Sapir, 13 June 1932, *BWP*). Behaviorism was uninterested in "human intangibles," he observed, such as matters of the "mind and soul" ("On Psychology," 1956: 41). Psychoanalysis, by contrast, dealt with mental life, but only works within the sphere of "the abnormal and the deranged"—and Whorf argued the abnormal was increasingly not "the key to the normal," and psychoanalysis's interest in intangibles "show[ed] almost a contempt for the external world" (42).

functionally important in the grammar" (1936c: 70). In essence, it was an extension of the phonemic principle—the patterned regularity of a sound system whose signal only linguists are trained to discern—toward a system of meaning. Through the cryptotype, Whorf wanted to examine the "linguistic side of *silent* thinking, thinking without speaking" (66; emphasis in original). It is here, perhaps, that we might detect the scandal of Whorf's empiricism: his commitment to observable truth was complicated by the requirement, indeed the necessity, of invisible cryptotypes to complete his analysis. Whorf anticipated the application of phonological methods for deriving abstract systems of relations to semantic categories—before there was an evidential framework ready to receive those calculations. Ultimately, the limits and play that had made his hypothesis productive in the first place gave way to his desire for greater generality, the longing to share in a common world—but also to be the singular genius who brokered it.

Whorf's ambitions therefore strained these emerging models of objectivity, and his colleagues and contemporaries often debated where to place him in relation to their ideals of method and mastery. Within these variegated responses, one common thread appeared that all would likely have agreed on—that Whorf possessed a brilliant but unruly mind. Sapir remarked, "He is sometimes inclined to get off the central problem and indulge in marginal speculations but that merely shows the originality and adventuresome quality of his mind" (to Kroeber, 30 April 1936, *AKP*). Amid the 1954 Language in Culture conference, Kroeber too pondered whether it was possible to integrate Whorf's metaphysical "forecasts" into the discipline's evidential regime: "Perhaps they are true insights. Perhaps they are true insights that can be verified. Perhaps they are true insights that can never be verified. Perhaps they are just verbal aberrations. I personally have always found them very interesting and very stimulating, but I do think they will be hard to prove" (Hoijer 1954a: 232). More conservatively, Stanley Newman asserted a need to diminish the "intuitive aspect" of Whorf's approach (232). By contrast, Charles Hockett endorsed Whorf's unwieldy curiosity, understanding the merits of his observations as representative of a deeper anthropological legacy:

> Whorf sat in New York for many, many hours and worked with a native speaker of Hopi [Ernest Naquayouma], who was a native of Hopi culture. Whorf talked with him in English, got Hopi words, got Hopi texts, asked all sorts of questions about the meanings of these words, asked how to say this, that, and the other. He was presumably aiming toward a linguistic description of the Hopi language, but *he got all sorts of other reactions from his informant*. This underscores the point I was trying to make earlier—that linguists and ethnologists actually go through many of the same operations, ask the same kind of questions, and watch for the same subtle clues on the part of their informants. (230; my emphasis)

Hockett lauded Whorf's deductions because they were not derived purely from a linguistic system. In his way, Whorf was a superb listener, attentive not only to his informants but also, as I will show here and in the following section, to different audiences, academic as well as lay. He listened to other fields, other ways of thinking, and sought to apprehend and absorb an array of concepts into his own work. Fungibility was characteristic of Whorf's attitude toward the mixture of scientific expertise and personal development, but it also typified the commensurability he imbued the forms of knowledge within and between those realms of expertise. Whorf's promiscuous epistemologies, replete with transient and transposable connections, characterized his way of thinking and formed the basis of his theory of linguistic relativity.

These intuitive leaps also led to what many consider the biggest gaffe of Whorf's career. In the memorial volume for Edward Sapir, Whorf cited an encounter with "empty gasoline drums" at his fire insurance inspection job as the initial spark of inspiration for his linguistic relativity principle (1939/1956: 135). In this example—infamous to some, bathetic to others—Whorf instructed his reader in one way that language might inform experience: the connotations of the word *empty*, in this case, belied the danger of the hazardous gas inside. In a prescient moment, given the example's notoriety now, Whorf thought to offer a clarification:

> I have thought of possibly adding a brief statement or a footnote saying that I do not wish to imply that language is the sole or even the leading factor in the types of behavior mentioned, such as production of fire-causing carelessness through misunderstandings induced by language . . . It didn't seem to me at first that this should be necessary if the reader uses ordinary common sense, but then one can never tell.
>
> (Whorf to Leslie Spier, 23 November 1939, *BWP*)

Indeed, a disclaimer may have helped: in Whorf's absence, the meaning of the gasoline drum example moved beyond the realm of common sense to that of caricature. Ridiculed by some today as an instance of Whorf's exaggeration of language's influence, I understand this failure to convey the intended meaning as emblematic of Whorf's insider-outsider perspective. Here and elsewhere, Whorf drew on his everyday experience and background in chemical engineering and blended it with the evidential framework he found in linguistics. He freely employed the genre of taxonomic classification in his fire insurance career ("A Suggested Classification of Insurance," *n.d.*, *BWP*) and, by the same token, extended concepts from fire safety inspection into his theory of linguistic relativity (the gas drum). The reception of the latter example demonstrates the jeopardy of not—or no longer—belonging to a set of established witnessing practices. Its capacity for miscommunication represents the gamble of any claim to knowledge—or to a scientific identity—whose risks a disciplinary matrix serves to contain. It is the inverse parable of the gasoline drum: the fear that a vessel thought full and secure would be suddenly emptied and incoherent. Whorf longed for the ethos of a scientific identity but not its boundedness—the nascent linguistic self was overwhelmed by other directions of torque, the amateur specialist imploding the "concave and convex" relationship of objectivity and subjectivity (Daston and Galison 2007: 197).

Whorf thus appears as a figure akin to Icarus (to Sapir's Daedalus), whose apparatus dissolved as he endeavoured to reach that radiant light of completion. In his quest for a system of knowledge that offered such transcendent vision, however, contradictions built up. As Donna Haraway (1988) cautions, no scientific system can offer perfect sight. The promise of linguistics, for Whorf, figured as this illuminatory realm of success, a space of belonging that would elevate him above the ordinary. Where there is light, there must be shade. As evinced by his attitude toward the expertise of informants in one of his final publications, however, that promise would prove emancipatory only for the few who could master it. In one of his last publications, Whorf described paid informants as the experimental "animals" of linguistics: "They are apparatus, not teachers" (1940b: 231). His theories only gained wing as a function of a structural imbalance between him and his informants, one that squelched Indigenous self-determination over their own language. At the moment he was thus entombed by success, Whorf limited the scope of his egalitarian principles in order to patrol the boundaries of scientific expertise, to which he himself held a tenuous claim. The not-yet-disciplined field of linguistics had indulged his fungible, associative—noisy—thinking, giving him a platform to perform the role of a scientist but also to maintain the perspective of a generalist, seeing patterns everywhere. His position outside the academy offered him an indeterminate space in which to fail and try again—but he lacked the resolve to extend these advantages to informants who, like him, participated in scientific work exterior to the university.

Whorf—through his irregular training, independent employment, and imaginative insights—was no hero of objectivity. If retrospective accounts are to be believed, he may even have been a villain, a splinter on the tree of knowledge. In a sense, my analysis resumes the popular interpretation that Whorf was not a *good* linguist: he resisted, or had yet to acquire, the boundedness of a strict "scientific self"—but because for him there wasn't one. During his lifetime, linguistics debated the terms and conditions of what constituted a credible knower: financial resources, academic affiliation, training procedures, and subjectivity were each remoulded and reconfigured. For Whorf, the promise of linguistics lay not in its contracting research culture, but in the open-endedness that allowed an amateur specialist to thrive in the first place. In this way, Whorf's fate within the popular imaginary mirrors the transition of linguistics from inchoate discipline to professional science, a transformation necessarily accompanied by a closure over who could and could not belong—of whose genius would, from then on, be dashed.

❖ Whorf—A Failed Genius.

In the previous sections, I considered how Whorf the "amateur specialist" cut across and through disciplinary matrixes. Exploring the contours of this seeming paradox, I examined how Whorf figured as both exemplar of and exception to a burgeoning disciplinarity—his identification with linguistics simultaneously full and empty. Here, I follow Whorf as he sought belonging not only in the domain of the professional scientist, but as well in the role of public intellectual, envisioning for himself and other linguists a moral responsibility to inform and guide the American public. Whorf was not only a practitioner but a scientific popularizer—and may even "be credited with being the first popularizer of modern linguistic science" (Carroll 1956: 28). Whereas Sapir's more popular works courted potential audiences for linguistic expertise in the other and better-established social sciences, notably psychology and sociology, Whorf made a concerted effort to bring their scholarship "to the notice of a wide public as well as linguists, anthropologists, and psychologists" (27). Like his contemporaries Ruth Benedict and Margaret Mead, who tried to make cultural complexity palatable for a broad American audience (Benedict 1934, Mead 1928), Whorf was eager to mobilize the moral imperative of Americanist anthropology and linguistics as a salve for the melting pot of modernity.[120] In his publications for the *M.I.T. Review* in particular, Whorf endorsed a relativistic stance that celebrated the differences between languages and the worldviews they appeared to structure—differences which were not irreducible, but made commensurable through the work of linguists. The notion that each language embodies a unique cultural perspective made sense as part of a rationale to document the threatened languages of Indigenous

[120] Peter Mandler (2013) speaks to the cultural history of Mead's work in the American imaginary.

peoples. It takes on different significance in the context of Whorf's efforts as a popularizer, revealing not only his sensitivity to a prevalent ethnocentrism within the academy and without, a dis-ease he shared with Sapir, but also his wavering between the ambivalent poles of moral arbiter and the disinterested professional, a partition along which scientific careers in the early twentieth century pivoted (Shapin 2008).[121]

In the first section below, I dwell on Whorf's attempts to fashion himself, not as he was posthumously consigned—as the "amateur specialist"—but rather as a public genius, of the same kind if not caliber as Albert Einstein. Though he generated limited success through local publications and talks in Hartford, Connecticut, his efforts to capture the attention of national periodicals were resolutely thwarted and, as his work came into recognition toward the end of his life, he faced unexpected, sometimes undesirable, uptakes of his work. The second section deals with the latter, showing how Whorf's linguistic relativity principle competed with other efforts to master the science of meaning; these debates belonged to a growing nexus of conflicting ideals interlacing perceptions of scientific genius, the nature of science, and the moral stature of the scientist—and control over language appeared as the centre of gravity.

---

[121] Attempts for scientists to appear "disinterested" represent an epistemic virtue as well—and, as such, are not actually disinterested. Donna Haraway (1997) explores this calibration of the body that erases its involvement through the figure of the "modest witness." Modesty renders the self invisible in an effort to be objective and get at the thing-in-itself. Stressing that this mode of objectivity is really the subjectivity of the modern, European man, Haraway argues that the modest witness is marked by the exclusion of women, both physically and epistemologically. She underscores that contemporaneous STS theory (by Latour, in particular) does not sufficiently remediate this exclusion and communicate with feminist, postcolonial, and other oppositional literacies.

c. Calibrating—the Public Intellectual.

To claim Whorf as the first popularizer of linguistic science demands some qualification and contextualization. What constitutes the "public" is not a given, nor is its relation to the sciences. Rather, as social and political scientists, historians, and STS scholars have shown, this relationship is in continual flux, re-emerging through different media ecologies, cultures of expertise, and perceived utility (or tractability) of scientific discoveries.[122] The notion of a public science conveys a sense of the "commons"—both a common experience or interpretation to communicate and a common ground on which to do so—but each, historically, has been greatly circumscribed, often belonging only to a property-owning white middle class (Habermas 1991). This middle class was largely a reading public and, from the onset of the nineteenth-century communications revolution (Fyfe 2012), potential audiences and opinions on science broadened and multiplied. Out of these conditions arose a need for intermediaries—science writers, journalists, publishers, and so on—among whom scientific popularizers acquired a significant role (Lightman 2007).[123] Scientific popularizers often held their own motives. They communicated heterogeneous modes for understanding science (rather than merely simplifying results, as a diffusionist interpretation suggests), sometimes with agendas at odds with those of scientists

---

[122] In Europe, the movement of science into public life and the advent of public opinion about science are often linked to the convergence of industry, intellect, and interest beginning in the seventeenth and eighteenth centuries (Cook 2007, Stewart 1992), when a growing middle class found opportunities for commercial gain and material comfort in scientific inventions and novelties. In a parallel course, an extension of experimental practices and their networks similarly induced junctures with public knowledge (Golinski 1992) and, by the nineteenth century, technoscience had taken on an increasingly powerful role in the operations of society. As Proctor and Schiebinger (2008) reveal, however, these networks have also been used as a platform to produce ignorance, obfuscation, and the suppression of knowledge.
[123] It was not until the twentieth century that "popular science" took on its pejorative meaning (Lightman 2007: 11).

themselves.[124] The ascent of popularizers figured into a broader tradition of science as spectacle. Performance had long been an integral part of public understandings of science, from the gendered and classed rhetorical strategies employed in early-modern scientific self-fashioning (Biagioli 1993, Terrall 1995) to the staged replication of experiments (Shapin & Schaffer 1989). But as Roger Cooter and Stephen Pumfrey (1994) argue, the outgrowth of mass culture radically altered the "means and meanings of scientific display and communication" (239). Not only was there an expansion of surfaces on which to reach and affect large publics (Kittler 1999, Mitman 1999, Orr 2006), with new media such as film rupturing the discursive space of an erstwhile republic of letters, but there was also the establishment of a new personage: the celebrity-scientist. Few exemplify this figure better than Albert Einstein, who "exploded into popular consciousness in 1919" and achieved celebrity status through the work of the American press (Fahy 2015: 2). Penny Lee (1994, 1996) has detailed the influence of physical relativity on Whorf's thinking, but in Whorf's lifetime, there was more than relativity in the air, and his efforts to popularize a scientific understanding of language and fashion himself as an intellectual must be understood in relation to this public character of genius—and his failure to obtain it.

Whorf had diverse publics in mind from the beginning of his linguistic studies. Newly a member of the LSA, Whorf received an SSRC Grant-in-Aid in January 1929 (for $920) to conduct research in Central Mexico to study and record modern spoken Nahuatl, an Uto-Aztecan language. In his 1928 SSRC proposal, he set a number of (ambitious)

[124] According to the "diffusionist" perspective, popularizers were akin to colanders through whom the correct uses and interpretations of science could "trickle down" from expert to laity; other analyses have pushed against this top-down model, arguing for a view of the sciences "from below" (Cooter and Pumfrey 1994, Harding 2008).

outcomes for this trip, including an Aztec grammar, a compact Aztec-English dictionary, a comparative analysis of Nahuatl and Piman, and a discussion of the relationship between oligosynthesis and behaviorism.[125] These stated goals were indicative of the academic audiences he hoped to reach: anthropologists, philologists, psychologists. What is not seen here, but evident elsewhere in his correspondence, was Whorf's interest in reaching non-academic audiences. Having had his self-funded work on ancient Aztec covered in *The Hartford Times*, Whorf sought on at least two occasions to publicize his latest research endeavour through nationally distributed newspapers. In April 1929, he contacted Barrow Lyons, a representative of "Science Service" at the *New York Evening Post*, with clippings from his work on two rare Aztec manuscripts. Later that year, he contacted Merritt Bond, of the *North American News Paper Alliance*, hoping to "furnish a series of weekly articles during [his] stay of two months or longer in Mexico and Yucatan, written on the spot about the people, the country, and [his] experiences" (21 November 1929, *BWP*). Both offers were rejected. While Lyons was diplomatic in his refusal, Bond replied flatly: "I see nothing in the stories you enclosed which will be of sufficient interest to justify our sending them to our membership" (Bond to Whorf, 5 May 1930, *BWP*). Ultimately, Whorf's only recognition in a national periodical was his obituary in *The New York Times* (1941), which made no mention of his contribution to linguistics.[126]

Nevertheless, we can discern from these failures more than a lack of interest in Indigenous languages and culture (though I will return to this topic below). Indeed, even

---

[125] Concepts from behaviorism were of "marked help" in shaping this concept, he wrote, but "behaviorism in turn must undergo modification in light of these phenomena of language" (1928 SSRC Research Fellowship Application, *BWP*).
[126] Whorf's close friend George Trager later wrote a letter to the editor of the *Times* to correct their oversight (29 July 1941, *GTP*).

the LSA at this point was endorsing the Dialect Atlas of American English over the cataloguing of Indigenous languages (Murray 1994: 142). Tracking Whorf's changing tactics after each rejection, rather, offers insight into his understanding of the relationship between anthropological linguistics and its potential publics, as well as the role he anticipated for himself as a popularizer. In his letter to Bond, for instance, perhaps hoping to rectify the response he had received from the *New York Evening Post*, Whorf made clear that his research trip was not merely some excursion but a "planned scientific *project*" (21 November 1929, *BWP*; Whorf's emphasis), as scientifically interesting as "big expeditions that require fortunes in equipment, apparatus, and personnel before they can start." Whorf employed a similar strategy two months later, when he turned his attention toward the private sector. He asked the Victor Talking Machine Company to "furnish a small, compact recording phonograph and supply of wax blanks such that [he] can carry it in [his] camp kit and use it in recording words and sentences in Aztec and Maya" (to Maxfield, 4 January 1930, *BWP*)—a donation needed, since his Grant-in-Aid would only cover costs of travel. He described this work as essential not only because of its "value to science but also on account of the publicity possibilities."[127] Here, we can make out his efforts to recalibrate his pitch, emphasizing his study's contribution to science (and science's allure to a wider public) as much as its appeal to curiosity over different cultures. While the mobility and affordability of this human science were prevalent in the first instance, in the second we glimpse Whorf's negotiation of the tools necessary to make his ephemeral phonetic analysis more enduring (and hence, perhaps, more befitting of a science in his mind, though the phonograph was not a standard tool for the linguist at this time). We can also

[127] Whorf is confident that universities and museums will want reproductions of these recordings.

make out the fault lines starting to form in the bifurcation of scientific and humanistic interest in language, echoed in the concurrent professionalization of the field.

Whorf's intended narrative for these unwritten articles might be reconstructed, in part, from his letter to Bond. Though Whorf ensconced his request in an appeal to the ethos of science, a closer inspection reveals only a loose attachment to the epistemic virtues of anthropology or linguistics. At this early juncture of his career, perhaps more the amateur than the specialist, Whorf's pitch broadcasted self-promotion more loudly than any particular scientific understanding of language or culture he hoped to convey. Indeed, he quickly downplayed the tenor of scientific input in the prospective series, perceiving his role as popularizer more along diffusionist lines:

> What comparatively little scientific matter went into them would be popularized, and diluted with human details, dramatic incidents, and exotic local color. The tone would be personal, adventurous—light, and yet with a touch of the mystery and drama that are inseparable from Mexico and especially from the aboriginal Mexico that survives among the Indians of the mountains. I would seek to bring the Mexican scene vividly to the eyes of the American readers and make them feel the spell of the strange environments in which I am going to be. (to Bond, 21 November 1929, *BWP*)

Whorf was clearly informed by the Americanist tradition, intending to structure his accounts in a way that resembled anthropological field journals: "In these articles I shall speak of their home life, their amusements, their women and their position and influence, dress books, traits, customs such as those relation to courtship and marriage, loves songs; the children, the young maidens, the folklore…." Notably, however, Whorf placed an emphasis on his own experience of events: "How will my classical Aztec be intelligible to the Indians from the start? Will my accent be terrible? What will the Indians think of a white man coming among them and knowing anything of their language at all?" Rather than introduce the Native peoples as potential collaborators, a cornerstone of the Americanist tradition (Darnell 2001: 17), Whorf's construction of their experience for a white middle-class audience would have been immensely ego-centric. Whether this proposal reflected his honest perspective, or whether he simply underestimated his readership, remains unclear.

Regardless of the authenticity of the viewpoint Whorf put forward, his letter to Bond nonetheless characterized the kind of intellectual persona he envisioned for himself and how his investment in linguistics might help him achieve it:

> I happen to be going into a field that is new and that promises large scientific returns, but where there is really only one absolutely essential piece of equipment needed, which requires only one man to work it, and is light, inexpensive, and very easily transported, because a man can carry it about under his hat. This equipment is a working knowledge of the Aztec language.

The draw of the language sciences, as he presented them here, was multifaceted. There was the novelty of it, the association with a pioneering spirit. The allure also lay in its methodology: its techniques for employing the linguist's own mind as a "laboratory" for phonetic analysis, which implied careful training and self-discipline to find patterns in human behavior. Linguistics had no need of a formal laboratory setting to follow through on the promise of research.

The wording of the passage, moreover, suggests a tug from outside the narrower disciplinary formation. It recalls the synecdoche of mind-brain for genius-level intellect, for which Einstein in this era was the archetype. This linkage surfaced earlier that same decade in the initial media coverage of Einstein's arrival in America, where he was depicted as a "man in a faded grey rain coat and a flopping black felt that nearly concealed the grey hair that straggled over his ears . . . underneath his shaggy locks was a scientific mind whose deductions have suggested the ablest intellects of Europe" ("Revolution in Science" 1921). For many, Einstein epitomized the figure of the singular genius, a fixture of twentieth-century imaginaries about scientific success. The scientific genius not only embodied prevailing ideals of what a scientist should be—but, through the unique qualities of their mind, also exceeded them.[128] In Einstein's case, he was conceived at once of fulfilling the virtues of the ascetic intellectual and disinterested professional (Merton 1942, Weber 1919), working in solitude, divorced from social and material contexts, and of transcending those categories through his individuality and creativity. This image was propagated through print, film, cartoons, and other media during Einstein's life and beyond. Though intuition and imagination were by this time qualities growing alien to the context of scientific justification, they were nonetheless admitted in the context of discovery—when they appeared in-born in the body of the genius (Radcliffe 2008: 64), specifically the brain.[129] Whorf's address to Bond thus disclosed his yearning to fashion himself within the

[128] Part of Einstein's fame in 1921 came from the recent confirmation of his predictions. A theory forged in the mind in 1915 was confirmed spectacularly in the big solar eclipse of 1919.

[129] Roland Barthes (1972) comments on the persistence of Einstein's brain as such an object of curiosity after his death, indicating how, at the same time as it serves as a paragon of human acuity, it is rendered nevertheless in dehumanized terms: "The mythology of Einstein shows him as a genius so lacking in magic that one speaks about his thought as of a functional labour analogous to the mechanical making of sausages, the grinding of corn or the crushing of ore: he used to produce thought, continuously, as a mill makes flour, and death above all, for him, the cessation of a localized function: '*the most powerful brain of all has stopped thinking*'" (68–69; emphasis in original).

same mythos, situating the locus of genius "under the hat" of the linguist, in the brain—his brain.

At the outset of his career as a popularizer, Whorf attempted—and failed—to install himself within the iconography surrounding Einstein. To better understand those unsuccessful performative acts, it is helpful to analyze the conditions of the felicitous ones he sought to imitate. Besides advancing theoretical physics, Einstein's theory of relativity functioned within American narratives of self-improvement and upward mobility (Missner 1985): depicted as it was in popular media, as fully comprehensible only to a select group of scientific experts but littered with strangely familiar concepts (such as a "fourth dimension"), Einstein's relativity offered a means of belonging among the best and brightest intellectuals of the Western world. Drawn into momentary identification with the celebrity-scientist, an experience irrespective of embodied conditions, readers could be rescued from "the humdrum of a daily chore," to borrow an aphorism from Whorf's obituary. Moreover, the success of Einstein's image helped manage questions over how to contain the figure of the genius, for so long understood as essential to the ideal of scientific progress, at a historical moment when the scientist was "on the cusp" of "moral ordinariness" (Shapin 2008: 46); the genius offered a site to navigate the uncertainties accompanying this newfound scientific self, representing crucial exceptions that could preserve qualities in process of being evacuated from the role of the scientist but which nonetheless remained essential to the perceived operations of science.

One of Whorf's later works, a manuscript unpublished during his lifetime, demonstrated how his rhetoric and his treatment of Indigenous experience transformed in response to his position amongst the inner circle of American linguistics and, perhaps, his failure to attain celebrity status for himself. Where his earlier unwritten projects would have described Indigenous language and culture through the lens of his personal experience, "An American Indian Model of the Universe" (published 1950, by Trager, but written in 1936) introduced the Hopi worldview as a similarly constructive challenge to Western metaphysics as Einstein's relativity principle: "the Hopi language and culture conceals a *metaphysics*, such as our so-called naive view of space and time does, or as the relativity theory does, yet a different metaphysics than either" (58; Whorf's emphasis). Where Whorf had once shown diffidence or indifference, here he demonstrated a stronger commitment to the epistemic virtues of anthropological linguistics, specifically Sapir's injunction to describe a language as much as possible in its own terms, rather than projecting Western categories:

> In order to describe the structure of the universe according to the Hopi, it is necessary to attempt—insofar as it is possible—to make explicit this metaphysics, properly describable only in the Hopi language, by means of an approximation expressed in our own language, somewhat inadequately it is true[.] (1936b/1956: 58)

Whorf understood the Hopi as having no sense of time as a continuous flow—no words or grammatical functions to correspond to the typical Western understanding of temporality, where time and space are conceived as separate and unconnected (59). Instead, he proposed the Hopi held a "kinematic" worldview, wherein the category of "time" folded into spatial language. In his fervor to draw out differences, Whorf perhaps overstated that the "Hopi language gets along perfectly without tenses for its verbs" (64), when in another document he had noted three tenses in the language (1936a). What was evidently more important to him, on this occasion, was the winnowing he witnessed in scientific modernity. What the "modern western scientist" deemed "mystical" in the Hopi, Whorf stressed, was indeed "justified pragmatically and experientially" (1936b: 59); despite its underlying differences, the Hopi language could correctly account for any temporal event an Indo-European language could, "in a pragmatic or operational sense" (57). Thus, Whorf planned to model a form of scholarship that incorporated metaphysical and scientific explanation; his stance was less individualistic, reflective instead of his newfound responsibility as Yale lecturer, upon Sapir's declining health, and his hopes to continue his mentor's legacy and refashion how his peers could exhibit linguistic insights into culture.

Whorf's career as a popularizer of linguistic science was not comprised only of failure. He found his audience, at long last, a year before his death. In 1940, two publications in M.I.T.'s *Technology Review* ("Science and Linguistics" and "Linguistics as an Exact Science") demonstrated his ability to reach institutional publics, in particular. In a letter to *Reader's Digest*, endeavoring to get the first article republished, he claimed that it "attracted rather wide attention" and received enthusiastic responses from a variety of sources—English professors, business executives, physicians, and more (3 September 1940, *BWP*). These works likely influenced Thomas Kuhn's (1962) *The Structure of Scientific Revolutions*, which cites Whorf in its introduction. Elsewhere, relativity enabled Whorf to make the achievements of linguistics legible to an uninitiated readership: "Discovery of the phonemic principle made a revolution in linguistics comparable to relativity in physics" (Whorf qtd. in Lee 1994: 176). In his papers for the *Technology Review*, physical relativity—specifically, the calibration of observers—proved integral to the development and uptake of own theory.

As Penny Lee (1996) elucidates, Whorf borrowed indiscriminately from the models of physical science, molecular chemistry, and gestalt psychology—but calibration, it seems, was as crucial a concept for Whorf as it was for Einstein, though in different ways.[130] In the first essay for the *Technology Review*, Whorf presented "a new principle of relativity, which holds that all observers are not led by the same physical evidence to the same picture of the universe, unless their linguistic backgrounds are similar, or can in some way be calibrated" (1940a: 214). Calibration was useful to Whorf in two ways. In the first, it offered a conceptual platform from which he could extend the phonemic principle to the study of meaning: where the acoustic range of a phoneme in any given language could be quite diverse and impossible to determine without indexing the expertise of a native speaker of that language, Whorf drew on a similar logic to construct subjective experiences informed by internalized linguistic systems (Lee 1994: 176–177). These experiences, too, realized the same underlying pattern as the phoneme, often unnoticed except to the discerning ear. Through this process of calibration, Whorf believed linguistics could function to decentre "rationalizing techniques" derived from Indo-European languages "as the apex of the evolution of the human mind" (1940a: 218). Whorf described his formulation of relativity as not "strictly rational," but based on principles of empirical observation: how the world came into focus through a "kaleidoscopic flux of impressions" organized by the observer. Calibration offered, for him, a means to appreciate the diversity of language and experience but maintain the spirit of "curiosity and detachment" characteristic of the empirical scientist (219).

[130] Peter Galison (2000), for one, stresses the material circumstances of Einstein's theory of relativity, showing how it was responsive to a widespread fascination over "electrocoordinated time" and the matter of synchronizing trains and clocks across great distances.

Secondly, calibration was a concept readily translated into a terminology already endemic to linguistics: agreement. Lee (1996: 228) describes three meanings of agreement at play within Whorf's work: (1) the checking of statements against one another; (2) the implicit agreement embodied by a shared language (the closer the dialect, the easier to calibrate); and (3) the modification of our internalized system against others' in order to maximize communicative effectiveness. Agreement, Whorf asserted, is "reached by linguistic processes, or else it is not reached" (1940a: 212). It is easy to see how the "strong" interpretation of the linguistic relativity hypothesis (known as "linguistic determinism") might be assumed from the tenor of these statements, but perhaps the occasional boldness of Whorf's claims can instead be understood to imbibe the conviction he felt for linguistics, his apprehension toward the shifting responsibility of the sciences in modern life, and his desire for a wide audience to take seriously an amateur specialist's conjuncture of science and salvation.[131]

It was these concerns, I contend, that motivated Whorf to conceive a special role not just for himself, as a singular scientific genius, but for the group he identified as "scientific linguists." Whorf believed scientific linguists (or elsewhere, "theoretic linguists") were best positioned to study the systematic tacit knowledge conferred by language, make solvent the constraints of grammar, and therefore broker agreement about the world (1940a: 211–212). He distinguished scientific linguists, here, from a linguist in the more general sense: someone who knows a lot of languages. The basis of this boundary work would become

[131] Whorf early on sought linkages between scientific and exegetical practices, as he expressed to his cousin: "I had begun to see that the cosmology of the Bible . . . could be so interpreted as to combine with the ideas and discoveries of our science to produce a philosophy of a very grand, elevating, and religious sort" (Whorf to William Rich, 1923, *BWP*).

clear in his next article. In “Linguistics as an Exact Science,” Whorf elaborated on the role he envisioned for the scientific linguist, presenting the democratization of scientific logic as one of their major contributions.[132] He framed the paper as an introduction to phonetic formulae for the “philosophical and mathematical analyst who may try to exploit the field of higher linguistic symbolism with little knowledge of linguistics itself” (1940b: 222–223). Whorf drew strong comparisons to the formal sciences, likening linguistics to mathematics: “The exactness of this formula, typical of hundreds of others, shows that, while linguistic formulations are not those of mathematics, they are nevertheless precise” (230). He also drew comparisons to the experimental sciences, like chemistry: “Its data result from long series of observations under controlled conditions, which, as they are systematically altered, call out definite, different responses” (230). These comparisons helped lay the groundwork for him to pursue the idea that language was the basis of scientific agreement to one of its possible conclusions. The “scientific use of language,” Whorf asserted, “is subject to the principles or the laws of the science that studies all speech—linguistics” (221). He believed that the worldview of modern science arose from a “higher specialization” in the grammar of Indo-European languages, but maintained that scientific results were not “caused” by a grammar—they were “colored by it” (221). Indeed, becoming “semiconscious” of the patterns of language, he argued, removed their binding power (225). His understanding of the role of language in science was not deterministic but—as was the role of linguistics in his own life—emancipatory. Emily Schultz (1990) argues that Whorf emphasized the constraints of grammar because he felt he needed to

[132] In a later paper, Whorf argued that the sciences themselves had the potential to become “mutually unintelligible” dialects (1941b: 246; emphasis removed).

break the “false consciousness” of American liberal individualism and disrupt the “monologic” language of science. Through the figure of the scientific linguist, we glimpse how he imagined this might be. Rather than imagine a society based on “multiple forms of belonging in difference” (Muñoz 2009: 20), Whorf’s utopic vision construed a singular solution, employing linguists to solve problems of difference through agreement.

Sometimes, as Lily Kay (2000) demonstrates in her masterful analysis of the mapping of information discourse onto the life sciences in the mid-twentieth century, the pathways of scientific exchange fall along lines of sustained analogy. Kay describes “catachresis” as the basis of these transfers—a metaphor of a metaphor, a signifier without a referent, capacious enough to invest a range of meanings but constrained enough to be productive. Relativity, for Whorf, formed such a catachresis. For Whorf, the patent office where Einstein worked or the coordination of times across vast expanses were far less immediate concerns than the conditions, rather, of the latter’s fame and its influence on the public understanding of science in America wherein he sought to make a name for himself and, later, for other linguists. In relativity, Whorf found a language that could reach a public already inundated by Einstein’s iconography and, in linguistics, a meta-language he believed could solve the problems besetting modernity and avert the narrowing commitments of the sciences. If Einstein’s relativity “matched the mood of uncertainty that followed the savagery of World War I” (Fahy 2015: 4), Whorf’s efforts to attain the status of public intellectual and popularize a linguistics centred on difference responded, in turn, to his desire to acknowledge a world spun out by centrifugal forces and recalibrate a common frame of reference therein. In my reading, however, Whorf ’s relativity was not premised on radical difference or the incommensurability of language and experience, as many interpretations suggest, but instead on their resolution into agreement—a containment of difference that in effect reaffirmed the indefatigable spirit of Western science, with linguistics, its greatest champion, operating as a meta-science capable of fostering agreement among its peers. The makings of Whorf's relativity emerged in this other context, amid debates over the nature of science, the role of the scientist, and the value of experience and observation, wherein language was an important vehicle. There, he was not alone.

d. *Between Semantics—and Pseudoscience*.

Previously, I addressed Whorf's minor works, which dwelt on the difference of Indigenous experience in his efforts to cultivate the persona of a scientific genius, and his more renowned papers, communitarian in their focus on the construction of the "scientific linguist" on similar grounds. In the latter case, however, he met with competition. The science of language, in its inchoate disciplinarity, had yet to solidify its cultural or epistemic authority. Though by this point in Whorf's lifetime the field had begun firming its boundaries against the contribution of non-experts, relying less on amateurs and instituting stricter guidelines for garnering evidence from informants, it had still to negotiate with other organizations—some intended audiences or potential allies, others not. As a potentially marginal figure himself, Whorf perhaps had a heightened sense of these boundaries and felt the need to enforce them at both ends: against the expertise of native speakers—his experimental "apparatus"—on the one end, and against other philosophies of language on the other. In this section, I show how Whorf's efforts to demarcate a strict role for linguistic scientists responded directly to the challenges posed by the contemporaneous logical empiricism movement and the Institute of General Semantics—each brandishing its own illumination of the science of language and meaning, each vying for control over its ethos. By triangulating these coordinates, I historicize a broader discourse on the role of language in uniting an increasingly specialized, internationalized science. Whorf shared in an imaginary that the science of language could be employed as a meta-science, a tool to reorient dispersed fields of inquiry and reverse a burgeoning "scientific babel" (Gordin 2015); at stake in this boundary work was not just Whorf's career as a popularizer, but the future of science itself.

In his inaugural address for the history of science journal *Isis*, Robert Sarton (1924) captured the urgency of the time to repair a fractured science. Extoling the virtues of a general education in science and the humanities, the article addressed the increasingly tapered vision that scientific specialization entails. Despite science dividing into branches, he argued, there was nonetheless an enduring "unity of science" (10)—an indivisible whole that was also, to him, a reflection of the utopian potential of humanity: "Unity of knowledge and unity of mankind are but two aspects of one great truth" (11). Like Whorf, Sarton believed that science was the "common thought of the whole world" (24), the only domain where "all have equal rights" (13). Whorf's vision, above, for a unified science was also a commentary on a branch of rational thought—logical empiricism—whose expression depended, he believed, only "on laws of logic or reason" and thus represented a way of distilling reality that was agnostic to the situation of the observer (1940a: 208). To fully grasp the extent of this disagreement and language's fixture within competing visions for science and the future, I must flesh out Whorf's encounter with this school of thought.

The *Cambridge Companion to Logical Empiricism* (2007) provides important contextual information on the initiative, its theoretical commitments and its major works. The publication "The Scientific Conception of the World" (1929) by Hans Hahn, Otto Neurath, and Rudolf Carnap, inaugurated the public phrase of the Vienna Circle, which proceeded to spread internationally in Europe through French and English networks (Standler 2007: 14). The term "world-conception" (in contrast to "worldview") conveyed a broadening of scientific method to other intellectual and practical domains. It was founded on the application of logical reasoning to empirical experience—hence the labels "logical empiricism" or "empirical rationalism"—and was meant to counteract overspecialization in science, particularly in scientific language.[133] The authors conceived of logical empiricism as an intellectual tool derived from and embedded within the everyday life of the scientist and, as a consequence, a means of consolidating the diverse "mosaic" of knowledge in a common sediment.[134] Their goal of a unified theory of knowledge was reinforced by the Unity of Science movement, promoted by Neurath, Carnap, and Charles Morris between 1934 and 1941 in North America (18), the same years Whorf was most active as a scholar. As part of this movement, logical empiricism found a warm welcome with New York City intellectuals and established a second base of operation in Chicago in 1935 (Reisch 2007: 61), in addition to their other in The Hague. However, the advent of World War II, in addition to Neurath's death in 1945, put a stop to their momentum. Within the ensuing political culture of Cold War America, the prospect of unifying science held uncomfortable

[133] Neurath (1955: 5–10) explained history of how empiricism became separate from "philosophico-religious constructions."

[134] Unlike Sarton, above, who conceived of scientific knowledge-making as a "tree" or Korzybski, below, who modeled it after the human "nervous system," Neurath (1955: 3) understood the history of empirical science as a "mosaic," with scientists inlaying and changing its pieces as they built it up.

associations with totalitarianism (71), and it met with a "climate of fear" that paralyzed leftism in American universities (Schrecker 1986), resulting in another failed project of scientific modernity.

The flagship undertaking of the Unity of Science movement was the *International Encyclopedia of Unified Science*, though only the *Foundations* (Neurath et al. 1970) was ever published, itself a multi-volume effort incorporating the works of the logical empiricists and other notable scholars, such as Niels Bohr, John Dewey, Bertrand Russell, and Thomas Kuhn. Through the planned *Encyclopedia*, Neurath (1955) aimed to show how a range of activities common to the sciences (observation, experimentation, reasoning) "can be systematized to help evolve unified science," in this way "creating *the* system of science" (2; emphasis in original). The logical empiricists conceived of unified science in these terms—as singular, definite—and asserted that a vast "comparative scheme could show the amalgamation of . . . the common and different features of various theories" (4). A utopic project of a larger scale than Whorf's dream for scientific linguists, the *Encyclopedia* would serve as a model of human knowledge-making by enacting a "scientific analysis of the sciences" (15). It would synthesize rational and positivist approaches that diverged centuries before in the early modern period (10). It would contribute to the ultimate aim of a "unified science departmentalized into special scientific activities" (20), coordinated through this central repository and the efforts of its founders.

Finding a common a language, not only underneath the specialized discourse and symbols of the sciences but applicable to daily life, was the cornerstone of this ambition. Carnap's (1955) inclusion in the *Foundations* highlights the centrality of language—

specifically, logical syntax and semantics[135]—to this endeavor, for its capacity both to yolk rationalism and empiricism and to reframe the problematic of the unity of science as a question of logic, not ontology (49): as a series of formal relations between terms.[136] Carnap defined the "language of science" as the "language which contains all statements . . . used for scientific purposes or in everyday life" (45). His analysis took scientific results and rendered them thus into a series of "statements asserted by scientists" (42), whose component parts could be distilled and reassembled into "ordered systems of those statements" (43). For him, "it is possible to abstract in an analysis of the statements of science from the persons asserting the statements and from the psychological and sociological conditions of such assertions" (43). Within formal syntax, science became a system of statements and their logical correlates. Semantics, in addition to the above formal analysis, took on the issue of "designation": how different scientific terms might refer to the same object, property, relation, of function (44). Through this process of abstraction, logical methods enabled the analyst to compare and connect seemingly disparate laws and reduce them to a common language of statements (60). Carnap admitted that not all derivations were possible at this stage—that there was not yet a unified science—but insisted there was a unity of language upon which to base that greater unity, namely "a common reduction bases for the terms of all branches of science" (61).

Reduction or agreement, then, formed the basis of dissent between the Whorfian and logical empiricist articulations. Whorf addressed the difference in the last published

[135] In the proceeding chapter, Morris extended this schema to the pragmatics of the language of science, taking into account the "psychological, methodological, and sociological aspects of scientific practice" that are essential for the process of "confirmation" (72). In this respect, he attended to the remainder of Carnap's theorization of syntax and semantics.
[136] Carnap prefers "term" over "concept" in his nomenclature: the word *concept* conveys a psychologism, i.e. "images or thoughts somehow connected with a word" (49), which he seeks to avoid.

article during his lifetime, also for the *Technology Review*. "Languages and Logic" (1941a) presented the reader with two figures, Mr. Everyman (a "natural logician," or native speaker) and the formal logician, neither of whom could see the similarities between two statements in English: "I pull the branch aside" and "I have an extra toe on my foot" (233). A linguist, on the contrary, had access to a range of languages to compare. In the Algonquian language of Shawnee, for instance, those two statements would be alike; a Shawnee logician, he surmised, might class the phenomena as similar, because linguistically they would be minimally different (234–235). Facticity, Whorf argued, was thus modulated by linguistic background, and even logical inference subsumed the properties of its original language, rather than foreswore them (236). Where the reduction principle threatened to obviate categories alien to the logician's native tongue, Whorf's relativity principle—founded on agreement and "multilingual awareness" (244)—meant that "science *can* have a rational or logical basis even though it be a relativistic one and not Mr. Everyman's natural logic" (239). Calibrating different types of logic would explain what appeared as irrational behavior and, through this exposure, defamiliarize the effects—or "embroidery" (239)—of language on our own forms of cognition (238), for example the dominance of Aristotelian subject-predicate logic among Indo-European languages (241).

From his various excurses on natural logic and the science of language, Whorf not only engaged the logical empiricists but also attracted the attention of the Institute of General Semantics (IGS). A representative of the Institute contacted Whorf in hopes of gaining permission to reproduce "Science and Linguistics" for their seminars (Kendig to Whorf, 7 May 1940, *BWP*). The permission was granted, but Whorf admitted his skepticism over their initiative to his contact at the *Technology Review*: "I overcharged partly to discourage them" (to Fassett, 24 May 1940, *BWP*). The recruitment pamphlet Whorf received featured a Memorandum from the Institute's director and intellectual leader, Count Alfred Korzybski, whose work *Science and Sanity* (1933/1958) served as the foundational textbook for the movement. The pamphlet included the Institute's first annual report, covering its "pioneer period," applications of general semantics in various fields, an announcement of seminar courses planned for the 1940 meeting and, finally, a list of Institute publications.

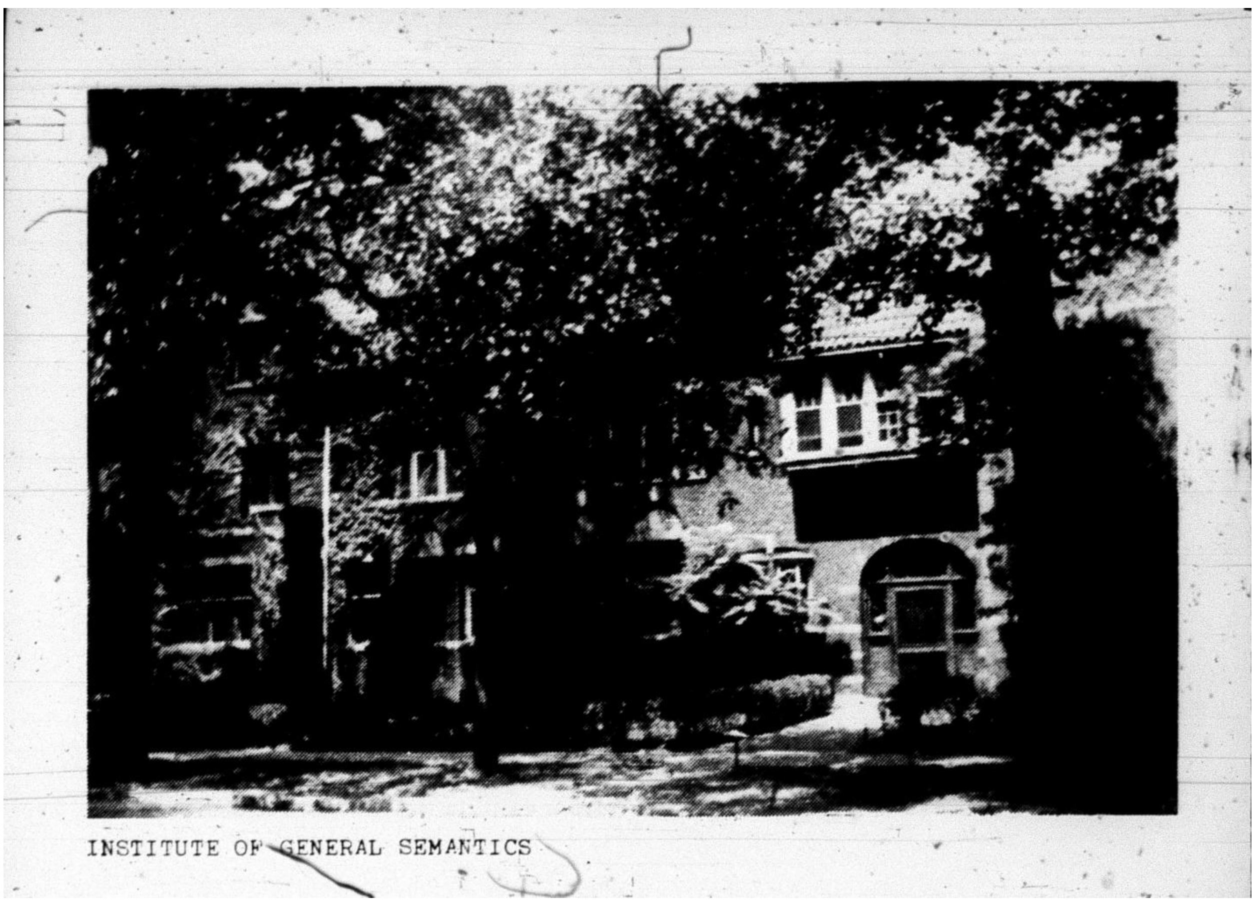


Figure 8. The Other Chicago School (7 May 1940, *BWP*)

The IGS was founded in May 1938 "for linguistic, epistemologic, scientific research and education," and it was "incorporated under Illinois law as a non-profit educational institution" the same year, its students comprising "professionals, scientists, physicians (psychiatrists included), lawyers, and educators, including specialists in speech, linguistic and reading difficulties, etc." (Korzybski, Memorandum, *BWP*: 5). Within a scant twenty-five hours of group training, Korzybski claimed a range of beneficial consequences: the treatment would dissolve inhibitions or excessive drinking; improve creative capacity, intelligence, and reading difficulties; cure stutter; work even where psychoanalysis fails; protect against suicide; and eliminate "semantogenic difficulties" related to heart, joint, arthritic, respiratory, or sex problems (11–12). Korzybski surmised that a vast majority of students (90%) experienced the benefit of general semantics in their professional or personal lives, sometimes with as little as one or two hours of personal interviews. The Institute wanted to republish Whorf's article because it lent credence to their claims that applied linguistics could effect behavioural change.

Whorf's chance meeting with the Institute taught him a lesson that would become an enduring feature of his archive. A facet of his work's going public meant it was no longer under the author's control: "The point is that it is so easy for individuals or organizations of a somewhat crackpot nature—I am not saying that the Institute of General Semantics belongs to this class—to broadcast this article with all sorts of contexts added, and you and I do not know where it goes or how it is used" (to Fassett, 24 May 1940, *BWP*). In Whorf's implied dismissal of the Institute as chicanery, pseudoscience figures as the failure that haunts any act of popularization—the potential for scientific theories or results to be applied differently than intended. Albert Wohlstetter and Morton White (1939) did more than imply: their article decried general semantics as a threat to the Unity of Science Movement over the scientific and cultural authority of semantics. The two factions comprised the general semanticists (Stuart Chase, Thurman Arnold, Jerome Frank, Korzybski) and logical empiricists (Rudolf Carnap, Alfred Tarski, Morris Cohen).[137] Wohlstetter and White claimed that the former group were "pseudo-semanticists" who did not, like the USM, employ "logical analysis as part of the scientific method"; rather, they used "garbled" language to prey on the Movement's prestige, for unscientific purposes (53). The works of Chase, Arnold, and Frank represented an unsavoury fad;[138] Korzybski's Institute, they insisted, was "a cult" (53). Wohlstetter and White were highly skeptical of the IGS's implicit claim that any problem, from the medical to the political, could be reduced to misunderstandings of language (52). Ultimately, they emphasized that this "inept exploitation of the theory of meaning" had to be distinguished "from the science of semantics" (57). It was only because the logical positivist method had not yet been popularized that such "cure-alls" as the IGS could gain traction (51).

It is tempting to regard the IGS as an isolated institution, or disregard it as pseudoscience, just as Wohlsetter and White did. As I commented above, regarding Whorf, sometimes it is more productive to interrogate how and why the claim of *pseudoscience* operates as a threat to scientific ethos than to deny its legitimacy. It is necessary, in other words, to take the IGS seriously, as it was most definitely not an isolated institution. Many universities offered accredited courses in general semantics; respected scholars endorsed its theories. It furthermore influenced an array of thinkers, finding its place within an

[137] The occasion of Wohlstetter and White's paper was Samuel Ichiye Hayakawa's (1939) defense of general semantics in the magazine *The New Republic*. They claim that Hayakawa displayed "ignorance of the authorities he cites and an extreme technical incompetence in semantics" (51), blurring the meaning of "semantics" entirely and grouping together unlikely thinkers. Wohlstetter and White used their response to Hayakawa as a platform to detail the errors of general semantics and derail its chief proponents. Whereas the Carnap collective were "interested in the formal and procedural aspects of inquiry," Korzybski's by contrast were concerned with the "latest best-sellers" and "get-rich-quick" schemes (51).
[138] Arnold, Chase, and Frank published, respectively, *The Folklore of Capitalism* (1937), *The Tyranny of Words* (1938), and *Save America First* (1938).

intellectual culture already disposed to treat language as a vehicle for addressing other social anxieties (Cameron 1995).[139] For example, in *The Tyranny of Words* (1938), Stuart Chase understood general semantics in terms of a language improvement initiative, convinced that it would offer a way to make language a better means of communication: "Language itself needed to be taken into the laboratory for competent investigation" (6).[140] He framed abstract or "bad language"—and those who abuse it, like politicians—as the enemy. Only through mastery of general semantics could one employ abstract language safely, conscious of it as an "expert lion tamer" would be their big cat (9). Like Whorf, Chase believed the basis of clear communication was agreement over a common referent (9), but unlike Whorf—whose analysis was founded on multilingual awareness—Chase located the basis of that agreement in words which corresponded to his own experience (5). That difference was of particular significance because the general semantics movement was, lastly, another site of uptake for results from anthropology and linguistics, though perhaps an unexpected or unintended one. While Korzybski asserted that his work derived independently of other theories of semantics, which did not espouse "a general theory of values" like his own (1958: xxii), the IGS nevertheless conjured an atmosphere to which Whorf and the Unity of Science Movement also belonged—one in which a "moral self" emerged through efforts to craft a linguistic self (Cameron 1995: 68), and one where debates over science and democracy were threaded through a contested expertise over language and meaning.

---

[139] Cameron (1995) understands "verbal hygiene"—when popular discourse (for example, style guides or political correctness) renders some language "good" and other "bad"—as standing in, symbolically, for real-world subjects that are too uncomfortable for commentators to address directly (217).
[140] Following Korzybski, Ogden and Richards, Chase defines *semantics* as the "science of communication" (7), at odds with today's meaning, which distinguishes itself as the branch of linguistics concerned with "meaning."

How, then, did the supposed cure-all of general semantics work? Korzybski explained:

> General Semantics formulates a new branch of natural science, an empirical theory and general method of human evaluations and orientations. This science is concerned with the control of the linguistic and semantic mechanisms present in all human nervous systems which condition all our knowledge, activities and adjustment. Thus General Semantics as a complete systematic methodology is applicable in all specialized fields of scholarship and scientific investigation, in the professions, in general education and psychotherapy, in everyday living, etc. (Memorandum, *BWP*: 14)

General semantics posited a direct channel between higher cognitive functions and "the extenionsialized structure of language," a process by which attending to one's language introduced "delayed reactions" into the impaired functioning of the cerebral cortex and thereby induced "emotional balance, normal blood pressure, glandular secretion, regulation of acid formation, etc." (10).[141] In *Science and Sanity*, Korzybski elaborated on the biological inferences informing this model of embodied cognition. He understood the human organism as a system of relations within an integrated "whole," in contrast to an "Aristotelian" understanding, premised on subject-predicate logic and thus with no opening for relations among parts (187).[142] Whereas the Aristotelian model must assume

---

[141] Chase (1938: 41) offered a succinct breakdown of the three regions of the brain invoked by this therapy: the spinal cord and cerebellum, which managed automatic response to stimuli; the midbrain and thalamic region, which took "care of vivid, dynamic, and emotional matters calling for quick response with little reflection"; and the cortex and higher brain, which were responsible for messages that require reflection, namely "thought."

[142] A rejection of binarism in Aristotelian systems of thought is central to Korzybski's text—which puts forward instead "multiordinality" as the main innovation of general semantics (14)—but not central for my purposes. Within Korzybski's conceptual order (194), "pre-human" or "primitive" reasoning functions

that impulses spread simultaneously through the body (akin to the immediate identification enacted by the copular verb "to be"), so order was not important, in a non-Aristotelian model this process was realized in terms of a spatialized coordinate system: within the "*finite* and known velocity of nervous impulses, and the *serial*, chain structure of the nervous system, order becomes paramount" (193; emphasis in original). Although the two did not reference each other, Korzybski's theory of language and mind, like Whorf's, belonged to an intellectual sphere stimulated by Einstein's work. For Korzybski, however, "the achievement of Einstein was the building of a linguistic system similar in structure to the world, which eliminated a pathological pre-human factor of objectification of terms" (655). Namely, Einstein's theories made it clear for Korzybski that ordinary language structured concepts of matter, space, and time differently—and erroneously—from the structure of the material world (657). As a map represents the world, Korzybski believed language should do the same for the nervous system (11); accordingly, transformations in linguistic structure would improve navigation, and consequently the function, of the human nervous system. Re-ordering one's language, in other words, re-ordered oneself.

In this way, Korzybski's general semantics initiative corresponds to Joe Dumit's (2003, 2004) analysis of "objective-self fashioning."[143] Dumit's notion of objective-self fashioning attends to the rhetoric surrounding the production and translation of brain images in late-twentieth-century America. Position emission topography (PET) scans, he

through unrestricted (one-valued) identification, for instance the absolute sameness of taking signal for food in the classic Pavlovian experiment; "infantile" or "Aristotelian" thinking through restricted (two-valued) identification, for example perceiving a strange animal as a dog, with the associations that entails; and "adult" or "non-Aristotelian" thinking through no identification at all (infinite or n-valued).

[143] Object-self fashioning belongs to a broader tradition investigating "brainhood" (Vidal 2009). Vidal speaks to the broader history of the rise of the "cerebral subject" in Western societies from the seventeenth century to the present, emphasizing its embeddedness within scientific understandings of the brain and sociocultural accounts of individuality.

argues, emerged in this period as a focal point between biomedical technology and identification. Drawing from rhetorical theorist Kenneth Burke, Dumit understands "identification" as a form of self-persuasion, whereby one is convinced of the objectivity of one's own experience—but where one is also, through that aperture, perpetually open to refashioning when ways of knowing the "objective self" change or fail (7–8). He cites a 1983 cover of *Vogue* as a crucial moment when the public eye was turned to the association between brain scans and normative identity; the cover juxtaposes three such images with the captions "Normal," "Schizo," and "Depressed." The images, however, represent idealized composites of behaviour types and resemble very little typical brain patterns. Dumit thus makes the observation that the authority that these scans convey often exceeds the control of the scientists and experimental practices that create them. Once a public began to identify—or not—with these images, their influence on normative personhood and self-knowing took on a life of their own.

The two main arguments that Dumit draws out in his analysis of objective-self fashioning—the pathologization of difference and the porosity of scientific authority—come to bear on Korzybski's project, as well. Korzybski framed general semantics as a guide to "mental hygiene" (1933: 9). In normal thought processes, Korzybski explained, impulses pass directly through the thalamus into the cerebral cortex, but in abnormal, the "main impulse [was] blocked semantically" and did not (193). The key to sanity, he claimed, was a rejection of entrenched structures of language that led one to false conclusions about the world and hence disruptions in the nervous system; for him, these errors of identification represented "extremely wide-spread delusional states" (195). Rather than rely on visual culture to convey its objective self, general semantics seized upon the zeitgeist of language improvement initiatives—"[W]e must build a new language" (373), Korzybski urged, one that is "structurally reliable and safe" (372)—which, much like discourses of endangerment, often positioned linguistic purity against virus-like threats to the health of the body-politic (Heller & Duchêne 2007: 4). General semantics also echoed narratives of upward mobility through scientific understanding that permeated the public imaginary alongside Einstein's theory of relativity, with Korzybski asserting that "very simple yet powerful structural factors of sanity can be found in science" (lxxxvi).

Nonetheless, Korzybski perceived popularization itself as a threat to the ethos of science, often doing more harm than good, because scientific facts could not be properly utilized with "antiquated psycho-logical orientations," which to him lent themselves to a form of "structural ignorance" (lxxxi). For Korzybski, the potential for scientific results to operate independently of their authorities was as much a hindrance to his model of the objective self as false identifications—indeed, part and parcel of the same problem. General semantics, on the other hand, promised to adapt scientific facts into the structure of language directly, forging an isomorphism between thought process and the structure of the world. Unlike modes of identification through the propagation of scientific images or celebrity-scientists, discussed above, general semantics instead sought to erase the analogy implicit in any act of identification—to flatten entirely the differences among language, cognition, scientific theories, and the natural world. The Institute of General Semantics took the international aspirations of the logical empiricists a step further: rather than one language for a united science, it posited one language for one cosmos—the ultimate unity, putting the rational mind not only in the driver's seat of the body, but in confluence with its environment, as well.

The general semantics movement thus struck their own balance between relativity and unity, difference and experience, and science and democracy that were features of this era—as did Whorf and the logical empiricists. The theories each group espoused sublimated their concerns over scientific modernity within the contested terrain of language and meaning; their fragile utopias appeared alongside attempts to guard against misinterpretations or misappropriations by each other and by the audiences they sought to persuade. Efforts to galvanize a science of meaning dealt, in other words, with the problematic of translation on both fronts. Within the budding disciplinarity of linguistics, scientific knowledge about language was detachable and afloat between varying modalities of expertise and authority. It was through an appeal to scientific ethos that Whorf and these movements aspired to avoid the complexities and uncertainties of translation, but it was within that flux that they also met them. If *pseudoscience* is a label meant to unite science as a whole by designating a common antagonist to its authority or credibility (Gordin 2012: 104)—for contemporary linguists who dismiss him, that antagonist is Whorf; for Whorf, it was the Institute of General Semantics—then in this way it functions as the inverse of and supplement to popularization. The designations of *pseudo* and *popular* are both a form of boundary work, both a means of extending and solidifying the cultural authority of science: one in the negative, the other positive. Despite such efforts, however, the ethos of science remains porous and uncertain. At times, each of the approaches discussed in this section was believed, endorsed, popularized—at other times, each was rendered unbelievable, illegitimate, pseudoscientific. Identification with scientific success—whether it be through the charisma of genius, the austerity of objectivity, or the dominance of a theory—proves to be a fleeting, fragile form of self-persuasion, a momentary passing. If Whorf's legacy of failed translations is of any indication, it is the porosity of scientific ethos that adheres well after the initial contexts of discovery and justification fade from view.

Conclusion: Benjamin Lee Whorf—a Failure.

In this chapter, I have portrayed Whorf as the last amateur and the first popularizer of linguistic science. Under the guise of neither persona did he make all the right choices. Whorf was not a master of communication and persuasion, arguably not even a master of linguistics. He was a hobbyist—and a professional. An engineer—and a *bricoleur*. A popularizer—and unpopular. A unifier—and divisive. Above all, he was a failure: in none of these identifications was Whorf ever fulfilled. But as Shoshana Felman (2003) reminds us in her close reading of J. L. Austin's speech act theory, every promise is predicated on the possibility of its failure. The promise of linguistics, through the eyes and ears of Mr. Whorf, was no different. An analysis of failure—elsewhere, and here, a task of some perversity—enabled me to consider how the amateur specialist and his unruly mind stacked up against the archetype of "good linguists" that the Linguistic Institutes sought to train, or how his self-fashioning depended upon callous newspaper editors and uncertain sites of uptake. Nonetheless, despite the litany of failures and mistranslations that followed his career and relativity hypothesis, Whorf's purported unbelonging remains a historical retrojection—a refusal of the amateur and, perhaps, of the history of the discipline itself. The persona of amateur specialist did not represent the oppositional logics of a scientific identity—its subjectivity was not moulded solely in relation the virtues of objectivity, was not repeatable within a standardized training regimen—but instead a conflation of categories, a desire to close the gap between longing and belonging. Belonging as a category of analysis thus ruptures the binarisms upon which scientific personas are often premised: of subject and object, amateur and master, success and failure. In my study as in Whorf's life, failure proved to be generative and, in its way, more inclusive category than success for assessing the common goals and relations that structured who belonged and how to the science of linguistics—and who did not.

Figure 9. Mr. Whorf—the Mis-fit (Miscellanea, *BWP*)

This chapter has thus examined the messy formative years at Yale through the eyes of an amateur specialist who, through his propensity to swim upstream, cut through the heart of them. I presented both Whorf's career and the theory of linguistic relativity—persistent bugaboos in the science's self-memory—as manifestations of the field's inchoate disciplinarity, a map hence redrawn to exclude them. Whorf's archive is often used to think about the influence of language alone: namely, the effects of language on thought. Here, I have used it to tell another story: how the emerging professional identity of the linguist, now taken for granted because of its success, obliges us to think about the linguist alone. Linguistics fused elliptical space by increasingly refusing the participation of amateurs and delimiting the scope of contribution for informants. Whorf's partial uptake of this identity reminds us—perhaps more apparently because of his slippages—that the linguist's subjectivity was formed at the height of the salvage paradigm and resonated its assumptions about the failed self-determination of Indigenous peoples over their language and culture. The next chapter examines the concurrent construction of an epistemic system that further marked who would count as a "linguist" through the bracketing of an object of study that was foremost their own.

~ 3 ~

"The decadence of linguistics is largely your own fault. You're an individualist and haven't built up a school."

- A. L. Kroeber to Edward Sapir, 1917

"One can describe what is peculiar to the structural organization only by not taking into account, in the very moment of this description, its past conditions: by omitting to posit the problem of the transition from one structure to another, by putting history between brackets."

- Jacques Derrida, "Structure, Sign and Play," 1978

Bracketing Time:

The First Yale School of Linguistics, a Study of Method

Introduction: Becoming Linguists.

The project of disciplinarity within linguistics intensified during Edward Sapir's years at Yale University (1931–1939).[144] Perhaps inspired by his colleagues at the Chicago School of Sociology, or motivated by the stifling museum culture in Ottawa, or incensed by recurring debates with his peers in anthropology, Sapir began to realize the advantages of distilling a disciplinary identity for linguistics. Indeed, Sapir's dedication to linguistics resulted in his declining positions at the Chicago Field Museum, Bureau of American Ethnology, the University of California, and the University of Pennsylvania (Darnell 2001: 122); he even turned down an offer to succeed Boas at Columbia the same year he took the position at Yale (131). Already a focal point for Indo-European studies and site of the Linguistic Society of America's (LSA) first summer Linguistic Institutes, Yale was fast becoming a

[144] The chapter subtitle references Sapir's (1916) "Time Perspective in Aboriginal American Cultures, a Study in Method."

major centre for linguistic research in America—and, with Sapir's addition, the only such program to offer advanced studies in Indigenous linguistics. It was there that Sapir trained a coterie of students to take up the mantle of salvage; with his support, they conducted fieldwork and amassed archives on Indigenous languages and cultures from across North America. What's more, they advanced methodologies for the elicitation and study of sound systems that Sapir had developed over previous decades. At this juncture, linguists were prepared not only to dash the competition but to bracket the conditions of their mastery. My final chapter examines how the Yale school codified their intimately distant methods but, in the process of securing this scientific expertise, failed to preserve its humanistic foundations.

Linguistics professionalized rapidly in North America, with the founding of the Linguistic Society of America (1924), its journal *Language* (1925), and the emergent training centres that were the Linguistic Institutes (1928). The national association was organized by George Bolling and Leonard Bloomfield, who recognized the need for a society for the general study of language rather than of specific language families (Murray 1991). In the first paper published in *Language*, Bloomfield (1925) articulated the exigencies which a linguistic society would assuage: it would bring together a generation of scholars who had scant opportunities to meet, yet whose aims were "so well defined," their methods "so well developed," and their field's "past results so copious" (1). It would also acquaint lay and academic audiences with a science of language, of which they were either unaware or, at most, believed a practitioner of it "merely a kind of crow-baited student of literature" (4). The independence of a linguistic science depended, then, on the consolidation of a network of scholars, institutions, professional associations, and publication venues across the space of the United States (and Canada) and, moreover, on the construction of a legible scientific identity for linguists to occupy in their own and their publics' minds. These conditions did not appear all of a sudden but, as my previous chapters demonstrated, emerged out of existing disciplinary and nationalistic frameworks for conducting science and in relation to shifting understandings of the role the scientist.

Chapters 1 and 2 traced, respectively, how salvage linguistics relied on a web of informants, stenographers, collectors, missionaries, amateur ethnologists, and professionals to enact their research and, through the figure of Benjamin Lee Whorf, explored the positionality of the amateur amidst the field's nascent disciplinarity. Becoming a science, however, required more than the organization of a network or the identification with a scientific self: linguists also needed to bracket an object of study that was exclusively their own. Bloomfield (1926) registered a set of axioms for this undertaking, but a science—even the science of language—is more than a series of statements. Hans-Jörg Rheinberger (1997) introduces the concept of "experimental systems" to describe the "genuine working units of contemporary research in which the scientific objects and the technical conditions of their production are inextricably interconnected" (2). Scientists, he contends, are not engineers: they are *bricoleurs* working within the limitations of their experimental systems to produce unexpected (and even unprecedented) "epistemic things." These epistemic things are "material entities or processes—physical structures, chemical reactions, biological functions—that constitute the objects of inquiry" (28). Where for nineteenth-century linguistics the regularity of sound laws discovered via comparative reconstruction was the process in question, in the twentieth century linguists inaugurated another epistemic thing: the phoneme.[145] In contrast to phonetics—the study of the physical properties of speech-sounds—phonology represented the study of the structural unity of a sound system despite its overt acoustic differences. The phoneme ultimately enabled linguists to assemble synchronic descriptions of linguistic systems, bracketing off the historical dimension of prior comparative work. Sapir (1925) and Bloomfield (1926) introduced this mode of analysis in the American context.[146]

The advent of the phoneme coincided with the proliferation of linguistics programs in America. Between 1927 and 1931, the number of American university positions in linguistics expanded, with trained faculty offering courses on the subject at California, Pennsylvania, Chicago, Washington, and Yale (Leeds-Hurwitz 2004: 160), in addition to the bastions at Columbia and Harvard. Due in part to the profile of the Committee on Research in Native American Languages (discussed below), the development of these university programs, and the launch of the summer Linguistic Institutes (LIs), professional linguistics in America began to rely less on the work of talented amateurs (though some, like Jaime de Angulo and Whorf, persisted) and more on their own university-trained peers. The

[145] Indeed, both the novelty and the thing-ness of the phoneme were subjects for debate at the outset. Behaviorist William Twaddell (1935) disputed each. For Twaddell, the concept of phonology was not new, but "its apparent newness [was] a product of increased accuracy of phonetic observation" (5). Awareness of the range of phonetic difference, "even within the usage of a single individual," induced the need for a separate technical term. Twaddell furthermore challenged proponents of the phoneme to restrict the indeterminacy of its descriptions, arguing that previous psychological and physical definitions were "open to serious if not unanswerable objection" (33), and proposing his own theory of the "macro-phoneme" in their stead. I would argue that the phoneme may not have been new as a concept, but it was novel as an epistemic thing, and consistent with Rheinberger's definition that such things "present themselves in a characteristic, irreducible vagueness" (28).

[146] A parallel articulation of the phoneme occurred overseas in the works, for instance, of Ferdinand de Saussure and Nikolai Trubetzkoy. Although there was some interchange between the two groups (Bloomfield reviewed Saussure's *Cours*), limited circulation of texts and American hostility toward European intellectuals in the interwar period kept the two streams independent of each other until after the Second World War.

cleavage between the expertise of the professional linguist and that of the amateur or informant grew commensurately, as linguists circulated their thoughts and practices increasingly amongst themselves. Of those institutions named, this chapter attends to a group of scholars who composed the "First Yale School" of linguistics (Hymes & Fought 1981), led by Sapir.[147] From its inception upon Sapir's arrival at Yale as the Sterling Professor of Anthropology and Linguistics to his death almost a decade later (1939), this collective served as a nexus for research in the Indigenous languages and cultures of North America—much of which unraveled soon after its leader's demise when his program was dismantled (Darnell 1998b).[148] The majority of Sapir's disciples in the Linguistics Department worked on Indigenous languages: Walter Dyk, Mary Haas, Fang-Kuei Li, Stanley Newman, Morris Swadesh, Charles Voegelin, and Mr. Whorf.[149] By the mid-1930s, Sapir and his former and current students "formed the core of the LSA committee on

---

[147] Sapir joined three other prominent linguists at Yale: Franklin Edgerton, Edward Prokosch, and Edgar Sturtevant. Together, they offered a graduate program in Linguistics that was independent of other language departments at the university (Darnell 1998b). Language departments at American universities were interested in "literature rather than linguistics and in the practical abilities of speaking, reading and writing rather than in general problems of structure" (Boas et al., Report on April 1937 ACLS Conference: 50, *APS*).

[148] Sapir's hiring was endorsed by the newly appointed president of Yale, James Angell, and by the Rockefeller Foundation (Darnell 1998b). Yale offered him a salary of $12,000 (with $5,000 for fieldwork), a tremendous amount within the Depression, which Chicago was unable to match (Murray 1994: 102). However, there were tensions at Yale from the beginning of Sapir's appointment: Angell hired him to act as a bridge between the social sciences for the Institute of Human Relations. Sapir was meant to be "as a superstar—an exception to the corporate rules" (Darnell 1990: 384). Boasian anthropology, however, was at odds with the evolutionary sociology practised at the university and the differences proved to be insolvent. For more on Sapir and the IHR, see Darnell (1990: 383–397; 1998b). At Yale, Sapir also faced anti-Semitism: Jews were not allowed to teach undergraduates at Yale, and Sapir was prohibited from joining the faculty club (1990: 327–328).

[149] When Sapir returned from Canada in 1925 to teach as Professor of Anthropology and Linguistics at Chicago, he attracted a number of students, many of whom later followed him to Yale and wrote dissertations on Indigenous languages of North America (Murray 1994: 101): Walter Dyk (Wishram, Yale, 1933); Mary Haas (Tunica, Yale, 1935); Harry Hoijer (Tonkawa, Chicago, 1931); Fang-Kuei Li (Mattole, Chicago, 1928); Stanley Newman (Yokuts, Yale, 1931); Morris Swadesh (Nootka, Chicago, 1931 M.A. Thesis). Only Hoijer remained at Chicago to teach linguistics; Li became a visiting professor at Yale from 1937 to 1939, during Sapir's declining health (103).

American linguistics" (Darnell 1990: 285).[150] The department at Yale—a font for vast funding agencies, a host to the first LIs, and a centre for the use and theorization of the phoneme—therefore became vital to the discipline, with Sapir's pupils at the vanguard of refining its epistemic things.

Only a fraction of the LSA's founders worked on Indigenous languages of America.[151] Nonetheless, these scholars were central nodes in the development of American linguistics, training a generation of scholars, elaborating on theoretical principles, and proving their methods of analysis. Within this experimental system, the unwritten languages of America were likewise integral for refining modes of elicitation and recording; they were frequently called upon to exemplify rarefied linguistic features. Moreover, they facilitated boundary-work between the lay or philological privileging of literary languages and the linguist's scientific view that all languages were equally complex and thus worthy of study.[152] Bloomfield, for instance, cites the systemic properties of non-literary languages as a basis of their scientific impartiality: "linguistics finds . . . a similarity, repugnant to the common-sense view, between the languages of highly civilized people and those of savages, a similarity which disregards the use or non-use of writing" (1925: 2).[153] The figure of the "savage," in other words, was foundational to the ascent of American structural linguistics,

---

[150] In her history of linguistics lecture, Mary Haas recalled that the Yale school "had the reputation, at the time, that MIT had in the '60s" (1976: 367, *MHP*). No other linguistics program in the country offered serious work on Indigenous languages (Darnell 1990: 359).
[151] Of the initial membership in the LSA, there were "nine philologists and more than ten modern language teachers for every two anthropological linguists" (Murray 1991: 5),
[152] The title of "linguist" was by no means a new designation, but in this disciplinary context it was invested with new significance to distance the "linguist's" activities from those of the philologist. Haas commented on the distinction: "one of the reasons why then 'linguists' objected to the term 'philology' [was] because they wanted to work with living languages and with unwritten languages and they didn't like this restriction in the use of the term 'philology'" (1976: 9, *MHP*).
[153] Bloomfield, like Sapir, was trained in Germanic philology and comparative Indo-European methods but expanded his purview to the study of Indigenous languages, particularly the Algonquian language family.

both in principle and in practice. Jodi Byrd (2011) recognizes that "the idea of the savage and the 'Indian' . . . serves as the ground and pre-condition for structuralism and formalism, as well as their posts-" (10). For Byrd, the transit of empire has depended upon a correlate process of "becoming savage," validating Western narratives of progress by relegating Indigenous peoples to the past perfect.[154] The "Indian" nonetheless remains the ghost in the machine and field upon which these becomings are predicated: "They might also be said, as savages, to signify the necessary supplement that continually haunts the edges of any evocation of civilization or Western thought" (9). So too, I contend, is the transit of linguistics in America haunted by "Indianness," the ghost in their experimental system that was used to legitimate their objects of study and advance their growth as a discipline.

However, even with the support of institutional patrons and the enthusiasm of a generation of newly trained workers, the scope of these linguists' goals to document all the extant languages of North America was impracticable. Sapir, along with his colleagues Bloomfield and Truman Michelson, surmised that the rate of decline of these languages would always exceed their ability to document them:

> These languages are rapidly disappearing; many, indeed, are extinct and will never be known to us. Those which remain are in all likelihood doomed to disappear in a generation or two. Students of language are in the position, let us say, of botanists who should see a vast and interesting flora which has been doomed to extinction; they must gather specimens before it is too late.

---

[154] Jodi Byrd's (2011) commentary draws attention to Derrida's evocation of "tattooed savages" (quoted from Gustave Flaubert) and Deleuze and Guattari's reference to the Hopi spiral. She finds in both these articulations of poststructuralist philosophy the presence of the "Indian" as a figure always already deferred, already known, and completed.

("Project for a Survey of North American Indian Languages," 1926, *APS*)

Salvage linguistics hence prefigured discourses of endangerment that gained prominence later in the century. Within these discourses, Fernando Vidal and Nélia Dias (2015) show, "endangerment" refers not only to a state of affairs in the world but becomes a means of understanding and responding to that world. Vidal and Dias review an "endangerment sensibility" that has come to activate a network of values and affects, rousing "the perception that vast portions of the human and non-human world are in danger of extinction or destruction" (2). In the realm of biocultural diversity, this sensibility renders Indigenous communities stewards of their local environments, their cultures experientially linked to their surroundings: they become akin to an "ecologically noble savage" (10). In contrast, Western science understands itself as reasoned, universalized knowledge, whose techniques of classification and description ensure a supposed emotional distance (27)—or as I argued previously, rather an intimate distance. Indeed, despite claims of impartiality, these cataloguing practices often carry with them a fatalistic attitude, "anticipating the failure of the cure" (2), such as that glimpsed in Sapir, Bloomfield, and Michelson's statement above. Caring for the communities themselves becomes secondary to gathering data on their language and culture.[155]

Both for contemporary preservation movements and their precursors earlier in the century, prognostications of endangerment and impending loss have been accompanied by this impulse to archive, as we saw through the work of the Geological Survey in Chapter 1.

[155] Rebecca Lemov (2015) historicizes another degree of displacement, which took place from the 1940s through the 1960s, when anthropologists sought to revivify their field by turning to its stores of dormant data. Here, concern transferred to the archives of anthropology ("an archive of archives"), which were themselves in peril and subject to a process of "second-order endangerment" (89).

In his reflections on scientific archives, Geoffrey Bowker (2005) troubles the "avowedly perfect memory" of the sciences (5), focusing instead on the informational technologies and habitual practices that scientists employ to memorialize their objects of study and maintain disciplinary histories. The archive establishes a series of traces that govern what is remembered and, by the same token, what is forgotten. Bowker argues that transformations in the dominant medium of storage evoke different "memory regimes." These regimes "articulate technologies and practices into relatively historical constant sets of memory practices that permit both the creation of a continuous, useful past and the transmission sub rosa of information, stories, and practices from our wild, discontinuous, ever-changing past" (9). For contemporary diversity studies, Bowker highlights the digital database as the central technological achievement supporting their cataloguing practices.[156] Salvage initiatives of earlier in the century relied on analog approaches to do comparable work, employing such mnemonic devices as journals, index cards, photographs, and audio recordings. Apprehension governed their work of apprehending cultural and linguistic data; adhesion to flat surfaces assuaged their concerns over their subject's finitude. To this end, they also developed systems of classification to sort out languages and cultures and synchronize them within the same archival framework. My chapter will foreground the documentation practices of the First Yale School, examining how the archival activities of these linguists were intermeshed with the field's disciplinary development. What was the cost of memory and which organizations funded it? How did

[156] The database, according to Bowker, is an invention that confers "the ability to order information about entities into lists using classifications" (2005: 108), dating back to rise of statistics and government archives in the nineteenth century.

the trained linguist generate and manage their memory prostheses? What was the fate of their stores of information, and how might those collections be remembered differently?

In addition to studying the discourses that instituted these archives and the memory practices that furnished them, I will consider the archives themselves as historical objects. Lorraine Daston (2017) evaluates how the archive manifests many functions in relation to the sciences: it not only establishes continuity with the past, as Bowker shows, but also renders comparable research in the present and stores material for future use (3). The archive serves "to annihilate time" and make data commensurable across periods (11). Judith Kaplan (2017) attests to the durability of practices in linguistics, examining how basic vocabulary lists were maintained as a tool across "disciplinary, technological, and linguistic" (204) systems over the nineteenth and twentieth centuries—a phenomenon which she terms "data drag." Kaplan configures drag as a recalcitrance toward change, but Elizabeth Freeman (2010) offers another articulation: her phrase "temporal drag" recalls "all the associations the word 'drag' has with retrogression, delay, and the pull of the past on the present" (62).[157] These practices proved to be a durative aspect of the linguist's experimental system (both for their durability across time and for their fixed temporal parameters), but the archives they supplied were nonetheless conjugated in the imperfective—as events without completion, as directions of pull dragging the past to an unknown future point. Where linguists once utilized their archives to look backward into deep history, the salvage program was oriented more so toward a speculative futurity: its adherents assembled archives of partial data in expectation of a later opportunity when

[157] Freeman defines temporal drag in relation to the "time" of queer performativity. She identifies that time as linked implicitly to the progressive, the avant-garde, the new, whereas she locates it in "cultural debris [that] includes the incomplete, partial, or otherwise failed transformations of the social field" (2010: xiii).

their work might be completed. These linguists were conscious that their efforts would never match the rate of diversity being lost.[158] Their memory avowedly imperfect(ive), they assembled descriptions of languages in anticipation of an occasion outside the temporalities of salvage, when there would be time for further analysis, theorization, and revision.[159] In this way, languages were not only objects to be archived but were also archives in and of themselves, each containing stores of potential information for their successors to resume and advance. Within the salvage framework, the anticipated failures of cultural and linguistic reproduction gave rise to the need to memorialize that which would be lost, but the durability of the archives themselves was not a given. The work of rendering data commensurable across periods depended on their eventual retrieval, for which there was no certainty. Bloomfield declared continued work in this area "a national duty": "If we fail, we shall be shamed before the judgement of posterity: we may be certain that it is by this kind of thing that future generations will judge us" (to Boas, 17 February 1937, *BWP*). This was a field of inquiry at a crossroads, replete with themes of salvage and loss that could apply equally to their own endeavours as to the subject of their study.

Time, therefore, was not so much annihilated in these archival practices but a precious commodity, on which these linguists hoped their investment would bring returns. Bloomfield's framing draws attention to an adherence to the settler state as a depository for collecting interest upon that investment. Mark Rifkin (2017) develops the notion of "temporal sovereignty" to discuss the ways in which the state restricts Indigenous self-

[158] In Charles Voegelin's (1941) account of the Native languages spoken north of Mexico, he estimated that there existed just under 150, excluding those already "extinct" (16), especially along the East Coast. Less than half of that number were studied by this point, at varying levels of detail.
[159] Boas commented on the need to revise early work: "It is very seldom that a single investigation, extended over a few months, clears up adequately the structure of a language, and a revisit of the tribe is often essential" (Report on Committee on American Indian Languages, January 1941, *APS*).

determination through asserting a monopoly over time. For Rifkin, "settler time" signifies "a particular way of narrating, conceptualizing, and experiencing temporality" (viii), one which limits how Indigenous peoples establish continuity with the past, inhabit the present, and envision the future. The state reproduces its authority either by consigning First Peoples to the traditional or by incorporating them into settler-governed modernity: either arrested in the past or winnowed into the present. Narratives of both stasis and transit, then, become mobilized to bolster settler frames of reference. Or as Rifkin puts it: non-native acts of translation work "not primarily to understand Native temporalities but to insert them within settler timescapes" (25). Linguists were enrolled in the modernist project to manage its excesses in this way, turning loss into a scientific commodity. Rifkin goes on to consider what conditions might rupture settler time and promote "temporal multiplicity," fostering different fields of relations to continuity and change. To this end, I will show that the failure of Sapir, Bloomfield, and their peers to complete a totalizing archive of languages, installed within the temporalities of the settler state, can be read as the condition of possibility for re-thinking settler frames of reference within the science of linguistics. Failure, here, evokes different mappings of time and potential retrievals of these archives, their latency a subject of fertile reworkings rather than regret.

My chapter thus extends Rifkin's argument to the temporalities of salvage linguistics, which were—and remain—entangled in questions of Indigenous sovereignty. As the collection practices in Chapter 1 suggested, over the course of the twentieth century the "lifespan" of endangered languages has come increasingly to intersect with linguistic science: with the theories that linguists profess, with the collective memory they curate, with the inscription technologies they employ, with the evidence they publish and discuss.[160] Taking seriously the metaphor of language as a living entity, I interrogate the ways it gets materialized in scientific practice and ask: what is lost when a language is lost, and who grieves its passing? I argue that American linguists established themselves as scientists in relation to these not-yet-dead languages and, as such, their successors must recognize a collective responsibility and "response-ability" (Haraway 2008) toward Indigenous peoples, upon whose knowledges and expertise they once depended. I therefore join recent postcolonial rethinkings of such scientific collection practices (Fabian 2010, Reardon 2005). Radin et al. (2013), for instance, stress how technologies for freezing and storing blood for genome diversity studies "reveal enduring colonial dimensions of scientific practice in our global age and demonstrate new openings for ethical action in the realm of the biosciences" (468). By a different measure, so too have linguists operated within parallel ethical frames—their methods, techniques, and technologies for isolating, storing, and classifying languages have no less formed the "connective tissue" (Stoler 2013) that ties together colonial legacies and contemporary language revitalization movements.

The installment of linguistics as a science in America marked these and other ways of inhabiting time. Rifkin explains how such temporal "orientations" are the product of repeated encounters: they are "shaped by existing inclinations, itineraries, and networks in which one is immersed, turning toward some things and away from others" (2017: 2). Accordingly, I cast the events described in this chapter as a series of turns. For linguists, the advent of their discipline turned the production of novelties within their experimental system into a recognizable expertise and career path; it disclosed an expectation that the conditions of labour to reproduce that system would persist without interruption within

[160] I deal with the metaphor of language-as-a-living-entity and its critiques in more detail below (§f).

and beyond their lifetimes. For Indigenous informants, it anticipated their willing participation in settler-colonial collection projects; it turned their tacit knowledge into linguistic data and, in turn, bound up the transmission of many of their languages with the archives of linguistics. The phrase "time binds" takes on several meanings here, but I follow Freeman's definition of it: "naked flesh is bound into socially meaningful embodiment through temporal regulation: binding is what turns mere existence into forms of mastery in a process I'll refer to as *chrononormativity*, or the use of time to organize individual human bodies toward maximum productivity" (2010: 3). What applies to bodies, I argue, is also true of the languages they (re)produce. The binding of bodies into settler time that turned amateurs into specialists and First Peoples into informants depended upon the actual bindings of texts, the brackets within which linguists organized languages and out of which derived their mastery. Bracketing time was necessary, in other words, to render productive others' words. My chapter therefore presents a history of turns and brackets (that which enables but also impedes turning). It explores two brackets in particular, unfolding in different time-scales but bound together, like vocal cords: the methodologies the Yale School developed for deriving linguistic data from living informants and the materiality of the records of those languages traced within the archives they left behind. My chapter raises the question, worked out in historical time but mindful of present engagements and future horizons: how does one care for a language? In the sections below, I examine the temporalities inscribed into the First Yale School's commitment to the salvage formation, their regimens for training the body, their rhythms of elicitation and transcription, their genres of synchronic description, and finally the archives of their material records. In each, I pick out strands of imperfection and incompletion that characterized this project's entanglements and unravelings over time.

❖ Living Proof

a. [Committing to Knowledge]

Much of the work of amassing data on Indigenous languages was orchestrated through the Committee on Research in Native American Languages.[161] The American Council of Learned Societies (ACLS) helped to sponsor the Committee, which was active from 1927 until 1937 when it ran out of funds. Franz Boas chaired the project, alongside Sapir and Bloomfield.[162] The Committee proved to be a crucial node for concentrating funding and support for American linguistics amid the Depression and interwar period, in addition to raising the profile of a predominately linguistic research program. It represented a renewed commitment to the salvage formation, and it was a space for negotiating different bracketings of knowledge therein: between the "conservative" and "imaginative" approaches respectively of Boas and Sapir. For Sapir, it was moreover a means of advancing an anthropologically informed Americanist tradition to his colleagues at Yale, who belonged to a "general" linguistics concerned with the written languages of Indo-European descent. Through the Committee, Sapir could also offer his students the opportunity to conduct fieldwork on yet-unstudied languages of North America and help maintain their status as scholars in hard economic times.

---

[161] For the fullest account of the formation, activities, and dissolution of the Committee, see Wendy Leeds-Hurwitz (1985, 2004).

[162] The Committee itself was composed of three subcommittees: the Council Committee (Edward Armstrong, Edgar Sturtevant, and John Swanton), who oversaw funds on behalf of the ACLS; the Committee of Execution (Boas, Sapir, and Bloomfield), who directed the project; and the Advisory Committee (later, the Advisory Board), a supplementary consulting group (Leeds-Hurwitz 2004: 129).

Before the Committee's establishment, there was no single body to coordinate the study of Indigenous languages across America. In ten years, it spent over $80,000 for 40 workers to survey over 70 languages (Leeds-Hurwitz 2004: 132).[163] The primary use of the Committee's funds was to facilitate fieldwork, rather than training or publication, with the aim of collecting expansive material on languages at risk of vanishing; administrative costs were to be kept to a minimum, with grants directed through existing scholarly infrastructures (127).[164] The April 1927 "Conference on Research in the American Languages" established the fundamental goals and procedures of the Committee, along with a provisional list of research centres through which those funds would be administered. These institutions included: Universities of California, Chicago, Columbia, Harvard, Pennsylvania, and Washington, in addition to the Bureau of American Ethnology and American Museum of Natural History (130). Invitations for advisory members were sent to scholars already active in the field: Roland Dixon, Pliny Goddard, John Harrington, Diamond Jenness, Alfred Kroeber, Truman Michelson, Paul Radin, Frank Speck, and John Swanton; all but Radin accepted, and each member (except Jenness in Ottawa and Bloomfield at Ohio) was located at one of the approved research centres (131).[165] Within this arrangement, universities operated as training centres; the majority of workers supported were already known to the Committee, usually students of Boas or Sapir, who

---

163 The Carnegie Corporation provided the first grant of $10,000 per annum for five years, administered through the ACLS.

164 On rare occasions, the Committee supported publication directly, such as Sapir's account of Southern Paiute (Leeds-Hurwitz 2004: 136). Otherwise, it recommended texts to be published by the American Ethnological Society and grammars by the Bureau of American Ethnology; full dictionaries were held back (168).

165 Later members of the Advisory Board included: Jaime de Angulo, Berard Haile, Melville Jacobs, Alfred Kidder, and Gladys Reichard (Leeds-Hurwitz 2004: 131). Bloomfield rejected Father Adrien-Gabriel Morice, deeming missionaries lacking in the scientific spirit (130).

often tested their charges with university funds before recommending them to the larger project (139). All of Sapir's linguistics students but Voegelin received support through the Committee, amounting to nearly one fifth of its total funds spent.[166]

The Committee began with two separate petitions to the ACLS in 1926: one by Sapir, Bloomfield, and Michelson, and another by Boas. The ACLS consolidated these applications into one, but the distinct perspectives would persist throughout the decade. At first, Boas had wanted a single large group responsible for the project, including Bloomfield, Goddard, Kroeber, Michelson, Sapir, and himself (Leeds-Hurwitz 2004: 128). ACLS representative Armstrong prevailed in recommending a smaller administrative committee of three, of which Boas and Sapir were the two first choices.[167] The different approaches of Sapir and Boas were known from the outset, but rather than a cause for reduction, Boas agreed that the Committee should accommodate these multiple epistemologies:[168]

> There are two lines of research represented in American linguistics; the one strongly imaginative, bent essentially upon theoretical reconstruction. This is represented by Sapir. The other more conservative, interested in the same problem but trying to reach it going back step by step; in other words more conservative. This is represented by myself. Both, of course, should be represented in the committee in charge of the work.

---

[166] The total was $14,941.23. It was distributed among the researchers as follows, with languages studied in parentheses: Dyk (Wishram, Washo): $4,625.02; Haas (Tunica, Natchez): $2,185.00; Li (Mattole, Wailaki, Hupa, Chippewyan, Hare, Sarcee): $2,457.43; Newman (Yokuts, Bella Coola): $2,669.90; Swadesh (Nitinat, Chitimacha): $2,253.88; and Whorf (Hopi): $750.00 (Wendy Leeds-Hurwitz 1985: 135–136).

[167] The choice of the final member was between Bloomfield and Michelson, who possessed similar qualifications: both were trained in Indo-European and both studied Algonquian. Michelson, who worked for the Bureau of American Ethnology, was closer to the matter of Indigenous languages, but Bloomfield proved to be the better worker and prevailed (Darnell 1990: 280).

[168] There was, ultimately, a parity between the two camps, with grantees of the Committee "fairly evenly split between Boas students and Sapir students" (Darnell 1990: 281).

(Boas to Armstrong, 19 February 1927, *APS*)

Despite their contrasting views on methods (discussed at greater length in Chapter 1, §13, and in the following section), Sapir and Boas promoted a harmonious portrait to ensure continued funding from the ACLS (Darnell 1990: 367).[169] Indeed, the topic of comparative reconstruction—so divisive in the previous decades—became a secondary concern amidst the urgency of salvage, and little comparative work ultimately took place under the aegis of the Committee.[170] As §e will make clear, rather than epistemological strife between Sapir and Boas being the cause, it was rather the conditions of fieldwork that disfavoured historical reconstruction in this period. Once, the comparative method was at the vanguard of linguistic science, "introduc[ing] into the order of sciences the peculiar rate of change known as history—a rate of change more rapid than the biologic, and therefore more subject to observation" (Bloomfield 1925: 3).[171] However, within the temporalities of

[169] Rarely were epistemological differences the cause of friction in Committee operations. Wendy Leeds-Hurwitz (2004: 145–172) details other problems they faced: choosing fieldworkers; conflicts of interest between researchers; lack of training; coordination across the country; difficulty of funding publication; and the expansion of territory into Latin America.

[170] Voegelin (1941: 15) observed that comparative work had begun for Algonquian and Athabascan, was in progress for Uto-Aztecan, and was minimal for Siouan and Salish language families. For other Indigenous languages north of Mexico, there were some comparisons between word lists, but not at the level of systematicity that reconstruction required; in many cases, dictionaries were lacking, and there existed comparisons of structure but not word lists, or vice versa. Sapir's (1929a) republication of his schematic super-stocks appeared to have satisfied the classificatory for delineated language families that initiated by the Bureau of American Ethnology in 1891, freeing linguists to focus on descriptive work (Darnell 1998a: 242).

[171] Bloomfield (1939) considered the discovery of regular sound change through the comparative method one of the greatest scientific accomplishments of the nineteenth century: "The method of this study may fairly be called one of the triumphs of nineteenth-century science. In a survey of scientific method it should serve as a model of one type of investigation, since no other historical discipline has equaled it" (2).

salvage, this experimental system became less productive.[172] There was not enough material available to reconstruct Indigenous proto-languages, nor enough time.[173]

Sapir encountered another peculiar rate of change at Yale. There, he occupied a median point between not only anthropology and linguistics but also "general" linguistics and the Americanist tradition. Relations between the latter two groups were more strained than the former two. Students interested in unwritten languages could belong to either the anthropological or linguistic streams at Yale, and Americanists interested in exploring historical relationships often had to seek training outside anthropology (Koerner 1984: 116). Sapir felt more resistance among traditional linguists toward nonliterary languages (though he and Sturtevant, who worked on Hittite, were mutually fascinated by each other's specialties), and he consequently made efforts to promote anthropological linguistics among his other colleagues (117). The LSA committee on American linguistics planned an introduction for "general linguistic scholars," but the publication only appeared in 1946 (Darnell 1990: 285). Earlier, Sapir had wanted to employ Committee results in that regard, advocating for his colleagues to publish materials in *Language*:

> In the first place, it is of the utmost important not to disconnect American linguistic work from the general linguistic current. Dr. Boas seems to cling to a somewhat adequated [*sic*] convention which looks upon American Indian linguistic material as somehow "anthropological" rather than strictly linguistic in character. It is, as a matter of fact, exceedingly unfortunate that

---

[172] This was not the case for Indo-European linguistics. Sturtevant's (1931) breakthrough glossary of Hittite and later comparative grammar demonstrated the continued vibrancy of the reconstructive method in that area of study.

[173] That is not to say there was no desire. From 1934–1936, Sapir offered a course in "Comparative Problems in Primitive Languages," less due to his own interests than to student demand (Darnell 1990: 362). Some of Sapir's students returned to these topics after the war.

> so much of our American linguistic work is buried in anthropological series that are practically inaccessible to the few people, linguists, who could make intelligent use of it. . . . I should certainly like to see my three Paiute papers published in the LSA series. I should then feel that they were being presented, as it were, to the linguistic world at large instead of to constituencies compose chiefly of ethnologists, archaeologists, and physical anthropologists, who get linguistic papers in the mail and neglect even to cut their pages.
>
> (Sapir to Kent [cc Armstrong, Sturtevant, & Bolling], 1927, *APS*)

Though Sapir shared with Boas a commitment to interdisciplinary synthesis, he increasingly guarded the autonomy of linguistics. Darnell (2001) comments: The "irresolvable divergence from Boas in models for cultural process was undoubtedly one reason Sapir increasingly identified himself as a linguist in the second half of his career. Boas's intransigence made Sapir's position increasingly awkward" (65). The advent of a linguistics society offered Sapir "an easy alternative professional identification" (66), where he could enjoy the pleasures of disciplinarity without the continued demand of justifying the basic parameters of their field. In the culture of experimentation he helped to foster at Yale, linguistics continued to develop methods and objectives that differed from those of Boasian anthropology. The parity between Sapir and Boas, which had previously held them in productive tension, became sedimented in the administrative procedure of the Committee rather than an active agent in its scientific process.

Sapir, meanwhile, found his time divided among other obligations. In addition to his duties on the Committee and his responsibilities as professor and mentor, he ran the

Impact Seminar (1931–1932) for the Social Sciences Division of the Yale Graduate School (Darnell 1990: Chapter 17), served on the Advisory Committee on Personality and Culture (1930–1934) for the Social Science Research Council (Bryson 2009), and chaired the Division of Psychology and Anthropology (1934–1936) for the National Research Council (Darnell 1990: 319–320), along with other conference and committee work. By the mid-1930s, he retreated from active research on Indigenous languages, except Athabascan, and contributed more to that field in the form of administration and supervision. His own studies shifted to topics of culture and personality and, late in life, to problems in Indo-European and Semitic languages (Eggan 1986: 12).[174] He remained available to the needs of his pupils, making efforts to train and secure fieldwork funds for them, but Sapir's dedication to his own work wavered.[175] He was no longer immersed in the experimental culture he helped cultivate for his students. In a sense, Sapir's commitment to American linguistics resulted in his withdrawal from it to this organizational role. Intimate distance relocated to managerial distance, and the "imaginative" dimension migrated elsewhere: epistemologically, from historical reconstruction to phonemic analysis and, generationally, from Sapir to his students, especially Haas, Swadesh, and Voegelin.[176]

---

[174] Fred Eggan (1986) speculates that Sapir's turn away from research on Indigenous languages of America toward questions of psychology and personality coincided with the loss of his first wife and his move to the interdisciplinary program at Chicago. There, he encountered Harry Stack Sullivan, who had lost his mother around the same time and whose teachings were influential on the similarly bereaved Sapir.

[175] Stephen Murray (1994) recounts the consistent stream of failures and false starts that characterized Sapir's late career. Sapir faced personal tragedy, institutional setbacks, and anti-Semitism during his years at Yale, but Murray speculates that he may also have been "self-defeating" (109): "I do not think that the increasing failure to prepare data for publication, to write planned books or monographs, to lay out a case for his synchronic and diachronic models, and the failure to produce exemplars of analyzing personality, intra-cultural variability, or any language studied after 1910 can be attributed entirely to external enemies or to bad luck" (110).

[176] Before his death, Sapir bequeathed to which students he wanted his linguistic materials to go (Jean Sapir to Mary Haas, 16 Feb 1939, *MHP*).

b. [Training the Linguist's Judgement]

Despite their accommodations on the Committee, the epistemological rift between Sapir and Boas nonetheless continued to widen. The divisions fell along institutional and professional lines: Boas had his school at Columbia and scholarly networks in American anthropology; Sapir developed his own at Yale and through the LSA. They also surfaced methodologically in the differentiation of phonology from phonetics that took place in the same period.[177] Sapir's (1925) "Sound Patterns in Language" and Bloomfield's (1926) "A Set of Postulates for the Science of Language" introduced the phoneme to American audiences as a new standard for the structural description of languages—and as an object of study exclusively linguistic. In contrast to the physical study of speech-sounds (phonetics), phonology investigated the patterning of *significant* sounds.[178] A phoneme represented a "distinctive sound" (Bloomfield, 157), capable of distinguishing meaningful elements in a language.[179] The linguist discerned these patterns through the intuitions of native speakers; they were otherwise imperceptible within the "grosser physiological substratum" of acoustics alone (Sapir, 37).[180] Both Sapir and Bloomfield believed that the

---

[177] The transition from phonetics to phonology was not a stark break. Linguists continued to assemble phonetic descriptions but increasingly supplemented them with phonological methods. Sometimes, the terms were used metonymically.

[178] The term *pattern* is key to understanding the distinction between phonetics and phonology and deserves clarification. Stephen Anderson (1985) defines the use of "pattern" in phonology as the variation permissible within the repetition of a sound for it still to be considered linguistically "the same" (7). For Anderson, "rule" versus "representation" distinguish phonology and phonetics, respectively: phonologists believe it is impractical to include predictable, or "irrelevant," information about sounds that could be derived instead from grammatical rules (9). Independently, he observes, the information theory of the 1940s and '50s pursued a similar elimination of redundancy, for instance in the work of Roman Jakobson (13).

[179] One tactic for identifying phonemes in a language was the use of contrasting pairs: if a pair of words with minimal phonetic differences (for example, *bit* and *bid*) demonstrated a meaningful contrast to a native speaker, then those sound elements were understood to be distinctive in that language. Swadesh (1936) discusses the uses and limits of such phonemic contrasts, especially in polysynthetic and other morphologically dense languages.

[180] Unlike contemporary generative linguistics, which understands the phoneme to be an underlying form realized in rule-governed phonetic output, their predecessors conceived of the phoneme as a subset of possible speech-sounds, those which informed meaning.

linguist could gain access to the totality of a linguistic system through such a process of induction. The rise of phonology follows Rheinberger's event structure for the production of epistemic things: "They usually begin their lives as recalcitrant 'noise,' as boundary phenomena, before they move on stage as 'significant units'" (1997: 21). The phoneme, as a measure of the significant sounds amidst the "noise" of the raw acoustic overflow of a language, epitomizes this course, almost literally. Later theorizing of the phoneme occurred throughout the 1930s alongside its use in the description and analysis of Indo-European and Indigenous languages; in this section, I examine the parallel development of training measures for the linguist to apprehend the phoneme and the corresponding modes of objectivity that marked the body of the knower.

Both phonetics and phonology relied on the collector's ear as an instrument to acquire linguistic data, as Chapter 1 showed through Sapir's network of fieldworkers. Even here, however, the aural dimension of their collection practices was put to different ends: Boas wanted to represent the phonetic features of a language indiscriminately; Sapir, to interpose the judgement of the linguist and uncover abstract rules about the language's sound system. The two approaches embodied, respectively, the principles of mechanical objectivity and trained judgement. Lorraine Daston and Peter Galison (2007) work out these "mesoscopic" categories of objectivity in relation to the visual cultures of science. Through the science of language, I extend them to a type of aural objectivity.[181] For their part, Boas and his students prioritized keeping records of the "essential character" of

[181] Below (§d), I consider in more detail the interplay of aural objectivity and the writing systems that supplemented it visually.

disappearing languages with the greatest technological accuracy possible (Darnell 2001: 179). Their efforts were consistent with Daston and Galison's "mechanical objectivity":

> By *mechanical objectivity* we mean the insistent drive to repress the willful intervention of the artist-author, and put in its stead a set of procedures that would, as it were, move nature to the page through a strict protocol, if not automatically. This meant sometimes using an actual machine, sometimes a person's mechanized action, such as tracing . . . to shift attention to the reproduction of individual items—rather than types or ideals. (2007: 121)

Boas was hostile toward phonologic methodology replacing phonetic because, he believed, phonemic forms could be inferred from the latter but not the other way around; he wanted investigators to be free of predispositions and criticized what he understood to be the imposition of the observer in the description of languages (Anderson 1985: 207–208).

By contrast, Sapir distanced the phoneme from the acoustic or articulatory properties of speech-sounds. In his words, the phoneme represented "the inner configuration of the sound system of a language, the intuitive 'placing' of the sounds with reference to one another" (1925: 40). In Chapter 1, I described how Sapir pioneered the use of native speakers' intuitions to construct grammars. In the 1930s, the phoneme was used to exemplify this approach. His (1933/1949) paper on "The Psychological Reality of Phonemes" outlined five situations where speakers' intuitions revealed the presence of phonemes. He argued that the phoneme's physical features were a "signal" for the "identification of the given entity as a functionally significant point in a complex system of relatedness" (46). Sapir stressed how this process of identifying phonemes within a "total system of sound relations" was not an abstract calculation imposed upon language, but a sensitivity felt in the speaker. This sensitivity was particularly evident in the "unguarded speech judgements of native speakers who have a complete control of their language in a practical sense but have no rationalized or consciously systematic knowledge of it" (47). For example, teaching an informant to write down their own language phonetically could produce consistent errors—to the point of view of the linguist—that disclosed phonemic patterning: "a native speaker's phonetic 'ignorance' proved phonologically more accurate than the scientist's 'knowledge'" (56).[182] Rheinberger (1997) argues that the productivity of an epistemic thing derives from the "mutual nonfitting" of theory and practice (56). Phonology depended, here, on the nonfitting between sounds heard by the observer and those held meaningful by the informant. The phoneme was therefore the appropriate unit of an intimately distant methodology: an abstraction from the physical event which yet maintained its relationality, one system signaling a constellation of features to the other, with the linguist expertly mediating.[183]

The works of Sapir, Bloomfield, and their students advanced this mode of expertise. It is important to note that informants were taught to transcribe *phonetically* and not phonemically in these encounters. The prioritization of pattern over sound manifested in the latter orthography:

---

[182] Morris Swadesh's (1934) article on "The Phonemic Principle" provided other examples of this kind. Swadesh presented six criteria to determine the phonemes of a language and account for their "range of deviation" phonetically. One example was the test of substitution: "This consists in pronouncing a word with some modification in one of the phonemes. If the modification cannot be perceived by a native, it is within the range of normal deviation. If the modification seems to trouble the native, it is an extreme deviation from the norm, a distortion" (124).

[183] In the Sapir memorial volume, Swadesh (1941) elaborated on this process of mediation: "We do not offer as established fact every golden remark of the native informant, but check it against the phonetics of his utterance, his handling of an experimental alphabet, his facial and verbal reactions to our attempts to speak words of his language, his pronunciation of other languages we know, and any other item that may suggest something. This is Sapir's method, the critical use of every bit of evidence" (59).

Phonetic Orthography	Phonologic Orthography
1. pa-	pa-
2. paβa-	papa-
3. paθA-[4]	papa-
4. pap·a-	pap·a-
5. pApa-	pap·a-
6. pap·A-	pap·a-

Figure 10. Phonetic vs. Phonologic Orthography in Southern Paiute (Sapir 1933: 50)

The phonological style was accompanied by a series of rules, similar to the laws of sound change, from which the linguist could derive the phonetic form. Sapir remarked: "phonologic orthography . . . is useless for one who has not mastered the phonology of the language" (1933: 50). This orthography added another layer of mediation to the procedure of taking down a language: the linguist's ability to interpret the pattern back to the speaker. Phonology thus exemplified an interpretive interposition characteristic of *trained judgement*:

> [T]wentieth-century scientists stressed the necessity of seeing scientifically through an interpretive eye; they were after an *interpreted image* that became, at the very least, a necessary addition to the perceived inadequacy of the mechanical one—but often [it was] more than that. The use of trained judgment in handling images became a guiding principle . . . in its own right.
>
> (Daston & Galison 2007: 311; emphasis in original)

Despite a tacit knowledge of their phonology, informants were not granted the same level of mastery or at least not the right tools to demonstrate it. Their knowledge was rendered a form of "ignorance" (even within the palliative of scare quotes) rather than another form of expertise. Phonetic orthography was yet adequate for descriptive purposes; more to the point, it maintained the "unguarded" judgements of the informant and elevated the trained judgement of the linguist. The phoneme, in this light, manifested not only as the significant sounds of a language but also as the socio-semiotic-material expression of the colonial power of linguistics: phonemic records of sounds were made accessible only through the interpositioning of the linguistic analyst and their scientific expertise.

Around the same time as the advent of the phoneme, procedures for teaching linguistics began to depend less on the apprenticeship model and instead became integrated into university programs. As a teacher, Sapir had once been eager to get in touch with any student who had "a good natural ear" and was willing to train in field methods (Sapir to Sturtevant, 9 April 1927, *APS*), including such amateurs as Whorf and Jaime De Angulo. In the 1930s, however, he became more invested in students who possessed not only a good ear but also the cultivated judgement of the phonologist. De Angulo, for example, had only acquired the methods of phonetic transcription. Sapir, who previously ranked De Angulo's grammars highly, lost confidence in him, believing that the moment of "indiscriminate salvage" had passed (Leeds-Hurwitz 2004: 18):

> The fact is that I think we are allowing too many poor or improperly qualified men to do linguistic work that should be entrusted to well-trained persons with a special flair for both phonetics and morphology. Boas still has very much the old pioneering attitude that the main thing is to rescue languages and put a lot of uncritical material on record. I do not subscribe to his view in the least. I think it is high time all the work that we sponsor be of quality that is high enough to satisfy the requirements of a genuine linguist.

(Sapir to Kroeber, 28 November 1930, *ALK)*

A "good natural ear" was no longer sufficient to be a "genuine linguist": the student of linguistics now had to acquire a discriminating ear, as well. American linguistics distinguished itself from philology in large part for its attention to non-literary languages and from Boasian anthropology through its emphasis on trained judgement: the phoneme was a vital tool in both regards. Within the span of only a few years, and with increasing confidence, linguistics guarded its expertise from the contribution of amateurs and other academic disciplines upon which it used to depend.

Nevertheless, curricula varied across the country, and not all subjects could be taught adequately in every linguistics department. In his final report for the Committee, for instance, Sapir identified the "Training Needs for Students of American Indian Linguistics" at American universities. Among his recommendations were improved standards for "phonetic recording," "morphological analysis," and "inductive field training." Future research directions in this area depended, in his opinion, on instruction in phonology:

> Above all, the student should be trained in the reduction of a set of minutely accurate phonetic distinctions into the basic phonemic system which characterizes the language. It is disappointing to note that many excellent phoneticians are incapable of a satisfactory phonemic interpretation of their records. Yet without the phonemic interpretation it is quite impossible to compare dialects or languages and to reconstruct from them the phonetic patterns which lie back of the dialectic ones. (1937, *APS*)

With an uneven distribution of training centres in the country, the Linguistic Institutes remained necessary sites for the emerging research culture: they could offer courses on specialized subjects available at few American universities. Whereas the early LIs discussed in Chapter 2 served to generate student interest in linguistics, later Institutes—which from 1936 on made a point to receive "Ph.D. guests without fee" (Hill 1963: 21)—were tailored to fill training gaps for scholars who already belonged to the academic profession.

Notable were the 1937 and 1938 Institutes, held at the University of Michigan at Ann Arbor under the direction of Charles Fries, where Sapir and then Bloomfield taught the "Field Methods in Linguistics" course. Sapir's class, especially, was deeply influential on important figures in American linguistics to follow, such as Bernard Bloch and Zellig Harris: "Sapir came as the star speaker, which would have some unanticipated and far-reaching implications for the whole field of linguistics" (Barsky 2011: 93).[184] These courses provided students and colleagues the opportunity to learn the inductive method and refine their judgement in recognizing the phonemes of a language unknown to them.[185] Sapir's course description placed a great value on the aurality of this practice: "The materials are not to be obtained from books but by direct questioning, the phonetic notations of the class corrected by the instructor being the final authority. No reference to printed literature will be allowed. It is hoped to show that a perfectly adequate grasp of any language, even a complex one, can be obtained by the direct phonetic approach."[186] Phonology was by no

---

[184] The event of Sapir's star performance was not without its toll. Swadesh (1939) believed Sapir's commitment to the summer Institute precipitated the decline in health that ultimately led to his death: "The enthusiasm and energy which he put into his teaching and scientific discussion and his cordiality in social contact with colleagues and students sapped his strength and brought on a heart attack" (132).

[185] Voegelin called this training the "spoonfed inductive method" (Handwritten notes from 1937 LI, *APS*).

[186] Here is the full course description for Sapir's 1937 "Field Methods in Linguistics":

> It is hoped to make this course as inductive as possible. The task will be set the members of the class to find out all they can about the phonetics and morphology of some language which is entirely unknown to them. The materials are not to be obtained from books but by direct questioning, the phonetic notations of the class corrected by the instructor being the final authority. No reference to printed literature will be allowed. It is hoped to show that a perfectly adequate grasp of any language, even a complex one, can be obtained by the direct phonetic approach. The phonetics needed to carry on the course will be developed as need

means limited to the Field Methods courses of Sapir or Bloomfield but permeated many of the classes and topics at these Institutes; nonetheless, for linguists to go beyond axioms and absorb the virtues of aural objectivity from the founders of phonological methods, they had to attune their ears specifically to the sounds of Indigenous languages.

Indeed, Sapir's model of elicitation was a major innovation of these Institutes and distinguished a uniquely Americanist brand of linguistic research. In this mode of training, the ear relied on the presence of Indigenous informants to volunteer their forms of speech:

> The effect of the Institute upon field work on unrecorded languages began with Sapir's course, Field Methods in Linguistics, in the session of 1937, supplemented by an informal course on the phonetics of Navaho, which Sapir gave during the same session. At Sapir's suggestion two American Indian informants were brought to Ann Arbor for the session of 1938 [taught by Bloomfield, who replaced an ailing Sapir], where their speech was studied and recorded under more advantageous conditions than can usually be secured by a single worker who visits an Indian tribe. In particular, texts recorded by a first rate phonograph, such as could not readily be taken on a field trip, yield samples of natural speech which can then be played repeatedly. In this way stylistic and other features usually distorted by dictation can be secured. More important still, linguists who were not

---

> requires. It is believed that the fear which so many students of written languages have of the direct phonetic method is entirely unwarranted, and it is hoped that this course may do something to make real the oft-repeated statement that languages exist primarily as oral phenomena, not as written symbols.
>
> (Qtd. in Joos 1986: 60)

> familiar with the methods developed by the Americanists observed and took part in this sort of study.
>
> ("Report of the Special Committee on the Linguistic Institute," 1940: 87)

Rheinberger (1997) reminds us of Michael Polanyi's formulation that all types of knowledge, including the scientific, involve the tacit dimension: "Being experienced enables us to literally embody the judgement in the process of making new experiences, that is, to think with our body" (77). These training procedures show how two categories of experience emerged within the linguist's experimental system and were distributed into the distinct but inseparable roles of expert and informant. Aural objectivity, whether mechanical or trained, depended on a repeated interchange between knowers: linguists acquired their embodied reasoning by extracting it from the bodies of their informants.[187] The Institutes thus replicated in a controlled setting the conditions that made the phoneme a productive epistemic thing: the contingency of salvage. Whether in class or in the field, the phoneme—and, in turn, the linguist's trained judgement—relied on this reproduction of differences between expert and informant, heard and meaningful, settler and native. These categories were nascent in Sapir's early experiences in Ottawa, but by the time he was established at Yale, they were becoming the standard.

[187] This mode of elicitation continued in future instantiations of the Field Methods course: "Demonstrations of eliciting Native American languages, a major if not exclusive focus of Sapir's and Bloomfield's field methods courses, were continued in a course 'Recording and Analysis of a Living Language' taught by C. F. Voegelin (1906–1986), Murray B. Emeneau (b.1904), and George L. Trager (b.1906) in 1939 with a Delaware native speaker, and by Voegelin in succeeding years with native speakers of Ojibwa in 1940 and 1941" (Murray 1991: 13).

c. *[Collecting the Linguistic Laboratory]*

I now address a crucial nonfitting in my translation of Rheinberger's theoretical apparatus to my own practice, and my re-working of it within the spatiotemporal coordinates of early-twentieth-century linguistics. Rheinberger works through the concept of the experimental system from the inside—specifically, from within the confined space of a biochemistry laboratory located in a university research hospital, within the temporalities of a bourgeoning Big Science and its demand to produce novelties.[188] Anthropological linguists notably operated in a different context: in the open air fieldwork and the temporalities of salvage. They were, as I remarked in the previous chapter, *bricollecteurs*. Where Rheinberger borrows Derrida's notion of the *bricoleur* to examine the play within a system of knowledge already given, my portmanteau extends this analysis to systems *taken*: from Indigenous languages and informants, whose presence would otherwise be erased in a history of the epistemic thing alone. Rather than follow the progression of the phoneme through an intellectual history of the theoretical and pedagogical anchors of this methodology, I attend to the nonfitting that resulted from the translation of this theorized methodology from the controlled setting of the classroom to an empirical practice in the field. Through this variability of theory and practice, the phoneme became an increasingly productive metre for the linguist to capture the modulations of Indigenous languages. As such, I argue that the cadence of fieldworkers and their Indigenous informants stimulated

[188] Rheinberger asserts: "It is not, in the end, the scientific or the broader culture that determines 'from outside' what it means to be a laboratory, a manufactory of epistemic things becoming transformed, sooner or later, into technical things, and vice versa. It is 'inside' the laboratory that those master signifiers are generated and regenerated that ultimately gain the power of determining what it means to be a scientific—or a broader—culture" (37). In the same manner that Rheinberger's work manifests "the Derridian program of reworking . . . oppositions from within, of trying to perfuse and defuse their limits" (19), I likewise employ a logic of supplementarity to reorient this opposition of outside/inside.

the uptake of the phoneme in an American context, their rhythms pulsing within its epistemologies.

Initially, the ACLS recommended that the Committee on Research in Native American Languages establish "a laboratory of phonetics, which might serve both for the education of field workers and for the settling of difficult points encountered by workers in the field" (9 June 1926, *APS*). The ACLS representative perhaps had in mind the laboratory for experimental phonetics at Collège de France, launched in 1897. Robert Brain's (1998) account of this truth-spot describes the "linguistic laboratory" as a crucial space for the language sciences where speech was made to "speak for itself." He emphasizes the role of the phonograph and chronophotograph as inscription technologies that forever changed the study of phonetics: acoustic images, he claims, helped to codify the concept of the phoneme in Europe (252). The phoneme could, as the European context demonstrated, suit the virtues of a mechanical objectivity. These technologies also afforded a newfound mobility to linguists, a result of which was a decentered locus of research (and corresponding shift in research subjects) from Paris to the provinces: "implanting the phonetics laboratory in the byways of the countryside as a means of making visible the elusive and prolific patois that thrived there" (269). The task of obtaining linguistic data in America similarly overlapped with the conditions of doing field science, in keeping with the dispersion of other American social sciences at the time (Gieryn 2006, Igo 2007, Lemov 2005).[189] However, across the Atlantic, the phoneme was not technologically driven in the same way; for its proponents there, the phonetic laboratory was not an indispensable site

[189] Alongside the collection of Indigenous languages, the LSA also encouraged the study of the dialects of North American English. These two initiatives, both directed toward the analysis of oral language, were the major developments of the early LIs.

for taking down knowledge.[190] Bloomfield (1933), for example, surveyed technologies akin to those Brain describes, but deemed them inadequate for distinguishing consistent patterns of form and meaning (i.e. phonemes): "The phonetician finds that no two utterances are exactly alike" (76).[191] Like Sapir, for Bloomfield phonemes exceeded "muscular movements" or "disturbances in the air." They were not sounds in themselves, but "features of sound" (80), patterns of relative variability shared by a speech community.[192] The American phonologist distilled such patterns from physical sound events, not in the hermetically sealed environment of the laboratory, but from timely encounters in the field when they had access to those communities, and where the linguist's trained judgement could be brought to bear.

Indeed, the failure of the phonograph to be employed for recording speech sounds illustrates a major difference of its uptake in American linguistics. The "cost and cumbersomeness" of the phonograph often made it impractical in the field (Bloomfield & Bolling 1927: 123), but it was nonetheless available despite economic and technical obstacles. Its main use, however, was to collect records not of speech but of songs.[193] Haas,

---

[190] It could be said that phonology was technologically driven in a different sense: phonologists certainly employed technologies of *transcription*, such as the "Indian typewriter" discussed in Chapter 1, even if they disregarded technologies of recording like their European or anthropological contemporaries.

[191] These technologies included the laryngoscope, a mirrored device that allowed one to examine the vocal cords; x-ray photography of the tongue's positioning in the mouth; a false palate, coloured to reveal the place of articulation; and a kymograph, for recording vibrations (Bloomfield 1933: 75–76).

[192] Bloomfield did not, importantly, dismiss phonetics but distinguished it as a separate scientific practice: "Only two kinds of linguistic records are scientifically relevant. One is a mechanical record of the gross acoustic features, such as is produced in the phonetics laboratory. The other is a record in terms of phonemes, ignoring all features that are not distinctive in the language" (1933: 85).

[193] Mechanical recording was sometimes employed to study the syntax of tonal languages: "I made some Dictaphone records (a half dozen or more) of Tunica speech for the purpose of making a more detailed study of sentence dynamics. It was observed that the sentence dynamics of Tunica and Chitimacha bore a certain resemblance. The Chitimacha tonal phenomena presented certain interesting features which required careful study and, because of my musical training and experience, my husband [Morris Swadesh] asked me to make the necessary musical analysis" (Haas to Boas, 16 Oct 1933, *MHP*).

for instance, provided Voegelin with detailed instructions on how to operate the phonograph for recording songs in the right pitch, which included a series of warnings: "Be careful not to try to play the record over (after recording) by means of the recorder as this will ruin the record. . . . [T]he records are breakable and need to be handled with reasonable care. Also it is a very good idea not to get fingerprints on the record" (Haas to Voegelin, 18 November 1935, *MHP*). Even here, mechanical recording did not displace the act of transcription, but introduced to it another layer of mediation: "Now we come to one more thing which I would like you to do as it is of considerable help to the transcriber (me, that is). At the end of the first record and while the recorder is still recording, blow into the horn (or, in other words, record) the pitch A above middle C by means of a pitch pipe. . . . [I]f at any time you do change the speed you should get the pitch again immediately afterwards" (*ibid.*). Recording linguistic data such as stories was another matter (literally and figuratively). The use of mechanical recording devices to enhance the linguist's imperfect senses was recommended when available, but as supplement to their bodies, whose "good natural ears" were still trusted above all.[194] The differences between recording phonetically and phonemically, which map respectively onto mechanical objectivity and trained judgement, did not translate to the *kinds* of technologies employed by both camps. Phonographs were available to each group, but for neither did it supplant

---

[194] In 1929, a two-day Conference on a Linguistic Atlas took place immediately following the second Linguistic Institute. There, the LSA flirted with the use of mechanical acoustic records of speech. G. Oscar Russell's talk on "The Mechanical Recording of Speech" put forward five reasons in support of it (from Joos 1986: 34):

> (1) Permanence, even after a recorded dialect is extinct.
> (2) Preservation of fine detail lacking in phonetic notation.
> (3) Repeated listening always possible, both (a) enabling interpretation of written phonetic symbols, and (b) picking up details which field workers may have disregarded as irrelevant.
> (4) No limit on the number of experts who can be invited to listen.
> (5) No time limit on discussion of disputed or puzzling observations.

the technology of transcription (see §d for more). The technological "failure" of the phonograph did not impel modes of objectivity, but rather those ideals shaped the implementation of the technology and linguists' strategies to remediate and re-mediate its limitations.

Bloomfield himself, however, only had intermittent opportunities to conduct fieldwork in his later career, and Sapir had none during his years at Yale. Indeed, Sapir had little participatory experience beyond the dictation of texts with a single informant and even less "dealing with the variability of having multiple informants" (Murray 1994: 109–110). When faced with multiple speakers of the same dialect, whose intuitions sometimes produced contradictory evidence, Sapir was frustrated by the difficulty of finding canonical forms (Darnell 1990: 249). With Sapir occupied in administrative roles at Yale, it fell to his students to negotiate the contingencies of the field. In her 1976 History of Linguistics lectures, Mary Haas reflected that there were no determined guidelines for this practice:

> It is something we usually don't know very much about: how people did their field work. Of course, the anthropologists argue about this endlessly, because they have all this participant-observer and all that kind of thing. And of course you can do the same thing in linguistics, but most people don't, for instance, most field workers in linguistics didn't learn to speak the language and so on.
>
> (Haas 1976: 346, *MHP*)

In spite of this variability, there were elements that most linguistic fieldwork had in common. For instance, research often took place during the summers or on longer stretches when the worker was on paid leave; there were limited opportunities for follow-up investigation.[195] It furthermore depended upon the availability of adequate informants. Fieldworkers relied on spontaneous or solicited speech to yield evidence of desired linguistic features, not always forthcoming: "you never quoted a form that hadn't been more or less volunteered by an informant by a speaker. Now more recently people have gotten further away from that. You can make up things and test them out" (Haas, 349). Field notes included texts and stories in interlinear translation, alongside and out of which linguists derived phonemic inventories, vocabulary lists, and grammatical paradigms, in addition to collecting ethnographic details. Interlinear translation imbricated a text through four tiers of progression: traditional spelling (in the original language); phonetic transcription (later phonological); word-for-word translation; and free translation (in English usually). In some cases, the circuits of collection continued at a distance with informants who were trained to write in their native language phonetically, as Sapir did from his headquarters at the Victoria Memorial Museum decades earlier. Ascertaining the total system of a language depended on partial encounters such as these.

In keeping with the relationships described in Chapter 1, the conditions of fieldwork also fostered an interchange between informants and researchers that exceeded the parameters of data collection alone. For one, informants found their stores of knowledge a source of remuneration: "To a certain extent, one can ask questions of natives without paying them for their time, but the most effective work is done when one can spend hours at a time with the same individual, and this is ordinarily possible only if one can

---

[195] Boas (1938) outlined the importance of follow-up research, though these sessions were not always forthcoming: "The best results are obtained if the field work is done in more than one separate period. After a season of say three months in the field, the worker spends a somewhat longer period in studying and classifying his material so as to get a fairly thorough control of the language and, especially, so as to formulate the problems and difficulties; in a second season of field work he is then able to round out and perfect his material" (Report of the Committee on Research in American Native Languages: 2 *APS*).

compensate him for his services" (Boas et al., Report on April 1937 ACLS Conference: 54, *APS*).[196] These encounters were not only economic exchanges but also transfers of knowledge and skills. Informants readily accepted instruction in the reading and writing of Native and European languages. Haas, for example, collaborated on her doctoral dissertation with Tunica informant Sesostrie Youchigant (Sam Young), "all but the last surviving speaker of the language" (Haas to Sapir, 29 August 1933, *APS*), who provided linguistic materials and aided her in checking them over. The encounter was a useful mnemonic prompt for Youchigant: "Sam said that 'in one way' he was glad I came back [to Marksville] because I reminded him of many words he had forgotten" (Haas to Sapir, 14 October 1934, *MHP*). Such collaborations created opportunities for informants to speak and recall their heritage languages, when they may not otherwise have had the occasion to converse with other living speakers.[197]

Under Sapir's tutelage, Haas continued her work on Indigenous languages through a series of fellowships at Yale.[198] She later studied dialects of the Muskogee on two occasions: the first from 1936–1938, funded by the Department of Anthropology at Yale, and again

---

[196] Boas (1938) elaborated on the day-to-day costs of fieldwork: "The cost of living on or near an Indian reservation is relatively small; most of one's money is spent in paying informants. There may be some expense for presents or entertainment; this varies with the traditions of the tribe that is being visited. Traveling expenses to and from the field will bring the cost to range between five and ten dollars a day" (Report of the Committee on Research in American Native Languages: 2 *APS*).
[197] There was often a generational divide between speakers. On one occasion, Haas persuaded the dean of a Junior College to let her interview students there: "I found that in a number of cases, students who felt very shaky about their ability to speak their languages were somewhat reluctant to talk to me. Two of them offered to write home and have their folks send them a list of words which they wouldn't be able to pronounce after they had them" (Haas to Swadesh, 10 May 1937, *MHP*).
[198] At this time, Haas also contemplated training for a high school teaching certificate. To avoid prejudicial anti-nepotism rules, she asked her then-husband, Morris Swadesh, for a divorce: "My decision to go to school again to complete my education requirements involves a difficulty—I think you know what it is—namely, that as a married woman I shall in all probability be unable to get a job teaching in the public schools, as a matter of fact, there is no doubt about it. However, quite aside from this it seems to me (and you doubtless feel the same way) that it might be the best thing for both of us if I were to get a divorce. . . . We have discussed these things before, and I do hope that it doesn't come entirely as a shock to you" (Haas to Swadesh, 10 May 1937, *MHP*). Their relationship continued amicably after the separation.

from 1938–1939, with additional funding from the APS (Haas 1945). Muskogee was "the numerically and politically dominant language of the Creek Confederacy," itself a "linguistic situation . . . of great complexity" (69). Within the Confederacy, she estimated there were at least eight different languages spoken, six from the Muskogean family. Political leaders hence needed a great deal of "linguistic versatility," a fact which remained evident in the informants she contacted. The Muskogee nation spanned most of contemporary Oklahoma and possessed considerable dialectal variation, still present at the time Haas conducted her research; she visited a number of informants in different towns that belonged to the former nation to access this diversity. Most of the transcriptions Haas took between fall 1936 and spring 1937 were phonetic, after which she switched to the phonemic style. She later recalled: "One of the really great advantages, of the great contributions, I should say, of phonemics, is that it forced people to be more accurate phonetically, because if you have a theory about what is the, important in the language, significant is the word I want, you had to check it out again in the field" (1976: 383). Over the 1936–39 period, she kept over a dozen detailed journals from the field, and with the aid of Creek informant James Hill ultimately filled a total of twenty-two notebooks, in addition to six that Hill sent Haas in between her fieldwork sessions.[199] From these notes, she prepared descriptive grammars of Muskogee before the U.S. involvement in the War (Haas 1940, 1941) and comparative reconstructions afterwards (Haas 1946, 1947).

[199] Haas maintained a friendly relationship with James Hill and his family. Hill later wrote that he "never forgot" Haas, who had "thought so much" of their sons, the youngest of whom, Alex, was killed in action during the War (Hill to Haas, 15 July 1944, *MHP*).

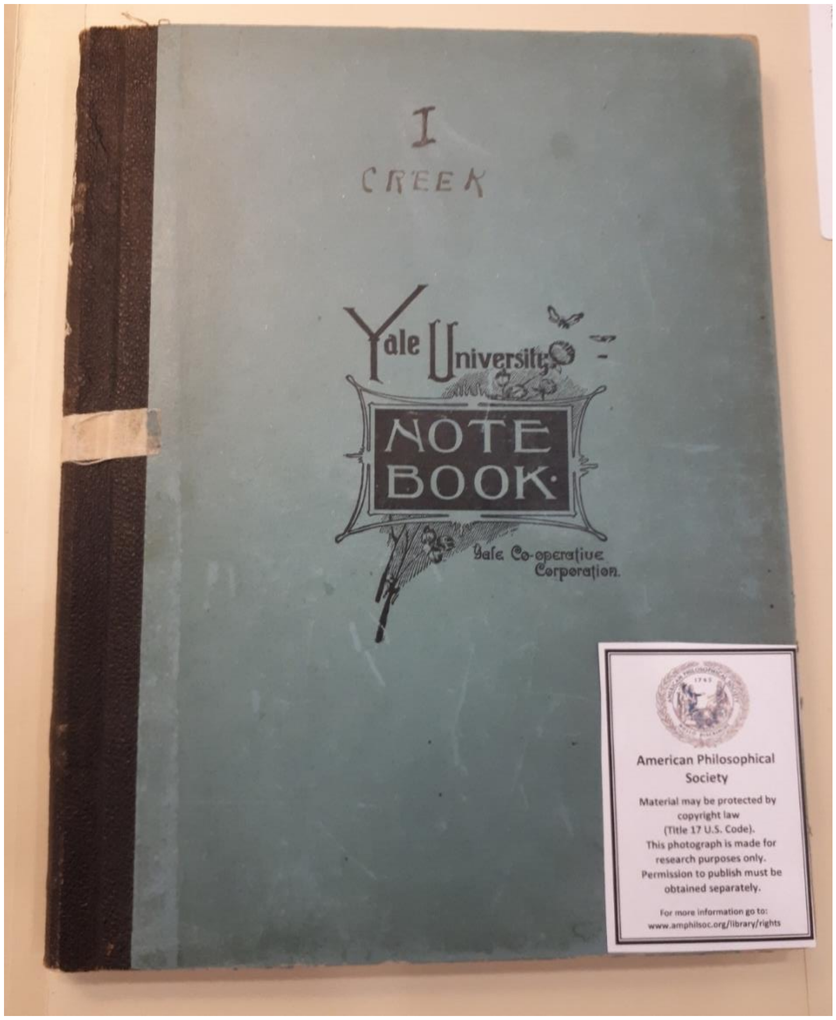


Figure 11. Mary Haas's Creek Notebook I (*MHP*)

Her work with Hill, recently republished (Haas & Hill 2015), offers a window into the realities of data collection. The editors of this corpus outline Haas's methods for taking down linguistic data in their Introduction, worth quoting at length:

> Haas began by eliciting verbal paradigms for person and tense. She then moved on to possession, infinitives and nouns, plural forms and verbs, and agent nominalizations. By page 25 [of the journal], she felt ready to collect her first story[.] . . . She did this by asking a speaker to read or dictate a text, which she transcribed in her own orthography. She would then have the text interpreted, usually with another speaker. She would transcribe the text in

> this second session along with word for word translations below each word. She would make a note of any corrections or alternate communications offered by the second speaker. . . . Before the advent of computers, linguists made extensive use of file slips. Every word in a text was copied on a piece of paper along with a phrase it occurred in so that the linguist would have an index of every word in context. These slips allowed vocabularies to be compiled and helped identify inconsistent transcriptions or speaker variation. Slips were also made of grammatical topics or specific affixes. Haas had students help copy her file slips, just as she had helped Sapir at Yale.
>
> (Martin et al. 2015: xxii–xxiii)

This approach was based on the preparation Haas received from Sapir during her Ph.D., later reduplicated at the Linguistic Institutes. Teaching his students in New Haven, however, where there was a dearth of informants, Sapir employed himself to ventriloquize forms of speech: "We finally persuaded Sapir to give a phonetics course, a non-credit course. He would dictate himself. He made an informant of himself for us to listen to in the various languages he commanded. He was very good at it, but as for determining the structures of the various languages, he did that by bringing in a sheaf of slips and talking about it" (Haas to Murray, 26 July 1978, qtd. in Darnell 1990: 361–362). At the Institutes, students likewise continued to learn from a single informant; even when two were present, they often belonged to different linguistics groups.[200] The cross-hatch of multiple informants that Haas employed thus represented an innovation of Sapir's students, a

[200] At the 1939 Linguistic Institute, for example, "the same method was employed not only with an Algonquian informant, but also with a Dravidian informant, brought to Ann Arbor for the purpose, and with Lithuanian and Polish informants" ("Report of the Special Committee on the Linguistic Institute," 1940: 88).

necessary improvisation to meet the demands of the field and supplement the material practices of collections.[201]

The inductive method conferred a greater role to the linguist's professional judgement, but it also facilitated a greater speed of collection: the linguist who developed a theory for how the sound system of a language worked could more swiftly bracket off the noise of phonetics to derive its phonology. As Haas suggested above, her shift to phonemic transcription was motivated by the rapid pace of data collection.[202] Swadesh's (1937a) "A Method for Phonetic Accuracy and Speed" affirmed how the experience of duration in the field necessitated techniques of rapid retrieval. The article promoted guidelines that even "a relatively unequipped person" could follow "in the field situation":

> The procedure may take from a couple of days to a week or so, depending on the difficulty of the language and the ability of the student. When he has followed it through, he will have a complete or nearly complete knowledge phonetics of the language, and be able to do further recording both accurately and at a rate that will justify the initial investment of time. (728)

Swadesh outlined eight steps to reduce the heard sounds of a language to a limited inventory of phonemes and a relatively narrow range of conditions that they may vary. It

[201] For example, Haas located an informant of the Siouan language Biloxi, Emma Jackson of Port Arthur, Texas, with whom she compared vocabulary items of ethnologist James Dorsey: "Of course, fifty words are far from sufficient to work out the phonemic system of the language, but they are sufficient to furnish evidence on some of the points that strike one in regard to Dorsey's treatment [of vowels]" (Haas to Sapir, 14 October 1934, *MHP*).

[202] Haas (1938) expands on her choice to employ phonological transcription over phonetic: "On the basis of my own experience, I would say that completely adequate phonetic results are not likely to be obtained by the linguistic field-worker unless, through the course of his work, the principles of phonemics are kept constantly in mind" (64). For Haas, it was perilous to interpret phonemically, after the fact, what had been recorded originally phonetically: "Whereas the phonemic structure of a language cannot be worked out without accurate phonetic data, accurate phonetic data are not likely to be obtained without consideration of the phonemic structure" (65).

began with a back-and-forth, the linguist soliciting their informants' judgement and getting them to repeat words back for the linguist's discernment: "Use your eye as well as your ear, by looking into the informant's mouth (in so far as you can and he will permit) to observe the mechanism of production" (730). It proceeded to drawing up lists and tables of sounds, to assessing their variability, and finally to settling on a corresponding alphabet. With practice, these instructions promised to make the researcher feel quickly "at home" with "strange sounds" in any language (732). Swadesh established a sequence to reliably mediate both the strangeness and the pace of collection. In this way, the linguists of the First Yale School fell into the rhythms of fieldwork, but to stabilize their experimental system and ensure its productivity, they had also to bracket off the temporalities of salvage on whose urgency they depended but, at the same time, could not sustain their work.

Collection practices were thus inextricably linked to the exterior: whether in training or in the field, linguists depended on the speaking bodies of their informants. Shoshana Felman (1980/2003) recognizes the radical potential of locating the speaking body within linguistic theory. Felman argues that speech act theory in particular "reintroduces the problem of the referent into linguistics" (49), which had been rendered foreign to the linguist's experimental system in both structural linguistics and transformational grammar. For Felman, the performative utterance does not index a closed, self-referential system, but the body of the speaker and their relationship to a pre-existing world. The authority of the performative coheres around its capacity, in each iteration, for failure: for the speech act to be infelicitous, for the promise to be broken. I extend Felman's analysis to encompass the promise of the languages sciences, a series of perlocutionary acts *par excellence*. The actions that compose any theory of speech unavoidably traverse the body and follow directions determined neither by reference nor by the intentions of language users. In the context of salvage, the "linguistic laboratory" was less a space for language to "speak for itself" than a temporally bounded linguistic encounter that depended on persuading informants to speak. Rather than take the "laboratory" as a settled space where languages acquired an authentic identity, it was instead a contact zone where bodies interacted and patterns inhered through repetition. The field conditions of salvage linguistics show how repeated encounters between the bodies of informants and *bricollecteurs* marked the linguist's epistemic system and rendered it productive within the scope of those limits. The velocity and volatility of field collection helped to propel the phoneme as the standard tool of descriptive linguistics. At the same time, the phoneme encapsulated the colonial power of the linguist, marking the slippage between the intimacy of their encounters with informants and the distance that their trained judgement cultivated to capture the phoneme as the fundamental unit of sound and therefore to guard their scientific authority and autonomy.

- **Analog Afterlives**

d. [Analyzing Writing Systems]

In the previous sections, I demonstrated how linguists trained their ears to perceive sounds from the speaking bodies of informants and how, amidst the zeitgeist of salvage, the First Yale School in particular employed these methods to meet the pace of fieldwork. Despite the importance placed on aurality in the disciplinary identity, training procedures, and collection practices of linguistics, a great deal of their activities remained textual: linguistic science could not proceed without systems of transcription to bridge the gap between the field and analysis. To betray an aphorism of Whorf's: the linguistic laboratory was not under the hat, but in the inscription technologies linguists employed to render sound systems a constellation of symbols and offset the temporalities of collection. Specialized orthographies were employed alike by phoneticians and phonologists, but in the phonological mode especially they authorized linguists speak on behalf of the languages they collected. Below, I show how the codification of the phoneme within specialized orthography and styles of transcription supplemented the provisional linguistic encounter: through the delimited space of the page, linguists re-framed the temporal limitations of these encounters as a source of productivity. Rheinberger (1997) characterizes research systems by their capacity for "differential reproduction": "the temporal coherence of an experimental system is granted by recurrence, by repetition, not by anticipation and forestalling. Its future development, on the other hand, if it is not to end in idling, depends upon groping and grasping for differences" (75). Repetition sustained the momentum of the linguist's epistemic system through a two-fold displacement of the speaking body: first, through the linguist's trained judgement, repeated speech forms were translated into patterns; and second, through the synchronization of linguistic and symbolic systems, those patterns were made further iterable on the page as structures relatively fixed in time. The development of writing systems enfolded languages within the linguist's experimental system as an assemblage of paper and ink. This assemblage proved to be an important crumple zone where the discordant tempos of fieldwork and analysis could be absorbed and re-mastered.

Fieldwork was contingent, fast-paced, and open-ended. For both Boasian anthropologists and dedicated linguists, it suited the material conditions of salvage that a small number of informants could confer substantial data on endangered languages: "Even if the language is no longer really alive, but still remembered by one or a few individuals, it is possible to gain a considerable amount of knowledge by working with the surviving speakers" (Boas et al., Report on April 1937 ACLS Conference: 46–47, *APS*). In the 1930s, Sapir and his students also commenced describing languages as closed systems, in part to mitigate the urgency of salvage that gave rise to their collection practices. Understanding endangered languages as closed systems enabled linguists to maximize their knowledge claims with the minimal data pool available to them. Swadesh in particular emphasized that the phoneme was the best tool to access the "totality" of these closed linguistic systems: "any scientific procedure that aims at finding the elements of a closed system is valid only if it takes into account the totality of the system" (Swadesh 1935: 248).[203] For Swadesh, the aim of phonemic analysis was to establish a "maximally simple, self-consistent, and complete" formulation of systematic sound relationships (1937b: 11). Swadesh acknowledged the concern that "pattern-conscious investigators" might distort material to make sound systems appear more symmetrical, but felt the risk was small compared to taking no notice of pattern at all (11). Like the structural linguistics of Saussure, patterning within the closed sound system was defined by a web of differences. Phonemic criteria were relative within a given language and depended primarily upon contrastive alternations rather than on any absolute standard for measuring sound (Anderson 1985: 262).

[203] Bloomfield elsewhere framed this relationality in behaviourist terms: "The persons in a speech community coordinate their actions by means of language. Language bridges the gap between the individual nervous systems: a stimulus acting upon any one person may call out a response action by any other person in the community. Languages unite individuals into a social organism" (Bloomfield 1942: 173).

For the purpose of their descriptive grammars, however, phoneticians and phonologists together sought to establish a phonetic alphabet with a standard correspondence between sound and symbol. The need to standardize a system of orthography was especially imperative for any linguist working on predominately oral languages or dialects: "Human utterance is so fleeting that even the field-worker placed most fortunately as he is in the immediate presence of his material, must seek to hold this transitory phenomenon—to make an artificial record of it for re-examination" (Bloomfield & Bolling 1927: 123). Certainly, in the 1920s and '30s, the major proponents of the phonetic alphabet in America were involved in collection practices, either through the Committee on Research in Native American Languages or the dialect atlas of American English. Toward the end of the Committee's funding, when its members most keenly felt their work's impermanence, the ACLS held a conference on past and future directions of the initiative.[204] One topic was a history of writing systems in the Americas. The authors of the conference report observed that, before the arrival of European settlers, traditional writing developed only in Central America, where Mayans inscribed monuments and made hieroglyphic codices; elsewhere, there existed instead picture writing or tally sticks and knotted cords used as mnemonic devices. Makeshift systems of writing emerged out of contact situations, often with missionaries. It was not until the Bureau of American Ethnology and Boas's *Handbook*, however, that these became formalized:

[204] The conference encompassed the history of Americanist linguistics, field methods and facilities for study of Indigenous languages, and recommendations for future research directions and initiatives. It comprised joint sessions of the LSA and AAA (American Anthropological Association) at Columbia University, 25–26 April 1937, with a lengthy report written by Boas (chair), Bloomfield, Kroeber, Sapir, and Swadesh (secretary). The report discussed the scientific significance of Indigenous languages of the Americas and recommended a permanent organization for their study and a continuation of the International Journal of American Linguistics to publish on these topics regularly.

> Out of the experience of the Bureau of American Ethnology there gradually grew a new conception of the scientific study of language. It was found that haphazard alphabets were inadequate for the accurate representation of the varied sounds encountered in such studies. It became apparent that the old conception of grammar, based on the work of Greek and Roman grammarians, though fairly well suited to these languages, was not adapted to the multitude of new linguistic structures. Two things resulted: the introduction of scientific phonetic alphabets, with a wealth of carefully defined symbols designed to represent in a relatively standard way many different kinds of sounds; and the notion that each language must be studied and described in terms of its own peculiar structure.
>
> (Boas et al., Report on April 1937 ACLS Conference: 27, *APS*)

Linguistic encounters thus affected both the observed subjects and the linguist's tools for recording observations: "the experience of workers in the field has impressed upon them the inadequacy of much that has been recorded. We know that it is possible by modern methods to obtain a dependable phonetic record of a language, and it is evident that much of what has been recorded falls far short of this ideal" (29). As the dissolution of the Committee attested, however, access to resources limited how this ideal could be realized.

Economy structured the linguist's records in many ways, often leveraging accuracy. In their reflection on symbol use in linguistics, Bloomfield and Bolling (1927) recognized that the phonograph could produce the most accurate phonetic recordings, but the least economical. The fieldworker had to rely on graphic symbols to describe speech sounds, although even this technology had its price: "We are rapidly reaching the point where linguistic matter is so expensive to print that the claim of our science upon the economic resources of society does not suffice" (124). Perfect phonetic accuracy was not only impossible but, in light of the phoneme, also a detrimental ideal to pursue: "No series of human speech-sounds can be represented exactly and completely by any system of written symbols" (125). Phonemic transcription, by contrast, gave linguists more leeway on what information to include and what to omit. All the same, Bloomfield and Bolling noted that linguists, who were best suited to comprehend the conventionality of symbols, yet felt the weight of their conventions:

> The special conventions of our science, the queer-shaped letters and the diacritic marks, are but creations of yesterday. They have sprung up almost before our eyes in a haphazard fashion, inventions to meet a momentary need, controlled largely by the native speech-habits of the inventor, or dictated perhaps by a passion for an illusory 'accuracy,' combined with the conveniences of a particular printer. (124–125)

Up until then, analysts had regularly established idiosyncratic systems of transcription for features specific to the language they studied or to the constraints of publication, internally consistent but with limited continuity elsewhere. With the advent of a linguistic science and its scholarly network, the authors saw the opportunity to establish a symbol set with more uniformity across different languages and scholars. The "queer-looking symbols"—those same apostrophes that Sapir "clearly loved" in the phonetic transcription style of the previous decade (Haas 1976, History of Linguistics Lectures: 380, *MHP*)—now seemed to them unwieldly relics: "A superfluous complication of the symbols cannot reproduce the sounds for a reader unfamiliar with the language; all it can do, and it will do it, is to confuse him" (Bloomfield & Bolling, 126). Diacritics would not be abandoned entirely but closely scrutinized (128).[205] Preferably, each mark would symbolize a discrete phoneme: "The gain in elegance (in the mathematician's sense) will repay us for whatever nostalgia may result. . . . [T]hese suggestions should be more welcome, because they lie in the direction not of crippling our science, but of greatly enhancing its power by giving it a suppler and more abstract symbolism" (129). The specialized orthography of phonology proved "economic" both in cost and presentational style: it fostered a more mathematically elegant analysis that took up less space on the page and, consequently, on the budget.

Phonetic alphabets were therefore an additional arena to register the conflicts between phonetic and phonemic styles of transcription. Phonetics and phonology largely made use of the same set of symbols. However, the two styles varied in the level of detail each demanded: phoneticians wanted to record, mechanically, all possible acoustic information about the sounds of a language, whereas phonologists trusted their judgement to unpack sound systems from a more terse description accompanied by a series of rules.[206] Swadesh (1934) more overtly advocated for phonemic values to be cemented within the standard alphabet: "phonemic orthography provides the most adequate, economical, and effective method of writing a language" (124). A phonemic alphabet

---

[205] Six Americanist linguists—George Herzog, Sapir, and Sapir's students Newman, Haas, Swadesh, and Voegelin—published their own orthographic guidelines a few years later. Like Bloomfield and Bolling, they believed that one symbol should equate a single phoneme; digraphs were misleading, to this end, suggesting sound was not unitary. Nonetheless, certain cases where providing a unitary symbol for a digraph created recurrent problems: sounds with timbre features (such as labialization or palatalization), nasalized consonants, and affricates. They recommend limited use of pre- or "post-posed superscript diacritical marks" that were "as easy as possible to print" (Herzog et al. 1934: 630): for example, p^w (labialized), p^y (palatalized), $^n p$ (nasalized).

[206] Anderson (1985) distinguishes phonology and phonetics through a different emphasis on rules or representation: phoneticians sought to represent every phonetic difference accurately; phonologists sought to represent only those sounds that could not be predicted by rule-governed regularity.

represented “all the pertinent facts and only the pertinent facts”: “Each sign in an alphabet represents one phoneme, and the implicit or explicit definition of each sign is an account of the norms (and deviations) of the phoneme in the various positions it may occur” (125). Besides economical, phonemic transcription was moreover fungible. The symbols themselves, whose value in a given language depended on their placement within a pattern, were interchangeable so long as the pattern was maintained: “a mechanical substitution of the values of the signs for the signs will reproduce the recorded forms correctly and completely” (125). Even though the sound features of phonemes in different languages could vary dramatically in their range of deviation and more so in their positional variance, Swadesh did not recommend using different symbols for each language, as a phonetic analysis might entail.[207] This “treatment would make linguistic science extremely difficult,” he opined: “It has therefore become conventional to use the same or similar signs in different languages to represent roughly similar phonemes. This method works out perfectly well as long as one does not carelessly assume standard or familiar values for given signs whenever they occur” (126). Phonemic orthography, rather than treat phonetic minutiae in precise and exhaustive detail, instead translated them into phonetic schemas that relied on a shared linguistic expertise to decipher. This choice facilitated a greater ease of standardization, reducing the demands of accuracy and making interpretations more replicable and persuasive. The symbolic realm synchronized the differing tempos of

[207] Swadesh argued that phonetic writing was lacking in four ways: it did not indicate significant phonetic units of a language (phonemes); was “overly microscopic, complex, and hard to handle”; did not “distinguish errors and distortions from normal forms”; and was “likely to be phonetically inaccurate” (127).

collection and analysis, keeping the machinery of linguistic science going even amid ebbs in fieldwork or constraints of economy.

In the latter half of the 1930s, Bernard Bloch and George Trager—the latter another of Sapir's students at Yale (1936–1938)—introduced a symbolic system that encompassed both the phonetic and phonemic approaches. They called it, informally, the "phoneticophonemic" alphabet (Bloch to Trager, 18 February 1939, *BBP*), which would later become the basis of the American phonetic notation. Together, Bloch and Trager dreamed up an alphabet to "end all alphabets": "It is guaranteed to last a lifetime, will not rust, can be used for writing any language dead or alive (including Esperanto and the Slavic tongues) either phonetically or phonemically, either correctly or incorrectly, and can be learned as a game during your spare time at home" (*ibid.*). Figure 12 shows the standardized set of symbols they established for the consonant chart, and Figure 13 the corresponding sheaves that linguists might take with them into the classroom or the field. It was through such prostheses that linguists learned to inscribe speech sounds into visual representational. In practising this language "game," they learned to distill patterns of sound from the speaking bodies of informants and transpose them onto the page. For the linguist, the symbolic realm of the page not only instantiated sound systems but also served to solidify their expertise and black-box the voice box.

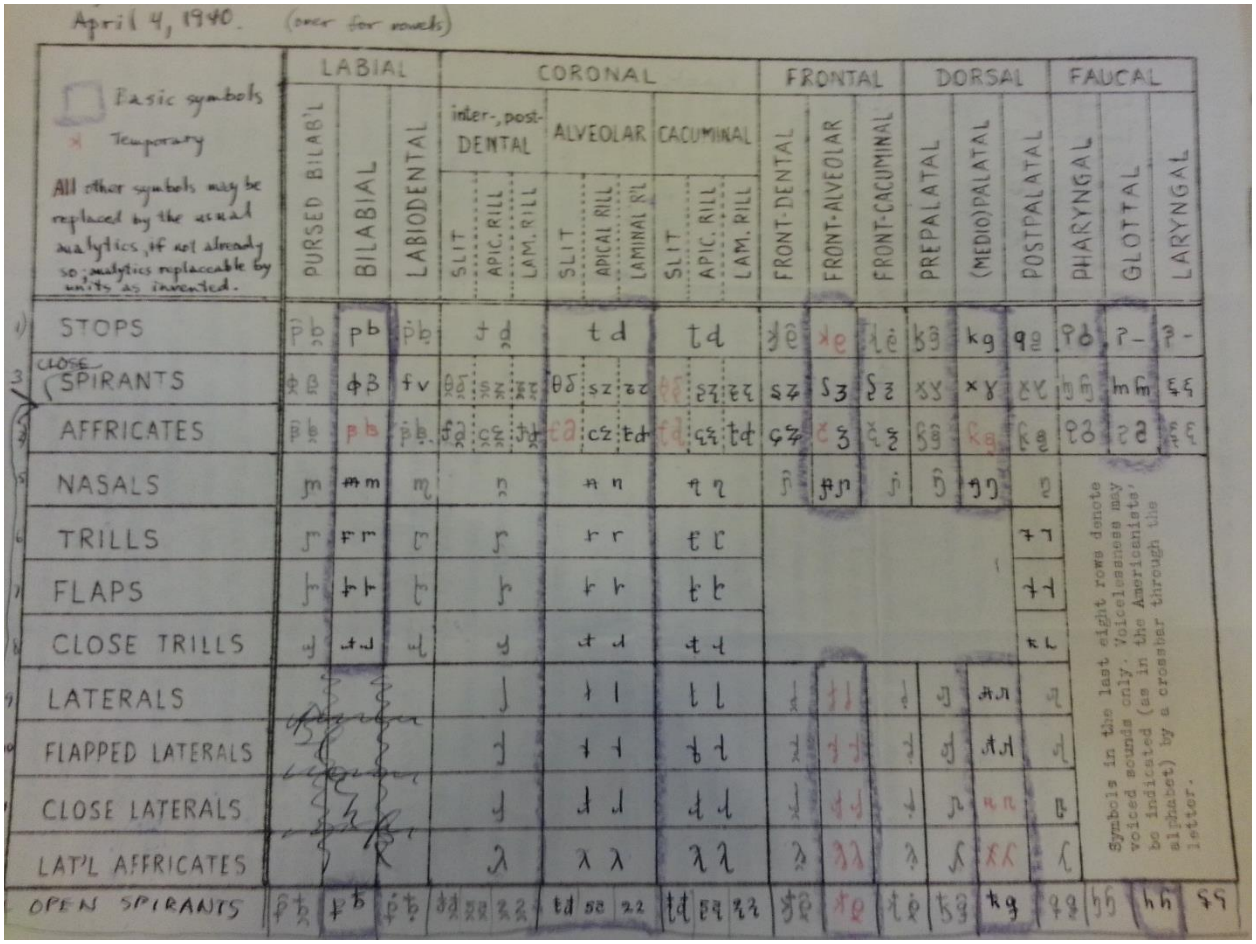


Figure 12. Full Consonant Chart (4 April 1940, *BBP*)

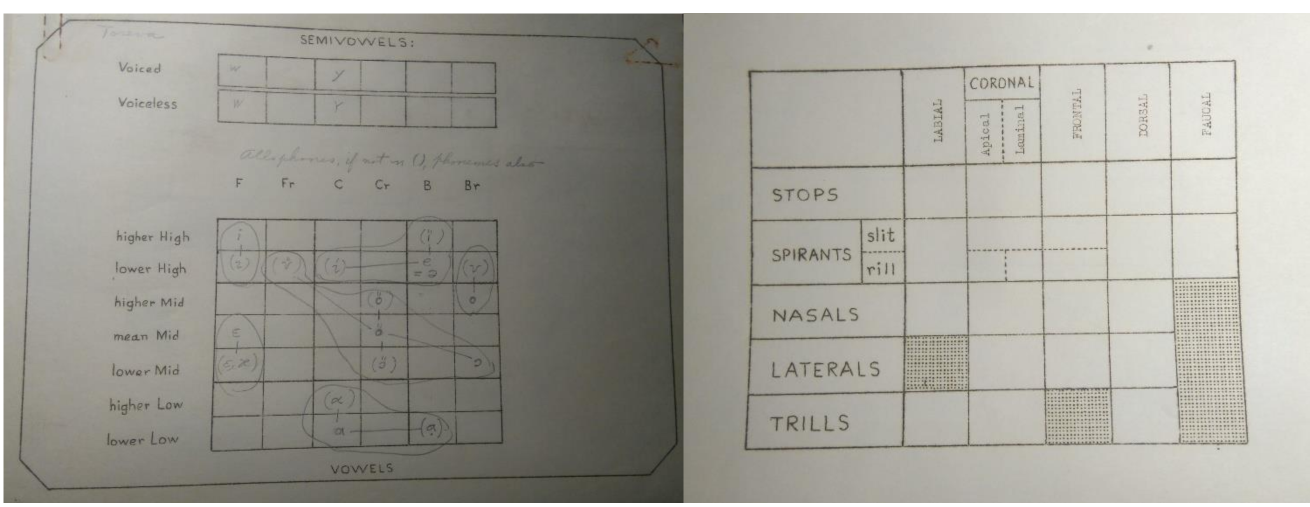


Figure 13. Vowel and Consonant Sheaves (*BWP)*

Sapir once commented: "Systems of phonetic recording have their history, like everything else cultural" (to Mason, 11 September 1936, *APS*). As we saw in Chapter 1, the standardization of transcription practices had a contested history—and so, in the following decades, it remained. Gladys Reichard, an anthropologist and linguist who also trained under Boas, concluded that the lack of agreement on a phonetic writing system was the greatest hindrance to the progress of linguistic research: "If the energy which has been expended during the past fifty years in quarreling over symbols had been diverted to the actual study of language and languages, our materials would be more adequate, our knowledge would be much greater, and our understanding more complete" (to Boas and Sapir, February 1937, *BWP*). To the contrary, I argue that the dissonance between the imperfect systems of collection and transcription was important for stimulating the discipline's growth. This remediation of language through writing recalls Derrida's (1978) concept of *différance*—but with an important difference.[208] Derrida argues that the technology of writing fissures the immediacy of the spoken sign and its correlate logic of presence. Writing distances the sign from its point of origin and hence provokes a moment for other meanings or uses of that sign to inhere. For the *bricollecteurs* of the First Yale School, the phoneme was such a tool of differentiation: it enabled linguists to abstract patterns from the physical event, dislodging language from its origins in the speaking body to another mode of materiality: writing. Where in Derrida's analysis, the technology of writing ruptures the rational self that was believed ever-present in European thought, here it functioned to braid the oral languages of Indigenous cultures into a Western knowledge-

---

[208] The term *différance* plays off the French words for "differ" and "defer," respectively a difference in space and in time.

taking system. The rational agents of this settler-colonial collection project employed writing in concert with their trained judgement to absent the speaking bodies of their informants and extract linguistic data therefrom. However, as I will show in the sections below, the synchronization of systems of languages with systems of writing failed to control the drift characteristic of either.

e. [Classifying Genres of Collections]

Specialized alphabets were not the only material-discursive practice through which linguists captured the voices of their informants and consolidated their scientific expertise. As Bruno Latour (1986) writes, the inscription alone does not empower science to displace that which it seeks to describe, but it is rather the "*cascade* of ever simplified inscriptions that allow harder facts to be produced at greater cost" (16; emphasis in original). Rheinberger (1997) takes up this line of argumentation, impressing upon it his notion of differential reproduction: "Languages, scientific ones not exempted, do not describe the world, they inscribe themselves into practices—whence their power, their seductive force, and the cross-fertilizing hubbub to which they give rise. Science does not work in spite of the fact that there are different languages on different operational levels, it works because there are so many of them" (142). These thoughts on discursive heterogeneity resonate with the work of Mikhail Bakhtin (1953/1986) on "speech genres." Bakhtin identifies the speech genre as a relatively stable form of communication that combines "thematic content, style, and compositional structure" (60). In everyday discursive situations, language users navigate an environment stratified with heterogeneous speech genres; within more regimented activities, such as those of the sciences, they develop secondary speech genres to formalize "complex and highly developed and organized cultural communication" (62). Charles Bazerman (1988), for one, has extended an analysis of genre to the forms of science writing, tracing how the experimental report came into the service of different scientific communities since the seventeenth century; he argues that scientific genres become conventionalized to allay recurring rhetorical problems that scientists face. These genres respond to rhetorical situations, on the one hand, but they also interact with other scientific practices to affect social reality. I follow Bazerman's model in my analysis of the generic activity of the First Yale School. Above, I showed how the linguistic encounter developed in the field as a regimented speech genre to meet the exigencies of salvage. I examine here how linguists further textured such encounters through "synchronic" grammars: the synchronic was a genre that bracketed off the historical (both in the form of comparative reconstruction and cultural ethnography) from the structural description of language, a deferral upon which future research platforms in linguistics depended.[209]

Any linguistic description must attend to some elements of language and not others. In the grammars of salvage linguistics, features of sound or word structure were usually central, alongside lexicography: "every study must include an analysis of the phonetics and morphology of the language and a fairly complete assemblage of vocabulary elements. This material constitutes a general description" (Boas et al., Report on April 1937 ACLS Conference: 29, *APS*). In the 1930s, there were two prevailing methods for organizing a grammar: "alphabetical, according to either the native language or the language of the

---

[209] The term *synchronic* refers to the structural description of languages in a single state, in contrast to the *diachronic* or historical reconstruction that compares two states. Voegelin and Harris (1947) put it this way: "In descriptive linguistics, a form is regarded as explained when found in the total structure of a single language. In historical linguistics, form is regarded as explained when the forms from which it developed reconstructed, or found in early records" (594)

linguist" and, less often, by "presenting small amounts of vocabulary scattered through a grammar, organized according to the parts of speech as they [were] analyzed" (Leeds-Hurwitz 2004: 119). The majority of linguists used these alphabetical or grammatical categorization schemes, although some, like de Angulo, recommended a semantic organization. Bloomfield (1926) explained the preference for organizing data around lexical or grammatical categories rather than semantic: "The morphemes of a language can thus be analyzed into a small number of meaningless phonemes. The sememes, on the other hand, which stand in one-to-one correspondence with the morphemes, cannot be further analyzed by linguistic methods. This is no doubt why linguists, confronted with the parallelism of form and meaning, choose form as the basis of classification" (157).[210] Constructing a grammar was a process of continual reduction, and morpho-phonemic data were the most readily systematized: "The only method which may hope to succeed in giving an exhaustive, yet finite, description of a language, accounting for all potential usages, is to organize the observed data into a system of generalizations, testing generalizations into larger generalizations whenever feasible, being always ready to experiment with an entirely new systematization if it promises to be more effective than the last" (Boas et al., Report on April 1937 ACLS Conference: 30, *APS*). The scientific grammar, in contrast to the classical prescriptive grammar, "must exercise the extremest economy in the number and complexity of [its] formulations, for otherwise [it] risks the possibility of losing the actual facts in the maze of complexity" (31).[211] Thus, the same

[210] Bloomfield (1926) defined the terms *morpheme* and *sememe* as such: "A minimum [grammatical] form is a morpheme; its meaning a sememe. Thus a morpheme is a recurrent (meaningful) form which cannot in turn be analyzed into smaller recurrent (meaningful) forms. Hence any unanalyzable word or formative is a morpheme" (155).
[211] Voegelin and Harris (1947) recounted the virtues of compact organization that grammars centered on the phoneme had epitomized:

principles of elegance and economy that marked phonetic notation also codified the linguist's scientific grammars.

The synchronic model was rapidly becoming the standard in the American tradition, notably displacing the comparative: though the two could work in tandem, the synchronic description was increasingly seen as an end unto itself rather than as a precursor to determining linguistic genealogies. In his report for the Committee, Voegelin (1937) observed the trend for publications on Indigenous languages to be primarily descriptive. He outlined the number and type of papers published in the International Journal of American Linguistics (IJAL), the only journal dedicated to Indigenous languages of America, from 1917 to 1937:

	Number of papers:
Texts (usually with grammatical notes)	17
Grammatical discussion of special features of a language	17
Grammatical discussion of all or most features of a language	14
Vocabularies (often with grammatical or comparative notes)	12
Michelson-Uhlenbeck Algonkin notes	12
Comparative studies	9
Classification studies	6

The older grammars showed some interrelations among the materials treated, usually in the form of paradigms; otherwise the materials were presented with something of an archival organization (with all the materials on nouns in one chapter, materials on the adjectives in another),—perhaps convenient but not compact. Such organizations often followed categories of meanings which reflected some kind of translation from Latin or other western grammatical tradition. These grammars were often neither exact nor consistent. For unwritten languages, a given word or morpheme (if not normalized) would be recorded in several different ways. The first great advance in the direction of exactness and consistency came with phonemic writing. (595)

Criticisms of specific Am. Ind. Languages published 6

Problems reviewed and recommended 4

(C. F. Voegelin, "American Linguistics," 1937, *APS*)

Nearly two-thirds of the papers published, he noted, were descriptive (60 of 97). The remaining focused on analytic problems in these languages, only nine of which being exclusively comparative. Voegelin cautioned readers against the thrall of epistemic things such as the phoneme and morpheme: "Technical advances such as these are obviously advantageous, but there is a danger that their seductive fascination will act as an incubus holding the worker to an exclusive occupation with descriptive material." He believed that descriptive data were "fertile crop for comparative uses and other special problems," but the sample suggested a distinct turn away from historical reconstruction to synchronic descriptions, especially with the advent of new techniques of description in the 1930s.

Voegelin himself trained in anthropology with Kroeber, Robert Lowie, and Melville Jacobs at Berkeley, studying the language and culture of Tübatulabal, an Indigenous people of Southern California. Later on, he became one of Sapir's postdoctoral students at Yale between 1933 and 1935. Voegelin reflected on the paucity of his own reconstructive training in an earlier paper. His discussion "On Being Unhistorical" (1936) commented on his recently published texts and grammar from Tübatulabal. On omitting reconstructive work, he disclosed: "I found it difficult to apply an historical point of view not because comparative data were lacking, but because I was lacking in an understanding of how to make use of these data" (345). The major advantage of synchronic grammars was their simplicity. Historical work, by contrast, was "more or less encumbered by considerations which tend[ed] to distract attention from the powerful simplicity of the central theme, from the clear-cut picture of movement arrested for a static moment" (345). Voegelin noted that Americanists were eager to discover the "genius of each language" independently, showing little interest in comparative work (345). With the aid of Whorf's historical data, Voegelin measured his synchronic account of vowel increments in Tübatulabal against a diachronic one. The two analyses revealed how different patterns emerged depending on the viewpoint of the analyst: the reconstructed material revealed a different pattern, more complex than the synchronic interpretation. Voegelin stressed how these presentation styles generated multiple ontologies of "living culture": "Subtle patterns in language and culture are difficult to delineate with certainty. Perhaps more often than is consciously admitted, alternative interpretations present themselves" (349). Historical work, for him, thus bestowed "additional perspective to a flat description" (349), and he worried that his peers would believe that the "preferred perspective is the only perspective" (350). In addition to being a worthwhile subject in itself, reconstruction could also improve synchronic interpretations, yet this genre of analysis remained in decline.

The reconstructive and phonemic principles were compatible in the linguist's eyes, but not in their practices. Reconstructive work held greater demands for data collection and analysis that did not suit the constraints of the field. The procedures for describing the phoneme and the morpheme, conversely, grew out of these rhythms. Phonemes could be discerned through "direct questioning or by observing reactions" of informants, a dialectic process of theorization and investigation: "Further deductions about the phonemic system are made after a collection of phonetic observations has been made, tabulated, checked, and various possible analyses tried out" (Boas et al., Report on April 1937 ACLS Conference: 41, *APS*). For morphemes, the procedure was similar: "to collect sentences, either by asking a bilingual native how one expresses a given meaning or by recording what one hears used. A considerable amount of material may be gotten by having natives dictate stories and other material while the student records it. It is then necessary to get a translation of then native material, if possible a word-by-word translation and also a sentence-by-sentence translation" (41). Historical reconstruction relied on the same materials, but to conduct a secondary comparative analysis it also required extensive vocabularies and consistency between datasets. The goal of historical linguistics was to establish genetic relationships, and "the final proof must depend on the discovery of consistent phonetic formulae of correspondence" (42). Building useable datasets was more time and resource intensive, and so were the interpretations that drew on them: "Since contemporary sister languages represent the end results of different lines of gradual change, the formulae of correspondence may be fairly complicated" (42). Indeed, even the results of reconstruction were unavoidably approximate: "it is hardly possible to expect an accurate reconstruction of what the sounds and structure of the original language might have been" (43). The methodological innovations of structural description meant that the linguist was no longer beholden to the process of linguistic drift that Sapir illustrated two decades earlier. The genre of synchronic description offered linguists a more controlled drift for their scientific advancement.

Even within synchronic grammars, however, styles of presentation differed. A structural description preserved different contents than did other types oriented toward ethnographic uses. Haas and Swadesh's (1933) work on Nitinat, for instance, represented an earlier style of organizing data (see Figure 14). It proceeded from an interlinear translation of a Nitinat text to a full English translation; commented on the morphological and phonetic structure of the language; derived phonological rules and performed a grammatical analysis; and finally listed a complete glossary of vocabulary items from the translated text (whose entries were connected to the text via endnotes).

TEXT AND INTERLINEAR TRANSLATION

ʼu·ʼusibä·ykuw[1]
Had so and so (each other) as chums, it is said,

tlul[2] *la·x̣uʼkʷ.*[3] *qä·ʼtl*[4]
good-looking youths. A long time, now,

ʼu·ʼusibä·yk.[5] *du·buw*[6] *wik*[7]
had so and so as chums. Both, it is said, (were) not

łutcida·k[8] *du·b.*[9] *ʼuwiyaʼtl*[10] *tlaʼɔ·ʼq*[11]
married, both. Went now to so and so the other

baʼas[12] *kʷissa·ʼtx̣.*[13] *ʼuwiy*[14] *t̓sawa·ʼk*[15]
tribe, different village. Went to so and so one

ła·x̣uʼkʷ. *tladä·ʼlaʼtl*[16] *t̓sawa·ʼk.*
youth. Stayed at home, now, one.

t̓sawa·ʼktl[17] *ʼuwiy* *tlaʼɔ·ʼq* *baʼtax̣.*[18]
One, now, went to so and so the other village.

wikuw[19] *qi·kʷa·l.*[20] *walctluw*[21]
Not, it is said, a long time away. Went home, it

yä·ʼtx̣aq.[22] *hida·wtl.*[23]
is said, yonder where they dwelt. Arrived.

"wa·saʼtlic[24] *ya·yaqsibä·yk̓aqs?"*[25]
"Where, now, might be the one whom I have as

wa·w.[26]
chum?" said, it is said.

"t̓ca·di·d,"[27] *wa·ʼtl*[28] *ʼabʼä·qsaq.*[29]
"We have not seen (him)," said, now, the mother.

ʼat[30] *qax̣ct̓laq.*[31]
But he was the dead one.

t̓a·da.ʼqat̓ltl.[115] *"cu,*[116] *tcatcabax̣aʼtlid,"*[117]
telescoped in, now. "Very well, we are ready, now,"

wa·ʼtluw, *qax̣citlt̓aq.*[118]
said, now, it is said, the (one who) had died.

hita·ʼdistluw.[119] *(cä·k.)*
Got to be on the beach, it is said. (Gosh!)

ya·latckat.suwiʼ[120]
Behold, there was on the water at the beach, it is

ʼi·x̣[121] *t̓ca·pukʷ.*[122] *ʼayistuw.*[123]
said, big canoe. Many in canoe, it is said.

hitqsu·ʼbtuw.[124] *ʼapaʼwadqsa·-*
Was put in canoe, it is said. Was kept in middle

ʼbtuw.[125] *tli·x̣ctluw,*[126]
of canoe, it is said. Paddled off, it is said,

tli·x̣citl[127] *tcitck̓a·waʼsaʼtx̣.*[128] *tli·x̣ctluw,*
paddled off dog salmon band. Paddled off, it is

walcitl.[129] *haya·ʼak̓tuw*[130]
said, went home. Was not known, it is said,

t̓a·da.ʼqatlqʼa.[131] *tli·x̣ak,*[132]
that he had (someone) telescoped within. Paddling,

tli·x̣ak. *du·kʷi·ks.**[133]
paddling. Singing a canoe-song as they went.

ʼa·tltciluʼkuw.[134] *datcɔ·ʼlaʼtluw,*[135]
Two days on the way, it is said. Saw, now, it is said,

t̓li·daqa·.[136] *datcɔ·ʼluw*[137] *t̓li·x̣stuw*[138] *t̓li·daqa·,*
smoke. Saw, it is said, red, it is said, smoke,

* A canoe-song belongs with the story at this point. The informant, however, had forgotten the song.

tlaʼu· *datcɔ·ʼl baʼas.* *t̓li·x̣stuw*
salmon. Again saw house. Red (smoke), it is

tlaʼu·, wik *x̣ax̣adaʼl.*[151] *tsu·wit*[152]
said, again, not very bright (red). Cohoe salmon

ʼux̣ʷaʼtluw.[153] *hidastluw* *tlaʼu·*
it was, now, it is said. Got to, it is said, another

baʼas. *ya·luwits*[154] *t̓calat̓cti·ʼwad*[155]
house. Behold, there, it is said, stripe-colored

t̓li·daqa·. *ʼux̣ʷaʼtl,* *ya·ltsaʼk̓aq.*[156]
smoke. It was, now, the (place) where (they were (going.

TRANSLATION

Two handsome youths were chums. They had been chums for a long time and were both unmarried. One day one of them went to live with another tribe at a different village, but the other one remained at home. Only one went to the other village.

He had not been away very long when he returned home. Arriving there he asked, "Where is my chum?"

"We have not seen him," said his chum's mother. But the fact was his chum was dead.

He asked for him for a long time, but nobody told him.

Finally someone told him. "Your chum is dead," he said. "He is dead."

Figure 14. Interlinear Translation of Nitinat (Haas & Swadesh 1933: 195, 197)

This genre of presentation was more anthropological, story-centered, and stood in contrast to structural descriptions that omitted many of these elements in favour of treating the phoneme or morpheme as the primary concern. Voegelin's (1935) report on Shawnee phonology was an example of the latter (see Figure 15). It began immediately with a table of phonemes; addressed the distribution of phonemic classes and the role of syllabification in the language (phonetic variety being determined with reference to its syllabic placement); and proceeded to a lengthy discussion of consonant variability. Absent were efforts to commemorate stories or enumerate lexical inventories.

TABLE OF PHONEMES

	Labial	Dental tongue-tip	Dental tongue-blade	Velar	Laryngeal
Voiceless Consonants					
stops and affricate	*p*	*t* (*T*)	*č*	*k* (*K*)	
fricatives		*θ*	*š*		
glottalic phoneme					?
Voiced Consonants					
nasals and lateral	*m*	*n*, *l*			
semivowels	*w*		*y*		

Vowels		
high	*i*	*o*
low	*e*	*a*

Figure 15. Phonemic Inventory of Shawnee (Voegelin 1935: 23)

Where the older genre was extensional, demonstrating the transliteration of text to grammar and then vocabulary, the structural model was intensional, foregrounding only the sound system and its rules. The choice of genre therefore had consequences for how data were organized and, in turn, what details entered into the collective memory that linguists curated. Both genres manifested the selective memory of the linguist's colonial power, but the latter increasingly bore the mark of the linguist's trained judgement and their decision to position the phoneme as the main object of linguistic analysis.

Indeed, linguists deliberated what elements of their study should be prioritized, given the limits of their resources. Approaching the end of the Committee on Research in Native American Languages, Boas and Sapir sent around a circular letter to other major figures in salvage linguistics, soliciting their views on past initiatives and future directions and on the possibility for a new society (8 February 1937).[212] Many of the respondents underscored the need of funding for research and publication; for an expansion to Latin

[212] Respondents to the circular letter included: Kroeber, Michelson, Kent, Bloomfield, Whorf, Mason, Swadesh, Newman, Voegelin, Herzog, Reichard, and Hoijer. Carl Buck, President of the LSA for 1937, appointed them as members of the Committee on a Society for American Indian Linguistics. Haas was away doing fieldwork and unable to attend the organizational meeting (to Whorf, 16 February 1937, *BWP*).

and South America; the consolidation of research platforms; and the desire to further standardize transcription symbols and disseminate phonemic training. Some, like Kroeber, lamented the dearth of comparative work. Most lauded their efforts to record the unwritten languages of North America and this work's contribution to linguistic science, particularly through the phonemic principle. Swadesh, for one, felt the "pioneers in this field ha[d] contributed more to the science of linguistics than they gained from it at the outset."[213] As a consequence of their strained budgets but also as the result of principled choices, these linguists were less inclined to publish full texts. Whorf, whose response was the most detailed, explained this rationale. He saw little need to prioritize the publication of texts because, in his view, they served now as "raw material" rather than the "final result":

> The data of a rapidly dying language can be preserved for science only in the form of a complete grammar with illustrative analyzed texts and as large as possible a dictionary—a mass of recorded texts with translations, no matter how voluminous, does not so preserve it, and assays so little linguistic ore to the ton that the future publication of texts *qua* texts should be avoided, lest it divert needed funds from the more important publications above-mentioned.
>
> (Whorf to Boas and Sapir, February 1937, *BWP*)

Texts were only valuable to him if they were worked out phonemically, a feat which most informants were not capable of performing. Where the realities of fieldwork were a leading

---

[213] Swadesh also recognized their failed ambitions: not enough collection work was being done while Indigenous languages continued to fall into obsolescence. At the same time, he believed that work "on dying languages should be done by experienced workers, because there are problems involved to which the novice may awaken only too late." Newman likewise believed it should not be necessary for the beginner "to travel several thousand miles and to waste time and effort on the many distracting problems that arise in the field, such as tracking down informants, attending to transportation in the field, arranging for shelter, food and water supply, medical facilities, etc." He suggested there were enough informants in large cities near universities to train and research.

factor in the reduction of comparative grammars, the movement away from storied to structural descriptions was a categorical decision sedimented into the genre of synchronic grammars and hence into published documents.

Structural linguistics came into even greater prominence in the following decade, and this closure of historical and ethnographic avenues was then hailed as central to the discipline's advance. Voegelin and Zellig Harris (1947) cited the shift from historical reconstruction to the synchronic description as the foundation of "modern linguistics" in America (594): historical reconstruction was "very demanding," and it was "not often profitable . . . to explain or state linguistic forms in one of these languages relative to earlier forms, or to forms in some other language" (595). In "the formulation of linguistic structure," furthermore, they discovered "problems of a mathematical or logical nature" that distinguished their inquiry from cultural anthropology (594). The science of language was a "handmaiden" no more. In this received narrative, linguists had established their autonomy in a double movement: they employed trained judgement to differentiate visual patterns from aural data and the genre of synchronic analysis to defer historical reconstruction and cultural ethnography. Within this increasingly closed system, we find an analogue in the analog approaches of salvage linguistics to the cybernetics discourses that would become dominant mid-century.[214] In the 1930s, however, that moment had not yet arrived: linguists still debated methodologies, their training and funding structures remained unstable, and their genres of analysis had not totally calcified in response to the

[214] Katherine Hayles (1999) describes the history of how information lost its body: "Seeing the world as an interplay between informational patterns and material objects is a historically specific construction that emerged in the wake of World War II" (14), precipitated by the interconnection of humans and computers. Where Hayles "recognizes and celebrates finitude as a condition of human being" within this uptake of informational technologies (5), I find similar themes in relation to the finitude of endangered languages.

exigencies of salvage. These linguists were "riding the cusp," as Katherine Hayles would put it: at the median point between real and virtual spaces. They attempted to develop a virtual, laboratory-like space on the page where they could identify totalizing patterns in language, but their efforts functioned instead in a *virtu-real* space where experimentation (linguistic encounters in the field) and simulation (modeled systems on the page) interacted. For the First Yale School, there was no closure, neither for their system of analysis—still marked, indelibly, by fieldwork—nor for their imperfect data, which they hoped—but had no proof—would be resumed. Writing, then, was not a technology of distantiation, emptying the sign of its origins; rather, it was one of dormancy, a relay signaling the intimacy of these hopes across time to an imagined future.

f. [Archiving Analog Afterlives]

In Rheinberger's (1997) theorization of the temporalities of experimental systems, he asserts that there is no totalizing exterior force guiding their development: there is "no all-encompassing theoretical framework, no overarching political program, no homogenizing social context effective enough to pervade and coordinate this universe of drifting, merging, and bifurcating systems" (181). Rather, these systems are characterized by their own internal time-scales, "shifting and drifting in an open horizon" (181), with their relative "age" a product of their continued ability to produce novelties. Rheinberger defers the conditions of emergence, which to him "seem accessible only by way of a recurrence that requires the existence of a product as a prerequisite for assessing the conditions of its production" (177). Certainly, for the *bricollecteurs* I have followed in this chapter, neither the impetus of salvage nor the practicalities of data collection overdetermined the phoneme or other epistemic things, which acted productively—and still do—in other branches of linguistics. However, the coordination of the linguist's experimental system with the sound systems of Indigenous languages was not an open horizon: it was embedded in the colonial mechanisms that foreclosed upon Indigenous sovereignty over their land, culture, and language. The temporalities of this knowledge-taking system, rather than eking toward an always already present "vanishing point" (Rheinberger, 185), reinscribed the surmised temporariness of these vanishing voices. Returning to the originary in this context is, in Byrd's (2011) words, a recognition that "Indigenous peoples, [their] ongoing colonialization, and [their] historical dispossessions and genocide continue to be pushed toward a vanishing point within critical theory and diaspora studies" (3)—and within science and technology studies, as well. In this final section, my most speculative, I reflect on the traces of the symbolic systems that the First Yale School assembled and the futures of the data they archived, which are now being reclaimed by the Indigenous communities from which they originated. Out of these entangled lines of descent, I contemplate how the metaphor of language-as-a-living-entity has become differently materialized in the archives of linguistics and in the practices of language revitalization movements.

The Committee on Research in Native American Languages came to an end in 1937 and with it much of the infrastructure for the collection, publication, and analysis of Indigenous languages of North America. The ACLS appointed other committees to assess the situation and find solutions that would facilitate future research. Boas recognized that a "few specialists and a limited number of university libraries [would] be the only purchasers of publications that deal[t] with a single American language," and he hoped to enlist the cooperation of universities to perpetuate this line of study: "It might be well if a successor to our Committee, armed with the prestige of the Council and the inducement of financial contribution, tried to bring universities to undertake systematic work in the recording of American languages" ("Report on the Committee on Research in American Native Languages," 1938: 3, *APS*). Publication remained "hampered by lack of funds," with even the $1000 allotment for manuscript completion reduced to $500, so that a majority of data remained in manuscript form: "We are in the unfortunate position that a great deal of material has been collected by the Committee but very few opportunities for publication have presented themselves, so that a large amount of material remains in manuscript" (Boas, "Report on the Committee on American Indian Languages," January 1941, *APS*). Further exacerbating these conditions, the International Journal of Linguistics ceased operations between 1939 and 1944, coinciding with the war and the deaths of Sapir (1939) and Boas (1942).[215] With the loss of these important nodes for organization, publication, and leadership, not only were the languages that linguists studied in peril, but their data too seemed threated to fall into obsolescence.

This exigence was allayed not through genre but the archive. Voegelin and Florence Robinett's report on "Obtaining a Linguistic Sample" (1953, *APS*) addressed the topic of language archiving, much needed to safeguard existing data in the decades following the collapse of salvage linguistics. Based on a four-day conference at Indiana University held in conjunction with the concurrent Linguistic Institute, Voegelin and Robinett's report focused on the practices of collecting, archiving, and analyzing data; archiving they divided

[215] The International Journal of American Linguistics was suspended due to lack of funds (Boas to Whorf, 24 November 1937, *BWP*).

into the tasks of storing, cataloguing, and indexing. Their principal example was the Franz Boas Collection.[216] At the time, the Boas Collection consisted of seven large steamer trunks that "Boas managed to cram with manuscripts—his own, those of contributors to the International Journal of American Linguistics, and those of graduate students" (2), much of them unpublished and even unedited written materials.[217] The authors highlighted how the process of cataloguing transformed the haphazard collection into an organized archive: "The Franz Boas Collection was changed or transformed from the status of being in storage—when it was in the basement of the American Council of Learned Societies in Washington—to that of being catalogued when it was classified by Voegelin and Harris for the American Philosophical Society in Philadelphia" (4).[218] Voegelin and Robinett further opined that such archives would need to store materials "either for a brief period of time, or else for decades" (2).[219] Accordingly, they assigned a newfound importance to the archive and the archivist in linguistic analysis: "The Archive will stimulate returning to the field, both for the sake of solving problems in analysis and for collecting additional materials to enable resident scholars to make additional analysis in the Archive" (7). They regarded analysis as a "post-storage, post-cataloguing, post-indexing task" (7), but the

---

[216] Boas's son donated the collection to the ACLS after his father's death and, in 1945, the ACLS presented it as a gift to the American Philosophical Society, which placed it in their library in Philadelphia.
[217] Voegelin and Harris (1945) offered a detailed index of the collection's range of contents: "Some manuscripts were written before [IJAL] was founded. . . . Some manuscripts are unorganized and appear to be field notes[.] . . . Some manuscripts have been published more or less in their entirety[.] . . . In many cases manuscripts have been published in part, or used as a basis of subsequent publication" (9).
[218] Voegelin and Harris made a decision to index the contents of the Boas Collection by separate languages. Voegelin and Robinett (1953, *APS*) named other possible systems of classification, "such as area of provenience, such as dialect, . . . or such as language family. Or the classification can be based on a common type of language (irrespective of linguistic history)," i.e. "typological" (4).
[219] Voegelin and Robinett also considered the problem of phonographic preservation: "Tape recordings are most efficient because they can be played back in stretches shorter than a word, and because their fidelity is not lowered in the slightest, no matter how frequently they are played back. Phonograph discs are known to have a long shelf life but deteriorate when played back. The shelf life of tapes is not now known" (2–3).

archive itself was not oriented to the posterior but the future. Synchronic descriptions were not therefore timeless, but marked by the temporalities first of salvage and then of the archive. They were not inert, or “arrested” in the present moment, but inertial: linguistic and epistemic systems drifting together into a speculative futurity, vibrating vocal cords reverberating in the vibrant matter of these records.

Reverberation refers to the phenomenon of sound being caught in an echo chamber, reflecting cacophonously off its surfaces until decaying as the sound becomes absorbed into objects in space and time. To an extent, reverb characterizes the fate of sound systems reflected in the archives of linguistics. The linguist’s trained judgement winnowed the channel of sound to categorical patterns and their writing systems absorbed the frenetic flow of fieldwork to produce manageable data, enfolding languages into the interior of their experimental system. The linguist’s mastery required a closure of the box to fortify their methodology as scientific—here, instantiated by the literal and existential remainder of black boxes in the archive, inside of which over 160 Indigenous languages have remained, dormant.

Figure 16. Photograph of the Archive's Black Box (Mary Haas Papers, *APS*)

As Derrida (1995) writes, the archive as a site does not embody the closure of the past but opens instead to the future:

> the question of the archive is not, we repeat, a question of the past. It is not the question of a concept dealing with the past that might already be at our disposal or not at our disposal, an archivable concept of the archive. It is a question of the future, the question of the future itself, the question of a response, of a promise and of a responsibility for tomorrow. The archive: if we want to know what that will have meant, we will only know in times to come, later on or perhaps never. A spectral messianicity is at work in the concept of the archive and ties it, like

> religion, like history, like science itself, to a very singular experience of the promise. (36)

The promise of salvage linguistics was elliptical and always already out of reach. When their boxes are re-opened, the spell of mastery is broken: the indeterminacy of these languages is reprised, and not even the expert linguist knows how or where they will go. The future that linguists anticipated in preparation of these archives is not now the present that we inhabit. The archive has become a platform for the uncharted "afterlife" of these boxed languages and for Indigenous revitalization movements that open these boxes and re-verb their speech with the voices that have persisted therein. The failures of the linguists' theorizations and the incompleteness of their empirical work, in this sense, become a site of creative recuperation, innovation, and play.

At this juncture, I remove the brackets (and scare quotes) from around the metaphor of language-as-a-living-entity that has underwritten my chapter. When we speak about endangered languages, we refer to a complex of issues, both matters of fact and matters of concerns. As such, the topic requires an account not only of the materiality of archives and the methods of collection and preservation that substantiated them but also of the semiotics through which they were and are transmitted and understood. I take the metaphor seriously not to reify an organicist understanding of language, which reached its zenith in the nineteenth century (Alter 1999), but to consider how this conceptual mapping has nonetheless informed the chimerical practices of linguistics and language revival. Even Sapir, who rejected a "superorganic" account of language and culture (1917), drew on it figuratively to discuss language obsolescence: "Language is probably the most self-contained, the most massively resistant of all social phenomena. It is easier to kill it off than to disintegrate its individual form" (1921/2004: 170). Criticisms of its recent application in biocultural diversity studies point out the inconsistencies of the organism metaphor: the "ecologizing" comparison obscures vastly different mechanisms of "extinction" (for instance, by a speech community shifting to a different language); furthermore, it exoticizes languages by associating them with rarefied animal species (Cameron 2007: 272, 281). What both proponents of this metaphor and its discontents have in common, though, is their use of the metonymic: each substitutes concerns over language for closely associated parts. Language endangerment comes to stand in for a constellation of other meanings and values, including the self-determination of speakers and their communities (Perley 2011), the ecological value of linguistic diversity (Nettle & Romaine 2000), the affects of "intergenerational continuity" and minority rights (Fishman 1991), and the effects of a globalized economy (Heller & Duchêne 2007). Without detracting from these issues, I observe that each consequently brackets off the question of "language itself." These displacements reduce the nature of language to a passive reflection of social structures, political economy/ecology, or ideology, leaving its ontology available only to linguists and their forms of expertise.[220]

As I have shown in this chapter, salvage linguistics encountered language endangerment as a symbolic problem: in response, they trained their judgement to condense speech into patterns and, through specialized orthographies and genres of presentation, transformed patterns into an assemblage of data on the page. Rather than understand the situation of language endangerment and revival through symbolism (a

[220] In their guidebook to language revitalization, Grenoble and Whaley (2006) suggest that local communities consult linguists as experts to help create language programs (192), rather than examining the values sedimented within linguistic expertise.

substitution of languages for rules and representations) or metonymy (a substitution of languages for their associated parts), I argue that revitalization should be treated instead through the figure of analogy. In this reconfiguration, I follow Paul De Man's (1983) formulation of the symbolic and allegorical (an allegory being otherwise a sustained analogy). De Man analyzes the symbolic as figural language that renders life and its forms identical along one spatialized plane, "always part of the totality it represents" (192); by contrast, the allegorical refers to a dialectic of subject and object, of an original meaning and its proxy, both "unveiling" in time (206). Where the symbolic corresponds to a relation of simultaneity, within the allegorical the sign consists of repetition: it has no identity, only a distance in relation to its origin (207). The aim of salvage linguistics was symbolic in the manner that de Man articulates: it sought to spatialize data on Indigenous languages along the static space of the page. Revitalization movements, by contrast, are analogous: they occur not in the confines of one flat system, but in the multidimensional relations of linguistic and epistemic systems unfolding over time. As Shaylih Muehlmann (2015) observes, the brackets around such archived languages are artefactual, referring to a point of origin but increasingly distant from it: they are not based on "internal consistency," but "rather on the historical circumstances that allowed them to be identified, documented, and standardized" (51). Regardless of their origins, Muehlmann explains that people's experience of "language practices and identity" nonetheless matters to their feelings of cultural belonging. Indigenous communities must employ settler categories even as they seek to rupture them, their becomings entangled but in a state of mutual unravel.

Revitalization initiatives thereby re-introduce the problem of the signifier into structural linguistics, from whence it had been so carefully bracketed off, and bring to the fore an ontology of language that is indeterminate, contingent, and multiple.[221] The material-semiotics of these boxed languages provides an opening for us to think about linguistic performativity on a different scale. Questions of language preservation ask that we take into account relationships of responsibility and response-ability beyond the scope of individual speaking bodies—indeed, to reconsider what can be acknowledged as a "body" in relation to languages and what bodies are required for their (re)production. In *Beyond Settler Time*, Rifkin (2007) mobilizes queer theory to show how ordinary experiences reveal the inertia of dominant formulations: they "help open ways of registering the imposed *straightness* of time while also highlighting alternative kinds of temporal experience" (37). In an analog(ous) way, the temporalities of revived languages have queered the genealogical model of language transmission: forms of speech are passed not from "mother" to "daughter" languages, but laterally through a salon of material actors. The queer genealogies of linguistic revival show that caring for language in its multiple ontology means recognizing, too, the stuff of language. This recognition also queers "the signifier's collapse into the letter's cadaverous materiality" (Edelman 2004: 7), revealing this materiality to be a lively space and open horizon of futurity. By positioning the linguist's archives as the scene of these intersections, I foreground the work that goes into assembling languages and their nature, arguing that they are "made to matter" differently in the environments that reproduce and activate them. As such, linguists must learn to think in allegorical terms when constructing their archives, anticipating a future not only for their successors but also for their failures. They must take into account how the

[221] I play off Annemarie Mol's (2002) argument for multiple ontologies of the body that, in the same gesture, reduces language (or "talk") such that it occupies a purely referential function (objects become clustered under the "same name").

reverberations reflected in the surface of their writing systems might decay, both materially and in the loss of metadata. They must also consider how their traces will get taken up again by the communities they once consigned to loss. In what other ways can the nature of language be included in discussions of its politics, rather than bracketed off and left only for linguists or grammarians to play with? Time will tell.

Conclusion: The Transit of Linguistics.

Time cycles and circulates, accelerates and decelerates, refigures and defigures the archives of linguistics. Amidst this shuffling of countless pages, the zeitgeist of salvage faded, the linguist's experimental system found other productive epistemic things, and a "Neo-Bloomfieldian" structuralism predominated. The work of the First Yale School was disrupted by the end of the Committee on Research in Native American Languages, the deaths of their intellectual leaders, their recruitment into the Army Specialized Training Program during the War, and their institutional scattering afterwards. Through the ASTP, however, linguistic methods found a new application: training soldiers and army personnel in strategically important languages. Haas (1943) expounded on the newfound role of linguist as teacher. She explained how the best practice of instruction employed a trained linguist alongside a native speaker of the target language:

> He [the linguist] persuades the informant . . . to talk in the foreign language; he listens carefully, and writes down what the informant says in a phonetic alphabet, which he converts as soon as possible into a practical orthography (a phonemic transcription); he compares and analyzes the forms of the new language; and classifies them in terms of its own grammatical system,

> without reference to the grammar of English or of any other language previously known to him. (205)

The linguist then guided students through a similar process, imparting onto them the same techniques of elicitation that Sapir pioneered decades earlier. The temporalities of fieldwork continued to reverberate in their methods, now matched by the demands of an intensive army training program. Once again, the "Indian" was the field of transit for linguistics: each time a proof of the success of their science. Mastery of the phoneme therefore incited the transit of linguistics from an elliptical network and inchoate identity to a scientific discipline with a valued expertise. The phoneme captured the linguist's knowledge of sound systems, but it also reiterated the salvage paradigm's logic of capture: the phoneme, as I have shown, was a more-than-linguistic substance that conjured an assemblage of social and material actors and reproduced power relations between linguists and their informants. Other epistemic systems have taken hold in linguistics since then, but neither the linguist nor the phoneme can be fully extracted from the unsettling history of salvage or its unsettled futures.

~ Conclusion ~

"Edward died with the feeling that he had an important point to make that he hadn't managed to get across. He gave up even hoping to get it all written, even before he accepted the fact that he was ill. His work on language was such a pleasure to him that he was able to remain 'busy' in that manner, but he did deeply feel that he died without saying his full say!"

- Jean Sapir, 1967

Toward an Intra-Linear Linguistics

Through this dissertation, I have told a thematic history of American linguistics from the perspective of its failures: my three chapters reconstructed the dynamics of space, identity, and time in the formation of this human science and dwelled on their imperfections and incompletions. I examined how Edward Sapir, a talented linguist, adapted to his role on the Geological Survey of Canada by employing an elliptical network of actors to retrieve intimate linguistic data from his distant offices at the Victoria Memorial Museum; there, Sapir learned to negotiate both the expanse of Canada and the limited recognition his work accrued within anthropology and museum culture. Professional and geographic circuits for linguistics drew closer together at Yale University, an institutional centre for linguistic research in the 1930s, around which the next two chapters revolved. I turned to Benjamin Lee Whorf, Sapir's controversial apprentice, and the mistranslations that plagued Whorf's career across his lifetime and beyond; this "amateur specialist" has dashed contemporary critics' expectations for a stable scientific identity because, in his generation, the category of "linguist" had only begun to form—and because Whorf himself was attuned to frequencies other than the academe. Finally, I attended to the First Yale School, led by Sapir, and the experimental system they worked out in the field and on paper to capture linguistic sound systems; tracing the development of the phoneme, their chief object of study, prompted further reflection on the transience of these linguists' collective memory practices and the loosening brackets surrounding their archives today. Failure emerged at every turn in the disciplinary history of American linguistics, but it was most evident in the enduring failure of the salvage mentality that animated these collection practices across three and more decades: specifically, the focus on extracting knowledges and cultural artifacts from Indigenous communities without contributing to the continuation of their civilizations.

As the epigraph indicates, Sapir ended his life thinking he was a failure: linguistics always approached the pole of perfection but, for him, never reached it. Indeed, following his death was a rapid dissolution of the scientific network that he had helped to establish for the study of Indigenous languages in America. In addition to discontinued patronage, his protégé Whorf died in 1941, his mentor Franz Boas in 1942, and Leonard Bloomfield (who took over the Sterling Professorship of Linguistics at Yale in 1940) suffered a disabling stroke in 1946. His other students, like Mary Haas, were scattered (geographically and epistemologically) by World War II: "people had been dragged into doing unusual languages that they had never done before during the War" (Haas 1976: 367, *MHP*). The "Neo-Bloomfieldians" of the next two decades—though they were the first generation to be employed as "linguists" in America—focused all but exclusively on synchronic descriptions of languages and eschewed the historical work to which Sapir had dedicated much of his career. No linguist ever fully recuperated the scope of Sapir's scientific interests or resumed his incomplete intellectual or empirical projects. His legacy was one of thwarted ambitions, but consistent with the manifold scenes of failure that interleaved the emergence of an independent science of language in America.

The great success of linguistics during and after the War also occasioned a divestment from the salvage paradigm, a forgetting upon which the discipline's humanistic ideals later depended. Linguists continued to elaborate on the experimental system that Sapir and his cohort worked out on Indigenous languages, but in application increasingly derived its productivity from more available world languages and through formalism rather than fieldwork. Ultimately, the most memorable tenet of the First Yale School proved to be the incomplete postulate of its most peripheral member: Whorf's theory of linguistic relativity. The "Sapir-Whorf hypothesis" re-emerged in the 1950s amid a new context of cultural relativism and liberal multiculturalism. Within anthropology, relativism had been employed methodologically to gain access to foreign cultures and ideologically to mobilize doubt against ethnocentric Western worldviews (Hollinger 2003). However, in part due to the influence of popular writers like Margaret Mead, the topic travelled beyond its initial context to other disciplines (among them the history of science through Thomas Kuhn) and into mass culture. The rise of postwar cultural relativism seized upon Whorf's notion that each language embodies a unique cultural perspective, which made sense as part of a growing rationale to appreciate (or at least tolerate) diversity. What was lost in these re-articulations, though, was a sense of the social and material conditions that a language needs to thrive. Attention to these conditions was characteristic of, and indeed inextricable from, the project of salvage linguistics and its practices of fieldwork and archiving. How might practitioners of linguistics today "salvage" this commitment to the materiality of language but re-orient its political force from the extractive agenda of its forebears?

As Donna Haraway (2016) writes: “It matters what stories we tell to tell other stories with; it matters what concepts we think to think other concepts with” (118). My dissertation has re-traced these scars of knowledge not to revel in their *kairosity* but to demonstrate how linguists have inhabited spatial dimensions, identity categories, and temporal orientations—and to suggest how they might otherwise. To trouble these modes of being and becoming, I conclude by suggesting an “intra-linear” style of linguistic inquiry. Drawing from the conceptual work of my previous chapters, the *intra-linear* names a nexus of concerns about the ways that linguists render languages and their speakers, even (or perhaps especially) when trying to keep their distance. Unlike the interlinear method of translation, a four-tiered technique for the extraction of linguistic and cultural data, an intra-linear approach takes seriously the consequences of colonial regimes and the complicity of the sciences therein. Rather than becoming linguists at the expense of these communities, they could *become-with* them, abdicating the intimate distance of their formalisms for the messiness of co-production or, following Kim TallBear (2013), “co-constitution.”[222] An intra-linear linguistics would develop its categories of analysis in participation with local communities, conscious of their contingency, and together they would learn to care for language in its indeterminate ontology, responsive and responsible to its fragile materiality. Linguistics was and will always be far from perfect, but a recognition of its failures could generate the opportunity to do good in the present.

[222] In her analysis of Native American DNA, Kim TallBear (2013) employs Sheila Jasanoff’s term *co-production* to discuss the mutual constitution of natural and social orders. She re-names it “co-constitution” to avoid the “overly constructionist tone” of the former (23).

❖ Future Research.

Here, at last, I set out to demarcate the failures that took hold in my own knowledge-making project which, nonetheless, chart a course for future research opportunities.

Horizontally, I would like to address the greater work of the vast institutionalized projects I encountered: namely, the Anthropological Division of the Geological Survey of Canada and the Committee on Research in Native American Languages. My first chapter explored only one of the four Boasian subfields that Sapir was responsible for managing; my third chapter focused on a narrow, if decisive, subset of researchers involved in the documentation of Indigenous languages. By attending more comprehensively to these initiatives, I could better relate linguistics to the history of anthropology. Alternatively, I could situate linguistics in relation to other human and social sciences of the times, especially on the subjects of sound recording and expert listening. Sapir began learning psychoanalysis before leaving Ottawa for the University of Chicago, where he befriended Harry Stack Sullivan and other psychologists. What influence did the psy-disciplines and these Chicago years have on Sapir's model of objectivity that he might have also imparted onto his students?

Vertically, I might think about the genealogy of the language-as-a-living-entity metaphor and the ways it has manifested in linguistic models and methodologies, intentionally or inadvertently. Looking backward from the 1930s, I could examine the interchange between linguistics and natural history in the nineteenth century. Looking ahead, I could track how linguistic models premised on this metaphoric transfer interacted with informational theories of life that permeated the cybernetics discourses of the mid-twentieth century. These routes could show how the language of linguistics has been informed by breakthroughs in the life sciences, or how that metaphoric language has in turn informed lay and scientific understandings of community and kinship.

Perpendicularly, I could build on my reception history of Whorf's linguistic relativity hypothesis. My chapter focused on audiences that were his contemporaries, like the Institute of General Semantics. However, perhaps the real story begins not with Whorf but with Harry Hoijer's 1953 Chicago conference on "Language in Culture" that re-introduced his ideas into a new context and currency, to which I alluded above. How might our understanding of the so-called Sapir-Whorf hypothesis change through a comparison of these two historically situated moments? Furthermore, how has the hypothesis itself taken on a life of its own and influenced other popular and scientific works irrespective of Whorf's initial conception or Hoijer's re-framing?

Intra-linearly, with the cooperation of linguists and Indigenous partners, I could consider the data histories of archived languages and the partial recuperations that now characterize their analog afterlives. This data has been taken up and re-materialized in (non)governmental organizations, new scientific practices, and local communities; it has been re-presented in discourses of language endangerment and their discontents. How does the situation now compare to the one prognosticated by the collectors and archivists of nearly a century ago? How do contemporaries interact with the modes of trained judgement embedded in the contents of these archives? Additionally, I might consider the phenomenon of present-day digital "language museums" and the role of language curators therein. How are new media transforming languages and linguistic sovereignty? And has linguistics finally found its place in museum display culture?

Bibliography

Archival:

A. L. Kroeber Papers, 1869-1972 (BANC MSS C-B 925). The Bancroft Library, University of California, Berkeley.

American Council of Learned Societies Committee on Native American Languages, American Philosophical Society.

Benjamin Lee Whorf Papers (MS 822). Manuscripts and Archives, Yale University Library.

Bernard Bloch Papers (MS 1129). Manuscripts and Archives, Yale University Library.

C. F. Voegelin Papers, American Philosophical Society.

Edward Sapir (I-A-236M). Archives, Canadian Museum of History.

Franz Boas Professional Papers, American Philosophical Society.

George Trager Papers (MS-M005). Special Collections and Archives, UC Irvine Libraries, Irvine, California.

Mary R. Haas Papers, American Philosophical Society.

Primary:

An amateur specialist. (1941, July 29). The New York Times. 14.

Arnold, T. W. (1937). *The folklore of capitalism*. New Haven: Yale University Press.

Barbeau, M. (1915). Classification of Iroquoian radicals with subjective pronominal prefixes. Memoir 46, Anthropological Series, Geological Survey of Canada. Ottawa: Government Printing Bureau.

Benedict, R. (1934). *Patterns of culture*. Boston: Houghton Mifflin.

Bloomfield, L. (1925). Why a linguistic society? *Language, 1(1),* 1–5.

———. (1926). A set of postulates for the science of language. *Language, 2(3)*, 153–164.

———. (1933). *Language*. New York: H. Holt, Rinehart and Winston.

———. (1939). Linguistics aspects of science. *International Encyclopedia of Unified Science, 1(4)*, 1–59.

———. (1942). Philosophical aspects of language. *Studies in the history of culture: The disciplines of the humanities (presented to Waldo Giffen Leland)*. Menasha, WI: Banta. 173–177.

Bloomfield, L., & Bolling, G. M. (1927). What symbols shall we use? *Language, 3(2)*, 123–129.

Boas, F. (1911). *Handbook of American Indian languages*. Washington: Government Print Office.

———. (1920). The classification of American languages. *American Anthropologist, 22(4)*, 367–376.

———. (1963a). *Introduction to the Handbook of American Indian languages*. Washington: Georgetown University Press, Institute of languages and linguistics. (Original work published 1911)

———. (1963b). *The mind of primitive man* (Rev. ed.). New York: Collier Books. (Original work published in 1911)

Boas, F., et al. (1916). Phonetic transcriptions of Indian languages: Report of committee of American Anthropological Association. *Smithsonian Miscellaneous Collections, 66(6)*. Washington: Smithsonian Institution.

Carnap, Rudolf. (1955). Logical foundations of the unity of science. In Neurath (Ed.), *Foundations of the Unity of Science*, 42–62.

Chase, S. (1938). *The tyranny of words*. New York: Harcourt, Bruce and Company.

———. (1956). Foreword. In J. B. Carroll (Ed.), *Language, thought and reality*, v–x.

Dixon, R. B., & Kroeber, A. L. (1913). New linguistic families in California. *American Anthropologist, 15(4)*, 647–655.

Frank, J. (1938). *Save America first: How to make our democracy work*. New York: Harper.

Goddard, P. E. (1920). Has Tlingit a genetic relation to Athapascan? *International Journal of American Linguistics, 1(4)*, 266–279.

Herzog, G., Newman, S., Sapir, E., Haas-Swadesh, M., Swadesh, M., & Voegelin, C. (1934). Some orthographic recommendations. *American Anthropologist, 36(4)*, 629–631.

Haas, M. (1938). Geminate consonant clusters in Muskogee. *Language, 14(1)*, 61–65.

———. (1940). Ablaut and its function in Muskogee. *Language, 16(2)*, 141–150.

———. (1941). Noun incorporation in Muskogean languages. *Language, 17(4)*, 311–315.

———. (1943). The linguist as a teacher of languages. *Language, 19(3)*, 203–208.

———. (1945). Dialects of the Muskogee language. *International Journal of American Linguistics, 11(2)*, 69–74.

———. (1946). A Proto-Muskogean paradigm. *Language, 22(4)*, 326–332.

———. (1947). Development of Proto-Muskogean *k^w. *International Journal of American Linguistics, 13(3)*, 135–137.

Haas, M., & Hill, J. (2015). *Creek (Muskogee) texts*. J. B., Martin, M. M., Mauldin, & J. McGirt (Trans. and Eds.). University of California Press.

Haas Swadesh, M., & Swadesh, M. (1933). A visit to the other world, a Nitinat text (with translation and grammatical analysis). *International Journal of American Linguistics, 7(3/4)*, 195–208.

Hayakawa, S. I. (1939). The meaning of semantics. *New Republic,* August 2, 354–357.

Hoijer, H. (1954). The Sapir-Whorf hypothesis. *Language in Culture*, 92–105.

Hoijer, H., ed. (1954). *Language in culture:* C*onference on the interrelations of language and other aspects of culture*. Chicago: University of Chicago Press.

Jenness, D., Jenness, S., & Canadian Museum of Civilization. (1991). *Arctic odyssey: The diary of Diamond Jenness, ethnologist with the Canadian Arctic Expedition in Northern Alaska and Canada, 1913–1916*. Hull, Quebec: Canadian Museum of Civilization.

Korzybski, A. (1958). *Science and sanity: An introduction to non-Aristotelian systems and general semantics*. (4th ed.; with new preface by Russell Meyers.). Lakeville, Conn.: International Non-Aristotelian Library Pub. Co.; Institute of General Semantics, distributors. (Original work published 1933)

Kurath, H., Hanley, M. L., Bloch B., et al. (1939–1943). *Linguistic atlas of New England (LANE)*. 3 volumes. Providence: Brown University for the American Council of Learned Societies.

Mead, M. (1928). *Coming of age in Samoa: A psychological study of primitive youth for Western civilisation*. New York: W. Morrow & Co.

Morris, C. (1955). Scientific empiricism. In Neurath (Ed.), *Foundations of the Unity of Science*, 63–75.

Neurath, O. (1955). Unified science or encyclopedic integration. In O. Neurath (Ed.), *Foundations of the Unity of Science*, 1–27.

———. (1973). *Empiricism and sociology*. M. Neurath & R. S. Cohen (Ed.). Dordrecht: Reidel.

Neurath, O., ed. (1970). *Foundations of the Unity of Science.* Chicago: University of Chicago Press.

Neurath, O., Carnap, E., & Hahn, H. (1929). Wissenschaftliche Weltauffassung: Der Wiener Kreis. Vienna: Wolf. Trans. as ''The Scientific Conception of the World: The Vienna Circle'' in Neurath (1973), 299–319.

Radin, P. (1919). The genetic relationship of the North American Indian languages. *American Archaeology and Ethnology, 14(5)*, 489–502.

Report of the Special Committee on the Linguistic Institute. (1940). *Language, 16(1)*, 83–101.

Sapir, E. (1911). An anthropological survey of Canada. *Science, 34(884)*, 789–793.

———. (1912). The work of the Division of Anthropology of the Dominion Government. *Queen's Quarterly, 20,* 60–69.

———. (1915). Abnormal types of speech in Nootka. *Memoir No. 62, Anthropological Series No. 5, Geological Survey of Canada*. Ottawa: Government Printing Bureau.

———. (1916). Time perspective in Aboriginal American culture, a study in method. *Memoir No. 90, Anthropological Series No. 13, Geological Survey of Canada*. Ottawa: Government Printing Bureau.

———. (1917). Do we need a "superorganic?" *American Anthropologist, 19(3)*, 441–447.

———. (1921). A bird's eye view of American languages north of Mexico. *Science, 54(1400)*, 408.

———. (1924). Culture, genuine and spurious. *American Journal of Sociology, 29*, 401–429.

———. (1925). Sound patterns in language. *Language, 1(2)*, 37–51.

———. (1927). The unconscious patterning of behavior in society. In E. S. Dummer (Ed.), *The Unconscious: A Symposium*. New York: Knopf, 114–142.

———. (1929a). Central and North American Languages. *Encyclopaedia Britannica (14th ed.), 5*, 138–141.

———. (1929b). The status of linguistics as a science. *Language, 5(4)*, 207–214.

———. (1931). Conceptual categories in primitive languages [Abstract]. Science, 74, 578.

———. (1949). The psychological reality of phonemes. In D. G. Mendelbaum (Ed.), *Selected writings of Edward Sapir in language, culture and personality*, 46–60. Berkeley: University of California Press. (Original work published in 1933)

———. (1963). A study in semantics. In D. G. Mandelbaum (Ed.), *Selected Writings of Edward Sapir in Language, Culture, and Personality*, 122–149. Berkeley: University of California Press. (Original work published in 1944)

———. (2004). *Language: An introduction to the study of speech*. New York: Dover. (Original work published in 1921)

Spier, L., Hallowell, I., & Newman, S., eds. (1941). *Language, culture, and personality: Essays in memory of Edward Sapir*. Menasha, Wis.: Sapir memorial publication fund.

Smith, H. I. (1923). An album of prehistoric Canadian art. *Victoria Memorial Museum Bulletin No. 37, Anthropological Series No. 8*. Ottawa: Canadian Department of Mines.

Sturtevant, E. H. (1931). Hittite glossary: Words of known or conjectured meaning, with Sumerian ideograms and Accadian words common in Hittite texts. *Language, 7(2)*, 3–82.

Swadesh, M. (1934). The phonemic principle. *Language, 10(2)*, 117–129.

———. (1935). Twaddell on defining the phoneme. *Language, 11(2)*, 244–250.

———. (1936). Phonemic contrasts. *American Speech, 11(4)*, 298–301.

———. (1937). A method for phonetic accuracy and speed. *American Anthropologist, 39(4)*, 728–732.

———. (1937). The phonemic interpretation of long consonants. *Language, 13(1)*, 1–10.

———. (1939). Edward Sapir. *Language, 15(2)*, 132–135.

———. (1941). Observations of pattern impact on the phonetics of bilinguals. In L. Spier, I. Hallowell, & S. Newman (Eds.), *Language, culture, and personality*, 59–65.

———. (1948). Sociological notes on obsolescent languages. *International Journal of American Linguistics, 14(4)*, 226–235.

Twaddell, W. F. (1935). *On defining the phoneme*. Baltimore: Waverly Press.

Voegelin, C. F. (1935). Shawnee phonemes. *Language, 11(1)*, 23–37.

———. (1941). North American Indian languages still spoken and their greater relationships. In L. Spier, I. Hallowell, & S. Newman (Eds.), *Language, culture, and personality*, 15–40.

Voegelin, C. F., & Harris, Z. (1945). Index to the Franz Boas Collection of materials for American linguistics. *Language, 21(3)*, 5–7+9–43.

———. (1947). The scope of linguistics. *American Anthropologist, 49(4)*, 588–600.

Whorf, B. L. (1927). On the connection of ideas. In J. B. Carroll (Ed.), *Language, thought and reality*, 35–39.

———. (*n.d.*). On psychology. In J. B. Carroll (Ed.), *Language, thought and reality*, 40–42.

———. (1931). A central Mexican inscription combining Mexican and Maya day signs. In J. B. Carroll (Ed.), *Language, thought and reality*, 43–50.

———. (1935). The comparative linguistics of Uto-Aztecan. *American Anthropologist,* *37(4),* 600–608.

———. (1936a). The punctual and segmentative aspects of verbs in Hopi. In J. B. Carroll (Ed.), *Language, thought and reality*, 51–56.

———. (1936b). An American Indian model of the universe. In J. B. Carroll (Ed.), *Language, thought and reality*, 57–64.

———. (1936c). A linguistic consideration of thinking in primitive communities. In J. B. Carroll (Ed.), *Language, thought and reality*, 65–86.

———. (1937). Grammatical categories. In J. B. Carroll (Ed.), *Language, thought and reality*, 87–101.

———. (1938). Some verbal categories of Hopi. *Language, 14(4),* 275–286.

———. (1939). The relation of habitual thought and behavior in language. In J. B. Carroll (Ed.), *Language, thought and reality*, 134–159.

———. (1940a). Science and linguistics. In J. B. Carroll (Ed.), *Language, thought and reality*, 207–219.

———. (1940b). Linguistics as an exact science. In J. B. Carroll (Ed.), *Language, thought and reality*, 220–232.

———. (1941a). Language and logic. In J. B. Carroll (Ed.), *Language, thought and reality*, 233–245.

———. (1941b). Language, mind, and reality. In J. B. Carroll (Ed.), *Language, thought and reality*, 246–270.

———. (1941c). A brotherhood of thought. *Main Currents in Modern Thought, 1*, 13–14.

———. (1952). *Collected papers on metalinguistics*. Washington, DC: Department of State, Foreign Service Institute.

———. (1956). *Language, thought, and reality: Selected writings of Benjamin Lee Whorf*. J. B. Carroll (Ed.). Cambridge: MIT Press.

Wohlstetter, A. & White, M. G. (1939). Who are the friends of semantics? *Partisan Review, 6(5)*, 50-57.

Secondary:

Ahmed, S. (2000). *Strange encounters: Embodied others in post-coloniality*. New York: Routledge.

———. (2006). *Queer phenomenology: Orientations, objects, others*. Durham: Duke University Press.

Alter, S. G. (1999). *Darwinism and the linguistic image: Language, race, and natural theology in the nineteenth century*. London: Johns Hopkins University Press.

———. (2005). *William Dwight Whitney and the science of language*. London: Johns Hopkins University Press.

Anderson, S. R. (1985). *Phonology in the twentieth century: Theories of rules and theories of representations*. Chicago: University of Chicago Press.

Anderson, W. (2006). *Colonial pathologies: American tropical medicine, race, and hygiene in the Philippines*. Durham: Duke University Press.

———. (2008). *The collectors of lost souls: Turning Kuru scientists into whitemen*. Baltimore: Johns Hopkins University Press.

Andresen, J. T. (1990). *Linguistics in America, 1769–1924: A critical history*. New York: Routledge.

———. (2010). Historiography's contribution to theoretical linguistics. In D. Kibbee (Ed.), *Chomskyan evolutions and revolutions: Essays in honor of E. F. K. Koerner*, 443–469. Philadelphia: J. Benjamins.

Baker, L. D. (1998). *From savage to Negro: Anthropology and the construction of race, 1896–1954*. Berkeley: University of California Press.

———. (2010). *Anthropology and the racial politics of culture*. Durham: Duke University Press.

Bakhtin, M. M. (1986). The problem of speech genres. In V. W. McGee (Trans.), C. Emerson & M. Holquist (Eds.), *Speech Genres & Other Late Essays*, 60–102. Austin: University of Texas Press.

Barsky, R. F. (2011). *Zellig Harris: From American linguistics to socialist Zionism*. Cambridge: MIT Press.

Barthes, R. (1972). *Mythologies*. New York: Hill and Wang. (Original work published 1957)

Bazerman, C. (1988). *Shaping written knowledge: The genre and activity of the experimental article in science*. Madison: University of Wisconsin Press.

Berlant, L. (2011). *Cruel optimism*. Durham: Duke University Press.

Biagioli, M. (1993). *Galileo, courtier: The practice of science in the culture of absolutism*. Chicago: University of Chicago Press.

Bijker, W. E. (1995). *Of bicycles, bakelites, and bulbs: Toward a theory of sociotechnical change*. Cambridge: MIT Press.

Bloor, D. (1991). The strong programme in the sociology of knowledge. In D. Bloor, *Knowledge and Social Imagery* (2nd ed.), 3– 23. Chicago: University of Chicago Press. (Original work published 1976)

Boroditsky, L. (2001). Does language shape thought? English and Mandarin speakers' conceptions of time. *Cognitive Psychology, 43(1)*, 1–22.

Bourdieu, P. (1977). The economics of linguistic exchanges. *Social Science Information, 16(6)*, 645–668.

Bowker, G. C. (2005). *Memory practices in the sciences*. Cambridge: MIT Press.

Brady, E. (1999). *A spiral way: How the phonograph changed ethnography*. Jackson: University Press of Mississippi.

Brain, R. (1998). Standards and semiotics. In T. Lenoir (Ed.), *Inscribing science: Scientific texts and the materiality of communication*, 249–284. Stanford: Stanford University Press.

Brantlinger, P. (2003). *Dark vanishings: Discourse on the extinction of primitive races, 1800–1930*. Ithaca: Cornell University Press.

Bryson, D. (2009). Personality and culture, the Social Science Research Council, and liberal social engineering: The Advisory Committee on Personality and Culture, 1930–1934. *Journal of the History of Behavioral Sciences, 45(4)*, 355–386.

Byrd, J. A. (2011). *The transit of empire: Indigenous critiques of colonialism*. Minneapolis: University of Minnesota Press.

Callon, M., & Latour, B. (1992). Don't throw the baby out with the Bath School! A reply to Collins and Yearly. In A. Pickering (Ed.), *Science as Practice and Culture*, 343–368. Chicago: University of Chicago Press.

Cameron, D. (1995). *Verbal hygiene*. New York: Routledge.

———. (2007). Language endangerment and verbal hygiene: History, morality, and politics. In A. Duchêne & M. Heller (Eds.), *Discourses of Endangerment: Ideologies and Interests in the Defense of Languages*, 268–285. London: Continuum.

Canguilhem, G. (1989). *The normal and the pathological*. New York: Zone Books.

Carroll, J. B. (1956). Introduction. *Language, thought and reality*, 1–34.

Casasanto, D. (2008). Who's afraid of the Big Bad Whorf? Cross-linguistic differences in temporal language and thought. *Language Learning, 58(1)*, 63–79.

Chicocki, P., & Kilarski, M. (2010). On “Eskimo words for snow”: The life cycle of a linguistic misconception. *Historiographia Linguistica, 37(3)*, 341–377.

Clark, W. (2006). *Academic charisma and the origins of the research university*. Chicago: University of Chicago.

Clifford, J. (1989). The others: Beyond the “salvage” paradigm. *Third Text, 3(6)*, 73–78.

Cohen-Cole, J. N. (2014). *The open mind: Cold War politics and the sciences of human nature*. Chicago: University of Chicago Press.

Colebrook, C. (2014). Not Kant, not now: Another sublime. In R. Askin, P. J. Ennis, A. Hägler, P. Schweighauser (Eds.), *Speculations V: Aesthetics in the 21st Century*, 127–157. Brooklyn: Punctum Books.

Conklin, A. L. (2013). *In the museum of man: Race, anthropology, and empire in France, 1850–1950*. Cornell University Press.

Cook, H. (2007). *Matters of exchange: Commerce, medicine and science in the Dutch Golden Age*. New Haven: Yale University Press.

Cooter, R., & Pumfrey, S. (1994). Separate spheres and public places: Reflections of the history of science popularization and science in popular culture. *History of Science, 32*, 237–267.

Cruikshank, J. (2005). *Do glaciers listen?: Local knowledge, colonial encounters and social imagination*. Vancouver: UBC Press.

Darnell, R. (1990). *Edward Sapir: Linguist, anthropologist, humanist*. Berkeley: University of California Press.

———. (1998a). *And along came Boas: Continuity and revolution in Americanist anthropology*. Philadelphia: J. Benjamins.

———. (1998b). Camelot at Yale: The construction and dismantling of the Sapirian synthesis, 1931–39. *American Anthropologist, 100*, 361–372.

———. (1998c). Mary Haas and the First Yale School of Linguistics. *Anthropological Linguistics, 39*, 566–575.

———. (2001). *Invisible genealogies: A history of Americanist anthropology*. Lincoln: University of Nebraska Press.

———. (2006). Departmental networks in Canadian anthropology. In R. Darnell & J. D. Harrison (Eds.), *Historicizing Canadian Anthropology*, 137–146.

———. (2009). *Edward Sapir: Linguist, anthropologist, humanist*. Berkeley: University of California Press. (Original work published in 1989)

Darnell, R., & Harrison, J. D. (2006). *Historicizing Canadian anthropology*. Vancouver: UBC Press.

Daston, L. (2004). Speechless. In L. Daston (Ed.), *Things That Talk: Object Lessons from the History of Art and Science*, 9–26. New York: Zone Books.

———. (2017). Introduction. *Science in the archives: Pasts, presents, futures*. Chicago: University of Chicago Press.

Daston, L., & Sibum, H. O. (2003) Introduction: Scientific personae and their histories. *Science in Context, 16(1/2),* 1–8.

Daston, L., & Galison, P. (2007). *Objectivity*. New York: Zone Books.

Dawn, L. A. (2006). *National visions, national blindness: Canadian art and identities in the 1920s*. Vancouver: UBC Press.

De Man, P. (1983). *Blindness and insight: Essays in the rhetoric of contemporary criticism* (2nd ed.). Minneapolis: University of Minnesota Press.

Deleuze, G., & F. Guattari. (1987). *A thousand plateaus: Capitalism and schizophrenia*. B. Massumi (Trans.). Minneapolis: University of Minnesota Press.

Derrida, J. (1976). *Of grammatology*. G. C. Spivak (Trans.). Baltimore: Johns Hopkins University Press.

———. (1978a). Ellipsis. In *Writing and difference*. A. Bass (Trans.). University of Chicago Press.

———. (1978b). Structure, sign, and play in the discourses of the human sciences. In *Writing and difference*. A. Bass (Trans.). University of Chicago Press.

———. (1995). *Archive fever: A Freudian impression*. E. Prenowitz (Trans.). Chicago: University of Chicago Press.

Deutscher, G. (2010). *Through the language glass: How words colour your world*. London: William Heinemann.

Devitt, M., & Sterelny, K. (1987). *Language and reality: An introduction to the philosophy of language*. Oxford: Blackwell.

Dumit, J. (2003). Is it me or my brain?: Depression and neuroscientific facts. *Journal of Medical Humanities, 24(1–2)*, 35–47.

———. (2004). *Picturing personhood: Brain scans and biomedical identity*. Princeton: Princeton University Press.

Dyck, N. (1991). *What is the Indian "problem": Tutelage and resistance in Canadian Indian* *administration*. St. John's, Nfld.: Institute of Social and Economic Research, Memorial University of Newfoundland.

Edelman, L. (2004). *No future: Queer theory and the death drive*. Durham: Duke University Press.

Edwards, P. N. (1996). *The closed world: Computers and the politics of discourse in Cold War America*. Cambridge: MIT Press.

———. (2010). *A vast machine: Computer models, climate data, and the politics of global warming*. Cambridge: MIT Press.

Eggan, F. (1986). An overview of Sapir's career. In W. Cowan, M. K. Foster, & E. F. K. Koerner (Eds.), *New perspectives in language, culture, and personality: Proceedings of the Edward Sapir Centenary Conference (Ottawa, 1-3 October 1984)*, 1–16. Philadelphia: J. Benjamins.

Eisenstein, E. L. (1979). *The printing press as an agent of change: Communications and cultural transformations in early modern Europe*. Cambridge: Cambridge University Press.

Epstein, S. (1996). *Impure science: AIDS, activism, and the politics of knowledge*. Berkeley: University of California Press.

Errington, J. (2008). *Linguistics in a colonial world: A story of language, meaning, and power*. Oxford: Blackwell Pub.

Fabian, A. (2010). *The skull collectors: Race, science, and America's unburied dead*. Chicago: University of Chicago Press.

Fahy, D. (2015). *The new celebrity scientists: Out of the lab and into the limelight*. New York: Rowman & Littlefield.

Falk, J. S. (1999). *Women, language, and linguistics: Three American stories from the first half of the twentieth century*. New York: Routledge.

———. (2003). Turn to the history of linguistics: Noam Chomsky and Charles Hockett in the 1960s. *Historiographia Linguistica, 30(1/2)*, 129–185.

Fausto-Sterling, A. (2000). *Sexing the body: Gender politics and the construction of sexuality*. New York: Basic Books.

Felman, S. (2003). *The scandal of the speaking body: Don Juan with J.L. Austin, or seduction in two languages*. Stanford: Stanford University Press.

Feyerabend, P. K. (1993). *Against method* (3rd ed.). New York: Verso. (Original work published 1976)

Figal, S. E. (2008). *Heredity, race, and the birth of the modern*. New York: Routledge.

Fishman, J. A. (1991). *Reversing language shift: Theoretical and empirical foundations of assistance to threatened languages*. Philadelphia: Multilingual Matters.

Fountain, C. (2013). Father Felipe Arroyo de la Cuesta's Work on California's Native languages. *Historiographia Linguistica, 40(1/2)*, 97–119.

Freeman, E. (2010). *Time binds: Queer temporalities, queer histories*. Durham: Duke University Press.

Fyfe, A. (2012). *Steam-powered knowledge: William Chambers and the business of publishing, 1820–1860*. Chicago: University of Chicago Press.

Galison, P. (2000). Einstein's clocks: The place of time. *Critical Inquiry, 26(2)*, 355–389.

Gentner, D. & Goldin-Meadow, S., eds. (2003). *Language in mind: Advances in the study of language and thought.* Cambridge: MIT Press.

Gieryn, T. F. (1983). Boundary-work and the demarcation of science from non-science:

Strains and interests in professional ideologies of scientists. *American Sociological Review, 48(6)*, 781–795.

———. (1999). *Cultural boundaries of science: Credibility on the line*. University of Chicago Press.

———. (2002). Three truth-spots. *Journal of the History of the Behavioral Sciences, 38(2)*, 113–132.

———. (2006). City as truth-spot: Laboratories and field-sites in urban studies. *Social Studies of Science, 36(1)*, 5–38.

Gilbert, A. L., Regier, T., Kay, P. & Ivry, R. B. (2008). Support for lateralization of the Whorf effect beyond the realm of color discrimination. *Brain and Language, 105(2)*, 91–98.

Glenn, C. (2004). *Unspoken: A rhetoric of silence*. Carbondale, IL: Southern Illinois University Press.

Goffman, E. (1963). *Stigma: Notes on the management of spoiled identity*. Englewood Cliffs: Prentice-Hall.

Golinski, J. (1992). *Science as public culture: Chemistry and enlightenment in Britain, 1760-1820*. New York: Cambridge University Press.

———. (2011). Humphry Davy: The experimental self. *Eighteenth-Century Studies, 45(1*), 15–28.

Gordin, M. D. (2012). *The pseudoscience wars: Immanuel Velikovsky and the birth of the modern fringe*. Chicago: The University of Chicago Press.

———. (2015). *Scientific Babel: How science was done before and after global English*. The University of Chicago Press.

Grenoble, L. A., & Whaley, L. J. (2006). *Saving languages: An introduction to language* *revitalization*. New York: Cambridge University Press.

Grosz, E. A. (1994). *Volatile bodies: Toward a corporeal feminism*. Bloomington: Indiana University Press.

Habermas, J. (1991). *The structural transformation of the public sphere: An inquiry into a category of bourgeois society*. T. Burger & F. Lawrence (Trans.). Cambridge: MIT Press.

Hackert, S. (2009). Linguistic nationalism and the emergence of the English native speaker. *European Journal of English Studies, 13(3)*, 305–317.

Halberstam, J. (2011). *The queer art of failure*. Durham: Duke University Press.

Hancock, R. L. A. (2006). Toward a historiography of Canadian anthropology. In R. Darnell & J. D. Harrison (Eds.), *Historicizing Canadian Anthropology*, 30–40.

Haraway, D. (1988). Situated knowledges: The science question in feminism and the privilege of partial perspective. *Feminist Studies, 14(3)*, 575–599.

———. (1989). *Primate visions: Gender, race, and nature in the world of modern science*. New York: Routledge.

———. (1997). *Modest_Witness@Second_Millennium.FemaleMan©_Meets_ OncoMouseTM*. New York: Routledge.

———. (2008). *When species meet*. Minneapolis: University of Minnesota Press.

———. (2016). *Staying with the trouble: Making kin in the Chthulucene*. Duke University Press.

Harding, S. (2008). *Sciences from below: Feminisms, postcolonialities, and modernities*. Durham: Duke University Press.

Harpham, G. G. (2002). *Language alone: The critical fetish of modernity*. New York:

Routledge.

Harrison, J. D., & Darnell, R. Historicizing traditions in Canadian anthropology. In R. Darnell & J. D. Harrison (Eds.), *Historicizing Canadian Anthropology*, 3–16.

Hayles, N. K. (1999). *How we became posthuman: Virtual bodies in cybernetics, literature, and informatics*. Chicago: University of Chicago Press.

Heller, M., & Duchêne, A. (2007). Discourses of endangerment: sociolinguistics, globalization and the social order. In: A. Duchêne & M. Heller (Eds.), *Discourses of Endangerment: Ideologies and Interests in the Defense of Languages,* 1–13. London: Continuum.

Herzig, R. M. (2005). *Suffering for science: Reason and sacrifice in modern America*. New Brunswick, N.J.: Rutgers University Press.

Hill, A. A. (1964). A history of the Linguistic Institute. *ACLS Newsletter, 15(3)*, 1–12.

Hollinger, D. A. (2003). Cultural relativism. In T. M. Porter & D. Ross (Eds.), *The Cambridge History of Science 7: The Modern Social Sciences*, 708–720. Cambridge: Cambridge University Press.

Hutton, C. (1999). *Linguistics and the Third Reich: Mother-tongue fascism, race, and the science of language*. New York: Routledge.

Hymes, D. H., & Fought, J. (1981). *American structuralism*. The Hague: Mouton Publishers.

Igo, S. E. (2007). *The averaged American: Surveys, citizens, and the making of a mass public*. Cambridge: Harvard University Press.

Isaac, J. (2012). *Working knowledge: Making the human sciences from Parsons to Kuhn*. Cambridge: Harvard University Press.

Jacoby, R. (1987). *The last intellectuals: American culture in the age of academe*. New York:

Basic Books.

Jones-Imhotep, E. (2017). *The unreliable nation: Hostile nature and technological failure in the Cold War*. Cambridge: MIT Press.

Joos, M. (1986). *Notes on the development of the Linguistic Society of America, 1924 to 1950*. Ithaca: Linguistic Society of America.

Kaplan, A. (2002). *The anarchy of empire in the making of U.S. culture*. Cambridge: Harvard University Press.

Kaplan, J. (2013). "Voices of the people": Linguistic research among Germany's prisoners of war during World War I. *Journal of the History of the Behavioral Sciences, 49(3)*, 281–305.

———. (2017). From lexicostatistics to lexomics: Basic vocabulary and the study of language prehistory. *Osiris, 32(1)*, 202–223.

Kay, L. E. (2000). *Who wrote the book of life? A history of the genetic code*. Stanford: Stanford University Press.

Keller, E. F. (2002). *Making sense of life: Explaining biological development with models, metaphors and machines*. Cambridge: Harvard University Press.

Kittler, F. A. (1990). *Discourse networks, 1800/1900*. M. Metteer & C. Cullens (Trans.). Stanford: Stanford University Press.

———. (1999) *Gramophone, film, typewriter*. G. Winthrop-Young & M. Wutz (Trans.). Stanford University Press.

Knorr-Cetina, K. (1999). *Epistemic cultures: How the sciences make knowledge*. Cambridge: Harvard University.

Koerner, E. F. K. (1978). *Toward a historiography of linguistics: Selected essays*.

Philadelphia: J. Benjamins.

———. (1984). *Edward Sapir: Appraisals of his life and work*. Philadelphia: J. Benjamins.

———. (1992). The Sapir-Whorf hypothesis: A preliminary history and a bibliographical Essay. *Linguistic Anthropology, 2(2)*, 173–198.

———. (1999). *Linguistic historiography: Projects & prospects*. Philadelphia: J. Benjamins.

Koerner, E. F. K., ed. (2007). *Edward Sapir: Critical assessments of leading linguists*. New York: Routledge.

Kristeva, J. (1989). *Language—the unknown: An initiation into linguistics*. New York: Columbia University Press.

Kuhn, T. S. (1962). *The structure of scientific revolutions.* Chicago: University of Chicago Press.

Lakoff, G. (1987). *Women, fire, and dangerous things: What categories reveal about the mind*. University of Chicago Press.

Latour, B. (1986). Visualization and cognition: Drawing things together. In H. Kuklick & E. Long (Eds.), *Knowledge and Society: Studies in the Sociology of Culture Past and Present.* Greenwich, 1–40. CT: JAI Press.

———. (1987). *Science in action: How to follow scientists and engineers through society*. Cambridge, Mass..: Harvard University Press.

———. (1993). *We have never been modern*. C. Porter (Trans.). Cambridge: Harvard University Press.

Leavitt, J. (2010). *Linguistic relativities: Language diversity and modern thought*. New York: Cambridge University Press.

Lee, P. (1994). New work on the linguistic relativity question. *Historiographia Linguistica,* *21(1/2),* 173–191.

———. (1996). *The Whorf theory complex: a critical reconstruction*. Philadelphia: J. Benjamins.

Leeds-Hurwitz, W. (1985). The Committee on Research in Native American Languages. *Proceedings of the American Philosophical Society, 129(2)*, 129–160.

———. (2004). *Rolling in ditches with shamans: Jaime de Angulo and the professionalization of American anthropology*. Lincoln: University of Nebraska Press.

Lemov, R. M. (2005). *World as laboratory: Experiments with mice, mazes, and men*. New York: Hill and Wang.

———. (2015). Anthropological data in danger, c. 1941–1965. In F. Vidal & N. Dias (Eds.), *Endangerment, biodiversity and culture*, 87–111.

Lenoir, T. (1997). *Instituting science: The cultural production of scientific disciplines*. Stanford: Stanford University Press.

Lightman, B. V. (2007). *Victorian popularizers of science: Designing nature for new audiences*. Chicago: University of Chicago Press.

Lucy, J. A. (1997). Linguistic relativity. *Annual Review of Anthropology, 26*, 291–312.

Lynch, M. (2009). Science as a vacation. Deficits, surfeits, PUSS, and doing your own job. *Organization, 16(1)*, 101–119.

M'Closkey, K., & Manuel, K. (2006). Commodifying North American Aboriginal culture: A Canada/U.S. comparison. In R. Darnell & J. D. Harrison (Eds.), *Historicizing Canadian Anthropology*, 226–241.

Mandler, P. (2013). *Return from the Natives: How Margaret Mead won the Second World*

War and Lost the Cold War. Yale University Press.

Martin, E. (1991). The egg and the sperm: How science has constructed a romance based o n stereotypical male-female roles. *Signs, 16(3)*, 485–501.

Martin-Nielsen, J. (2010). 'This war for men's minds': The birth of a human science in Cold War America. *History of the Human Sciences 23(5)*, 131–155.

———. (2011). A forgotten social science? Creating a place for linguistics in the historical dialogue. *Journal of the History of the Behavioral Sciences, 47(2)*, 147–172.

McWhorter, J. H. (2008). *Our magnificent bastard tongue: The untold history of English*. New York: Gotham Books.

———. (2014). *The language hoax: Why the world looks the same in any language*. Oxford University Press.

Merton, R. K. (1942). The ethos of science. In P. Sztompka & R. K. Merton (Eds.), *On social structure and science* (267–276). Chicago: University of Chicago Press.

Mialet, H. (2012). *Hawking incorporated: Stephen Hawking and the anthropology of the knowing subject*. Chicago: University of Chicago Press.

Missner, M. (1985). Why Einstein became famous in America. *Social Studies of Science, 15(2)*, 267–291.

Mitman, G. (1999). *Reel nature: America's romance with wildlife on film*. Cambridge and London: Harvard University Press.

Mol, A. (2002). *The body multiple: Ontology in medical practice*. Durham: Duke University Press.

Muehlmann, S. (2015). "Languages die like rivers": Entangled endangerments in the

Colorado Delta. In F. Vidal & N. Dias (Eds.), *Endangerment, biodiversity and culture*, 41–61.

Muñoz, J. E. (2009). *Cruising utopia: The then and there of queer futurity*. New York: New York University Press.

Murphy, M. (2006). *Sick Building Syndrome and the problem of uncertainty: Environmental politics, technoscience, and women workers*. Durham: Duke University.

Murray, S. O. (1994). *Theory groups and the study of language in North America: A social history*. Philadelphia: J. Benjamins.

———. (1998). *American sociolinguistics: Theorists and theory groups*. Philadelphia: J. Benjamins.

Nettle, D., & Romaine, S. (2000). *Vanishing voices: The extinction of the world's languages*. New York: Oxford University Press.

Newmeyer, F. J. (1996). *Generative linguistics: A historic perspective*. New York: Routledge.

Nurse, A. (2006). Marius Barbeau and the methodology of salvage ethnography in Canada, 1911–1951. In R. Darnell & J. D. Harrison (Eds.), *Historicizing Canadian Anthropology*, 52–64.

———. (2007). The ambiguities of disciplinary professionalization: The state and cultural dynamics of Canadian inter-war anthropology. *Scientia Canadensis, 30(2)*, 37–53.

Orr, J. (2006). *Panic diaries: A genealogy of panic disorder*. Durham: Duke University Press.

Parezo, N. J., & Fowler, D. D. (2007). *Anthropology goes to the fair: The 1904 Louisiana Purchase Exposition*. Lincoln: University of Nebraska Press.

Peiss, K. L. (1986). *Cheap amusements: Working women and leisure in turn-of-the-century*

New York. Philadelphia: Temple University Press.

Perley, B C. (2011). *Defying Maliseet language death: Emergent vitalities of language, culture, and identity in Eastern Canada*. Lincoln: University of Nebraska Press.

Pettit, M. (2013). *The science of deception: Psychology and commerce in America*. Chicago: University of Chicago Press.

Pinker, S. (1994). *The language instinct*. New York: Morrow.

———. (2007). *The stuff of thought: Language as window into human nature*. New York: Allen Lane.

Porter, T. (1995). *Trust in numbers: The pursuit of objectivity in science and public life*. Princeton: Princeton University Press.

Pratt, M. L. (1991). Arts of the contact zone. *Profession*, 33–40.

Proctor, R. N., & Schiebinger, L., eds. (2008). *Agnotology: The making and unmaking of ignorance*. Stanford: Stanford University Press.

Pullum, G. K. (1991). *The great Eskimo vocabulary hoax, and other irreverent essays on the study of language*. Chicago: University of Chicago Press.

Qureshi, S. (2011). *Peoples on parade: Exhibitions, empire, and anthropology in nineteenth century Britain*. Chicago: University of Chicago Press.

Radcliffe, M. (2008). Absentminded professor or romantic artist?: The depiction of creativity in documentary biographies of Albert Einstein. *Journal of Popular Film and Television, 36(2)*, 63–71.

Radin, J., Kowal, E., & Reardon, J. (2013). Indigenous body parts, mutating temporalities, and the half-lives of postcolonial technoscience. *Social Studies of Science, 43(4)*, 465–483.

Raj, K. (2007). *Relocating modern science: Circulation and the construction of knowledge in South Asia and Europe, 1650–1900*. New York: Palgrave Macmillan.

Reardon, J. (2005). *Race to the finish: Identity and governance in an age of genomics*. Princeton: Princeton University Press.

Reisch, G. (2007). Logical empiricism, the Unity of Science Movement, and the Cold War. In T. Uebel & A. Richardson (Eds.), *The Cambridge Companion to Logical Empiricism*, 58–87.

Rheinberger, H. (1997). *Toward a history of epistemic things: Synthesizing proteins in the test tube*. Stanford: Stanford University.

———. (2010). *An epistemology of the concrete: Twentieth-century histories of life*. Durham: Duke University Press.

Rickert, T. J. (2013). *Ambient rhetoric: The attunements of rhetorical being*. Pittsburgh: University of Pittsburgh Press.

Rifkin, M. (2017). *Beyond settler time: Temporal sovereignty and indigenous self-determination*. Duke University Press.

Sarton, G. (1924). The new humanism. *Isis, 6*, 9–42.

Schöntag, R., & Schäfer-Prieß, B. (2007). Colour term research of Hugo Magnus. In R. E. MacLaury, G. V. Paramei, & D. Dedrick (Eds.), *Anthropology of color: Interdisciplinary multilevel modeling* (107–122). Philadelphia: J. Benjamins.

Schultz, E. A. (1990). *Dialogue at the margins: Whorf, Bakhtin, and linguistic relativity*. Madison: University of Wisconsin Press.

Schor, N. (1992). “Cartes postales”: Representing Paris 1900. *Critical Inquiry, 18*, 188–241.

Schrecker, E. (1986). *No ivory tower: McCarthyism and the universities*. New York:

Oxford University Press.

Searle, J. R. (1995). *The construction of social reality*. New York: Free Press.

Secord, J. A. (2000). *Victorian sensation: The extraordinary publication, reception, and secret authorship of Vestiges of the natural history of creation*. Chicago: University of Chicago Press.

Sera-Shriar, E. (2014). What is armchair anthropology? Observational practices in 19th-century British human sciences. *History of the Human Sciences, 27(2)*, 26–40.

Serlin, D. (2004). *Replaceable you: Engineering the body in postwar America*. Chicago: University of Chicago Press.

Shah, N. (2006). Adjusting intimacies on U.S. frontiers. In A. Stoler (Ed.), *Haunted by Empire*, 116–139.

Shapin, S. (2008). *The scientific life: A moral history of a late modern vocation*. Chicago: University of Chicago Press.

———. (2010). *Never pure: Historical studies of science as if it was produced by people with bodies, situated in time, space, culture, and society, and struggling for credibility and authority*. Baltimore: Johns Hopkins University Press.

Shapin, S., & Schaffer, S.. (1989). *Leviathan and the air pump: Hobbes, Boyle and the experimental life*. Princeton: Princeton University.

Simpson, A. (2014). *Mohawk interruptus: Political life across the borders of settler states*. Durham: Duke University Press.

Slobin, D. I. (1996). From "thought and language" to "thinking for speaking." In J. J. Gumperz & S. C. Levinson (Eds.), *Rethinking linguistic relativity* (70–96). Cambridge: Cambridge University Press.

Spencer, S. B., ed. (2012). *The ballad collectors of North America: How gathering folksongs transformed academic thought and American identity*. Lanham, Md.: Scarecrow Press.

Stadler, F. (2007). The Vienna Circle: Context, profile, and development. In T. Uebel & A. Richardson (Eds.), *The Cambridge Companion to Logical Empiricism*, 13–40.

Star, S. L., & Griesemer, J. R.. (1989). Institutional ecology, "translations," and boundary objects: Amateurs and professionals in Berkeley's Museum of Vertebrate Zoology, 1907–39. *Social Studies of Science, 19(3)*, 387–420.

Stewart, L. R. (1992). *The rise of public science: Rhetoric, technology, and natural philosophy in Newtonian Britain, 1660-1750*. Cambridge: Cambridge University Press.

Stocking, G. W. (1968). *Race, culture, and evolution: Essays in the history of anthropology*. Chicago: University of Chicago Press.

Stoler, A. L. (2006a). Intimidations of empire: Predicaments of the tactile and unseen. In A. L. Stoler (Ed.), *Haunted by Empire*, 1–22.

———. (2009). *Along the archival grain: Epistemic anxieties and colonial common sense*. Princeton: Princeton University Press.

———. (2013). Introduction. *Imperial debris: On ruins and ruination*. Durham: Duke University Press.

Stoler, A. L., ed. (2006b). *Haunted by empire: Geographies of intimacy in North American history*. Durham: Duke University Press.

TallBear, K. (2013). *Native American DNA: Tribal belonging and the false promise of genetic science*. Minneapolis: University of Minnesota Press.

Terrall, M. (1995). Emilie du Chatelet and the gendering of science. *History of Science, 33*, 283–310.

Teslow, T. (2014). *Constructing race: The science of bodies and cultures in American anthropology*. Cambridge University Press.

Tomalin, M. (2006). *Linguistics and the formal sciences: The origins of generative grammar*. Cambridge: Cambridge University Press.

Uebel, T., & Richardson, A. (2007). *The Cambridge companion to logical empiricism*. New York: Cambridge University Press.

Vidal, F. F. (2009). Brainhood, anthropological figure of modernity. *History of the Human Sciences, 22(1)*, 5–36.

Vidal, F., & Dias, N. (2015). Introduction. *Endangerment, biodiversity and culture*. New York: Routledge.

Vodden, C., & Dyck, I. (2006). *A world inside: A 150-year history of the Canadian Museum of Civilization*. Gatineau, Québec: Canadian Museum of Civilization.

Waiser, W. A. (1989). *The field naturalist: John Macoun, the Geological Survey, and natural science*. Toronto: University of Toronto Press.

Waldrum, J., & Downe, P. (2006). Expatriates in the Ivory Tower: Anthropologists in non-anthropology university departments. In R. Darnell & J. D. Harrison (Eds.), *Historicizing Canadian Anthropology*, 183–195.

Weber, M. (1946). Science as a vocation. In H. H. Gerth & C. Wright Mills (Eds.), *Max Weber: Essays in Sociology,* 129–156. New York: Oxford University Press. (Original work published 1919)

White, P. (2003). *Thomas Huxley: Making the "man of science"*. Cambridge: Cambridge

University Press.

Williams, R. (1977). Structures of feeling. In *Marxism and literature*, 128–135. Oxford: Oxford University Press.

Willmott, C. (2006). The historical praxis of museum anthropology. In R. Darnell & J. D. Harrison (Eds.), *Historicizing Canadian Anthropology*, 212–225.

Wittgenstein, L. (2009). *Philosophical investigations* (Rev. 4th ed.). G. E. M. Anscombe, P. M. S. Hacker, & J. Schulte (Trans.). Malden, MA: Wiley-Blackwell. (Originally published in 1953)

Zaslow, M. (1975). *Reading the rocks: The story of the Geological Survey of Canada, 1842–1972*. Toronto: Published by the Macmillan Co. of Canada in association with the Dept. of Energy, Mines and Resources, and Information Canada.

Zimmerman, A. (2001). *Anthropology and Antihumanism in Imperial Germany*. Chicago: University of Chicago Press.