
British Social Anthropologists and Missionaries in the Twentieth Century

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Since 1980 there has been an open discussion on the hostility that anthropologists typically have for missionaries. A consensus in this conversation has been that anthropologists dislike missionaries because they are engaged in cultural imperialism. This article, however, explores another hidden factor: the professionalization aspirations of those self-identifying with anthropology as a discipline which created a strong desire to eliminate missionaries as potential rivals. Missionaries indisputably acquired a deep knowledge of indigenous languages and cultures which made it all the more important to dismiss them as biased amateurs lest they should be accepted as competing experts. This dynamic is documented and explored across the twentieth century in the context of British social anthropologists. One particularly telling example is evolving critiques of missionaries in regards to fieldwork as the practice of anthropologists themselves changed in this regard from armchair anthropology, through survey work, to intensive participant observation.¹

The hostility that anthropologists typically have for missionaries has been a matter of much public discussion in the discipline ever since 1980. In that year, *Current Anthropology* published a landmark article by Claude E. Stipe, “Anthropologists Versus Missionaries” (1980). The journal arranged for anthropologists from a range of countries around the world (including Britain) to respond, and all eighteen commentators agreed with Stipe that anthropologists were generally antagonistic to missionaries. The subsequent literature has continued to affirm the accuracy of this observation. To take just one example, a few years later a volume published by the Department of Anthropology, College of William and Mary, went so far as to claim that “the hostility of anthropologists toward missionaries” not only unquestionably existed, but “seems to be on the rise” (Whiteman 1983, 2). A British contribution to this conversation was a special issue in 1992 of the *Journal of the Anthropological Society of Oxford*. In its introduction, the guest editor, W. S. F. Pickering, observed that anthropologists have “a sort of love-hate relationship” with missionaries in which while they are grateful while in the field for the help and hospitality

they receive from missionaries, “the latent ‘hate’ element began to appear” when they returned home and started lecturing and writing (Pickering 1992, 101).

There has even been a general consensus in this literature as to why anthropologists have this antipathy: it is because they believe that missionaries are engaged in cultural imperialism. The related charge of ethnocentrism is also standardly made, but I see it as a subset of the cultural imperialist critique: one might almost say that cultural imperialism is ethnocentrism in action, impinging negatively on others. The archetypal expression of this point of view was a collection of essays edited by Søren Hvalkof and Peter Aaby, *Is God An American? An Anthropological Perspective on the Missionary Work of the Summer Institute of Linguistics* (1981). As tendentious as the title is, it was nevertheless a cloyer version of the essay in it by Bernard Arcand, “God is an American.” Bernard’s PhD in Social Anthropology was awarded in 1972 by the University of Cambridge. While the cultural imperialism charge was, of course, thoroughly leveled, the contributors seemed to feel that even it was not sufficient to generate the desired level of repugnance, and thus they hinted darkly that American

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missionaries might be working with the Central Intelligence Agency and thus were also agents of straightforward political imperialism. While these accusatory anthropologists did not have any evidence to support this charge, the editors helpfully reminded their readers that secrecy is stock-and-trade for the CIA and therefore one should expect such connections to be “difficult to prove” (1981, 183).

As the debate unfolded, one corrective to this critique of missionaries was to point out that anthropologists, for most of the history of their discipline, had actively colluded with colonialism and imperialism. Indeed, in a British context (which is the focus of this article), anthropologists successfully marketed themselves during the first half of the twentieth century as possessing an expertise which was essential for the proper training of colonial officers.² In 1970, for example, E. E. Evans-Pritchard observed that a strategically directed stream that had steadily brought students to Oxford to study anthropology for decades had now dried up: “In the past we had much to do with the teaching of Cadets and Officers in the Sudan and Colonial Services” (1970, 108). A few years later, Talal Asad’s landmark edited volume, *Anthropology and the Colonial Encounter* (1973), helped to launch a period of confession and remorse regarding anthropology’s complicity in colonialism (amidst also recurring attempts to explain matters more sympathetically by some of those who had been involved or had been mentored by those who were).

Likewise, the social evolutionism of James Frazer and others in the early decades of the twentieth century was based on the deeply ethnocentric assumption that Africans, for example, were “savages” who had not yet progressed to be “civilized” like Britons and therefore were in some ways still like children. Bronislaw Malinowski replaced social evolutionism with functionalism but as he would refer to the Trobriand islanders as “niggers” and observed that he saw “the life of the natives” as “something as remote from me as the life of a dog,” he can hardly be credited with freeing the discipline from ethnocentrism (1967, 167). There is no need to pile on evidence of British social anthropology’s shortcomings in the past in terms of either colonialism or ethnocentrism as they have been

well rehearsed before. The reply in the context of the condemnation of missionaries, however, was often that anthropology *now* had its own house in order, but no such reform was ever possible for Christian missions: “Missionization is ‘essentially’ and ‘intrinsically’ unjustifiable ethnocentrism, nationalism, and exploitation. Mission action must, therefore, be regarded as a form of ‘colonialism,’ ‘imperialism,’ and even ‘ethnocide’ and ‘genocide’” (Whiteman 1983, 7f.).³

Missionaries and their defenders have rejected such assertions but, for the purposes at hand, what needs to be highlighted is that the hostility toward missionaries is much older than the primary reason that has been given for it in this open discussion that began in 1980. In other words, even back when anthropologists were often themselves ethnocentric imperialists they still typically disliked missionaries. Malinowski vented his own “hatred of missionaries” and fantasized about launching an “anti-mission campaign,” and then went on to a career in which he aggressively and systematically forging links between the Colonial Office and the discipline of anthropology (1967, 31, 41). Lucy Mair, who was a student of Malinowski’s in the late 1920s and early 1930s—and who survived to join the debate in 1980—was one of the most unsympathetic to the case of the missionaries of all the respondents to Stipe’s gripe. She argued that “the anthropologists of my day” were right to protest against these Christian workers because they were agents of “unwanted changes” (Stipe 1980, 171). It is hard to know what to make of such a critique coming from someone who, after the period in which she recollects holding such a view, then went on to a career in which her faculty position was in Colonial Administration. In short, the fact that the antipathy toward missionaries is older than the reason given for it invites us to look for an additional explanation.

For the first two-thirds of the twentieth century anthropologists needed missionaries in numerous ways. In the field, they were almost invariably dependent on them for a wide variety of practical support ranging from transportation to linguistic expertise. In their research, writing, and lecturing, they could not avoid relying on the work of missionary ethnographers.⁴ In various vital contexts such as the

² For the wider issue of British social anthropologists and the Christian faith, see Larsen (2014).

³ In this quotation, Whiteman is offering a composite summary of what is asserted in many sources.

⁴ For a source by a historian that is careful to document this debt, see Stocking (1983). For a generous acknowledgment of it by anthropologists, see Plotnicov, Brown, and Sutlive (2007).

International African Institute collaborating harmoniously with missionaries could result in opportunities and funding for oneself and one's postgraduate students. It also did not do gratuitously to insult missionaries in one's lectures and books because they were a natural constituency to populate the former and to buy the latter. Indeed, along with serving the Colonial Office, anthropologists routinely argued that their discipline should be expanded at the universities because it met the practical need of providing useful training for missionaries (for an early example, see Read 1906). Anthropologists actively schemed for the entire first half of the twentieth century to increase enrollment in their courses by targeting those already in, or preparing for, Christian missions.

The result of all these desires and dependencies was that overt hostility was not expressed publicly. Malinowski's candid comments come from his diary which was published posthumously in the changed context of 1967. His public face at the height of his career, on the other hand, included an address to a monthly Methodist missionary lunch in London during which he was the one doing the proselytizing: "Those responsible should see to it that the missionaries they send out have some anthropological training, thereby following the example of an enlightened Colonial Office" (*Methodist Recorder* 1930). To take just one more random example of a very widespread phenomenon, the obituary in *Man* for A. R. Radcliffe-Brown (which was written by Meyer Fortes) credited the Oxford professor with emphasizing "the utility of anthropological studies" for missionaries (Fortes 1956, 150). One of the most frank statements of the antipathy was made by the anthropologist Hortense Powdermaker who studied with Malinowski in the mid-1920s and recalled of the conventional wisdom of that time period: "Missionaries were the enemy" (1966, 43). Once again, the point is that this was something that anthropologists then said amongst themselves in private which she was now revealing in print in the very different context of 1966.

Rather than concerns regarding cultural imperialism providing a full and sufficient explanation, the thesis of this article is that much of the antipathy toward missionaries in the first two-thirds of the twentieth century and sometimes beyond can be attributed to the professionalization goals of anthropologists. Frank M. Turner, who was John Hay

Whitney Professor of History at Yale University, argued persuasively that the notion of a conflict between theology and science was generated as part of a campaign of professionalization by would-be scientists (1993, 171-200). In the mid-nineteenth century there was no such profession. Charles Babbage, the brilliant mathematical thinker who first conceived the programmable computer, observed in 1851: "Science in England is not a profession: its cultivators are scarcely recognized even as a class. Our language itself contains no *single* term by which their occupation can be expressed" (Turner 1993, 177). In other words, this was before there were "scientists." Instead, there were only "men of science," a term, like its counterpart, "men of letters," that referred more to the leisure pursuits of gentlemen than to what someone did for a living.

Until several decades into the nineteenth century, there were only two universities in England, Oxford and Cambridge. Both saw Classics as the rightful core of a university curriculum and therefore had few faculty positions in the natural sciences. Moreover, in order to hold a position at these universities one would need to be ordained in the Church of England and thus be also a clergyman (Engel 1983). The same would have been true of schools for children and youths. There were no state schools until 1870, and therefore, most schools, especially the elite ones such as Eton, Harrow, and Rugby, had an explicitly Anglican identity. Indeed, being a priest in the Church of England was widely seen as the most sensible way to make a living for someone who had scholarly interests he wished to pursue. The Christian ministry was a learned profession that often allowed one considerable time to invest in intellectual pursuits of one's own choosing. Therefore, most scientific work in England was being done by clergymen. Moreover, much of it was remarkably good work. Not only were many of the nation's greatest men of science also clergymen, but numerous more obscure clergymen up and down the country were carefully, patiently, and accurately cataloguing the natural world and discovering its secrets.

One can see how this would be very annoying to someone such as T. H. Huxley who wanted to be a man of science himself but, not least because of his agnostic views, was unable to make a living either as an Oxbridge professor or as a clergyman (Desmond

1998).⁵ In fact, as celebrated as Huxley was, his career was not as a university professor or some other such position that we would assume to be a fitting one today for a scientist of his reputation. Rather, he was fortunate to make a living by lecturing at the Government School of Mines, and even this opportunity would not have been available earlier in the century. Huxley and others who aspired to turn scientific pursuits into a profession therefore had a vested interest in presenting religion as fundamentally in opposition to science. The purpose of the warfare model was to discredit clergymen as suitable figures to undertake scientific work in order that the new breed of professionals would have an opportunity to fill in the gap for such work created by eliminating the current men of science. Clergymen were branded amateurs in order to facilitate the creation of a new category of professionals and then they were branded as biased or anti-science so as to exclude them from becoming competitors in this new profession. Francis Galton's *English Men of Science: Their Nature and Nurture* (1874) illustrates this point. Galton was also trying to generate a perception of inherent conflict. His research for the book included sending out questionnaires to men of science. To his disappointment, the overwhelming majority reported that religious beliefs were in no way a hindrance to scientific work. In an ironically unscientific way, he decided to ignore these results and simply to assert in his book that religious convictions were "uncongenial" to the pursuit of science, despite the fact that his own data which he had collected specifically in order to make it did not support that conclusion (Turner 1993, 185).

In a directly parallel way, anthropologists were faced with the problem that many missionaries seemed to have already acquired the expertise that they were claiming was distinctively their own contribution. Worse, missionaries often apparently had a much deeper and fuller knowledge of indigenous languages and cultures than anthropologists. As the new breed of professional scientists had done with the ordained men of science, it therefore became highly desirable to attempt to discredit the missionaries-ethnographers as biased amateurs.

Generation after generation, however, it was impossible to deny that some of the best anthropological studies had been done by missionaries. Therefore, a kind of "present company excepted" rhetoric was developed in which anthropologists were carefully taught to label any missionary whose work they were using positively as a curious anomaly, while simultaneously insisting that missionaries as a class were incapable of doing good ethnographic work. This approach was there from the beginning of the discipline back in the Victorian age. E. B. Tylor is generally identified as the founder of the discipline of anthropology in Britain. His seminal classic *Primitive Culture* is cluttered with approving citations of the works of scores of missionaries in order to provide the bulk of the evidence for his statements. Nevertheless, Tylor insisted that while "some missionaries" do really understand "savages," "for the most part" they have a "hating and despising" attitude which blinds them from comprehending, whilst anthropologists, in marked contrast, are able to discern indigenous cultures accurately (Tylor 1874/I, 420f). Likewise, in a Royal Geographical Society publication, Tylor pointedly contrasted the "unfavourable" perspective of the missionary with the clear-sighted anthropologist (1883, 240).

This pattern was continued with J. G. Frazer. His anthropological writings were also overwhelmingly dependent on missionary sources—not only published ones but also through his soliciting information from them directly. If this work by missionaries was not sound, then Frazer's own scholarship would be *ipso facto* fundamentally compromised. Frazer was willing not only to lean on missionary anthropology, but to laud it. Nevertheless, if the ethnographic work of a missionary happened to get in the way of a pet theory of his, then suddenly Frazer was quick to dismiss it on the very grounds that it came from a missionary and thus was not to be trusted. Thus when Frazer and his allies disagreed with an ethnographical judgment by Carl Strehlow they rushed to pronounce that it was because his profession proved that he could not be relied upon: "the differences between us are due to the fact that Strehlow is a missionary" (Spencer 1932, 110)⁶ (This accusation of bias was itself so unfair that R.

⁵ Huxley served as the president of the Ethnological Society and helped to bring about its merger with the (later Royal) Anthropological Institute, so the connections with anthropology are direct as well.

⁶ Baldwin Spencer to James Frazer, 10 March 1908.

Angus Downie [1970, 45]—a personal disciple so fervent that he even named his daughter Frazer—had to concede in his thoroughly sympathetic study of his master that anthropologists have since determined beyond dispute that Strehlow was right in this dispute). Likewise, the anthropological work of the Anglican missionary to the Ainu people of Japan, John Batchelor, endured the test of time. In 1959, for example, an article in the Royal Anthropological Institute's *Man* declared it to be “still the outstanding ethnographic study of that area” (Rosenstiel 1959, 109). Nevertheless, when Batchelor's evidence did not support a hypothesis cherished by the author of *The Golden Bough*, Frazer was quite willing to assert that the fact that Batchelor was a missionary should undermine one's confidence in the reliability of his ethnography. Frazer's own gullibility in this clash of views now makes for rather risible reading:

Mr. Batchelor denies that bear-cubs are suckled by the women. He says: ‘During five years’ sojourn amongst, and almost daily intercourse with, them—living with them in their own huts—I have never once witnessed anything of the sort, nor can I find a single Ainu man or woman who has seen it done’. But as a Christian missionary Mr. Batchelor was perhaps not likely to hear of such a custom, if it existed. (Frazer 1900/II, 376f.)

This mode of attack could even be made through degrees of separation. Andrew Lang, for example (again, on a point where he would be vindicated by subsequent anthropology), although he was not a missionary, nevertheless had his ideas dismissed with the slur that he had probably been influenced by missionaries (Marrett 1912).

No matter how respected a missionary was as an anthropologist, in the heat of a disagreement his anthropologist opponent would always be apt to claim that the very fact he was a missionary was *ipso facto*

evidence that he was probably wrong.⁷ This was the case for Wilhelm Schmidt, for example, when he dared to disagree with A. R. Brown (later Radcliffe-Brown). Despite Schmidt's high reputation in the discipline, and his even being the founder of the journal *Anthropos*, Brown was quick to assert that “his arguments are rendered suspect from the beginning” because his mind is tainted by “preconceived opinions,” whereas Brown himself (in his own self-reporting) was a true anthropologist who followed the “scientific method” of “seeking truth with an open mind” (1910). Schmidt responded by observing how touchingly naïve it was of Brown to imagine that non-religious thinkers have no biases and by pointing out that Brown had actually set out on his fieldwork with the very purpose of discovering evidence for a pre-formed theory of his which—surprise, surprise—he convinced himself he did find and now he would not let go of his preconceived opinion despite the fact that those who had been in the field much longer rejected it (Pels 1990; Schmidt 1910).⁸ (Once again, subsequent anthropology has confirmed the correctness of Schmidt's side in this dispute.) Malinowski was particularly good at discrediting the work of missionaries as a class. In *Argonauts of the Western Pacific*, for example, he asserted that “for the most part” they were full of “biassed and pre-judged opinions,” in contrast to anthropologists, who are committed to “the objective, scientific view.” He then saved for a footnote the “present company excluded” disclaimer regarding “a few delightful exceptions” (1922, 5f.).

Although it will not be belaboured in this article, one clear, related factor in anthropologists' dislike of missionaries was the more straightforward rivalry of the hunt. For most of the twentieth century, both Christian missionaries and anthropologists dreamt of being the first to encounter what in missionary parlance was called an “unreached people group.”⁹ Alas, the missionaries virtually always won these races and thus

⁷ I have discovered no examples of such clashes in which the missionary was a woman and thus I have used male pronouns to preserve the fact that we are discussing how particular individuals are treated.

⁸ I am glad to acknowledge here more generally that Pels' excellent chapter is one of the existing pieces of scholarship most in accord with some of the key arguments being made in this article.

⁹ This is another example of a counter-theme that has developed of emphasizing how similar to each other anthropologists and missionaries are: see, most notably, Van Der Geest (1990). A desire to at least pretend that they had the field to themselves helps to explain why twentieth-century British social anthropologists often left the missionaries that were there already out of their ethnographies: Van Der Geest and Kirby (1992).

anthropologists were perpetually annoyed with them for contaminating what would have otherwise been a pure, scientific sample of “primitive” culture. Malinowski explained to the supporters of Methodist missions that they needed to understand that, from the point of view of his profession, “the missionary was spoiling the game for the anthropologist” (1930). From the late 1960s onward, anthropologists often tried to reduce such continued losses by proclaiming that it was a matter of high principle and ethical duty that everyone (I’m talking to you, missionaries) follow Star Trek’s Prime Directive in which introducing anything into a society which might deflect or accelerate its course of development is forbidden (while sometimes also being seemingly willfully naïve about how much change was brought about in a previously “unreached people group” by the very event of having a western anthropologist come to live with them).

The professionalization aspect of the antipathy of anthropologists toward missionaries is strikingly illustrated by changing judgments about fieldwork. James Frazer’s approach was a carefully policed division of labour in which missionaries in the field were merely collectors of data.¹⁰ A true anthropologist, however, was someone who developed grand interpretations and this, the author of *The Golden Bough* insisted, could not be done from the field. Anthropological theory must be based on the comparative method and a missionary fieldworker was stuck in one, remote place, whilst an armchair anthropologist in Britain was at the centre of empire with reports for around the globe pouring in for him to analyze and synthesize. Frazer’s main source for Africa was the Anglican missionary to Uganda Canon John Roscoe. In an obituary tribute to Roscoe, Frazer praised him both for being unrivalled as a “field anthropologist” and for not having the presumption to attempt to explain his evidence with any theoretical statements (1935, 77). (This tribute, alas, makes someone who was actually an astute and pioneering ethnographer sound rather like he was Frazer’s errand boy.) In his correspondence with obliging and generous fieldworkers, Frazer tried hard to keep them in their place as humble chroniclers: “What we want

in such books . . . is a clear and precise statement of facts (as far as they have been ascertained) concerning the particular people described—that and nothing else” (Spencer 1932, 23).¹¹ The “we”, of course, is not the general reading public, but rather the self-styled professional anthropologists who wanted raw data for writing their own theoretical books and not rival claimants to their status.

After the armchair phase came the survey phase in which anthropologists would land in a region for a few days and interview locals through an interpreter (often famously travelling on a missions ship and conducting their interviews on the veranda of a missionary’s house). In 1910, W. H. R. Rivers insisted that missionary ethnographic efforts were “amateur” ones that were probably inaccurate. Even though they knew the local language and people thoroughly, Rivers himself had developed a Gnostic technique (‘the genealogical method’) that meant he could discover better anthropological evidence in a fly-by visit than the missionaries could who had been in the field for decades. To feel the full chutzpah of this boast, it is worth quoting Rivers at length:

From this point of view the method is more particularly useful to those who, like myself, are only able to visit savage or barbarous peoples for comparatively short times, times wholly insufficient to acquire that degree of mastery over the native language to enable it to be used as the instrument of intercourse. . . . By means of the genealogical method it is possible, with no knowledge of the language and with very inferior interpreters, to work out with the utmost accuracy systems of kinship so complicated that Europeans who have spent their whole lives among the people have never been able to grasp them. It is not an exaggeration to say that in such a matter as this or in that of the regulation of marriage, it is possible by this method to obtain more definite and exact knowledge than is possible without it to a man who has lived for many years among the people and has obtained as full a knowledge as is ever acquired by a European of the language of a savage or barbarous people. (1910, 9f.)¹²

¹⁰ An astute source that also makes this point and which, in general, is attentive to issues of professionalization is Harries (2005).

¹¹ J. G. Frazer to Baldwin Spencer, 26 August 1898.

¹² As to ethnocentrism, a recurring figure in this article is an informant whose name was Kurka, but whom Rivers refers to throughout as “Arthur.”

In short, Rivers convinced himself that although he did not have extensive experience in the field and the missionaries did, his magical method made him a professional but not them.

The next phase, of course, was the adoption as best practice of what the missionaries had been doing all along: fieldwork in which one stayed among one people for a year or more, learning their language and ways. The need to find a way to explain why missionaries were disqualified from being true anthropologists still remained however. During this new phase, a particularly brazen attempt was made by John Mavrogordato, Fellow of Exeter College, Oxford, in his presidential address to the Oxford University Anthropological Society in 1943 which was subsequently published in *Man*. Mavrogordato argued that missionaries were not clear-sighted ethnographers precisely because they stayed in the field too long: “We all see what we expect to see; and such misinterpretation may arise either through the shortness of the traveller’s visit, or through the length of his residence, which may lead, as in the case of some missionaries, to stubborn and preconceived opinions” (1943, 129). (Part of the genius of this critique is to tie what would appear to be an advantage that missionaries have—length of time in the field—with the old trope that they are uniquely tainted by “preconceived opinions.”) Echoing Rivers’ approach from the survey stage in this new era of participant observation, Isaac Schapera reassured anthropologists coming to Africa in the 1950s that they need not be intimidated by missionaries who seemed “to know all about the culture of the natives” because one could always expose them as amateurs due to their lack of initiation into the Gnostic rites of structuralist kinship studies (Schumaker 2001, 241).¹³

The professionalization thesis helps to explain the “love-hate” nature of the attitude of anthropologists to missionaries. If a principled objection to Christian missions (as, for example, that it is inherently cultural imperialism) was a complete explanation, it would not seem to explain sufficiently the “love” element. What one sees throughout the twentieth century is British social anthropologists who were quite willing to express publicly their affection for particular missionaries who

were not in danger of becoming their rivals but were rather content to play subservient roles as their informants, students, or admirers. This has already been illustrated in the case of Frazer with Canon Roscoe. Perhaps most symbolically—not to mention spectacularly—this was even true in the relationship between the LMS missionary William Saville and Malinowski. Saville was the very person who occasioned Malinowski’s famous “hatred of missionaries” outburst. This, however, seems to have been largely a defensive reaction to the fact that Saville had the expertise (not least linguistic) that Malinowski lacked. When Saville later actually came to the London School of Economics and attended his seminar—thus demonstrating that Malinowski was the expert—then the professional anthropologist suddenly developed fond feelings for him, and was even happy to praise him as “a modern type of missionary who has been able to fashion himself into an anthropologist” (Young 2004, 332).

The exception that proves the rule is therefore the British missionary-anthropologist Edwin W. Smith (1876-1957). Smith was a Primitive Methodist missionary in what is now Zambia before returning to Europe (and eventually England) to work for the rest of his career for the British and Foreign Bible Society. The main scholar who has attended to Smith’s life and work, W. John Young, has observed that he “identified himself as a missionary throughout his adult life” (2013, 245). Nevertheless, Smith was extraordinarily successful and honoured as an anthropologist. His ethnographic monograph, *The Ila-Speaking Peoples of Northern Rhodesia* (1920), was admired immediately—and its high reputation endured.¹⁴ The review in *Man* prophesied correctly that it was “a work which must take rank with the classics of anthropology” (Wener 1921, 125). In 1933, the anthropologist (and religious sceptic) A. C. Haddon (1933, 54) identified it as one of the three great monographs on an African tribe (tellingly, the other two were both written by missionaries as well, John Roscoe and H. A. Junod). In 1949, Max Gluckman (another anthropologist who was also an agnostic) acknowledged that *The Ila-Speaking Peoples of Northern Rhodesia* had “founded modern

¹³ Schumaker’s work focuses on the related effort to keep colonial administrators who did ethnographic work from being accepted as true anthropologists.

¹⁴ Smith generously shared the authorship credit with Andrew Murray Dale, even though it was overwhelmingly his work.

anthropological research in British Central Africa” (Colson and Gluckman 1951, ix). As late as 1966, the anthropologist Elizabeth Colson (1968, 1) testified that it had stood the test of time: “*The Ila-Speaking Peoples of Northern Rhodesia* is one of the great classics of African ethnography. This has been recognized since it first appeared in 1920 and the years have not diminished its reputation.”

Smith became a Fellow of the Royal Anthropological Institute in 1909.¹⁵ He served on its council in 1927-30, was awarded its Rivers medal in 1931, served on the council again 1932-33, and gave its Henry Myers Lecture in 1952. Most of all, he was elected president of the Royal Anthropological Institute for the years 1933-35, the only missionary ever to be so honoured. In 1926, Smith helped to found what would become the International Africa Institute and he served as a founding council member. He was awarded the Silver medal of the Royal African Society and became the editor of its journal, *Africa* (he was succeeded by Daryll Forde). He gave the Frazer lecture at the University of Liverpool. One emblematic example of the Revd Edwin W. Smith as an anthropologist amongst anthropologists is his inclusion in the festschrift for C. G. Seligman. This volume was edited by no less august a cast than E. E. Evans-Pritchard, Raymond Firth, Bronislaw Malinowski, and Isaac Schapera, and its other contributors included A. C. Haddon, Robert H. Lowie, R. R. Marett, and Audrey I. Richards (Evans-Pritchard et al. 1934).

Nevertheless, Smith’s remarkable favour within the discipline was undoubtedly aided by his careful, lifelong efforts to reassure anthropologists that he was in no sense a professional rival. In the preface to *The Ila-Speaking Peoples of Northern Rhodesia*, Smith and Dale demurely wrote, “While not professing to be scientifically trained anthropologists, we have written with such experts in mind, and if we have succeeded in giving them any valuable material for their studies we shall be glad” (1968/I, xiii). Smith had also sent the manuscript to W. H. R. Rivers and therefore presented the published version to his readers as a text by mere fieldworkers that had been vetted and improved by this true anthropologist. James Frazer was so delighted with Smith’s willingness to acquiesce in his division of labour between missionary-collectors and theorizing-anthropologists that he quickly

befriended him and had the Primitive Methodist missionary as a frequent house guest. Moreover, far from outgrowing this deferential stance as his expertise became more established and recognized, Smith employed it all the more as his reputation rose. This culminated in the opening remarks of his first presidential address to the Royal Anthropological Society: “When you elected me to this Chair I imagine you did it with your eyes open. You knew that you were bestowing the highest honour you have to bestow upon one who is not a professional anthropologist but an amateur” (1934, xiii). Anthropologists were quite willing complacently to accept Smith’s self-designation. For example, Isaac Schapera, writing Smith’s obituary in *Man*, pronounced unequivocally that the former president of the RAI was “not a professional anthropologist” (1959, 213). This is particularly rich as Smith had been the external examiner on Schapera’s own 1929 PhD thesis at the London School of Economics and Schapera had asked Smith to serve in 1948 as a visiting professor to replace himself while on leave (Young 2002, 143, 190). Moreover, it must be borne in mind that it would be anachronistic to use having a degree in anthropology or a university appointment in anthropology to define who was a professional in Smith’s era. (Two great profession-policing figures we have met in this article, for example—Rivers and Frazer—would not themselves readily qualify, although perhaps Frazer’s counsel might try to make the most of his purely honorary and quickly abandoned professorship at Liverpool.) Smith had put himself out of competition so successfully that even Powdermaker in her memorable “missionaries were the enemies” confession, immediately offered the “present company excluded” clause, “except for Edwin Smith” (1966, 43).

There are also additional, hidden factors why the hostility of anthropologists toward missionaries became more public and overt from the late 1960s onward. The dramatic expansion of the university sector allowed anthropologists to think much more in terms of doing pure research in the confidence that there was funding to be found and university posts to be had. Correspondingly, Christian missions and colonial administration were no longer significant enough sectors to make catering to them as potential generators of students and funding opportunities a path of prudence. While one can read earnest, wooing

¹⁵ The details in this paragraph may be found in Young (2002).

statements that it was essential for missionaries to take courses in anthropology by eminent anthropologists and by Royal Anthropological Society officer holders and publications throughout the first half of the twentieth century, by 1987 Stipe could report that an anthropologist had told him that she found the tendency of a growing number of missionaries to pursue a postgraduate degree in anthropology as part of their training “quite frightening” (Stipe 1987, 60). Still, in the last third of the twentieth century anthropologists continued to go into the field only to find that they were dependent on missionaries for practical help and to be embarrassed that, for example, they sometimes had to make it appear in their publications that they had learned the local language much more thoroughly than they did whilst knowing that the missionaries truly possessed this knowledge. Thus one constant across the twentieth century was the recurring temptation by British social anthropologists to define missionaries as biased amateurs in order to shore up their own place and self-perception as professionals.

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