Phantom Scandal: On the National Uses of the “Thailand Controversy”

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The episode known as the “Thailand Controversy” centred on claims that, during the Vietnam War, American anthropologists in Thailand had been collaborating with U.S. and Thai military interests and in ways that harmed social life and efforts towards equality and justice in that country. Reflection and comparison suggest that the allegations of impropriety allowed for emotional identification and understanding in terms of outrage, and that this dynamic is common in the context of anxieties over the transgression of ethnic boundaries. The case is compared with an episode of fears of witchcraft involving the Mien of northern Laos, when an ethnic militia took charge of spiritually and militarily guarding the ethnic boundary. Examination of how the anthropological crisis involving Thailand played out suggests various national differences in the academic discipline, in the United States, Australia, Austria and Thailand. The facts of the case are strongly connected to emotional identification, to a divide in the United States between theoretical and applied anthropology, and to responses to decades of political suppression in the United States.

Keywords: anthropology, Thailand, United States, Mien, Thailand Controversy.

The so-called “Thailand Controversy” was a debate regarding the boundaries of anthropology in 1970 that still has some currency. It revolved around allegations that some American anthropologists had been engaged in improper collaboration with agents of U.S. counterinsurgency efforts in Thailand, and further that they had collected data that the Thai military could use in attacks on villages. Once the allegations were made public, they acquired a truth value that was independent of the facts of the case.
The allegations allowed for an emotional reaction — outrage — and for identification in such terms. I suggest that the alleged misuse of anthropological expertise created an aura of transgressive scandal, and that the creation of this aura set in motion dynamics that resemble the policing of an ethnic boundary. In the United States, the episode intersected with debates on the Vietnam War and with civil rights agitation. The Thailand Controversy played out differently in Austria and in Australia, and these differences indicate the ways in which academic disciplines are continually shaped in relation to national histories.

I first heard of the Thailand Controversy while in graduate school in the late 1980s. It was most often presented as a fact that certain Western anthropologists in Thailand had collaborated with U.S. military interests in the region. This collaboration was presented as an example of transgression of which to be aware, and recognition of the anthropologists who defended the purity of their discipline against such improper behaviour sometimes came along with this warning. More than forty years after the so-called controversy, I want to examine the way that it played out and suggest that it has many parallels in what anthropologists have noted in their “peoples” but seem less keen to notice among themselves — categorical concerns with purity and with the dangers of transgression. In part, the episode indicates a disciplinary disengagement from society that contrasts markedly with the Second World War era, when the majority of U.S. anthropologists were positively inclined to lend their expertise to the war effort and to various other social engagements.

My angle on the case comes from an ethnographic parallel. The Iu Mien peoples of Laos were made into a militia during the Second Indochina War (1958–75). While both Mien social life and the dynamics of war were for the most part multi-ethnic, the militia was for some time involved in monitoring the ethnic boundary in relation to an outbreak of fears of witchcraft. I draw the parallel to show that, in certain moments of stress, concerns with the ethnic (or disciplinary) boundary isolate particular marginal insiders as the
problem. In both settings, there was no concern with outsiders who were engaged in the activities deemed transgressive or dangerous. There was instead in each setting a general focus on sealing the boundary between insiders and outsiders. The meaning of these episodes resides primarily in how knowledge relates to identification: people “know” the Thailand Controversy in relation to how they want to situate themselves within anthropology and social life in the United States, Austria, Australia or elsewhere. For the Mien, I think, there was no option of alternative positions or perspectives. They were affiliated with and under the command of a militia, and they “knew” or “experienced” the witchcraft scare as a matter of a vulnerable ethnic-and-spiritual boundary that the militia took to guarding.

To a degree, I am implicated in the case, in the sense that I inherited its legacy. I studied with scholars who did research in Thailand in the late 1960s and after, and during the course of my dissertation research on the Mien I had an affiliation with the Tribal Research Institute (TRI) in Chiangmai. One of the allegations of the instigators of the controversy was that what was at the time called the Tribal Research Center (TRC) was central to the gathering of information that might be used in decisions regarding counter-insurgent attacks on particular villages. For the longest time I studiously avoided dealing with the controversy, and I do not presume to resolve the matter in this article, but I hope to make the issue comprehensible by moving outside the framework of antagonistic debates and offering comparative perspectives on the case.

Rhetoric, Debate and Anthroversy

Rhetoric can make two identical examples seem diametrically opposed, as in the following examples from Siamese history. A fifteenth-century Thai historical poem commemorates Ayutthaya’s conquest of the Lanna (Yuan) domains — in contemporary terms this was Central Thailand’s attack on Northern Thailand. Near the end, the poem weaves together violence, plunder and the king’s glory.
See how our men follow the enemy everywhere to surround them!
[See how] they pounce forward to attack the city and destroy it!
[See how] they carry off a profusion of silver and gold on their shoulders, and take the glorious ladies of the city, the horses and elephants, to present to His Majesty! The prisoners of war, with their hands tied, are pulled along the road by horses; and when they are exhausted they are bartered [by our men] for liquor. His Majesty’s renown fills the earth and sky with fragrance.... A hundred kingdoms girdling the earth live in peace, and a hundred kings hasten to come and do him honor, offering him golden lotuses as tribute. (Griswold and Prasert 1976, pp. 152–53)

Compare this passage to another description from a Siamese (Central Thai) perspective, in this case concerning the Burmese destruction of the court at Ayutthaya in 1767:

The victors behaved like vandals. The palace, the principal buildings, and thousands of private houses were soon prey to flames, and their sacrilegious lust for destruction did not permit the visitors to spare even the temples dedicated to the cult of their own faith. All the largest and most beautiful images of Buddha were hacked to pieces, and many of them were burnt for the sake of the gold leaf with which they were coated. Plunder, and still more plunder, was the watchword. Men, women, and children were flogged and tortured to make them reveal the hiding places where their few treasures or savings were concealed. (Wood 1925, p. 249)

A comparison of these two Ayutthaya-related accounts provides some perspective on the use of language to anchor violence and political struggles. Similar acts of intimidation, violence and plunder is divine in the former case and cruel in the latter. References to Buddhist virtue and un-Buddhist violence describe the same acts, within and between societies that shared the same Buddhist orientation towards hierarchy, virtue and power. There is every indication that a social order is blind to the harm in its ways when the same acts are described as sacrilege and the lust for destruction in one instance and as lotus-filled and fragrant delights in another. This is not to pick on some medieval Thai people; it may apply to us all.
To a large degree, the knowledge of these two episodes is a matter of identification. If one identifies with the Ayutthaya court then one may celebrate its assault on Chiangmai as glorious and view the Burmese assault on Ayutthaya as despicable. The Thailand Controversy allows for similar kinds of identification. One may view the whistle-blowers as commendable guardians of the anthropological discipline’s ideals and the accused transgressors as traitors to these same ideals. That view may hold if one regards anthropology as a singular discipline with a clearly defined agenda that sets it apart from some of the vagaries and complexities of social life.

By so much as touching the so-called Thailand Controversy, I risk being drawn into the fray of finger-pointing and assigning blame. The Thailand Controversy expressed an intersection of the Vietnam War, student protests and civil rights activism in the United States, tensions within the American Anthropological Association (AAA), U.S. military involvement in Thailand, American journalism, and a university student’s betrayal of his professor’s trust. My concern is with the structural quality of a field. I refuse to settle scores and instead arrive at the matter somewhat like a refugee viewing a landscape where all sides lost something important.

On the night of November 19, 1971, during the Council Meeting of the American Anthropological Association [in San Diego, California], Margaret Mead was hissed as she spoke from the floor. [Mead] was speaking, in the capacity of Committee Chairperson, to the “Report of the Ad Hoc Committee to Evaluate the Controversy Concerning Anthropological Activities in Relation to Thailand … during the Vietnam War.” (Helm 1985, p. 1)

That is, a fact-finding committee constituted from within the AAA and headed by Mead found no basis to the allegations of misconduct on the part of American anthropologists who had worked in Thailand, but the majority of the AAA’s members were unconvinced and wanted to hold on to their outrage. The episode started with “six documents [that] had been stolen from the files of an anthropologist who had done extensive research in Thailand” (Davenport 1985, p. 67).
The files came from a graduate student who worked as a research assistant to anthropologist Michael Moerman at the University of California, Los Angeles, and had keys to his office. They then went to activists with the Student Mobilization Committee to End the War in Vietnam (SMC), who published portions in their *Student Mobilizer*: “Copious extracts [from the files were published, and accompanying commentary accused and named] American anthropologists [as involved] in clandestine and secret activities in Thailand” (ibid., p. 65). At the American Anthropological Association’s meetings in San Diego,

copies of the *Student Mobilizer* of April 2, 1970, were available to anyone who wished to purchase one. Also, copies of the six documents were in the hands of some persons who were very willing to allow them to be duplicated. (ibid., p. 67)

The purloined files indicated that Moerman and other scholars had been at meetings where some American military and Agency for International Development staff discussed possible strategies for combating or deflecting peasant sympathies for leftist insurgents (Wakin 1992, pp. 45–78; Jorgensen and Wolf 1970, pp. 3–4). In Eric Wakin’s words, the “documents detail[ed] extensive contacts between distinguished American academics and the U.S. Defense Department” (Wakin 1992, p. 1).

The student activists sent their documents to anthropologists Eric Wolf and Joseph Jorgensen, who were, respectively, chairman and member of the AAA’s Ethics Committee. The two anthropologists went into action mode and wrote four of the implicated anthropologists letters that by all accounts were quite threatening, while stating that the “announced purpose of the Ethics Committee is to deal with cases on as anonymous a basis as possible, in an effort to develop an approach — without penalizing any individuals” (Jorgensen and Wolf 1970, p. 2). Jorgensen and Wolf published an exposé of the meetings on Thailand attended by Moerman and the others in the 19 November 1970 issue of the *New York Review of Books*, and then the whole world “knew”.

Once the Student Mobilization Committee had come into possession of the documents, it “held a press conference in
Washington which was covered by the New York Times in a confusing report” (Jorgensen and Wolf 1970, p. 2). Jorgensen and Wolf indicate a battle for the soul of anthropology, but they assert that theirs is a “revolutionary discipline, which, in the tradition of Montaigne and Rousseau, radically questioned the pretensions to superiority of Western civilization, while seeking alternative visions of man” (ibid.). As they establish the discipline’s credentials, they note that “anthropologists condemned the assault of the American government on American Indians” and only parenthetically state that “the ‘solutions’ [suggested by these anthropologists] were not, and perhaps could not have been, better than those from any other source [at the time]” (ibid.). Acknowledging that anthropologists one hundred years earlier had been caught in the blind spots of their own times, the two activist scholars suggest no such awareness of themselves or their contemporaries.

On the theft of the files and the SMC’s having gone public with them, Jorgensen and Wolf are simultaneously coy and righteous as they assert their own professionalism in recycling allegations from the stolen files.

We regret this action, and would certainly not have taken it ourselves, nor would we have encouraged anyone else to do so. But the documents seemed to us of such significance that, while taking care to protect the names of those mentioned, we none the less felt compelled to pursue the questions raised by them because of our concern for the integrity of our profession (ibid., p. 3).

Near the end of their long piece, the two anthropologists indicate that they wish simply to wash away the colonial legacies that they associate with their discipline’s origins. I am a little mystified by their implied biologism, by the idea that traceable origins or conception determine the identity of anthropology. This view is all nature and no nurture or history.

Admittedly, anthropology was ambiguously conceived. Now, in our view, it must disengage itself from its connection to colonial aims or it will become intellectually trivial. The future of anthropology, its credibility, depends on sustaining the dialectic between knowledge and experience. Anthropologists must be
willing to testify on behalf of the oppressed peoples of the world, including those whom we professionally define as primitives and peasants. (ibid., p. 14)

This passage seems to suggest that the world has oppressors and oppressed, and that anthropologists should be the free intelligentsia that will testify on behalf of the oppressed. It may be that anyone not busily speaking up against oppression and the U.S. military was thereby suspect. Today, many of the alleged anthropological improprieties relating to Thailand would number among the activities of applied anthropologists and not cause a stir. But the times were not calm, and Jorgensen and Wolf implicated any not-explicitly-leftist researchers in Thailand’s countryside and singled out the Tribal Research Center in Chiangmai.

Although the documents copied by SMC make no reference to them, anthropologists have known for some time of the operations of a Tribal Research Center in Chiangmai, Thailand, which underwrites large convocations of scholars and other interested parties, maintains a considerable staff, has installed a computer, provides facilities for occasional users of their resources, and other amenities. [As] the Thailand papers show, the [U.S.] government is less interested in the economic, social, or political causes of discontent than in techniques of neutralizing individual or collective protest. As governments request more information of a particular kind, moreover, they are able to also furnish the technological devices to simplify the gathering of their one-dimensional data. Infrared photography and miniaturized microphones, for example, can help to provide exact descriptions of areas and people, while computers can be used to store and retrieve information on command. (ibid., pp. 8, 12–13)

Reality Check

By about 1975, New Zealand geographer John McKinnon joined the Tribal Research Center. He had wanted to expose the TRC’s sinister workings on the basis of inside knowledge.³ He was there for years, but uncovered no sign of the alleged computer, and there was not much going on other than research. The TRC was a Thai
government agency, and members of its staff were thus in no position to criticize government policy. But the centre was never involved in state violence. In 1969, both Australian and Thai members of the Tribal Research Center actively refused involvement with a U.S. Defense Department subcontractor that had set up a Tribal Data Center (Hinton 2002, p. 161). Jorgensen and Wolf cannot distinguish the Tribal Research Center, an actual organization, from the Tribal Data Center, which existed on paper as a potential organization but was intended as a counterinsurgency clearinghouse.

The inquiry by the AAA task force involved allegations of impropriety by named individuals.

The individual named was indeed engaged in the secret research described, but was neither an anthropologist nor a present or former member of the AAA. There was, however, a member of the AAA with the same or similar name, but that person was not a Southeast Asian specialist and had never set foot in Thailand. It was a clear case of careless sleuthing. (Davenport 1985, p. 70)

The episode left all the anthropologists involved with the centre and with research in Thailand more generally somehow implicated and affected in many ways. Thailand was perhaps close enough to Vietnam that American scholars and others could see themselves doing something important to change the course of an awful war and to make the world a better place by criticizing the activities of their colleagues there. In West Germany, students revolted in 1968 and came to critical positions on history and academics that lasted for years. “Protest against the Vietnam War combined with protests against the silence surrounding continuing local respect for old Nazis” (Gingrich 2005, p. 145). And one Austrian anthropologist was affected by this ferment: “In Vienna, museum director Hans Manndorff fell into public disgrace for alleged involvement with the CIA in Southeast Asia” (ibid., p. 149).

Manndorff had been hired as an advisor to the United Nations and the Thai government to lead a team conducting socio-economic research in the highlands of Northern Thailand in 1961–62 (Manndorff 1962, pp. 1–35; Manndorff 1967, pp. 553–56; Tannenbaum 2001,
Under international pressure, Thailand had declared opium illegal in 1958, and the United Nations was doing its part to make the Thai government enforce the law. Manndorff was not involved with the CIA in any way, but it is fair to suggest that he was as naive as were most anthropologists during the 1960s about the potential political misuse of their research findings. The most obvious explanation for Manndorff’s naiveté was that he was concerned with “urgent anthropological research” on peoples and cultures that were assumed to be disappearing following the onslaught of modernity, development and the like.4

Following the recommendations of Manndorff’s research team, the Thai government established a Tribal Research Center in 1965, and, with funds from the anti-communist Southeast Asia Treaty Organization, or SEATO, the TRC hired anthropologist William R. Geddes as its first director:

A footnote to the Thailand Controversy in Australia is that a small Australian journal reprinted an article written by Alfred McCoy in 1970 on the overreaction by the Thai military to population pressure, causing a group of Hmong to move. The journal apparently asserted that W.R. Geddes had been instrumental in the death of Hmong at the hand of the Thai military. Geddes sued the journal and won a libel that bankrupted it. (Wakin 1992, p. 199)

Geddes never fully recovered from the baseless assault on him; Wakin’s treatment of the matter considers only the lawsuit’s impact on the Australian journal’s finances. Michael Moerman, whose UCLA office had been burgled at the onset of the episode, never regained his previous standing as an academic. Gerald Hickey, who had worked for the RAND Corporation and other subcontractors in the Central Highlands of Vietnam, lost his position at the University of Chicago and never again held an academic appointment. Carrying the stigma of fabricated allegations of impropriety, many other scholars had a hard time getting published, getting research grants, getting jobs and being taken as colleagues.5

For the most part, scholars implicated in the crisis have not written about it, with the exception of Australians Peter Hinton (2002) and...
Douglas Miles (2008). I was briefly in email correspondence with Hinton around 2002 and knew that he was battling a very serious illness; the treatment often wiped away all his energy. Perhaps that brush with death gave him the strength to write about this difficult personal and professional crisis that had remained a no-fly zone beyond the terms of Wolf and Jorgensen’s case. Neither Eric Wolf nor Joseph Jorgensen ever had any familiarity with Southeast Asia; sometimes ignorance and disconnect help boost confidence.

Peter Hinton replaced Geddes as the director of the Tribal Research Center, which he led with a Thai counterpart during 1966–69. He first presented his account of the Thailand Controversy in Australia at a conference in 1991, and published it a decade later. I will not try to rephrase his important overview and contextualization, but will simply quote one passage.

For many years my students, as soon as they felt familiar enough with me to tread on what might have been delicate ground, would ask “what did you do in Thailand?” [At] the time of the controversy, the truth of the matter was made all but unreachable by the assumptions and obfuscations of those alleging malpractice. One basic proposition taken as axiomatic by many — not only on the left but many liberals as well — was that there was a clear-cut struggle in mainland Southeast Asia between progressive, liberating (communist) movements and reactionary, repressive (U.S. backed) forces. As far as Thailand was concerned, the fact that the military junta in power was allied to the U.S. was sufficient grounds to identify it absolutely with the authoritarian and incompetent U.S.-backed regimes of Ngo Dinh Diem and Nguyen Cao Ky in Vietnam, and Lon Nol in Cambodia. As Dell Hymes claimed [in 1974], in Southeast Asia at the time, the identities of the “bully and victim are clear to us”. (Hinton 2002, p. 170)

Hinton’s account and the quoted statement from Hymes give a clear indication of the binary frame that trapped most of those engaged with the issues surrounding the controversy and the place of anthropologists and their discipline.

Much of the agitation against alleged transgression by researchers in Thailand expressed American and Australian politics-at-a-distance. Hinton relates that many Western leftists considered the ostensibly
An indictment of the Khmer Rouge regime, written by a French priest who had lived in Cambodia for ten years and had interviewed many refugees from the Khmer Rouge regime, was “subjected to trenchant criticism by Noam Chomsky and others who had never been to Cambodia, but who considered themselves better informed on the area than he” (Hinton 2002, pp. 170–71). The less one knows, the surer the firing.

Anthropology’s Debate Land

In part, what I am calling the “Anthroversy” is just another example of contentious social life within the field of anthropology, where debates and disagreements are common. Service (1985) offers an overview of controversies in the discipline, but ends his history by 1960. By 1982, when I first came to anthropology in college, the work of Clifford Geertz served as a lightning rod. It was idealist and interpretive, and as such it was contrasted with the materialist and political or scientific work of Marvin Harris and Eric Wolf (Geertz 1973, 1980; Harris 1968, 1979; E. Wolf 1959, 1969, 1982). Scholars seemed to write past one another and have no common ground. Among the first controversies that I encountered was Derek Freeman’s attack on Margaret Mead’s work on Samoa, published only after Mead had passed away (Freeman 1983; Mead 1928).

Mead had been the icon of American anthropology in the public sphere for much of the twentieth century, while Freeman was a Southeast Asia specialist who had done work on the Iban of Borneo (Freeman 1955, 1970). His attack on her work and character was in many ways underhanded, and much of it was based on inflated charges, levelled in two books and several articles. Paul Shankman has been busy with the counter-attack on Freeman and in defence of Mead, in at least seven articles and one book. Shankman is the only one of the three still alive, but the “debate” continues (Shankman 2009, 2013).

When Freeman’s critique of Mead’s work appeared, debates regarding Clifford Geertz’s work were fading away. Benjamin White
(1983) has examined the changing reception of Geertz’s *Agricultural Involution* (Geertz 1963) over twenty years. The text of *Agricultural Involution* had first appeared in mimeographed form in 1956, and when the book was published the Western reaction was almost uniformly positive. The book came out in Indonesian translation in 1976, and met a less enthusiastic reception. By the late 1970s, Western criticism of the book emerged and over some years became almost unanimous. Some scholars engaged in serial attacks, to the point of “qualify[ing] for the title of chief Geertz-bashers” (White 1983, p. 18). Benjamin White finds nothing new, interesting, or even supportable in the book, and declares his puzzlement over why it “should have had any great influence at all” (ibid.).

If Geertz is no straw man, *Agricultural Involution* at least has become a rather tired punch-bag, an easy target for criticism which often goes no further than pointing to something wrong in the Geertzian picture, without proposing alternative views of Java’s agrarian transition in its place. Researchers might have more usefully applied theoretical advances made in the study of other agrarian transitions, rather than simply taking another bash at Geertz. (ibid., p. 29)

If such debates are not about analytical matters, but are rather a means for people to situate themselves in relation to political or social factions, then it is impossible to settle the debates over the work of Geertz and Mead in academic terms.

One further anthroversial example, from the early 1990s, concerns the debate between Marshall Sahlins and Gananath Obeyesekere regarding Hawaiian history. Did Hawaiians think of Captain James Cook as a manifestation of the god Lono (Sahlins 1981, 1985, 1994; Obeyesekere 1992; Hacking 1999)? Many academics came to hold strong convictions about this matter on what appear to be rather shallow emotional grounds. In Obeyesekere’s rendering, Sahlins’ claim was a recycling of the European colonial myth that the “natives” in the tropics took the Europeans for divinities. Sahlins’ case is, in my reading, a bit more complex and interesting. He notes, for instance, that there had never been any singular Hawaiian view on local or extra-local affairs. Chiefs, priests and commoners, and men and
women had different perspectives, engagements and agendas and came to different conclusions. Obeyesekere, assuming the role of one standing up for the Hawaiians against Orientalist stereotypes, ends up (in the minds of some of us, at least) homogenizing them as rationalists. Sahlins, who, some allege, turned the Hawaiians into a cultural Other, suggests much more complexity, historicity and systemic diversity in Hawaiian views than his critics recognize. To some of us, that lends his case considerable credibility. But the notion that a contemporary Western scholar had alleged that the natives took a white man for a god translates easily into (postcolonial, anti-Orientalist) outrage, and can as such become an attractive identifier.

Wording suited to public assemblies is in every way like scene painting, since the bigger the crowd is the farther off the view, and hence in both cases precise details are a waste of effort and make things appear worse. (Aristotle 2009, p. 270)

Regarding the Vietnam War era, one aspect of such dynamics of identification emerges in very clear moral geographies. People who side with the U.S. (or Australian) effort in the Vietnam War write of the Hmong in Laos — important U.S. allies — as natural freedom fighters, while those against the effort describe the Hmong as warlords and drug dealers (compare Hamilton-Merritt 1993; Castle 1993; Quincy 1995, 2000; McCoy et al. 1972). It seems very clear that the ethnologies of outside commentators are in some ways mimetic identifications with or in opposition to the peoples described, as well as claims to a particular political badge. “The Hmong have a 4,000 year old culture which treasures concepts of honor, commitment, loyalty, and freedom” (Hamilton-Merritt 1993 p. 1). Compare this description to the Meo (Hmong) who served in General Vang Pao’s Hmong militia in Laos, mercenaries and internal refugees who devoted their energies to growing opium to supply the international market for heroin because they did not have to farm rice; supply planes dropped rice to them in order to sustain the communities and the war effort, while other planes transported the opium out (McCoy et al. 1972, p. 277).
Mien Comparisons

The Mien — one of many so-called Yao people — of northern Laos lived under a chief who came into the Lao semi-royal title of Phaya Luang (Phya Long in Mien) in perhaps the 1870s and eased into retirement by the 1930s. His son then became district chief (tasseng in Lao) under the French colonial government of Laos. By 1958 the CIA had contacted this Mien leader in an attempt to form an ethnic militia, as it did with Hmong, Khmu and various other groups. The Mien population had been living in the mountains near Luang Namtha, Muang Sing and Phongsali in northern Laos, but, as the communist Pathet Lao forces gained control over the northeast border regions of the country, these people had to leave. For they would not join the communists.9

It does not seem that people’s decision to side with the Lao royalists or the Lao communists was a matter of political ideology. Rather, the Mien leader at the time had a second-generation link to the royalist authorities, and the Pathet Lao acted with considerable hostility to that side, as the royalists had themselves acted towards their own opponents (see Tappe 2010, pp. 238–40). The Mien had strong connections to the royalist government. In contrast, the Lanten, another Yao group in the same area, had only minimal links to that government and none at all to the opium trade. They later came into a rewarding relationship with the Pathet Lao (Badenoch and Tomita 2013, pp. 57–59). Further, the Mien under the Phya Long represented only one segment of the Mien population in Laos. Another segment followed the Hmong leader Vang Pao, and yet others had no connections to militias during the war. These divisions were identified as Northern, Central and Southern Mien (Jonsson 2009, p. 137; Jonsson 2014, pp. 68–69).

The Mien population had always inhabited a multi-ethnic environment. Its leaders made contracts with lowland Tai rulers, prosperous farmers would hire Akha labourers to work in their fields, and there were neighbouring Khmu, Black Tai and others. People made certain ethnic distinctions and at the same time frequently crossed ethnic boundaries. After the Mien population fled the northeast...
region of Laos, it settled near the Mien militia in the area around Nam Keung, near Huai Sai, where the Mekong River forms a border with Thailand. This lowland area was multi-ethnic. Its population included Lao, Black Tai, Khmu, Akha, Lahu, Chinese and others, and businessmen from the Thai side of the border were involved in logging on the Lao side. There was no way to seal the ethnic boundary socially, but during the witchcraft scare described below the militia tried to seal it spiritually. One indication of the boundary is that my contacts in settlements affiliated with the Mien militia refer to anyone on the communist side as *jan* (“alien”, “non-Mien”; *janx* in Mien; see Jonsson 2014, pp. 69–70).

As the war intensified in the late 1960s and after, more people took to wearing Buddha amulets for some measure of protection and invulnerability (Jonsson 2009, p. 140; compare Ruth 2012, pp. 139–46), even if Mien themselves did not practice Buddhism. That is, there was considerable inter-ethnic traffic in religious and other ideas related to the war. Hmong General Vang Pao had detailed knowledge of Buddhist amulets, which he apparently gained from an ethnic Lao colonel in his militia (Warner 1996, p. 170).

Several children had died from illness in the process of the Mien resettlement to Nam Keung, about a week’s journey by foot from their previous mountain home areas. At least some of the deaths were attributed to the old-tiger spirit. “No Iu Mien will become [a host-body for the] *lau-hu-gwe* [old-tiger spirit]. It is the Akha, Lahu and Lue”¹⁰ — other ethnic groups in northern Laos and neighbouring areas of China, Burma and Thailand. Children in particular might become victims of this spirit, but Iu Mien people were by (ethnic) definition never its host.

The *lau-hu-gwe* was not a matter of any apparent concern in the Iu Mien settlements in which I conducted my previous research in Thailand.¹¹ Only once did I see a spirit medium drive such a spirit from an afflicted child. At that time, there was no search for a resident witch to hold responsible for the illness, and the treatment was analogous to how people would drive off a “wild spirit” from a household.
Because the events in Nam Keung in Laos came to focus on Mien people who had been adopted as children from other ethnic groups, I must note that there is no indication from my own research on the Mien or from that of any other scholar that there has ever been an anxiety that such children might harbour dangerous spirits (Jonsson 2005, pp. 73–94). The fears of witchcraft, especially the fear that this spirit was endangering the lives of children, manifested social collapse at a very special moment. It was a moment in which a militia — like these fears, another phenomenon with no precursor in Mien history — saw its duty as guarding the ethnic boundary between Mien and others with violence, something that in fact could not be done. When I first learned about the old-tiger spirit, in Thailand in the early 1990s, there was neither fear of witchcraft nor anxiety about the ethnic boundary or about people who had been born into other ethnic groups and later adopted into Mien households. In the witchcraft-fear episode during the war in Laos, in contrast, all those elements figured in the militia’s use of prisons and weapons to guard the ethnic boundary.

One Iu Mien woman in Nam Keung was accused of hosting an old-tiger spirit that had made a child sick and caused the child’s death. Some Tai Lue people suggested that the woman be let go on condition that she lived far away. At a village meeting among the Mien in settlements identified as Nam Keung, people decided to give her an ultimatum requiring her to leave the settlement. Her husband was at the meeting, and he was to convey to her the notice of eviction. If she would not leave, then Mien militia leader Chao La would send his henchman, Fu Tsing, to arrest her. The woman had children and did not accept either alternative, to leave or be arrested. Others in the village did not think the husband and children afflicted, as victims or hosts. The accused woman took her own life the night of the village meeting, and the husband later remarried. Another Iu Mien woman was accused of hosting the old-tiger spirit. She promptly fled and was never heard from again. The latter woman did not have any children. I did not learn if she was married.
Ideas of *lau-hu-gwe* did not emerge for the first time during the war. Nor is the notion specific to the Iu Mien peoples. It resonates for instance with Chinese ideas of evil spirits or *gui* (Diamond 1988, pp. 1–10). Iu Mien people had learned some of the remedies for witchcraft that they employed from Tai Lue neighbours, and in some Iu Mien- and English-language conversations about the episode people used the Tai Lue term for witches, *phi-pop*, when clarifying my questions about *lau-hu-gwe*. But as much as there was a regional, inter-ethnic continuity in ideas about witchcraft spirits, the form that they took reflected an anxiety about ethnic boundaries that had emerged in the context of war and was reproduced in relation to an ethnic militia leader’s command over “his” people.

Another alleged incident of witchcraft in Nam Keung during this same period was attributed to an ethnic Tai Lue noodle vendor. Someone supposedly overheard this woman wonder why one boy had not paid her, and her alleged comment was brought up after the boy had become gravely ill. But no action was taken against this woman. Policing against witchcraft was limited to the Iu Mien people, and it took the form of occasionally ruthless acts of imprisonment, beatings and expulsion by Chao La’s henchmen (ibid.).

Endangered Ethnic Groups, Endangered Academic Disciplines

This case of fears of witchcraft among the Iu Mien of Nam Keung around 1970 is the basis of the comparative perspective on which I wish to draw for an understanding of the so-called Thailand Controversy. In both episodes, common elements had never before come together in such a configuration. The purity of anthropology, in the sense of a clear isolation from social entanglements in wartime, had not been an issue during the Second World War (Yans 1986, pp. 208–14), and there are no apparent traces of anxiety over Edmund Leach’s research with the Kachin of northern Burma (Leach 1954), conducted while he operated a Kachin regiment fighting against Japanese forces during that war. The old-tiger spirit was not a new idea, but it had not earlier been anchored to an anxiety regarding
the ethnic boundary. But neither had there ever before been a militia identified with the Mien ethnic group.

The Mien militia did not make up the witchcraft scare at Nam Keung, but they took advantage of it in ways that identified them with the core values and well-being of their ethnic group. Something similar can be suggested for the Thailand Controversy. It was not made up by Eric Wolf and Joseph Jorgensen, but they took advantage of the apparent scandal to align the disciplinary identity of American cultural anthropology and its national professional association, the AAA, with opposition to the Vietnam War and by extension to any involvement with the U.S. military. The sense of atrocity, that anthropologists were feeding military agents with information that could be used for the bombing of villages, was insinuated. Nothing of the sort took place, but the insinuation was enough to trigger outrage through which people could come into identity and knowledge grounded in emotion.

In Nam Keung, Chao La’s henchmen threatened any marginal insider who came under suspicion, but they could do nothing regarding the non-Mien peoples perceived as in some cases more dangerous than those marginal insiders. The outbreak of witchcraft occurred at a time when there was no way of sealing social boundaries, and many of the remedies to the outbreak drew on traffic across those boundaries. We can say much the same of the Thailand Controversy. The attempt to ensure the purity of anthropology drew on links to the media, student activists and anti-war ferment, each of which in turn drew on the war to fuel their enthusiasm and indignation. Wolf and Jorgensen are in some ways the equivalent of the Mien militia’s moral guardians and henchmen in catching the offenders and insisting on the guarding of the ethnic or disciplinary boundary.

Like Mien militia leader Chao La’s henchman Fu Tsing, Wolf and Jorgensen acted to defend collective identity from external threats of contamination. Ostensible transgressors were named, through the enthusiastic national media and at meetings of the AAA, to an audience that wanted assurance in its understanding of American involvement in Southeast Asia in clear moral binary terms. The atmosphere of
antagonistic binaries allows for no complexity, internal diversity or hybrids. There are only two kinds of people in the world: “The Mead committee’s report [on the alleged improprieties in Thailand] in essence chastised the ethics committee while vindicating those originally charged. At the next meeting of the [AAA], the membership roundly rejected Mead’s report” (Silverman 2005, p. 290).

While the war in Laos has never received as much attention as the Vietnam War, what little has been published tends to play up moral binaries, particularly regarding the Hmong. Authors who identify with the U.S. war and its allies depict them as noble and heroic freedom fighters (Hamilton-Merritt 1994, p. 1; Castle 1993, pp. 136–37). Those who position themselves against the U.S. war and its allies describe the Hmong as sinister mercenaries and drug traffickers (McCoy et al. 1972, pp. 264–315). Such works play to readers’ wish for a perspective on U.S. wars abroad grounded either in emotional identification with the attractive Other or in opposition to its repulsive counterpart. The same is true in the case of the ostensible Thailand Controversy, it would seem. People gain an easy grasp of noble heroes and their repulsive counterparts through a binary drama that plays itself out as scholarship and a matter of “just the facts”.

National Anthropologies

In the absence of broad familiarity with the international history of anthropological research in Thailand, it may be appropriate to call some attention to the very “dangerous” work done by Austrian scholar Hans Manndorff and his associates in the mid-1960s. In 1961–62, Manndorff led an eight-month study sponsored by UNESCO and the Thai government whose goal was to make recommendations regarding opium suppression and national integration (Tannenbaum 2001, p. xv). He did not have any particular knowledge of Thai society, language or history, as his previous work had been in various parts of India (Heine-Geldern 1955, p. 627). In 1964-65, Manndorff was one of three German-speaking scholars who, with funding from the Volkswagen Foundation, made fifty-four short
ethnographic films among Hmong, Lisu, Akha and Lahu peoples in Tak and Chiangrai provinces. The films were edited by 1968, and in 1974 articles to accompany each of them appeared in *Encyclopedia Cinematographica*, the journal of the Institute for Scientific Film, in Göttingen, Germany (see Trupp and Kosita 2009, pp. 153–61).

I viewed the films and the articles at the Thai Film Archives, in Salaya on the outskirts of Bangkok, in the summer of 2012. Some affiliates of the then-defunct institute in Göttingen were trying to spread knowledge of their important work, and they had donated two sets of these materials to the Sirindhorn Anthropology Center, in Bangkok. They also wished that DVD versions of the films be sent to the villages in which they had been recorded. The films, of between four and twenty-two minutes in length, each focus on a particular activity, and most are silent. They have titles such as “Plaiting a basket with a lid”, “Fitting up a cotton rack”, “Fluffing and fulling cotton”, “Men’s dances” and “Funeral of a boy”.

The titles say it all, so to speak, in that there is no interaction in the films, only ostensibly ethnic activities. In many ways the films reflect the naive empiricism of Western anthropology as it tries to capture traditional activities before the tidal wave of modernity, capitalism, the state or whatever it is that we put in our monster slot sweeps them away. Margaret Mead (1975, pp. 3–5) recommended such salvage ethnography as the major contribution that film could make to anthropology. Film was more accurate and free of observer bias than any written record. It could be revisited and restudied as analytical or theoretical winds changed.

In the early years of the twentieth century, German-language anthropology had been top-notch international scholarship, but Nazi-era purges of non-compliant scholars had a devastating impact (Gingrich 2005, pp. 111–48; Penny 2008, pp. 81–95). During the Nazi era, anthropologists were integrated into the racist national project, and there was practically no revamping after the war. Most professors of anthropology at German universities held on to their posts. This history motivated students to try to achieve some change by going after Manndorff, in an act that may have been as lame and counterproductive as some during the Thailand Controversy.
As Gingrich (2005, pp. 137–53) relates this history, it was not until the 1970s that German-language anthropology shed its analytical isolation with the arrival of a new generation of scholars at universities and in museums. The films recorded in Thai villages in the 1960s project the condition of German-language anthropology at the time more than they capture anything of interest in the villages or in Thailand more generally. These are parallel universes, and it takes time to notice some of their intersections.

The enthusiastic pushing of the allegations on the part of Wolf and Jorgensen that gave rise to the Thailand Controversy was in many ways a reaction to the escalating horrors of U.S. wars in Southeast Asia by 1970. Robert Heine-Geldern’s push for “salvage anthropology” by the early 1950s was a different emotional reaction to a different war. During the Second World War, anthropology had been made to serve various Nazi agendas of ranking, classifying and exterminating particular peoples (see Gingrich 2005, pp. 119–34). Robert Heine-Geldern had been in exile in New York during this time. His writings from the period after his return to Vienna make the history of his discipline’s wartime entanglement disappear from sight, such as in his overview of post-war anthropology in Austria and Switzerland (Heine-Geldern 1955, pp. 619–49) and, more strikingly, in his overview of a century of ethnological theory in German-speaking countries (Heine-Geldern 1964, pp. 407–16). Instead, he issued an “S.O.S.” for urgent anthropological research on peoples who were seen as culturally or otherwise endangered (Heine-Geldern 1959) and has remained best known among Southeast Asianists for his *Conceptions of State and Kingship in Southeast Asia* (Heine-Geldern [1942] 1956).

I do not point an accusatory finger at Heine-Geldern, but I think that there is much to learn from the trajectory of his scholarship, from the apparent blind spots and from the emotional thrust of his “S.O.S.” regarding disappearing peoples. I suggest, however, that in some ways anthropology can make us blind to certain things. One further example is French anthropologist Jacques Lemoine, who studied the Hmong in wartime Laos during the 1960s and early 1970s. He wrote about Hmong culture and social life in ways...
that made the war completely fall from view (Lemoine 1972). In a British television documentary on the Meo (Hmong) released in 1972, as part of the “Disappearing Worlds” series, we see Lemoine with Hmong villagers who are doing their best to stay away from the war, even to the point of hoping to spread the rumour that the men were all opium addicts and thus of no use to the militia agents who were looking for new recruits (Moser and Lemoine 1972).

But writing on the Mien (Yao) subsequent to this time, Lemoine depicts their culture as endangered by “zealots” of a Christian or communist bent who are intent on destroying their spirit-religion paraphernalia (Lemoine 1982, p. 7) and argues that their society was historically endangered by antagonistic policies of the Han Chinese who intended to make them “naturally disappear” through extinction in their mountain isolation (Lemoine 1991, pp. 600–601). It seems to me that Lemoine is able to write about ethnic minority endangerment in relation to Christian missionaries, communist armies and governments and the ancient Chinese empire, but that he has never managed to write about the actual dynamics and changes that he observed first-hand among the Hmong over the course of the war in Laos in 1962–75. I do not point an accusatory finger at Lemoine either, but I think that his example is useful in suggesting the issues that anthropologists as individuals may and may not be capable of conceptualizing and reflecting on. I make the point in part because I have had to reconstruct the war’s impact on the Mien from historical recollections almost forty years after the fact, as the available written sources variously ignored the context of war or sought to glorify or demonize the Hmong militia.

War and the perception of a fundamental threat take people’s judgement away, rob them of their ability to recognize the humanity of select others. In part, this effect may be a case of what Zygmunt Bauman (2000, p. 64) calls heterophobia. The apparent consensus within anthropology about what exactly the Thailand Controversy was — that certain American and other anthropologists in Thailand were colluding with U.S. and Thai military interests and providing information that might be used for the bombing of villages (Wakin 1992, pp. 137–41; Berreman 1991, pp. 43–49) — is wrongheaded,
based on lies, fabrications and a burglary. It precludes recognition of the damage done within the field of anthropology. I cannot change the way that these things played out, but by suggesting that we view the controversy as a particular accident of intersecting realms at a highly specific historical moment I hope to make less likely its replay in new form.

The shape of the Thailand Controversy is profoundly American — meaning, of the United States. It represents a re-enactment of a pattern set in motion by the anti-communist witch-hunts of the McCarthy era.

McCarthyism helped divide Americans into oversimplified categories of “loyal followers” and “enemies of state.” All those who did not consent to support a gamut of policies ranging from an arms policy of mutually assured destruction to America’s mid-century bigoted system of racial, gender, and economic stratification were fair game for the unchecked scrutiny of loyalty tribunals. But the magic of McCarthyism hid from view the essential fact that protest, dissent, and Socialist and Communist activism all had long, rich American traditions that could not easily be enveloped under the dismissive heading of foreign threat. (Price 2004, p. 9)

The process at work in the Thailand Controversy was a heterophobic denial of internal diversity and complexity. But it was also a case of the sufferers of McCarthyism replicating McCarthyite interaction patterns. Gregory Bateson labels such dynamics “schismogenesis”: the creation of divides, or “a process of differentiation in the norms of individual behavior resulting from cumulative interaction between individuals” (Bateson 1958, p. 175). He draws a contrast between complementary and symmetrical schismogenesis. The former involves divergence, in which one side becomes more assertive and the other more submissive, and the latter increased similarity, in which boasting by one side leads to more boasting by the other, and so on.

Eric Wolf comes across in the Thailand Controversy as employing very much the same tactics to which the agents of U.S. national security had subjected many a leftist academic, and this parallel calls for a brief look at history. When university professors started to organize teach-ins as a way of educating the public about Southeast
Asia and the U.S. wars there, Wolf and many others came under FBI scrutiny and surveillance.

Because of anthropologist Eric Wolf’s involvement in the first Michigan teach-ins and the national teach-in, the Detroit FBI field office notified J. Edgar Hoover that Wolf and approximately eight others were affiliated with the Inter-University Committee for Debate on Foreign Policy. The Detroit field office notified headquarters that although it had information on a number of the individuals affiliated with this group it had only basic information on Wolf, and a request was made to “initiate a limited investigation” of Wolf and other individuals associated with IUCDFP. (Price 2004, p. 335)

The intimidation, arrests and blacklisting of the McCarthy Era and its precursors were revived and transformed in the context of campaigns during the 1960s and early 1970s for civil rights and of protests against American military involvement in Vietnam and Cambodia. No civil discussion about social and political problems and their possible solutions was possible, when the issue of the war was seen as a litmus test of national loyalty. My language here is perhaps overly dramatic, but I choose it with the intimidation of those American academics involved in the teach-ins about the wars in Southeast Asia during the late 1960s in mind (see Price 2004, pp. 333–40; Sahlins 2009, pp. 3–5). When there could be no calm discussion, and in the absence of any sense of politics as a quest for the accommodation of diverse interests, combativeness and the naming and shaming of transgressors won out. Exposing the evils of the other side became the way to connect, act and communicate. In my view, as someone too young to have been there and as someone brought up in another country, the context poisoned all social life at the time.

The old-tiger spirit episode in the Mien militia settlement and the Thailand Controversy each express the collision of many separate fields and forces, at particular moments of social collapse. The Mien people do not cultivate the memory of the witchcraft fears, but that memory came up in some people’s conversations with me. In some ways I had become a magnet for historical recollections of various
kinds, and was then left to figure out on my own how the pieces added up and what stories they told. It is very different with the Thailand Controversy, which many have considered as a matter of established facts.

What the two episodes have in common is the anchoring of collective identity — of anthropologists in a national association and of an ethnic group identified with a militia — to outside threats at a moment of crisis. For the most part, Mien engagements with spirits have been at the levels of household and village. The old-tiger spirit episode is the only case that I know in which Mien have treated spiritual well-being as a matter of ethnicity. It is the only setting that I know in which a militia guarded the Mien ethnic boundary. Something similar took place in the Thailand Controversy, in which it seems that certain scholars took any engagement with U.S. agents of rural development and anti-communist organizing as a threat to the credentials of the discipline.

In part, we must understand this latter episode as a response to lasting and widespread political suppression in the United States. But it is also a telling indicator of a particular trend in mainstream cultural anthropology in the United States, of a reverence for university-based academic and theoretical work that is disengaged from society or practical applications (Greenwood 2008, pp. 323–29). The willingness of Australians William Geddes and Peter Hinton to lead the Tribal Research Center with Thai counterparts suggests that they did not have the same distrust of engagement with government agencies.

The national shaping of anthropology in the United States, through the AAA, is uncommon. There is no analogous dynamic in German, French, Australian, British or Japanese anthropology. Debates among scholars in these other countries do not emerge as battles for the soul of the academic discipline. The Thailand Controversy is very much a case of border anxieties regarding ritual uncleanness, of the sort that Mary Douglas has studied in *Purity and Danger* (Douglas 1966). In Douglas’s study of the Old Testament, she finds that ideas regarding purity were not a fundamental concern in daily life, but rather came to the fore in
two settings. “The culture of the Israelites was brought to the pitch of greatest intensity when they prayed and when they fought” (Douglas 1966, p. 64) — that is, in the temple and during wartime. Walter Irvine’s study of Thai nationalism and Thai notions of mental illness suggests, in a similar fashion, that during the 1970s the predominant Thai understanding of the nation was that of a “bounded entity under threat” (Irvine 1984, pp. 44–77). This notion helped justify the widespread and violent official suppression of internal diversity and of any agitation for the rights and interests of farmers and labourers.

In Thailand, local academic anthropologists were not involved in research with the ethnic minorities in the hills. There, only government researchers in occasional collaboration with foreign scholars were involved. This division may have contributed to the long-time isolation of “tribal” studies from other social sciences in Thailand, paralleling the general isolation of highland peoples from Thai society in the twentieth century. The clear separation of government research and academic research was not inevitable, but as both “tribal research” and academic studies took hold they did so in ways that entrenched mutual distrust between university-based scholars and government researchers.

The Tribal Research Center was placed within the campus of Chiangmai University, but it was administratively, intellectually and otherwise firmly cut off from whatever research, conferences and publication that occurred at the university. It seems quite likely, though I have no proof, that if the Thailand Controversy came to anyone’s notice in Thailand it simply furthered the divide between academic and applied research, and that this divide among Thai researchers lasted until the Tribal Research Institute was officially dissolved in 2002 (see Kwanchawan 2006).

Reflections

The so-called Thailand Controversy expressed a reaction to the Vietnam War and to a legacy of political repression within the United States. The charges of impropriety were inflated but drew their force
from an attempt to wield a national academic organization for purposes of political debate in ways that stifled civil disagreement and instead rested on outrage over supposed transgressions. My comparison with an outbreak of fears of witchcraft among the Mien in Laos during wartime is perhaps unconventional among attempts to historicize anthropology, but it seems that the two episodes share elements of moral panic and the jealous guarding of a social boundary.

The national association of anthropologists in the United States has occasionally been wielded for political agendas, in ways that do not reflect the diverse fields in which American anthropologists work. The anxieties of the Vietnam War era stand in sharp contrast to the atmosphere of the era of the Second World War. They also illustrate dynamics unlike the national dynamics of anthropology in France, German-speaking countries, Australia and Thailand.

Emotional identification carries certain risks, but it appears rampant in scholarship on Southeast Asia, as in the divergent typecasting of the Hmong of Laos as noble freedom fighters or as ignoble mercenaries and drug traffickers. The Thailand Controversy suggests a variant on this dynamic, in that people have been offered a way to come into knowledge and identity through outrage at putatively improper engagements on the part of Western anthropologists in Thailand. In Thailand itself, the episode is likely to have accentuated a divide between academic research and government research that lasted for at least forty years.

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NOTES

1. Here I extrapolate from what I have learned about these histories. What made the events striking to me, as an ethnographer of the Mien, is that I had only known of concerns with the spiritual boundaries of households and occasionally villages, never regarding the ethnic group.

2. Note that page numbers in all citations to Jorgenson and Wolf 1970 refer to the 14-page printout of the online version of this article cited in the references.


4. Robert Heine-Geldern, an Austrian anthropologist who had gone into exile in New York during the Nazi occupation of his country was a leading figure in this “urgent anthropological research” on disappearing peoples. Heine-Geldern had issued “An S.O.S. of ethnology” regarding “the extermination of whole tribes, the complete change or total destruction of non-European cultures” in 1952 at the Fourth International Congress of Anthropological and Ethnological Sciences, held in Vienna (see Heine-Geldern 1957, pp. 281–91; Heine-Geldern 1959, p. 1076). Such research had funding from UNESCO. In the 1950s, Hans Manndorff had done research in India, apparently — both institutionally and for the purposes of publication — under Heine-Geldern’s wing (see Heine-Geldern 1955, p. 624). In 1962, Heine-Geldern published “A Proposal for the Establishment of a Tribal Research Centre in Thailand” (Heine-Geldern 1962). Hans Manndorff held on to his position at the Vienna Ethnology Museum until he retired in the early 1990s. In 1969 (prior to the controversy in the United States), Manndorff had come under harsh criticism for alleged involvement with the CIA at a meeting of German-speaking anthropologists and was not welcome at their meetings afterwards (Andre Gingrich, personal communication, 7 March 2014; Helmut Lukas, personal communication, 7 March 2014. Andre Gingrich and Helmut Lukas are both with the Institute of Social Anthropology at the Austrian Academy of Sciences). These were, it seems, generational political divides within German-speaking anthropology at the time (see Gingrich 2005, pp. 127–53).

5. Beyond the accusatory terms set by Jorgensen and Wolf (1970), the supposed affair remains one of the unspeakables of American anthropology. I draw here on A. Thomas Kirsch (personal communication, 1 April 1991), John McKinnon (personal communication on several occasions in 1994 and 2003), and other colleagues who have asked to remain anonymous.

6. Dell Hymes was a linguistic anthropologist who edited the book Reinventing Anthropology (Hymes 1972), which for many served as a call to action regarding politicized cultural anthropology.

7. See Stocking 1989, pp. 254–69, on Freeman’s critique and his career.
8. Clifford Geertz himself responded to the critique of this book, suggesting that it was overly determined by the rival convictions of neo-Marxist mode-of-production analyses on the one hand and, on the other, by assumptions about economically maximizing individuals. Each set of critics, he alleged, also failed to situate this book in the context of his other studies of Javanese society (Geertz 1984, pp. 511–24).

9. I summarize from material gathered during various interviews in Portland, Oregon, and in Redding and Oakland, California, from November 2005 to September 2011; a more thorough account appears in Jonsson (2014, pp. 53–72). So far, the only source on Mien in the war in Laos has been McCoy et al. (1972, pp. 265–302), which mentions them briefly in the context of CIA machinations regarding militia formation and the opium trade.

10. Field notes, Portland, Oregon, 10 December 2007. Most of my contacts asked to remain anonymous when speaking about this matter.


12. In contrast, lowland Shan and highland Lisu notions have it that contagion results from living together or eating repeated meals with someone who has a witchcraft spirit (Durrenberger 1993, pp. 50–51; Tannenbaum 1993, p. 67).


15. The three scholars were Hans Manndorff, Klaus Volprecht and Friedhelm Scholtz, assisted by cameraman Hermann Schlenker. The films have been distributed both individually and in the form of a set from Encyclopaedia Cinematographica, 1952–1995. One set has been deposited with the Human Studies Film Archive at Pennsylvania State University, which was the American distributor for the Institute of Scientific Film in Göttingen, Germany. The Institute in Göttingen made films across physics, natural science and ethnology. Its ethnological film-making was devoted to the salvage model of capturing disappearing ways (see G. Wolf 1967, pp. 113–63).
16. The term “monster slot” is my invention and is meant to suggest the ways in which scholarly analyses sometimes rest on unstated notions of causation, on that easy assumption that monsters like “modernity”, “capitalism”, “the state” and the like “explain” how the world takes shape through the destruction of community, tradition, moral economy and the like. My terminological invention is meant to echo “the Savage Slot” that, Michel-Rolph Trouillot (2003) suggests, enabled the notion of “the West” as an entity. Ideas of “the Savage” were shaped in relation to a binary contrast of “Order” and “Utopia”. It was the “primitive”, “uncivilized” or “traditional” Other that facilitated understandings of the Euro-American West as modern, rational and so on, and that thus justified colonial-era violence, slavery and more. The concept predated but informed the establishment of anthropology as a scientific project. I do not contest Trouillot’s term. Instead, I agree with his critical analysis and share his conviction that avoiding certain terms is not enough for the reshaping of anthropology. If efforts towards reshaping anthropology are confined to a critique of the texts of earlier generations of scholars then we may, in spite of our intentions, “morosely preserve” the logic of a racist enterprise (Trouillot 2003, p. 28). In relation to my case in this article, the Thailand Controversy is one such monster slot that allows for easy (unexamined) convictions about the world and its peoples, convictions based on the social categories and animosities of the Vietnam War era and of the Cold War more generally. The idea that modernity (however defined) was threatening the cultures of Thailand’s hill tribes — the idea, that is, which motivated the German film project — completely brushed aside issues of how “hill tribes” as an object of study were entangled with Cold War nation-building, and with how anthropology was entangled with international relations. I do not suggest that, had I been on the scene in the 1960s, these things would have been obvious to me. Instead, my point is that this history of scholarly blind spots is important. The resultant, more balanced sense of history may enable us actively to resist taking the apparent moral high ground and instead recognizing that scholarship, caught up in factional disputes, can leave people blind to their social conditions.

17. See note 2, above.

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