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Rhetorical Scarcity: Spatial and Economic Inflections on Genre Change

This study examines how changes in a key scientific genre supported anthropology’s early twentieth-century bid for scientific status. Combining spatial theories of genre with inflections from the register of economics, I develop the concept of rhetorical scarcity to characterize this genre change not as evolution but as manipulation that produces a manufactured situation of intense rhetorical constraint.

Spatial metaphors have proliferated within genre studies in recent decades. Metaphoric constructions of genres as sites, spaces, constellations, ecosystems, and locations have proven enormously useful to scholars seeking to understand the complexity of rhetorical activity and the role of genres in organizing and enabling such activity. Such spatial metaphors have gained currency in part through the influence of the broader “spatial turn” in English studies, but additionally through the power of such metaphors to describe genre in ways that foreground the qualities of rhetorical activity that genre scholars have come to emphasize. For instance, Catherine Schryer’s influential formulation of genres as “stabilized-for-now or stabilized-enough sites of social and ideological action” (“Records” 204) emphasizes flexibility by portraying genre as ground that shifts, gradually, beneath one’s feet: neither bedrock nor quicksand, but

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something in between. Anis Bawarshi’s formulation of genres as “rhetorical environments within which we recognize, enact, and consequently reproduce various situations, practices, relations, and identities” (“Genre Function” 336) highlights the constitutive quality of genres as structures that organize possibilities for writers. Spatial metaphors for genre are also both analytically supple and sufficiently commonsensical to challenge, if not thoroughly supplant, the genre-as-container and genre-as-tool metaphors that shaped discourse about genre for decades. In contrast to the genre-as-container metaphor, for instance, spatial metaphors often emphasize the role of genres in enabling creativity and innovation; viewing genres as locations for rhetorical action recognizes a broader range of possible activities than do metaphors that treat genres as static containers that mold individual purposes into textual similarity.

Despite these benefits, extending spatial metaphors—such as genre as ecosystem—to the study of genre change poses challenges for researchers. In particular, examining genre change as ecological change, relying on language drawn from evolutionary theory, can have the inadvertent effect of naturalizing the social and communicative processes that genre scholars investigate. For instance, although some scholars have borrowed evolutionary language to describe genre change through terms such as selection pressure, niche, adaptation, fittedness, and so on, these terms carry with them connotations of cause and effect that may fail to describe the social processes that underlie genre change. As Dylan Dryer notes, “not all genre systems experience the pressure of transformation as continuously, as mercilessly, or as indiscriminately, as species experience the pressure of natural selection” (518). Similarly, rhetorical resources are not subject to inherent limitations in the same way that material resources in an ecosystem often are. Consequently, representing genre change in a vocabulary borrowed from evolutionary theory can deflect attention away from the concerns that many scholars aim to examine through genre—concerns with the constitutive or epistemic functions of genres within the communities that use them, for instance, or concerns with the extent to which individual writers can revise the collective uses and meanings that inhere in the genres they take up. Though ecological metaphors allow us to understand certain aspects of genre change, such as the often gradual accretion of variations that can ultimately transform a genre, such metaphors can also misrepresent deeply social, historically contingent practices by masking them in language borrowed from biology, with the unintended effect of naturalizing the very social and discursive processes that genre scholars aim to investigate.
Given the social and material nature of genres, metaphorical borrowings from registers other than biology may be more appropriate. To this end, I offer the concept of rhetorical scarcity, a term meant to evoke not only biological ecosystems but also unnatural, power-saturated inflections drawn from the register of economics. Rhetorical scarcity is a manufactured situation of intense and increasing constraint within a genre that significantly restricts rhetors’ access to key rhetorical resources. Of course, all genres delimit possibilities for the writers and readers who use them, enabling certain rhetorical actions by circumscribing others. Constraints built into genres are the foundation of the creativity that genres enable, as Amy Devitt, Richard Coe, and others have shown. Nevertheless, writers experience some genres as more flexible than others, not merely because certain genres become so familiar that they feel comfortable to the writers who take them up, but also because genres vary in the intensity with which genre users police genre and community boundaries. Some genres accommodate enormous variety while others come to serve as “symbolically charged landmarks over which to mount a not-an-inch-further last-ditch defense of the status quo” (Freedman and Medway 14). Although all genres enable action through the constraints and resources they organize, rhetorical scarcity helps us examine the direction of genre change over time and treats genre constraints as both manipulated and relative. That is, relative to prior possibilities embedded within a genre, does the genre change in the direction of greater capaciousness and flexibility, extending the ways in which the genre can be taken up and inhabited? Or does the change move in the direction of a kind of hardening of norms into absolute requirements, or a retrenchment into narrower limits and more severely delimited constraints? Whose interests are served by the direction of these changes?

The case I use to illustrate this approach is drawn from anthropological discourse, which, though little studied in rhetoric and composition, can offer significant insights into the intersections between power relations and genre change. Enormous shifts in the discursive practices of anthropologists took place during the roughly fifty-year period between 1888 (when the American Anthropologist began publication) and 1945 (after which post–World War II restructuring altered many academic disciplines). Over these decades, anthropology shifted from being a proto-professional community, characterized by
diverse research methods, practitioners, and publication outlets, and became a thoroughly professionalized academic discipline, with standardized avenues for entry into the profession and a large and complex institutional apparatus for credentialing new members (Stocking, “Ideas”; Patterson 64–65). This was also the period when anthropologists solidified their claims to scientific status. As historian George W. Stocking Jr. has argued, despite anthropologists’ roots in diverse fields across the humanities, physical sciences, and social sciences, “in facing the public, they have in general insisted on their status as members of a larger scientific community, and on the whole, the world of science has given credence to that claim—though not without . . . serious questioning” (“Paradigmatic” 342). Such “questioning” highlights what scholars have shown to be true of sciences in general: that scientific status is not the inherent prerogative of a community but instead a collective rhetorical accomplishment (Gross; Gieryn). Investigating the rhetorical dimensions of anthropology’s disciplinary transformations, I argue that changes in the ethnographic monograph supported anthropologists’ early twentieth-century bid for scientific status.

I use the concept of rhetorical scarcity to focus on how the ethnographic monograph genre becomes more circumscribed or more flexible over time because framing genre change in this way keeps considerations of power and access firmly in view. Instead of metaphoric descriptions that treat genre change as evolutionary, terms such as circumscribed, narrowed, and delimited carry an evaluative tenor: presumably, a genre that is flexible and capacious sounds to my readers, as it does to me, preferable to a genre I describe as severely constrained and aggressively policed. This evaluative tenor is something that genre scholars have sometimes been reluctant to embrace, as Freedman and Medway have argued, asking: “How do some genres come to be valorized? In whose interest is such valorization? What kinds of social organization are put in place or kept in place by such valorization? Who is excluded? What representations of the world are entailed?” (11). Because genres are intimately connected to the workings of power, our accounts of how genres function should not only describe but also critique situations in which genres are configured so as to reinforce or enable domination, exclusion, and acts of epistemic and material violence. The economic inflections of rhetorical scarcity
move such questions to the center of studies of genre change. Instead of viewing genres as ecosystems that evolve, with or without human intervention and in response to limitations and pressures that are inherent, we can retain our awareness that social artifacts like genres are the product of power-inflected historical choices, and that the limitations and pressures that shape them are human constructions.

In what follows I first link specific changes embedded within the monograph genre to anthropologists’ efforts to shore up their discipline’s scientific status. Drawing on my analysis of dozens of early anthropological monographs, I trace the ways in which the trajectory of this crucial anthropological genre—from flexible, variable, and capacious to more rigorously bounded and policed—supported disciplinary transformations taking place between the late nineteenth century and the onset of World War II. The constraints that marked the ethnographic monograph after 1920 emerged, I argue, because they allowed the genre to perform gatekeeping functions, limiting anthropological membership and delimiting anthropologists’ relationships with their audiences and the subjects of their knowledge. I then use this example to further outline the concept of rhetorical scarcity, showing how this concept can help scholars in rhetoric and composition investigate the role of power in shaping genre change.

**Anthropology’s Professionalization**

This short synopsis of anthropology’s development into a rigorous science of culture contextualizes the analyses of monographs that follow. Early American anthropology was an extremely heterogeneous and loosely organized field in which the topics considered anthropological, the manner and the institutions in which these topics were discussed and researched, and the practitioners who constituted the anthropological community were all exceedingly varied. Anthropology emerged as a distinct endeavor in the United States in the nineteenth century, shaped by a variety of (sometimes competing) intellectual, social, and political currents: by the early naturalist research of Thomas Jefferson, whose studies attempted to deflect European criticisms of the American environment as inferior, for instance, as well as that of physician Daniel Brinton, whose historical research supported scientific racism. When the Anthropological Society of Washington formed in 1879, the listed occupations of its all-male membership included “government official, physician, banker, engineer, ‘antiquarian’ and ‘none’” (Rossiter 80). In addition, anthropology’s formative post–Civil War period was indelibly shaped by the context of American settler colonialism, in
which the U.S. government sought to rationalize treaty breaking and westward expansion and to control, intellectually and militarily, an ever-larger national landscape. Thus a great deal of the earliest anthropological research in the United States was carried out under the auspices of government-sponsored expeditions and military campaigns.

After the Civil War, a series of geological, topographical, and, eventually, ethnological surveys of new territorial acquisitions in the West resulted in the creation, in 1879, of the Bureau of American Ethnology (BAE), directed by Major John Wesley Powell, scientist, Civil War hero, and famous explorer of the Colorado River (Fowler). Powell developed a large-scale and well-staffed program of field research over the next two decades, winning congressional appropriations for the BAE by arguing for the strategic importance of anthropological studies of American Indians. The institutional apparatus Powell generated through the BAE—including early publication venues for anthropological studies in BAE Annual Reports and Bulletins and the Contributions to North American Ethnology series—provided the impetus, the personnel, and the institutional resources that enabled anthropology’s nascent professionalization. As the field became established in this proto-professional period, anthropologists continued to draw intellectual resources and personnel from natural sciences such as geology, natural history, zoology, and paleontology, from evolutionary science, from history and folklore studies, and from pre-professional versions of the fields that would later become psychology, economics, and political science. In part because anthropology was initially located institutionally in museums and government agencies, only a very few PhDs in anthropology were granted before 1900; by 1912, some twenty doctorates in anthropology had been awarded, all to men (Patterson 50).

Several significant shifts in anthropology’s disciplinary identity became evident after 1920. First, the discipline’s primary institutional location shifted toward universities and away from museums and government agencies. This transition is evident in the growing importance of the PhD as a credential and the steep rise in the number of doctorates awarded during the 1920s and 1930s; between 1929 and 1941, 149 men and 30 women earned PhDs in anthropology from U.S. universities. Franz Boas was the most prominent figure working throughout the early twentieth century to establish anthropology as a professional and university-based discipline, to extend academic credentialing to women, and to promote an anti-racist and anti-eugenic agenda for the profession (see Darnell; Lewis; Liss). Boas began teaching anthropology at Columbia University in 1896; his first PhD student at Columbia, Alfred Kroeber, received
his degree in 1901 and then joined a new graduate department of anthropology at the University of California, the first established west of the Mississippi. As this pattern continued—newly credentialed anthropologists left from major centers of instruction at Columbia, California, and Harvard to establish new graduate programs—universities increasingly became the centers for disciplinary practice, and the flexible standards of participation that had welcomed bankers, engineers, physicians, travelers, and other varied practitioners in earlier decades became less flexible as well.

In addition, a changed cultural and social context after World War I heightened anthropologists’ push toward professionalization and altered funding structures in significant ways. The relative prominence of female, Jewish, and other minority practitioners in the field contributed to a perception of anthropology as leftist, pro-immigrant, pro-Jewish, and feminized—a perception that was damaging, if not damning, in a post–WWI context marked by heightened anti-Semitism and anti-immigrant sentiment in the United States, well-funded eugenic research, and general suspicion of leftist intellectual and political activity (Patterson). At the same time, new institutions of research funding emerged, particularly the National Research Council (NRC), the Carnegie-funded American Council of Learned Societies, and the Rockefeller-funded Social Science Research Council (SSRC), all of which became major funding sources after WWI. Rockefeller philanthropies alone “injected more than fifty million dollars into the advancement of the social sciences in the United States during the 1920s and early 1930s,” hoping to make “the social sciences more scientific in order to promote social and economic stability, to eliminate subjective studies of social phenomena, and to develop more effective methods of social control” (Patterson 73). Anthropologists were eager to join sociologists, economists, and political scientists in securing the status and funding available to rigorous, academic, highly specialized professionals undertaking scientific research into emerging social problems and concerns (Stocking, “Ideas”). Thus interwar anthropologists undertook to define their discipline’s legitimate practices and practitioners more strictly in order to establish a firmly scientific identity for anthropology—an identity that was in many ways threatened by anthropology’s reputation as a welcoming science for women. When Alfred Kroeber wrote to Elsie Clews Parsons that “if ever Anthropology gets to be prevailingly a feminine science I expect to switch into something else” (Letter), he voiced a widespread worry about the repercussions of such feminization for anthropology’s disciplinary prestige.
In the midst of these institutional changes, how did anthropologists shape the direction of change within their most privileged genre, the ethnographic monograph? And how did anthropologists manage the crucial rhetorical resource organized by this genre, namely the practice known as fieldwork, which was essential in anthropological knowledge production? In anthropological arguments, firsthand observation and its textual representation constitute perhaps the most valuable rhetorical resource for establishing the legitimacy of anthropological knowledge claims. How was access to fieldwork shaped by access to other institutional resources across these transformative decades?

Ethnographic Monographs before 1920: Coordinating Anthropological Activity

In analyzing early ethnographic monographs, I have attempted to take seriously Carolyn Miller’s suggestion that we define genres not by what they look like, but what they do. That is, genres should be identified in relation to their function within a discourse community, by the purposes they serve among the rhetors and audiences who use them, rather than by formal characteristics such as length or the presence or absence of any particular trait. Conceptualizing genres in this way resists identifying any single characteristic that a given text must have to “count” as an instance of a particular genre, making this approach particularly appropriate for analyzing texts produced during a period of transition, when a community pursuing shared goals might develop shared uses for texts with otherwise dissimilar textual features.5

Indeed, potential for variation—in length, method, scope, and other measures—is one of the characteristics of the early ethnographic monograph genre that is most crucial for its function within anthropologists’ developing discourse community. The primary actions undertaken by early anthropological monographs include creating new anthropological knowledge and sharing that knowledge with a community of diverse practitioners still only loosely defined. The term monograph—as it emerges from my analyses and as it appears in reviews and bibliographic articles published in the early twentieth century in anthropological journals—denotes an extensive treatment of subject matter
connected in some fashion to the range of interests anthropologists maintained in human physical, historical, linguistic, and cultural variation. “Extensive” is relative, ranging from Albert Gatschet’s 1,500-page work *The Klamath Indians of Southwestern Oregon* to Kroeber’s thirty-page dissertation. Relative to what anthropologists knew about Kroeber’s topic, “Decorative Symbolism of the Arapaho,” Kroeber’s thirty pages constituted an appropriately exhaustive treatment to merit identification as a monograph by his colleagues.

In addition to significant variation in length, early monographs also varied in the disciplinary training of the writer, revealing the ongoing presence of pre-professional members throughout the field’s proto-professional development. Between 1885 and 1920, the field of anthropology was populated simultaneously by researchers who had completed advanced training in another discipline, such as medicine or natural science, as well as by the first generation of writers to earn PhDs in anthropology. For example, Aleš Hrdlička, foremost physical anthropologist in the United States, studied medicine at the New York Homeopathic Medical College before publishing his monograph, *Physiological and Medical Observations among the Indians of Southwestern United States and Northern Mexico*, in 1908; the same year, the monograph *The Pima Indians* was published by another physical anthropologist, Frank Russell, whose 1898 PhD from Harvard was the first doctorate awarded for physical anthropology in the United States (Spencer). Franz Boas’s graduate training was in physics; his numerous ethnographic monographs were published alongside the work of graduate students earning the first PhDs in anthropology at Columbia under his direction. The monograph genre these writers enacted was thus capable of accommodating shifting and overlapping research careers, as researchers trained in diverse pre-professional disciplines—or scarcely trained at all—continued to work alongside those who began earning more specialized credentials in anthropology.

The degree of analysis to which data was subjected varied substantially in the genre as well. For instance, Alice Fletcher’s 1904 monograph, *The Hako: A Pawnee Ceremony*, follows each description of a ceremonial event with interpretive explanations and concludes with an extensive (roughly ninety-page) “Analytical Recapitulation” that offers further discussion of the significance of each element in the ceremony. In contrast, George Dorsey’s 1905 monograph *The Cheyenne* deploys an organization similar to Fletcher’s yet omits interpretive remarks throughout and concludes with a much less extensive (roughly seven-page) discussion of the ceremonial data. Many other monographs, including
Pliny Earle Goddard’s *Jicarilla Apache Texts* and many of Boas’s publications, contain still less analysis, consisting entirely of myths recorded by the anthropologist for future analysis as linguistic and cultural data.

Finally, the degree of technical language deployed in early monographs varies as well. Fletcher’s monograph *The Hako* includes exceedingly complex musical and linguistic notations; Albert Gatschet’s 1884 monograph *A Migration Legend of the Creek Indians* uses several specialized registers for discussing Creek kinship structures, tribal divisions, and linguistic characteristics. In contrast, texts such as Frank Hamilton Cushing’s *Outlines of Zuni Creation Myths*, published by the Bureau of American Ethnology in 1896, is presented as a highly accessible narrative. As might be expected from a writer who also published his Zuni research in the *Century* and other literary periodicals, Cushing notes with light irony that around 1598 “the Franciscan Friars, although sometimes baptizing scores of the Zuni—much to their gratification, doubtless, as quite appropriate behavior on the part of such beings when friendly,—had not antagonized their ancient observances or beliefs. . . . But all this was soon to change” (327). A typical mixture of technical and narrative language is evident in James Mooney’s famous 1896 monograph, *The Ghost Dance Religion and the Sioux Outbreak of 1890*, also published by the BAE. In this work Mooney sometimes deploys a scientizing third-person voice that creates distance from the data, noting, for example, that “the author . . . carried a kodak and a tripod camera, with which he made photographs of the dance” (654). Elsewhere, as when Mooney remarks that across cultures and times, a community’s “hope becomes a faith and the faith becomes the creed of priests and prophets, until the hero is a god and the dream a religion, looking to some great miracle of nature for its culmination and accomplishment” (657), he draws on language more characteristic of nineteenth-century humanistic studies.

*Early anthropological monographs constituted a site for knowledge production that, crucially, was flexible and spacious enough to accommodate the variety of forms of anthropological knowledge that marked the earliest decades of the discipline’s emergence.*

Thus the monograph genre across the late nineteenth and early twentieth centuries accommodated significant variation, variation that allowed monograph writers to analyze and interpret or to primarily collect and transcribe data; to deploy technical and scientific or humanistic and historical registers; and to write authoritatively whether their research was paid or unpaid, accomplished as their occupation or their avocation. Early anthropological monographs constituted a site for knowledge production that, crucially, was flexible.
and capacious enough to accommodate the variety of forms of anthropological knowledge that marked the earliest decades of the discipline's emergence. Among these diverse textual features, two commonalities stand out: first, the consistent use of a practice I identify as *spatial synecdoche* to link ethnographic data to a large, imagined map and second, the flexible denotation of fieldwork as a knowledge-making practice.

**Spatial Synecdoche**

Spatial synecdoche, a rhetorical resource often used in pre-1920 monographs, functions to coordinate the anthropological community's diverse knowledge-making practices, helping early anthropologists perceive their (often dramatically) different projects as organized by and related to a shared institutional goal. In synecdoche, the conversion between part and whole "stresses a *relationship* or connectedness . . . that, like a road, extends in either direction" (Burke 428), allowing the user of synecdoche to emphasize relationships from one scale to another (such as species to genus). Spatial synecdoche, in ethnographic monographs, likewise works to represent spaces in their relations and connections to one another. During a period characterized by anthropologists' feverish collecting, early ethnographic monographs often situate the community being studied *in space*, in relation to an emerging cultural, archaeological, and linguistic map. Many monographs from this period begin, for instance, with a spatial orientation that positions the community being studied in relation to other tribes and within regional or national frames. Such positioning implies a synecdochal relationship between the small cultural part the anthropologist has studied and the whole encompassing world of human variation. For example, in *A Migration Legend*, Gatschet organizes his ethnographic material spatially to connect the Creeks, the primary object of his study, with "the tribes and nations living around them" (9). In an extended spatial orientation that is typical for pre-1920 monographs, Gatschet writes: "Beginning at the southeast, we first meet the historic Timucua family . . . [and then] we pass over to the Yuchi, on Savannah river, to the Naktche, Taensa and the other stocks once settled along and beyond the mighty Uk'hina, or 'water road' of the Mississippi river" (11). Mapping the territory that surrounds the focus of his study, Gatschet provides other anthropologists with a mechanism for positioning his individual study relative to others' studies; that is, he indicates where his contribution fits within the larger map he portrays as under construction by the whole community of anthropologists.
Such spatial descriptions simultaneously construct boundaries around a particular community and link that community to a larger project of anthropological mapping. Spatial descriptions enable anthropologists to link their circumscribed studies to the vast project of creating a detailed, complete, coherent map of human cultures. For instance, in his 1897 monograph on the Kwakwaka’wakw, *The Social Organization and the Secret Societies of the Kwakiutl Indians*, Boas uses spatial descriptions to nest a specific tribe within the regional frame of the North Pacific Coast, which in turn he inscribes within national (Canadian, American) and continental (North American) boundaries. Through such detailed spatial descriptions, the knowledge produced and circulated by a monograph is meaningfully *bounded* and yet simultaneously *articulated*, linked, within an encompassing anthropological map. This persistent portrayal of data in relation to an imagined map functions institutionally, allowing early anthropologists to link data spatially and thus coordinate the collection activities of diverse, dispersed practitioners. Spatial synecdoche in early monographs suggests that anthropologists need only continue their feverish collecting activities to fill out their map of human variation; although the world is large, each piece of collected data could be fitted into this geographic framework and could thus contribute to the anthropological project.

**Flexible Fieldwork**

Although Boas emphasized fieldwork in training generations of anthropologists during his four decades at Columbia, fieldwork was by no means universal in its meaning or its application as a resource for knowledge production in monographs during the formative pre-1920 period. Anthropological fieldwork could take a number of forms, most of which depended less on linguistic competence and long-term participation than later forms of fieldwork demanded. In much of Boas's published research, the model of fieldwork he used involved touring through a region and finding bilingual informants who would discuss customs, beliefs, and rituals for Boas to transcribe and collect; Boas worked especially closely with George Hunt (Tlingit), who served simultaneously as informant, interpreter, and collector of linguistic and ethnographic material (Berman; Briggs and Bauman). Others relied on a similar though less intensive model of fieldwork, remaining in one village for a few weeks at a time and collecting firsthand reports from a number of informants. Many early monographs follow
a model of fieldwork as text collection; this model is evident in Goddard’s 1904 monograph, *Hupa Texts*, in which Goddard records chants, songs, and stories in an indigenous language and then pays a bilingual interpreter to translate them. These less intensive forms of fieldwork meant that a researcher could collect data even without particular linguistic competence by relying on multilingual informants and interpreters, and it allowed even short stays in the field to yield copious data collected in such a manner. Thus flexible forms of fieldwork served early anthropologists’ urgent desire for collection; they permitted researchers to gather data even amid haphazard, truncated, and disrupted itineraries. When contrasted with the rigidity that the term *fieldwork* would attain during the 1920s, the varieties of methods of firsthand observation deployed by writers of early anthropological monographs is striking.

These shared characteristics of monographs can be understood as meeting crucial institutional needs, especially the need for the diverse activities of heterogeneous practitioners to be (and to be perceived as) coordinated. While the monograph remained a highly flexible site for the rhetorical actions of knowledge production and distribution, the resource of fieldwork remained readily available—to trained as well as untrained practitioners, to amateurs as well as professional researchers, and to those undertaking only short-term and unfunded trips into the field. The availability of fieldwork as a rhetorical and epistemological resource meant that the diverse participants in this emergent science could use fieldwork to generate and authorize new knowledge that could contribute to anthropologists’ large-scale intellectual project.

**Ethnographic Monographs after 1920: Enacting Rhetorical Scarcity**

Amid the myriad historical changes that characterized the interwar period, the monograph genre was substantially altered through the practice of writers who constructed fieldwork in more constrained ways. Anthropologists throughout the 1920s and 1930s report on their field methods more specifically and extensively than earlier writers, and what they construct through such statements is a version of fieldwork as intensive, long-term participation in the everyday life of a community. Bronislaw Malinowski articulates the cluster of values connected to a more constrained form of fieldwork in the introduction to his famous 1922 monograph, *Argonauts of the Western Pacific*, where he writes, “I have lived in that one archipelago for about two years, in the course of three expeditions to New Guinea, during which time I naturally acquired a thorough knowledge of the language. I did my work entirely alone, living for the greater part of the
time right in the villages. I therefore had constantly the daily life of the natives before my eyes, while accidental, dramatic occurrences ... could not escape my notice” (xvi–xvii). The key points of divergence from earlier, flexible forms of fieldwork are highlighted here: two years rather than a few months of habitation, resulting in “thorough knowledge of the language” gained by working “right in the villages”—rather than through interpreters and informants—and requiring the anthropologist to remain “entirely alone” at the same time. These elements identify new requirements for firsthand observations, requirements that anthropologists articulated repeatedly in monographs from the interwar period. Through such repetitions, anthropologists reshaped the monograph as a high-status institutional genre and the crucial resource of fieldwork as less readily available, limiting the flexibility that marked the genre in earlier decades and manufacturing a situation of rhetorical scarcity for subsequent writers.

One major component of the more limited version of fieldwork articulated in later monographs is simply a repeated expectation that researchers spend longer periods of time in the field. Writers after 1920 generally indicate some period from eight to twelve months as devoted to field research exclusively: Robert Redfield cites eight months of fieldwork for his 1930 monograph on Tepoztlán, though he laments this duration as “too short ... for such a monograph” (vii). Margaret Mead draws on nine months of fieldwork for her famous 1928 monograph, *Coming of Age in Samoa* (10); Hortense Powdermaker spent more than ten months conducting participant observation on Malinowski’s model for her 1933 monograph *Life in Lesu*. Horace Miner’s study of folklife in a Canadian village, *St. Denis: A French-Canadian Parish*, is based on “continuous residence in the parish from July 1, 1936, to June 1, 1937” (vii), and Wendell Bennett and Robert Zingg explain similarly: “We began our residence here in October, 1930” and spent “the next nine months” investigating highland Tarahumara life in northern Mexico (vii). The growing importance of a fall-to-spring academic calendar is evident in these timelines.

Such longer terms of field research demand funding; indeed, most of these texts also specify the organization that supplied funds for their field research during the academic year. These sources include national and international organizations such as the Social Science Research Council, cited by Redfield, Miner, and others, and the National Research Council, which funded Mead’s research in Samoa. Local and university-based sources are also identified, such
as Columbia University’s Research Council of Social Sciences as well as the Acculturation Fund, which Reo Fortune cites in his 1932 monograph *Omaha Secret Societies*.

In addition to statements attesting to the length of time spent in the field, later monographs frequently include a detailed itinerary that traces *where* and *for how long* each stage of fieldwork lasted, crafting an explicitly scientific rationale for research travel. Ralph Linton’s 1933 monograph *The Tanala: A Hill Tribe of Madagascar* traces the author’s movements through Madagascar with elaborate specificity, charting month-by-month progress with specific place-names and dates, such as this description: “In November, 1926, I sailed from Tamatave to Farafangana, on the southeast coast, and after two months there, devoted to study of the Antaifasina and Antaimorona tribes, continued overland to Fort Dauphin. After a month’s stay at Fort Dauphin I crossed the southern end of the island to Tulear” (15). Malinowski includes a list of sub-expeditions with their dates (16) and a detailed narrative account of how each month was occupied during the two years he spent in the Trobriand Islands. An even more exaggeratedly detailed itinerary is provided in J. Alden Mason’s monograph *Archaeology of Santa Marta Colombia: The Tairona Culture*, in which Mason devotes ten pages to describing his progress toward and movement through Colombia. He includes specific dates, sub-itineraries, and descriptions of the weather, such as “That afternoon, May 21, the sea calming considerably, the canoe was urged past the cape of San Juan de Guía, and safely landed on the sandy beach at Cañaveral” (16–17), and even accounts for such mundane research activities as “seventeen days spent in packing the large collection, boxes and packing materials being difficult to secure” (20). Such itineraries provide a scientific specificity to the vagaries of “fieldwork” and represent research as the all-encompassing activity of the anthropologist. In this way including itineraries argues for a kind of purposefulness in anthropologists’ travel, which these texts portray as determined exclusively by research interests and scientific problems, rather than undertaken alongside colonial government or mission work.

The argument that all time in the field was occupied by research and that fieldwork decisions were directed solely by considerations of scientific merit is now known to be something of a fiction. For instance, Malinowski’s fieldwork in the Trobriand Islands was so extended in part because the outbreak of World War I prevented his return to his native Poland; he punctuated his stay in the Trobriands with lengthy intervals in Australia, and the publication of his field diaries in 1967 revealed extensive periods of time spent reading novels, socializing with other white Europeans, and isolating himself in his tent rather than...
pursuing the observation of everyday life as he claimed in *Argonauts* (Stocking, “Maclay, Kubary” and “Ethnographer’s Magic”; Visweswaran; Young). The historical circumstances, colonial governments, and institutional structures that enabled long-term field expeditions like those undertaken by Malinowski, Linton, Mason, Mead, and many others are eliminated from most monographs after 1920, which instead represent research travel as motivated exclusively by scientific rationales rather than being shaped by historical accident or global imbalances of power.

The longer periods of fieldwork prescribed in later monographs are constructed as intensive—that is, devoted entirely to the observation of community life and collection of data—to distinguish proper fieldwork from the kind of research that could take place alongside other official duties, such as education, mission work, and colonial administration. None of the writers of post-1920 monographs portray themselves as conducting fieldwork alongside other work, and many take pains to demonstrate the degree to which they lived among their research subjects, in a state of constant observation. Mead, for instance, writes: “I spent the greater part of my time with them [the girls of the community]…. Speaking their language, eating their food, sitting barefoot and cross-legged upon the pebbly floor, I did my best to minimize the differences between us and to learn to know and understand all the girls of three little villages on the coast of the little island” (10). Such a statement contrasts strongly with those of pre-1920 monographs, where writers frequently provided only the names of informants and interpreters who visited the writer’s residence to produce and translate oral texts.

The emphasis on intensive firsthand observation of ongoing daily life is heightened in discussions of method that increasingly appear in monographs after 1920. Miner’s 1939 monograph on French-Canadian folk culture assures his reader that throughout his months of “continuous residence” his “own language and creed were no bar to social contact and full participation in the life of the community” (vii). Similarly, Bennett and Zingg begin their long methodological discussion by asserting the intensive nature of their fieldwork: “First and foremost of the methods used was the observation of native life in an isolated, non-Spanish speaking, Tarahumara region. We lived with the Tarahumaras, went to their homes, observed them in their daily activities, and attended their fiestas” (x). The implicit message of such statements—that other anthropologists should adopt similarly intensive field methods—is made explicit later, when they write that “field work should be carried on while living with the people to be studied, participating as fully as possible in their daily
The demand for more long-term and intensive periods of fieldwork generates a related demand for fieldwork to take place in a state of isolation from other white people, further distinguishing legitimate fieldwork from the activities of missionaries, traders, and other “nonscientific” amateur anthropologists. Describing “proper conditions for ethnographic work” in *Argonauts*, for instance, Malinowski writes that these requirements “consist mainly in cutting oneself off from the company of other white men, and remaining in as close contact with the natives as possible, which really can only be achieved by camping right in their villages” (6). Although few writers make such overt claims to isolation as Malinowski does, essentially all post-1920 monograph writers assert their isolation in a negative way, by refraining from textual indications that might suggest otherwise. Professional anthropologists’ correspondence from the field amply documents their interactions with a sprawling network of colonial agents: the traders who ran stores on reservations, the Bureau of Indian Affairs agents who enforced federal policy in local communities, the vast structures of colonial administration that supported and enabled anthropologists’ presence “right in the villages” they studied. Across post-1920 monographs, however, anthropologists obscure such interactions to support their self-portrayals as distant from other European or American people and immersed in isolation in the daily lives of members of a different culture.

In post-1920 monographs, *spatial synecdoche* still functions to link local studies with a cultural whole, but this spatial strategy is altered to accomplish the more overtly scientific aim of generalizability: that is, by suggesting that the part has been studied so fully that its relation to a larger whole can be taken as reliable. In this way spatial portrayals in later monographs construct the microcosm/macrocosm dynamic that Burke claims is a hallmark of synecdochal reasoning. This “relationship of convertibility” (Burke 427) between two terms is posited by ethnographic writers who assert that the small scale of their study mirrors and recapitulates the relations that would be observed
at a larger scale. Malinowski, for instance, writes: “the geographical area of which the book treats is limited to the Archipelagoes lying off the eastern end of New Guinea. Even within this, the main field of research was in one district, that of the Trobriand Islands. This, however, has been studied minutely” (xvi). Consequently, he asserts that his “Ethnographic material” covers “the whole extent of the tribal culture of one district” and represents “the totality of all social, cultural and psychological aspects of the community” (xvi). As in the examples from Boas and Gatschet above, Malinowski carefully positions his site of study in relation to a broader map.

Yet Malinowski’s claims to completeness depend less upon the work of other researchers in nearby districts than they depend on his assertion that the life in this one district has been studied so “minutely” that his findings can be extended to cover a larger cultural area than the one he observed directly. Mead, likewise, asserts generalizability from part to whole in her study of Samoa, claiming that “because one girl’s life was so much like another’s, in an uncomplex, uniform culture like Samoa, I feel justified in generalising although I studied only fifty girls in three small neighbouring villages” (11). Similarly, Robert Redfield asserts that because the village of Tepoztlán lies within the most populous state of Mexico’s central plateau, that village can stand in synecdochally for villages in general, which in turn mirror the spatial and cultural relations that characterize all of Mexico. Thus, in post-1920 monographs, one village in one district stands in for an entire archipelago; one group of individual adolescent girls can substitute for an entire population; one small community can represent in microcosm the social relations at play across a vast and complex nation-state. By such synecdochal maneuvers, the intensive study of one small population can achieve the aims of generalizability that are so important in scientific knowledge production.

As these mechanisms for creating generalizable, method-bound knowledge helped anthropological field research more closely mirror the research practices of other scientists, the more constrained form of fieldwork repeatedly constructed in post-1920 monographs helped to create clearer mechanisms for distinguishing real scientists from nonscientists. Malinowski asserts in *Argonauts* that only those with scientific training merit the title “Ethnographer.” Only such training, completed *before* embarking on fieldwork, provides a crucial measure of reliability for an anthropologist’s results: “the Ethnographer has to be inspired by the knowledge of the most modern results of scientific study, by its principles and aims [because] foreshadowed problems are the main endowment of a scientific thinker” (8–9). As anthropologists revised
the monograph after 1920 to demand longer periods of intensive, problem-focused fieldwork, they reshaped who could be considered an anthropologist. The routine of fieldwork advocated by monograph writers during the interwar period—a routine that begins with scientific training in a university setting, where extensive reading of anthropological theory identifies a problem that can be solved by fieldwork, followed by at least nine months of intensive firsthand observation, and several subsequent months devoted to “working up” the material for publication—limited the community of anthropologists in material ways. By enacting a situation of rhetorical scarcity in the discipline’s most privileged site of knowledge production, anthropologists collectively narrowed the boundaries of their discipline.

Limiting the availability of rhetorical resources provided several important benefits to anthropologists, including fewer practitioners, less variable and more rigorous methods, clearer distinctions between insiders and outsiders, and consequently greater access to the material resources that became available in the interwar period for rigorously scientific social research. Post-WWI transformations in institutional structure generated an urgent need for what Thomas Gieryn has called “boundary work.” Through boundary work, scientists distinguish between their practices—and the credible, objective knowledge those practices ensure—and some “less authoritative residual non-science” (5). Such distinctions serve to reinforce the “link between ‘science’ and knowledge that is authoritative, credible, reliable, and trustworthy” (30) and to guard the power and prestige afforded to those who produce it. Specialization and professionalization became the dominant features of interwar anthropology, as anthropologists sought to clarify their professional boundaries and shore up their status relative to other social sciences jockeying for SSRC, NRC, and Carnegie funds (Fisher; Stocking, “Philanthropoids”). If “fieldwork” served as a rhetorical and epistemic resource of particular importance to anthropologists, then the constraints placed on access to this resource can be seen as maneuvers to define the boundaries of the anthropological community in more limited ways.

**Delimiting Rhetorical Scarcity**

In the early twentieth century, anthropologists built constraints into the monograph genre that allowed them to alter prior requirements for participation in their disciplinary community. Studying how these requirements are instantiated within the discipline’s privileged genre reveals the accumulated consequences of individual rhetorical choices. Individual writers in an ongoing
chain—Malinowski, Powdermaker, Mead, Bunzel, Redfield, Miner, and many others—collectively remade what a monograph could look like and could do during the 1920s and 1930s. As individual writers inhabit a genre differently, they effect changes in what is possible and available for future genre users. Viewing these changes as choices that construct a situation of scarcity, this study calls for greater attention to moments when writers’ choices restrict other writers’ access to the resources that a genre makes available.

Rhetorical scarcity extends recent scholarship that emphasizes the activity of genres in organizing rhetorical resources for social action, subject formation, and knowledge production. Whether viewed as “constellations of regulated, improvisational strategies” (Schryer, “Walking” 450), as “rhetorical ecosystems” (Bawarshi, “Ecology” 70), or as “the familiar places we go to create intelligible communicative action” (Bazerman 19), genres powerfully shape relations among rhetors, audiences, and the world. Spatialized theories of genre highlight the capacity of genres to change in response to the accumulated decisions of individual genre users and reveal genres not as static backdrops but as ecosystems that shape, and adjust to, the practices of the rhetors who inhabit them.

As rhetorical ecosystems, genres supply rhetors with resources as well as constraints, and many scholars have recognized that constraints serve a significant role in genres’ usefulness. We witness the productivity of genre constraints for individual writers, for instance, when a student, having identified her genre—“Blue Book essay exam,” for instance, or “breezy news feature”—is better able to make subsequent decisions to shape a piece of writing. This productivity has led Richard Coe to argue that genres are “both constraining and generative—or, better said, generative because constraining” (“Arousing” 185). Because genre-based constraints focus a writer’s attention, scholars have suggested that genres are not “restrictive curbs on creativity, [but instead] can be imagined as invitations, as forms that give certain kinds of ideas or arguments expression” (Danielewicz 427). As Amy Devitt explains, “both constraint and choice are necessary and therefore positive components of genre” (Writing 139–40). The constraints that genres organize are fundamental to the work—of arguing, identifying, communicating, and organizing social life—that genres enable.

While I recognize the productivity of genre constraints for individual writers and for broader communities and organizations, it is still possible to critique the effects of the social and rhetorical activity that a genre enables. In
their tendency to permit—or invite—only certain kinds of arguments, genres are “inherently ideological,” embodying “the unexamined or tacit way of performing some social action” (Schryer, “Lab” 108). In this way genres frequently normalize and reproduce relations of power and stabilize the worldviews they imply—for instance, by constructing some people as knowers while positioning other people as consumers or objects of knowledge (Henze) and by authorizing certain versions of reality at the expense of other versions (Winsor). As I argue above, in early American anthropology the monograph genre performed such normalizing functions; the monograph became an increasingly narrow space for locating anthropological arguments, circumscribing rhetorical possibilities even as the genre gained importance as a primary site for anthropological knowledge production.

Such a critique is particularly significant in scientific contexts, where genres and the textual practices they organize are crucial in the construction of “objectivity” and the privileged epistemic status that scientific and technical discourses enjoy (Little). Not only is knowledge production “carried out and codified largely through generic forms of writing: lab reports, working papers, reviews, grant proposals, technical reports, conference papers, journal articles, monographs, and so on” (Berkenkotter and Huckin 476), but shared attitudes toward appropriate kinds of knowledge-making practices are produced as well, as genres typically “conform to a discipline’s norms, values, and ideology” (476). Consequently, the genres that a discipline uses to generate appropriate, methodologically sound knowledge also function as mechanisms for disciplining the knowledge that members of the community create. By privileging the ethnographic monograph as the primary site for anthropological knowledge production, for example, the professionalizing community of early American anthropology was able to construct a boundary between legitimate and delegitimized knowledge-making practices, affirming certain textual and material practices and discounting others.

Such considerations of power are too easily sidelined in accounts of genre change that rely upon evolutionary metaphors. Such metaphors have led some scholars “to display the same respect for the intricacy and functionality of a social system as we have learned to show for a biological one” (Freedman and
This is seen, for instance, in Richard Coe’s claim that just as “one explains the development of a species as an adaptation to a niche in an ecosystem, so one should explain a recurring text type as a functional response to a recurring rhetorical situation” (“New Rhetoric” 198). Although genre changes might be explained in part as functional adaptations to an evolutionary niche, the limitations of such explanations are also clear: namely, if we adopt these terms, then the features we see in contemporary genres seem provided with a ready-made explanation for their emergence. Evolutionary explanations suggest that genre features exist because they emerged as successful adaptations that improved in some way the “fittedness” of a certain genre to the environment of its use. For instance, Alan Gross, Joseph Harmon, and Michael Reidy extend the explanatory power of evolutionary metaphors in their impressive longitudinal study of the scientific article, identifying pressures toward efficiency and objectivity as mechanisms shaping the development of the article over several centuries. The carefully theorized evolutionary framework they develop is especially useful in showing how micro-level decisions in individual texts (or “organisms”) ultimately accumulate to shape “the species [i.e., the genre] as a whole” (218).

Yet the pervasive effects of relations of power are (perhaps inevitably) marginalized in such evolutionary studies, where biological metaphors mask the historically contingent nature of genre change. For instance, although Gross, Harmon, and Reidy note that “the scientific article has evolved, not in the sense of becoming better (or worse), but in the sense of changing to cope with the communicative and argumentative needs of an evolving set of disciplines” (219), nevertheless to argue that specific textual features emerged because of their fittedness for certain ends still naturalizes those features that “succeeded,” rather than historicizing textual artifacts against configurations of social and institutional power. Evolutionary explanations also forestall investigation into other explanations of the emergence of genre characteristics: for instance, that other textual features might have appeared “more fit” to some members of a community, but those members were not in positions of institutional power to achieve their adoption. I argue, as does Dylan Dryer, that accepting such ready-made explanations for genre change (or genre stability, in some cases) “constrain[s] our understanding of—and thus our ability to intervene effectively in—the injustices that some genre systems reflect and produce” (Dryer 504).

Building on contemporary scholarship that views genres as “a way in which power is constructed, organized, and put into effect” (Winsor 10), rhetorical scarcity aims to historicize rather than naturalize genre change.
As a manufactured situation of intense and increasing constraint, rhetorical scarcity complicates models that represent genres as evolving naturally toward greater efficiency or efficacy. Combining the ecological register of new genre theories with inflections from economics, the concept of rhetorical scarcity prompts scholars to ask how the direction of genre change can be manipulated to manufacture scarcity out of resources that are not inherently limited. In the ecological register, scarcity reminds us that rhetorical resources flow less easily into and out of a space with less permeable boundaries. In contrast, an environment that is loosely bounded or expansively defined is likely to make a greater variety of rhetorical resources available to practitioners.

At the same time, economic inflections of the term scarcity remind scholars that genre boundaries are not constructed naturally or inevitably, but through the actions of genre users who are always embedded in relations of power. Genre users can narrow the purposes, audiences, and available arguments of a genre by drawing firmer boundaries around a smaller center, manufacturing a situation of greater rhetorical scarcity, wherein access to some rhetorical resources becomes sharply limited. Furthermore, rhetorical resources, in an economic sense, can have greater or lesser value, and a situation of scarcity typically increases a resource's value. For instance, in a setting where few individuals have relevant experience, such as a newly formed community organization composed of people who have not previously taken part in grassroots organizing, any such prior experience might become an invaluable rhetorical resource, in part because of the scarcity of that resource within that context. Furthermore, in the economic register, we recognize that both scarcity and value can be artificially manipulated by changes to a market. Access to resources can be limited by erecting firmer boundaries or by delimiting the market in a new way, such that participants who were previously inside find themselves outside newly drawn boundaries.

These inflections from economics also stress that rhetorical resources are not inherently limited; rather, they are constructed within genres as available or unavailable, appropriate or inappropriate. Consequently, access to certain resources is what a genre constrains. For example, in anthropological monographs, there are surely as many opportunities for firsthand observation available to writers after 1920 as there were before. What becomes constrained is access to a particular rhetorical resource—in this instance, by the repeated
rhetorical act of defining the resource of fieldwork in far more limited ways. Genre users can work to instantiate increasingly sharp boundaries between a genre they own and those affiliated with neighboring communities, or can work to control access to genre-based resources in order to limit or expand community membership. Thus the economic register can help us see scarcity as a constructed, artificial phenomenon, rather than any limitation inherent in the rhetorical environment. Ultimately, these economic inflections remind rhetoricians that access to resources is mediated by relations of power.

Evolutionary descriptions of genre change only partially explain how genres come to take their specific shape. Certainly the features anthropologists built into the monograph were functional and in that sense can be understood as developing in response to specific institutional and historical pressures. For instance, the lack of consolidation of norms in early monographs served institutional ends, by permitting anthropologists to perceive their extraordinarily varied discursive and epistemic practices as coordinated within a shared intellectual project. Yet in the case of the ethnographic monograph, to say that a more constrained form of fieldwork “evolved” as anthropology professionalized would be to mask the human effort that was necessary as anthropologists built this constraint into their privileged genre. In contrast, historical studies of genre change must press beyond evolutionary models in order to denaturalize textual features that can come to seem inevitable when viewed in evolutionary terms. Although anthropologists responded to a felt need for rigor by creating constraints that made the monograph genre more exclusive and less widely available, this connection between rigor and exclusivity is historical, not inevitable. Genre scholars can denaturalize this link by uncovering the role of power in the operation of genre change.

It is important to note that this particular tactic—generating scarcity in the resources available in a privileged institutional genre—is not the only possible response to concerns about a community’s professional identity. For instance, Jan Golinski describes the tactics of early chemists who responded to a crisis in their institutional legitimacy by involving the public in more fundamental ways in their research process; limiting amateur participation or outsider involvement is not the only avenue available to scientists when their legitimacy appears to be in question. Indeed, even using such terms as specialization and professionalization to describe changes in scientific communities implies a teleology of scientific progress that historical studies should aim to disrupt rather than reify (Golinski 10). Consequently, attending to how
a genre becomes more or less flexible, capacious, and rich with possibilities for writers can generate important insights about who is permitted to use a genre and to what ends. For instance, we might look to the example of the personal essay to see how individual users have expanded the collective possibilities available in that long-standing genre. Indeed, scholars such as Sidonie Smith and Cristina Kirklighter have explored the powerful possibilities opened up by writers who have repurposed the genre of the personal essay in exciting ways, politicizing its function as a location for subject formation and expanding dramatically the resources for rhetorical and political action that can be located there. The rhetorical richness that is rhetorical scarcity’s counterpoint becomes more evident in comparison, as do the long-term, accumulated effects of individual writers whose decisions can ultimately narrow or expand a genre’s possibilities.

Consequently, attending to how a genre becomes more or less flexible, capacious, and rich with possibilities for writers can generate important insights about who is permitted to use a genre and to what ends.

In addition, rhetorical scarcity provides a mechanism for not only tracing genre changes over time but also evaluating the degree to which such changes serve to maintain or to adjust relations of power. Offering “an archaeological unearthing of tacit assumptions, goals, and purposes” (Freedman and Medway 2) embedded within genres that persistently mask their function within (often inequitable) social institutions, the concept of scarcity prompts us to tell more politicized stories of genre change. Which community users are shut out of the space of a privileged genre or denied access to the rhetorical resources organized by that genre? Those who also lack access to other forms of institutional support? What lines of argument are held as falling outside a privileged genre’s boundaries? Arguments that insist a professional community must engage the needs of its marginalized constituents, for instance? In addition, rhetorical scarcity raises questions about the capaciousness of privileged genres, which is particularly crucial when we recognize that any genre that becomes a prerequisite to further disciplinary participation serves a policing function; how capacious are the genres, such as the exquisitely valuable journal articles, that one must write to become a full-fledged member of our own discipline? Which of our many, sometimes conflicting institutional needs are served as certain genres become more privileged? Scholars in rhetoric and composition have historically attended closely to the political repercussions of our institutional prerogatives and our discursive choices; rhetorical scarcity reminds us
how tightly these dimensions of our disciplinary life are bound together. As we inhabit our discipline’s genres, we shape our discipline’s boundaries as well.

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Notes
1. On spatial metaphors overtaking container and tool metaphors—although those metaphors persist in scholarship and public usage—see Devitt, “Creating.”
2. Brent Henze has examined the formation of ethnology in Britain in the nineteenth century. No other historical research in rhetoric has examined anthropological discourse, to my knowledge, although anthropological concepts and methods, especially ethnographic methods, have been significantly incorporated into composition studies (see Cintron; Chiseri-Strater). In rhetoric of science, the “historical sciences” and the “human sciences” have been generally understudied, as Miller and Halloran indicate (108).
3. Although extensive critique is outside the scope of this article, I want to acknowledge that anthropology’s professionalization, as a science of “other men in another time” (Fabian), held profound material, psychological, and political consequences for the communities of Native American and worldwide indigenous people whom anthropologists studied. Scholars will find a rich body of such critique in contemporary scholarship in anthropology and Native studies. See especially Asad; Fabian; Grek-Martin; Michaelsen; Wilder. Wolfe provides a highly useful critique of the shared logics of anthropology and colonialism.
4. Sources for these figures vary somewhat, depending on how broadly or narrowly historians constitute “anthropology” as a field; all chart a similar pattern of gradual increase from 1900 to 1920 and much more rapid increase from 1920 to 1940. See Bernstein; Patterson; Rossiter. On the steep rise in doctorates awarded to women in anthropology, see Rossiter 157, which indicates that the number of doctorates in anthropology awarded to men doubled between 1921 and 1938 (from 54 to 100), while those awarded to women in anthropology increased tenfold (from 2 to 20). On the particular importance of Columbia University in awarding doctorates to women, see Rossiter 150–52.
5. The summarized results presented in this article are drawn from more extensive analyses of some one hundred ethnographic monographs published between
1888 and 1945. I identified monographs for analysis by first consulting all works published in anthropological series by the departments and museums where American anthropology was first established, including the Columbia University Contributions to Anthropology, the University of California Publications in American Archaeology and Ethnology, the University of Chicago Series in Anthropology, the University of Washington Publications in Anthropology, the Anthropological Papers of the American Museum of Natural History, and the Field Columbian Museum Anthropological Series. I also included governmental publication series, such as the Bureau of American Ethnology’s Reports and Bulletins series. I located additional monographs published outside of any regular series by searching book reviews and review essays from the American Anthropologist (including all volumes from 1888 to 1945), the Journal of American Folklore (1888 to 1945), and Man (1901 to 1945); texts referred to as monographs by early anthropologists were included in my analyses. Using 1920 to delimit the proto-professional and professional periods follows the practice of other historians of anthropology; see Collier and Tschopik; Hallowell; Stocking, “Ideas.”

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