Personal Equations

Reflections on the History of Fieldwork, with Special Reference to Sociocultural Anthropology

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ABSTRACT

In the latter part of the nineteenth century, diverse sciences grounded in natural history made a virtue of field research that somehow tested scientists’ endurance; disciplinary change derived from the premise that witnesses were made reliable by character-molding trials. The turn to the field was a function of structural transformations in various quarters, including (but hardly limited to) global politics, communications systems, and scientific institutions, and it conduced to biogeographical explanations, taxonomic schemes that admitted of heterogeneity, and affective research styles. Sociocultural anthropology, which took specialized shape at the beginning of the twentieth century, shared many properties with other field sciences, but its method—participant observation—was distinctive. Critical to the method’s definition were the efforts of the British experimental psychologist-anthropologist W. H. R. Rivers, who relied on notions then widespread in Europe and the United States. The discipline’s future mythic hero, Bronislaw Malinowski, embraced Rivers’s model. For both men, proper fieldwork meant using the researcher’s body as an instrument and entailed understanding both the anthropologist’s body and the research subject’s body as an energy system; this symmetry facilitated a relativist perspective. Participant observation remains central to sociocultural anthropology, but the discipline’s pedagogic habits contributed to loss of memory of its energetic conceptualization.

IN 1905, the eighty-two-year-old Alfred Russel Wallace—presumably short of funds, as he had been throughout his life—sought a buyer for his inventory of “foreign coleoptera.” He consulted a respected entomologist, David Sharp, who reported that possible

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Isis, 2011, 102:xxx-xxx
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0021-1753/2011/1021-0001$10.00
purchasers were “an almost extinct form of life in this country.” Sharp knew of only one likely customer: a Frenchman who frequently visited London, who had previously bought collections from the naturalist traveling companion of Wallace’s youth, Henry Walter Bates. As young men, Wallace and Bates had earned their livings gathering specimens for sale. By the end of Wallace’s long lifetime, however, the scientific market that had once sustained him was collapsing because a fundamental reorganization of scientific work was under way. The erstwhile division of labor between collectors and theorists was eroding: the once-separate tasks of accumulating and interpreting evidence were increasingly likely to be joined in the persons of professionals—formally trained and formally employed scientists.

For the young Wallace and Bates, career decisions were compounded of both scientific and commercial considerations. They had an agent, Samuel Stevens, who attended to both. While they were in foreign parts, Stevens sent them scientific reading materials to guide their activities. He also asked potential buyers what they should collect; displayed their finds at meetings of learned societies and published advertisements for them; sought to stimulate market demand by having their letters from the field reproduced in natural history journals; insured their trophies during shipment, sparing them catastrophic losses; and invested their profits. Wallace and Bates were at the high end of a social scale of naturalists who served a congeries of gentleman-scientists. Representing a transitional life form among scientists, they were accepted to a degree—but only to a degree—in elite scientific circles; although Bates (unlike Wallace) managed to find employment as a scientist—as Assistant Secretary of the Royal Geographical Society—it is notable that his paid work was limited to the lowly tasks of classification and editing.

Wallace and Bates’s story, like many others in my narrative, is drawn from the history of naturalists based in Britain. But the developments I identify were not peculiar to scientists in that country. Moreover, methodological changes in British sociocultural anthropology, which constitute my major case, affected practitioners worldwide. Overall,


4 Unless otherwise specified, my references to “anthropology” are to its sociocultural subspecies, not other subspecies (in the United States, biological anthropology, linguistic anthropology, and archaeology; disciplinary arrangements vary from one country to another). In Britain and British-influenced places, this was called “ethnology” in the late nineteenth and early twentieth centuries; “cultural anthropology; “social anthropology,”
the professional collector approached extinction as the status of the fieldworker rose. In explaining how the experience of fieldwork gained prestige among Western scientists of various types, historians and sociologists of science have found decisive moments from the early modern period to the recent past. Nevertheless, as I show in this essay, a qualitatively distinct shift occurred in the second half of the nineteenth century. I describe how and why armchair scholars, who relied on evidence gathered by others, ceased to be credible; how the reliable scientific fieldworker was defined; and the relationship between field methods and concepts.

Until the latter half of the nineteenth century, armchair scholars were accustomed to claiming the intellectual high ground. Although such pioneering intellectual giants of comprehensive natural history as Alexander von Humboldt and Charles Darwin had earlier made journeys of observational exploration, theorizing was largely the province of armchair scholars. Reviewing one of Humboldt’s works in 1807, Georges Cuvier pronounced the field naturalist’s observations “broken and fleeting,” while, by contrast, the “sedentary naturalist” was able to survey all available reports and “compare them with each other as often as necessary to reach reliable conclusions.”

Sometimes the collectors who served scholars undertook commissions. Others responded to the instructions that scholars broadcast to travelers, requests for specimens that the latter might chance upon while pursuing their regular occupations: collectors could be drawn from any walk of life. But because their labors could be physically arduous, in dirty and dangerous places (both close to home and far away), fieldworkers were often accorded low status. Indeed, some collectors were coerced participants in science, among them nineteenth-century Australian prisoners and slaves purchased for the specific purpose of gathering specimens.

And persons who earned their livings as collectors did not invariably identify themselves as scientific workers, not least because their clients frequently represented commercial interests. Thus collectors’ contributions were often rendered invisible, however important

and (sometimes) “sociology” during the interwar period; and (usually) “social anthropology” after the Association of Social Anthropologists was founded in 1946—an event denoting the complete differentiation of its members’ professional lives, although the Royal Anthropological Institute (founded in 1871 as the Anthropological Institute) continued to incorporate all varieties of anthropologists (and the differentiation of social anthropology had been long in the making). In the United States and its intellectual sphere of influence, the subspecialty has been called “cultural anthropology” or “sociocultural anthropology.” Unless a specific circumstance requires a specific reference, I use the term “sociocultural” to refer to all variants. The word “ethnography” denotes straightforward data collection. At different moments, there have been distinctly different analytic emphases in different national contexts. For example, as David Mills argues in Difficult Folk? A Political History of Social Anthropology (New York: Berghahn, 2008), the identification of explicitly British social anthropology originated with a 1951 dispute between the British Raymond Firth and the American George Murdock in the pages of the American Anthropologist: British anthropologists’ insistence on their distinctiveness became especially intense during the 1960s (see esp. pp. 25–26, 160–162). But the enterprise had an international character at many moments prior to its mid-twentieth-century squabbles, and it is arguably relatively international today.


7 Endersby, “Garden Enclosed,” p. 321. See also the early twentieth-century backwoods trappers hired by Berkeley’s Museum of Vertebrate Zoology, discussed in Susan Leigh Star and James R. Griesemer, “Institu-
they were for scientific judgments: the mid-nineteenth-century paleontological feud between Edward Drinker Cope and Othniel Charles Marsh, for example, was understood by their contemporaries—and is generally understood today—as a conflict between the two men, although they (especially Marsh) were dependent on intelligence transmitted by informants in the field.8

We can understand disdain for the collector’s task: life in the field could be grim. Consider the adventures of a relatively obscure explorer, Karl Mauch, who figured among a number of variously financed mid-nineteenth-century German naturalists whose reports from foreign parts fed the middle classes’ growing appetite for “vicarious natural history adventuring.” An erstwhile secondary school teacher who roamed southern Africa in the late 1860s and early 1870s, Mauch supported himself with his savings as well as the meager funds raised for him by the proprietor of the popular German geography magazine that published news of his exploits. His achievements included compiling topological and geological maps and finding two goldfields, but in 1871 he earned his place in history for an archaeological find: the ruined stone structures soon to be known as Great Zimbabwe, which had been the stuff of legend in Britain and the Continent since the early sixteenth century, imagined as vestiges of the biblical city of Ophir, the headquarters of King Solomon’s fabulous gold mines. Mauch was the first European to locate and describe the ruins for a European audience, after searching for them for more than three years—frequently hungry, cold, exhausted, and sick; stumbling in his disintegrating shoes and relying on faulty instruments; often collapsing in tears; resenting his dependence on his African servants and guides, who were none too trustworthy and prone to decamp; and fearing that, as one of but a few Europeans then present in the area of his explorations, he was “threatened with murder at every moment” by Africans who had lately acquired powerful firearms.9 His was an experience of a European operating on a colonial frontier (the area of his explorations did not become a colonial possession—and then one of a peculiar sort—until 1889, when Cecil Rhodes’s British South Africa Company gained title to it with a royal charter).10

MEANS, MOTIVES, AND OPPORTUNITIES

A number of historical changes conduced to the professionalization of the field sciences, rendering amateurs’ judgments noncredible. As scientists themselves recognized, the expansion and consolidation of empires—both formal and informal—provided them with valuable sites for the prosecution of their inquiries.11 Colonial developments would have
been impossible without improved communications systems, as nineteenth-century statesmen understood: the telegraph, increasingly efficient sea and road transport, and newly laid railways both facilitated and followed pacification of large portions of the globe by European powers. And with the subjugation of territories came alterations in fieldworkers’ experiences as well as appreciation of their labors. Certainly, frontier conditions did not disappear overnight. Often, an enlarged colonial presence exacerbated intergroup hostilities; in the early days of formal colonial rule, officials’ authority could be precarious, and scientific investigations could be quasi-military operations.12

In effectively pacified places, however, European visitors anticipated little difficulty in satisfying their needs for local assistants of various types, ranging from local informants and translators to servants, since they could rely on the authority of the colonial regime—threatened or actual—to secure the personnel they required.13 (Note that the end of colonialism has to a degree reinstated the status quo ante in once-subject territories, which have imposed various constraints on foreign field scientists.)14 Pacified colonies became important workplaces for all sorts of naturalists, if not the only significant sites. Sociocultural anthropologists were distinguished by their preference for them. (Anthropologists, easy access to exotic places was loosely associated with conceptual change, and exclusive attention to those peoples who were once unabashedly termed “primitive” or “savage”—including subjects of internal colonialism, such as Irish countrymen and Native Americans—could be justified as the best way to identify human universals from observed particulars.)15 In colonial places, diverse types of scientists anticipated that they

13 For example, when Margaret Mead and her then-husband Reo Fortune were unable to staff their New Guinea household in 1932, Fortune uncovered the local people’s “darkest secrets” and threatened to reveal his findings to colonial authorities unless potential servants came forward; described in Margaret Mead, letter of 15 Jan. 1932, reproduced in her “Field Work in Pacific Islands, 1925–1967,” in Historical Review of Anthropology, 2005, 18:11–24, esp. p. 15; and Andrew D. Evans, Anthropology at War: World War I and the Science of Race in Germany (Chicago: Univ. Chicago Press, 2010), Ch. 4. On militarized North American frontier science see Thomson, Legacy of the Mastodon, p. 175.

14 These constraints have caused much distress among anthropologists, who have (mistakenly) believed that they alone are restricted as punishment for their discipline’s colonial taint. For anxieties that anthropology has been a colonial creature see Talal Asad, ed., Anthropology and the Colonial Encounter (London: Ithaca, 1973). For other field scientists bound by official restrictions see Kate Jackson, Mean and Lowly Things: Snakes, Science, and Survival in the Congo (Cambridge, Mass.: Harvard Univ. Press, 2008); and Paige West, “Tourism as Science and Science as Tourism: Environment, Society, Self, and Other in Papua New Guinea,” Current Anthropology, 2008, 49:597–626, esp. pp. 603–604.

15 The quest for universals was especially important to the British; for their generic argument see Godfrey Leinhardt, Social Anthropology (London: Oxford Univ. Press, 1964), p. 2: “The fundamental features of social institutions will be more apparent . . . in ‘primitive’ (sic) societies] than in modern metropolitan communities.” For other testimonials to the centrality of the exotic subject see Evon Z. Vogt, Fieldwork among the Maya (Albuquerque: Univ. New Mexico Press, 1994), p. 52; and Sidney Mintz, “Sows’ Ears and Silver Linings,” Current Anthropology, 2000, 41:169–189, esp. p. 171. Until the 1980s, anthropologists who worked in nonexotic places courted professional disaster; see Suzanne R. Kirschner and Emily Martin, “From Flexible Bodies to Fluid
might become famous after reporting the novel phenomena they could expect to see. Sometimes, those who came for other reasons developed scientific interests that led to careers at home; more often, recently trained scientists found positions that were stepping-stones to metropolitan careers (an occupational path associated with the development of routinized scientific careers in the metropoles). Scientists were, in fact, just one among the many social types who enjoyed more opportunities for self-promotion and self-aggrandizement in colonies than were available at home.

The diverse expatriate types attracted to colonial places—including (but not limited to) commercial figures, missionaries, and naturalists—could support one another’s endeavors. For example, when the future anthropologist A. C. Haddon, then professor of zoology at the Royal College of Science in Dublin, determined that he should make a year-long research trip to an exotic locale to study marine biological phenomena similar to those he had observed in Irish coastal waters, his personal contacts were critical to his decision to go in 1888 to the islands of the Torres Strait, near mainland Australia, then a British colony; his wife’s family had a friend who had until recently been stationed at the London Missionary Society’s outpost there, and Reverend Macfarlane advised Haddon as to how he could set about living and working there and wrote him letters of introduction to people who could give him practical assistance. (When Haddon returned to the Torres Strait a decade later, in his anthropological incarnation, missionaries would object when he mobilized islanders to stage traditional ceremonies that they wished to suppress; such conflicts between anthropologists and missionaries were to become routine.)


Colonial opportunities for extravagant self-reinvention, demonstrated by the likes of Henry Morton Stanley (1841–1901), are well known. Just as significant is the colonial element in upward mobility among the upper-middle classes; see Noel Annan, “The Intellectual Aristocracy,” in *The Dons* (Chicago: Univ. Chicago Press, 1999), pp. 304–341. Persons fairly low on the social scale also found opportunities in colonial places, although scholars have paid little attention to them. For example, at the turn of the twentieth century the Torres Strait Islands were places of refuge from desperate circumstances in Glasgow for the Bruce brothers—Robert, a shipbuilder and wheelwright, and J. S. (Jack), a schoolmaster who became an important informant for A. C. Haddon—although Robert suffered from downturns in the islands’ economy, dependent on the international pearl trade; see their letters to Haddon, dated 1894–1905, Haddon Papers, Cambridge University Library, Envelopes 1004, 1006.


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the German-born and -trained future leader of American anthropology, had a similar experience. Working on Baffin Island in 1883–1884, when he was primarily in pursuit of geographical information, he relied on a Scottish whaler, Jimmy Mutch, who had been on the island since 1866 and was married to an Inuit woman. And the rise of American paleontology to international preeminence by 1900 was considerably assisted by agents of transcontinental economic expansion, such as employees of the American Fur Company and various railroad companies.19

As the trials of Karl Mauch indicate, scientists working under frontier conditions could not survive (either literally or in their research capacities) without the assistance of bearers of diverse forms of local knowledge. Other narratives convey the same message in less sensational fashion. Consider the first American attempts to establish a boundary between the United States’ Oregon Territory and Canada, made in 1859; the 1866 report of the official survey party noted that “almost all the first information of the topographical features was also obtained by & from Indians.” Likewise, the young Boas mapped Baffin Island with the aid of geographical intelligence elicited from Inuit informants.20 But fully established colonial societies became inward looking—a development usually associated with conditions permitting the installation of European nuclear families, who led lives approximating (at least to some degree) those they would have expected to sustain at home.21 At this juncture, the extent to which scientists availed themselves of locals’ knowledge of the natural world depended on a range of factors—from scientists’ ability to substitute already-routinized expertise for their own observations to their personal attitudes and circumstances—although it has remained in the self-interest of figures ranging from ornithologists to sociocultural anthropologists to “develop natural resources, so to speak,” as the anthropologist Sol Tax put it in 1934.22

Also critical to the closing of the colonial frontier was the development of tropical medicine. Practitioners of this new specialty were notably effective in implementing public health measures; for example, they reduced the incidence of insect-borne diseases by eliminating or treating the pools of stagnant water in which insects bred, following the procedures devised by Ronald Ross, who won the Nobel Prize in Medicine in 1902 for


21 By this standard, the elimination of interracial sex from the behaviors (tacitly if not explicitly) acceptable in official colonial circles marks the closing of the colonial frontier, and it has a date in the British Empire: in 1909, the Secretary of State for the Colonies, Lord Crewe, whose jurisdiction was all British colonies save those managed by the India Office or the Foreign Office (only the Sudan), issued an order saying that colonial employees who had liaisons with indigenes would suffer “disgrace and official ruin.” The secret memorandum, Confidential Enclosure “A” in Crewe’s circular of 11 Jan. 1909, is unavailable in the British National Archives, but the Oxford University Colonial Records Project, Rhodes House Library, Oxford, holds a copy.

working out the mosquito–human vector in malaria transmission. (Physicians were less successful in developing and promoting remedies that were safe and effective. Although quinine had been used for centuries, at the turn of the twentieth century an exotic traveler’s medicine chest was also likely to contain substances like arsenic, as Bronislaw Malinowski’s did at the time of World War I.) Although it took some time for new scientific judgments to become conventional wisdom, changes in medical opinion, as well as the implementation of practical sanitary measures, evidently made life and work in the tropics more appealing to diverse colonial types.

Finally, there were significant changes in metropolitan institutions of higher learning. Universities added subjects to their curricula, including the differentiated natural history sciences, which became the bases of newly defined academic careers. (The work of laboratory-based scientists, classicists, and philosophers—indeed, of practitioners of knowledge-based occupations in general—also changed in the late nineteenth century, but I will say no more along these lines; comprehensive comparison is not the object of this essay.) Professional naturalists became fieldworkers. Among the vanguard were marine biologists, one of whom explained the necessity of fieldwork metaphorically: “the idea of learning biology proper in a laboratory or a museum is as preposterous as the idea of learning navigation from a toy ship on a mill pond.” Programs of field-based research were facilitated—if not necessarily fully realized—with the creation of marine biological stations. The first of these, founded in 1859 in Concarneau in Brittany, heralded an efflorescence of European stations: ten were founded in the 1870s (including the conspicuously successful Naples Zoological Station), twenty in the 1880s, and eleven in the 1890s. In the United States, particularly notable were two Massachusetts institutions: the short-lived summer school on Penikese Island in Buzzard’s Bay, run by the brothers Louis and Alexander Agassiz, and the still-operating Marine Biological Laboratory founded fifteen years later, in 1888, at Woods Hole (then Woods Holl).

Scientists resident at marine stations did not routinely immerse themselves in unimproved nature: they lived and worked conveniently near their live subjects but usually relied on organized teams of fishermen to stock their laboratory tables; and their specimens were sometimes cultivated rather than caught in the wild. But the division of labor between scientists and collectors was neither absolute nor invariant: for example, at the Naples station, where the emphasis was on laboratory research, each scientist was furnished with a diving suit that enabled him to accompany fishing expeditions and see his...
creatures in situ; at Wimereau, near Lille, observations in the field were of paramount importance. And at least some visitors to Naples were inspired to forsake laboratories to examine aquatic life in natural habitats: A. C. Haddon, for one, who had spent six months there following his graduation from Cambridge with a First in Natural Sciences in 1879, determined that he must make a marine biological field trip because the scientist without firsthand investigative knowledge was “retailing second-hand goods over the counter.”

Academic scientists such as Haddon enjoyed advantages not available to amateurs who did science in their spare time. The university calendar allowed for research at periodic intervals—during summer vacations—such as that undertaken by the Oxford-trained Baldwin Spencer during the Australian summer of 1896–1897, when he, the University of Melbourne’s foundation professor of biology, worked in Central Australia in collaboration with the civil servant F. J. Gillen; their findings yielded The Native Tribes of Central Australia (1899), arguably the first approximation of a modern anthropological monograph, which was celebrated in its day as a realization of the possibilities of fieldwork method and reached an international audience. Academics were also well placed to secure financial support from government and private philanthropies, as Haddon did when he organized the 1898 Cambridge Anthropological Expedition to Torres Strait, a challenge to the viability of armchair scholarship that constituted a move toward the professionalization of anthropology, taking British academics to do field research; when Haddon and his research team left Britain on 10 March 1898, the Times of London observed, “Travelling scholars are often sent out by Oxford and Cambridge but this is probably the first time that a real exploring expedition has been sent out by either University.”


27 A. C. Haddon, quoted in A. Hingston Quiggin, Haddon, the Head Hunter (Cambridge: Cambridge Univ. Press, 1942), p. 77.

28 For an account of the international debate in which Spencer and Gillen’s work was central see Kuklick, “‘Humanity in the Chrysalis Stage’” (cit. n. 18). Bronislaw Malinowski reported on Spencer’s fieldwork prowess in his letter to C. G. Seligman, 4 May 1915, Malinowski Papers, London School of Economics and Political Science (LSE), File 27/3 (hereafter rendered in the form MP 27/3). On the general point that academic scientists could do research during their summer vacations see Kohler, Landscapes and Labscapes (cit. n. 25), pp. 82–85.

29 Quoted in Sandra Rouse, “Expedition and Institution: A. C. Haddon and Anthropology at Cambridge,” in Cambridge and the Torres Strait, ed. Herle and Rouse (cit. n. 18), pp. 52–76, on p. 61. The expedition’s research was conducted from late April through mid-November. The pioneering academic anthropological field study may seem to be the Jesup North Pacific Expedition of 1897–1902, mounted by the American Museum of Natural History and organized by Franz Boas, which plotted human and other connections between the American and Siberian sides of the Bering Strait. But it was episodic, not sustained, and less focused intellectually than the Cambridge expedition; see Douglas Cole, Franz Boas: The Early Years, 1858–1906 (Seattle: Univ. Washington Press, 1999), pp. 188–203.
The reorganization of naturalists’ work facilitated the reconceptualization of evidence. When collectors worked in the field to satisfy the demands of the armchair scholars who purchased their finds, they had considerable incentive to identify new species: these had high market value. Indeed, vendors of natural specimens had “a positive prejudice against intermediate forms,” as Charles Lyell observed in 1867. Among professional naturalists, however, the prestige attached to discovering new species declined, and the appeal of intermediate forms grew. Such forms documented theoretical positions, the currency of intellectual trade: field-going naturalists were prominent among those biologists who embraced Darwinism, finding associations between variations in life forms and geography and explaining their observations as testimony to the operation of natural selection. Whatever the origin of their scientific perspective, it was surely reinforced by their experiences: traveling naturalists could not ignore concomitant variation in land and life forms. Fieldworkers also identified fewer species after they observed variability within breeding populations (consistent with Darwinian argument). Finally, the biogeographical approach compatible with taxonomic schemes that admitted of heterogeneity, yielding classificatory lumping rather than splitting, suited fieldworkers who were employed by museums—more important loci for the development of professional careers in the late nineteenth century than they were to be later—because museum displays could easily be (although they were not necessarily) organized along geographical lines.

Consider the intellectual framework used by the fieldworking biologists-turned-ethnologists Baldwin Spencer and A. C. Haddon, whose careers were linked. Both

30 Charles Lyell to Wallace, 2 May 1867, Wallace Papers, British Library, Additional Manuscript 46435. See the 11 July 1867 letter from J. Aspinall Turner (once Stevens’s customer) to Wallace, stipulating that he would pay Wallace £50 for an insect collection only if it enlarged his inventory of species: Wallace Papers, British Library, Additional Manuscript 46435.

31 There is still prestige attached to identifying a new species, evidenced by the honor of naming it that routinely accrues to the discoverer, but a potential recipient of this honor may now willingly forego it. For example, employees of the Wildlife Conservation Society who identified a new species of Bolivian monkey allowed the right to name it to be auctioned to fund conservation measures; see Henry Fountain, “Observatory: Have Your Very Own Species for a Price,” New York Times, 8 Feb. 2005, p. F3.

32 For a record of one traveling naturalist’s observations see Joseph Dalton Hooker, Himalayan Journals; or, Notes of a Naturalist, in Bengal, the Sikkim and Nepal Himalayas, Khasia Mountains etc. (London: John Murray, 1853). On the association between Darwinian argument and biogeographical reasoning see, e.g., Ernst Mayr, One Long Argument (Cambridge, Mass.: Belknap, 1991), p. 132.


35 Spencer bested Haddon in competition for Melbourne’s biology professorship. After Haddon became Cambridge’s first appointment in ethnology, he gave Spencer anthropological advice; see, e.g., A. C. Haddon to Baldwin Spencer, 5 May 1902, Spencer Papers, Box I, Pitt Rivers Museum, Oxford. Unlike Haddon, Spencer
stumbled into anthropology—at the historical moment when armchair scholars had grown suspicious of the reports of casual travelers and called for rigorous inquiries.36 When Haddon did his zoological work in the Torres Strait Islands, he pursued anthropology as a sideline, both because he intended to supplement his research grant by collecting artifacts to sell to museums and because he had agreed to answer questions posed for him by J. G. Frazer, an archetypal armchair anthropologist, as well as by the sociologist Edward Westermarck (of whom more shortly); when he returned to the Torres Strait in 1898, he was gambling that he could make a career in the still-nascent discipline of anthropology, notwithstanding the caution issued by Thomas Huxley, his referee in his many (largely unsuccessful) job applications, that employment prospects were dim in his projected field—despite the abstract merits of anthropological inquiry (which Huxley himself pursued).37 Similarly, Spencer was the zoologist in the Horn Expedition when he made his first trip to Central Australia in 1894; in the field he met Gillen, and the two formed an anthropological partnership that lasted until Gillen’s death in 1912. Most important, Spencer and Haddon transferred the analytic perspective that informed their zoological research to their anthropological work. It did not matter whether they were analyzing giant earthworms (of special interest for Spencer), sea anemones (Haddon’s enthusiasm), or human societies. They plotted variation along geographical axes, consistently understanding variations in life forms in ecological terms and observing that geographical isolation was an important factor in speciation. As a marine zoologist, Haddon belonged to a population that had been obsessed with geographical distribution since the second third of the nineteenth century, but Spencer was no less committed to a biogeographical approach.38

Field-going anthropologists did not have to begin their careers as Darwinian biologists, as Spencer and Haddon did, in order to conceptualize cultural variation biogeographically. When the young Franz Boas, trained in physics and geography, went to Baffin Island, he had expectations similar to Haddon’s and Spencer’s. Moreover, twenty-five years after his first fieldwork, Boas would attribute the enduring “scientific success of the Cambridge Torres Strait Expedition” to its conceptualization, arguing that one should anticipate that a population’s “isolation” would be associated with its preservation of “an older type” of culture. Unlike Haddon and Spencer, however, Boas did not understand cultural variation as environmental adaptation. As his student Alexander Lesser observed, Boas judged that geographical factors explained behavior only “in trivial and shallow ways” and that people often did things “in spite of” geographical conditions.39

Fieldworkers in the diverse natural history sciences embraced similar research designs and objectives. Many agreed that extrapolation to general principles required sustained

36 Charles Hercules Read, for example, urged travelers to confine themselves to taking photographs or “making careful drawings,” since “even superficial answers” to questions about social relationships required “long-continued residence among a native race”: Read, “Prefatory Note,” in Notes and Queries on Anthropology, 2nd ed. (London: Anthropological Institute, 1892), p. 87.

37 Huxley not only wrote anthropological analyses but also figured prominently in organized anthropology. On Haddon’s career see Henrika Kuklick, “Islands in the Pacific: Darwinian Biogeography and British Anthropology,” Amer. Ethnol., 1996, 23:611–638.

38 See Rozwadowski, Fathoming the Ocean (cit. n. 26), p. 115ff.

and close attention to specific situations in circumscribed locations, where the possibilities of variation were naturally bounded. When sociocultural anthropologists turned away from surveys of human possibilities across all times and places, their ideal research project became the “detailed study of a single tribe or natural assemblage of people”—Haddon’s 1890 description of his plan for the Cambridge expedition.\(^{40}\) In 1897, a method of close ecological analysis of the life forms of strictly delimited geographical areas was contrived in Nebraska by Frederick Clements and Roscoe Pound (Clements’s former student and then supervisor, who would apply biological reasoning to legal issues and become dean of the Harvard Law School; his legal theory arguably influenced sociocultural anthropology). British ecologists shortly adopted methods similar to Americans’, investigating circumscribed natural communities. Early twentieth-century Austrian climatologists cultivated particular local knowledge in order to recognize the operation of general phenomena. In sum, the detailed study of a limited area became a significant research approach in enterprises as apparently unlike as anthropology and oceanography, although it became overwhelmingly important in anthropology.\(^{41}\)

Readers of fieldworkers’ accounts of transient phenomena in remote places confronted the issue that had long troubled armchair scholars: How were they to assess the accuracy of information they could not inspect directly? In part, a narrative of the development of a reliable scientific fieldworker could be extrapolated from the long-established understanding of nature study as a moral exercise, a means to spiritual growth. It was also important that, from the end of the eighteenth century, high-minded (and prosperous) tourists had sought enlightenment through travel: travelers expected to be spiritually uplifted while contemplating the beauties of picturesque scenery. This was an experience understood as equivalent to other types of exalted aesthetic pleasures. For example, it made sense to Thomas Huxley to say that he “felt as if [he were] listening to beautiful music” while he was enjoying drawing the landscapes he observed as assistant surgeon on the H.M.S. \textit{Rattlesnake}, which cruised the Pacific between 1846 and 1850. Indeed, the process of learning to draw from nature was believed to be transformative. As Edwin Lees asserted in 1838, drawing “functioned as ‘a moral engine that [led] to habits of accurate observation.’”\(^{42}\)

Enhanced regard for fieldwork as moral education also derived from a new, Victorian-era mind-set: the view that personal growth (of an implicitly masculine sort) was effected


through pilgrimages to unfamiliar places, where the European traveler would endure physical discomfort and (genuine or imagined) danger. The characteristics of fieldwork that had once made it dirty work now made it a purifying ordeal. Scientific inquiry accomplished by daring exploits was exemplified by the Alpine adventures of the polymath John Tyndall in the second third of the nineteenth century. Because Tyndall could not gather the information he wanted with the instruments he carried, he made his body into a research tool, perilously edging his way over deep glacier crevasses in order to prove that glaciers moved in their beds as solid objects. And for persons such as Tyndall, the assumption that outdoor physical activity builds moral character justified the expectation that the rigors of the field would inculcate the personal discipline required to make scientists reliable.

Consider what C. G. Seligman observed in 1912 to Bronislaw Malinowski, then his protégé at the London School of Economics and the person an international population of sociocultural anthropologists still credit with setting their enterprise’s methodological standard: “field research in anthropology is what the blood of martyrs is to the Church.” Heroic fieldwork was distinguished from the leisure-time activities, ranging from museum visiting to field club participation, that became widely available to the European and American middle classes with the expansion of the railroads in the latter half of the nineteenth century, as well as with the development of the bicycle and the automobile at the end of the century. To late nineteenth- and early twentieth-century naturalists, then, “collecting close to home, however productive, could seem tame, and too much like amateur collecting,” as Robert Kohler has observed.

Nevertheless, the location of fieldworkers’ research did not by itself suffice to establish their reliability. An apparently promising remote field site could be judged insufficiently wild to lend itself to the production of good science. Consider the reception of the findings of the self-taught American primatologist Richard Lynch Garner, who went to French Equatorial Africa in 1892 to station himself in a protective cage in the forest in order to listen to what he conceptualized as primates’ rudimentary language: upon his return home, he was accused of contriving his results “in comfortable quarters at a mission station.” But features of Garner’s personal history—that he both lacked academic credentials and courted (and achieved) popular fame, performing in such venues as Madison Square Garden—played an important role.

43 David Robbins, “Sport, Hegemony, and the Middle Class: The Victorian Mountaineers,” *Theory, Culture, and Society*, 1987, 4:579–601 (on personal growth through travel to unfamiliar places); and Bruce Hevly, “The Heroic Science of Glacier Motion,” *Osiris*, 1996, 2nd Ser., 11:66–86. Tyndall’s findings were also relevant to debate about whether nature revealed either the steady operation of identifiable processes or the occasional intervention of a willful deity; see Lyell to Wallace, 24 Mar. 1869, Wallace Papers, British Library, Additional Manuscript 46435. When women became fieldworkers, they were expected to behave as if they were men. See, e.g., Malinowski’s letter to his student Audrey Richards, then doing research in Northern Rhodesia; writing that she “always err[ed] on the side of softness and consideration,” Malinowski suggested that she permit herself occasional confrontational behavior: Malinowski to Richards, 6 Jan. 1931, Richards Papers, London School of Economics and Political Science, File 17/3.


Garden—surely explain the criticism he received.47 The accuracy of the charge against Garner is less meaningful than its form: the evidence he purveyed could not be believed because he had not suffered in acquiring it. Naturalists who worked in sites that were relatively easy of access gained credibility by stressing that their knowledge was hard won. For example, the ornithologist Edmund Selous demonstrated his seriousness of purpose (and presumably his superiority to rank amateur birdwatchers) by emphasizing that his observations of the mating habits of the ruff in Holland in 1906 necessarily mired him in “wretchedness, cold, and discomfort.”48 Significantly, scientists of this era whose work never took them outdoors, such as the pioneers of radiology, welcomed the afflictions that were the occupational hazards of their day, understanding them as analogues to suffering in the field and, as such, as testimonials to—and productive of—moral and scientific virtues.49 Moreover, the heroic fieldwork ideal has proven remarkably durable. Certainly, field scientists have built formidable reputations with research conducted in safe places, but evidence of having endured danger and discomfort for the sake of one’s work still testifies to authentic witnessing for many species of fieldworkers and may be especially important for those who—for whatever reason—aspire to public attention.50

Because fieldwork was understood to test and build character, it is not surprising that its practitioners were inclined (and permitted by their audiences) to develop deliberately personal investigative styles, as opposed to the ostensibly affectively neutral style normative in laboratory research. Empathy became and remains an essential investigative skill. Consider, for example, that in 1841 James David Forbes, professor of natural philosophy at Edinburgh and an Alpine fieldworker, observed that repudiation of conventional wisdom about glaciers required “a protracted residence amongst the Icy Soli-
tudes which imbues one truly with their spirit [and] enables one to reason confidently concerning things so widely rumored from common experience.” In 1893, the professional anthropological attitude that is conventionally termed “rapport” was prescribed in a disciplinary foundational moment, when A. C. Haddon pronounced that it was essential to understand “native actions . . . from a native and not from an European point of view.”51 Contemporary naturalists still undertake inquiries with the objective of understanding their subjects on their own terms. Indeed, the professional creed of ethologists is to develop such a strong sense of emotional connection to their observed animals that they feel they have become those animals. A student of killer whales, for example, recently described “the challenge of having to think like a killer whale . . . having to strip your biases as a terrestrial, visually based mammal.”52 Sociocultural anthropologists are distinctive, however. Unlike ethologists, whose objective of recording natural behavior requires invisibility (or a close approximation thereof) in the field, anthropologists’ enduring commitment to the method of participant observation—perpetuated even as their definition of “the field” has been renegotiated—means that they must “interact personally and socially with informants,” as Harold Conklin remarked, and cannot maintain distinct private and work lives while doing research.53

Sociocultural anthropologists’ affective relationships with their informants have had various consequences. In colonial settings, anthropologists who identified closely with the people they studied were often thought to be subversives (in thought, if not necessarily in deed): a far from exhaustive inventory of objects of official disapproval includes Northcote W. Thomas in Nigeria and Sierra Leone before World War I; Ralph Piddington, Olive Pink, and Ronald and Catherine Berndt in Australia in the 1930s and 1940s; and Max Gluckman in Northern Rhodesia in the years around World War II.54 But personal interactions between anthropologists and informants have not been invariably (or consis-


tently) pleasant—or even tolerable—for any one or (conceivably) all of the parties to them. Nor have unhappy relationships been confined to oppressive colonial situations. For example, in 1960–1961, when Jeremy Boissevain worked in a village in Malta, then poised to become a full member of the British Commonwealth, villagers worried that he was “remarkably well informed about matters they thought no nonvillager should know” and were concerned “to determine for which country [he] was spying.” The fictional Berkeley anthropologist in Mischa Berlinski’s recent novel *Fieldwork*, dismayed to find that she did not like the people she chose to study, represents one dimension of the sort of relationship Malinowski had with the Trobriand Islanders, who enraged him intermittently.55 (See Figure 1.) Malinowski himself commented that what anthropologists are able to learn is some function of the social roles that they are inevitably assigned by their research subjects, urging the fledgling fieldworker to anticipate “the [traditional customary] status in which you will probably be placed”—which at that moment in the history of anthropology also entailed his recommendation to try to “associate yourself with [the] best category [of European] for your purposes.”56 In sum, though the specific observational positions of all types of scientific fieldworkers surely affect their findings, anthropologists have been highly sensitized to the role of their own social status, and have frequently been deeply disturbed by the nature of their interactions with local residents.


56 The European social roles Malinowski identifies in his “Outline for Chapter on Field Techniques” are those with which non-Westerners were likely to have been familiar—“government official, missionary, school teacher, trader”: unpublished and undated typescript, MP 23/26; its presence in the LSE archives dates it before 1939, when Malinowski departed for Yale (taking only some of his papers with him).
pologists’ interpersonal relationships have been critical to their research—which is to say that different sorts of relationships can elicit different findings, not that conclusions should be dismissed as mere epiphenomena of observers’ perspectives."57

THE PECULIARITIES OF THE ANTHROPOLOGISTS

If sociocultural anthropological method has much in common with the field practices of other natural history sciences, it also has distinctive characteristics, to which I now turn. What eventually became the methodological standard for fieldworkers everywhere was arguably defined by British anthropologists. Their premier theorist was W. H. R. Rivers, who came to anthropology through his participation in the 1898 Cambridge Anthropological Expedition to the Torres Strait. At that time, the medically trained Rivers was Cambridge University’s lecturer in experimental psychology and the physiology of the senses, renowned for the rigor of his experiments; it was quite a coup for the professionally insecure Haddon to enroll him in his expedition, which was planned as a vehicle for elevating the scientific status of anthropology, not least because its members would create a laboratory in the field. Haddon repeatedly asserted that “investigations in experimental psychology in the field were necessary if we were ever to gauge the mental and sensory capabilities of primitive peoples” and observed that Rivers had to be credited with “contribut[ing] more than anyone else to ... the raising of Ethnology to the status of a science.”58

The Torres Straits Expedition was team research, conducted by seven men, each of whom undertook investigation of portions of the collective project.59 But after the expedition’s work had ended, Rivers proclaimed that its organization and objectives had been irreconcilable. The most influential of his prescriptive statements was his contribution to the 1912 edition of Notes and Queries on Anthropology, a book-length questionnaire that was one iteration of a recurrent publication; originally addressed to travelers, directing them to acquire information armchair scholars wanted, it became in the 1912 edition a guide for fieldworkers, Malinowski among them. Rivers allowed that the Torres Straits Expedition had visited the right sort of field site: anthropologists should study a “com-

57 Lise Dobrin and Ira Bashkow have provided a compelling example—the different conclusions Margaret Mead and Reo Fortune derived from their work among the Mountain Arapesh in Papua New Guinea, not least because Mead’s bad ankle confined her to the village that was their headquarters during the eight months they were based there, while Fortune both moved about and was temperamentally inclined to different sorts of interactions; see Dobrin and Bashkow, “‘Arapesh Warfare’: Reo Fortune’s Veiled Critique of Margaret Mead’s Sex and Temperament,” American Anthropologist, 2010, 112:370–383. Alma Gottlieb observes that ethnography is predicated on the recognition that “data are not just gathered like grapes on a vine but are also created by human effort” and “that scholars who ‘produce data’ are complex creatures whose perceptions and communications are shaped at every turn by the context in which they find themselves”; see Gottlieb, “Ethnography: Theory and Methods,” in A Handbook for Social Science Field Research, ed. Ellen Perecman and Sara R. Curran (Thousand Oaks, Calif.: Sage, 2006), pp. 47–84, on p. 48. Gottlieb is tacitly addressing those tempted to dismiss observation as epiphenomenal, as many were in recent decades.

58 A. C. Haddon, Headhunters: Black, White, and Brown (London: Methuen, 1901), p. viii; Haddon is quoted in Peter Gathercole, “Cambridge and the Torres Straits, 1888–1920,” Cambridge Anthropology, 1976, 5:22–31, on p. 27. At the time he organized the expedition, Haddon still held the chair in zoology in Dublin, but his teaching required him to be there for only four months of the year, and so since 1893 he had been living in Cambridge for the rest of the year and lecturing on physical anthropology on an authorized freelance basis, collecting fees directly from students; see Rouse, “Expedition and Institution” (cit. n. 29), pp. 52–55.

59 The expedition team also included C. G. Seligman, a physician who became an anthropologist; the psychologists Charles Meyers and William McDougall, Rivers’s former students; Sydney Ray, an expert on languages who worked as a primary school teacher; and Anthony Wilkin, a recent veteran of Haddon’s Cambridge undergraduate lectures, who died in 1901 while working on an archaeological dig.
munity of perhaps four or five hundred people” that was “more or less sharply marked off from surrounding peoples,” such as “the inhabitants of an island or distinct geographical district,” but he argued that, for the sake of scientific rigor, anthropologists must abandon team research and become solitary participant observers. Why was this shift not seen as retrograde, since a team could engage in the acts of dispute and corroboration that have conventionally been seen as the foundation of scientific method?

Rivers insisted that the deployment of a survey team bent on rapid results was counterproductive. Describing what is now known as the “anthropologist effect,” he stated that “the temporary visit of a set of strangers among a people of rude culture” induced “such a condition of excitement” as to disrupt the normal behavior anthropologists wished to observe. (Note that Rivers was transferring to the field a perennial methodological concern of laboratory psychologists; he also worried about experimentalists’ influence on their subjects.) Indeed, the expectation that a survey team could work efficiently was unrealizable: because all elements in primitive societies were linked, members of teams would find themselves working at cross-purposes when they attempted to divide research into discrete tasks. Anthropologists must abandon teamwork along with research projects of the “extensive” variety. A lone generalist should do “intensive” research, living “for a year or more” among a people, mastering the vernacular and developing a personal relationship with each member of the population. And probably not least because it was readily appreciated as heroic experience, the intensive study accomplished through solitary fieldwork—often operationalized as research by a married couple—became the ideal, notwithstanding some notable departures from it, including Rivers’s own.

W. H. R. Rivers, in Notes and Queries on Anthropology, ed. J. L. Myres and Barbara Freire-Marreco (London: Royal Anthropological Institute, 1912), p. 143. Published jointly or separately by the British Association for the Advancement of Science and the (from 1907 Royal) Anthropological Institute of Great Britain and Ireland, the manual’s original title was Notes and Queries on Anthropology for the Use of Travellers and Residents in Uncivilized Lands, and it was issued in 1874, 1892, 1899, 1912, 1929, and 1951.


Why were the observations of a lone anthropologist more scientific than those of a research team? Rivers argued that the anthropologist’s body served as a reliable measuring instrument. I stress that Rivers’s conceptualization of the anthropologist’s body was identical to his model of the Torres Strait Islander’s body, some characteristics of which were identified through a feature of the expedition’s work that Rivers judged positively: the psychological tests administered by members of the research team, under his own supervision.63 These demonstrated that the islanders’ senses of hearing, touch, smell, and vision differed little from those of European experimental subjects. Absent test results, the researchers might have imagined that islanders were the savages of legend, acutely responsive to all manner of sensory stimuli and by this token closer to a lower animal state than Europeans—humankind in embryonic form, as it were. But testing proved that islanders’ remarkable observational powers were learned, not innate—the products of long-established habits of close attention to those features of their habitat that figured in their mode of subsistence. Islanders’ habits fit the pattern characteristic of primitive peoples, made up of the adaptive responses required to survive in unimproved nature. Indeed, those islanders who had relatively high exposure to the civilization brought by colonialism—young people, as well as those who lived on the Torres Strait islands that were relatively close to Australia—had nearly modern body economies, with relatively limited powers of utilitarian sensory attention. Significantly, traditional islanders did “not take the same aesthetic interest in nature which is found among civilized peoples”—critical evidence of their impoverished intellectual development and generally low cultural condition. They were endowed with the capacity for higher mental functions, but because they had to expend so much of their energy “on the sensory foundations” of sheer survival, it was “natural that the intellectual superstructure should suffer.” When comparable research was subsequently done in Egypt and India, Rivers’s judgment was reinforced: “The general conclusion which may be drawn from the available evidence is that pure sense-acuity is much the same in all races in the absence of definite pathological conditions.” Note that Franz Boas wrote in 1911, “Primitive man . . . is not in the habit of discussing abstract ideas. His interests center around the occupations of his daily life.”64 And the judgment that biological and cultural differences were not covariant called for a separation of the investigative tasks of the physical and the sociocultural anthropologist—even if, as in Boas’s case, one person might undertake both types of research.

Exercises in anthropological psychology substantiated the physiological model that
informed Rivers’s and Haddon’s other scientific work. Torres Strait Islanders exhibited qualities induced in European experimental subjects whose judgmental and sensory abilities were deliberately reduced through either surgery or drugs, making them rather more physical than rational beings—subjects who included Rivers himself, as well as his collaborator and friend Henry Head.\textsuperscript{65} Ultimately, while serving as a military psychiatrist during World War I, Rivers would interpret as degeneration on a behavioral evolutionary scale the functional psychological disorder that was named “shell shock” by his former student and associate in the Torres Straits Expedition Charles Myers, who served as consulting psychologist to the British Army in France: shell shock sufferers, traumatized by the struggle to survive on the battlefield—the most perilous of environments—presented various characteristics similar to traits of primitive peoples. Their condition demonstrated a general principle: “in morbid states early instinctive modes of reaction tend to reappear.” Most important, Rivers stressed that, given sufficiently stressful circumstances, anyone could succumb to shell shock, regardless of either his hereditary endowments or his accumulated experiences.\textsuperscript{66}

In sum, various forms of evidence, produced both fortuitously and experimentally, indicated that the anthropologist who immersed himself in an exotic culture could serve as a reliable research instrument. In the \textit{Reports of the Cambridge Anthropological Expedition to Torres Strait}, Rivers provided anecdotal evidence consistent with the investigative approach he later formulated: the experience of K. E. Ranke, a German specialist in tropical medicine who was also an anthropologist and an active participant in the German exploration of South America. Ranke had recently returned from a sojourn among Brazilian indigenes, during which he imitated their behavior and found himself a changed person. Becoming, of necessity, consumed by attention to the minute particulars of his natural surroundings, he “lost his capacity for the aesthetic enjoyment of scenery” and for contemplation of “the more serious problems of life”; nevertheless, he retained his ability to understand his transformation.\textsuperscript{67} But though Ranke may have been the prototype for Rivers’s ideal-typical anthropological fieldworker, his fortuitous mode of inquiry fit a pattern long familiar in physiological-psychological circles in Europe and America—that of the sympathetic observer, who could reproduce in himself the emotions of the person he wished to understand by imitating that person’s postures, gestures, and facial expressions.\textsuperscript{68} Although Rivers assessed Torres Strait Islanders’ capacities using some of the new methods that were transforming psycho-physiological inquiry in the late nineteenth

\textsuperscript{65} Perhaps because he questioned the results of previous research on alcohol’s effects, Rivers experimented on himself, concluding that alcohol induced behavioral degeneration—increased muscular strength and decreased intellectual capacity; see Rivers, \textit{Influence of Alcohol and Other Drugs on Fatigue} (cit. n. 61), p. 100. In the famous Rivers-Head experiments of 1903–1908, the nerves in Henry Head’s left forearm were surgically severed in 1903; Rivers administered tests for the next five years. Supposedly, Head’s injury effected a regression to primitive sensibility, and his (incomplete) nerve regeneration recapitulated evolution. For one summary see Ian Langham, \textit{The Building of British Social Anthropology} (Dordrecht: Reidel, 1981), pp. 57–60.


\textsuperscript{67} Rivers, in \textit{Reports of the Cambridge Anthropological Expedition to Torres Straits}, ed. Haddon, Vol. 2 (cit. n. 64), p. 70. Ranke was the son of Johannes Ranke, the occupant of the first (and for decades the only) full chair in anthropology in Germany (in Munich), a political liberal allied with Rudolf Virchow, who argued that human physical variation had no relation to psychological or cultural variation. See Benoit Massin, “From Virchow to Fischer: Physical Anthropology and ‘Modern Race Theories’ in Wilhelmine Germany,” in \textit{Volksgesetz as Method and Ethic}, ed. George W. Stocking, Jr. (Madison: Univ. Wisconsin Press, 1996), pp. 79–154, esp. pp. 83–89, 135; and Evans, \textit{Anthropology at War} (cit. n. 12), esp. Ch. 2.

century, his recommendation for future anthropological work sustained the older investigative style: if the anthropologist conducted himself as his subjects did, he would become an embodied instrument, literally thinking and feeling as they did.

What reasoning justified Rivers’s method? Rivers began as a Spencerian and ended as a Freudian, but his intellectual changes were not disjunctive. In his obituary of Rivers, Head observed that Rivers considered all of his varied interests to be “aspects of the same problem, the biological reaction of man to his environment.”69 That is, evolutionary change (which included degeneration) was central to all of Rivers’s—and Haddon’s—research; anthropology could address in the field questions about human evolution that could not be investigated in laboratories. The underlying rationale of Rivers’s—and Haddon’s—research method was provided by the physiology of Michael Foster, who taught Haddon and was a mentor to Rivers. Defined as a science of evolutionary adaptation and requiring attention to ecological variation, Foster’s physiology was compatible with the broadly influential arguments of Herbert Spencer, and it accounted for “the actions of living beings on their surroundings (the study of these necessarily involving the correlative study of the effect of the surroundings on the living being).” Its explanation of adaptation depended on a physico-chemical model widely accepted during the nineteenth century: the individual was conceptualized as an energy system, in which forces were variously transferred and transformed into—as Foster put it—“movement, feeling, and thought.” At the heart of Foster’s research program was embryology—arguably the most important area of laboratory science inspired by Darwin’s scheme. Embryology identified lines of evolutionary filiation and interpreted the development of any given organism in energetic terms: if, as Haddon explained, an embryo grew in a challenging environment, it had to expend a considerable portion of its energy reserve, and its developmental potential would be limited. The Freudian model of the person that Rivers endorsed late in his life (with the qualification that Freud exaggerated the motivational importance of both humans’ innate sex drive and feelings grounded in their early childhood experiences) also fit Rivers’s established explanatory habits: the individual body housed a set of competing forces, the expression of which depended on environmental factors—and the health of the individual represented the positive direction of impulses.70 In sum, Rivers was just one among the many nineteenth-century scientists who were engaged in the effort to translate the law of the conservation of energy into


observations of every feature of the natural world—physical, chemical, biological, geological, and so on.\(^{71}\)

Theirs was the framework that explains Malinowski’s understanding of himself as he ventured into the field, a journey he was bound to take at the insistence not only of his mentor and, later, colleague at the London School of Economics, C. G. Seligman, a participant in the Torres Straits Expedition, but also of another person who was critically important to his education, Edward Westermarck, whom Malinowski credited with inspiring him to pursue anthropological training at the LSE and who made the first of his many field trips to Morocco in 1898. It was Seligman who acted to secure financing for Malinowski’s fieldwork, however, enlisting Rivers and Haddon to testify to the young man’s professional promise.\(^{72}\) In the field, Malinowski consulted Rivers’s writings, corresponded with him, and translated his precepts into action: he mastered local languages, dispensing with interpreters’ services; obeyed Rivers’s injunctions to be as unobtrusive a presence as possible; and interacted with indigenes as individuals, recognizing considerable variation among them. In 1916, he wrote to Haddon that Rivers was his “patron saint in fieldwork,” and he stated in his landmark study, *Argonauts of the Western Pacific* (1922), that scientific anthropological method had “been developed to its fullest extent in the works of Dr. Rivers.”\(^{73}\) Indeed, his understanding of himself fit the Riverstian model; as one biographical account observes, Malinowski’s “method consisted in using one’s whole personal life as a scientific instrument,” an approach denoted by his observation in *Argonauts* that it was necessary to attend to “the personal equation of the observer”—the well-known psychological dictum that reception of sense data varied to some degree from one individual to another.\(^{74}\) Notwithstanding his formidable language skills, Malinowski rejected the late nineteenth-century anthropological judgment that learning a language

\(^{71}\) See, e.g., William Robert Grove, Presidential Address to the British Association for the Advancement of Science in 1866, in *Report of the Thirty-sixth Meeting of the British Association for the Advancement of Science* (London: John Murray, 1867), p. ixii.

\(^{72}\) See Seligman to William Pember Reeves, Director of the London School of Economics, 20 June 1912, MP 27/1; this is a plea for funds that would enable Malinowski to make his first field trip (which Seligman then hoped would be to the Sudan). On Westermarck see Malinowski to Kittredge, 4 Mar. 1932, MP 8/5. In Westermarck’s definition, sociology included anthropology (and Westermarck promoted the young Malinowski in sociological circles). See Edward Westermarck, “Sociology as a University Study,” in *Inauguration of the Martin White Professorships in Sociology* (London: John Murray for the University of London, 1908), pp. 24–32; and Westermarck to Malinowski, 5 July 1923, MP 29/20.


would reveal its speakers’ mind-set (though he insisted that mastery of a people’s language was vital to good research). Rather, he argued, the anthropologist came to understand his subjects by embracing their way of life: “I had to learn to behave like the native in personal and social intercourse. Through this, I learned how they think [and] daydream.”

Consider Malinowski’s psycho-physical understanding of himself during his initial fieldwork. In his diary, he interpreted his feelings as functions of his body’s economy, in Riversian terms. While traveling by ship, he wrote, “Loss of subjectivism and deprivation of the will . . . living only by the five senses and the body (through impressions) causes direct merging with surroundings.” In the field, he frequently chastised himself for lapses from his ideal moral standard, which he understood as descents into sensuality. When he had no “energy to get to work,” his moral tone fell and he succumbed to “lecherous thoughts,” with the result that his “metaphysical conception of the world [was] completely dimmed.” And he complained that, in his weakened condition, “I am unable to control things or to be creative in relation to the world.” Malinowski has been criticized for misrepresenting his fieldwork, both exaggerating its duration and inadequately acknowledging the assistance he received from other Europeans in his areas of operation. But it is evident that he took Rivers’s injunctions very seriously; his practices were consistent with his life-long habits of self-examination, self-discipline, and self-reproach (surely reinforced by the necessity of attention to the problematic health he suffered throughout his relatively short life), as well as with his strong commitment to what he understood as scientific method, grounded in his early studies of science and philosophy. Moreover, during the period when he was articulating the basic tenets of his approach, Malinowski understood individual motivation and behavior in near-Freudian terms, as Rivers had: “the beginning of culture implie[d] the repression of instincts,” and human “emotional life [was] definitely coordinated with the environment.” As Rivers had done with rather

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76 Malinowski, Diary in the Strict Sense of the Term (cit. n. 23), entries for 11 Feb. 1914 (written during his Mailu fieldwork) and 26 Nov. 1917 (written in the Trobriands), pp. 33, 131.

77 At Cracow’s Jagiellonian University, Malinowski initially studied physics and mathematics, but he wrote a Ph.D. thesis in philosophy, supervised by a historian of philosophy. See Andrzej K. Paluch, “The Polish Background to Malinowski’s Work,” Man, 1981, N.S., 16:276–285, esp. p. 278; and Young, Malinowski (cit. n. 75), esp. pp. 79–81, 494–492. Malinowski died at fifty-eight, in 1942. He was obsessed with health throughout his life—his own and that of his first wife, Elsie, who succumbed to multiple sclerosis in 1935, at forty-five. Seligman’s relationship with Malinowski was evidently sustained by his willingness to dispense medical advice to both husband and wife. Rivers also had a lifetime of health problems; he too died at fifty-eight, in 1922.

78 Bronislaw Malinowski, Sex and Repression in Savage Society (London: Kegan, Paul, Trench, Trubner, 1927), pp. 182, 176; see also pp. 157–172. Malinowski’s differences with Freud were both with Freud’s identification of basic drives (he thought Freud exaggerated the importance of sexual impulses and ignored other instincts) and with the Lamarckian evolutionism that underpinned Freud’s arguments, making, say, development of an Oedipus complex a universal feature of human males’ maturation, built into their developmental program through a specific historical experience at the dawn of the species’ existence (an argument substantiated with reference to anthropological texts of the nineteenth-century armchair variety, grounded in a unilinear evolutionary model, which Malinowski’s variant of anthropology was designed to discredit). Note that Malinowski agitated to get a Nobel Prize for Freud (for academic and political reasons, it was to be a peace prize); see Malinowski to Seligman, 4 Oct. 1938, MP 27/7. For an account that both chronicles Malinowski’s thinking and
different emphases, Malinowski argued that biological needs were the bases of social institutions and that human action also led to developments irreducible to biology. But the biological feature of his theory was widely rejected after his death. The antibiological turn may to some extent have been a function of sociocultural anthropology’s changing recruitment base. But the most important factor in the decline of Malinowski’s reputation as a theorist was surely the increased professional power of Rivers’s first anthropology student, A. R. Radcliffe-Brown, who was militantly antireductionist, though no less scientistic than Malinowski.  

George Stocking memorably termed Malinowski the “mythic culture hero of anthropological method.” Indeed, it would be difficult to overestimate the international renown that Malinowski gained in this capacity. In his 1960 Inaugural Lecture of the Chair of Social Anthropology at the Collège de France, for example, Claude Lévi-Strauss observed that Malinowski “instituted the ethnographer’s uncompromising participation in the life and thought of the natives”—a judgment that can be seen as especially notable, because Lévi-Strauss might have cited a homegrown figure with an illustrious intellectual pedigree, Arnold van Gennep, a chronically underemployed student of Émile Durkheim’s nephew and disciple Marcel Mauss who joined field observation and generalization at the same time as Malinowski did, in the World War I era. When anthropologists assessed Malinowski’s work upon the occasion of his death, many of them were extremely critical, not least because the relationships they had with him in life were problematic in both personal and professional terms. But they disaggregated his method from his functionalist theoretical orientation, focusing on the defects of the latter; recent anthropologists have sustained their predecessors’ judgment, seeing his method as fundamental to the working definition of their enterprise.


79 Rivers argued that some social phenomena were irreducible to psychological or biological factors, but his student Radcliffe-Brown made irreducibility a defining property of sociocultural anthropology. Radcliffe-Brown asserted that Malinowski’s theory presumed that all societies were essentially identical, precluding understanding of humankind’s “manifold diversity”: see A. R. Radcliffe-Brown, “A Note on Functional Anthropology,” Man, 1946, 46:38–41, on p. 40.


81 In general, twentieth-century sociocultural anthropologists turned against previous efforts to plot a unilinear course of evolutionary development that obtained in societies everywhere, at all times. Malinowski’s functionalist approach prevailed in Britain and appealed elsewhere; its object was not historical reconstruction but analysis of the ways that any given society’s component parts interacted, making the whole what Meyer Fortes memorably termed a “going concern.” See Kuklick, Savage Within (cit. n. 54), pp. 72–73, 120–121, and passim. For analysis of the differences between British and American approaches see Stocking, “Basic Assumptions of Boasian Anthropology” (cit. n. 74). Malinowski’s problematic personal relationships with some of his students ranged from the intermittently difficult (as with Meyer Fortes) to the consistently antagonistic (as with E. E. Evans-Pritchard). It is of some interest that Evans-Pritchard’s differences with Malinowski were purely personal; as Malcolm Crick wrote to Phyllis Kaberry, in Evans-Pritchard’s last Oxford lecture he said that though “he hated Malinowski and liked Radcliffe-Brown, Malinowski did a great deal to advance anthropology, whereas Radcliffe-Brown did nothing”; Crick to Kaberry, 24 Nov. 1972, Richards Papers, London School of Economics and Political Science, File 17/1. Malinowski’s relations with his early mentors worsened over time. Consider their reluctance to endorse his election to the Royal Society. On 21 Nov. 1933 Arthur Keith, an eminent physical anthropologist who judged Malinowski “the keenest brain in anthropology,” urged Seligman to propose...
APOSTLES AND AUDIENCES

Rivers’s methodological contribution was of vital importance to a discipline that was institutionally insecure and of relatively low status at the turn of the twentieth century. To substantiate my claim that his prescription for fieldwork had a major impact, I could detail his professional lineage. But a summary of occupational kinship suffices: virtually all members of the first professional generation of British sociocultural anthropologists were somehow the intellectual progeny of the Torres Straits Expedition. In particular, Radcliffe-Brown became an international agent for the dissemination of his teacher’s scientific ideal; between 1921 and 1937, he was employed in turn at Cape Town, Sydney, and Chicago before becoming the foundation professor of social anthropology at Oxford.

Although Radcliffe-Brown and Rivers had notable differences, the student credited his mentor with working out “the scientific methods of ethnographical observation”—and, significantly, was known throughout his career as Rivers’s student. As should already be clear, Malinowski represented himself as Rivers’s methodological disciple (indeed, Malinowski did not dare to criticize any aspect of Rivers’s work in public until nearly a decade after Rivers’s death—by which time he must have judged that the great man’s reputation had dimmed sufficiently that a limited critique would not be impolitic). Radcliffe-Brown

Malinowski’s election, but Seligman refused: Keith to Malinowski, 25 July 1928, Malinowski Papers, Yale University Archives, New Haven, Connecticut, MS 19, Box 4. On 27 Nov. 1933 Haddon told Seligman that he was not prepared to move Malinowski’s election himself but would second a motion, saying, “As I have said before, I do not trust Malinowski”: Haddon to Seligman, 27 Nov. 1933, MP 27/6. The election became moot. See also Seligman to Haddon, 25 Nov. 1933, MP 27/6. Seligman bore some responsibility for their troubled relationship, however, quarreling with Malinowski over the LSE curriculum and reproaching him for his failures as an academic citizen. Perhaps his most outrageous act was collaborating with Joseph Oldham, director of the International Institute of African Languages and Cultures, in censoring an article of Malinowski’s prior to its 1929 publication in the institute journal, Africa; he claimed that they acted to make the article “as little harmful as possible” to the reputation of the discipline of anthropology: Seligman to Malinowski, 5 Aug. 1931, MP 27/4. For posthumous criticisms of Malinowski see, e.g., Radcliffe-Brown, “Note on Functional Anthropology” (cit. n. 79); and Clyde Kluckhohn, “Bronislaw Malinowski, 1884–1942,” Journal of American Folklore, 1943, 56:2008–2019, which granted the quality of Malinowski’s fieldwork but argued that he had been credited with innovations made by Boasians, who lacked his devotion to self-promotion. For recent anthropologists’ perspective see Robert A. Rubinstein, “Introduction,” in Doing Fieldwork, ed. Rubinstein (cit. n. 22), pp. 1–35, on p. 14; and Wagley, “Learning Fieldwork” (cit. n. 63).

82 For outlines of anthropology’s torturous paths to institutional security in Britain, Germany, and France, respectively, see Henrika Kuklick, “The British Tradition,” in New History of Anthropology, ed. Kuklick (cit. n. 2), pp. 52–78; Penny, “Traditions in the German Language” (cit. n. 2); and Sibeud, “Metamorphosis of Ethnology in France, 1839–1930” (cit. n. 80). There is no clearer illustration of the difficulties of making an anthropological career in the United States at the turn of the twentieth century than the struggles of Franz Boas; see Cole, Franz Boas (cit. n. 29). For one illustration of early twentieth-century anthropologists’ insecure professional status, note the relatively low pay of the two anthropologists employed by the Geological Survey of Canada’s Arctic Expedition of 1913–1918: the Frenchman Henri Beuchat, a recognized scholar (his collaborators included Marcel Mauss) who was doing his first (and last) fieldwork (he disappeared in the field); and the New Zealander Diamond Jenness, a relative neophyte. See Stuart E. Jenness, Arctic Odyssey: The Diary of Diamond Jenness, 1913–1916 (Hull, Quebec: Canadian Museum of Civilization, 1991), p. xxi.


84 In 1924, two years after Rivers’s death, Malinowski declined to review a posthumous book, even though it was a product of the enthusiasm for the diffusionist school that Rivers developed late in his career and Malinowski engaged in energetic dispute with living diffusionists; see Malinowski to Margaret Green, subeditor of the New Leader, 8 May 1924, MP 18/9. In his teaching he expressed reservations about Rivers’s “genealogical method” (to be discussed shortly), which he eventually made public. See lecture notes presumably written in 1926 (Malinowski wrote many notes on the backs of various papers, which date them), MP 23/1 (iii); and Bronislaw Malinowski, “Must Kinship Be Dehumanized by Mock Algebra?” Man, 1930, 30:19–29.

85 In 1924, two years after Rivers’s death, Malinowski declined to review a posthumous book, even though it was a product of the enthusiasm for the diffusionist school that Rivers developed late in his career and Malinowski engaged in energetic dispute with living diffusionists; see Malinowski to Margaret Green, subeditor of the New Leader, 8 May 1924, MP 18/9. In his teaching he expressed reservations about Rivers’s “genealogical method” (to be discussed shortly), which he eventually made public. See lecture notes presumably written in 1926 (Malinowski wrote many notes on the backs of various papers, which date them), MP 23/1 (iii); and Bronislaw Malinowski, “Must Kinship Be Dehumanized by Mock Algebra?” Man, 1930, 30:19–29.
also acknowledged Malinowski’s methodological refinements, although relations between the two men were strained by intermittent personal antagonism, theoretical disputes, and contests for dominance of academic turf.\footnote{See A. R. Radcliffe-Brown, “The Present Position of Anthropological Studies” (1931), in Method in Social Anthropology, ed. Srinivas (cit. n. 83), pp. 42–95, esp. p. 66. During their early careers, Malinowski and Radcliffe-Brown were allies. See Radcliffe-Brown (from Cape Town) to Malinowski, 22 May 1923; and Radcliffe-Brown to Malinowski (from Sydney), 3 Jan. 1929: MP 7/10. But competition between them grew. See Malinowski to Seligman, 16 Feb. 1932, MP 27/5, saying that Radcliffe-Brown (then at Chicago) ought not to be consulted, since “He thinks only of himself”; and W. Lloyd Warner to Malinowski, 3 Oct. 1932, MP 29/20, attempting to broker peace between the two men for the sake of the discipline. Radcliffe-Brown was no less extravagant a personality; when he became Oxford’s professor of social anthropology in 1937 he attempted to impose his will on the discipline, prompting the British Museum’s William Fagg to call him “our own sociology Hitler”: quoted in Mills, \textit{Difficult Folk?} (cit. n. 4), p. 60.}

Most important, Malinowski had an international reputation and a truly international pedagogic reach. Students from all over Europe, North America, and the British Commonwealth came to work with him at the London School of Economics, and they spread his message as they found employment around the world. The Rockefeller Foundation was largely responsible for bringing an international population to study with him, supporting students either directly through its own fellowship program or through the International Institute of African Languages and Cultures, a venture that incorporated British, French, German, and Belgian parties, although it was headquartered in Britain. Malinowski’s global influence followed from foundation policy: one component of the fellowship selection process was an assessment of the probability that candidates would have academic employment at home upon completion of their training, which would make them sound investments.\footnote{By 1930 Malinowski’s work had been translated into French, German, and Spanish, and he thought of himself as an actor on an international stage; see Malinowski to John Van Sickle of the Rockefeller Foundation, 11 Feb. 1932, MP 8/1. For fellowship selection criteria see Kittredge to Malinowski, 6 May 1932, MP 8/5. Malinowski had little enthusiasm for some of the students he owed to Rockefeller largesse, but he turned foundation biases to his own purposes: see Malinowski to Kittredge, 24 June 1933, and Kittredge to Malinowski, 4 Aug. 1933, both in MP 8/5, debating the case of Hilda Beemer (later Kuper), whom Kittredge imagined too consumed by romantic interests to become a serious anthropologist and Malinowski argued had a certain academic future in South Africa.}

The Rockefeller Foundation was not the only source of funding for sociocultural anthropology in the interwar period, but it was certainly the most generous source, and its patronage had an enormous impact.

In explaining the appeal of Rivers’s energetic model of the body, extradisciplinary factors are just as important as (or components of) professional ones: his was just one expression of a scheme that was in widespread circulation. That is, Rivers and his exemplar, Ranke, were hardly the only naturalists to explain the differences between primitive and civilized lifestyles in energetic terms, and, indeed, their argument had a considerable lineage.\footnote{As Maureen Harkin explains, Adam Smith and other Scottish Enlightenment thinkers judged that “savages” lacked “sympathy” owing to their “paucity of resources,” because “sympathy, feeling for others, depends on the kind of material surplus that is only possible in the latter stages of historical development”: Harkin, “Adam Smith’s Missing History: Primitives, Progress, and Problems of Genre,” \textit{English Literary History}, 2005, 72:429–451, on pp. 438, 439. Greg Myers observed that Scottish Enlightenment notions inspired nineteenth-century physicists: Myers, “Nineteenth-Century Popularizations of Thermodynamics and the Rhetoric of Social Prophesy,” \textit{Victorian Studies}, 1985, 29:35–66.} A report similar to Ranke’s is found, for example, in W. H. Hudson’s immensely popular \textit{Idle Days in Patagonia}, first published in 1893 and subsequently reprinted many times in a number of languages. An Argentine-born British naturalist who belonged to a scientific network that included Alfred Russel Wallace,
Hudson described himself as regressing to a savage sensibility while following the indigenes’ lifestyle in Argentina.

Consider some other uses to which the energetic model of the body was put. Karl Marx read and cited W. R. Grove’s *On the Correlation of Physical Forces* (1867), one of the major sources of the model’s popularization (and one that advanced a moderately distinctive version of it); Marx used it to support his definition of human labor power, arguing that since the portion of human energy devoted to material production declined as the economic system’s productive capacity increased, historical change was leading to more time for “the free intellectual and social activity of the individual.” A congeries of European and North American practitioners of what Robert Brain calls “physiological aesthetics” sought to develop an energetic science of art appreciation, which explained, for example, that engagement with a work of art meant reproducing the psychological state that shaped the creator’s production of it. One expression of this project, the scheme of the British art critic Vernon Lee (born Violet Paget), was predicated on the assumption that the intensity of a human body’s physical response to an art object or landscape was directly proportional to the aesthetic quality of the object or scene—provided that the witness’s body had not been corrupted by base sensuality, which rendered it incapable of an authentic physical response to beauty. During the nineteenth century, athleticism became a cult that extended to the valorization of scientific work performed outdoors: exercise was “an incomparable psychic instrument,” in the words of the French Baron Pierre de Coubertin (whose efforts were critical to the revival of the Olympic Games in 1896), understood as a means to discipline the body’s animating forces so as to preclude the possibility of degeneration. And the American secondary school biology curriculum that was developed in the early twentieth century relied on “laboratory exercises through which students learned the fundamental similarity of energy transformation in steam engines, plants, and boys,” which taught them to understand evolution as “successful adaptation to changing environments,” as Philip Pauly observed.

Or consider the immensely influential *Principles of Scientific Management*, published by Frederick Winslow Taylor in 1911 but specifying human engineering practices that he had been recommending and implementing since the 1890s. Taylor’s book attracted international attention and inspired changes in management practices in many settings (although, as is well known, the gap between its recommendations and their realization was considerable). It is often interpreted as a brief for unrestrained managerial authority, justifying control of every feature of workers’ occupational performance—and it is that. But our understanding of scientific management changes somewhat if we shift our focus

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from the omniscient manager to the worker as the manager is supposed to conceptualize him: as the repository of a limited portion of energy, which will be expended efficiently if the manager thoroughly controls its distribution.

As widely diffused a scheme as the energetic model cannot possibly have meant the same thing to all of its exponents. Both Taylor and Rivers, say, were confident that their procedures were rigorously scientific, but they had very different views of humanity. Their energetic understanding allowed Rivers and his associates to preserve notions of cultural hierarchy without explaining this hierarchy in racist terms, rejecting the hypothesis that non-Western peoples behaved as they did because they were less evolved members of the human species than Europeans.90 Riversians could assume that Torres Strait Islanders, removed to the material comfort of a Western society and free to turn their attention to the higher things of life, could follow civilized ways, just as Europeans who found themselves consumed by the exigencies of survival in the field might find abstract thought difficult. Quite unlike Rivers’s Torres Strait Islander, Taylor’s model worker, the long-suffering Schmidt, seems inherently incapable of higher thought, motivated to follow managerial directives he (putatively) finds counterintuitive in order to gain the increased pay that will enable him to realize his modest hopes for his family.

Rivers’s definition of the method of participant observation and his recommendations for field-site selection were not his only contributions to the transformation of sociocultural anthropology. On the Torres Straits Expedition he devised a specific technique, his “genealogical method,” that enabled fieldworkers to identify social structures on the basis of a people’s distinctive understandings of kinship ties and proved of durable research value. Mastery of this method, he asserted, enabled experts to understand social relationships so complex that “Europeans who have spent their whole lives among the people have never been able to grasp them.” This was a strategic argument: fieldwork of a year or more in an exotic place might have been impressive by comparison with a passing traveler’s visit, but anthropologists’ research trips were brief by contrast to the sustained experiences on which colonial officials based their claims to superior knowledge.91 Indeed, two early anthropological monographs—Baldwin Spencer and F. J. Gillen’s 1899 Native Tribes of Central Australia and Charles Hose and William McDougall’s 1912 The Pagan Tribes of Borneo, each the result of collaboration between an academic and a veteran colonial civil servant—can be seen as products of suspicion that colonial administrators’ pretensions might be justifiable.

Arguably, however, the strongest claim that sociocultural anthropologists could make to

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90 In this regard, Rivers was more progressive than Haddon in 1898. Haddon granted that the various characteristics of human types were independently assorted, each type having some apparently primitive and some apparently advanced traits, but he argued that “there can be no doubt that, on the whole, the white race has progressed beyond the black race”: A. C. Haddon, Study of Man (New York: G. P. Putnam’s Sons, 1898), p. xxii. But by 1935, when the first volume of Reports of the Cambridge Anthropological Expedition to Torres Straits (Cambridge: Cambridge Univ. Press, 1935) was (finally) published, Haddon had disavowed correlations of race and culture, and the volume—originally intended to be devoted to physical anthropology—contained only three pages on it; see Anita Herle and Sandra Rouse, “Introduction,” in Cambridge and the Torres Strait, ed. Herle and Rouse (cit. n. 18), pp. 1–22, esp. p. 20ff.

91 W. H. R. Rivers, “The Genealogical Method of Anthropological Inquiry,” in Kinship and Social Organization (1910; London: Athlone, 1968), pp. 97–109, on p. 107. Anthropologists’ anxieties that their expertise might be challenged by persons with long residence in their research domains persisted as late as 1951: in the sixth edition of Notes and Queries on Anthropology (London: Routledge & Kegan Paul, 1951), an unidentified writer observed, “It is necessary to add a warning that long residence in a community, whether as missionary, government official, medical officer, trader, or settler, does not by itself qualify anyone to speak with authority about its social activities” (p. 31).
superior observational powers was grounded in their commitment to embrace alien lifestyles wholeheartedly—the indispensable quality of professional character that has been preserved in disciplinary lore—and this claim made sense in both Malinowskian and Riversian terms if anthropologists’ bodies were reliable measuring devices. Sociocultural anthropologists trained in the interwar period could sound very Riversian, arguing, for example, “If one must act as though one believed, one ends in believing as one acts.” And anthropologists have remained certain that their research method works—that they “they learn to see things (or hear them, or touch them) in the ways their teachers and companions do” by participating in the lives of others, so that the fieldworker “becomes the other through experience”—but in the post-Malinowskian era it seemed that personal transformation “can’t be done by any kind of scientific technique.”

This is especially curious because, contrary to disciplinary myth, pioneers of fieldwork method taught courses in it. Indeed, Malinowski drafted a guide to field method along with its ostensive replication over professional generations has been that opportunities for formal preparation for fieldwork have been virtually nonexistent until relatively recently, notwithstanding a few conspicuous—and conspicuously successful—exceptions.4 This is especially curious because, contrary to disciplinary myth, pioneers of fieldwork method taught courses in it. Indeed, Malinowski drafted a guide to field method at some point during the interwar period, and his students received considerable prepa-

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92 In order, these quotations come from E. E. Evans-Pritchard, quoted in a film produced and directed by Andre Singer, Strange Beliefs: Sir Edward Evans-Pritchard, shown on television in the 1990s and available online at http://www.youtube.com/watch?v=8q9HyONL_10&feature=email; Tim Ingold, “Anthropology Is Not Ethnography,” Proceedings of the British Academy, 2008, 154:69–92, on p. 82; Salzman, “On Reflexivity” (cit. n. 62), p. 808; and Evans-Pritchard, quoted in Strange Beliefs, following his statement that “to understand the Nuer, you’ve got to learn to think like a Nuer, to feel as a Nuer, in a kind of way to be a Nuer.” Coming to anthropology with an undergraduate degree in history, Evans-Pritchard was Seligman’s student; in the post–World War II period, he led many British anthropologists to decide that anthropology was not a science. Statements resembling those quoted are found in Evans-Pritchard, “Fieldwork and the Empirical Tradition” (1950), in Social Anthropology and Other Essays (New York: Free Press, 1962), pp. 64–85.


94 Note, for example, the remarkable careers of students affiliated with the Harvard Chiapas Project from 1957 to 1980; see Vogt, Fieldwork among the Maya (cit. n. 15), pp. 431–437. There are signs of change, however, not least because in the relatively recent past graduate students have demanded that methodological instruction replace the “sink or swim” initiation approach; see Stanley Barrett, Anthropology: A Student’s Guide to Theory and Method (Toronto: Univ. Toronto Press, 1996), p. 107. Fieldwork Methods (before 1998, Cultural Anthropology Methods), a nominally international journal (with largely American editors) devoted to field methodology, has been published since 1989. Many departments currently require students to do some sort of fieldwork early in their graduate careers. The American Anthropological Association now publicizes special short-term field method courses for sociocultural anthropologists, at least one of which has been in recurrent operation since 1994; see http://www.qualquant.net/training/. And some departments, including those at Chicago, Illinois (Urbana), the LSE, and Michigan, offer practical courses. But students can complete Ph.D.’s in sociocultural anthropology in high-ranked departments without taking methods courses, and it is still respectable to proclaim to a professional audience that field method cannot be taught; the full gamut of opinion on the teachability of field method was represented at a recent session of the American Anthropological Association, “The ‘Training’ Problem in Sociocultural Anthropology: Methods Courses and the Politics of Method in Anthropology Today and Tomorrow,” 3 Dec. 2009, Philadelphia. For discussion of the persistence of the “sink or swim” approach in the journal of the European Association of Social Anthropologists see Rajni Paliwala, “Fieldwork in a Post-Colonial Anthropology: Experience and the Comparative,” Social Anthropology, 2005, 13:151–170, esp. p. 154.
ration for research—as well as concrete, practical advice in his responses to their letters from the field, tailored to their distinctive situations and objectives. It is of some interest that Malinowski’s guide disparaged the preference for thoroughly exotic research sites, referring disdainfully to researchers determined to “study people who have rarely seen a white,” an objective that he thought likely betrayed “a wish to be spectacular rather than scientific.”

How was the myth of Malinowski’s own fieldwork embedded in professional memory? Malinowski’s own oracular utterances were surely important. In his lectures, he bolstered his heroic image by issuing a standard narrative of the course of field research: the shock of arrival all alone in a remote village; the loneliness of living without the companionship of other Europeans; and the depression (often described as a breakdown) that the anthropologist was bound to suffer at some point. Nonetheless, in what came to be anthropologists’ received history of Malinowski’s professional development, the decisive factor in the creation of proper fieldwork method was a historical accident: as a citizen of the Austro-Hungarian Empire, Malinowski was identified as an enemy alien and trapped in Australia and its environs for the duration of World War I because he happened to be there (to attend a meeting of the British Association for the Advancement of Science) when war broke out; he found himself with no option other than to pursue funding that allowed him to live in the field for an unprecedented period, and—so the story goes—he recognized the merits of his experience in retrospect. In fact, he had intended to spend two years in the field and, thanks largely to Seligman, had the wherewithal to do so; he was able to secure additional funds from the Australian government by promising to identify information useful to Australia’s colonial administrators. But no matter how this false memory was implanted, a peculiar course of disciplinary apprenticeship was sanctioned by the belief that methodological innovation was a matter of chance rather than reason. Some mentors in other field sciences have insisted that their students work out for themselves how to do research, but generations of sociocultural anthropologists elevated this pedagogic style to a professional creed.

There is abundant documentation of subscription to this creed. For example, in 1925, in an early French fieldwork manual, Alfred Metraux observed that there could be no rules for research.
“beyond certain principles of prudence and impartiality, [for] the freedom of the researcher must be complete.” In the 1940s, Alfred Kroeber, Boas’s first Ph.D. student, dismissed the very possibility that field method could be formally taught: “Some can, and some can’t.” Half a century later, Clifford Geertz remarked that research method is “something one picks up as one goes along.” 

Generations of desperate graduate students turned for guidance to Malinowski’s introduction to *Argonauts* or even consulted his archived fieldnotes if they could. In the 1980s, Chris Shore’s mentors understood first fieldwork as an initiation rite: it was “‘character building’; the tough crucible in which good ethnographers were forged”—“you either had the knack and could cope, or you did not.” Shore’s only preparation came from transmission of the cautionary injunctions of the disciplinary giant E. E. Evans-Pritchard, which included “Bring a stool with you” and “Don’t get food on your fieldnotes.” Even today, fieldwork instruction may be limited to cryptic aphorisms and anecdotes featuring famous anthropologists. Students learn that Evans-Pritchard also said, “For God’s sake, don’t sleep with the natives,” but they comment that their mentors have been “notoriously private when it came to talking about their [own] field experience”.

Compatible with—although not necessarily linked to—pedagogic negligence has been the sustained tradition that to design research prior to one’s arrival in the field is a waste of time. In 1932, Malinowski urged his student Lucy Mair to “do bits of work here and there, which apparently seem irrelevant to any scheme”—though he assured her that the bits would eventually “begin to shape and fall into pattern,” at which point “method comes in.” Later anthropologists made similar pronouncements. In 1977, Margaret Mead exhorted the fieldworker to “be continually prepared for anything, everything—and perhaps most devastating—for nothing.” A recent assessment of fieldwork practices endorsed the “traditional aim” of “seeking serendipity, or at least the unpredictable.” Even a proclamation of intent to create innovative field method echoes the time-honored theme: “Openness to what one encounters ‘in the field’ (however ‘the field’ is defined) is part of what makes ethnography a distinctive approach.”

Contemporary prescriptions may suggest that the working anthropologist’s body must remain in a state of alertness, always prepared for anything. The statement of intent to become a “savage mind,” first coined by Margaret Mead, is now a common phenomenon in the anthropological community. But what does it mean to be a “savage mind”? And how do we reconcile this approach with the traditional dictum of “character building”? This essay explores the limits of ethnographic fieldwork in the contemporary world, drawing on examples from my own research experience as well as those of other anthropologists.

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100 See Wagley, “Learning Fieldwork,” on his years as a graduate student at Columbia in the 1930s; and Boissevain, “Fieldwork in Malta” (cit. n. 55), p. 79. An LSE student, Boissevain consulted the Malinowski archive around 1960. Apparently, apprentice sociocultural anthropologists in Britain were not urged to consult the 1951 edition of *Notes and Queries on Anthropology*, although notables (including Radcliffe-Brown, Evans-Pritchard, Firth, Fortes, and Daryll Forde) were responsible for its “sociology” section. Lack of pedagogic use may explain why this was the last edition.


ready to register unanticipated stimuli that might provoke insight, but such injunctions do not preserve the energetic rationale for fieldwork.

MEMORY LOST AND FOUND

Considering the family of natural history sciences that were transformed at the end of the nineteenth century, we can see that many of the characteristics then introduced have persisted and that the various enterprises still have much in common. The heroic ideal has endured. Moreover, when sociocultural anthropologists ventured into the field, they sustained a perceptual change resembling that experienced by other types of naturalists: they observed that the species they studied admitted of considerable apparent variation and that the success of adaptive behavior could be judged only with reference to circumstances. It is not surprising that the anthropological project of determining where given societies stood in a natural hierarchy (for many nineteenth-century figures, on a unilinear developmental scale) was displaced by efforts to document the sheer abundance of realized human behavioral possibilities—whether these efforts were undertaken to reveal diversity per se or justified as quests for some underlying regularities animating all forms of social organization. Historical trends in anthropologists’ explanations of human variation and the emergence of relativistic perspectives have not been considered as some functions of the field-going naturalist’s experience, but they evidently can and should be.

But what are we to make of the vicissitudes of the energetic explanation of both cultural differences and fieldworkers’ capacity to understand different ways of life? For Rivers himself, the energetic model had various implications: he treated shell shock compassionately, in nearly Freudian fashion—a style different from the punitive approaches favored by many military psychiatrists who served with him in World War I; and he saw diverse ways of life as appropriate, adaptive responses, asserting that humans everywhere had equivalent natural potential. But for the population of sociocultural anthropologists in the aggregate, belief in the energetic model of the body was not required to understand fieldwork as the means for reaching sympathetic interpretations of exotic peoples’ ways of life. The energetic model was consistent with both Rivers’s and Malinowski’s views, since each appealed to biological processes in explaining human variations (albeit with qualifications specific to each). But once they had defined ideal field method, others could take it up and explain its positive results in different ways. And the argument that individuals’ moral careers must be understood as a series of ascents or descents on a behavioral evolutionary scale that are some function of circumstantial demands on bodies’ energy reserves—that any given individual is potentially capable of the full range of human behavioral modes—constituted a quasi-relativism that gave way to a commitment to find value in virtually every way of life (if not necessarily the same measure of value). There is an apparent paradox here: the development of a relativistic perspective was closely associated with attention to the possibilities of degeneration. Notably, while Malinowski’s early efforts referred to “lower races” in the aggregate, he came to abhor discussions of “progress,” and his own experience of personal change in the field involved what he would have seen as degeneration; Rivers was obsessed with degeneration—in

103 For one discussion of this issue see Ingold, “Anthropology Is Not Ethnography” (cit. n. 92).

experimental subjects (including himself), soldiers on the battlefield, and entire populations. A relativistic view was born not so much from seeing so-called savages as noble as from seeing that even the most civilized of humans were inherently capable of degenerating to a primitive condition.

The value of cultural relativism has been represented as self-evident: there is no need to justify a morally unexceptionable position. And the energetic explanation of cultural variation represented a transitional moment in disciplinary transformation. Once the case was made that fieldworkers could develop empathy, it was no longer necessary to explain how it happened: evidence of successful negotiation of the heroic ordeal that effected researchers’ personal transformation substituted for an explanation of it. For us, however, comprehending the scientific mind-set that informed Rivers’s and Malinowski’s understanding of the possibilities of research method is itself as much an anthropological exercise as a historical one—identification of a way of thinking different from our own—and is, as such, a contribution to the anthropological project as it has been defined for over a century. Finally, there is a satisfying irony in my narrative: while colonial expansion facilitated field scientists’ professionalization, for social anthropologists, in particular, a rationale for fieldwork practice adapted from widespread scientific discourse conduces to the development of relativistic arguments that were far from apologies for colonialism.

AUTHOR QUERIES

AUTHOR PLEASE ANSWER ALL QUERIES