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Modern Tibetan Studies and the Issue of Substitutive Ethnography

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I Where the Issue Lies: Two Trends in Tibetan Studies?

The words “modern Tibet” represent something that is so complex and diverse that it confounds simple-minded observers, and this diversity has for many years hampered the establishment of Tibetology as part of regional studies in the broad sense of the term. This paper aims to undertake the work of probing the actual structure of this diversity. Specifically, while reflecting on the history of Tibetan studies from the standpoint of social science, I hope to accomplish my objective by broadly positioning such a history in the context of modern Tibetan studies. Anthropology has played a unique role in studies on modern Tibet, and one task of this paper is to use the aforementioned process to appraise and contextualize its role.¹

We will begin by using the question raised by Geoffrey Samuel, a renowned scholar in Tibetan studies, as a springboard. Samuel points out that anthropological studies of Tibetan society rely on ethnographical studies of Tibetan communities in the Nepalese Himalayas (the southern foothills of the Himalayan mountain range). He argues that this situation results in many anthropologists focusing their research on the Sherpa people, and upholding them as representatives of the Tibetan people. Samuel has referred to this research trend as “Sherpa-centrism,” and he has contrasted this with the “Lhasa-centrism,” which is the trend among political historians [SAMUEL 1993. 1994.] Lhasa-centrist studies refers to historical studies that seek to shed light on the true state of society in central Tibet under the former Dalai Lama regime, which collapsed in 1959. What is most intriguing is the fact that in the process of explaining this dichotomy, Samuel uses the expressions “Sherpa-centric anthropologists” and “Lhasa-centric textual scholars” [SAMUEL 1994:700]. In other words, Samuel ultimately presents this dichotomy as a dichotomy of anthropologists and textual scholars. It is certainly true that in Tibetan studies, as in many regional studies, there exist two contrasting academic trends: the trend followed by social scientists, who place weight on field surveys, and that followed by textual scholars, who place weight on textual studies. It is, in fact, standard practice among many exemplary reviews on Tibetology to begin by discussing the importance of this distinction.² The issue that Samuel presents is itself extremely useful, and I have no objection to the terminology of “dichotomy” itself.³ The real issue is that Samuel considers Lhasa-centrist research as the work of textualist scholars. The areas of study that actually make frequent use of Tibetan texts, aside from Buddhism studies, include ancient history and medieval history. These research areas fall outside the framework of Samuel’s schema, which primarily assumes modern research. Thus, the Lhasa-centric research that Samuel mentions refers to the history of modern Tibet. However, as far as I am aware, Lhasa-centric studies, i.e. the history of modern Tibet, is a research area that is led not by historians, but by anthropologists. It is therefore my assertion that the two trends of Sherpa-centrism and Lhasa-centrism did not belong respectively to the two divergent traditions of anthropology and history, but were in fact two trends *within* anthropological Tibetan studies. In fact, this is not an argument that requires much debate. Researchers such as Melvyn Goldstein, whose work [GOLDSTEIN 1989a, 2007] is said to represent the pinnacle of Tibetan modern history studies, and Tony Huber, who produced the

outstanding monograph on social history of popular pilgrimage in era of the Dalai Lama regime [HUBER 1999], belong, institutionally speaking, in anthropology.⁴ Thus, the fact that it is anthropologists who have led the way in the study of the history of modern Tibet is incontrovertible. However, we are at this point presented with an enigma. Why is it that a field of modern historical studies has been led by anthropologists as opposed to historians? From this point on, I will address this question while reflecting on the history of Tibetan studies.

II The Difficulty in Conducting Field Surveys in Tibet

While we can no longer cling to the quaint image of “the forbidden kingdom of Tibet,”⁵ it is nevertheless true that twentieth century Tibet did not make it easy for outsiders to gain access. It is known today that Tibet’s famous closed-country policy was, at the beginning of the twentieth century, a product of international politics, including the central and south Asian policies of the Qing Dynasty and then those of the British Empire.⁶ Despite the country being closed off, there were a handful of outsiders who managed to gain access and conduct surveys. For example, there were exceptional researchers, such as Tucci and some high-ranking colonial officers of the British Raj.⁷ However, with the signing of the 17-Article Agreement in 1951, Tibet was formally placed under Chinese rule. As a result, Tibet became even more sealed-off from the world than it had been previously. Despite the considerable qualitative difference between the “isolationism of the mysterious kingdom” that existed before 1951 and “Mao’s bamboo curtain,” from the point of view of outside researchers, there was no major change to the fact that they could not enter the country. While the context had changed, the severe restriction on academic access to Tibet was a constant. In later years, China enacted open-door policy that also encompassed Tibet. Accordingly, in the 1980s, a limited number of researchers were permitted to undertake field surveys in the Tibetan Autonomous Region, including the aforementioned Goldstein and also Graham Clark, who is known as a researcher of the Nepalese Himalayas.⁸ However, while it seemed in the late 1980s that an air of freedom had finally started flowing, the Chinese government once again strictly reinforced regulations partly as a response to the destabilization of Chinese institutions. Thus, these field surveys remained the exception to the rule up until the end of the 1990s.

In this sense, social scientific research in Tibet faced a major issue. The issue was the difficulty or the unfeasibility of field surveys. One crucial factor in this was the fact that social scientific research was itself restrained throughout China with the exception of official government-led surveys in the 1950s, owing in part to the fact that during the Cultural Revolution, sociology and anthropology were themselves dismissed as bourgeois science.⁹ Although the issue is now being gradually improved, it was a major impairment to Tibetan studies until at least the 1980s.

Above all, the issue had a decisive impact on the trend of anthropological Tibetan research. A major part of the academic identity of anthropology, however we may define it, comes from micro-level social scientific research based on field work, and so if field surveys cannot be conducted, this is tantamount to anthropological Tibetan research declaring bankruptcy. The lack of access to Tibet prompted researchers seeking to conduct social scientific studies of Tibet to switch to a field that could substitute field surveys in Tibet. I will refer to these studies and the results they produced as “substitutive ethnography.” A parallel can be drawn between how Sinologist anthropologists—who have also dealt with the problem of closed opportunities for field work in China—have conducted substitutive surveys in Taiwan, Hong Kong, and among the Chinese diaspora in Southeast Asia.¹⁰

However, it is worth noting that when researchers of Tibet sought a substitute field, they were presented with two possible options. The first option was to conduct a field survey among the ethnic Tibetans living in the Himalayas, and the second option was to conduct listening surveys in the Tibetan refugee camps in India in order to research an oral history. It was these two options that linked respectively to Sherpa-centrism and Lhasa-centrism.¹¹ Hereunder, I will give an overview of both these options.

III Modern Tibetan Studies as “Substitutive Ethnography”

1 The First Substitute Field: Nepalese Himalayas

Nepal allowed foreign researchers access in 1952. This was welcome news to the foreign researchers who sought to study Tibetan society. It meant that they could begin conducting Tibetan studies based on field surveys in the Tibet cultural sphere (in the broad sense of the term) including the Nepalese Himalayas.

As for Japan, region-wide surveys were conducted in 1953 and 1958 in the Nepalese Himalayas by a group led by Jiro Kawakita. In particular, a study on the Tibetan community living in the Dolpo region gained international attention.¹² Kawakita published for general readership the survey records of 1958 in *Choso no Kuni (The Kingdom of the Sky Burial)* [KAWAKITA 1960]. The opening passage of the book, shown below, conveys well the atmosphere in Tibetan studies at the time.

The more Tibetan studies advance, the more urgent it becomes to once conduct a thoroughgoing study of those Tibetans who are the least affected by modern civilization. “The least civilized Tibetans” are most likely living deep in the mountains around former Xikang and Qinghai. However, to begin with, there were no prospects of gaining entry permission from the Chinese Communist authorities. I searched for them along the Himalayas, then I ultimately selected the Dolpo region as the field for study [KAWAKITA 1960: 12].

Since there were no prospects of conducting a survey in Chinese-ruled Tibet, Kawakita instead headed to the part of the Tibet cultural sphere in the southern foothills of the Himalayas. This approach was one of the standard international practices during the 1950s. *The Kingdom of the Sky Burial* became the bestseller among books of its kind, and this fact, along with its title, which referred to a very rare type of funeral, was instrumental in forming the popular image of Tibet held by Japanese.

During the same decade, Chie Nakane, who was undertaking a four-year overseas study period, conducted rigorous survey activities in various places in India, and also conducted a survey in the Tibetan Himalayan kingdom of Sikkim [NAKANE 1958]. Nakane, nevertheless, spoke frankly about her regret at not being able to enter Chinese-ruled central Tibet, but she resolved to conduct a survey in India, which permitted surveys, and this survey produced outstanding results. Shown underneath is an example of Nakane’s candid recollections. It is most interesting to compare it with Kawakita’s statement above.

To begin with, the reason I went to India was because I expected there might be a chance to visit Tibet or, even if I could not enter Tibet, I wished to get as close as possible. Since I ultimately could not enter Tibet, I conducted surveys in Assam, the Himalayas, and I also studied the Hindu community. I then started tackling anthropology in earnest and consequently studied in Britain [NAKANE 1990: 186].

When comparing these words to those of Kawakita, it is interesting to note that Nakane accepted the reality of not being able to reach Tibet in a much more candid way than Kawakita. For example, she never presumed to uphold Sikkim as a substitute or a representation of Tibetan society. However, this somewhat restrained approach ended up distancing Nakane from Tibetology. For Nakane, since field surveys were not possible there, Tibet was not an area for anthropological study. In fact, until later on, when the Tibetan exile community emerged and it became possible to survey them as “informants,” Nakane abstained from discussing Tibet.¹³

As explained above, the difficulty in conducting surveys in Tibet proper drove anthropologists towards what they saw as the foremost substitutive field; the periphery of the Tibetan cultural sphere. While it is true that this trend had already begun during the 1950s, the trend continued to exist in anthropology for many years later, and it is this very trend that Samuel is referring to with the term Sherpa-centrism. This is not to say that all researchers of the Nepalese Himalayas headed to the Himalaya-dwelling ethnic Tibetan community in search of a substitute representation of Tibet.¹⁴ However, anthropologists who were credited for their surveys on the Tibetan community in the Nepalese Himalayas, including Graham Clark, Charles Ramble, Nancy Levine, and Hildegard Diemberger, all began their careers as researchers of Nepal, but they headed toward Tibet proper soon after an opportunity to conduct surveys in Chinese Tibet was open. It is therefore clear that the reason why these researchers headed to the Nepalese Himalayas was to seek a representative substitute for Tibet proper, where surveys were not possible.¹⁵

2 The Second Substitute Field: Tibetan Refugee Camps

Many Tibetans became refugees after the Tibetan uprising of 1959 and the exile of the Dalai Lama. The significance of this fact is made clear from the context discussed above. In other words, by using the refugees emigrating from Tibet as informants, researchers were for the first time able to conduct intensive surveys of central Tibetan society with a higher level of accuracy without ever setting foot in Tibet proper. For this very reason, 1959 was an epochal year for Tibetan studies.¹⁶

It is particularly important to note that during this time, China passed through the Anti-Rightist Movement and the Great Leap Forward and was on the threshold of the Cultural Revolution. Accordingly, sociology and anthropology lay dormant until 1978. This situation was another major cause of the greater *raison d'être* of external research.¹⁷ The new Tibetology, which was constructed based on listening surveys with Tibetan exiles, aimed to shed light on the actual state of the Dalai Lama regime which had continued to exist until 1959.¹⁸ In this sense, it corresponded to the Lhasa-centrism coined by Samuel. One of its key contributions was the research produced by Goldstein.

During his years as a graduate student, from 1965 to 1967, Goldstein conducted an intensive listening survey in the Tibetan refugee camps in southern India. What is noteworthy about this survey is the fact that Goldstein was primarily interested in obtaining information about traditional Tibet before the exile, i.e. before 1959. In other words, Goldstein did not study the refugee community itself, but used the refugees as informants to obtain information about the past (at the time, it would have been the extremely near past). In short, Goldstein aimed to undertake ethnographic research based on a reconstruction of the past. Goldstein found a group of Tibetan refugees who had grown up in the same village (Samada village in the Btsang region) and attempted the reconstruction of their community in the refugee camp. This methodology allowed information to be crosschecked as there were multiple informants, enabling Goldstein to secure a much greater level of accuracy compared to more equivocal methodologies, where a number of exiles are called for interviews to study rooms in the U.S.¹⁹ With

the publication of his doctoral thesis [GOLDSTEIN 1968], modern Tibetan studies had now become established as an academic study that met a certain academic standard. Though Goldstein was the pioneer of this style, there were in fact a number of similar studies being attempted during this time. For example, the anthropologist Nimir B. Aziz presented a reconstruction of the history and social makeup of the Tibetan region of Dingri based on verbatim accounts from refugees who had fled from there [AZIZ 1978].

In any case, the studies that used refugees as informants entered the mainstream of social scientific Tibet research, particularly from 1960 to 1970, the period immediately after the emergence of refugees. It is worth noting that most of the people in charge of such research were anthropologists.²⁰ Some would consider this to be a very odd phenomenon. As part of China's democratic reform in 1959, the social structure of the Dalai Lama regime was completely destroyed, and Dalai Lama-ruled Tibet was reduced to a chapter in history as "old Tibetan society." Therefore, though it would seem that studying the "old society" should be the work of historians, it was in fact taken on by anthropologists and continues to be performed by anthropologists even today. Arguably, the definitive work is Goldstein's *A History of Modern Tibet* [GOLDSTEIN 1989a], a monumental piece of research into the history of modern Tibet that stretches some 898 pages. What must be noted at this point is the fact that these listening surveys did not initially purport historical research but were designed to be contemporary research, akin to regular ethnographic research. Let me introduce an episode that symbolizes this phenomenon.

While undertaking his survey of the refugee camps during the 1960s, Goldstein acquired a contractual document from a subject of the old Tibetan government called Nyima. This document was a contract authorizing Nyima to leave the manor to which he belonged. Nyima was initially reluctant to provide the document. Nyima feared that if Tibet should soon regain independence, allowing the exiles to return to their land, then without this document he would find himself in trouble. In the end, Goldstein obtained the document after promising Nyima that if the day came when the people could return to Tibet, then he would promptly return it to him [GOLDSTEIN 1989b]. Considering how difficult it was for the outside world to know about Tibet's situation after the Tibetan Uprising of 1959, this is a most illuminating episode.

It was difficult for anyone outside China, including the Tibetan refugees, to have an idea of what radical change traditional Tibetan society had to sustain due to the 1959 democratic reforms of "new" China. This being the case, as far as the refugees at the time were concerned, their haven of India was ultimately nothing more than a temporary abode, and they would have been in no doubt that their original world (traditional Tibet before 1959) was the one and only "true Tibet." Considering the fact that little time had passed since the Tibetan Uprising, and the fact that the refugees had not imagined that their exile would last for so long (it has already lasted half a century), such verbatim accounts at the time of the survey would represent, in a sense, contemporary Tibetan research.

However, since the refugee informants were indeed refugees, they were not aware of the situation in Tibet proper following the exile (from 1959 onward). Thus, the accounts of the refugees' experiences in Tibet could only ever be used to reconstruct old Tibetan society, which was in fact already uprooted from 1959 thanks to China's democratic reforms. The facts of the cataclysmic changes that took place in Tibet were only ever known in detail by outsiders after China had come through its Cultural Revolution and had begun taking timid steps to release information in the early 1980s. As people became increasingly aware of the sheer scale of the changes and damage in Tibet, it began to be understood that the old society the Lhasa-centrist studies sought to reconstruct had already been consigned to history. In other words, the reality that contemporary Tibet, the very target of the project which had begun as contemporary research, no longer existed anywhere, was finally understood after

a time lag of nearly twenty years from the destruction of the actual old society in 1959. To borrow terminology familiar to anthropologists, these studies would today be considered modern history, but they were originally designed to be ethnographic studies that defined pre-1959 Tibetan society as the “ethnographic present.” I do not know about journal articles, but for a doctoral thesis, how many years does it take for a doctoral student to prepare a thesis after research, find tenure at a university, search out a publisher and negotiate a publication grant, and then publish the results as the sole author? In ethnography, it is by no means rare for it to take ten years after a survey begins before the book appears on the shelf. For researchers of modern Tibet who follow the trend set by Goldstein, the time lag was not ten years, but thirty years, and even fifty years.

That being said, there is no denying that this process invites confusion. Though central Tibet suffered such cataclysmic and irrevocable changes in the democratic reforms of 1959, it was scarcely possible for people in the outside world to know the exact nature of these changes. The year 1959 was in reality an epochal year, but the outside world would have to wait until the early 1980s before they could have certain knowledge of this fact. This time lag is a crucial factor. For the refugees, the “old society,” prior to the changes of 1959, represented “contemporary Tibet,” and anthropologists used information collected from these refugees to reconstruct the pre-1959 society. However, for anthropologists, there remained the tricky question of whether the model of this reconstructed Tibetan society was “history” or “contemporary ethnography.” Naturally, the anthropologists also recognized that this model was a reconstruction of pre-1959 Tibetan society, but the opacity of China’s internal political process made anthropologists hesitate to use the label of “history.” During the Cold War, the pre-1959 model of Tibet represented the reality for Tibetan exiles in a way that is difficult to imagine today; indeed, even as the changes that occurred in Tibet gradually came to be known, there was an idea that this change was nothing more than a temporary aberration that would be rectified when Tibet reestablished “independence.” Thus, during this time lag, it remained for a long time unclear whether these studies were modern Tibetan ethnography or the historiography of a lost past, and it is a fact that the weight shifted progressively from the former to the latter. Thus, it was increasingly understood that there existed no more the society depicted by anthropologists, who had started their research as contemporary research, and such work gradually became what is now modern history. The fact that this state of affairs could have ever come about in the first place can be attributed to the abuse of substitutive ethnography and, in particular, Lhasa-centrist studies. In other words, because the surveys took place in locations far removed from the field of study, the research entailed a defect in that it failed to properly assess the changes that were underway in the informants’ “old home” even while the surveys were being carried out.

Viewing the research from the historical research perspective is of crucial importance. Since Tibet had scarcely any relations with China during the first half of the twentieth century, all the Chinese language documents were nothing more than secondary sources, and the use of Tibetan language documents, the primary sources, was restricted for political reasons.²¹ In this sense, historical research on this period was much the same as historical research on non-literate societies.²² Arguably, therefore, an oral history that used refugees as informants was the only methodology that could encompass the target time and space. Using verbatim accounts of exiles is far outside of a direct historical research method, and it is not technically valid historical research, either. As such, it is not an attractive method for young historians who have received philological training. There is also an institutional issue, which tends to get overlooked: It is very difficult for young graduate students to obtain a grant they need to head out to India, while enrolled in a Chinese studies course ostensibly for historical research, set up base in a refugee camp, and acquire fluent Tibetan language proficiency required to record verbatim accounts. Conversely, it follows that those students who succeeded in making such a proposal were researchers belonging, institutionally, in

anthropology. What is hidden in this background is the reality that the disciplines of anthropology and history are not defined by the “subject” of study (past or present), but by the “methodology” (textual study or field survey). This is rarely acknowledged in the syllabus or seminar homepages. As long as there is a need for field surveys in developing nations, whether the subject of study is historical or current, these institutional conditions have helped to ensure that such surveys remain the strong suit of anthropologists. It is for this very reason that the study of the history of modern Tibet has been led by anthropologists.

Based on the above facts, it should be understood that the dichotomy between Sherpa-centrism and Lhasa-centrism was not, as Samuel thought, the dichotomy *between* anthropologists and historians, but instead represented two trends *within* anthropology.

IV The Problem with Substitutive Ethnography

Up until now, I have demonstrated the inadequacy of the Sherpa-centrism/Lhasa-centrism dichotomy. I will now point out a notion that is present in both Sherpa-centrism and Lhasa-centrism. This is the notion that the study of modern Tibet is not the study of post-1959 Tibet. For the history-oriented, the history of modern Tibet refers to the history of the Dalai Lama regime prior to 1959. For others, it is something that refers to the “Tibetan-type” society that “survives” among Tibetan communities in places such as Nepal. Neither group considers Chinese-ruled Tibet to be a genuine subject of study. Such an attitude stems from the way in which the cataclysmic changes in Tibet after 1959 were understood. Irrespective of political differences, no one disputed the fact that the democratic reforms of new China brought unprecedented upheaval to Tibet. In addition, these changes have for many years been negatively assessed in mainstream Tibetan studies, and what occurred after 1959 is understood not so much as a new facet of Tibet, but rather as Tibet’s destruction. This being the case, those who have a desire to study Tibet would come to the conclusion that, as noted by Tsering Shakya, “there was nothing worthy of study in post-1950 Tibet” [TSERING SHAKYA 1994: 9]. This is the stark reality that steered Tibetan research for thirty years and gave rise to the two trends. For example, it is possible to see a direct expression of this attitude in the following passage from the epilogue of *A Cultural History of Tibet*:

Thus all individual characteristics [of Tibetan culture] have to be firmly eliminated [under Chinese rule]. It follows therefore that the changes that have taken place in Tibet over last nineteen years are largely irreverent to the subject-matter of this book. [SNELLGROVE and RICHARDSON, 1980: 273].

Thus, many researchers consider the history of Tibet to have stopped in 1959,²³ and the study of modern Tibet has for many years been the pursuit of “Tibetness,” which each researcher defines themselves. Accordingly, some researchers investigated central Tibet under the Lhasa administration before dramatic changes took place (Lhasa-centrism), and other researchers investigated the Tibetan communities in neighboring countries (Sherpa-centrism). Only after understanding Lhasa-centrism and Sherpa-centrism in this way can one understand the subtle collusion existing between them. Researchers from both groups share the same desire to avoid the more problematic challenge of discussing the political relationship with China in favor of representing a purer version of Tibet. It is this desire that has fuelled the cathexis for the substitutive ethnography trend in Tibetan studies.

V Conclusion

In the past, Jiro Kawakita argued that there were only four truly original civilizations in Asia: East Asia (China), South Asia (India), West Asia (Islam), and Tibet [Kawakita 1987], but Tibet is now a stateless civilization. This political situation is itself the causal factor of poor dialogue in Tibetan studies. It is directly linked to the whole history of how researchers turned to substitutive fields in the first place and then branched out into several different substitutive fields. The details of the history have already been made clear in this paper. In order to avoid misunderstandings, I shall add that there are aspects worth assessing positively in such an indirect method. For example, the fact that anthropologists hold a major share in modern historical research is indicative how such substitutive anthropology has contributed to academia in a way that transcends the framework of anthropology. On reflection, this contribution is a powerful testimony to those outside anthropology of the usefulness of anthropology in the humanities. The central argument of this paper is that this diversification must be understood through the framework of the attempt of substitutive ethnography to respond to the problematic situation in ethnography whereby field surveys were forbidden.

I will conclude my analysis here, adding that the situation I have described was the case up until the 1990s. This situation is undergoing major changes thanks to the opening of China and the advance of Chinese political studies. For example, at the present time, it is possible to tentatively label the new trends in modern Tibetan studies as “Beijing-centrism” and “Amdo-centrism.” Based on the framework of political studies on modern China, Beijing-centrism uses Chinese documents to analyze the situation in modern Tibet from an ethnic-issue perspective, and it is for the most part positioned as a sub-field of Chinese political studies. The flourishing of Beijing-centrism indicates that the era of anthropology’s monopoly over modern Tibetan studies is drawing to an end. By contrast, Amdo-centrism, which seems to have become the mainstream of anthropological Tibetan studies, is ethnographic research conducted in the Amdo region (northeast Tibet). Amdo is not as strictly controlled as the Tibetan Autonomous Region, and so many anthropologists have entered the region in recent years.²⁴ While space constraints do not allow me to discuss it in this paper, the trend of analyzing the refugee community within the framework of refugee studies continues to be ever present. Regarding the Tibetan Autonomous Region, while it is still difficult for foreign researchers to conduct surveys there, in recent years, a number of collaborative research projects have been underway, and channels to studying in the Autonomous Region are steadily being opened.²⁵ Thus, research on modern Tibet is currently undergoing a major boom, and research results based on field surveys are being released. Aside from Beijing-centrism, most of these research trends are led by anthropologists, but I am concerned with the lack of interactive dialogue even within anthropology.²⁶ That being said, this dialogue must not simply be a process of emphasizing the expansion and commonality of Tibetan society. Regarding this point, we can learn lessons from the experience of Chinese substitutive ethnography. Harrell produced a disquisition that may be viewed as self-criticism at how he in the past headed to Taiwan to conduct a substitutive field study and wrote it up as “Chinese ethnography” [HARRELL 1999]. However, he avoided the patchy “Taiwan is not China” style discourse. The issue is not the search for commonality between China and Taiwan itself, but is rather the way such a search is premised on the existence of a holistic Chinese society (anthropological holism). If such holism becomes the premise, then no matter to what extent particular characteristics are discovered in Taiwan and no matter to what extent Taiwan was affected by over half a century of Japanese colonial rule, such phenomena end up being viewed as a mere variation in the results of such a holistic social structure or cultural characteristics adapting themselves to individual environments. There is a danger here that the research findings in individual

fields end up being unreasonably reduced into a large, abstract model. In his thesis, Harrell never really presented a solution to this state of affairs in Chinese ethnography. However, regarding attempts at synthesis and interactive dialogue in Tibetan ethnography, one should understand that the very same danger is entailed in simplistically assuming and then searching for such commonality. In view of these facts, if I were to offer a prescription to synthesis-seeking modern Tibetan studies, my first recommendation would be to continue making reference to history, and my second would be to further promote anthropological history research, where there are still many unexplored fields.²⁷ The synthesis of Tibetan ethnology is a necessary work, but when undertaking such a work, it is vital to give due consideration to the different historical experiences in individual regions. It is of course essential to incorporate the findings of Beijing-centrism (political studies), but what must really be emphasized is the importance of positioning one's own research in the sphere of continuity with history. For example, there exists different historical experiences between central Tibet where ruled by the Dalai Lama's government and Amdo region where neither ruled directly by the Dalai Lama's government nor Chinese authority in the first half of twentieth century. We have to pay much attention to such difference when we go into anthropological history research. However, while largely forgotten today, much of this history has actually been taken on by anthropologists. This makes it all the more important that the history of modern Tibetan studies be written from a fresh perspective, and I hope that with this paper I have contributed in some small way to this enterprise.

Notes

- (1) Note that in this paper, the term anthropology refers to sociocultural anthropology.
- (2) For example, [CLARK 1983: SNELLGROVE 1966: TSERING SHAKYA 1994] etc.
- (3) However, what requires attention is the fact that Samuel himself did not necessarily take a neutral approach to this contradistinction. Rather, Samuel considered the stateless Sherpa people to be a valid representation of Tibetan society [SAMUEL 1982. 1994: 225 etc.]. Samuel personally took the distinctive view that Tibet under the Dalai Lama was, by contrast, a rigid, centralized, statist system and that it therefore had lost the attribute of statelessness. Samuel then made the case that these two polar opposite qualities, i.e. statelessness and statist attributes, correspond respectively to two antithetic aspects in Tibetan Buddhism. These are the shamanist aspect, which exists in Tibet's indigenous Bon teaching and the Nyingma branch of Tibetan Buddhist schools, and the rigid and logical doctrinarianism of the Gelug branch of Tibetan Buddhism. This correspondence forms the basis of Samuel's great work [SAMUEL 1993].
- (4) In addition, Tsering Shakya, who produced worthy modern history research [TSERING SHAKYA 1999] equal to that of Goldstein, also spent the first part of his career gaining attention as an anthropologist. Other researchers who authored works on old Tibetan society in the early twentieth century, including Aziz and Rebecca R. French, are also classified, institutionally, as anthropologists. Their published works include [AZIZ 1978: FRENCH 1995].
- (5) For more information on the process by which this stereotype of Tibet was formed, please refer to the research of Donald Lopez Jr. [LOPEZ JR. 1988] and Peter Bishop [BISHOP 1989]. However, there already exist persuasive disquisitions regarding the problems of incorporating post-colonial criticism into Tibetan studies [DREYFUS 2005: HANSEN 2003: SAMUEL 1994: 696–697], casting doubt on the value of such research.
- (6) For details on the circumstances, please refer to [McKAY 1997] and [HIRANO 2004].
- (7) For more information about Tucci's Tibet survey, please refer to [TUCCI 1987]. [McKay 1997] provides outstanding research on the interest in Tibet among officials of the British Raj.
- (8) For the results of the surveys conducted in the 1980s, please see [CLARK 1988: GOLDSTEIN and BEALL 1990: GOLDSTEIN 1986, 1994].
- (9) The official Tibet survey project conducted by the Chinese government during the 1950s left valuable results thanks to the work of team leader Li Youyi, who inherited the high-level bourgeois sociology of pre-liberation China. As an example of research that has utilized the results of this survey, please see my work [OKAWA 2007].
- (10) For more on the circumstances, please refer to [NISHIZAWA 2006]. However, the researchers who studied Hong

Kong and the Chinese diaspora in Southeast Asia saw this phenomenon as a chance for a theoretical leap forward in Chinese anthropology and so were partly optimistic about it [cf. FRIEDMAN 1963]. On the other hand, as is revealed in the poignant and somewhat sarcastic recollections of the leading American Sinologist anthropologist Stevan Harrell [HARRELL 1999], the researchers who have used Taiwan as a substitute field saw the phenomenon as a much more serious issue.

- (11) There are studies that have analyzed the Tibetan refugee community by considering the refugee community as a refugee community; in other words, within the framework of refugee studies or migrant studies (there was a time when I too was involved in such research to some extent [OKAWA 2006, 2008]). Such research has also been viewed as one trend in modern Tibetan studies. The volume of studies is staggering considering the refugee population, which is less than a hundred and some thousands, making the research somewhat unique. However, it will not be possible to discuss the research in this paper.
- (12) In terms of the research results, see [KAWAKITA 1961] and also [TAKAYAMA 1960; 1990]. The latter is by Ryuzo Takayama, who was a member of the same expedition.
- (13) While it might not be considered particularly outstanding among her extensive body of work, Nakane subsequently conducted a Lhasa-centric survey (history of modern central Tibet based on verbatim accounts from exiles) [NAKANE 1972; 1981].
- (14) For more information, please refer to Nawa's arguments in [NAWA 1999: 184].
- (15) The research results from the Nepal survey era include [RAMBLE 2007; DIEMBERGER 1997; LEVINE 1988]. All of these researchers subsequently entered Tibet, and Diemberger even went beyond the framework of anthropology by, for example, helping Tibetan researchers reproduce ancient Tibetan texts [PASANG WANGDU and DIEMBERGER 2000].
- (16) Already by 1960, the Rockefeller Foundation had invited a large number of Tibetan intellectuals (mainly scholar monks) living in the refugee camps to cities such as Seattle, London, Tokyo, Munich, Rome, and Copenhagen, and this significantly advanced colloquial Tibetan language studies and studies of Tibetan Buddhism. The Tibetan uprising was thus a major event that changed the schema of Tibetology as a whole.
- (17) One interesting study concerning anthropology in China at the time is [GULDIN 1994].
- (18) Though the Dalai Lama regime was officially ended in 1951 by the 17-Article Agreement, the regime itself continued to survive under Chinese rule, if only by a thread, as a traditional polity based on a special dispensation referred to rightly in Tibetan as "one country, two systems." Therefore, in investigatory research into old Tibetan society, the year 1959, as opposed to 1951, is considered epochal. For more information, please refer to my work [OKAWA 2001, 2007].
- (19) [CASSINELLI and EKVALL 1969] is one example of such armchair ethnography, which has lost all value today.
- (20) The research produced by Franz Michael, a historian of the Qing Dynasty [MICHAEL 1982], is certainly an exceptional case in that an interview survey was conducted by someone who was a historian institutionally, but unfortunately, it is not a particularly brilliant exception. The low level of accuracy in this survey has been acknowledged several times [GOLDSTEIN 1989a: 16n25; SAMUEL 1993: 568n9]. The crudeness of the quantitative data in particular is enough to make one want to avert one's eyes.
- (21) I once heard the following from a local researcher who had experience organizing texts in the Archives of the Tibet Autonomous Region: "My job at the time was to go through the historical documents and pick out any document that bore the words *bod rgyal khab* (Kingdom of Tibet) or *bod chen mo* (Greater Tibet) and affix them with the label "not to be shown to the public." (Interview, anonymous, Lhasa, August 21, 2006)
- (22) In this respect, Tibetan anthropology is vastly different from Chinese anthropology, which provides a much more fertile ground for philological research.
- (23) I have named this phenomenon "the freezing of Tibetan history." While there is no space to go into detail in this paper, such "freezing" of history is something that can even be observed in the everyday lives of the people living in Tibet proper today.
- (24) Examples of fine Tibetan analysis from the Beijing-centrist scholars include [MATSUMOTO 1996] and [MORI 1998]. As for pioneers of field survey-based Amdo ethnology, a good place to start would be the research of Makley [MAKLEY 2007].

- (25) Since the University of Vienna had from the early years formed a partnership with the Tibetan Academy of Social Sciences, there were at the time many Tibetan researchers from all over Europe gathering in Vienna. Today, the Network for University Co-operation Tibet-Norway (University of Oslo, etc.,) and the Tibet and Himalayan Digital Library Project of the University of Virginia are collaborating with local academic organizations, and exchanges are constantly taking place. However, these exchanges have ebbed following the anti-government protests that took place in various places in Tibet in 2008.
- (26) For example, looking at the annual conference of the Japanese Society of Cultural Anthropology, it is currently the case that reports on Amdo ethnology are organized in China-related venues so that they are given separately from Sherpa-centrist studies and refugee studies that are given in South Asia-related venues.
- (27) My work [OKAWA 2007], for example, should be positioned as an extension of such anthropological history research.

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