



# Collaborations, Legacies, and Shifts in Chinese Anthropology:

## In Conversation with Myron Cohen

Junbin Tan

**Myron Cohen** is Professor of Anthropology at Columbia University. He received his PhD from Columbia University and has been a member of its faculty since 1966. His research has focused on Han Chinese culture and society in late imperial and modern China. His earlier works focused on tracing, through fieldwork in peasant societies in Taiwan, and at Hebei, Shanghai, and Sichuan on Mainland China, the socio-cultural convergences and divergences in Han Chinese communities. This research culminated in *Kinship, Contract, Community, and State: Anthropological Perspectives on China* (2005). At present, Cohen has returned to do historical anthropology at his first field site at Meinong, southern Taiwan, where *House United, House Divided: The Chinese Family in Taiwan* (1976) was based. Through studying contracts and documentation in Qing dynasty Taiwan, he hopes to write about state, society, and cosmology in imperial times. Cohen was Director of the Weatherhead East Asian Institute (WEAI) at Columbia University between 1975-1976 and 2006-2014. For his contributions, he was awarded the Chinese Anthropology Lifetime Achievement Award (Shanghai Society of Anthropology, 2016), and First Class Professional Medal in Hakka Affairs (Taiwan International Conference on Hakka Studies, 2016).

**Interviewer's remarks:** I had approached Myron Cohen who, to my knowledge, is the longest-practicing Chinese anthropologist today, for an interview on his early professional experiences. They include his fieldwork and collegiate collaborations, and his views on intellectual exchanges, legacies, and shifts in Chi-

nese anthropology from the 1960s to the present. I had hoped for us to learn about broad shifts in the sub-discipline and its institutional culture.

Unsurprisingly, Cohen's responses exceeded what I sought to find out. You will read about, for example, Cohen's thoughts on traditionalism, which he situates in historical context (i.e. the inability of anthropologists to do fieldwork in Maoist China); the different kinds of issues that piqued the interests of scholars of the 1960s and 1970s, working 'in the spirit of those times'; and the shrewdness with which he approaches fieldwork in the past and present. While he spoke about 'waving *gongwen* (公文 official documents) in people's faces' during fieldwork in the 1960s (which may raise eyebrows today), he also reminds us that, even in the face of state-imposed obstacles, 'you put anthropologists anywhere for long enough, they will see interesting things going on, for local life has its own quality.' Lest we assume that we can criticise the anthropology of yesteryears given our advantage of hindsight, his experiences and reflections also, in my view, help us appreciate these as intellectual developments situated in their own contexts and existing in their own right.

**Junbin Tan (JT):** Thank you for agreeing to this interview. You started fieldwork in Taiwan in the 1960s, and then moved on to different parts of China. By means of introduction, could you share about how you got acquainted with anthropology and started research on the Chinese-speaking world?

**Myron Cohen (MC):** How I entered into anthropology and China is an interesting story. In fact my involvement with anthropology goes back a long time. I gave a lecture when I was 15 years old to a college class, on *australopithecus*. I was interested in physical anthropology. My science teacher set that up for me. After that, I went to Columbia College and majored in physical anthropology, in which I later lost interest. I switched to philosophy, then psychology, and got interested again in cultural anthropology. My teachers were Marvin Harris, and a slew of others, and this was before my fascination with China. What got me interested in China was the question, ‘why is it, or how is it, that China has held together as the world’s largest society for 2000 years?’ What got me interested in China was not any kind of emotional attachment, but that question. So, I wanted to figure it out and took undergraduate courses in what was then called ‘Oriental Civilisation.’

JT: And what decade or year was this?

MC: It must have been 1956 or 1957 when I took Oriental Studies. It mainly dealt with China, Japan, and India; Korea only came later in the program. When I went to graduate school—I stayed here at Columbia—I worked under Morton Fried, my advisor, the China anthropologist, and then, for my second year in the program, I was devoted almost entirely to learning Chinese. In those days, Columbia had an intensive course, which combined two years in one; first, you took two years of Chinese; the following summer, you took third year Chinese; and the next year, you take fourth year Chinese. The goal was to have you be able to read a newspaper by the end of the sequence, which I could do. I worked a lot with Marvin Harris, Morton Fried, and Elliot Skinner as a graduate student, here at Columbia.

JT: I see, so these were your teachers, your professors who left a deep impression.

MC: The person who was most influential was Marvin Harris, and Morton Fried, of course, on the China side. Morton Fried has an interesting history. During

WWII, he was in the army. Fried was sent to Harvard to study Chinese, and the instructor was Chao Yuan Ren (趙元任), a famous linguist. Chao was teaching at Harvard and the US government put him in charge of the language program for soldiers. In those days, the US state department would invite Chinese intellectuals from ‘Free China’—China that was not under Japanese control—for a year-long stay in the United States.

So, Fei Xiao Tong (費孝通, also Fei Hsiao-tung) was invited to Harvard. One day, Chao invited Fei to give a talk to his soldiers about China, which Chao did, and Fried was among these soldiers. That’s how Fried got interested in both China and Fei. It was a special situation because at that time in Europe, it was the Battle of the Bulge, the last big Nazi offensive. Students who did not do well in the course were sent there. They were strongly motivated to do well, because it was literally a matter of life-and-death.

At any rate, Morton Fried graduated from the course with a strong interest in China and knowing Fei. He was still in the army, and was sent to Georgia to do something unrelated to the training that he had received—typical army-style—but for him, China was still important. After he was discharged from the army, he came to Columbia, got his degree here, and was teaching when I came here. So he was my professor.

JT: So, it was through Morton Fried that you approached Taiwan. What was it like, researching the Chinese-speaking world as an American graduate student and a young fieldworker?

MC: This was during the era of Mao Zedong. In those days, you couldn’t go to Mainland China to do any research, so there were two options: one was Hong Kong, and the other was Taiwan. After my graduate training in the United States, I decided to go to Taiwan first for one year of advanced language training at the Cornell Center, and in the middle of that year, the Cornell Center folded up and was merged with the Inter-university Center that now still exists in Beijing. I spent a year in Taipei doing language work, but also thinking of where to do

fieldwork. I went to Hong Kong and I looked around there, then I decided that Taiwan was a better bet. This was during the era that I call 'traditionalism.' Most people doing fieldwork in Taiwan or Hong Kong were not focused on changes that were going on there, but were doing research for insights on 'traditional culture'! They were trying to make a contribution to the larger field of Chinese studies, although one couldn't do fieldwork in China.

I should go back a moment. For my M.A. at Columbia, I did an essay on the Hakka, and the importance of language as a 'sociocultural variable.' I focused on the Hakka Punti wars in central Guangdong. That was the reason why I wanted to study Hakka (客家) villages, later when I was in Taiwan. In Taiwan, I asked people where to find a traditional village. That was the spirit of the times. 'Oh you got to go to Meinong', I was told, so I went to Meinong (美濃), and, sure enough, in certain respects, it was incredibly traditional, especially with its huge families. This was what surprised me, 30 to 40 people straight out of the Dream of the Red Chamber. This was not a lineage, not clan, but an undivided family, a *jia* (家), with common meals and where money was pooled: a classical joint family where every married couple had their own room, the cousins slept together, and the parents ran the show for three or four generations.

JT: So, was this how you ended up at Meinong, where you did research for *House United, House Divided* (1976). Could you tell us about your Meinong fieldwork?

MC: Yes, I did fieldwork in a village in Meinong. I decided to focus on this family scene. I made a deal whereby with one *joint* family, I had my meals; with another joint family, I had my sleeping quarters; and with a third, I had my office. So I had close contact with three joint families at one time. The whole point of that, given the anthropology of those days, was to disentangle family structure from the idiosyncratic and the personal. In other words, there was a basic structural force in play in all of them, which made them joint families.

I wrote a book on this: *House United, House Divided*. I was lucky because the other person who studied joint families was Margery Wolf, who wrote *The House of Lim* (1969), a study of one family. It's a beautiful book, aesthetically more attractive than my work. But her main problem is that she is unable to distinguish the idiosyncratic from the structural, in dealing with just one family. I had 30 families as a matter of fact, but three who I lived with. And so, I saw all sorts of things: the process of family division, the relationship between women and the family economy, the notion of women's private property, and more. And the huge complex array of relationships, such as brother-brother, husband-wife, and mother-in-law and daughter-in-law relationships, all entangled with each other.

These are things that people like Marion Levy and Olga Lang talked about in the 1940s. There they were! I was astonished to find this when I went there. This was not what I originally had in mind, but it became the major interest, obviously. I attempted an overall field study, but I was certainly going to focus on those families. And it was good I did so, because ten years later, they were all gone.

JT: Could you tell us a little about the significant moments that you experienced in the field: the challenges and the happy moments?

MC: Frankly, I was lucky; it was easy. In terms of acceptance, you learn a few tricks. First, I was fortunate to get an affiliation with the Institute of Ethnology (民族學院研究所) of the Academia Sinica (中央研究院) in Taiwan. The Institute Head at that time was Li Yih-yuan (李亦園), an excellent anthropologist. He passed away a couple of years ago. He was extremely helpful. He gave me something extremely valuable: an official document or *gongwen* (公文), which stated the purpose of my research. When you show that to people, they were very impressed.

I remember the other thing that was interesting was that I found a gentleman assistant, Zhong Fusong. He

was, in fact, too good. I was the first foreigner he had spoken to in his life. He spoke perfect English, which he had learnt through his knowledge of the IPA and listening to BBC broadcasts. I met him after my first arrival, and I invited him to be my assistant. He accepted. As a smart local person, he was extremely invaluable. But he also crippled my language studies. I never really picked up Hakka as much as I wanted to, because he was there right next to me.

The other issue was that the younger people with whom I was more comfortable all spoke Mandarin as a second language. This meant that my Hakka didn't improve much. In those days, for people over 40, their second language was Japanese, with Japanese colonisation that ended only in 1945. Japanese culture was still prevalent in many ways. Once I was in a family's home, and they were in the middle of renovating something so they pulled a big cabinet away from a wall. They were astonished that behind the cabinet, there was a large portrait of the Meiji emperor, which had been concealed since 1945, presumably, after Japan surrendered and they forgot about it. The Japanese impact could be seen in various ways. The food was largely traditional Hakka Chinese, but there were Japanese elements involved in wedding banquets and so on. One of the strangest dishes definitely from Japan was macaroni and cheese.

JT: They had macaroni and cheese in a Hakka village?

MC: In a Hakka wedding. It became popular in Japan in the 1880s, so it was a cultural transmission. Most people won't think of macaroni and cheese as being a classic Japanese dish, not to speak a classic Hakka dish, but there it was. Anyway, I don't think that they eat it anymore, but that was the case then.

Anyway, about the *gongwen*, which I waved in everybody's face when I had to, the important thing is to know where the powers are, and you get to be friendly with them immediately. Don't hang around. One of the first people I visited was the district chief of police, to whom I showed my *gongwen*, and we became friends. He was an interesting character. He was a

mainlander from southern Fujian, which meant that he could speak Hokkien, the dominant local language. He regaled me with stories of how, during WWII, he was assigned to be in charge of Taiwanese prisoners of war, that is, Taiwanese who were in the Japanese imperial army, and found that none of those Taiwanese officers could speak Taiwanese. They could only speak Japanese.

At any rate, my routine was to walk through the village everyday. The village was such that you could walk in a circle around it.

JT: So it wasn't a big village.

MC: There were 68 households, some of which were very big. Usually, I would just make my rounds, but not on special occasions such as weddings or funerals. As I went around the village, accompanied by Zhong Fusong, I would say 'hello' to everybody. I would stop by different places for conversations and be saturated with tea. People were friendly. Fieldwork went quite smoothly. I didn't even get sick!

JT: So when you came back from fieldwork, and as you worked on *House United, House Divided*, who were the people who shaped your thoughts?

MC: As I said, Harris was a big influence. It was a materialist book. The focus was on the relationship between tobacco cultivation, with a very heavy labour requirement, and joint families had the labour power and were held together by economic pressure. They didn't split. That was the classic analysis at that time.

JT: What about scholars of your time? Your contemporaries.

MC: Maurice Freedman. That Freedman was a major influence epitomised the strand of traditionalism in those days. He talked about lineages in the Qing dynasty, and here we are, in the 1950s and 1960s, in Taiwan and in Hong Kong. Hong Kong had such lineages, but to have that as our guiding point implied a heavy focus on the traditional. The example I like to give is, suppose, an anthropologist goes into a

valley, somewhere in Taiwan, where there is a huge computer factory and also a little earth god shrine. The anthropologist would run to the earth god shrine, and forget about the computer factory. Nowadays, it would be the reverse. That's a big change.

JT: Yes, and changes are also happening in terms of where you can do research, when China opened up. You started research in China in the 1980s...

MC: In 1986, in Hebei.

JT: What motivated you to do move from Taiwan to China? And were the fieldwork experiences any different when you visited different parts of China?

MC: What motivated me was that from the outset, I was interested in China. But then I had a very special question in my mind, after my Taiwan fieldwork, namely about the Han Chinese: 'Who are the Han Chinese?' The best way to find that out was to compare several villages of the Han Chinese that are very far apart from each other. I had been to Taiwan, so I went to Hebei, up north and just south of Beijing. That was in 1986. And then in 1990, I spent four months in a village near Shanghai, and then four months in a village in Sichuan [in the East and West of mainland China]. My focus in all of that was 'families'. I wanted a comparison. There were very interesting similarities and parallels. So my conclusion, actually my hypothesis before I did the work, which was in the end validated, was that there is something called the 'Han Chinese', as a cultural formation with some variations but widely distributed.

JT: And that was covered in the most recent book that you wrote, *Kinship, Contract, Community, and State* (2005).

MC: Yes, that most recent book brought to bear this analysis. One difference between fieldwork in Mainland China and in Meinong was that I have kept on going back to Meinong for about 50 years, most recently in January 2020 for ten days. As I have written, old traditionalism in anthropology gave way to a large

focus on contemporary China when Mainland China opened up. Yet, traditionalism didn't completely die, but morphed into historical anthropology, involving scholars such as David Faure. My own traditionalism also went this direction, and a historical anthropology approach informs my present-day Meinong project. Among other things, I am dealing with Qing dynasty contracts in the Meinong region, several hundred of them, which I plan to include in a book on Meinong society during the Qing.

JT: So, what differences are there between historical anthropology, with its historical methods and analyses, and fieldwork?

MC: Documents tell you whatever they contain and that's it. Whereas in fieldwork, you see real people, live people. It's very, very different. It's often the case that some questions you answer with documents are secondary to what you observe among people. What's important in terms of this historical material is 'how rich are the data?' With a lot of historical information, you can do interesting things. For example, I have a contract. I see who signs it and who are the witnesses. Some of Meinong's earliest contracts are by the people who came from Mainland China and founded Meinong. So I know that Meinong society formed very rapidly. Many characteristics that other scholars found on Mainland China also appeared in this new context, which weren't there 20 years earlier. Meinong society, I would then argue, formed as if the founders carried it in their heads, which my material suggests, when they came to Taiwan.

JT: These documents are proof of similarities between what was on Mainland China and places where the Han Chinese had migrated?

MC: These are evidence that people who came from the Mainland to Taiwan brought their culture with them. Obviously the environment was different, as were many other issues. For example, there is a shared genealogical orientation. In the Mainland area, there were surname temples in the big cities that focused on remote ancestors whereas in the Taiwan

countryside, they used the exact same ancestral practices not to unite lineages but to unite individuals. So there you have, sociologically, a totally different scenarios, but culturally operating within the same framework.

JT: I have two more questions, big questions... Could you tell us about the scholarly collaborations between anthropologists of the Chinese world in the 1970s and 1980s?

MC: Well, I would go back to the 1960s, when several of the then prominent China anthropologists and social scientists, including Morton Fried, Maurice Freedman, G. William Skinner, Marion Levy, C.K. Yang, and several others in the joint committee of SSRC and ACLS on Chinese Culture and Society, organised a closed seminar for its members. Morton Fried invited me as a graduate student, to be the rapporteur for one of the seminars, so I got to know these people. There were several seminars. The ones that I went to were really weighted towards traditionalism.

In that atmosphere, the interest was in issues involving such matter as kinship, lineage, local religion, but not contemporary China. I went to seminars in Bermuda, at Cornell, and to yet another at a resort not far from New York City. These seminars put me in touch with leaders of the field, which was small. Chinese anthropology expanded only later.

There was also a whole series of conferences that led to many conference volumes— eight or nine of them. Scholars interested in China were interacting constantly, mainly the anthropologists and the social historians, less so economists and political scientists. Again, it was the traditionalism focus. Among anthropologists, contemporary China was not only fashionable, but it was also inaccessible.

JT: Of course, a lot of people worked on Taiwan at that time, some of whom worked near the Taipei Basin. Can you tell us about scholars who did fieldwork in Taiwan?

MC: The Taipei Basin Haishan project involved Arthur Wolf and his students. Morton Fried also had two students in Taiwan at that time, me and Burton Pasternak. When I was doing fieldwork at Meinong, Pasternak was doing his at Datie (打鐵, or Ta-t'ieh) village in Pingtung county. Meinong was at the northernmost point of the Hakka settlement zone in South Taiwan, and Datie was the southernmost part. This entire Hakka region had been united during the Qing into a huge community that was also a militia organisation. We were doing our respective fieldwork in the mid-1960s. Then, in 1971, Pasternak and I had a joint project back in Meinong. We managed to obtain an NSF grant for this project, which used G. William Skinner's 'local systems' approach. In those days, people in anthropology had grand visions. Marvin Harris had his vision of cultural materialism, and Skinner had his 'nested local systems'. But the notion of 'integrated systems' was very attractive. Maurice Freedman referred to Chinese religion as a 'total system', and the notion of a well-gearred, well-oiled total mechanism was linked to traditionalism. One of the biggest ideas at that time was Skinner's 'marketing scheme'. Skinner, by the way, taught at Columbia at that time, and was very friendly with Marvin Harris. They socialised together.

JT: In the 1980s, certain scholars left the field, like Emily Martin, while other scholars stayed on. Might certain changes have prompted some to leave, and others to stay?

MC: Emily Martin left the field before China opened. She said that since she can't do fieldwork in China, she was not going to stay in Chinese studies. Other did not share the same view, and jumped on the bandwagon and went to China when it opened up. Also, it is important to keep in mind that the opening up of China made anthropology in Taiwan really the anthropology of Taiwan. Those who stayed on were interested in researching contemporary Taiwan, and not in Taiwan as a representative of Chinese culture during the Qing dynasty. But, of course, certain places in Taiwan also possess scarce but important historical material that date back to the late

imperial period, which could be used for the kind of historical anthropology analysis that I am doing right now. Use of this material in historical anthropology must be distinguished from an anthropology that assumes traditionalism.

JT: So, in both Taiwan and China, we see a move towards what was happening *in situ*, shifting from looking at the here-and-now as representing the traditional, to looking at the contemporary.

MC: Some, among the Taiwan anthropologists, started out with a strong orientation towards the non-Han. This non-Han focus was brought in by anthropologists who came from Mainland China and did fieldwork among the Taiwan Aborigines. They produced important works. Likewise, Taiwanese who did graduate anthropological research in Taiwan almost exclusively worked among the Aborigines. Sociologists did research on the Han, and anthropologists did the Aborigines. The same thing had been going on in China. Right here, in the USA, there was a similar divide between sociologists studying industrial (mainly White) society, and anthropologists studying mainly non-White and 'primitive' people. To further answer your question, scholars like Li Yih-yuan really fought to have the Han brought into the anthropological fold. And of course it happened.

JT: A lot of changes have happened since the 1990s and also the 2000s, in terms of fieldwork, theoretical frameworks, and so on. What are some of your thoughts?

MC: One problem has been the lack of fit between today's major anthropological interests and the circumstances of China and Taiwan. For example, the whole focus on post-colonialism leaves out Taiwan's post-colonial experience and also Hong Kong's. There really has not been much effort to incorporate Taiwan and Hong Kong into the larger domain of postcolonial or subaltern studies. Even within East Asia, the Korean and Hong Kong experiences under the Japanese were very different. To get back to your point regarding key anthropological interests, with

China becoming a super power, it becomes all the more complicated. Anthropologists past and present aren't happy studying powerful countries.

JT: What about margins and borders? China's western border has been much talked about, and perhaps my work on Jinmen.

MC: With China's west border, again, you are talking about China's borders with the non-Han. Jinmen is an interesting case, because its marginality is linked to a very strategic position, since Jinmen is famous for being the one place where the Chinese communists were defeated. The nationalists retreated until finally, they stopped the communists at Jinmen. The point I'm trying to make here is that Jinmen's marginality is linked to its centrality. But it's up to you to deal with these issues.

JT: Of course. Now that we are nearing the end of the interview, might you have other things to share with us?

MC: All I can say is that doing fieldwork in China is not the same as doing fieldwork in Taiwan. You are dealing with a more complicated political structure, with a more complex administrative arrangement, but it's doable. And maybe, that's why it's interesting. And the other thing is that anthropologists complain of lack of freedom in choosing a field site. Even for anthropologists who were put in 'model villages' [they were assigned to these villages by the Chinese government], the important point, to let me conclude, is that when you put anthropologists anywhere for long enough, they will see some interesting things going on, for local life has its own quality, no matter where you are. Gregory Ruf, who was my student, was put in isolation every night but he learned a lot from the man assigned to watch him. With long-term fieldwork, many things may remain concealed, but there is also a lot revealed, of one kind or another. Who's to know that a broken-down house in the middle of the village was a haunted house? Until you've been there long enough for someone to tell you!

JT: We've come to the end of this interview. Thank you for sharing your experiences, your fieldwork and collaborations, and your views on the connections and disconnects from past to present. I look forward to many more conversations.

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**Junbin Tan** is a PhD student at the Department of Anthropology, Princeton University. Through fieldwork in Jinmen (金門), a Taiwanese territory located merely 2km from mainland China, he seeks to examine how religious rituals, festivals, and processions at this post-Cold War border facilitate the making of sociopolitical imaginaries that do not necessarily adhere to dominant geopolitical-territorial definitions.

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