



The Anthropologist without a Village:

Reflections on the Work of G. William Skinner

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This paper is in part an exercise in academic filial piety, but its larger goal is to assess the published work of G. William Skinner, a fairly uncommon anthropologist. He was an anthropologist without a village, who identified himself as a social scientist first and an anthropologist second. His work ignored disciplinary boundaries, which may in part be attributed to his education in colleges and departments that stressed area studies and interdisciplinary approaches, as well as to his choice of research topics. Much of his work, although based on data from China, is not limited to that society and should apply equally well to any society with a market economy but little or no mechanized transport. It should thus have much to offer to anthropology in general. I suggest that one reason for what seems to be its limited influence is the paradoxical idiosyncrasy of Skinner's work. Although it aspires to be impersonal and objective social science, it all strikes me as extremely distinctive and personal. That strong and original personality is reflected in the work. In some of his published work, the exposition of complexity may trump clarity and so deter some readers.

Keywords: social science, area studies, history of anthropology, regional systems, exposition

I

I first met Bill Skinner in September 1960 at Cornell University when I enrolled in his course on traditional Chinese society and culture. In the first week he handed out blank outline maps of China, and our task was to identify major rivers and mountains, provinces, and cities. After geography, the course moved on to demography. In the unit on the family, a major topic was variation in family size and complexity and the connection between such variation and social stratification. Later on he passed out a twenty-odd page mimeographed bibliography of English-language books and articles, arranged by topic. I do not recall much about marketing, but themes such as geography, demography and bibliography were there from early on.

Later, writing an undergraduate honors paper under Professor Skinner's supervision, I had the privilege of working with his data from a survey administered to Overseas Chinese in Java in the mid 1950s. The data took the form of a deck of IBM punched cards, and I learned to use the card-sorting machines in the University computer center and to do tests of statistical significance. Along with learning about the Chinese of Java, I was introduced to the common research methods of 1950s social scientists. His research office contained a large desk calculator with many keys that printed out its results on a small paper tape. On one occasion he remarked on my use of a slide rule to calculate some test of significance, and we had a brief discussion of the relative advantages of the slide rule and the desk calculator.

In 1964, as a graduate student in Anthropology at the London School of Economics, I was recruited by Professor Skinner as one of the first wave of coder/evaluators for the Chinese Society Bibliography Project. I recall sitting in the British Museum Reading Room with my copy of the extensive coding forms (reproduced in the endpapers of the printed Bibliography, Skinner 1973) and processing a large number of books and articles that I would certainly not otherwise have read. My impression is that each bibliographic record was designed to fit the 80-column IBM punched card. Looking back, the Chinese Society Bibliography Project (1963-1973) was probably not the best use of Professor Skinner's time and effort, but it was characteristic of much of his work in its ambition, its efforts to use state-of-the art information processing technology, and in its entirely self-directed and intellectually autonomous project design.

In July of 1965, with an LSE Master's degree in hand, I returned to Cornell intending to work on a PhD in anthropology under Professor Skinner. This however was not to be, since by September he had departed for Stanford. Nevertheless, his intellectual influence on me was profound. By then the marketing papers had been published, and

I, having digested them, set off for Taiwan in 1966 intending to study a market town and its middlemen and brokers, rather than a village and its farmers. That was not exactly what I ended up doing – I worked in Lukang, a former seaport city – but Skinner's work remained a major influence on my dissertation, published papers, and eventual monograph. (DeGlopper 1972, 1980, 1995) Later I found myself responsible for teaching courses on Chinese society and culture, and of course I always began by consulting Professor Skinner's old course outlines and recent publications. How else would one do it? This is one minor example of his influence on many younger anthropologists.

II

Many of us have probably heard the old joke about how you identify the anthropologist in a gathering of academics. The anthropologist is the one who begins every remark with "Well, in my village..." What often follows is an account of some incredible or bizarre behavior – people invite their fellows to feasts where the host destroys his own property, or they insist that women may have babies with no contribution from a man, or they claim that twins are birds. Or they may marry ghosts or adopt infant girls to marry them to their sons. The anthropologist then explains to us why those villagers do or say such things, and adds more stories about life in the village. One of the ways we learn and teach anthropology is through such stories.

But, the long and distinguished career of G. William Skinner provides a counter-example, for he was an anthropologist without a village. And while he could tell a story with the best of them – I will never forget his account of participating in a Brahms concert to entertain the People's Liberation Army troops in Chengdu in 1950 – his published work, extensive though it is, lacks those striking and memorable stories that we get from Malinowski, Geertz, or Evans-Pritchard. Of course, Professor Skinner was not trying to match their stories or to produce those illuminations of the exotic that lend themselves to lectures in introductory anthropology courses.

What he was aiming to do, however, is not quite so clear. In a general sense, of course, he thought of himself as a social scientist. As he put it in a paper delivered to a conference at UC Berkeley in March of 2002, "I see myself as a social scientist first and an anthropologist second" (Skinner, 2004). He was an anthropologist insofar as his research concerned societies and cultures other than his own, and he was concerned with translating between those societies and his own. He was presumably one of the few presidents of the Association for Asian Studies who was also a member of the U.S. National Academy of Sciences.

I recall his remarking more than once that with some 900,000-odd villages in China, a research program that consisted of intensive fieldwork in one village after another was hardly practicable or promising. To him, the need for research to go beyond descriptive village studies was obvious, as was the need to put those villages that were studied into some larger explanatory context.

Let us consider one of his programmatic pieces – an essay published in the August 1964 *Journal of Asian Studies* called “What the Study of China Can Do for Social Science” (Skinner 1964a). The categories he employs are sinology (old-fashioned, parochial, an end in itself), Chinese studies (modern, inclusive, a multidisciplinary endeavor with specific research objectives), and social science (modern, universalistic, comparative). For examples of social science, he refers generally to sociology, economics, and political science. As examples from his own recent experience, he refers to reviewing “the general social science literature” on three topics: correlates of sibling order, community leadership and power structure, and acculturation. Not surprisingly, he finds it all deficient because it confines itself only to Euro-American cases and examples.

He does mention what might be termed the subject matter of anthropology – village and family – only to dismiss it as not really all that interesting or theoretically significant. “There is nothing particularly exceptional about Chinese villages when compared to those in other peasant societies, nor for that matter about the Chinese family when compared to the family in other societies with patrilineal, patrilocal kinship systems.” Some of the topics he suggests for investigation by social scientists include Chinese cities, China’s historic large-scale socio-political integration, the imperial bureaucracy, the ways “primary social groups” (i.e., villages and families) are “structured to form larger systems” and “the enormously complex modes of integrating intermediate systems to form a single society *cum* polity *cum* economy.” In closing he alludes briefly to questions of modernization, of comparative Communism, and to migration (as to Southeast Asia).

Another programmatic statement appears in his 1973 preface to volume one of *Modern Chinese Society: An Analytic Bibliography*, which asserts on page ix that in the 1950s Western social scientists (none are named) were “coming to recognize the importance of Chinese civilization for a universal social science” and that the leading scholars of Chinese institutional history (none are named) “were developing a social-science-like concern for rigor and quantification.” Looking back from 2010, it seems that although the goal of a universal social science as well as a concern for rigor and quantification could be said to characterize Skinner’s own work, the widespread adoption of these concerns by either social scientists or historians seems to have represented more of his own hopes than a description of how academic fields actually developed.

III

I think that much of Skinner's self-definition as a social scientist can be explained by both his personal history and by the spirit of the times – the late 1940s and early 1950s – as manifested in the goals and priorities of his graduate department and major teachers, as well as the major sources of research funding in the U.S.

First, briefly, the personal. (Hammel 2009; Ohio University Libraries 2009) He was born in Oakland California in 1925. In 1942 at the age of seventeen he decided to attend a very distinctive tertiary institution called Deep Springs College. This was (and still is) a small two-year college located in the remote California high desert. It was founded by an eccentric millionaire who wished to educate promising young men by combining plain living, including significant amounts of manual labor on the school's cattle ranch, with high thinking, which took the form of a common curriculum of three seminars devoted to (1) the humanities, (2) the social sciences, and (3) natural science. The seminars were taught by a few tutors with generalized educations. The college recruited only those young men with very high academic scores and an interest in spending two years in an isolated total institution. It offered full scholarship support and assistance in transferring, once the two years were up, to such major U.S. universities as Stanford, Harvard, Michigan, and Cornell.

I believe that the usual course for a bright young man from the San Francisco Bay area would have been to enroll in the local public university – Berkeley, known locally simply as "Cal." At Berkeley the young Skinner would, had he been interested in anthropology, have been introduced to the department of Kroeber and Lowie, which taught the dominant U.S. four-field anthropology: cultural anthropology, archaeology, physical anthropology, and linguistics, and focused on Native Americans. His career would have probably been quite different.

When he turned eighteen in February 1943 he became subject to conscription, and ended up enrolled in a program for Naval officers that sent its cadets for training at U.S. universities before commissioning them. The Navy assigned Cadet Skinner to the Supply Corps, and sent him to a college in Missouri to study in its business school.

After a year of business school, he then, presumably at his own initiative, joined the Navy's 18-month Chinese language training program, at the University of Colorado. By the time he completed that, in 1946, the war was over and he returned to civilian status. He then enrolled as an undergraduate at Cornell, perhaps because of the connection to Deep Springs College and the associated Telluride Foundation. He received a BA in Far Eastern Studies in 1947, which would have meant more Chinese language, some Chinese

history and perhaps a tutorial with an historian or anthropologist. He then enrolled in Cornell's graduate school, aiming at a PhD in anthropology with training in what was at that time a joint Sociology-Anthropology Department. After perhaps two years as a graduate student, and a teaching position as an instructor in sociology, he headed off for fieldwork in Sichuan in mid or late 1949, funded by a grant from the Viking Foundation.

In the late 1940s the major figure in anthropology at Cornell was Lauriston Sharp (1907-1993), who had a long-standing interest in Southeast Asia and had served in 1945-1946 as assistant chief of the Division of Southeast Asian Affairs in the U.S. Department of State. Sharp was explicitly interested in pursuing anthropology in complex societies, in understanding social and economic changes in villages, in villages in their contexts, and generally interested in culture change and in applied anthropology. He was not a man to insist on narrow definitions of disciplines. He was also in touch with the world of foundations, such as the Ford Foundation, and one of the founding fathers of what became known as area studies. He founded Cornell's Southeast Asia Program in 1950 – it is still in existence. According to a recent book on area studies and the disciplines in U.S. universities, “in founding the Center for Southeast Asian Studies at Cornell... Sharp quite explicitly hoped to create a model that would be the basis for radically re-orienting Cornell into a set of multi-disciplinary topically-focused units” (Szanton2004). The author also speaks of Sharp's goal of “subverting the departmental structure of the university.” In the long run of course, the departments and disciplines have endured and such bold early 1950s initiatives as Harvard's Department of Social Relations turned out not to be the wave of the future; still, one can understand why a bright and ambitious graduate student such as Skinner would not invest a great deal in disciplinary purity and loyalty.

It is no doubt also significant that after losing all his research materials and being deported from Sichuan in 1950, Skinner spent the next seven or eight years in the employ of Cornell's Southeast Asian Program, both pursuing his own wide-ranging research on Overseas Chinese in Thailand and Indonesia and managing research offices in Bangkok and Jakarta. His first academic position in the U.S. (1958-1960) was as assistant professor in the Department of Sociology at Columbia University.

IV

Let me now return to the question of the sort of social science Skinner pursued. In all his work I see a distinct intellectual style. There is an obvious fondness for formal approaches, numbers, testing and verification. He prefers to rigorously work through the implications of an approach, to keep pushing the analysis to deeper or more inclusive

levels, to not get off the bus until it reaches the end of the line. He is also drawn to large-scale systems and to explanation in terms of total systems. He is interested in change, but prefers to speak of change either as the intensification of a system, or as cycles. Very much a hedgehog, not a fox (Berlin 1953). One comes to recognize a characteristic vocabulary and style. "Logically constructed hierarchical typology," "hierarchical set of verified generalizations," "complementary diversity within a region." Common words include analytic, differentiation, and complementarity.

Skinner aims to convince us by demonstrating how logically necessary the structure he identifies is. There is an air of sweet (and sometimes weary) reasonableness. How else could it work? What else would you do? Just as specific atoms (carbon, nitrogen, oxygen) and the nature of the chemical bond generate molecules with completely determined structures – think of the benzene ring – so his basic level regional systems will hook together in predictable and consistent ways, because they have to.

At least for some topics, his approach has deep explanatory power. After reading his work, such as the 1964 marketing papers and the 1977 essay on cities and the hierarchy of local systems, one will never think of a village, in any country – not just China – in the same way. Even if one does not share his concern with the details of the model and the formalisms, one brings away a very useful and indeed seductive vision. One might perhaps be justified in using the term "paradigm." He brings a great deal of disparate data together in a coherent pattern. I doubt that any student of Skinner's work would ever be comfortable using, in writing or teaching, such simple binary distinctions as urban/rural or center/periphery. Rather, one automatically thinks of ordered differentiation, so that a primary market town is more "urban" than a village, but less "urban" than an intermediate market town, and so on, step-by-step, all the way up to Shanghai or New York. Familiarity with Skinner's work permits one to avoid so many dead ends, self-generated paradoxes or nonsenses.

A nice example of his mature style and demonstration of the power and subtlety of his regional systems approach is provided by his 1997 eighteen-page "Introduction" to Leong Sow-Theng's essays on the history of the Hakka (Leong 1997). In a short compass this essay deals with such topics as migration, ecological adaptation, relations with an "aboriginal" ethnic group, population dynamics, the Hakkas' "surprisingly egalitarian gender system" and the consequences of that system, occupational specialties, male sojourning, rents for upland fields, lineage systems, foster daughter-in-law marriage, and ethnic mobilization. A distinctly Skinnerian observation is that: "... of all the major cultural/linguistic groups of South-Central China, only the Hakkas had no substantial drainage basin of their own" (*ibid.*:3). Another characteristic sentence tells U.S. that; "The net effect of Hakka/Pengmin settlement and exploitation of the near periphery of regional systems

was to transform a dichotomy into a continuum" (ibid.:14). Transforming dichotomies into continua strikes me as a good summary of much of Skinner's published work.

V

In Skinner's work we have an ambitious, wide-ranging total system that aspires to explain, if not everything, at least a very great many things. It would seem to some of us that it should have much to contribute, not only to Chinese Studies, but to anthropology in a general sense.

A view of the world that sees most or many elements of social life as derived, in the last analysis, from transport costs and sheer physiography is, of course, not restricted to any particular society. It should work, apply, everywhere. Or, at least for societies with market economies but without significant mechanized transport.

One possible reason for the relative lack of influence of Skinner's work on anthropology in general may be its paradoxical idiosyncrasy. Oddly enough, for an approach that aspires to be "social science": universal, empirical, impersonal, objective, and derived from logical necessity, it all strikes me as extremely distinctive and personal. I believe that if I were presented with anonymous paragraphs from the work of various distinguished anthropologists or China experts, I could usually identify those written by Professor Skinner, on the grounds of personal style. I do not believe there is anything like a school of Skinnerian anthropologists, nor any set of scholars who could be called his disciples.

If you knew the man, the personality and the voice comes through in all the work. Just as he was a person whom I recall as carefully organizing the rubber bands and paper clips in his desk drawer by size, so he was apparently compelled to set out every possible state of a periodic marketing system or in the essays on "Cities and the Hierarchy of Local Systems" to tell his readers in considerable detail about possible variations in Qing dynasty local field administration (Skinner 1977). Re-reading his various publications, I frequently found myself figuratively reaching for my blue editor's pencil.

Although he was perfectly capable of writing direct and simple prose, such as the several paragraphs on the standard marketing "community" in the first marketing paper ("If Mr. Lin, the 45-year-old peasant with whom I lived, may be considered typical ..."), too often such bursts of clarity and insight are buried in the midst of protracted discussions of such matters as schedules of markets. (Skinner 1964b) If I were called in to prepare an edition of Skinner's work for young anthropologists, I would begin the exposition with Mr. Lin (who "knew details about peasant families on the other side of the market town which most Americans would not know – and would not care to know – about

their next-door neighbors”) which tells us why we should care about marketing systems, even if our field site is in India or Nigeria, and relegate schedules to an appendix. Here, I should acknowledge Dr. Lawrence Crissman’s comment, in a personal communication, that, at least in his later decades, Professor Skinner’s brief introductions or lectures, such as his *Presidential Address* (1985) to the Association for Asian Studies, often provide a relatively accessible introduction to his ideas and work.

My own experience with assigning his essays to undergraduates and trying to teach his systems was that it was quite difficult. Many students felt overwhelmed by the details and variations and proved incapable of summarizing the basic points in a written examination. Ghost marriage was much easier to teach.

To reiterate a point made earlier, I do not think Skinner was trying to write for undergraduates. I would imagine he was writing for his professional colleagues and perhaps for some of the more competent and diligent graduate students. One might also argue that it is the responsibility of a teacher to translate the dense and idiosyncratic prose of such scholars as Professor Skinner into words that can be grasped by students who are bright but have no background knowledge of the topics and issues at hand.

VI

Of course, no scholar is perfect, and the career of G. William Skinner provides a model that most of us can only hope to emulate. Even the most superficial survey of citations of his work will bring home the wide range of scholars and writers who have learned from him. Reading an interview with Professor Charles Keyes, a noted scholar of Thai society and culture, I was impressed to find him referring to Skinner as a major figure in Thai studies, who helped to demonstrate that “the Thai” were far from being an homogenous people (Keyes 2007). Some of the most fulsome praise of Skinner’s work that I have seen is provided by a Hong Kong–based British business journalist and writer who, in a book on *Asian Godfathers* (2007, the reference is to gangsters, not kinsmen), refers to the monograph *Leadership and Power in the Chinese Community of Thailand* (1958). This is described as “only one really compelling piece of empirical research” reflecting “an extraordinary level of access to Thailand’s tycoon fraternity...No academic or journalist has produced a survey of such quality since” (Studwell 2007: 48). Presumably, the influence of Professor Skinner’s work can be expected to grow and intensify as the years pass by.

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沒有村子的人類學家： 反思施堅雅的作品

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美國國會圖書館（已退休）

本文是為緬懷先師而寫，但更重要的目的是評介非傳統的人類學家：施堅雅教授的出版著作。他是位「沒有村子」的人類學家，他的自我認定，首先是社會科學家，其次才是人類學家。他的作品不受學科界線所拘束，部分可以歸因於他在大學系所所受的教育——強調區域研究和跨學科的取徑。他選擇的研究課題，雖然以中國的資料為基礎，但他大部分的作品並不局限於中國社會，而是可以同樣應用到其他有市場經濟、但鮮少或未出現機械化運輸的社會，因此，其研究本應能更廣泛的適用於人類學並獲致更大的貢獻才是，但筆者認為使其影響力受限的原因之一，乃在於他作品中呈現的弔詭特質。他雖然立志成就非個人而客觀的社會科學，但對筆者而言，他的作品卻非常獨特和個人化，他強烈而獨樹一格的人格特質反映在他的作品上。在他的某些著作中，複雜的闡釋可能高過清晰的解釋，這也因此使得一些讀者卻步。

關鍵詞：社會科學，區域研究，人類學史，區域體系，闡述
