Doing fieldwork inside the PRC is an eye-opening but sometimes also deeply frustrating experience. Fieldwork-based studies form the foundation for our understanding of Chinese politics and society, but there are conspicuously few detailed descriptions in the China literature of how people actually do their fieldwork, and of the problems they encounter. This lack of public methodological debate not only undermines academic standards of openness: it also stalls constructive discussion on coping strategies to shared problems, and it leaves graduate students going to the field for the first time with a feeling of being the only ones to encounter difficulties.

In this volume scholars from around the world reflect on their own fieldwork practice in order to give practical advice and discuss more general theoretical points. The contributors come from a wide range of disciplines such as political science, anthropology, economics, media studies, history, cultural geography, and sinology. The book also contains an extensive bibliography.

This work is of relevance to postgraduate students from the social sciences and humanities who plan to do fieldwork in China; to experienced scholars who are new to the China field; and to experienced China scholars with an interest in methodological issues.

This book is essential reading for graduate students planning fieldwork in China. It brings out in the open information usually shared informally, and has lessons for students about theory, methods, and about China in these very personal cases. – Joseph Bosco, Asian Anthropology, 6 (2007)

Even though not written in the step-by-step format to chart the course of fieldwork planning and execution, this book contains first person narratives that provide the kind of immediacy not readily available in other standard textbooks. – Shu-min Huang, in Chinese Studies, 25:1 (2007)

This is not a nuts-and-bolts manual, but it is more than just a set of stories from the field. Here are some sobering and helpful accounts of how other anthropologists have struggled, sometimes succeeding and sometimes failing ... to carry out the research they envisioned from home. – Susan D. Blum, Journal of the Royal Anthropological Institute, 16:3, 2010
DOING FIELDWORK IN CHINA
DOING FIELDWORK IN CHINA

Edited by

Maria Heimer and Stig Thøgersen
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PREFACE

This book grew out of the workshop ‘Fieldwork Methodology and Practice in China’ held in Copenhagen, 2–3 October 2003. The workshop papers were later substantially rewritten, and we solicited additional chapters to cover aspects that we found important. We would like to thank the participants in the workshop who all contributed to an open and constructive discussion, and in particular Jørgen Delman and Cecilia Milwertz who organized it together with us. The Danish Social Sciences Research Council and the Nordic Institute of Asian Studies (NIAS) generously sponsored the workshop.

We are also grateful to the two anonymous reviewers and to Leena Höskuldsson, Gerald Jackson and Janice Leon of NIAS Press for many constructive inputs during the editorial process.
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INTRODUCTION

Stig Thøgersen and Maria Heimer

Doing fieldwork inside the People’s Republic of China is an eye-opening but sometimes also deeply frustrating experience. Fieldwork-based studies form the foundation for our understanding of Chinese politics and society, but there are few detailed descriptions in the China literature of how people actually do their fieldwork, and of the problems they encounter. In the best of cases, difficulties are brought up in the methodology section of a book or in an appendix, but more often they are stuck away in a footnote only to be quickly dismissed as insignificant. It often looks like this (a fictive example):

This study uses both documentary and interview sources. The author visited China on several occasions (1994, 1995, 1997 and 2001) and interviewed officials, workers and Chinese scholars. Interviews have provided valuable material for this study and I was surprised at the frankness with which interviewees described the situation. Minor problems were encountered in the collection of data but I judge they have not influenced the main findings of the project.¹

This tendency is in no way restricted to the China field. As Lareau and Shultz (1996: 2) point out:

There is always a gap between instruction and implementation, but this pattern of success and regret has been traditionally private. Though often acknowledging briefly that there were some aspects of the project that did not proceed as anticipated, researchers – including those who use field research techniques – often skimped across and minimized the inevitable difficulties in the field.
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One reason for glossing over problems encountered in fieldwork is no doubt a fear that the validity of our entire research will be jeopardized if we go public with those deviations from ideal methodological standards we are forced to make in the field. The resulting lack of public debate on fieldwork problems does, however, not only undermine academic standards of openness, it also stalls constructive discussion on coping strategies for shared problems, and it leaves graduate students going to the field for the first time with a feeling of being the only ones to encounter difficulties. Many have probably experienced the same misery at the beginning of a PhD project that Elin Sæther captures so well in her chapter. Minimal public information on the organization of fieldwork can add to the misery. For example, one of the editors (Heimer) felt inadequate when she engaged a Chinese student to take notes and to help out with interpretation at times during her first visits to China, only to discover later that practically nobody in the field worked without such assistance. The absence of such information in a public form hides how dependent foreign scholars actually are on local assistance and it blurs the picture of how fieldwork is carried out. Collective problems are, totally unnecessarily in our opinion, turned into individual ones.

Exchanging experiences would help us all, graduate students and experienced researchers alike, to make better compromises between ideal methodology and actual practice. To publicly debate difficulties in fieldwork – which we in this context shall define very broadly as going to the People’s Republic of China to collect information – would be conducive to the credibility of researchers and the quality of projects in the long term, and it would help to create a greater awareness of how fieldwork shapes our findings and, ultimately, our understanding of China.

AIMS OF THE BOOK

This book has two main aims: to provide a frame of reference for students and scholars who are new to China, and to provoke a public discussion among scholars in the China field on the problems we encounter in fieldwork, on possible coping strategies, and on how fieldwork methods affect our understanding of China. The individual authors give their personal accounts of how fieldwork actually gets done, along with reflections on how their experiences are linked up with more general questions around fieldwork methodology and findings.

We find available books on methodology and fieldwork wanting in these respects. Advice from general textbooks on methodology, to begin with, is of course invaluable to have at the back of one’s mind, but this luggage is often of
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limited help when faced with realities in the field. Instead of offering general advice, the contributors to this volume discuss their practice in the field, how they have coped with different types of problems, and how they have made acceptable (or sometimes unacceptable) compromises between methodological rules and reality. Also the more practical fieldwork handbooks are often overly general in their expositions. It is easy to agree that you should establish rapport with your main informants, or that you should write up field notes the same evening, but we want to move beyond such general advice. The readers should therefore neither expect a general methodology handbook or a guide on how to apply for a research visa or other practical matters. The contributors’ personal accounts do have more general relevance for fieldwork methods, but their broader points and generalizations grow out of their personal experiences.

It is our aspiration that the book will be of help to graduate students who plan to conduct fieldwork in China, particularly – but certainly not only – those who do not have a senior adviser in the China field to teach them the tricks of the trade. Another intended readership is researchers with experience from other regions who plan to incorporate China in their research. But above all we hope to engage experienced China scholars in reflections and debate on fieldwork methodology. Such discussions are relevant for all disciplines in the social sciences and humanities. Unlike many other books on fieldwork, the contributors to this volume are not exclusively anthropologists but also come from media studies, history, geography, economy, sinology, and political science. The reader will find many interesting reflections from the anthropological point of view in a recent volume edited by Xin Liu (2004) with contributions from a large group of prominent China anthropologists. Our approach is different, however, because we focus exclusively on doing fieldwork and on fieldwork methods.

We believe that these characteristics also make this volume useful as a textbook in more general courses on fieldwork methodology. The fact that we concentrate exclusively on fieldwork in China does not mean that we regard the problems as being unique to the PRC. On the contrary, many of them are common across countries, and given the fact that limited access is a problem in many countries, China fieldworkers with their long experience with various coping strategies should be able to contribute to ongoing discussions on general fieldwork methods. Our point of departure is that although fieldwork in the PRC is subject to many political restrictions the fundamental issues are universal. Compromises always have to be made between methodological rules and the actual reality in the field – whether carrying out a local community study in Sweden or writing an ethnography on religious rituals in Zimbabwe.
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– and the choices we make in the process always need to be highlighted and discussed. At the same time, in order for the discussion on fieldwork methods to be meaningful it should focus on concrete examples. China is unique, and so are all other places. Each country – or situation to be more precise – presents its own specific conditions and difficulties and calls for different solutions and coping strategies. De Soto and Dudwick (2000: 3), for example, argue for the specificity of the fieldwork dilemmas that post-socialist societies raise for the ethnographer, and Bestor, Steinhoff and Bestor (2003) present a number of personal narratives on fieldwork in Japan. For China, Anne Thurston and Burton Pasternak (1983) summed up experiences from the early 1980s when opportunities to do fieldwork in mainland China had just opened up. Much has happened since then, however, and we feel it is high time for an update.

At the same time, this book is also an argument for what we see as the most fruitful approach to fieldwork: to stay open to unexpected empirical findings in the field and to allow for a redesign of the project. This approach lies implicitly behind most of the papers, but it is presented most forcefully in Kevin O’Brien’s opening chapter, where he argues that some of the most interesting findings emerge exactly when our entire conceptualization of ‘the problem’ is reshaped in the process of fieldwork.

The specific context of doing fieldwork in China changes over time. We will begin with an exposition of the history of Western fieldwork in China up to the early 1990s in order to contextualize the fieldwork experiences of the contributors to this volume, who mainly discuss the period from 1990 onwards. This historical outline will lead up to a discussion of what we see as the most important general themes in the authors’ accounts.

W ESTERN FIELDWORK IN CHINA UP TO THE 1990S

A good way to see what is happening in a building would be to take its roof off, could that be done without disturbing its inmates. If we wish to comprehend the Chinese, we must take the roof from their homes, in order to learn what is going on within. This no foreigner can do. But he can imitate the Chinese who apply a wet finger to a paper window, so that when the digit is withdrawn there remains a tiny hole, through which an observant eye may see at least something. (Arthur H. Smith 1899: 16–17).

When Arthur H. Smith wrote these lines in his Village Life in China he had been a missionary in China for more than 25 years. The subtitle of his book, A Study
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in Sociology, shows how the observant eye peeking through the paper window was affected by the dawning social sciences, and in this sense Smith’s works, which also include the even more popular *Chinese Characteristics* (1894), are among the earliest, and certainly among the most influential, Western studies of China that can be said to build on fieldwork as a conscious methodological approach. They clearly bear the imprint of a time when racial stereotypes and generalizations were standard elements in social science discourse, but at the same time they reflect an intimate knowledge of Chinese society gained from the several decades Smith lived and worked in the country. When we compare the scope of his experience to the short-term visits under official supervision that post-1949 researchers have often had to settle for, his modest talk of a ‘tiny hole’ through which to observe Chinese everyday life becomes almost overwhelmingly ironic.5

The ideal image of the social scientist observing China ‘without disturbing its inmates’ fitted the scientific norms that had been established by the prestigious natural sciences, but it was, in fact, very far from the actual practice of Arthur H. Smith and other Western social scientists. Smith was clearly missionary first and social scientist second, and the aim of his social investigation was to find a way to convert the Chinese to Christianity. ‘There are in China many questions and problems,’ wrote Smith in his conclusion, ‘but the one great question, the sole all-comprehending problem is how to set Christianity at work upon them, which alone in time can and will solve them all’ (Smith 1899: 52). In the 1910s and 1920s, Western missionaries continued to ponder this great question, and some turned to the tools of the quickly developing social sciences in order to bring about fundamental social and spiritual change. In its early years, sociology as an academic discipline in China was almost completely dominated by American missionaries (Wong 1979: 11–19). John S. Burgess, the first head of the Sociology Department at Yenching University, and Sidney D. Gamble who ran his own institute for social research in Beijing were both associated with the YMCA, and they were also among the most influential pioneers of quantitative social surveys. In a large-scale investigation of living conditions in Beijing they collected empirical data with the triple purpose of forming the foundation for Christian social work, making Chinese students aware of the acute social problems of their own society, and developing ‘a social program that will influence the life of Peking and all of China’ (Gamble 1921: 26).

Gamble later did a comprehensive rural study in Ding county, Hebei province (Gamble 1954). The material for this study was collected between 1926 and 1933 when Ding county was one of the main centers for the Rural Reconstruction Movement that tried to reform China’s villages through social
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experiments in designated areas. One of the leading figures of this movement, James Yen (Yan Yangchu) praised Gamble’s report for having ‘contributed more than any other single factor to a scientific approach to the social and economic problems of the Chinese peasants’ (Gamble 1954: ix). John Lossing Buck’s (1968 [1937]) monumental study of land utilization based on data from almost 17,000 farms and 40,000 farm families also counts among the major contributions of survey research to an understanding of the economy of rural China. The American missionaries were very explicit about the close link between social studies and social reform, and some Chinese debaters accused their work of being more propaganda than social science. However, Chinese scholars and social reformers soon adopted the survey methodology with great enthusiasm and it is estimated that around 1,000 social surveys were conducted each year between 1927 and 1935 (Wong 1979: 16–17).

Not all foreign social scientists saw it as their mission to convert the Chinese or reform Chinese society. The Russian anthropologist Sergei M. Shirokogoroff, for example, who at Qinghua University became the teacher of China’s most famous anthropologist and sociologist Fei Xiaotong was a specialist on the Tungus and apparently did most of his fieldwork in China proper on anthropometrics, meticulously measuring the bodies and skulls of the Chinese he came across in order to identify the physical characteristics of the East Asian ethnic groups (Arkush 1981: 39; Guldin 1994: 44–46). However, to most researchers social investigation and social reform were intimately connected, and the very process of establishing sociology and anthropology as academic disciplines in China ran parallel to the development of social work.

While foreigners dominated the early years of survey research in China, community studies – the other popular fieldwork methodology in the pre-1949 era – were mostly carried out by ethnic Chinese who had either been trained abroad or by Western teachers in China. There were exceptions, such as the American Daniel H. Kulp II (1925) whose study of a Guangdong village was a pioneering work in the field of rural sociology (Wong 1979: 28), but even when we look at publications in English the most influential anthropological and/or sociological studies of China’s pre-1949 rural communities were works by native Chinese scholars such as Peasant Life in China by Fei Xiaotong (1939), Under the Ancestors’ Shadow by Francis L. K. Hsu (1948), and A Chinese Village by Martin C. Yang (1948). In his preface to Peasant Life in China Bronislaw Malinowski particularly emphasized that this work was ‘not written by an outsider looking out for exotic impression from a strange land; it contains observations carried on by a citizen upon his own people’ (Fei 1939: xix).
Introduction

Fieldwork during the Mao era

When the CCP took power in 1949 it meant an almost immediate stop to fieldwork-based research in mainland China. C. K. Yang who since 1948 had been conducting a village study near Guangzhou with some of his students was allowed to continue until 1951. When he left for Hong Kong, however, he was forced to leave all written materials behind and later had to reproduce the data from memory. His book gives a first-hand account of the land reform, but (very illustrative of the general situation) all information on the fate of the village after 1951 had to be ‘gleaned from the Communist and non-Communist press’ (Yang 1959: vii). Sociology departments in Chinese universities were closed in 1952 (Wong 1979: 43), and over the next 30 years only a handful of foreigners could do anything that even resembled fieldwork. Isabel and David Crook (1959, 1966) and William Hinton (1967, 1983) who had won the trust of the Communist leadership before 1949 were allowed to do follow-up studies in ‘their’ villages in 1959/1960 and 1971 respectively, and Swedish journalist and writer Jan Myrdal (1965, 1984) produced influential books based on interviews he did with peasants near Yan’an in Shaanxi province in 1962 and 1969. Except for such trusted ‘friends of China’, however, the door was firmly closed on foreign social scientists. When limited cultural and academic exchange with the West was re-established in the early 1970s, some scholars were granted three-week guided tours to carefully selected institutions (Kessen 1975), but this was a very meager substitute for the real thing.

Effectively barred from doing meaningful research within the PRC, foreign social scientists, particularly sociologists and political scientists, turned to refugees and emigrants in Hong Kong as a substitute. In the 1950s there were still few PRC immigrants in the crown colony and hardly any researchers who paid attention to them. Already in the early 1960s, however, there were enough relevant informants among the Hong Kong refugees to enable A. Doak Barnett (1964) and Ezra Vogel (1969) to write substantial, partly interview-based studies of what was going on in New China, and from the mid-seventies legal PRC emigrants poured into Hong Kong in such numbers that by 1984 one tenth of Hong Kong’s population had emigrated from the mainland since 1978 (Walder 1986: 260). This opened new possibilities for designing large-scale interviewing projects with informants who had recent and personal experience with practically all aspects of social life in China, and the Universities Service Centre for China Studies in Hong Kong became the academic base for scores of researchers on longer or shorter stays.
For several years works based on emigrant interviews gave deeper insights into the way the basic cells of Mao’s China worked than any other methodology, regardless of whether we look at villages (Parish and Whyte 1978; Chan, Madsen and Unger 1984, Madsen 1984), urban communities (Whyte and Parish 1984), industrial enterprises (Walder 1986), schools (Unger 1982, Shirk 1982), Red Guards (Rosen 1982), or political activism (Chan 1985).

It is striking that the methodological discussion in many of the works based on emigrant interviews is much more detailed than what we generally find in later works coming out of fieldwork in the PRC. The scholars working in Hong Kong evidently found it necessary to defend their method against accusations of bias and lack of representativity. They admitted that their data had certain problems, but claimed that they could be overcome by asking the right questions to the right people. ‘It would be a mistake to assume that most refugees are bitter anticommunists’, wrote Parish and Whyte (1978: 45), and went on to describe a number of procedures they used to detect and correct this and other possible problems during interviewing and data analysis. In the 1980s, when most of the books based on emigrant interviews were published, the PRC was opening up to foreign researchers, and it could be claimed that going to China would be a better way to create an authentic picture of what the country was really like. As Jonathan Unger pointed out in 1987, however, the PRC authorities still placed numerous constraints on foreign researchers. In Unger’s view Hong Kong at that time still offered several advantages:

There is no need to secure the official sponsorship of an organization inside China in order to begin one’s work; no pre-selection by government authorities of research sites; and no pre- or post-research censorship on topics and questions. Moreover, interviews in Hong Kong can be conducted in a manner that is normal to most social science research: that is, in a manner that safeguards the confidentiality and anonymity of respondents (Unger 1987: 28–29).

The Hong Kong based studies have certainly stood the test of time, and it would be difficult to point to issues where later research conducted in the mainland substantially revised their findings on pre-reform China. As things eased up in China, however, most sociologists and political scientists preferred to make their own observations there, and Hong Kong lost its importance as a substitute field site.

The anthropologists were in a different situation because interviews with emigrants from many different localities could not replace the close observations
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of small communities which most of them preferred. Some anthropologists did use Hong Kong as their field site, however, but they studied villages in the New Territories rather than emigrants. Others worked in Taiwan or with overseas Chinese, particularly in Southeast Asia. In an interesting commentary senior British anthropologist Maurice Freedman chose to see the advantages of the new situation. He felt that the continued piling up of samples of local communities that characterized pre-1949 research would have brought little new understanding of the larger issues, while the ‘new experiences in the study of Chinese society outside mainland China have taught new lessons’ (Freedman 1979: 383). Inside the PRC, large-scale anthropological fieldwork was carried out by Chinese researchers in the 1950s in connection with the classification of China’s ethnic minorities, their languages, and their social history (Guldin 1994: 105–144), but Western anthropologists, like all other social scientists, were banned from the mainland.

New openings – new restrictions

Between 1979 and early 1981 Deng Xiaoping’s policy of ‘opening up’ to the outside world gave foreign researchers opportunities for doing fieldwork for the first time in 30 years. China’s post-Mao rulers wanted to promote contacts with the West, not least in the fields of science, technology, and education, and, somewhat surprisingly perhaps, the social sciences and humanities were not excluded from the ‘scientific’ company. In early 1979, the first group of seven US scholars went to China for long-term study and research supported by the Committee on Scholarly Communication with the People’s Republic of China (Thurston 1983: 5), and around the same time other countries and universities all over the world set up exchange programs with Chinese counterparts. Anne Thurston describes the atmosphere among US China scholars during this early phase as ‘enthusiasm that sometimes bordered on euphoria’. Not only were they let in, they were even allowed to stay for long periods of time in rural villages. A revival of the community studies of the Republican era seemed to be within reach.

The euphoria did not last long, however. A US graduate student who did fieldwork in rural Guangdong from 1979 ran into troubles with the Chinese authorities (and with his own university), and the conflicts triggered a Chinese moratorium publicized in early 1981 on long-term fieldwork by foreigners (Thurston 1983; Pieke 1987). The situation never returned to the complete ban of Mao’s days, however. Visits of shorter duration remained possible, and
alternative research designs compensated for the loss of opportunities for long-
term participant observation. One of the most prominent examples was the
Shandong Field Research Project in Zouping (1988–93), where a group of US
scholars over a period of several years worked on different projects inside the
same county (Walder 1998).

During the 1980s foreign scholars were testing the limits of the new rules
governing their fieldwork, and the uncertainty about what it was possible to do
was evident on both sides. A personal example may illustrate this. In 1984 one
of the editors (Thøgersen) was based at Beijing Normal University preparing a
thesis on secondary education reform, and as part of this project the university
organized a field trip to Yantai, Shandong. Part of the plan was to do a survey
on the social background of students in different types of middle schools, and
the questionnaire to the students included questions on their parents’ job, edu-
cation, income, and Party membership. The whole questionnaire had been cleared
with the Ministry of Education in Beijing, and the authorities in Yantai had
no objections to it either. When I was leaving China, however, the custom
authorities would not let me take the questionnaires out of the country because
they regarded the information on Party membership as too sensitive. I had to
leave without the questionnaires, and without much hope of ever seeing them
again. After I had left, a Chinese friend solved the problem by erasing all answers
to the ‘illegal’ question by hand – in 700 questionnaires – under the supervision
of a custom officer (Thøgersen 1990: 132–53). It was also quite normal in the
1980s that local cadres would tell you that they could not photocopy articles
from ‘internal’ (neibu) journals or books for you, but they were willing to leave
them in your room for a few days and give you the address of a private shop with
a photocopy machine.

As these examples show, most rules were open to local interpretation and
therefore negotiable, but negotiations were often cumbersome and tended to
absorb much time and mental energy on both sides (for examples see Pieke 1996:
to do your own selection of interviewees, and could you talk to them without the
presence of officials? Which documents and publications would be accessible?
Such questions could lead to lengthy discussions between foreign researchers
and Chinese officials, often with the foreigner’s Chinese academic counterpart
captured somewhere in the middle desperately trying to make the foreigner under-
stand Chinese rules and make the cadres concede to international criteria for
academic work. The possibilities for doing fruitful fieldwork existed, but the
many uncertainties made it so dangerous to base one’s research plans primarily
on interviews and participant observation that Elisabeth Croll suggested that we should think of fieldwork as ‘the icing on a very substantial cake of documentary work which can stand on its own if, for some reason, the field work investigation does not eventuate or materialize in the anticipated form’ (Croll 1987: 18).

Official control over data collection and a general lack of autonomy were the overriding problems, and in order to solve them many researchers resorted to variations of what Thomas Gold called ‘guerrilla interviewing’, which he defined as ‘unchaperoned, spontaneous but structured participant observation and interviews as opportunities present themselves’ (Gold 1989: 180). Gold went to market places, rode taxis, and had his hair cut, and he used these opportunities to strike up conversations with private shopkeepers and entrepreneurs in an attempt to get those vivid ethnographic details and uncensored personal opinions that official media and supervised interviews did not provide. It is evident that foreigners in the 1980s learned much about China simply from being there over extended periods of time and talking to people they met. When someone asks you for the tenth time which work unit you belong to, you realize that work units are important elements of Chinese urban social structure, even if your research topic happens to be something completely different. In this sense the sudden opening up after decades of isolation together with the unpredictability of Chinese bureaucratic control contributed to developing a research style characterized by the opportunistic and unforeseen rather than the carefully planned. Many projects were pushed in new directions by sudden discoveries or unexpected barriers.

The violent suppression of demonstrators in Tiananmen Square in June 1989 was, in some ways, a watershed in the history of Western fieldwork in China. A considerable number of Western researchers were eyewitnesses to the dramatic events in Beijing and reports on the demonstrations in the provinces show how foreign scholars by this time had spread far beyond the capital (Unger 1991). Immediately after June 4th the prospect for future fieldwork looked grim. The large majority in the academic communities in Europe and North America turned away from the Chinese regime in disgust, and some recommended a boycott or at least a moratorium on official academic cooperation. On the Chinese side the State Education Commission issued a directive prohibiting Chinese universities from collaborating with foreign researchers, and foreign social scientists were monitored particularly closely (Brady 2003: 234).

The setback turned out to be temporary, however. Chinese academics were desperate at the thought of being isolated once again and did their best to maintain their international contacts, and few foreign social scientists could see any perspective in long-term sanctions in the academic field. After Deng
Xiaoping’s ‘Southern Tour’ in 1992 the climate again became more favorable for international contacts. The number of foreign business people and other permanent residents in China grew dramatically, and so did the number of foreign researchers and students, academic exchange programs, and collaborative projects. This trend has continued ever since.

DOING FIELDWORK IN CHINA IN THE 1990S AND ONWARDS: THREE THEMES

Since the early 1990s fieldwork in China has become more multifaceted and diverse. Each contribution to this volume discusses a particular dimension of fieldwork in more detail, and here we shall only highlight three important themes that come back again and again in the chapters: the overriding presence of the party-state; limited access to the field; and collaboration with Chinese academic partners and assistants. These issues loom large among the problems that call for coping strategies.

The presence of the party-state

Perhaps the largest challenge is the dominant presence of the party-state, or, to borrow from the title of Mette Halskov Hansen’s chapter, the fact that fieldworkers are ‘walking in the footsteps of the Chinese Communist Party’. There are two main aspects of this issue: direct political-ideological control, and the more intangible influence of the dominant Party discourse.

In spite of a more open atmosphere in China some topics are still, in the eyes of the Party, too politically sensitive to do research on, and some conclusions too sensitive to draw, as Emily Yeh’s chapter on her research in Tibet clearly shows. This can lead to problems with data collection, but may also – and more seriously – have unfortunate consequences for the informants. Although the political climate in Tibet is not representative for China as a whole, the underlying logic that Yeh describes is present, more or less, in all fieldwork, although it may lead to nothing more than that we prefer to keep an informant anonymous or keep secret the precise location of a field site. One dilemma for all involved is that the limits for what sort of information foreigners (and Chinese academics for that sake) are supposed to get access to are not clearly defined. It is revealing that Hu, the local cadre in Yeh’s paper, has no idea about what constitutes a politically sensitive issue and what could potentially get him into trouble. This is often decided *ex post facto*, depending on the political climate and the political effects...
of the report. From the perspective of the authorities the whole point is, of course, that when people cannot define the borderlines with any certainty they prefer to play it safe. Research reports published in an academic context are, however, rarely of interest to the Chinese authorities – in contrast to, say, negative reports in the international media, especially if they are based on internal information and used to bring sanctions upon China. Political sensitivity thus becomes a question of timing as well as of the audience and the use of the information.

For these reasons it is difficult to know beforehand which research topics will encounter political difficulties in data collection. As Marina Svensson’s chapter shows, human rights and the death penalty may unexpectedly prove to be easier topics than, for example, cultural heritage protection. There are also considerable local variations in the political climate, as is evident from the contrast between Björn Kjellgren’s experiences in Shenzhen and Emily Yeh’s account of her much more restrictive research environment in Tibet. Whatever the obstacles, however, there are many ways to work around them as the following chapters will show. We are no longer as dependent on what we are told or what we see in one place. There are a number of alternative sources of information, from critical articles in scholarly journals to the hairdresser’s opinion and comments in Internet chat rooms, which we can use to contextualize our studies and shape our understanding. We can also go through official channels and at the same time make our own independent interviews and observations, as Dorothy Solinger shows in her chapter. It is now possible to follow such complementary strategies without getting the feeling of doing ‘guerrilla interviewing’, because the people we meet inside and outside official contexts are often willing to discuss social and political problems more openly than in the 1980s.

In this sense it has become easier to study Chinese society, but we should not forget the other aspect of the Party’s presence, which is its role in constructing the discourse and in defining which topics we and our informants can and cannot talk about. One of our main concerns should probably be that no matter how politically ‘innocent’ our topics are, we often depend on official institutions, publications, and media. This leads to an immediate risk of getting stuck in official interpretations of Chinese social reality as formulated in the general political discourse, as Stig Thøgersen shows in his chapter on language strategies. It is worth noting that when social issues such as unemployment, poverty, and inequality (quite rightly) became major areas of research for both foreign and Chinese scholars in the 1990s, one reason was that the problems had grown more urgent, but another important factor was that the authorities themselves decided to pay more attention to these topics and also allowed more public focus on them.
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Access

Access to field sites and informants still has to be negotiated from case to case, but the reasons for limited access have partly changed. In the past scholars were under closer surveillance, but they did have the necessary arrangements made for them – with all the methodological dilemmas this involved. Today few institutions are eager to receive foreign scholars. Like elsewhere, Chinese state institutions have been downsized and had their budgets cut. Many are overloaded with requests for information and cooperation. When receiving foreign academics on shorter visits the host institution usually bears the costs in terms of meals, transportation, and time. Academic projects may not be directly useful for the host institution or the locality, and the trouble involved in receiving foreigners is greater than in the case of domestic scholars, who may also be more useful since they participate in the national debate and may influence policy-making. As a consequence, official approval of a project is sometimes of little help. Even though the majority of foreign scholars probably still work through an official affiliation with a Chinese work unit (such as a university, the Chinese Academy of Social Sciences, a government unit, or a social organization), this affiliation in some instances amounts to nothing more than fixing an invitation for the purpose of getting a research visa. Depending on their status in the administrative hierarchy some universities and academic institutes themselves lack the official weight and contacts to make arrangements for fieldwork for foreign and Chinese researchers alike. Especially with projects focusing on urban Chinese society, it may just complicate things to go through official channels. In relation to gaining access, it is interesting to note that working as a consultant may open up new gates to the field, as Baogang He shows in his chapter.

The social diversification and polarization of Chinese society have had their own consequences for our access to certain types of informants. Li Zhang has described how difficult it is to enter middle-class housing enclaves that are ‘gated, walled, and protected by surveillance cameras’ (Zhang 2004: 67). Rich entrepreneurs often cannot fit lengthy interviews into their busy schedules, and Chinese urban people in general evidently have less time for talking to foreigners than they used to. It could also be argued that we have less access to the party-state today. In some sense, the ‘friends of China’ of the 1960s were more integrated into the Chinese political establishment than we are now. Even though they were controlled and sometimes reported upon, foreign scholars were sent to visit model areas and institutions, which were by no means representative, but which were, at least, highly instructive for what the government wanted to achieve. Since models were very often designed as counter images to what
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did not function in other places, the most urgent problems in Chinese society also became visible in this way. As a consequence of less access and because our research interests are turned elsewhere, it could be argued that we actually have a more shallow understanding of the party-state today.

During the Mao years, scholars worked out coping strategies to handle the limited access to the field, and today’s strategies are in some ways surprisingly similar to those of the past. The difference is, perhaps, that we discuss them less today. One reason for the lack of reflection may be that fieldwork problems in China have become more like those found in many other countries so the need to defend doing fieldwork in China at all is no longer there. Limited access to the field and difficulties in data collection have created a need for multiple documentation. When short-term field investigation replaced sustained and intensive field experience in the 1980s, it was, according to Croll (2004: 89), conceived as a supplement to and framed by extensive documentary and comparative research. Today, Western fieldworkers still see the need to supplement fieldwork with documentary sources and to make comparisons with findings from other areas to validate the data they collect (Kim 2004: 150–51). A common coping strategy that we see after the 1980s is to conduct fieldwork in multiple field sites. Instead of basing research in one or two localities as in the classic ethnographic study, many fieldworkers from all disciplines tend to carry out their work in a relatively large number of different field sites in China. Maria Heimer suggests in her chapter that while this tendency is actually a response to the difficulties in organizing fieldwork and collecting data, the use of multiple field sites is not often discussed in this light, so a valuable coping strategy remains hidden.

Collaboration

For most types of access we rely on contacts inside the PRC, formal or informal. It would be an exaggeration to say that the quality of a project correlates with the quality of one’s relations within the PRC, but good contacts are often a necessary prerequisite for doing research. A continuous feature of China studies is the dependence on Chinese academic partners and assistants which is reflected in many of the contributions to this volume. In some instances, part of the data collection may be carried out by a local assistant or partner but this piece of information is not mentioned in the final research product, or even in the acknowledgements. Such omissions are of course both an ethical and a methodological problem, and they hide how dependent foreign scholars actually are on local assistance.

However, truly collaborative research where foreign and mainland-based scholars jointly design surveys, analyse the data, and publish the results are
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definitely on the rise (Walder 2002, Chan and Unger 2003: 329). It seems to us that economists are often among the most successful in establishing such partnerships with their PRC colleagues, and the chapter by Björn Gustafsson and Li Shi is the result of such collaboration. A large proportion of joint publications are actually based on survey research, where it is a formal requirement that a domestic institution licensed to carry out surveys together with a foreign partner sponsors the project. The Chinese authorities place more restrictions on quantitative than on qualitative research, which probably reflects that they find the former more threatening, and that they do not regard the latter as real research (see Bu Wei’s chapter for an elaboration on this point). Collaborative research based on qualitative methods appear to be less common, at least when it comes to analyse the data and publish together. Mette Thunø describes in her chapter the problems but also the benefits of collaboration in two joint projects based on ethnographic fieldwork.

It should also be noted that market mechanisms also work in the field of academic cooperation. It is not difficult to engage PRC colleagues in large-scale collaborative projects if you are a professor from a famous university with ample funding and scores of publications to your name, but many PhD students and even senior researchers do not have similar opportunities, and many Chinese institutions run on meager budgets that make it difficult for their staff to engage in field research without external funding simply because of the travel and living expenses involved.

Looking ahead
China’s economic and political reforms have not followed a one-way street and have often been contradictory in nature. In some areas the state has withdrawn and left more space to private actors, while it has tightened its grip on market forces and political institutions in others. Likewise, there has not been a linear development towards increasingly unrestricted fieldwork conditions in China over the last decades. While it has become easier to approach ordinary citizens and at least some non-governmental organizations, access to official institutions has in some ways become harder to negotiate. So although the specific constraints will vary from fieldworker to fieldworker, between places, and over time, we anticipate that the overall constraints surrounding fieldwork today are not a passing stage but will remain a reality we have to cope with for years to come.

One important negative effect of these constraints is that even though we are able to work our way around the obstacles, fieldwork tends to become quite frustrating and time-consuming. Some students of China may feel that
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the outcome of a fieldwork-based project is too uncertain and therefore exclude fieldwork from their project design, or only do a short trip to the field. Others may carry out extensive fieldwork for one project but decide that it is too troublesome for the next and instead opt for other ways to access data. So there is a real risk that fewer scholars will spend time on extensive fieldwork in the future.

Constraints and coping strategies can, however, also lead us in new and unforeseen directions and deepen our understanding of how things work in China. Many of the contributions illustrate exactly how this happens and the gains and insights to be made. We have argued for staying open to unexpected findings in the field and allow for a redesign of the project. Frank Pieke (2000) goes further when he suggests that we integrate serendipity – defined as the faculty of making happy and unexpected discoveries by accident – into the design of projects. According to Pieke, unexpected discoveries are less random than the above definition suggests, and can be created. He writes that anthropology requires a methodology (or at any rate a strategy) to invite serendipity in, and he favors a project design that creates enough opportunity for the unexpected to happen (2000: 45–46). Without idealizing problems in the field, constraints and coping strategies must also be seen in this light, and be used to that effect. To gain a better understanding of how China is changing – and why it may be changing in a different direction than we anticipated – it is absolutely essential that we build a stronger base of empirical observations from the field. We need more fieldwork-based studies, and we hope the following chapters will serve as an encouragement in this respect.

ORGANIZATION OF THE VOLUME

The first group of chapters deals with the role of fieldwork in overall research design. Kevin O’Brien discusses the crucial role of fieldwork for redesigning research projects and for theory-building. Elin Sæther reflects, as a newcomer to the field, upon the uncertainty inherent in fieldwork and shows how the frustrations in the field and the fieldwork itself can be a learning process. Maria Heimer’s chapter focuses on the selection of field sites and examines different approaches to field sites in the literature and their relation to the type of findings we make.

The second section is titled ‘Official China and Beyond’. Mette Halskov Hansen discusses the limitations involved in ‘walking in the footsteps of the Chinese Communist Party’ and suggests how we may be able to get around them.
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Emily Yeh describes the political constraints on doing fieldwork in Tibet but also shows how those same constraints led to unexpected insights. Stig Thøgersen looks at the importance of being aware of official and unofficial language codes and suggests some elements in a strategy that is sensitive to the socio-political aspects of language use.

The third section deals with how we collect data, and what these data collection procedures mean for our interpretations. Björn Gustafsson and Li Shi discuss the advantages and disadvantages of various strategies chosen by researchers in the large-scale collection of quantitative data on household income. Dorothy Solinger has rich experience in interviewing high-level officials, local bureaucrats, factory managers, rural migrants and laid-off workers and she tells us about the art of interviewing. Baogang He shows how consultancy can offer an alternative gate to the field with significant advantages for data collection. Stig Thøgersen looks at different types of written sources and how collecting and reading documents can form a dynamic part of the fieldwork process.

The fourth section elaborates on the insider-outsider issue and illustrates that no simple division line can be drawn here. Bu Wei looks at the status of qualitative research in the social sciences in China, and then goes on to analyse her own ‘outsider’ experiences as a Chinese social scientist among trafficked women in Sichuan. Björn Kjellgren considers the impact of the researcher’s personal background (discipline, ethnicity, age, gender, etc.) on the fieldwork process and the collection of data. Mette Thunø discusses the potentials and dilemmas of research collaboration. Finally, Marina Svensson describes the problem of being drawn into local conflicts and conflicting loyalties, and how the researcher must balance involvement and distance.

Anja Møller Rasmussen, Inga-Lill Blomkvist and Mads Kjeldsen have compiled an annotated bibliography of fieldwork accounts from China published since 1990; we hope that it will be of help to the reader. It is attached as an appendix.

AUTHORS’ NOTE

We wish to thank Kevin O’Brien, Mette Halskov Hansen, Jon Unger, and two anonymous reviewers for their comments.
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NOTES

1 Many omit to mention when, where, who (here referring to profession or position rather than name) and how many persons they have interviewed, which should be a basic standard.

2 An excellent book in this genre that gives very realistic accounts of how fieldwork is placed in the research process as a whole is Lareau and Shultz 1996.

3 Such practical advice is actually hard to find, but it is worth consulting Thurston 1994 although some conditions, of course, have changed over the last ten years.

4 Agar (1980) wrote in the first edition of The Professional Stranger that there is a tradition in cultural anthropology that one cannot be told how to do fieldwork and, as a consequence, there has been a lack of tradition of fieldwork training. Of course, this has changed since then and Agar’s own book (an expanded second edition came out in 1996) is an excellent example of the opposite. The fact remains though that there is a gap between general research methodology and stories from the field, and this is true across disciplines.

5 According to an obituary Arthur H. Smith lived in China for no less than 54 years (Claremont Courier, Sep. 2, 1932), www.beloit.edu/~libhome/Archives/acoll/alum/ahsmith.html

6 A discussion of the development of sociology and anthropology in China as well as of the foreign scholars involved in this process can be found in Arkush 1981; Gransow 1992; Guldin 1994; Wong 1979.

7 To get an impression of the University Service Centre’s importance for China studies, see The China Journal, vol. 53, Jan 2005, which is a special issue celebrating the centre’s 40th anniversary.


9 See Tang 2002 for a very useful and practical research guide for doing surveys in urban China.

10 There are several examples of successful collaborative projects based on qualitative research methods between Chinese and foreign scholars, but few of the Chinese involved are apparently based in the PRC.
REFERENCES


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Section I

THE ROLE OF FIELDWORK
IN THE RESEARCH PROCESS
Since it became feasible in the late 1970s, interviewing has become the heart of much fieldwork conducted in China. From western political scientists who speak with grassroots officials to Chinese ethnographers who do oral histories in their home villages, conversation has become a signature form of data collection in the China field. At the same time, many Sinologists are more deeply embedded in the social sciences than ever before. Although these scholars usually continue to publish in area studies outlets, they also write for anthropology, economics, political science, and sociology journals, in which the expectation is that their research will contribute to theory development in some fashion or other.

Bringing what we know about China to our disciplinary colleagues is certainly a worthwhile goal. But it is often a challenge to adapt the freewheeling stories that interviews elicit to the demands of theorizing. The strength of interviewing is, of course, the particular and the vivid. Claims to representativeness are always problematic, especially in a place like China, where meetings are often arranged through the most idiosyncratic of channels. (Some of the best interviews I ever did arose from literally bumping into a well-connected former student on Wangfujing – one of Beijing’s busiest shopping streets.) Given the opportunistic way many of us gather information and the ‘non-standardized data’ (Dean et al. 1969: 20) that interviewing produces, how can interviewing in China contribute to theory building?

In this chapter, I will sketch an approach to research design that hinges on staying open to unforeseen ideas (and even new topics of inquiry) that emerge in the course of interviewing. I will use my own study of local people’s congresses in the early 1990s (as well as examples drawn from more recent work on village
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elections, policy implementation and popular resistance) to illustrate how one young fieldworker fumbled his way toward a type of research that aims to speak to both China scholars and disciplinary audiences. The strategy I espouse bears some relationship to ‘grounded theory’ (Glaser and Strauss 1967) and is a form of ‘exploratory’ analysis (Gerring 2001: 231–32). It is a modest enterprise that accepts (even revels in the fact) that the best generalizations are usually bounded. It encourages us to shop for existing theories and concepts before and after we head to the field, but not to buy much of what we find (Jones 1974: 219), especially when interviews make it clear that our preconceived notions have led us to miss the real question or imagine a dilemma that does not exist. This strategy treats research design as an ongoing process and emphasizes discovery rather than verification (Gerring 2001; Glazer and Strauss 1967). It attaches considerable importance to the subjective experience of interviewees (Schwartz 1996: 107) and regards shoe-horning evidence into ‘ill-fitting a priori categories’ (Gerring 2001: 231; also Glaser and Strauss 1967: 253) to be just about the greatest sin imaginable.

When the approach works, the result is knowledge about China and, equally importantly, insight into some theoretical question or problem (Gerring 2001: 22–23; Kaplan 1964: 77–78). When convincingly done, such research may even succeed in nudging our disciplinary colleagues to take Chinese experiences more seriously and to acknowledge that a theory they hold dear is not quite as universal as they thought.

RESEARCH (RE)DESIGN

The best advice I ever heard about research design centered on how to approach a prospectus defense. R. William Liddle frequently told PhD students at Ohio State University that submitting a dissertation proposal was like applying for a fishing license. We, the representatives of your field, are granting you permission to go fishing in a certain place where we are fairly confident there are some fish. In presenting your research proposal, you need to persuade us that the project is both feasible and interesting. But we don’t know which fish you will find, and more importantly, we can only guess which fish is going to be the biggest. You have to figure that out when you are in the field. Some contacts may prove to be better than others. Someone may drop a fascinating book based on fieldwork into your hands. An aspect of the project that you thought was minor may suddenly start to look like the big payoff, both empirically and theoretically. This counsel underscored what I take to be the one unalterable key to developing
rich, sound data that also has theoretical repercussions: flexibility. Researchers must recognize, in other words, a big fish when they catch one, and not throw it back, just because they started out looking for some other fish, which when they located it turned out to be tiny, uninteresting, and not-too-tasty.

This small, even mundane point, which is particularly relevant because China is still a relatively closed polity and much social science theory was developed to study very different sorts of places, has many implications for research design. First, fieldworkers should always strive to be open to new ideas, theories that did not originally seem pertinent, and new research foci. Like good journalists, we ought to arrive in the field with an area of interest and some hunches about how a social process is unfolding, but then be ready (and eager!) to let our informants redirect us by telling us what concerns them most.

Consider, for example, my experience in a Shandong village in 1994 when I thought I was doing a project on village elections. Lianjiang Li and I had prepared dozens of questions on the implementation of the Organic Law of Villagers’ Committees, and the local cadres we spoke with tried their best to answer them, but honestly, they were completely listless on the subject of grassroots democracy. Then, without warning, they suddenly perked up when someone brought up the topic of ‘ten-star households’. These ‘stars’ were related to village charters that were being drawn up under one of the lesser known features of the villagers’ autonomy program, and households received stars for tasks such as paying their taxes, respecting the law, keeping their yard clean, keeping their pigs off other people’s property, and taking good care of the elderly. Interestingly, the stars were awarded to cadres as well as villagers and where everyone stood was public knowledge: how many stars a household had received was indicated right on top of the family doorway. Most fascinating of all, our interviewees told us that young people preferred not to marry into households that had fewer than eight stars and cadres who fell below eight or nine were at risk of losing their position or at least their status in the village.

Our goal during this research was to examine the implementation of basic-level elections, but in this village the real story concerning political accountability and social control centered on these stars. In these circumstances, the best way to discover what there was to be learned was to let our informants talk and just listen. We occasionally drew them back toward our original area of interest, but mainly let them fill in one piece in a mosaic that might be called ‘village political life’ or ‘grassroots cadre-mass relations’. We did not use forced choice questions and in fact abandoned many of the open-ended questions we had intended to ask. Nor did we employ a formal coding scheme to make sense of their remarks.
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on this new and entirely unanticipated topic. It did not even bother us greatly that we had never asked about stars in any other village we visited. This was the story in this location; this is what interviewees became most animated and most concrete about, so we were happy to learn what they had to say. We would figure out how their remarks fit into our project later, or even better, we would mull the interviews over and consider whether our current project (and a set of theories and concepts we were just itching to deploy) should be modified to incorporate the insights derived from our stay in this village.

In this particular case we did not alter our research focus or go searching for new concepts or theories to lend order to our findings (on placing ‘ten-star households’ in a broader context, see Thøgersen 2000: 138–40), but fieldwork that same summer in other parts of Shandong and Hubei did lead us in a direction we could never have foreseen. This occurred when a series of cadres repeatedly brought up problems they had encountered dealing with diaomin (shrewd, unyielding people) or dingzihu (nail-like (i.e. resisting) households). Some of our informants also made a distinction between these two varieties of villagers based on how ‘reasonable’ (jiangli) they were. Before our interviews, it had never occurred to us to develop a typology of rural protesters, but we were quickly persuaded that this project (Li and O’Brien 1996; O’Brien 1996) was far more interesting (and better supported by the evidence we had gathered) than our original aim of investigating whether electoral reforms designed to increase stability had actually decreased it.

Students of social science methodology call these mid-course corrections ‘mutual adjustments’ (Gerring 2001: 231) or ‘successive definitions’ (Kaplan 1964: 77). This simply means that a researcher’s topic of inquiry and the concepts or theories employed to understand it may evolve in the course of conducting research (in our case, by doing interviews). In such exploratory work, ‘doing’ often precedes ‘knowing’ (Jones 1974). Theory and evidence are closely intertwined and theories, concepts and evidence come to be aligned at the end of the research (Gerring 2001: 231; also Dean et al. 1969: 22–23). The ‘analysis is at once inductive and deductive, like someone who is simultaneously creating and solving a puzzle, or like a carpenter alternately changing the shape of a door and then the shape of the door frame to obtain a better fit’ (Emerson et al. 1995: 144). Data-fitting, the horror of researchers who seek to confirm or disconfirm preformed hypotheses, is a boon (Gerring 2001: 232) for people like Lianjiang Li and me, who continually adjust the scope of our investigation as our fieldwork unfolds, and who only settle on which concepts and theories to employ once the data is all in.
That prolonged ‘soaking and poking’ can alter the empirical and theoretical questions under study (quoted text in Fenno 1978: 250; Gerring 2001) encourages researchers to focus on the specific – precisely the forté of interviewing. In my experience, the most important statement to interject when speaking with Chinese officials or ordinary citizens is ‘please give me an example’ (qing ju yige lizi). I have conducted interviews on the people’s congress system in the Great Hall of the People, where scholars or officials started quoting the Constitution while our precious time was wasting away. Lianjiang Li and I have talked with villagers who understandably enough had difficulty deciphering western social scientists’ questions, and who did not even quite comprehend what academic research was. In these circumstances, keeping questions factual and down-to-earth is crucial. I inevitably learn most when an interviewee tells a long, involved story full of incidents that I can chew over for their import. Upon hearing the tale, I might not immediately grasp what many of the twists and turns mean, but that’s not important. Field researchers need examples to think with in order to refine their topic and test their ‘priors’. Especially for projects with conceptual or theoretical aspirations, it is useful to treat a detailed, winding story like a prism, which the researcher can keep turning around and around (sometimes years after leaving the field) to see what colors it casts off. What is not comprehensible now might become so at a later date, perhaps in the midst of writing, when a hole in the analysis suddenly appears. Abstractions generally will not help fill the hole. If relied upon unduly, the researcher will most likely slide right over an important counterintuitive or counter-literature point and do what we all do sometimes: deduce what may be happening rather than provide a rich and telling example of what actually is occurring somewhere. Much of the concrete, experience-near data I am extolling is frankly anecdotal (it is not even appropriate to call it a ‘case study’), but if it is deployed fairly and well, readers will recognize the kernel of truth in it. It will, in other words, ring true, and accord with the experience of other researchers in other parts of China or elsewhere. It may even help other scholars see their own topics in a new light. And should others examine what has been claimed and discover that an anecdote is not truly representative, or has been misconstrued, all the better. Attaching scope conditions to generalizations is an important way that a research community makes progress.

Let me offer some examples. My wife is a forest ecologist, so when she has accompanied me to the field I have often had an opportunity to gather stories about forest guards threatening local cadres who attempted to fire them, or...
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people’s congress deputies supporting better fire-fighting, improved schools or
gun control in remote, mountainous areas. I have never written about forestry per
se, but conversing with foresters about the specific concerns that trouble them has
frequently had a payoff in producing nuanced data that I can review and bounce
off the theoretical predispositions I arrived with. Interviewees most of the time,
not all the time, want to please us. They just don’t know what we need. If we
talk with them about rarefied concepts like representation we will most often get
blank stares. But we can find out much of what we need about representation
and how well the concept travels, for example, by asking interviewees to explain
who votes for a family where one person casts the ballot for each household,
or what an informant’s views are on raising people’s congress deputy quality
(which also implies downgrading the role of worker and peasant representatives),
or by hearing, as I did in 2002 in Yunnan, a woman candidate give a speech
moments before an election, in which she pointed out that her only female rival
had withdrawn, the village had been allocated a quota of one female villagers’
committee member, and voters should act accordingly. Intensive, open-ended
interviewing (and unabashed story-mongering) is the best way I know to locate
the topic I should have been studying in the first place (see Fenno 1978: 250–51:
Dean 1969: 22), to avoid ‘tendentious characterizations’ of a phenomenon, and
to resist the ‘impulse to vindicate a particular theoretical outlook’ at the expense
of empirical validity (for the quoted text, see Shapiro 2002: 597).

There are of course well-known problems related to bias and even truthfulness
that spring from relying on a limited number of informants in a small range of
places. For fieldworkers who rely on interviewees as much as Lianjiang Li and I do,
it is a continual struggle to sift out remarks that are unreliable, unrepresentative,
self-serving or ideologically-driven. But at this point in our study of China, at
least for the questions that interest me, I would opt for depth of knowledge over
breadth of knowledge nearly every time. And this is doubly important when
one of our primary goals is to determine if a given theory or concept should be
jettisoned, modified, or employed to organize what we have turned up in the
field.

Readers may question whether so much can be gleaned from interviews.
And they would be right: interviews have limits. They are never complete and
Lianjiang Li and I always wish we had asked certain questions at the outset
that we only came up with three months later. None of my comments so far
should be understood to denigrate the importance of other types of evidence.
Few topics can be studied based solely or even mainly on interviews. I myself
have never published anything in which written sources were not at least half of
the total citations. Interviews, as critical as they are for filling in missing links in an argument and identifying frameworks that fit, are inevitably partial.

This is why materials developed by others can be invaluable. More than once, superb secondary sources have helped Lianjiang Li and me interrogate the theories and concepts we planned to deploy and have led us to revise our understanding of what we were studying. Toward this end, some of the best written materials I have come across include: (1) field studies by Chinese researchers (including an internal book (Zhao and Wu 1990), which was crucial to my work on local people’s congresses); (2) inside-the-bureaucracy communications and documents in which officials related problems they faced in their work (this has been pivotal for exploring the implementation of village elections); (3) unpublished and pre-edited articles and letters to the editor that Lianjiang Li and I obtained from a journal on township affairs that employs thousands of stringers scattered across China; and (4) responses to open-ended questions on a series of village surveys Lianjiang Li conducted from 1997 to 2004. All these written materials gave us a little more confidence that interpretations we had derived mainly from interviewing were not overly idiosyncratic or flat-out wrong.

That said, I still believe that interviewing can play a crucial role in theory building by illuminating (better than any other method) the institutional environment in which informants live. This is especially true for officials. Like most people, Chinese cadres are usually willing to go on at great length about how hard their life is and the many incompatible demands they face – at the grassroots, this would include building roads without levying too many fees, carrying out the birth control policy but not using coercion, implementing village elections but ensuring that only reliable people win. This may all sound like garden-variety grumbling, and much of it is, but it also allows the researcher, just briefly, to see issues the way people on the ground see them. Interviewing, in a word, enables outsiders to locate research problems of genuine importance (and to prioritize among possible explanations) by discovering what is agitating people in the thick of the system. This is how Lianjiang Li and I ended up writing about the ‘cadre responsibility system’ (ganbu gangwei mubiao guanli zerenzhi) (O’Brien and Li 1999; also Edin 2003), a topic that was not on our agenda when the village elections fieldwork began. Again and again we heard about 100-point scales, hard and soft targets, hard aspects of soft targets, and other mysterious terms entirely unfamiliar to us. This is what a series of officials wanted to discuss when Lianjiang asked why they had so little enthusiasm for carrying out popular policies, like village elections, and so much enthusiasm for implementing unpopular policies, like birth control and collecting taxes and fees. This brings
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us to a key point: exploratory research is always circular and it matters little where it commences. We may ‘begin with a hunch, a question, a clearly formed theory, or an area of interest’ (Gerring 2001: 22). There is only one place from which we can start – where we are (Kaplan 1964: 86). For me, this means engaging social science theories before and after arriving in China, but also taking interviewees’ problems and the way they explain them to heart. Go to the field, as I have, with discipline-driven questions about representation, institutionalization, and political participation, but be ready to come out with answers about remonstrating (O’Brien 1994a), embeddedness (O’Brien 1994b) and rightful resistance (Li and O’Brien 1996; O’Brien 1996; O’Brien and Li 2006).

INTERVIEWS ON LOCAL PEOPLE’S CONGRESSES

By late 1989 I had finished a book on the National People’s Congress (O’Brien 1990), based largely on archival research, with a short stint of fieldwork at the last moment (when it became possible to attend the 1989 NPC). Conducting interviews with deputies and staff members enlivened the book, and running into a former student helped get me out of the public gallery and the staged press conferences and into some small group meetings and interviews. Still, the book was mainly historical and a product of library research. To the extent it engaged social science theories and concepts, it borrowed familiar ones from the study of western parliaments (e.g. representation, responsiveness, oversight, liberalization, rationalization) and the state socialist systems of the Soviet Union and East Europe (e.g. regime support, inclusion).

In the late 1980s, when the book was nearly complete, I began designing a project to study local people’s congresses. I wrote several grant proposals employing one of the key concepts – representation – I had used in my book. But when I went to the field for three months in 1990 and six months in 1991 progress was at first slow, and not only because Sino-American relations were at a low ebb and interviews often began with a ritual denunciation of the United States and the US Congress. More to the point, interviewees were continually raising issues and suggesting interpretations that could not be accommodated by the framework I had come equipped to use. In the Procrustean bed I had built, I was not cutting off a few toes and a lock of hair, but rather everything other than a sliver of my interviewee’s midsection. The project obviously had to be altered.

The primary way I reoriented myself was through open-ended, semi-structured interviews (see Wildavsky 1989: 57–102). In four cities (Beijing, Harbin, Tianjin,
and Wuhan) I spoke with 39 individuals engaged in people’s congress work or scholarship. Interviews were conducted in Chinese without a tape recorder (but often with an assistant taking notes), and were transcribed immediately on a laptop computer. I always sought individual meetings, but in some cases had to settle for the three-ring circus of ‘discussions’ (zuotanhui) with three to five individuals. I arrived with a set of questions, but as suggested above I allowed respondents a fair amount of freedom to take the discussion where they wished (on ‘guided monologues’, see Wildavsky 1989: 77–78), and my questions evolved both as my reading progressed and as interviewing opened fresh lines of inquiry. In several cases, interviewees also provided written responses to my questions, and I found that making an interview plan available in advance (something I had resisted), quite often allowed deputies, scholars and staff members to display considerable subtlety in their arguments. The only clear drawback of providing questions beforehand was that interviewees sometimes prepared a ten or twenty minute comprehensive statement that might drift a little too far afield – and I then had to decide whether to interrupt them and whether this would spoil the interview.¹

To allay fears and demonstrate sufficient ‘friendliness’ while investigating a sensitive topic at a tense time, I began all interviews by distributing a stack of documents and introduction letters from my sponsors and Chinese colleagues. I also prefaced each interview with a statement confirming that I was not a journalist, that anonymity would be maintained, and that my goal was simply to introduce China’s political system to westerners who often did not understand China very well (on the ‘teach-me approach’, see Wildavsky 1989: 69). In almost every interview, after I followed this protocol, my informants protested that there were no problems at all, that I should not be worried, and that they were glad to assist me and to do their bit for improving Sino-American relations. After more pleasantries and some gift giving they usually relaxed, and most interviews proceeded quite well.

In this research, as always, I did not attempt to code informants’ answers or treat them in any formal way. Instead, for an issue (local people’s congresses) on which few scholars had conducted empirical research, I simply tried to squeeze all the meaning out of whatever information my interviewees chose to share. Letting interviews take their own shape was particularly beneficial with informants who conceived of themselves as intellectuals or local notables. In my experience, economic, social, and political elites do not like to be led around by the nose. They find it confining and insulting, and more than once during this fieldwork I dropped my original line of questioning and let the conversation flow where my informants wanted, usually to considerable advantage.
In practice, a flexible approach to question choice, question order, and follow-up queries greatly increased the probability that my neat analytical categories and unexplored assumptions would be challenged. Invariably, it was most helpful not when my informants answered a question, but when a response demonstrated that my question was poorly put: that it had been conceptualized incorrectly or that I had posed a dilemma that did not exist or missed a dilemma that did. Examples of such wrong-headed but ultimately useful questions included asking local legislators: ‘Do you represent the interests of the state or your constituents?’ The most interesting ones typically said something along the lines of: ‘Both, and here’s how we do it.’ Or asking other individuals involved in people’s congress work: ‘How can a legislature develop without becoming more autonomous, without moving farther from the Party?’ And the thought-provoking ones said: ‘The only way to develop is to avoid conflict and to move closer to the Party. That’s how we’ll get more resources, better staff, and expanded jurisdiction.’

These responses, though unnerving at first, encouraged me to consider why I had come up with questions that so woefully failed to capture the complexity of what was happening in China. They also led me to modify my main project and to begin exploring two new topics. My original study swiftly morphed into an examination of a Chinese analogue of representation (‘remonstrating’) (O’Brien 1994a). At the same, I decided to pursue what would eventually become a comparative reassessment of the early stages of legislative development (what I called ‘embeddedness’) (O’Brien 1994b) and an examination of ties between people’s congresses at different levels, which focused on why many leaders and staff of lower congresses preferred to be subject to ‘leadership relations’ (lingdao guanxi) rather than ‘guidance’ (zhidao) or ‘contact’ (lianxi) relations (O’Brien and Luehrmann 1998).

This iterative approach to research design continued throughout the interviewing, such that by the end of my stay I was testing my arguments directly on interviewees, typically saying: ‘Some other people in people’s congress work have told me [X]. What do you think about this?’ A continuing effort to draw theory, concepts and evidence together (Gerring 200: 2) also affected how I ultimately presented my findings. Developing ‘hooks’ for three articles I wrote based on this fieldwork entailed organizing illustrative anecdotes and interpretations from my interview notes around puzzles inspired by the wrong-headed questions that got me started. The puzzles were: How can a legislator represent the interests of constituents and be an agent of the state at the same time? How can parliaments develop even when they generally avoid confrontation and are subservient to strong executives? Why are leaders of higher-level people’s congresses not empire-builders while leaders of lower congresses are willing to sacrifice their autonomy?
These were real puzzles in one sense, but not in another, in that the interviewing had already shown how to solve them. Writing then mainly involved establishing the apparent conundrum and showing how Chinese legislators and staff explained it could be made to disappear. And my typical conclusion was that the theory and concepts I had set out with (the ones that inspired my original, misconceived questions and nearly foreclosed more germane questions) had to be refined, usually because they arose from unwisely extending a western understanding of political reality to a place where it did not apply.

This is ground-level up theorizing and it is markedly different than case studies, small-N comparison, hypothesis testing, or confirmatory analysis. I did not have a theory and a crucial case. I did not have a number of cases that I subjected to systematic, side-by-side comparison. Nor did I have a set of hypotheses that my interviewing and library work sought to verify. I most certainly did not have a predisposition to find what a theory told me to look for and to pry rich data into a framework that was inappropriate. I found remonstrating, not parochial representation; embeddedness, not institutionalization; ‘looking for leadership’, not a yearning for autonomy – and, for me, that was enough.

GENERALIZING AND THEORETICAL ECLECTICISM

Keeping our theorizing close to the ground is a good way to avoid wooden, predictable research that may appear solid, but somehow misses the mark. If village elections concern state-building as much as democracy (O’Brien and Li 2000), we need to say it, even if doing so makes China less ‘legible’ (Scott 1998) to disciplinary colleagues who would prefer to hear about political participation, electoral connections, popular support, and civil society. Common analytical vocabularies, attractive as they are, can mislead as well as clarify. This point is especially relevant for Sinologists today because China is in transition – and to what, we don’t truly know (cf. Steinfeld 2002). Should the pace of change moderate and China’s sociopolitical system become more like that of other nations, more familiar theories and concepts will apply. But at least on the topics I study, that time has not arrived and uncritically accepting the ‘stale categories which now dominate our discourse’ (Schwartz 1996: 112) threatens to transform striking examples of legislative remonstrating into dull and misleading accounts of ‘parochial representation’.

This suggests that perhaps we should not be too concerned with sorting out the ‘wild profusion of new labels’ which Baum and Shevchenko (1999: 333)
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gently mock in their review essay on ‘the state of the state’. For now, I will cast my vote (provisionally) in favor of theoretical and conceptual promiscuity. Let’s let a hundred flowers bloom as multiple lines of inquiry proliferate and compete. Let’s try to explain what occurs in China more analytically than we have in the past by clustering our findings under a host of often made-to-order concepts and bringing Chinese experiences into social scientific ways of thinking more than we have up to now. Let’s take some chances and present what we come up with in a form that can be tested, if not by us, then by others. But let’s postpone a quest for broad covering laws and sweeping generalizations until searching for them makes more sense than ‘looking for a general theory of holes’ (Shapiro 2002: 601).

If we heed this advice, it is unlikely that China scholars will soon reach a consensus on a single model that characterizes, for example, the local state or state-society relations that is immediately recognizable to disciplinary colleagues. But a parade of paradigms need not be a formula for exoticizing China. Sinologists have much to offer the social sciences. What is most exciting about China, from a disciplinary perspective, is how much low-hanging fruit is waiting to be harvested – if only we keep ourselves open to new ideas and topics that emerge in the course of fieldwork. China is teeming with things that shouldn’t be (i.e. theoretical anomalies), which our interviewees are glad to serve up to us. One of our key jobs is merely to recognize and repackage these insights for disciplinary colleagues who have been working with grand theories that actually apply only in a limited context. Our simplest, most obvious findings (e.g. much contention in China is neither ‘transgressive’ nor ‘contained’, but ‘boundary-spanning’ (O’Brien 2003; O’Brien and Li 2006)) may be news to scholars weaned on theories and conceptualizations that have seldom engaged Chinese experiences firsthand. This is a time, in other words, when challenging assumptions that stand behind existing theories is a particularly flavorful and easy fruit for China scholars to pick.

Research (re)design involves locating whatever theory might be helpful (some of it beforehand and more of it afterwards) to make intelligible what we find on the ground, and then immediately using one’s fieldwork to suggest inadequacies in the theories or concepts themselves. This entails theoretical eclecticism and more reading of the comparative literature, not less. In my own work, I have successively dipped into theories of institutionalization, role accumulation, popular resistance, elections, implementation, principals and agents, social movements, citizenship, and law & society – whatever it took to help understand the significance of my interviewees’ comments, to place my findings in relation to others, and to generalize (ever so modestly). For me, the challenge of linking
high-flown theory to the most local of detail is what makes fieldwork in China so exciting and rewarding.

AUTHOR’S NOTE

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NOTES

1 When I sensed that a demand to review questions in advance was purely a screening device, I sometimes omitted the most sensitive questions and then casually reintroduced them during the interview. Most informants were too polite to decline to answer.

2 I say this as a person who has examined ‘citizenship’ in China’s countryside (O’Brien 2001) and who has just finished a book with Lianjiang Li (O’Brien and Li 2006) about how evidence from rural China can contribute to social movement theory and how social movement concepts can be applied to the study of Chinese popular resistance.

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Discovery, Research (Re)design and Theory Building


Fieldwork is based on an idea of knowledge as situated in space. To carry out fieldwork is to recognize that relevant knowledge is located in a certain place, somewhere outside the walls of your office or library. Hence, fieldwork is commonly perceived as going out there to conduct observations and coming back here to write about it. To conduct fieldwork is a process of initiations, where the fieldworker gradually crosses boundaries that separate insiders (those who are or have been there) from outsiders. This is a process that is saturated with insecurity, and fieldwork is thus about learning while coping with multiple sources of insecurity.

The main aim of this chapter is to show how this insecurity is an inherent aspect of the learning process that postgraduate students go through during fieldwork. All students doing research need to develop strategies for obtaining information and advice. The process of seeking information often positions the graduate or PhD student in relationships which are asymmetrical in nature. Usually the student is placed in an inferior position that may create a strong sense of having little control over the work that is so essential for getting the degree. Lack of proficiency in Chinese is only one aspect contributing to the asymmetry. The fieldworker’s status as an outsider is in itself a challenging social role that it takes time to become familiar with. This comes in addition to well-known challenges of acquiring and making sense of information about the topic under investigation.

For most students, doing fieldwork for the first time involves a learning process that is characterized by trial and error. They are facing an open-ended process which might take an entirely different direction than imagined. This uncertain nature of fieldwork is both an advantage and a problem. The advantage lies in the great spectrum of possibilities involved in the process, the opportunities to gain
new insight, and to develop the work in the most promising direction, as Kevin O’Brien’s chapter shows. The problem is that there are no secure outcome and no guaranteed result, as everything depends on access to the right informants, their will to answer questions and the fieldworker’s ability to interpret the acquired information.

Surprisingly little of this uncertainty is reflected in students’ narratives, however. We tend to represent fieldwork as a linear and rational process, originating in a research design, realized through data collection in the field and ending up as a thesis or dissertation. The fieldwork accounts tend to rationalize problems and emphasize achievements. The actual experiences of fieldwork often diverge from these representations. Hence, one objective of this chapter is to present a personal narrative, which exemplifies the numerous frustrations and lack of smooth progress that constituted my PhD fieldwork in China.

Through these reflections, I wish to highlight how a learning process took place even though the fieldwork procedure did not correspond to the idealized representations. During my PhD fieldwork, I found the initial steps to be especially difficult, and imagined that my stay would end up as a complete failure. Later, I realized that the learning process was continuous, even when the fieldwork was perceived to be at a standstill. It was continuous, because it also took place while coping with daily life, spending leisure time with friends, reading newspapers, and watching TV. In the end, I actually had something to communicate about my fieldwork topic, information that had become knowledge I could use, and this discovery convinced me that in spite of problems and frustrations, fieldwork was worth doing.

FIELDWORK AS COPING

My fieldwork experiences originate from working on a PhD project in Human Geography about critical journalism in China. I had spent some time in China before embarking on the fieldwork, and during my master’s degree I spent one year on a language scholarship in Beijing. I also chose to spend one term at the beginning of my PhD to study Chinese in Shanghai. This language strategy made it possible for me to conduct interviews in Chinese, but I depended on taping them and had to rely on assistance to transcribe the interviews from the tape. For social science students who have not studied Chinese as part of their degree, the language problem can be one of the major sources of insecurity during fieldwork. There are many possible language strategies; studying Chinese
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is one, relying on interpreters another, but none of them can guarantee that no information is lost, as Thøgersen’s chapter shows. However, insufficient language skills do not inhibit learning, even though it might complicate the process.

The objective of my PhD project is to gain an understanding of the political roles of a new kind of problem-oriented journalism in China. Before embarking on the fieldwork I had been working on a research design which outlines two complementary approaches: qualitative interviews and discourse analysis. The data comes in part from interviews with journalists about their work experiences, their perceptions of media control and limits to free expression, and their strategies and objectives. The intention is to focus on journalists working for the critical Guangzhou-based paper, *Nanfang Zhoumo*, or preferably journalists who have worked there in the past, since the paper is known to have become less radical. The second approach is a discourse analysis of Chinese newspaper articles about social problems aimed at identifying how they construe the context and the origin of these problems. This analysis will exemplify how critical journalism is regulated and how journalists use discursive techniques to expand their freedom of expression. The focus on representations is a central dimension in post-structuralist discourse analysis, which emphasizes that every representation of the social world is political. The combination of interviews and discourse analysis is intended to connect the critical journalistic practice with the discursive context in order to see how critical journalism can be understood as a concrete case embedded in the politics of reform in China.

Subject positions

Through the process of doing fieldwork, a student engages in many situations where there are a limited number of possible social roles, or subject positions. The subject positions are defined in relation to different groups such as supervisors and senior scholars during the planning of the project, and informants during the fieldwork. The different subject positions have in common that the student has an inferior position, and is approaching those who know and belong. This can be seen as a liminal role. The term describes the ambiguity of being in contact with people, being connected, while remaining different (see e.g. Mohammad 2001). Hence, the inferiority connected to the fieldworker’s subject position is also linked to possibilities of creating new knowledge. The liminal role is an expression of an interplay of difference and sameness. The difference is necessary because it legitimates the observation and the questions the fieldworker asks, while the construction of sameness, of some kind of common ground, is necessary because it enables communication.
One such subject position is experienced during the preparatory stage before actually entering the field. Most students who decide to work on a topic related to China will try to locate China scholars locally, nationally, and maybe also internationally. The first approach is often an e-mail where the student presents the project and asks for advice. This can be seen as the first meeting with the insider/outsider dichotomy. The student enters a subject position characterized by an asymmetrical relation with the academic insiders, who are perceived as experienced and in possession of the required knowledge about how to proceed in the research process. Moreover, seen from a student’s point of view, they seem to master the field. They have lived there, they speak and read Chinese, and they have friends among Chinese academics and other people.

The academic insiders are often researchers and teachers who usually do their best to prepare the students for the challenges that lie ahead of them. They play a crucial role as door-openers to the field, as they can provide access to their own networks and contacts. Conversely, through their advice and requirements, they also become gatekeepers to the China field. Their suggestions signal which qualifications the student should have, or obtain, to be able to do qualified work. My first meeting with the academic insiders was at the time when I was figuring out the topic of my master’s thesis and the clearest pointer was that I had to learn Chinese. This was undoubtedly good advice, but it was also part of the academic gatekeepers’ screening process, which established language skills as a marker separating insiders from outsiders. Compared to other fields within the social sciences, it seems that the gatekeepers to the China field advocate rather strict requirements. The China field is characterized by a strong focus on the necessity of proficient language skills and contextual knowledge in order to do qualified academic work. This attitude probably reflects the gatekeeper’s own experiences. They have spent years acquiring these skills and they want to encourage rigorous research standards. This means that the fear of not having the necessary qualifications to conduct successful fieldwork can be strengthened in the meeting with academic insiders. Their requirements and the quality of their work easily become the standard the student’s own initial work is measured against. The construction of the ideal standard takes place in literature as well as in personal communication. For me, the following paragraph made an especially deep impression:

Whereas Western universities, media, and government(s) never would regard anyone with a reading ability in English of ten pages an hour as qualified to speak with authority on British politics, a similar standard does not yet apply to
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China and Chinese politics. Here even those who are functionally illiterate in Chinese may become authorities. (Schoenhals 1992: 6)

This text does several things. It makes the entrance criterion to the field explicit and this immediately places most young China-students from the social sciences in an outsider position. However, the text can also be read as an effort to strengthen the status of China-research by removing its ‘exotic’ label. It claims that recognition as a China scholar should demand a similar level of knowledge as expert status in other fields. When the paragraph’s context is taken into consideration, another aspect is revealed. Schoenhals writes this as part of an explanation of why Western scholars have ignored the crucial role of language in Chinese politics. This struck me, because an interest in the political importance of language and discourse was an important reason for my choice of research topic and design. However, understanding Schoenhals’ statement literally, I should not be doing this work before I have acquired satisfactory language skills. In this sense the text can be accused of representing a very narrow conception of how valuable contributions to the social sciences can be made.

Contrary to this conception, it can be claimed that there are diverse sources of understanding. My recognition of the value of language in Chinese politics did not come from any China-specific knowledge, but rather from literature such as Laclau and Mouffe’s discourse theory (Laclau and Mouffe 2001) and Fairclough’s critical discourse analysis (Fairclough 1995). This literature shows how language is central in constructing political change, maintaining political hegemony and in creating our understanding of society. This is why I assumed that language had to be important in Chinese politics, and that changes in the media practice were political in nature, due to the media’s central role in political communication. In other words: it should be possible to make a valuable contribution to academic research without fulfilling an absolute entrance criterion like the one Schoenhals presents here. Students should keep in mind that there is always a danger that the academic insiders, the senior China scholars, have forgotten or suppressed the memory of how the beginning of their learning process was.

Communicating with experienced China scholars or more experienced students is important, not only for the development of a theoretical and methodological approach, but also for a number of practical reasons. Access to those people is not easy for those of us who come from other disciplines than Sinology. I did not apply for a research visa before leaving. I was a PhD student in Human Geography at the University of Oslo, my institution had no established contacts with Chinese academic institutions, and I simply did not know how to
obtain such a visa. After asking around, I concluded that it would be possible to go on a tourist visa. In the end, I went to China as the accompanying spouse of my husband (who was going to study Chinese). This did not cause any problems, but nor did it provide me with any advantages. Later, I realized that if I had contacted a few more China scholars, one of their contacts could probably have issued an invitation to me, and provided me with an affiliation with a Chinese academic institution.

Beginning fieldwork

Once one has arrived in China, the outsider/insider dichotomy suddenly no longer only applies to a limited group of academics. On the contrary, the insiders are the Chinese, this multitude of people who trade, shout, walk, talk, drive and just belong in the place where the fieldworker is foreign. Arriving in a foreign place means entering a new subject position, where the fieldworker’s identity is defined in relation to people who see this place as their home. To do fieldwork is a conscious way of dealing with being a foreigner. The fieldworker is acutely aware of her lack of understanding of how things work. This consciousness is rewarding in the sense that it strengthens the willingness to take notice and observe, but it can also be exhausting and frustrating.

When I arrived in Shanghai during the summer of 2002, the first few days were filled with practical arrangements and formalities, but when these things were done I was going to start. But how do you start fieldwork? The obvious way is to begin with the data collection, which in my case meant interviews. To get the first appointments, I was going to call or e-mail my contacts in China.¹ Making this initial approach was intimidating and I felt nervous and insecure. When there was no response, it seemed to me that the whole project was flawed. At this time of the summer my contacts were on holidays, on maternity leave, or about to go abroad. In short, they were not sitting there waiting for me. This should not have come as a surprise. After all, fieldwork is about going there, entering people’s daily lives in a different place. It is not surprising that it took some time before the first interview appointments were established. However, the first two weeks of my fieldwork was the most difficult part, because the lack of progress strengthened my sense of insecurity. I felt hesitant and delayed the telephone calls that I had to make, and my own lack of progress made me feel miserable. These feelings were linked to a fear of failing, of being denied access and maybe being told that what I was doing made no sense.
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The first interviews

In the research proposal I had indicated approximately how many interviews there should be, with what kind of people, and how the information should be employed. While waiting for replies from the relevant contacts, I realized that there was no reason to be so selective. I started making a list of all the people I had met and talked to at some point or another in China. If only a few of them knew somebody it would be interesting to talk to, it would contribute to my understanding of Chinese media. This showed me that there were more possible entries into the field. Likewise, the research proposal focused on the press and journalists working for newspapers. When it turned out that a friend knew a journalist working for the CCTV, China's national TV station, I realized that as long as time was abundant, I should grasp every opportunity to talk to people, regardless of my previous theoretical and methodological focus.

Contrary to my expectations, the replies I finally got from the persons I had contacted in Beijing were positive. I wanted to make set appointments and was puzzled when people kept telling me to give them a call when I arrived in the city. In the Nordic countries, appointments are often made weeks in advance. Later, I realized that appointments at short notice are a common way to organize meetings in China. This is in fact a great advantage, as it makes fieldwork more flexible.

Textbooks dealing with qualitative interviews emphasize that this method is based on common conversational skills, the goal is to find out what people know, and the interviewer should be critical and always ready to make follow-up questions (Rubin and Rubin 1995). While reading through the transcripts of my first interviews, I discovered that my questions seemed to be shaped by a search for affirmation. Rather than representing an open and critical approach, they revealed that I had wanted to be reassured that my project reflected something real, something that could be identified and talked about. The questions were sometimes leading, and often longer and a lot more confusing than the answers. The language barrier contributed to this problem. During the first interviews in Chinese I found it very hard to formulate follow-up questions, as I was concentrating so hard on understanding what I was being told. Recording the interviews proved to be very useful. They were transcribed by a Chinese student and I read them to learn their content, but also to improve my questions and interview skills.

Language problems continued to be a frustration. In some interviews I brought a Chinese friend with me. She assisted in formulating questions when I prepared for the interviews and she was a great help when I wanted to summarize and analyse the interviews afterwards. During the interview, if I struggled with
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a question, she would help me, and as she became familiar with the topic, she sometimes asked questions herself. Her help made me more relaxed during the interview. In spite of my unsatisfactory language skills, I generally felt that I was taken seriously. As a foreigner I was not expected to know every term or understand everything, and it was legitimate to ask people to explain.

Cultural ‘mistakes’ of the type that how-to-do-business-in-China manuals warn you against were also accepted. Once, when my friend and I interviewed a journalist at our hotel, I asked him if he wanted tea. I perceived his answer as negative. A few minutes later his voice sounded rusty, and suddenly my friend got up and poured him a glass of tea. I realized that he had only been polite in the Chinese way. In literature about foreigners in China, such things are sometimes presented as critical failures. Even though I experienced it as very embarrassing at the time, my general impression is that foreigners are given a lot of leeway. So in a way, I was not being impolite, I was only being foreign and a bit stupid.

Learning through interviewing

The first interviews represented very different views on critical journalism in China. One interviewee, a university professor, was very optimistic, and felt that a new culture was being constructed, where criticism was no longer primarily perceived as subversive. His impression was that critical journalism had started locally, but would evolve towards topics of more profound importance. A CCTV journalist, on the other hand, focused on the negative impact of the hierarchical structure in Chinese media. In his view, editors are afraid of losing their positions, and carefully abstain from publishing anything that might be too critical. This journalist felt that the new freedom of the media was illusory and that the new possibilities were limited to the politically irrelevant field of consumption. He exemplified this by telling me about a program he had made about the recurrent problem of mining accidents in China. The program drew a connection between mining accidents and uneven economic development, as these dangerous mines are located in the poorest provinces. The program implied that no fundamental changes would take place as long as poverty continued to force workers to carry out dangerous work. The journalist put a great deal of effort into the program, and was disappointed and surprised when it was stopped from being broadcast. He viewed this act of censorship as an example of a permanent risk-management in Chinese media institutions. Journalists’ work is evaluated in relation to the consequences it might cause, and if the editor assumes that a critical report might lead him into trouble, he will stop it. The editors face personal risk when they publish or broadcast critical journalism, and this prevents journalists from
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going too far. These completely opposite assessments presented an indication of the spectrum of different points of view regarding media development in China. Every research proposal represents a simplification of the empirical world. My research project was based on an idea about changes within the Chinese media. This journalist’s statement claiming that the changes were politically irrelevant contradicted my preconceived conceptions. This meant that I had to include new questions in my interviews and to be open towards the possibility that the changes were less important or of a different nature than I had expected. Dealing with complexity is one of the great challenges in conducting empirical research, but it also makes it inspiring and fun.

Most interviews started out with very general questions. I presented my interest in critical journalism, and asked the journalists to talk about their work experiences. My follow up questions focused on how journalists select their material, how formal and informal regulations work and what consequences their work had caused. Some interviews contributed little to my understanding of critical journalism in China, but it was sometimes difficult to see why they failed. During one interview with a journalist who had most of his work experience in mainland China but was presently working in Hong Kong, I got the feeling that he did not want to talk. He was polite, answered my questions, but kept his answers to a minimum. The interview felt like a struggle. It was not until we started talking about new trends in the education of journalists, and how new journalists get recruited, that he showed some enthusiasm. At this point he mentioned two journalists in Beijing that I could contact. He did not have their contact information, but gave me their names and employer. Although I thought it would be difficult to trace people without having their telephone number or e-mail, this actually did not represent any major problem.² In the end, these two contacts turned out to be very valuable. Although the interview with the Hong Kong based journalist did not lead to many new insights, it gave me a chance to practice my interview skills. I became more aware of the importance of pursuing unexpected openings during an interview. The moment I finally found a topic he took an interest in led to the information about his contacts in Beijing.

Even after the first interviews, I still felt nervous when calling people for the first time, but it got easier with time. Towards the end of the fieldwork period, I started noticing how the interviews left me less exhausted and more satisfied. Throughout, my ambition was to let the journalists talk, and to fit in my questions where they connected to what was being told. This was very difficult in the beginning, but became easier towards the end. I was no longer asking those long and muddled questions. This had something to do with learning new concepts, becoming
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familiar with the interview situation, and learning to know the discursive field of critical journalism in China. My questions changed and with them the answers. Arranging the interviews seemed easier too. I, who in the beginning could hesitate for two days before making a phone call to a possible informant, would now make calls every day, without having to mobilize energy and courage to do it. My perception of my role as a foreigner and fieldworker was changing; in other words, my subject position was not as limited and inferior as in the beginning. As I became more familiar with my role, I discovered that my status as a foreigner could also be useful. The journalists often gave me an introduction to modern media history in China, and by telling this history they presented the context which framed their own work experiences.

I found that my role as a foreigner made it easier to admit ignorance, and it seemed that my foreignness also encouraged the insiders to tell their stories in a detailed way. As a foreigner I could ask questions about the self-evident without causing too much annoyance, and in this sense foreignness contributed in a positive way. Sameness, on the other hand, implies a common frame of understanding which means that the communication is perceived as meaningful. In constructing sameness, the fieldworker uses part of her own identity to connect with the person she is talking to. Sameness can be constructed around nearly anything. In interviews with critical journalists I often tried to construct a feeling of sameness around political views, as questions concerning poverty, uneven development and injustice were areas where a feeling of sameness could be expected. Sometimes I used gender issues to connect. In a discussion with a female editor about coverage of domestic violence in the news, I was using feminist discourse to build sameness between us. I do not perceive the construction of sameness as false play acting, but sometimes I may have stretched my identity fragments more than I was comfortable with.

INVISIBLE LEARNING AND TACIT KNOWLEDGE

During fieldwork, the learning process is often hidden, which means it is difficult to identify progress. During my time in China, the sense of failure never completely left me. I felt that I should have been more active and have created more opportunities. A sense of failure makes it difficult to describe the fieldwork experience in an open way. A common fear among fieldworkers is that admitting difficulties will undermine the entire value of the work. However, the rationalization of fieldwork experiences means that the gradual, or tacit, learning involved in the process often
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remains invisible. The reason for this might be that the tacit learning does not happen in clearly demarcated stages, it simply seemed to be there by the time of departure. The aim of the last part of this chapter is to reflect on the different ways learning takes place during fieldwork, and to argue that a move away from writing rationalized, linear representations of fieldwork will enable students to understand how the learning process depends on their failures as well as on their successes.

Outside the interview situation

Much of my learning took place when I was not interviewing or collecting data. Most of my time was actually spent on things other than doing interviews, especially in the beginning, and I often found I was wasting time. It was a relief when the Chinese university semester started and a few friends came back to the city. I started studying newspaper texts together with a Chinese student, and was spending time discussing relevant topics and searching for texts together with a friend of mine who studied at Fudan University. This helped me improve my language skills and get some perspectives on how newspaper texts could be understood. I was also working with a research assistant who transcribed my interviews. I could then read the transcriptions in Wenlin, a software program with a Chinese-English dictionary. If there were sentences or expressions that I found difficult to understand, she could explain. She also assisted in collecting text material, and we spent several days together at Shanghai Library, looking for relevant articles. I found it inspiring and useful to work together with these Chinese students and believe they contributed a lot to my learning process.

Failures as a source for learning

While I was in China obstacles to progress felt like failures. My location was a major reason why the fieldwork did not develop gradually, but during short, intensive periods. Shanghai was clearly not the best place to study critical journalism in China. This was surprising to me, because the advice I was given before starting on the project pointed to Shanghai as a good location. My efforts to identify informants among journalists in Shanghai only resulted in very few names. Late in the fieldwork I found out that Shanghai’s propaganda department was more restrictive than its counterparts in Beijing and Guangzhou, which led to a relative lack of innovative journalism. The result was that most of my interviews with journalists took place in other places. The location problem had a negative influence on the number of interviews I made, and it was a constant source of frustration. However, this problem became part of a crucial understanding of
geographical differences and their influence on the possible scope and content of criticism in Chinese newspapers. In fact, I learnt something important from choosing a non-ideal location.

Another obstacle that turned out to have a positive side was the 16th Party Congress in November 2002. At first it seemed to delay my work, because journalists became more cautious. In October, when I e-mailed a contact in Guangzhou and told him that I was planning to come, his response was clear: ‘Nobody here wants to talk to foreigners now’. The reason was increased surveillance because of the Party Congress. I mentioned this in an interview with two Xinhua editors in Beijing, and one of them expressed his surprise and declared that he did not experience any such special restrictions. These differences in the perceptions of control and freedom were important information to me. They made me more aware of the great variation between media institutions, and between different journalists. The different perceptions of control and vulnerability were highlighted when a journalist explained to me the probable reason why his colleague never responded to my e-mail or telephone messages. This person had been central in the development of Nanfang Zhoumo, and he had lost his job after giving an interview to a French journalist. Even though I did not succeed in interviewing this journalist, it nevertheless taught me something about the risk some of my informants faced in their daily work.

Tacit learning

When all different sources of learning are taken into consideration, it becomes obvious that the learning process is going on throughout the whole fieldwork. In my case, I learnt about China while I was establishing a daily life in Shanghai. I was learning through conversations with students and friends (both Chinese and foreign), and by having to face obstacles, in addition to the learning process connected to the interviews. During almost every interview something was added to my understanding: the non-verbal information, the way things are told, body language, laughter, and omissions, which can also be a source of increased understanding. Through doing interviews in Chinese, my vocabulary increased, which probably also contributed to a sense of progress towards the end of my stay.

Being in China means being involved in a multitude of interactions. Living in a neighborhood, eating in restaurants, taking trains and taxis, shopping, dealing with formalities, spending time with informants, Chinese students, friends, and studying language – these experiences can all be understood as connecting points between the fieldworker and the local context. In addition, there are other
textual and visual connections, such as reading newspapers and magazines (both English-language publications and Chinese), watching television and Chinese movies, listening to music or radio, observing people, street decorations and festivities. Media representations are a rich source for understanding the framing of different problems, which connotations they carry, and how they are valued in Chinese public discourse. Through everyday interactions the fieldworker can learn about local understandings of the world, about local problems, and about what is considered fun or repulsive, important or trivial. Some of this knowledge is tacit, which makes it difficult to describe. Nevertheless, tacit knowledge is easy to recognize among those who share it, and because of this it plays a crucial role in the data collection, that is, in the learning of codified knowledge.

This shared knowledge is important in developing an interpretative framework for understanding one’s research topic. The knowledge about how certain topics are represented will be reflected in the interview questions. Through communicating an understanding of a phenomenon, the fieldworker expresses a common starting point, which facilitates the later exchange of viewpoints and information. In my case, the preparations for the 16th Party Congress, with its flower decorations, front page portraits of the CCP leadership, and red banners became something we could joke about during the interview, demonstrating that we had a shared understanding of the spectacle connected to Chinese politics.

Writing

Writing is probably the best way to become aware of the new knowledge that develops during fieldwork. Keeping a fieldwork diary ensures that the early experiences, which after some time seem prosaic and commonplace, do not disappear. This is important in being able to reflect on the learning process. In the final part of the fieldwork period, the knowledge gained during the first few weeks will often seem banal and self-evident. But the learning that took place during the initial stage was important in the process, and it will probably be important information for the future readers of the thesis too.

It is difficult to write about failures. In any representation of fieldwork, there are days that are omitted just because nothing happened. Days wasted watching American DVDs, reading spy novels or in other profitless ways. I cannot claim that these days contributed much to my understanding of the critical press in China. Still, they were there, in between the days when things were happening, as days where the feeling of insecurity and failure crushed the ability to cope with that insecurity and move on. Those days are difficult, as much because they are so very much out of place in fieldwork discourse, which emphasizes the active
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approach taken by the fieldworker. The empty days made me doubt the entire project, or at least question my ability to get something done that later could contribute to academic discourse.

There are many normative statements within fieldwork discourse. In addition to constructing a particular image of the fieldworker, the different social relations within fieldwork are graded. Friendships with key informants are very highly ranked, while interactions in the tourist field are regarded as low value. The ranking reflects the perceived degree of difficulty in establishing the relation. The lack of established high status relations might become another source of a feeling of failure. Alternatively, instead of estimating the value of the connections against a preconceived value scale, it is much more useful to use the writing process to reflect on how these connections contributed to the research, and to the understanding of the field.

The writing process following fieldwork will be familiar to most students, even though it has its own challenges. Seen from the depressed viewpoint of the wasted days, coming home and dropping plastic bags filled with materials down on the desk can be quite a relief. Suddenly, it seems like you have actually done something, that time has been well spent. At home, you are cut off from the context from which your materials originate, where people are familiar with the world that you want to write about, and suddenly you relate only to a fragment of all the things that are going on in China.

As the writing process begins, many master and PhD students ask themselves what they can possibly write on the basis of their material. It seems very usual to doubt the quality of one’s data. This partly reflects the insecurity connected to the fieldwork process, but some of the distrust of the material is probably caused by the remnants of positivism that most of us still carry with us. As the social sciences have moved towards a post-positivist paradigm, ideas about the objective researcher have been criticized and undermined, but positivist thinking still seems to influence the experiences of fieldwork. Even in qualitative work, which does not aim to generalize the empirical findings, 30 interviews still sound better than 12. It does not seem to matter that neither 12 nor 30 interviewees make a representative selection. If the qualitative method is taken seriously, the number of interviews in itself is uninteresting. What is important is the kind of interviews, observations and interactions, and what the fieldworker gets out of them. This is not intended as an argument against rigorous fieldwork methods, but as an argument for utilizing and recognizing the richness of the learning process involved in fieldwork. Based on the multiple sources of learning, it is possible to communicate a story which will be a meaningful addition to academic discourse.
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CONCLUSION

Fieldwork is an open-ended process without clear procedures. Young researchers, MA and PhD students, are expected to produce valuable academic research, but doing fieldwork for the first or second time often leads to a feeling of not mastering the situation. During the planning process and the initial part of fieldwork, the student is an outsider in the field who approaches people with expert knowledge. In addition, representations of fieldwork often establish an idealized version of the process, where all the trying and failing remains hidden. The representations create a stark contrast to the experience of fieldwork as an untidy and frustrating process. All of this contributes to insecurity.

Coping with insecurity means finding strategies that make the project move forward or identifying alternative approaches to the topic in cases where the process is at a standstill. In spite of problems and frustrations, most fieldworkers find that towards the end of their time in China they are able to explain to others the case they have been working on, and they are able to situate this case in a context. This knowledge is in part a result of the information gathered through interviews or other forms of data collection, but it is also due to the tacit knowledge obtained during the process of fieldwork. This is the learning that grows out of the interaction with Chinese people in everyday life. Acquiring tacit knowledge is a precondition for understanding how things work, because it creates a common interpretative repertoire, which enables communication between outsiders and insiders. Towards the end of fieldwork, the initial subject position characterized by insecurity and inferiority will usually have changed.

The fieldwork process will always be a challenge for new students who enter the field. However, new representations of fieldwork, which include the problems as well as the multifarious ways of learning can contribute to less insecurity. Senior scholars can share their own initial experiences, and in this way students will know that the frustrations they face are part of every fieldwork process. Facing problems and obstacles is an inherent aspect of the learning process, and cannot be avoided, but making them part of our common fieldwork discourse will make them signs of a learning process instead of indications of failure.
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NOTES

1 To connect to the Internet from your computer is easy in China.

2 There is a telephone information service in every province. You can call 114 to ask for telephone numbers and addresses, or ask somebody else to make the call for you.

3 There are now signs that the Shanghai media will catch up with the other cities. The propaganda department has a new leader, and a new newspaper has been established, which has recruited several established journalists known for their critical and investigative work (Wiest and Cheung 2003).

REFERENCES


Fieldwork can be used in many different ways and be part of many different research designs. O’Brien’s chapter emphasizes how important it is to stay open to unforeseen ideas that we come across in fieldwork and to integrate what we discover into our research design. This chapter focuses on the role the selection of field sites plays in the fieldwork process, and its relation to the case study method and to research design.

In social science methodology, a prevailing view held even within qualitative studies is ‘the larger the number of cases the better’. In the small versus large N debate, breadth is associated with a large number of cases (large N) and depth with a small number of cases (small N). Using the case study method, it is common to study a small number of cases looking for variations between them according to the method of difference or method of agreement. This paper does not follow these conventional lines of thinking but tries to point to other available strategies. While these methodological rules are of course not wrong, with regard to field sites the dichotomies breadth-large N and depth-small N conceal yet another available research design: an in-depth study of one case but conducted in a number of different field sites. In this way of thinking, one field site does not necessarily constitute one case. Instead of studying variations across a number of cases and controlling for variables, this research design, to conduct fieldwork in a number of different sites, focuses on similarities across field sites and falls somewhere between the above-mentioned research designs. In China studies, this particular research design seems to be commonly applied although not explicitly discussed. I argue here in favor of a non-quasi-experimental, exploratory research design with a large potential to generate new findings.
Field Sites, Research Design and Type of Findings

I will discuss different research designs with regard to field sites and their respective advantages and drawbacks. As a starting point, I will use my own PhD project, before proceeding to three examples from the literature. I will briefly mention some of the problems I experienced in the field and then discuss coping strategies I employed to overcome the difficulties. The coping strategies I adopted may be a common response to constraints in fieldwork. Practical circumstances always influence one’s actual research design, yet adaptations made in the field are often overlooked or downplayed when a project is written up as if the design was fixed from day one. This prevents us from not only discussing difficulties encountered in fieldwork but also from learning about available coping strategies and their methodological advantages. Which coping strategies can facilitate fieldwork in rural China? Which different research designs are available for selecting field sites? And, maybe most importantly, how does a research design affect the type of findings we are able to generate?

WORKING OUT A COPING STRATEGY

The principal point of my PhD dissertation (in political science) was to explain why many local Communist cadres had turned into entrepreneurs in China, and to examine the political institutions that had produced this phenomenon. A secondary aim was to describe how rural cadres concretely assisted enterprises in their area and promoted local economic development. My topic was inspired by an article by Jean Oi (1992) on local state corporatism as well as a range of works on the East Asian developmental states. Oi’s important work on local governments functioning as business corporations led me to inquire about the political incentives faced by local leaders. My research design, from the outset, was open-ended and exploratory in nature, as I had not formulated a hypothesis, neither did I have an idea at the time what the explanation could be. In my research proposal, I had merely rehearsed a number of theories which might apply to the Chinese case but which could not fully explain the puzzle. Oi’s (1992) focus on fiscal incentives provided a starting point but I felt it could only form part of the answer or, at least, that it raised new questions in and of itself. In the East Asian developmental states debate, the dominant explanation had always been external to the countries themselves, and Robert Wade (1993) had named the ‘why question’ as the jackpot to be pursued. So while my topic was very open-ended, the project was driven by a research question that was both empirically and theoretically relevant. In the initial framing of the project, I put discovery before...
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verification (see Kevin O’Brien’s chapter). For the ‘how question’ I built upon Oi’s work and the work on the East Asian developmental states.

My first trip to a rural area was to southern Jiangsu province (Sunan), well known for its collective industry and local government involvement. Before that I had studied for six months at the Beijing Languages Institute to improve my Chinese. As research on China was practically non-existent at my home university (Uppsala), I came to China in 1996 without official, or unofficial, connections. I was later able to take part in an exchange program between Stockholm University and the Chinese Academy of Social Sciences (CASS), which provided the organizational framework of my fieldwork. My first year in China was primarily spent on finding out how things worked and getting to know people who did research on similar issues. CASS arranged through its official channels for a township government in Sunan to receive me, but then I was on my own.² Things did not go very well at the outset. In the field, I realized I hadn’t paid enough attention to how other scholars had collected their empirical materials, preoccupied as I had been with their substantive arguments. In 1996, the Sunan model was also going through difficulties and heavy local government involvement in the economy was under review at higher levels, so many local cadres were not overly forthcoming during interviews. My level of Chinese was not satisfactory at this stage, and the student I had engaged in Beijing with my meager funding turned out to speak very little English. Later I was told by other researchers that local cadres in Sunan were often more reluctant to provide information than in some other places, but even though that may have been true the main reason for my slow progress was probably my inexperience and lack of connections.³

To overcome difficulties in collecting information, I worked out two coping strategies which I think are of general relevance. The first was to move between administrative levels and interview cadres and enterprise managers at the county, township, and village levels. Although my main focus was on township leaders cum entrepreneurs, the county was important because of its leadership relations with the township, and the village and its enterprises were also crucial players in the implementation of industrial policies. To move between three levels accorded with my original design, but my aims at first were mainly to cross-check information and gain insight into the level at which my fieldwork was conducted. For example, when township officials claimed they had assisted one of their leading enterprises in obtaining a bank loan, I visited the enterprise in question (and also often the bank) to double-check if this was right. Also, I interviewed county officials to learn about the county’s goals and policies. I
soon found out, however, that one level could also provide information about another level which the latter might not volunteer. Hence, I could feed what I learned at one level into my questions at the second level and thereby get a fuller picture. Concretely, I used a bottom-up approach when learning, for example, from leading entrepreneurs how local leaders assisted local enterprises and a top-down approach when learning from the county how they exercised pressures on township leaders. Vice versa, I also learned from the township leaders how entrepreneurs were integrated into the party-state and which county directives they ignored. When using this strategy, of course, each piece of information cannot be taken as fact but always needs to be confirmed or disputed by officials at the level under study.

Gaining insight about one level from another level, in combination with extensive cross-checking, produced an even clearer picture of my object of study. In my opinion, this is a under-utilized method and one that I recommend. The main drawback is that it is fairly time-consuming. All in all, I carried out 153 interviews with local officials and entrepreneurs at these three administrative levels. Some researchers may use this method but without going back and cross-checking with the level under analysis, and this will place them on shaky ground. For example, it is common that data about the township government are presented which originate from complaints made by villagers that have not been cross-checked. Given that researchers rarely state whether the information is based on interviews or who is being interviewed (an official, at the county or at the township level, or a villager), it is impossible for the reader to evaluate the source. In my view, it is just as necessary to cross-check information from villagers about local government as the other way around. There is a tendency in the field to romanticize ordinary villagers and to take everything they say at face value, especially if they are critical of officialdom (‘if they dare to be critical it must be true’). Needless to say, everything officials say needs to be scrutinized as well, but that is if not common practice at least something we all agree is crucial.

The second strategy I adopted to cope with difficulties in collecting information was to increase the number of field sites. In the original research proposal, I had planned to study two townships in depth to explore the mechanisms that induced local leaders to promote economic development. To reduce dependence on the cooperation of local officials in any particular site I decided to increase the number of field sites. Elisabeth Croll (1987: 21) has recommended – I read later – choosing two or three sites for fieldwork in order to reduce dependency and spread risks. For my project, this was a successful coping strategy because local informants turned out to be more forthcoming in Shandong and Zhejiang.
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The same advantages you accrue when moving vertically between levels are also present when moving horizontally between field sites. I used what I knew about one place when conducting fieldwork in another, asking questions like: ‘In that place they had these targets that were important, how is it here?’ It was also possible to cross-check information on the same topic across locations, and to see whether my understanding from one place carried over to another. I ended up conducting fieldwork in 12 different counties (all, except one, in three provinces in developed coastal areas). The drawback with a larger number of field sites in a qualitative study is again that it is time-consuming as a lot of energy is spent on arrangements and the establishing of contacts in each. This disadvantage has to be weighed, however, against the potential payoffs bearing in mind that it can also be very frustrating and time-consuming to be stuck in one field site without making progress.

Anthropologists have strongly influenced our perception of fieldwork methods, and it is customary for them to stay for an extended period in one place. My final research design resembled the conventional anthropological approach but with an important modification: I went to several places to study one phenomenon in depth. My research design thus relied on the case study method but not on studying variation between a small number of cases. Instead, all my field sites constituted one case (of successful local governments promoting development in China) with regard to the why question. With this design, I was mainly looking for similarities and common denominators across places, and wanted to compare data between places to gain a better understanding of one phenomenon. This design is different from, say, going to four field sites and treating them as four different cases of one phenomenon, comparing them with each other and looking for variations between the four cases. Concretely, this research design meant that I was not primarily interested in the differences in the cadre evaluation system across provinces (for example, the various ways points were being calculated, or which particular party-state agency coordinated the evaluation). Instead, I wanted to focus on aspects of the cadre responsibility system that were common in many places and which could cast light on township cadre behavior. With regard to how local cadres assisted their rural enterprises, in contrast, I divided the field sites into two different cases (one case of collective industry development and one case of private industry development). The local state used different methods in assisting rural enterprises across the two different cases, but both cases were explained by the same factor.

Two townships, which I visited more than once and where I stayed longer formed the basis of my study, while I only paid shorter visits to the others. After
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having discovered my main finding (on the cadre responsibility system), I made enquiries about the details of this system (whether for example there were any priority targets with veto power) in my respective sites. In some of the townships where I stayed for shorter periods, though, I sometimes concentrated on one dimension of the cadre responsibility system that I had less information about. As a consequence, every aspect of the system was not given equal attention in the different field sites, which would have been necessary had I been conducting a structured comparison or studying variation between cases. Andrew Walder is one of many scholars who conducted émigré interviewing in Hong Kong before extensive fieldwork on the mainland was possible. His émigrés were all from different places in China but he constructed one case on the basis of his interviews just as I construct one case from multi-sited fieldwork. Walder (1986: 257) called this the method of triangulation at a higher level of generality. My reasoning with regard to field sites can also be applied to interviews; different aspects of the question can be obtained from different interviewees. In semi-structured or open-ended interviewing, the same questions are not asked to all interviewees for the simple reason that interviewees are positioned to provide different types of information. The fieldworker commonly discovers an unexpected dimension in the middle of fieldwork that he or she wishes had been included in the questions to the first round of interviewees.

Conducting studies in 2 counties might suggest that I opted for breadth over depth. However, that was not the case. On the contrary, I studied one phenomenon in depth in a number of different field sites in pursuit of a general mechanism. I found the mechanism to be that township leaders promoted growth not only because it was rewarded by higher levels but also because it helped them cope with uncertainty and gain control over their work situation in the developed coastal areas (see Edin 2005). Carrying out fieldwork in a large number of field sites also increased the likelihood that my findings were applicable across places. Methodological textbooks commonly recommend the case study method, and a small number of cases, to identify mechanisms (see, for example, Hedström and Swedberg 1998; Gerring 2001), but this recommendation can be misleading since it gives the impression that a small number of cases and depth of study are correlated with a small number of field sites. I argue that conducting fieldwork in a larger number of field sites, say a dozen or less, can be an optimal strategy for identifying mechanisms.

To sum up the points that emerged from my fieldwork: difficulties in collecting information can be dealt with (1) by moving between different administrative levels and (2) by moving between areas. For my project, these two coping strategies
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turned out to be central to producing my findings. During my fieldwork, local cadres were generally not forthcoming with information on the cadre responsibility system and it was only when I asked about very specific events that I extracted small pieces of information here and there. Naturally, I didn’t ask them directly why they had assisted their local enterprises but reconstructed their behavior from what I had learned about their work situation and the nature of local political institutions. In southern Jiangsu, I was told very early – on my first trip when things were not going very well – about the evaluation system of local leaders and the bonuses it could bring (see also Whiting 1995 and Ho 1994), but that was only one part of the responsibility system and did not explain much cadre behavior. It was, thus, neither the voices of local cadres that structured my findings nor the voices of enterprise leaders. Nor, in fact, was it any single source – it was my own construction, tile by tile, of a mosaic pieced together by the interpretation of multiple sources. To do so, it was first invaluable to triangulate the information I gained by moving between counties, townships and enterprises. Secondly, I would probably not have been able to identify the important mechanisms without increasing the number of field sites. My project also illustrates the importance of putting discovery before verification. I strongly second the fieldwork approach outlined in O’Brien’s chapter in which the most interesting result is a new set of questions. It is my conviction that I would never have discovered the cadre responsibility system (see Edin 2000 and 2003 for a presentation of the results; see also O’Brien and Li 1999) if the project had been built on a hypothesis to be confirmed or rejected.

When I returned from my first stay in the Chinese countryside, determined to do better on the following trips, I consulted other studies on contemporary China but found that actual research methods were rarely discussed in any detail. The fieldworker who runs into problems wants to know how other colleagues have handled similar situations, and what different methods they have used to analyse similar topics, so I shall now broaden the discussion from my own individual project to the wider field.

FIELD SITES AND OVERALL RESEARCH DESIGN

I will here examine a small number of projects and what I perceive to be their research design, especially with regard to field sites, and then discuss the advantages and drawbacks of each approach. I have selected a few authors who have adopted different research designs to study rural problems, such as the status of the Chinese
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countryside in the reform era, peasant burdens, and popular contention in rural areas. I have to some extent had to reconstruct their fieldwork methodology and research design. The principal reason for selecting these specific scholars is that they have applied three different approaches to study related issues. They are also, in my opinion, very worthy representatives of their respective approaches. To minimize the discussion of their substantive findings, I have selected authors who draw broadly similar conclusions, enabling us to concentrate on the relation between fieldwork methodology and the type of conclusions drawn.

The three approaches that are discussed are: the one-field-site approach to be represented mainly by Gao Mobo’s *Gao Village*, published in 1999; the all-of-China-field-site approach to be represented mainly by Thomas Bernstein and Xiaobo Lü’s project on *Taxation without Representation in Contemporary Rural China* from 2003; and the one-case multi-field-site approach to be represented mainly by Kevin O’Brien and Lianjiang Li’s research on ‘rightful resistance’ and popular contention published either by one of them or jointly in a number of articles in the 1990s.

To begin with the one-field-site approach, Gao Mobo writes about Gao village where he grew up but which he left at age 21 to study at university in Xiamen. After moving to Xiamen and later to England and on to Australia, he regularly returns to Gao village to visit his relatives, collect material and to stay updated with the village’s development. Gao thus conducts his fieldwork in one field site, and he also makes use of the detailed knowledge he has of village affairs and takes the cases of himself and his own family members still living in the village as examples. His book is an intimate and very persuasive account of village life in central China which serves to illustrate macro developments in the countryside in both the pre-reform and reform eras.

By telling the story of one community, Gao offers a different picture of China’s development since 1949 than that constructed by the current leadership and the urban elite. He emphasizes that the understanding of developments in China in the outside world has largely been shaped by the experience of established elites, such as party officials and urban intellectuals who themselves were targets of movements like the Cultural Revolution and who have benefited greatly from the post-1978 reforms. Gao village and many other rural areas, in contrast, prospered under Mao Zedong’s radical policies, especially in the areas of education and health, and have lost out in the reform era. Although it is often claimed that schools were closed during the Cultural Revolution, this was not the case in many rural areas. During this period, a primary school was set up for the first time in Gao village, attendance rates rose, and secondary school teachers were transferred from urban
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to rural areas. Today, however, many secondary schools have been closed and the primary school in Gao village has been privatized, leading to increased school fees. In the realm of health care, two barefoot doctors were stationed in Gao village and paid for by the production brigade, and the government launched a campaign against schistosomiasis. After Mao’s death, the health clinic was closed and all the collective buildings were dismantled with building materials divided among the villagers.

Gao concludes that post-1978 reforms have largely amounted to a restoration of the pre-1949 situation with the local state apparatus done away with in a single stroke. Although Gao does not attempt to generalize about all of rural China on the basis of a single village, he does seek to show that the circumstances in this one village differ widely from the conventional understanding of post-1949 development in China. His method is to upset established truths, and he uses one case to great effect to show that conventional thinking is wrong. The type of empirical material he developed, which would be difficult to acquire with another approach, includes detailed descriptions of how clan politics are woven into the economic and political structure of the village in both the pre-reform and reform eras. Another example of the payoffs of his fine-grained approach is his remarkably detailed account of his brother's family’s annual income from farming and their tax payments in 1992 compared with 1972. His book is clearly the product of a scholar from mainland China who conducted his study in his native village. Still, a similar design has been used by, for example, Xin Liu (2000) who did an ethnographic study of one village in northern Shaanxi that was not his native village. And there has also been a small number of Western scholars who have carried out fieldwork in one field site for a considerable period of time, despite the difficulties of arranging such a project for a non-native scholar. The difficulties in arranging long-term fieldwork in China might be one reason why the one-field-site approach is not more commonly applied.

Bernstein and Lü adopted an opposite approach, which I have termed the all-of-China-field-site approach, in studying peasant burdens. The authors base their work, which deals with the peasant burden and peasant resistance, on newspaper articles, government reports and Chinese research publications. Both authors have made numerous research visits to China over the years and have conducted interviews with local officials, farmers and Chinese researchers that probably provide the basis for much of what is presented in the book. But they have not directly relied upon these interviews in their study, as far as I can tell. There are very few references to interviews in the book. Secondary material on the situation in rural China thus constitutes the bulk of the empirical
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evidence. Bernstein and Lü provide an excellent overview of the question of peasant burdens, first placing it in a historical perspective, then identifying the origins of and popular responses to irregular taxation, as well as discussing the central government’s effort to contain burdens and various avenues to strengthen farmer representation. Their study tries to make sense of diverging views of the predatory and the developmental state in rural China, and focuses on those parts of the countryside, largely in central and western China, that were less successful in rural industrialization and that have received less attention in the literature on rural political economy. A distinction is made between industrializing rural China, middle income agricultural China, and low income western China, and it is in the middle income agricultural areas of central China where, according to the authors, the peasant burden is most keenly felt – and where the impetus for democratization is likely to be the strongest.

Although the corruption of local government is highlighted, Lü and Bernstein also see that the problem of peasant burden is rooted in the administrative system as a whole. The uniformity of the system is ill suited for western and central China, and to the authors the Chinese system is reminiscent of the administrative set-up that produced the Great Leap Forward mentality. At the same time, the central government has sometimes sided with the peasantry and provided them with various venues to protest and seek legal or administrative redress for their complaints. They conclude that the capacity of the state is low, at least with regard to containing peasant burdens, especially in middle income agricultural China.

The advantage of their approach is that their impressive range of written materials enables them to provide a broad picture of an important issue. With a different approach, we would not have had so much information on popular protests in China as a whole, or come to understand that it is in the agricultural areas in central China that the peasant burden is most strongly felt. For the reader, however, it is difficult to evaluate the validity of empirical material ranging from press reports to unpublished documents and research reports. It is also difficult for the reader to place the material geographically, and to know which part of the country any particular source concerns. It does not matter as long as rural China is treated as one case, but Bernstein and Lü sometimes make a distinction between the three rural Chinas by treating them as three cases.

Then this research design poses some problems. The large body of material is, in other words, suitable for macro-analysis but it is less appropriate for conducting the micro-analysis that will help us to understand the mechanisms that produce and alleviate arbitrary taxes and fees. For example, the readers are left to themselves
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to make sense of the contradictory facts that the central government has often sided with the peasants but also done very little to reduce peasant burdens. Relying on secondary material, though, may be the only method available for sensitive topics such as corruption. To use press reports and documentary sources to construct a broader picture is a quite common approach in the China field, especially when analysing elite politics.

In between the one-field-site approach and the all-of-China-field-site approach is what I call the one-case multi-field-site approach, and here I place O’Brien and Li’s pioneering work. In the first stage of their ongoing project on popular resistance, they conducted fieldwork in twenty-two villages in three provinces between 1992 and 1995. In addition to interviews, they also, like Bernstein and Lü, had great help from local government circulars, field reports by Chinese researchers and unpublished contributions to journals and newspapers. Although O’Brien and Li have carried out interviews in twenty-two field sites, they gathered material on one phenomenon, popular or rightful resistance, and thereby treated their many field sites as one case. Applying this approach, O’Brien and Li did not study variation in protesters’ behavior across field sites but gave priority to deepening their understanding of the phenomenon of rightful resistance. The authors were the first to spot a new form of popular resistance in rural China where protesters cite laws and government policies in their actions against local officials and their unjust implementation of central policies.

Li and O’Brien developed a typology of villagers: compliant villagers (shunmin), recalcitrants (dingzihu) and policy-based resisters (diaomin), of which the latter two are different categories of rural protesters. While dingzihu come close to lawless rebels who violate laws and policies in their resistance to the state and have little concern for public welfare, diaomin on the other hand use laws and policies to challenge cadre misconduct in defense of their lawful rights and interests. Diaomin employ their knowledge of government directives to protest, for example, against peasant burdens by questioning the legality of local cadres who seek to collect more than the centrally prescribed 5 per cent. From the perspective of state officials, diaomin are much more difficult to deal with since they take the values and programs of the political elites to heart while demonstrating that some authorities do not. O’Brien’s earlier work on local people’s congress deputies likewise shows that remonstrator-style appeals can be found both within and outside the state.

This new form of popular contention has sometimes proved to be effective, and herein lies the possibility that growing rights consciousness in the Chinese countryside may contribute to significant political changes. The authors argue
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against a neat division between marginalized popular classes on one side and representatives of the state on the other, and show that popular resisters can sometimes locate powerful allies (including journalists) within the elite. In a later article by O’Brien, the phenomenon is dubbed rightful resistance and he compares it with other forms of popular resistance such as everyday resistance, in-between forms of resistance, reformist activism and reasonable radicalism found in other countries. In so doing, he extends the same logic of the one-case multi-field-site approach, looking for similarities rather than variation across places, now applying it to phenomena outside China. To see how well the concept travels, he looks into other cases to suggest partial or complete episodes of rightful resistance outside China. O’Brien writes that his aim is to highlight a common dynamic and to locate parallels that help break out the relevant part of the narrative. With the one-case multi-field-site approach, authors can gain a deeper knowledge of one phenomenon by probing for similarities, while downplaying variations across place (or across regime types). Having said that, such comparisons can be helpful for identifying the essential characteristics of a phenomenon under study. Although it is not usually made explicit, many authors seem to employ the one-case multi-field-site approach. Jean Oi and her work on local state corporatism, for instance, appears to be another example.9

Comparing the three research designs and the type of findings they generate, we can discuss the advantages and drawbacks of each approach. Different approaches to the issue of generalization seem to have influenced the research designs and here the number of field sites and cases are central. Also within the context of qualitative studies, there is a sentiment that the larger the number of cases, the more possible it is to generalize or to make causal inference. This thinking is misguided, however, as qualitative studies rarely reach the large number of N needed for statistical generalization. Bengtsson (1999), who is concerned with the quasi-experimental design built into the comparative case study method, argues that it is not possible to claim a causal relation between independent and dependent variables on the basis of, say, six cases. Gerring (2001 and 2004) is likewise skeptical of the comparative case study method, including the possibility of finding variations on the dimension of theoretical interest and holding other factors constant across cases. According to these critics, a quasi-experimental design with control cases only gives a false impression of scientific design. In the following pages I am going to argue in favor of a strategic use of fieldwork with the ultimate goal of generating interesting and important findings. A quasi-experimental design risks neither achieving a scientific design or generating interesting results. In my opinion, it is more fruitful to undertake in-depth studies of a single phenomenon
or a mechanism. As an alternative to a quasi-experimental design, Bengtsson (1999) proposes a case study that can cast light upon mechanisms. The criteria for selecting cases would then not be variation in the independent variable (King, Keohane and Verba 1994: 137) but to go where there are reasons to believe something interesting can be found (in deviant cases for example). 20

Although Bernstein and Lü do not base their study on comparisons between different cases controlling for certain variables, they have to rely on secondary material as a consequence of their ambition to say something about rural China as a whole. Bernstein and Lü (2003: xvi) give a high priority to constructing a generally applicable picture of conditions in agricultural rural China, even going so far as to shy away from intensive local research. One advantage of their approach is that they can concentrate on the analysis of peasant burdens, but their analysis is naturally dependent on the quality of secondary sources, and it is difficult to make independent new findings. For projects depending on newspaper reports, it is difficult to go beyond the official discourse, however rich and varied it has become these days. The all-of-China-field-site approach is best for discussing issues that are already recognized as problems in China but less appropriate for discovering problems that are not publicly discussed. There is a risk of getting stuck in the official interpretations of Chinese social reality. The central authorities view peasant burdens as a huge problem, they want to portray themselves as siding with the peasantry (and have publicized regulations to that effect) and it is not against the interest of the center when local governments are portrayed as corrupt. The national media assist the central government in this regard by bringing up serious problems that the government wishes to solve, but it is also careful not to directly blame the central government or dig deep into the structural causes of the problems. 21 Still, Bernstein and Lü’s study on peasant burdens and rural taxation in the reform era is the most comprehensive to date, and will serve as a reference work for future scholars. There is always a balance between keeping a study open for unexpected empirical findings and putting an end to the explorative phase. Going through a large body of secondary material, as in Bernstein and Lü’s case, the explorative phase may be shorter than in the one-case multi-field-site approach but longer than in the one-field-site approach.

Gao’s explorative phase was short as he used his prior knowledge of Gao village to carry out a project which challenges our conventional understanding of post-1949 developments in China. He uses what is often called a deviant case study (although he argued that Gao village was representative of many rural areas in China) to formulate hypotheses and to point to the need for further studies. He argues that rural areas, in contrast to the cities, prospered under Mao’s radical policies but have
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lost out in the reform era. Gao’s work is an illustrative example of how a one-field-site approach can be used to great effect to influence the field. His in-depth study of Gao village not only makes a contribution through its main findings but his detailed description of village life can also cast light on other on-going debates. Even though recent analysis (Khan and Riskin 2001; Wang and Hu 1999) of the origins of poverty in China indicate that poverty has increased since 1985 due to the unequal development strategy, Chinese official statistics and World Bank figures show on the contrary that the number of poor people has been significantly reduced since 1978. This discrepancy can be better understood by evidence from Gao village: While gross income has increased rapidly since the 1970s, the expenditures for production costs have also increased along with taxes and levies paid to local governments. So for Gao’s brother’s family, the net income from agriculture in 1992 was the same as in 1972. On top of that, the expenses for education and health must also be deducted from the net income. These services were basically free in the 1970s but a major household expense in the 1990s. Although we do not know if the situation is the same for the majority of households depending on agriculture in different areas of rural China, this single piece of information from Gao village suggests an important factor.

O’Brien and Li’s approach differs from Gao’s and also from Bernstein and Lü’s in that it is more exploratory and open-ended in its research design. It is thus no coincidence, I argue, that their project generated new and interesting findings that influenced the whole discourse on social protests and government responses, and have large theoretical implications. O’Brien and Li made the discovery of a new form of popular contention – rightful resistance – on the road in China, they tell me, as a result of hearing local cadres continually harping on about dingzihu. According to them, before their fieldwork started they had never heard the terms dingzihu or diaomin and they certainly had no idea that the distinction between them would become the focus of their research. By undertaking an in-depth study of one phenomenon, in different field sites, they were also able to compare rightful resistance to other forms of popular contention, both inside and outside China. Increasing the number of cases (within qualitative studies) does not increase the ability to make statistical-like generalization, but increasing the number of field sites can increase the probability that a phenomenon may also be found in other locations. More importantly, conducting fieldwork in a larger number of sites makes it easier to capture the phenomenon in all its various dimensions – and to discover it in the first place. It also brings into view mechanisms that stand behind the phenomenon. Had O’Brien and Li confined their study to a small number of villages they would not have been able
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to describe diaomin and their strategies in all their complexity simply because there were not many diaomin in the villages they went to or because the diaomin only displayed certain characteristics in those villages. Comparisons help us see which characteristics belong to a phenomenon and which do not.

THE CONNECTION BETWEEN RESEARCH DESIGN AND TYPE OF FINDINGS

The ultimate reason why we should be interested in coping strategies and research design is that they affect our findings and our understanding of China. How do different types of fieldwork approaches and research designs, particularly with regard to field sites, generate different types of findings?

An all-of-China-field-site approach is useful when we already know a fair amount about a phenomenon and want to get a feeling for how widespread a phenomenon is, or to get an overview of the state of the field on a given issue. Due to its dependence on secondary sources, an all-of-China-field-site approach is however less likely to generate findings that upset our existing knowledge. I have argued here in favor of a strategic use of fieldwork that is more likely to generate new and unexpected findings. It is often more fruitful to design an explorative study and to study one phenomenon in depth rather than setting up a quasi-experimental design for explanatory purposes. A one-field-site approach can be used to great effect to challenge a well-established view in the field, insomuch as deviant cases can be used for theory development. When you study one case in depth with an established truth firmly in mind, however, the fieldworker needs to have a good idea what he or she is going to find at the outset, skipping the exploratory stage. If not used in this strategic sense, the one-field-site approach can of course also generate new empirical findings with a more open-ended research design, but it is more dependent on length of time and fortunate circumstances. A one-case multi-field-site approach is appropriate for studying a phenomenon in depth, to uncover general mechanisms and to generate new empirical findings. It is very suitable for research designs with unabashedly exploratory ends. To have a larger number of field sites makes it easier to discover a phenomenon and to describe its various dimensions. In this way, the one-case multi-field-site approach is not as dependent on fortunate circumstances as the one-field-site approach since there are more opportunities to make discoveries and the fieldworker can employ comparisons to distinguish various dimensions of a phenomenon from others.
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In this chapter, I have only been able to look at a few studies and in order to say anything about the prevalence of different approaches a review of a larger number of studies would be necessary. My impression, though, is that the in-depth one-field-site approach is not so common in the China field as in the studies of other countries, owing to the difficulties in organizing long-term fieldwork for non-native scholars in China. The quasi-experimental research design is also not very common in the China field, maybe for similar reasons. In fact, among those who carry out intense local research I believe that the one-case multi-field-site approach is very common, even among anthropologists. In China studies, it might be a usefully pragmatic approach to adopt in response to the sorts of problems I encountered in my own PhD project. Elisabeth Croll (2004: 93) has described how short-term field investigation became a localized strategy that developed in response to the absence of opportunity for ethnographic observation in the pre-reform era. She raises the question whether this alternative field method is outdated today when longer periods of fieldwork are possible. My answer is that it is certainly not outdated. The one-case multi-field-site approach definitely holds some promise if our aim is to make new discoveries and to identify mechanisms, not least because it takes the coping strategies we all must develop and turns them into a methodological advantage.

AUTHOR’S NOTE

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NOTES


2 The foreign affairs office of CASS sent down my documents to the Jiangsu Academy of Social Sciences (JASS), who made the concrete arrangements. I had expressed a wish to go to a specific county and then the township was selected by JASS on the basis that it had a hotel where foreigners were allowed to stay.

3 Fortunately, graduate students are forced to learn quickly, and the situation invariably improves. Even though I felt disaster looming during my first trip, I collected valuable material already back then which contributed to the overall picture and gave more precision.
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to my questions in later interviews. For later trips I established informal contacts, my Chinese improved, and I learned how to phrase questions to get meaningful answers (see Solinger’s and O’Brien’s chapters).

4 My fieldwork was carried out during a number of trips to rural China. Altogether I spent seven months at the county level and below (and 22 months in China as a whole) for the PhD project.

5 This research design slightly resembles John Stuart Mill’s Method of Agreement in that it looks for similarities across cases, but this design does not aim to make comparisons in the same structured fashion as in the Method of Agreement (for a description see, for example, Ragin 1987: 36-39).

6 They were viewed as two ideal types on the opposite end of a continuum, and all field sites fell under one of these two ideal types.

7 Even though I didn’t reformulate my question in this specific project, the eventual findings did not frame the question as a puzzle any longer (and maybe it never was a puzzle).

8 These three approaches are of course not the only ones in the China field. The approach of studying a small number of cases by focusing on variation across cases in order to make causal inference, for example, has not been discussed here.

9 I have mainly looked at their book of 200 but I have also used their article published in 2000 and, to some extent, Lü’s article on the peasant burden from 1997.

10 I have mainly looked at O’Brien 1996; Li and O’Brien 1996; and O’Brien 1994.

11 Gao discusses his methodology on pp. 255-56 and also in the preface pp. xi–xiii.

12 The largest village in the township dominates politics, the government seat and the secondary school are situated in the largest village and township leaders come from that village. As a consequence, smaller villages, like Gao village, are discriminated against and are not allowed to be in debt to the township, whereas the largest village has not paid its debt.

13 Gao’s brother and sister-in-law each earned 0.8 yuan per day in 1992 which is more or less the same amount as the best village labourer could earn back in 1972. From the gross income of 1,500 yuan in 1992, 728 yuan were paid for production costs, 480 yuan were paid in taxes and levies, leaving a net annual income of 292 yuan for the family.

14 Xin Liu writes from the perspective of ordinary villagers rather than cadres and uses his three hosts as main informants. He did not focus on events and hardcore figures but was mainly interested in the macrosocial transformation in a mode of everyday behaviour and performance, and in identifying a general pattern of change across Chinese society.

15 See, for example, Litzinger 2000.

16 The media publications mentioned are Cheng Ming, Kaifang, Ming Pao in Hong Kong, which claim access to confidential Chinese sources. Mainland publications such as Minzhu yu Fazhi and Nanfang Zhoumo occasionally carry detailed accounts of individual and collective protests. The authors discuss methodology and sources in Bernstein and Lu 2003 p. xvi and 118.

17 See, for example, Lü 2000.
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18 Their fieldwork is described in Li and O’Brien, 1996, p. 28 with an interview list in appendix, p. 56 and also in O’Brien, 1996, footnote 13.

19 Oi describes her fieldwork in detail in a methodology appendix. From her discussion, I say it fits the one-case multi-field-site approach perfectly. Oi’s book Rural China Takes Off of 1999 is based on 335 interviews conducted in ten different provinces between 1986 and 1996. She did extensive fieldwork in two counties in Shandong province in which she carried out 198 of her total interviews, others were spread more or less evenly across the other nine provinces. Her interview sites were not chosen randomly so few statistical generalisations can be made based on her findings. Her intention was instead to get a purposive sample to gain as much detailed information as possible about how and why rural industrialization occurred.

20 The principal concern for Bengtsson is how to make it possible to generalize on the basis of very few N. He argues that social mechanisms can be general and in his reasoning these social mechanisms are of rational character (without belonging to hard-core rational choice theory).

21 Zhao Yuezhi and Wusan Sun, ‘Yulan Jiandu as technologies of governance: Role of media in constraining local officials’, a paper presented at the conference on ‘Grassroots Political Reform in Contemporary China’, organized by the Fairbank Center, Harvard University, 29-31 October 2004.

22 Official estimates indicate that rural poverty declined from roughly 260 million poor in 1978 to 42 million in 1998. According to the World Bank (2001), using the 1 dollar per day poverty line, there were 106 million rural poor in 1998 which is seen as a remarkable decline. There is a general agreement among scholars that poverty alleviation policies were most effective up to 1985.

23 When scholars make comparisons across a few cases, they seem largely to do it for descriptive purposes as I did when describing the different ways local governments can promote rural enterprises (see also Whiting 2001 who I believe does the same thing).

REFERENCES


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Section II

OFFICIAL CHINA AND BEYOND
Chapter 5

IN THE FOOTSTEPS OF THE COMMUNIST PARTY: DILEMMAS AND STRATEGIES

Mette Halskov Hansen

The Chinese government and the Chinese Communist Party (CCP) have long established histories of carrying out fieldwork-like investigations down to the lowest level of society, the household. This constitutes a special challenge for the anthropological fieldworker – a challenge which has, however, rarely been recognized or discussed in academic texts. Based on examples from my own fieldwork experiences, this essay discusses some of the implications this established fieldwork tradition has for other researchers (Chinese or foreign) inevitably walking in the footsteps of the Communist Party.

Ethnographic fieldworkers in China are faced with a number of challenges which may not be specific for China only, but which certainly are distinctively different from fieldwork in many other parts of the world. Practical circumstances such as the political restrictions on research topics, limited access to data, closed areas, and control of researchers’ movements, have forced anthropological fieldworkers to develop other ways of studying social life and culture than the ‘traditional’ fieldwork method which Malinowski so tellingly called ‘participant observation’. Indeed one of the great impediments for many anthropologists doing fieldwork in China has been the fact that the celebrated methodology of participant observation and long term settlement in, for instance, a private household in a rural community has mostly been impossible to carry into effect.¹ At the same time, due to China’s long and impressive history of producing
influential texts in the fields of philosophy, politics, literature, history and for administrative purposes, a large body of textual material is often available to the field researcher. Representatives at all levels of the administrative structure are used to receiving investigators, and through the right connections they will also often provide foreign researchers with relevant, often locally produced, reports and statistics. Useful data produced by the local administration can sometimes relatively easily and within a short period of time be obtained by working through the established political and administrative hierarchy. While such data is often essential, and may save the fieldworker a lot of unnecessary work, there is also a considerable risk that short term fieldwork produces a remarkable amount of data which is not necessarily reliable and should be treated more as an example of official or semi-official discourse than as a reflection of life in a given community. Due to the often close relationship between local bureaucracy and social investigators the very process of collecting these data — approaching officials, finding out what can (or cannot) be obtained, and why — often constitutes in itself a part of the fieldwork which should be recognised and read into the analysis and discussion of the material.

However, we also need to recognize that behind many of these Chinese (published or unpublished) reports or statistics about social practices, private economy, land use, number of children, education, etc. lie weeks or months of surveying, interviewing and investigating households and individuals. Since scholars, local administrators, and representatives of government and Party have, for a long time, been engaged in social investigations into various aspects of people’s social lives, many Chinese households have long become accustomed to visitors doing ‘kaocha’ (investigations or research). People in rural areas especially, may even feel obliged to receive visitors from the outside whom they consider — rightly or wrongly — to represent official powerful institutions. Over the years, many Chinese peasants have discovered that investigations into their household have had direct economic consequences for them: Loss of illegally cultivated land, children sent to school, birth control, granting of loans, to mention some common examples. Justified or not, due to the state’s long established tradition of intruding into the lowest levels of society, the fieldworker (Chinese or foreign) is vested with a special authority and power, and placed in a recognizable role as a researcher or investigator. She is walking in the footsteps of the Communist Party, as I shall discuss below.
In the Footsteps of the Communist Party: Dilemmas and Strategies

OFFICIALLY APPROVED FIELDWORK

With minor exceptions all of my own fieldwork since 1991 has been carried out with official permission from Chinese authorities and organised through my contacts with either an academic or a government institution. My first fieldwork (after having been a student in China for two years) was as a Master student. During five months in 1991 I interviewed members of the Naxi minority at the Minority Institute in Kunming, and Naxi intellectuals in Lijiang. My second longer fieldwork experience was as a PhD student. During a period of 10 months in 1994-95 I studied minority education in two locations of Yunnan Province. Due to the nature of the fieldwork this project in particular would have been impossible to carry out without official permission. During my fieldwork I followed classes in minority schools, interviewed students, teachers and officials working in the local education departments of Lijiang and Sipsong Panna, and talked to parents and other family members of students and children outside the school system (see Hansen 1999). I was able to use my research permission to freely contact schools, to follow classes and to talk formally and informally to students and teachers attached to schools and to cadres in various administrative positions. The official permission also allowed me to obtain large numbers of unpublished official documents and reports about schooling and education which would otherwise have been out of my reach.

My latest fieldwork-based project focused on the experiences and influence of Han immigrants taking part in the Chinese state’s colonization project in the Western minority areas (Hansen 2005). Here I combined the methodologies of interviewing Han immigrants, taking part in local activities involving Han immigrants, and analysing various types of texts relevant to the topic of Chinese migration to minority areas (local reports, local histories, recorded life histories, novels, and academic studies). This fieldwork made up a total of seven and a half months in the period between 1997 and 1999, but it was divided between two different locations (in one of which I had previously spent more than six months) and split into several shorter trips. Chinese migration to minority areas was a politically sensitive topic, not least since one of the field sites was a Tibetan area. I nevertheless deliberately chose to work having obtained an official research permission rather than attempt to travel as a tourist while in reality doing research. Why? An official research permission opens doors, something which is especially important for a non-Chinese researcher. Sporadically interviewing Han immigrants in minority areas open to foreigners would certainly have been possible without it. However, staying in one of the smaller towns in these areas
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for several months at a time, systematically interviewing Han immigrants on the local market, conducting interviews with people sent to the border region as official representatives of the Party and government, or getting access to local Party archives containing numerous relevant historical and other reports would not have been possible. Nor would the rubber producing state-farms, which in Sipsong Panna virtually function as enclosed Han communities within the minority area, have agreed to receive me. Unofficial fieldwork in China may be necessary and convenient in some cases. However, in my experience it can also be more time consuming, can prevent you from staying long term in one area (outside the large cities), and can block access to what may be valuable written material. Finally, the researcher easily becomes too vulnerable to situations which might force her to interrupt fieldwork altogether.

So much for the benefits, but what are the drawbacks of obtaining official permission to do fieldwork in, in this case, rural China? When organizing my fieldwork through legal official channels in China, I made use of an already existing hierarchical infrastructure. I therefore went through certain stages in my preparation of fieldwork. These will be known to most ethnographic fieldworkers in China, and they often have serious consequences for the whole fieldwork situation, the choice of field sites, the people one meets, the organizations which receive one, and the data one gains access to. Therefore, the very process of organizing ethnographic fieldwork within China can be understood as an integrated part of the whole fieldwork endeavor. The process of organizing fieldwork through the existing administrative infrastructure is often more or less predictable: One first presents a project description to an administrative unit (for instance a university). Through its office for foreign affairs this institution serves as a mediator to the provincial government’s office for foreign affairs which then issues the research permission. Normally the unit one works through then helps one going to the actual field site and, if necessary for some reason, finds an official ‘research assistant’ (peitong). At the local field site this assistant, and often also one representative of the university, then helps to establish the first contact with the most relevant local institution. This local institution is often crucial because it is through this contact one gets access to do systematic fieldwork in villages and is introduced to other local authorities.

Even in cases where one is allowed to carry out field research without a peitong the researcher is often (at least in the initial stage of fieldwork) more or less dependent on someone who is already used to representing the village officially, someone who is used to the role of being an intermediary. In a village study this role is often taken by the village head (cunzhang) who is the
‘middleman’ between villagers and higher levels of government. Although the concrete steps for the fieldworker starting up research in a new rural area may differ from the above description, the point I want to make remains the same: being an anthropological fieldworker in China is not entirely different from being a ‘veranda anthropologist’ in a traditional colonial setting. The derogatory term ‘veranda anthropologist’ has been used to refer to the pre-Malinowskian anthropologist who did not merely stay at home to read books (the armchair anthropologist), but who sat on comfortable verandas in, for instance, colonial India, and organised interviews by making full use of the colonial infrastructure.² Peter Metcalf, in an honest description of his fieldwork in Borneo, has shown how his own way of gaining access to an indigenous community was in fact not so different from the way earlier anthropologists during the colonial period used a superimposed administrative infrastructure (Metcalf 2002: p. 65). In China, fieldworkers working through official research permissions and operating, at least initially, through the official administrative hierarchy, are in a situation comparable to this because we depend on initial official contacts to gain access to the field and to informants.

In China I have often found that only my official letter of invitation could convince cadres in important state institutions that they should receive me, talk to me and provide me with data. At the same time, this practice sometimes turned out to be much less useful – even counter-productive – when trying to organize interviews with ‘ordinary’ employees in the same institutions. In what I regard as one of my most embarrassing (and therefore also eye-opening) interviews, the unequal level of status and power between the people involved in the interview created a completely impossible situation which forced me to stop fieldwork altogether in that particular work unit. It also prompted me to combine, to a greater extent, official and unofficial fieldwork.

The interview I refer to took place in a rubber-producing state farm in Sipsong Panna (on the borders with Burma and Laos). Sipsong Panna was historically a Tai kingdom inhabited by various ethnic groups. The Han only arrived in large numbers after the establishment of rubber farms from the mid-1950s. Ever since, these state farms have been dominated by the immigrant Han coming from mainly two counties in Hunan Province, Liling and Qidong, and their descendants. Proportionally a larger number of people from Liling have gained positions as cadres in state-farms and they are often very explicit in their stereotypical depictions of immigrant Han from neighboring Qidong. The Qidong immigrants were regarded as being less educated than those from Liling and therefore more prone to find jobs of lower status. During one of my
fieldwork trips I had planned to carry out a series of interviews in one particular rural branch of a state farm. However, compared to some other state farms where I had already done fieldwork, the Party secretary in this one felt particularly uncomfortable at having a foreign researcher walking around, talking to workers and cadres. Nevertheless he wanted (and probably felt obliged) to help me and accepted that I did have official permission to carry out this research. At the same time he was afraid of getting into trouble himself. He therefore insisted upon organizing interviews with workers for me.

The Party secretary first summoned a middle-aged worker who was brought into the room where my local assistant, the Party secretary and myself were present. We were seated at a table on ordinary chairs, while the worker was seated by himself on a couch, with the result that he had to look upwards to talk to us, thereby facing a row of three people questioning him. He was then briefly introduced by the Party secretary not merely as a ‘worker’ but – with a telling look – as a ‘worker from Qidong’. The Party secretary had at that time already entertained us about what he saw as the essential differences between people from Liling, including himself, and the less educated (suzhi bijiao di) workers from Qidong in his and other state farms. The worker was then politely instructed by the Party secretary to answer all our questions as honestly as he could. Feeling both embarrassed and trapped in a situation we did not know how to get out of without offending both the cadre and the worker, my assistant and I tried to make the situation a bit more relaxed by chatting and not really starting the interview. However, the whole situation was so awkward that any of our desperate attempts to improve the atmosphere quickly proved hopeless.

The worker clearly, and understandably, felt extremely uncomfortable and so did we. When approaching the worker the Party secretary changed his voice, his vocabulary and body language in ways that emphasised the unmistakable difference in status and position. He spoke to him as to a child, was very patronizing, and did not hesitate to explain to us that workers in state-farms are ‘simple and plain’ (hen pusu). This term was often used also by cadres to explain to me the nature of workers in state-farms. As I understood it, this was a way of expressing that these workers were regarded as children, or as adults who were naive and non-pretentious, simple-minded and unsophisticated. We did feel obliged to ask a few questions, and the worker politely answered in as short sentences as possible while always looking at the Party secretary as if searching for clues that he was providing the correct answers. As soon as we could, we finished the interview and politely but firmly refused more organised interviews with workers. The result was that no further fieldwork was possible in this state farm.
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When leading cadres accept an official research permission as an entrance ticket into their institution, they often also feel responsible for the outcome of interviews. In my experience, this is not always simply because they want to control the content of interviews. Often leading cadres have a genuine wish to provide what they expect the researcher wants. The Communist Party’s and government’s long term practice of carrying out fieldwork-like investigations through local official institutions has made many local cadres used to receive ‘investigators’ and provide them with the information they presume the investigators want. This situation often forces the researcher to combine officially accepted fieldwork with a more unofficial approach. The problem is that this sometimes makes it impossible to carry out meaningful interviews and conversations with, for instance, people of highly differentiated status within the same work unit or government institution. As a fieldwork experience, the one described above stood out for me as an example of how powerless and helpless one can feel while at the same time realizing that as a researcher making use of established hierarchical power structures one is unavoidably placed in a powerful position within this structure. While the ‘interview’ was completely useless for providing insights into the views of a worker in a state-farm, it was a clear demonstration of how important differences of class and status are among people who are often categorised as one homogenous group of powerful Han immigrants in minority areas. It was, of course, also a good example of how a foreign researcher and a local research assistant, due to their status as such, are often forced to comply with specific roles in the fieldwork situation – the role of privileged persons closely connected to official power. Thereby, I also came to the same conclusion as thousands of fieldworkers before me, namely that time works in one’s favor. Short term fieldwork and organized interviews have often been the only possible way for me to collect data (largely due to other working obligations and my responsibility as a mother of three children). However, the best – and sometimes the only – way to combine official and unofficial fieldwork is to be able to stay for a long time or to return often to the same places, and not to have to rely completely on the official invitation to do fieldwork, or on the data provided by local and other institutions and cadres.

WHO ARE YOU?

Working with an official invitation in China prompts the question of who you actually are and represent in the encounter with people in the field. Historically, China has not practiced or even promoted an ideal of separation between scholars
and bureaucrats, and in the Communist period countless fieldwork based studies and investigations have had as their clear purpose to facilitate policy making. In the encounter with ordinary citizens without any official positions, even the foreign researcher is therefore easily placed in a category of people or institutions who are supposed to simply have the right to be received and have their questions answered. This situation can be very difficult to handle for an ethnographic fieldworker who ideally wants people to talk freely and by no means wishes to be associated with government policies. Due to a number of coincidences, it was first of all my experience with structured interviews in villages in rural Gansu which made me realize the extent to which previous government investigations could affect my fieldwork. As discussed below, peasants questioned my identity and associated me with government policies, and I was therefore also forced to reconsider my own role as a researcher being connected to a local very powerful official institution.

In the spring of 1998 I set out to study Han immigrants and their descendants in Gannan Tibetan Autonomous Prefecture in Gansu Province. At this time nearly all my previous fieldwork had been in minority areas in Yunnan Province. I was for the first time going to do fieldwork in a Tibetan area and was aware that my topic of research was more politically sensitive in a Tibetan area than in the area of Sipsong Panna. Due to the large scale immigrations of Han and Hui to Lhasa, I would have liked to carry out research there, but realising how difficult that would be to organise, I opted instead for the Tibetan Gannan Prefecture in Gansu Province. It turned out to be surprisingly easy to obtain a research permit there and, as often in China, this was first of all due to coincidental connections between people I made contact with. While my initial contact was with a researcher at Lanzhou University, this person used his connections with a former colleague, employed in a leading position in the Committee for Family Planning (jihua shengyu weiyuanhui) at the provincial level, to obtain permission for my research. Consequently, my unit at the county level also became the Committee for Family Planning. I lived in their office building and the unit became my gateway to numerous formal and informal contacts in the county. This turned out to be at the same time a blessing and a curse.

While it is maybe unusual for a researcher studying migration to be connected to what is in effect a local birth control office, the coincidental factors that determined this outcome are very common. The distinctions between academics and officials – and academic institutions and administrative units – are often blurred in China. Even when one attempts to work mainly through an academic unit, personal and professional connections between people in this and other non-academic institutions may turn out to be decisive for the researcher’s contacts.
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and fieldwork opportunities. In my own experience, this often lies beyond the fieldworker’s own decisions and sometimes even beyond her knowledge. In the case of my first fieldwork in Gansu, it took me at least two weeks before realising that my initial academic contacts at the university had found it more convenient to completely shift my unit affiliation to the Committee for Family Planning. In their view, this administrative unit had several advantages: It was powerful and had strong local connections. It could therefore ensure that my initial wishes for rural fieldwork in specific sites were fulfilled. I am still not sure whether or not my initial academic contacts had foreseen that they would lose all control over – and income from – my study by handing over the sole responsibility to a strong administrative unit, but that was in any case what happened. After some weeks I became aware of the fact that all the necessary arrangements in connection with my fieldwork – the obligatory assistant, a place to stay, transportation, and local permission to do fieldwork – had been organized by either the provincial or county level Committee for Family Planning.

The main tasks of the local committee at the county level was to carry out practical and propaganda work related to controlling the number of births in the county, and to keep records and statistics on local population growth and success in controlling the number of births. In the yearly ‘competition’ between all counties in the province about how well rules on birth control were followed this county had come out very badly. The committee was therefore under great pressure from the provincial level. At the same time, this was problematic because the county was inhabited by many Tibetans and Hui in addition to Han. The nomad Tibetans and the Hui were especially difficult to control, according to the committee. The topic of birth control among ethnic minorities was clearly sensitive as was my topic of research. Therefore, when I arrived in this county I was called in to two interviews with the local police (gonganju). I gave an account of the research I wanted to do, and after some negotiations between the police and my assistant from the provincial Committee for Family Planning, it was decided that I could get special permission to take residence in the office building of the local Committee for Family Planning. This was where much of the daily work of the Committee took place: interviews with women, control of pregnancies, preparation of abortions, planning of sterilization, etc.

My research was not related to birth control and that was an important reason why I was allowed to stay in the office building of this unit. The local police were conscious of the fact that the issue of birth control in China was sensitive in relations with foreign governments and NGOs. Moreover, this was a Tibetan autonomous prefecture, and the police and local government wanted
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to avoid problems which could potentially arise from the sensitive cocktail of my research on Han migration, the practice of birth control in the county, and the Tibetan cultural and historical significance in it. Therefore, before being allowed to take up residence in the birth control building and before giving me the final and necessary local approval of my research, I was asked to agree never to use the phrase ‘human rights’ (renquan) in publications reflecting my fieldwork and stay in this particular county. Moreover, I had to agree never to publish any information on birth control practices obtained through my stay in the office building. These were the most concrete and explicit compromises on the content of research and later publication of it that I have ever had to make with local authorities before starting fieldwork.

On my initial field trip I stayed for three months in the building of the Committee for Family Planning. First I was there alone with my assistant from the provincial capital, and after some time my husband and children came. When I was out doing interviews they normally stayed among the people working in the Committee, and it did not take long before we were all friends with the staff and took part in their various internal celebrations, dinners and parties. Although I regarded my research as disconnected from the work of the Committee for Family Planning, others for obvious reasons associated me with this unit: I spent a lot of time there, I lived door to door with the clinic and offices of the Committee, and as I had become friends with members of the staff I was frequently seen with them in restaurants and other public spaces. Even more important in my daily fieldwork routines was the fact that my two assistants in different ways were connected to the work of the Committee for Family Planning. In order to illustrate some of the consequences of doing fieldwork in the footsteps of countless other researchers or investigators connected to the Party and government, I therefore need to discuss some of the implications of working with an assistant in China.

WHO ARE YOUR ASSISTANTS?

In my work on Han immigrants in Tibetan Gannan I depended on an assistant when carrying out formal interviews. It was compulsory for me to be assisted by someone appointed by my unit, and for several reasons I also needed help from an assistant. Most of my interviewees were Han but especially the older ones spoke dialects which were difficult for me to understand, and when I interviewed Tibetans I often needed somebody to translate for me between Tibetan and Chinese. Furthermore, I was not allowed to actually stay overnight in villages
and due to my limited time for fieldwork I was therefore depending on somebody to introduce me into villages and allow me to start up surveys and interviews within a relatively short time. In general I have also always preferred to work together with an assistant who can help clarify questions and solve language problems, who sometimes joins in interesting discussions with interviewees, and who may contribute to the research in many other ways.

Much has been written about the ethnographer’s relation to local key informants, people whose social relationships and interpretations of local life have often been of paramount importance to the anthropologist’s own work. The Chinese ‘assistant’ (peitong), if recruited locally, is often both such a key informant and an officially assigned observer of the research carried out. As pointed out by Metcalf there are two sides to the English word for informant: ‘… the unsettling confusion between our good-natured informant, cheerfully helping science forward, and the sinister figure of the informer’ (Metcalf 2002: p. 43). The informant can provide the ethnographer with important insights into the local community but she can also ‘act by betraying the weak to the strong’ (ibid: p. 43). In the case of an officially appointed Chinese local assistant there is a third dimension to this word, namely that the ‘informant’ also can (sometimes must) inform on the researcher herself. I have had several assistants who had to register all the people we officially interviewed and bring this back to the provincial authorities together with brief summaries of the interviews and a report of my whereabouts and the questions I asked. Therefore, working with officially appointed local assistants in China I have experienced the great advantages of having immediate access to a local ‘informant’ and contact, while at the same time having to struggle with the potentially serious implications of working with somebody who could inform on the interviewees (and on me, although with much less serious consequences).

In the concrete example of my fieldwork connected to the Committee on Family Planning in Gansu this double role of the assistant became very explicit. When I first started fieldwork in Gannan I was accompanied by an assistant from the Committee in Lanzhou. She was told to organize everything for me in the initial stage and then help me find another local assistant to continue her work. She was not local, but she represented an important administrative unit at the provincial level. Right from the start of our fieldwork it became clear that officials down to the lowest level of administration (the xingzhengcun) were used to receive researchers or investigators connected to government offices. They were especially used to those working with family control, education and tax collection. During my fieldwork, I realized that many people in this area had
not followed the rule which allowed all peasants, including the Han, to have two children, and the proportion of children attending the state schools had also been too low.\(^3\) It was not uncommon that people had opened up land for agriculture which was not part of the legal contracting of land, and there were some well known conflicts over access to grassland for animals. Due to these and other potential conflicts, village heads and local government and Party representatives were quite used to receive higher level officials and to organize household investigations. They were also used to being criticized if too many people in the village did not follow the rules. Consequently, peasants were accustomed to being contacted and asked questions concerning issues of family, education, property, livestock, and land use. And sometimes such local investigations were known to result in new policies or enforcement of regulations.

It was therefore not difficult to imagine why some households received us with a certain skepticism and others clearly only wanted the minimum of contact with us which they probably presumed was obligatory considering our ‘connection’ with a powerful government institution. My first assistant was not locally known, but we of course always explained to all potential interviewees who we were, why we had come, and what we were planning to do (and not do) with the data we eventually collected. Knowing how unpopular the birth control unit was we sometimes tried to avoid the question of which unit my assistant belonged to, but in many cases we were, understandably, specifically asked this question. So right from the beginning I was facing the problem that quite a few peasants assumed that the data I was collecting were intended for use by the Committee for Family Planning. The fact that I also resided in the building of this unit was known to many, and it strengthened the assumption that I must be closely associated with the work and investigations of this office.

This situation improved in some ways and deteriorated in others when my first assistant left and a local young Tibetan man took over her job. He had the great advantage of being born, raised and educated within the local area. He knew a lot of people, he could introduce me into villages where he had relatives, he was bilingual in Chinese and Tibetan, and he was used to local dialects. He was in many ways the ideal ethnographic informant. But he also happened to be employed in the local Committee for Family Planning, beyond doubt the most unpopular local administrative unit. When I started up my research, the local Committee for Family Planning had, as mentioned above, been in trouble due to its poor record of birth control. It had therefore been forced to introduce new measures based on local investigations. I had initially chosen a few villages where I wanted to carry out long term interviews and surveys, but with a locally
known assistant employed in the Committee for Family Planning it turned out to be extremely difficult to carry out meaningful interviews. People did receive us but it took some time before I realised just how reluctant they were to talk to us. We were, for instance, sometimes given very strange answers concerning the composition of households, marriages, schooling of children, and the family relations of children running about in the various houses. Clearly, my previous assistant from the provincial unit and I had both had the advantage of being outsiders and therefore less threatening in spite of our dubious connection to an unpopular government institution. Compared to this, my new local Tibetan assistant seemed to be regarded almost as a personification of the local institution of family planning at the lowest level where the dirty work was carried out.

The problem found a reasonable solution when I decided to stop interviews in the planned field sites and instead let my assistant suggest a village where he himself believed we could carry out my research. He chose his native village where he personally knew everybody. This also meant that he already had all information about family sizes and connections, property, education, etc., and people therefore believed that these issues were not of major importance to me. There was a notable difference in how we were received in this village compared to others, and eventually it therefore also became possible for me to carry out interviews without the assistant present. Consequently, I could combine the officially recognized part of fieldwork (interviews and surveys mainly) and the unofficial part which consisted of informal conversations, observations and participations in daily and special activities. Having an official status as a researcher and being connected to a local administrative unit had the advantage – and disadvantage – of giving me a position in the local setting. Once it was accepted that my research interests were different from the ‘normal’ investigators’ connected to the Committee’s work, and once I gave up the idea of collecting data on family size, family composition and family planning outside the one village where my assistant came from, my connection to the local unit by and large became an advantage rather than a hindrance for opening doors and gaining access to various kinds of data.

CONCLUDING REMARKS: DOING FIELDWORK ON THE BEATEN TRACK

As a culturally and economically heterogeneous and complex society China offers endless possibilities for fieldwork. As long as the political system in China restricts researchers as it does today, I will continue to carry out fieldwork that combines the official approach and research permission with unofficial ‘part-
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time participant observation’. Obviously some topics of research are so sensitive that it would be useless to even try to obtain official approval, but for many topics of research – including many politically sensitive ones – an official research permission is useful and even necessary. Most fieldwork based projects need also to include studies of various types of text material, often produced locally and not necessarily available for the public. In order to collect such material and study the context in which it is produced, an official permission and very good personal contacts are often necessary.

Fieldwork can, and should, be prepared, but it remains partly a matter of ‘flying by the seat of one’s pants’. What has proved possible for one researcher in one area of China may be out of the question for another (or the same) researcher in another area. Restrictions and limitations in China are not absolute, they vary over time and space, and sometimes restrictions on one kind of field activity create unexpected opportunities for collecting other kinds of data. In any case, as I have argued, researchers in China need to be sensitive to the fact that we are walking down a track already beaten by investigators or researchers whose goals were more clearly of a political nature, but whose methods in the eyes of those ‘investigated’ resembled our own. In rural areas of China I have often been introduced to peasants and other people as a foreign scholar doing kaocha. This term implies that one is doing an investigation or inspection. It is more specific in its reference than the more general term for research, yanjiu. Local officials and assistants may prefer to use the term kaocha because its particular meaning is so familiar to peasants and other Chinese citizens who have been used to investigations into all aspects of local social life. As a foreign researcher in rural China one sometimes has the advantage of being an outsider and therefore also relatively innocuous. People can, and often will, tell you things that they would not necessarily like or dare to reveal to a local person. At the same time, the long tradition of social investigations into the level of households means that also the foreign researcher is easily placed in a larger category of officials/investigators with certain rights and powers to check and control social behavior. This creates ethical dilemmas which deserve attention not merely in the fieldwork situation but in the processing and presentation of data. While it is not ideal for an ethnographic researcher to be categorised as one of many social investigators with close connection to policy implementing institutions, it does at the same time open doors and facilitate access to the field.
NOTES

1 This situation is now slowly changing, and since the mid-1990s especially more Chinese and foreign researchers have managed to carry out long term ethnographic fieldwork living in private homes in villages of China. See for instance Mueggler 2001 and Jing 1996. My own first fieldwork period was in 1991 as a graduate student, but it was only 14 years later, since 2004, that I have finally been allowed to stay with private rural families carrying out officially approved fieldwork in Fujian and Shaanxi.


3 The Tibetan nomads were allowed to have three children. Cadres and others with a household registration in the county capital were allowed one child if they were Han and two if one of the parents belonged to an ethnic minority, normally Hui or Tibetan.

REFERENCES


Chapter 6

‘AN OPEN LHASA WELCOMES YOU’: DISCIPLINING THE RESEARCHER IN TIBET

Emily T. Yeh

‘An open Lhasa welcomes you.’
‘An open Tibet celebrates the 50th anniversary of the peaceful liberation of Tibet’
‘An open Tibet welcomes you.’ – Street banners, Lhasa, summer 2001

Since the implementation of national economic reforms, thousands of Han Chinese farmers have migrated to Lhasa, capital of the Tibet Autonomous Region (TAR), to rent land from peri-urban Tibetan villagers for greenhouse vegetable cultivation, an activity from which they earn considerable profit. In September 2000, I arrived in Lhasa planning to study ethnographically the political ecology of greenhouse vegetable farming in one village. Political restrictions made this impossible, forcing a change in research design. Nevertheless, through ten months of fieldwork, I was able to analyse the political economy and cultural politics of greenhouse vegetable farming and land rentals, and to use the case as a lens onto broader questions about landscape, development, and state incorporation. This was made possible in part by two factors.

First, I gradually realized that my fieldwork difficulties were produced by the same strategies of state control that shaped the lives of the people with whom I was working. Thinking reflexively about my internalization of certain common idioms of state discipline, for example, helped me to understand why local Tibetan farmers and Han migrants made some of the decisions that they did. Second, Tibetans with whom I worked incorporated me into their everyday ‘repertoires of resistance’ by shaping the way my identity was read in different situations. In particular, they creatively made use of the fact that I am a Chinese-American
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who speaks Tibetan. This in turn gave me insights that I otherwise might not have had. In this essay, I use my fieldwork experience to illustrate how political constraints on access to data can significantly re-shape field research, but also how those same constraints can lead to unexpected insights. I also show how, as fieldworkers, we are not always in control of how people interpret our identities.

THE POLITICS OF FEAR

The well-documented political repression in the TAR is the product of the Chinese state’s inability to naturalize completely a particular narrative of Tibetan history. The active work needed to produce and maintain the narratives on which state legitimacy is based takes the form of an ongoing ‘struggle against splittism.’ The resulting politics of fear is not unlike the situation that Mayfair Yang (1994: 20–21) described concerning her fieldwork in Beijing in the early 1980s:

In the first half of the 1980s, the culture of fear was still a powerful force in constraining actions and speech in everyday life… even the absence of direct threats to one’s personal security did not signal a dramatic change in the habits of wariness and self-protection.

Visible forms of surveillance help maintain this politics of fear. In Lhasa, I learned about the fact of surveillance before my field research, while I was a language student at Tibet University. Like other schools in China, Tibet University strictly segregates foreign students from the rest of the student body. Unlike many other schools, however, Tibet University still requires anyone who enters the foreign students’ dormitory to provide extensive information, including whom they visit, age, name, and identification number. More importantly, this careful record of visitors is collected and reviewed by the school’s foreign affairs office. This system clearly gave foreign students the message not only that tabs were being kept on us, but also that we could easily bring trouble to local Tibetans.

This politics of fear means not only that access to interviews and data is far from ideal, but also that all interactions and relationships are shaped to some extent by wariness of political trouble. Thus, fieldwork constraints can be more than just headaches to work around; they can be constitutive of the final research product. In my fieldwork, this worked through my own inculcation into the politics of fear, and its corollary, the fear of politics. As I have discussed in detail elsewhere (Yeh 2003), the politics of fear produces subjects who internalize surveillance, and this self-surveillance is directed at anything which might be construed by
state agents as ‘political.’ However, the boundary between the political and the non-political is ambiguous and unstable, a condition which further encourages self-policing. For example, many of my Tibetan friends in Lhasa were convinced that their phones were tapped, and thus were quite careful about monitoring what they said on the phone.

To the extent that I became an active participant in this politics of fear, I contributed to the production of state power (Yang 1994: 21). I unconsciously adopted specifically Tibetan idioms of fear, in particular the frequently expressed notion that “what we know in our minds/ hearts (sems), we don’t need to say with our mouths.’ I did not realize the extent to which I had begun to internalize these disciplinary techniques until an American friend visited Lhasa and invited me to dinner with some other tourists. At a Sichuanese restaurant, they joked boisterously about the fact that the Dalai Lama was ‘the name that can’t be said,’ and loudly bemoaned human rights violations in Tibet. I found myself becoming increasingly nervous and agitated. I looked around the restaurant, wondering if any of the other patrons were state security agents. Could anyone else understand what the tourists were saying (in English)? Why couldn’t they keep their mouths shut? Finally I told them in a low, urgent tone, ‘There are some things that you know that don’t need to be said!’ In other words, I found myself unconsciously adopting local forms of self-surveillance. Reflecting on this later gave me greater context to help interpret various comments and behavior that I observed.

RESEARCH ACCESS AND THE RE-SHAPING OF FIELDWORK

How, then, did this politics of fear shape my research access and methodology? As in other parts of China, official research requires a sponsoring work unit to issue a letter of invitation, but in the TAR the unit must also obtain a Tibet entry permit. During a year of language study in Lhasa two years prior to dissertation research, I tried to cultivate relationships with every work unit that might possibly be able to issue an invitation. By the end of the year, one had extended an oral invitation to me. However, not long before I was to leave for Tibet, I received a terse fax stating that the letter of invitation would not be issued after all; no further explanation was offered. The circumstances of the withdrawal were unusual, but not that surprising considering that, to my knowledge, up to that point no US-based graduate student had received official permission for village field research in the TAR. Fortunately, when I scrambled to contact everyone I knew in Lhasa after the invitation fell through, a relatively high-level employee
at another unit, whom I had met at a conference, was able to issue an invitation and an entry permit. Relieved, I asked no questions and arrived in Lhasa hoping that this was, in fact, all I needed for my dissertation fieldwork there.

Things did not turn out to be so simple. My sponsor was understandably nervous. He called me into the office three times in the first week after my arrival, each time to give a lengthy warning that I must never say or write anything critical of the Chinese government or its policy. He told me in no uncertain terms that I was not to become involved in ‘politics’ in any way. He also suggested that I might not need to conduct interviews at all. Perhaps I could just sit in his office and read locally published research journals. Furthermore, official permission to live in a village was definitely out of the question, as it would have required additional permission from other administrative units. It became clear (though he never said this explicitly) that my sponsor was unwilling to request this permission because it would have attracted more attention to his invitation and my presence. When I maintained that I really would need to talk to people, even if I did not stay overnight in a village, he told me that I should have a *peitong* – a minder from the work unit who would assist me but also report on my actions. I replied that this would be fine with me, and that we should negotiate the pay and time commitment of the *peitong*. However, my sponsor then dropped the issue without ever raising it again.

At first I thought this was an advantage: without an official worker watching me at all times, I would be free to talk about whatever I wanted. Soon, however, I realized that this was not the case. Not only did I find myself monitoring myself at all times, perhaps more effectively than a *peitong* could have done, but I also found it difficult to find an appropriate research site. Spending every day in, and interviewing all of the households of, one village would inevitably attract a great deal of attention. Village leaders would therefore want assurance that there would be no political trouble from such concentrated attention. Their fear of being held responsible for not preventing any political trouble leads officials to want multiple forms of documentation, not because it is required legally, but rather to serve as insurance against unforeseen consequences. In fact, there seemed to be no uniform regulations or understanding of what exactly would constitute proper official documentation for a systematic village study. However, having a letter of introduction with an official red seal stating that I would conduct a village study surely would have helped put village leaders at ease.

Because a clear letter about a village study was not forthcoming from my sponsor (more on this later) my fieldwork took on a very different form than I had originally intended. Instead of an intensive study of vegetable farming in
one village, I conducted interviews extensively, in each of the more than twenty peri-urban villages and state farms around Lhasa where land rentals were taking place. In contrast to intensive daily interviews in one village, visiting households in different villages on different days seemed much less troubling to everyone. Many village leaders agreed to be interviewed once or twice. Furthermore, I was able to make repeat visits to certain villages over a period of months.

Like other researchers, I found that spending a relatively long period and making repeated trips to China worked to my advantage. Toward the end of my first six-month trip, I met by chance a high school teacher from one of the peri-urban villages in which I was most interested. Through her introduction to her family and friends, I was able to conduct a mini-ethnographic study of that village on a later trip. Finally, another very important way in which these constraints shaped my fieldwork was in expanding the scope of my study. In particular, I learned about urbanization partly because it was much easier for me to participate, observe, and conduct interviews in urban Lhasa than in villages. Later, I saw that both urbanization and greenhouse farming contributed to a larger argument about development and landscape transformation.

Having discussed the ways in which restricted access changed my research methods, I want to return to the question of why my sponsoring unit acted as it did. At first, I did not give the invitation letter I received much thought. Only later did I learn that my sponsor had secured the invitation through a fast-track process, often used for donors and consultants, which bypassed the main regional foreign affairs office. The fact that this is possible is an ongoing irritation to the main foreign affairs office, which would prefer a complete monopoly on the right to issue entry permits. My sponsor knew that this route was the easiest (and at the time, possibly only) way for me to obtain the invitation and entry permit, but also that it would make things more awkward for him should any trouble arise.

Once I was in Tibet, he adopted a ‘one eye open-one eye shut’ way of dealing with my presence. He warned me repeatedly to stay away from politics (and by doing so, partially fulfilled his responsibility) and frequently said, ‘well, just stay out of trouble,’ and ‘don’t draw attention to yourself.’ My burden therefore was not only to avoid trouble for my interviewees, but also to avoid implicating my sponsor in anything that the Public Security Bureau or other government units might not like. The largest difference between my research difficulties in Tibet and the experience of many foreign researchers in China was thus that many of the access restrictions placed on me were never clearly spelled out. I had no peitong, and yet the palpable tension and my internalization of fear meant that I was not free from surveillance: I found myself constantly under self-surveillance.
While my access in villages was precarious, my access to higher-level government officials was virtually nonexistent. My sponsor informed me right away that statistics, policy information, and any interactions with government officials were completely out of the question. I tried to maneuver around this through unofficial relationships, but this was one arena where no one dared to take the risk. For instance, the parents of a Tibetan friend now living in the US were mid-ranking government officials with many personal friends who worked on land policies directly relevant to my research. However, though they were very welcoming and helpful to me in other ways, they told me there was absolutely no way they could introduce me to these friends, even informally, unless I had very explicit letters of introduction. My friend’s mother explained, ‘You know how it is in Lhasa. If you don’t have the right papers, if I take you to go to talk to someone – they will immediately have to report to their superiors about everything you asked, and why you asked it.’

An incident later on suggested that this was indeed the case. A young woman who was a foreign affairs officer at another administrative unit befriended me and offered to help me gather some data. She examined my invitation letter and permit, and in her inexperience, believed that I was all set. She took me straight to a relevant government office. Rather than being allowed to conduct an interview, however, she and I were both questioned extensively about who I was, what I was doing, and who had invited me to Tibet. Fortunately, the fact that her father and sister were both officials in a closely-related government unit prevented them from further pursuing the matter.

RESPONSIBILITY, IDENTITY, AND REPERTOIRES OF RESISTANCE

At the same time that my fieldwork was heavily shaped by political restrictions, it was also shaped by my incorporation into local repertoires of resistance – forms of everyday resistance that are ‘simultaneously conscious of and constrained by the social and political limits of collective and individual action’ (Peluso 1992: 13) – of the Tibetans I worked with. Their attempts to guide me into their everyday tactics of maneuvering around official restrictions allowed me to achieve certain understandings that might not otherwise have been possible. This started with my sponsor, who counted on me to ‘avoid drawing attention to yourself’ by virtue of my ability to blend in. When I lived in Lhasa, I was very frequently mistaken by Tibetans and Han alike to be Tibetan. Less often, I was assumed to be a Han PRC national. Occasionally when I informed local residents that I was not Tibetan, they refused to believe me.
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This presented interesting opportunities and dilemmas, and greatly complicated my thinking about my own fieldwork. Unlike some other researchers in Lhasa (e.g. Adams 1996), for example, I never felt particularly worried about going to Tibetan friends’ homes. Many Tibetans who invited me over for meals explicitly said things like, ‘You should come over to our house to eat whenever you want, poor thing, so far from home. Come over, any time! It’s no problem at all. After all you don’t have blonde hair, blue eyes. You look just like one of us. No one would ever know the difference.’ In fact, my Lhasa friends were sometimes willing to take much greater risks than I thought were wise. Several Tibetans offered to let me live in their homes, because ‘nobody will know the difference.’ ‘Don’t worry,’ they said, when I reminded them that this was not permitted, ‘we’ll just tell them you’re a relative.’ When I complained of my research troubles, I was almost always told, ‘Just pretend to be Tibetan (or Chinese); no one will know the difference!’ My sponsor took the same approach. When I asked him to write a letter of introduction for me to give to village leaders, he did in fact produce one, rather than saying no directly. However, it was somewhat vague about my research (and said nothing about an in-depth village study). More importantly, his letter used only my Chinese name. It did not state that I was from the United States.

In addition to the many ‘Why don’t you just pretend?’ suggestions I received, over the course of fieldwork, five different Tibetan men offered to make a fake Chinese identification card (shenfenzheng) for me, and a handful of Tibetan women offered to lend me their cards. What struck me was the way in which these Tibetans – some of whom I barely knew – took pleasure, indeed downright glee, in thinking about how to help me get around my access constraints. It seemed that they really relished the possibility that I would ‘get around the rules’ – but not because they had personally invested in my success or even cared particularly for my research or for me as an individual. The frequency and motivation with which Tibetans offered to make a shenfenzheng for me upon any mention of my problems as a foreigner suggested to me that this sort of identity deception is part of a repertoire of resistance to state disciplinary strategies. In de Certeau’s (1984: 37) terms, finding ways to get around such obstacles is a tactic, ‘a calculated action … [that] takes advantage of “opportunities” and depends on them … an art of the weak.’

Although I declined these offers, I was unable to escape the question of how my identity would be read and interpreted. When I began to carry out village interviews, the fact that I had no clear, official letter of introduction, no peitong, and no other village contacts made me nervous about how to proceed. I hired several urban Tibetan friends as assistants. They all agreed, but on the condition
that they would make the introductions in the interviews. One friend with whom I conducted a few preliminary interviews introduced me to a village acquaintance as a Chinese college student who was writing a book. Another Tibetan friend, about my age, insisted on presenting us as two (Tibetan) college students from Tibet University. A third always referred to me as ‘a student from Beijing.’ In all cases it was very clear that I was a student doing research. What varied was the presentation of my ethnicity and assumptions about my citizenship.

I felt quite strongly that the correct and ethical thing to do was to present myself very clearly not only as a student, but as an American, despite what my sponsor chose to write in his letter. However, this view was quickly challenged by a middle-aged Tibetan cadre who agreed to help me. Cosmopolitan, well-educated, and traveled, he understood my research and seemed genuinely enthusiastic about my research questions. He promised to help by introducing me to a retired official in a nearby township, an old high school friend of his. This official, he said, would be able to provide me with valuable information about one aspect of my project.

One evening, we took a taxi and then walked the rest of the way to his high school friend’s home. In the taxi, he examined my sponsor’s letter of introduction, and was satisfied to see that it had only my Chinese name on it. ‘It’s good that it doesn’t say anything about the US,’ he said. ‘That would only cause trouble.’ When we arrived, he introduced me to his friend. I was flabbergasted to hear him introduce me as a distant relative of his from a Tibetan area of Sichuan province. He explained to his high school buddy that I had gone to school in eastern China from a young age, but nevertheless spoke some Lhasa-dialect Tibetan, and was now here doing research in the TAR. He added that I had both a Chinese name (as indicated on my letter of introduction) and a Tibetan name. He then asked his buddy to introduce me to other township officials, and to help me with my work. I became very nervous and uncomfortable and, after we left, told my cadre friend in no uncertain terms that I would not dissemble about who I was and that I thought that his introduction had definitely not been a good idea.

He, in turn, was completely exasperated, even angry, with me. Back in his house, he explained his interpretation of the situation to me: ‘Look! First of all, if you tell them you are from America no one is going to tell you anything real. You won’t be able to learn anything at all. But if you just tell them that [my sponsoring unit] sent you and don’t tell them anything else, then they are not responsible. If someone asks them, then they can honestly say they have no idea who you are, and that all they know is you are from [the work unit]. It’s better for everyone that way, don’t you see? It’s better for them, and it’s better for you. They don’t want to know.’ To him, the ethical course of action was to give the
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people with whom I spoke a way out. They did not want to be responsible for the knowledge of who I was, and if I forced them to be, they would either have to refuse to talk to me altogether, or they would have to bear responsibility that they did not want.

The pressure of ‘responsibility’ is a disciplinary technique of the state. As I realized this, I began to believe that giving my interviewees room to maneuver by pleading ignorance was a more, rather than less, ethical choice. The other alternative seemed to be completely withdrawing from the research altogether. Instead, I gradually settled on a framework to help me resolve the question of identity that arose in each interview. I decided that any time I conducted interviews with Tibetan assistants, I would let them decide how they would introduce me, and abide by their introductions (as long as they were clear about my status as a student doing research, which they always were). When I conducted interviews on my own (as became increasingly common as time went on), or when I intended to do a series of follow-up interviews, I informed my interlocutors straight away that I was from the US.

Two interesting observations came of this: who decided to introduce me how, and the way my assumed identity affected my interviews. First, I found that almost uniformly, the Tibetans who assisted me preferred to dissemble about my citizenship. Only two of them initially introduced me as American. Both were relatively well-educated cadres who were more confident in their social position than most of my other helpers. After a few interviews, however, they too changed their ways of introducing me. One was a local Tibetan researcher whom I met through a foreign scholar. Accustomed to working with the other foreign scholar, the Tibetan researcher began our first few interviews together quite formally, introducing me as an American researcher. After a handful of interviews, however, he changed his tactics without warning or explanation. One day, out of the blue, he introduced the two of us as being from the same work unit. After a few more days, on our way to an interview, he warned me, ‘Don’t let any Chinese escape from your mouth … and heavens, no English! Not that you’ve spoken English at all so far. If we tell him we’re from [his work unit], it will be better.’

Being assumed to be a PRC national by villagers was not always the best option for my research. Each reading of my identity had its advantages and disadvantages. Overall, I found that being presented as a PRC national helped primarily in terms of access, especially to low-level officials (village leaders). However, introducing myself as an American sometimes encouraged villagers (rather than officials) to be more, rather than less, frank. In no case did a villager
refuse outright to talk to me when I presented myself as American right away, though village officials did. (Some villagers did, however, say very little). Although villagers were more wary of talking to a foreigner in the first place, those who did so frequently had quite a lot to say.

For example, in one peri-urban village, a high-level government program had built greenhouses specifically for Tibetans to use. However, township officials had rented almost all of the greenhouses to Han migrants in order to profit from higher rents. Nevertheless, I found two Tibetan farmers who were growing greenhouse vegetables, despite corrupt local officials’ attempts to stop them. The Tibetan assistant who accompanied me to this particular village introduced me to both farmers as an American student. I made the mistake of interviewing the first farmer in the village teahouse. He laughed nervously at my questions about greenhouses and stated several times in reply that he ‘couldn’t say,’ because if he did, ‘there will be trouble.’

Learning from this mistake, we interviewed the other farmer in his greenhouse. Although my assistant had said only that I was an American student, the farmer assumed that I was a Tibetan-American. This made him even more eager to talk, in the privacy of his greenhouse, about village politics and local leaders. He added, ‘Before you came, the Chinese television stations came … a lot of television stations, municipal TV stations, leaders … they all came to find out about our greenhouses! But when they ask questions, I always say, “I don’t know!”’ When they asked me, “How many Tibetan families are there [planting vegetables] here?” I told them, “I don’t understand these things. I don’t know.” All of these questions that the two of you are asking – when they asked, I said “I don’t know!” “I don’t understand.” If I answered otherwise, well, that would make it hard for me to stay here [in this village.]’ Later, he said ‘You must have gone over there [to America] when you were very young.’ It was not until that point in the conversation that I realized that he had assumed I am ethnically Tibetan. He added, ‘the Tibetans over there benefit us [in Tibet] a great deal.’

My identity in the field was a joint production between myself, the Tibetan friends who assisted me in interviews, and the Tibetan (and Han) farmers whom I interviewed. All long-term researchers adopt certain patterns of behavior, dress, and speech which help them blend in or live in greater harmony with their host communities. Many are forced to change their presentations of self or modify their positionality to conform to the demands of the fieldsite (e.g. women often have to say that they are married). However, my ‘passing’ as Tibetan was not only a matter of how I looked, spoke, or behaved, but also required that others wanted me (on some level) to be Tibetan. Indeed ‘passing’ may not be an
adequate concept for this misreading of national or ethnic identity since it seems to suggest a degree of control that I frequently did not have. In other words, it should not be assumed that researchers alone have agency to determine how they are recognized by the people with whom they work.

LEARNING ABOUT STATE CONTROL: AN EXAMPLE

Nevertheless, being read as Tibetan allowed me access to situations and insights that would not otherwise have been possible. As part of a larger field of deception, my Tibetan assistants saw it as an effective tactic for negotiating state strategies of control. Here I turn to an example which shows how unintentionally ‘passing’ put me in a situation which then allowed me to analyse responsibility as a disciplinary technique.

A few months into my research, I was placed in the unusual position of being hired as a ‘local interpreter’ on a large bilateral development aid project. At the time, a team of foreign development experts had arrived in Tibet on their first appraisal mission. The government office representing the Chinese side was responsible for finding local interpreters for the foreign experts. They tried to hire my sponsor for his English skills, but he was busy and not particularly interested given the low pay. He asked me to do it instead, presenting it as ‘an excellent opportunity for you to learn more about agriculture in Tibet.’ I was unsure of how to interpret the offer: did he genuinely have my best interests in mind, offering me an opportunity to travel outside of Lhasa to an agricultural area I would otherwise not be able to visit? Or was he merely trying to unload the constant stream of extra work that came his way? Either way, I thought I had better agree to it, since it was thanks to him that I was in Tibet at all. I cautioned him that I might not be able to fully understand the local Tibetan dialect spoken in the distant villages. He assured me that this would not be a problem as there were other interpreters, and that I would only be required to use Chinese and English.

The night before we were to drive to the project site, the Tibetan official who was responsible for coordinating interpreters, and whose office had issued my fast-track invitation letter, met with me. He asked to see my paperwork (which his office had processed), saying, ‘We actually wanted [my sponsor] for this job. We have to make sure all of your paperwork is correct, otherwise – you know what the situation in Tibet is like – this could be a big burden on my head if anyone complains.’ I quickly told him that I was in no way asking to be an interpreter, and that I would be quite happy to withdraw. However, he seemed satisfied with
my papers, and told me what time to be ready in the morning. I was surprised
that even someone in his relatively high position would be concerned about what
might happen ‘if anyone complains.’ I was also surprised that the expediency of
hiring me (because he was having trouble finding interpreters) would outweigh
these concerns. After arriving at the project site, we met the Chinese deputy
director of the relevant prefectural unit, whom I will call Hu. He greeted the
Tibetan official as an old friend and the two smoked cigarettes together during
a long lunch. Afterwards, the Tibetan official introduced the interpreters. I
was taken aback when he introduced me simply as being ‘from [my sponsoring
unit].’ That is, he failed to say where I was from, and thus Hu assumed that
I, like the other three, was a PRC Tibetan. The Tibetan official had clearly
weighed the probability that my citizenship would be uncovered against the
possible consequences of being criticized for recruiting an American interpreter;
he had decided that the former was unlikely enough to make avoiding the latter
desirable. In doing so, he had not lied, but merely played upon Hu’s assumptions.
I witnessed this particular tactic many times in my fieldwork. In the meantime,
I had become a ‘local interpreter.’
This was particularly worrisome to me because I did, indeed, find myself
having trouble understanding the local Tibetan dialect. After a few days, while
I was trying to work out how I would get myself out of this situation, Hu called
the four ‘local interpreters’ to a series of private meetings. One of the other
interpreters had complained about our compensation. After discussing this issue,
Hu turned to more important matters. He told us that we should not translate
any sensitive questions that the foreign experts might ask the villagers. According
to him, this included questions about contagious diseases, birth control, politics,
human rights, the Dalai Lama, and military districts. He commented we must
keep his warnings in mind. Otherwise, he lamented, he might be notified out
of the blue, weeks later, that there was a political problem. At that point, he
said, it would make no difference that he was the leader and we the interpreters.
‘Everyone, all of you and I as well, will have trouble.’ He went on to explain:

Actually, I should count as someone who is relatively familiar with politics,
but even I don’t know sometimes what kinds of things will suddenly become a
‘problem.’ One never knows when something will suddenly become a ‘problem.’
Therefore, the principle is to stick closely to the project. For example, if a villager
says, ‘There was no well here in the past. Now we have a well, but we still need
more water;’ – that is perfectly fine. After all, that is the kind of work this project
is supposed to do. But you must avoid sensitive issues, especially politics. You
should also avoid talking about resettlement …This is our dilemma. On the one
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hand … they are the project experts so we have to let them do their work. On the other hand they are foreign project experts, so we also have to keep that in mind. Otherwise I may get a phone call, and there may be a problem, and then I won’t even know where or why the problem arose. So we must understand and help each other.

This incident showed me that even someone as experienced and familiar with politics as Hu very much feared ‘politics’; yet he was unsure about what exactly counts as politics at any particular point in time. In the absence of clear rules, Hu casts a wide net, forcing very conservative behavior for all lower-level officials. The rubric of responsibility thus becomes a political technology which disciplines subjects through mutual surveillance and self-surveillance. This insight later contributed to an analysis of why the trajectory of economic reform in the TAR has differed in certain ways from that of other provinces. My point here is not only that these forces strongly shaped my fieldwork, but also that I only learned about them through the agency of local sponsors who deliberately put me in a situation where my identity was mis-read.

A note on ethics

I want to conclude by noting that ethical dilemmas in fieldwork are not frequently discussed in depth in a typical graduate education in the social sciences (on ethics, see also Svensson’s chapter). In thinking through my own questions about ethics, I have found the Statement of Professional Ethics of the Association of American Geographers (1998) to be quite helpful. In particular:

Informants and local researchers should be asked whether they prefer anonymity or recognition, and the project should be implemented and its results should be presented in keeping with these individuals’ preference … local traditions and mores should be respected unless they directly undermine the basic human rights of affected individuals. Moreover, assistance provided by local informants and co-researchers may be controversial, and protecting these persons should be paramount.

In this light, the particular circumstances of my ‘passing’ seem to me to be ethical. As discussed above, I never hid my role as a researcher. On the question of reading ethnic and national identity, I let local co-researchers decide what they believed, based on their local knowledge, was in the best interests for themselves, local informants, and me. In many instances this allowed people to talk without burdening them with the responsibility of knowledge of my
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citizenship or ethnicity (even if, perhaps, they had their private suspicions). The way responsibility is used by the state to discipline or control subjects suggests that providing the opportunity for local people to honestly say they didn’t know much about me could reduce risks. Based on similar logic, I tried to follow measures in fieldwork and in later writing (as in this essay) to maintain the anonymity of Tibetan interviewees.

NOTES

1 See for example the extensive reports by Tibet Information Network. http://www.tibetinfo.org

2 A number of caveats are in order here. American professors Melvyn Goldstein and Cynthia Beall had been given permission to conduct long-term fieldwork. A number of European graduate students, especially from Norway, who worked under official cooperative projects were able to obtain official permission. A few American and other graduate students were also able to conduct research unofficially. Finally, note that since my fieldwork ended, the establishment of the Tibet Himalayan Digital Library has allowed a number of US graduate students to do research officially.

3 Zheng yi zhi yan, bi yi zhi yan, i.e. to turn a blind eye, to pretend not to see

4 These are reconstructions of his speech based on my notes taken shortly afterwards.

5 http://www.aag.org/Publications/OtherPubs/EthicsStatement.html

REFERENCES


Chapter 7

BEYOND OFFICIAL CHINESE: LANGUAGE CODES AND STRATEGIES

Stig Thøgersen

It is possible to do research in China without speaking Chinese, but it is necessary to have a language strategy during fieldwork as practically everything we learn about Chinese society and culture in interviews, surveys, and ordinary conversation is mediated by the Chinese language. In spite of the striking linguistic complexity that surrounds us in the field, however, methodological reflections on the language issue are extremely rare in the China literature, and we often refer to interview data without even mentioning whether the interviews were conducted directly in Chinese or through an interpreter, in Putonghua or in a local dialect.

In this chapter I shall suggest the use of a language strategy that makes it possible to focus both on what our informants say and on how they say it. Such a double approach can often be fruitful because the Chinese we talk to not only speak a large variety of dialects but also express themselves in different language codes depending on their socio-political position and the specific speech situation. Even though we can never fully master all nuances of this complexity we can get interesting hints about Chinese perceptions of social reality by paying more attention to how people express themselves.

One important facet of this issue is how the speaker’s relation to political power is reflected in ‘official’ and ‘unofficial’ language codes. Mette Halskov Hansen’s chapter shows how we tend to ‘walk in the footsteps of the Communist Party’ when we do fieldwork, and this problem has a linguistic dimension as well. As foreigners we are almost always trained in standard textbook Chinese, and our reading of government documents, newspaper articles, and academic writings further reinforces our confinement to the official code. If we want to know how Chinese people outside the bureaucracy think and speak, however,
we must find ways to loosen these linguistic restraints and enter a less familiar discursive terrain.

I shall take as my starting point some of the practical language problems I have faced in China and discuss how they reflect the question of language codes. The second part of the paper exemplifies how these codes work by looking at the terms used to designate officials and ‘ordinary people’ in one locality in Yunnan. The very deliberate use of language codes in political discourse suggests that the same diversity that makes Chinese so difficult to master for the non-native speaker also makes it a valuable source for understanding China. In the last section I shall suggest some elements in a strategy that is sensitive to such socio-political aspects of language use.

DO YOU SPEAK CHINESE?

When I first arrived in China in 1974 I had studied the Chinese language at university for a couple of years. In the conversation classes our native Chinese teacher, in her beautiful, educated Mandarin, had taught us to exchange polite pre-1949 greetings and order exotic dishes, while the rest of our lessons had been equally divided between People's Daily editorials in the Maoist jargon of the day and equally opaque philosophical texts in wenyanwen (classical Chinese). My exposure to this blend of texts made me realize that the term ‘Chinese’ covered a motley crowd of language phenomena, but it still came as a shock and a disappointment that I could hardly make any sense of the guttural snarl produced by the driver sent by the Beijing Languages Institute to pick me up at the railway station.

‘The Chinese language’ is in itself a political construct, of course. It may, in different contexts, refer narrowly to the national standard language Putonghua or more broadly to seven (some would say up to ten) major dialect groups that are mutually unintelligible and would all qualify as languages in their own right if it were not for the written characters and the political and cultural unity of the Chinese state (Ramsey 1987). Add to this an endless number of sub-dialects and local varieties and the numerous non-Chinese languages spoken by ethnic minorities and it is evident that non-native speakers of textbook Chinese run into language problems as soon as they leave the protected world of educated informants (and often well before that time, but that is another story). Even native speakers doing fieldwork outside their own dialect area will either have to communicate in standard Chinese, which their informants sometimes speak poorly, or rely on a local interpreter.
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When the driver addressed me at the station it was my first encounter with one of the many kinds of Chinese that people actually speak in their daily lives – as opposed to what I had read in textbooks and the People’s Daily. I shall call all these varieties ‘Baixingese’ after the laobaixing, the ‘ordinary people’. In contrast, I use the term ‘Ganbunese’ to refer to the official language of the state apparatus and its cadres (the ganbu). I have come to find the difference between these language codes to be quite informative about social and cultural life in rural China, which is my main field of interest.

Ganbunese is found in documents, public announcements, and newspapers, and it is transmitted in written form from the central leadership down through the administrative hierarchy. From written texts it flows through speeches and oral reports into the spoken language of officials and other citizens. It counts Guanhua, the old lingua franca of the imperial officials, among its ancestors, but in its present form it is closely tied to the construction of the party-state and by using Ganbunese one signals identification with the state, or at least demonstrates one’s knowledge and mastery of the state’s rules.¹

Baixingese, on the other hand, comes in many varieties, from the local dialect of peasants in a remote village to an intellectual’s comments sprinkled with classical quotations, and it can therefore only be analysed in its particular manifestations. It has considerable prestige in some situations, and it can be used to express an authentic belonging to Chinese culture, which is not automatically granted to Ganbunese. Speakers may therefore on some occasions use it to deliberately differentiate themselves from the mental universe of Ganbunese and, implicitly, from the party-state.

The Ganbunese/Baixingese dichotomy I use here should be understood as a simple heuristic device meant to draw attention to socio-political aspects of Chinese language use, and I shall not claim any elevated linguistic status for it. It should, however, not be confused with the question of dialect. Ganbunese spoken in the Shanghai dialect is unintelligible to a cadre from Beijing, but its terminology and mental universe is familiar. In contrast, there is not necessarily any phonological difference between Ganbunese and Baixingese spoken within the same area. The distinction is also different from the traditional concept of class-based social dialects (or ‘sociolects’) because it reflects a hierarchy which is primarily based on political power. Many Chinese master both codes as part of their linguistic repertoire and are able to switch between them depending on the situation. Those who do not master Ganbunese, however, are clearly disadvantaged on many public occasions.²
Baixingese manifests itself in a local dialect, the speaker talks at natural speed (which normally means fast), and non-native speakers are often only able to contextualize his or her words in the most rudimentary way. It therefore represents a serious challenge to the fieldworker’s linguistic competence as the following example illustrates:

I was doing interviews in the year 2000 in a township in Xuanwei municipality, Yunnan province, on the role of the local state and cadre–peasant relations. A local, low-ranking official, Xiao Wang was appointed to be my local guide because of his fine standard Chinese. During my interviews with villagers I heard much criticism of the local cadres, and it was my impression that people spoke quite openly without feeling intimidated by the presence of my guide. As a break from the interview routine I one day went with a European colleague to a market in a nearby town. Xiao Wang disappeared to buy some things for himself. Without a guide, outside the official interview situation, and in a town where nobody knew who we were or what we were doing our exchanges with the villagers gradually took a different form from what we had become used to during the more formally organized interviews.

Almost as soon as we started talking to the traders in the market they began to complain about their situation. One man said that the difference between rich and poor was increasing, and that he could see very little improvement over the last years. They were still very backward (luohou) out here, he said. An old woman complained even more. She said that the laobaixing were very pitiable. People in the mountain villages were extremely poor and the tobacco price was low. The ‘office-holders’ (dangguande) were corrupt, and when the area had received aid after a recent flood it had all been eaten up (chidiaole) by the officials. The villagers had nobody to complain to, nobody who would listen, so she hoped we would tell people from outside the area about their situation.

This part of their talk was not much different from the official interviews, and it was not so hard to follow. The man obviously addressed us in Ganbunese. A term like ‘difference between rich and poor’ is regularly used in official reports, and the word ‘backward’ is clearly part of the Ganbunese discourse on the rural areas. The woman presented her complaints in a way that came closer to what I call Baixingese. She talked about ‘office-holders’ (dangguande), a derogatory term for officials, and ‘ordinary people’ (laobaixing) rather than cadres and masses, and she made use of one the most popular Baixingese metaphors for cadre-peasant relations when she described officials as predators ‘eating up’ the villagers.

Before long we had a whole group of discontented farmers around us, and soon we could hardly understand a word of what they were saying. One problem, of
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course, was the dialect. Technically speaking the language spoken in Yunnan is a
variety of Mandarin, which Putonghua is based on, but it is still far enough from
the national standard to make it hard for me to understand unless the speaker
makes a serious effort. On top of this came the *tuhua*, the local vocabulary that
differs from what one could call the local standard, that is, Yunnanese as it is
spoken by educated people in Kunming. There was also the question of speed,
as people generally tend to speak more slowly when they adapt their language
to a local standard. But on top of this came something else. During my official
interviews I was at least able to follow what people were talking about even
when they spoke in dialect, and when Xiao Wang translated key phrases into
Putonghua I could maintain the illusion that I was interviewing people in their
‘native language’. Listening to unprocessed local Baixingese, however, made me
realize that he had not only translated from the local dialect to standard Chinese
but also from Baixingese to Ganbunese or, more precisely, the fact that we came
as foreign visitors who were out to ‘investigate’ (*kaocha*) local conditions in the
company of a cadre from the township had marked our interviews as official
occasions where Ganbunese was the correct code to use.

The situation in the marketplace was quite different. When people realized
that we spoke Chinese they did, of course, expect us to understand what they were
telling us. They knew, after all, that they were speaking ‘Chinese’ and not some
obscure ‘minority’ language. After a while I got the impression that most speakers
were addressing the rapidly growing crowd rather than us, and this change of
audience made them even less inclined to adapt to our limited linguistic abilities.
When we left they were still complaining – now in a language that was practically
incomprehensible to me.

What did we miss from not fully understanding their complaints? Not many
hard facts about cadre-peasant relations I think, although, in the nature of
things, I cannot know for sure. In our formal interviews villagers had raised all
sorts of problems about confiscated land, excessive taxes, birth control, poverty,
and cadre corruption, and it is hard to imagine that people at the market would
have been able to add much to this already quite comprehensive list. But a full
understanding of what they were telling us would still have been a strong source
of information. How did they express their dissatisfaction when they were
not confined by more formal ways of expression, which social categories and
metaphors did they use to describe their problems, which cultural resources did
they draw on to air their grievances? What is said is inseparable from how it is
said, and an analysis of the totality of form and content might have thrown a
different light on cadre-peasant relations.
Beyond Official Chinese: Language Codes and Strategies

It is hardly surprising that very few foreigners, in my experience, ever become so fluent that they can pick up the finer points and subtexts if people speak non-standard varieties of Chinese at a natural speed, and the need for a laowai (foreigner) to have a specific strategy on this issue is therefore obvious. However, the difference between Ganbunese and Baixingese means that also native speakers of Chinese, and even native speakers of the local dialect, may get entangled in very awkward communication patterns. I recently read an academic paper where a group of researchers from a Chinese university who worked on the ‘ideological and spiritual situation’ of young peasants had included a transcript of one of their interviews. After asking for some factual information about their informant’s family background, education, and income, they proceeded to their main interest, the present state of mind of young rural people. Some of the questions and answers went as follows:

Q.: Do you find life very boring?
A.: Extremely boring, there is no purpose in living.
Q.: Don’t you strive for anything in life (ni mei you rensheng zhuiqiu ma)? Like performing some great acts in the future?
A.: What a joke. What can I do? It’s up to you guys to perform great acts!
Q.: Have you ever considered that you have some responsibilities (fu diar zeren) towards your people and nation?
A.: Yes, when I was young, but I am a peasant. What can I do?
Q.: Do you know about Marxism and Communism?
A.: Of course I do, it’s about how the Communist Party should serve the people!

I am not convinced that this informant was really fed up with life, but he was certainly fed up with his interrogators’ questions, which were so obviously part of the Ganbunese discourse on ‘the meaning of life’ and social responsibility, and which must have reminded him of his teachers back in school. His replies express a combination of annoyance and mockery, and he takes every chance to turn the Ganbunese expressions of the interviewers against themselves. His last answer may even be read as an accusation against the CCP for failing to ‘serve the people’ and thus indirectly against the researchers, as representatives of the Ganbunese-speaking elite, for not practicing the Marxism they question him about. The interview was followed by an analysis where the researchers pointed out that young rural people are depressed, confused, and without ideals, but it is not difficult to see that if the authors wanted to know what their informants were actually thinking they should have chosen another language strategy.
Chinese social scientists are becoming increasingly aware of the problem of language codes, and some have started quoting or even meticulously listing popular sayings and expressions such as satirical sayings (shunkouliu) and rhymes in their surveys. Cao Jinqing, author of *China Along the Yellow River*, a best-selling book on rural problems, argues that this type of material should be systematically collected because it allows us to ‘correctly understand the social mood (shehui qingxu) of all social strata, and particularly the lower strata’. According to Cao (2000: 578), such social moods ‘are social facts, and the most important kind of social facts’. Translated to the terminology of this paper this means that to Cao there are aspects of peasant life which cannot be understood or expressed in the usual language of Ganbunese ‘reports’, but which are reflected in condensed form in Baixingese set phrases.

In relation to survey research the language code issue has been discussed by the sociologist Li Qiang, who describes the existence of what he calls two ‘discursive systems’ (huayu tixi), one public and one private. He sees these two systems primarily as remnants of the Cultural Revolution when a single mistake in terminology could cause political disaster. Li Qiang thinks that people are now under less pressure to express themselves in politically correct terms, but if their local leaders are present or if they are asked questions of a political nature it is his experience that people still relapse into the public discursive system and produce ‘correct’ replies in the standard political terminology (Li 2000: 41). I think the question of language codes is more general than Li indicates here, and although it found some extreme expressions during the Cultural Revolution I sincerely doubt that it can be linked directly to that time. However, Li Qiang’s awareness of different ‘discursive systems’ as a direct, practical problem for social research in China is very much to the point.

So while a solid grasp of Chinese is definitely an advantage for doing research in China, it would be a mistake to think that even a native speaker’s fluency guarantees a successful communication, or, for that sake, that insufficient Chinese language skills prevent you from conducting meaningful interviews. In actual communication ‘speaking Chinese’ is not a plain technical skill that can be measured on a one-dimensional scale ranging from ‘knows a few phrases’ to ‘native speaker’. There are many varieties of Chinese and even more ways to establish fruitful – or barren –communicative relationships.
Beyond Official Chinese: Language Codes and Strategies

SOCIAL CLASSIFICATION IN GANBUNESE AND BAIXINGESE

While the rich diversity of the Chinese language(s) constitutes a very real problem during fieldwork it also offers gates to different ways of thought, as I shall try to show with an example from Xuanwei where I worked on cadre-peasant relations in the broadest sense, from education and propaganda to taxation and social control (Thøgersen 2000, 200). One way of approaching this topic was to look at social classification: how did people conceptualize the state and its representatives, and how did they divide their social world into ‘we’ and ‘they’ groups? The pattern that emerged helped me understand local perceptions of the socio-political scene.

In Ganbunese the state representatives are normally referred to as cadres (ganbu). In the strictest sense, the term ganbu should only be used to identify state officials who receive their salary from the state (Liu 1987: 36), but in practice it is applied more broadly to people who are invested with some sort of official political authority. Coexisting with the term ‘cadre’ with its links to Leninist ideas of a vanguard party we find other terms with somewhat different connotations. A number of expressions containing the traditional term for ‘official’ (guan) can be used in Ganbunese, such as guanyuan, which is widely used in the media about officials in other countries than the PRC. The term has connotations of apolitical, technocratic state employees who exercise their authority in a rule-governed and impartial way. In this sense it represents a search for a rational and bureaucratic type of legitimacy to replace the compromised idea of the selfless and idealist Communist cadre. Another term with somewhat similar connotations is gongwuyuan, government employee, which is extensively used in newspapers and documents and is now seeping into the spoken language.

In Baixingese the word ‘cadre’ is also used, but we find other expressions that are less respectful. Because the character guan points backwards in history to political regimes with dubious records of public administration – such as the late Qing state and the pre-1949 Nationalist government – it is also an obvious choice for expressions indicating that ‘you cadres are just a bunch of bloodsuckers like the old corrupt officials’. The term ‘office-holder’ (dangguande), which the woman at the market used, is often used in this way in Baixingese. It practically always has negative connotations, and the cadres themselves very rarely use it.

The have-nots in relation to political authority are called ‘the masses’ (qunzhong) in Ganbunese. There are several alternatives to this term, however. In rural areas the word ‘peasant’ (nongmin) is often used in both codes as the direct antonym to ‘cadre’. The word is a quite recent import from Japan, and
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has strong connotations of being ‘passive, helpless, unenlightened’ (Cohen 1994: 154–55). Some township leaders, for example, talked about the rural population in their district as ‘my peasants’ (wode nongmin) in a clearly patronizing tone. A more recent alternative is ‘villager’ (cunmin). It signals a person with certain legal and political rights and is often used in connection with village elections, lawsuits, etc. In this sense it is related to the term for citizen (gongmin), but it has significantly lower status because of its association with the rural areas.

In Baixingese the most popular and commonly used antonym to ‘cadre’ is the traditional term ‘laobaixing’ (lit.: the old 100 family names), which I translated above as ‘ordinary people’. Its use is in no way restricted to villagers, but they frequently use it to mark their own exclusion from the political-administrative elite. The term in itself plays on belonging to a surname group (xing) and these genealogical connotations give a sense of rootedness and original Chineseness, which can be a strong source of legitimacy for claims made against the power-holders.

In real-life discourse Ganbunese and Baixingese appeared as different registers that were combined for different purposes. An illustration of this is an unofficial poster distributed in Xuanwei. In 1999 one of Xuanwei’s township governments had confiscated land from a village, and the villagers were dissatisfied with the compensation they had received. They protested vehemently but to no avail. To make matters worse, seven villagers were brought to court and sentenced to pay 170,000 yuan for damages caused by their acts of protest. As a last resort, the villagers in January 2000 put up a poster on the walls of the county seat.

The term of address used in the headline of this poster was ‘fellow villagers’ (fulao xiangqin). This phrase has connotations of kinship and common place of origin and thus serves to establish a community of belonging which significantly excludes the top leaders of the township, who are always recruited among outsiders. The term is used again later in the text in a direct appeal for economic support, and in the concluding sentence.

In the main text the villagers generally use the term qunzhong about themselves and ‘township government’ about their opponents. I would suggest that qunzhong is used here to emphasize that the authorities have certain obligations towards the villagers. According to the principle of the ‘mass line’ (qunzhong luxian), the Party is obliged to listen to and serve the interests of the masses. The villagers claim that the government does not live up to this obligation, and the Ganbunese term ‘qunzhong’ is useful in this context. The word cunmin is used in those (and only those) sentences that involve legal rights, such as the arrest of seven villagers and the court trial against them. The poster is signed by ‘all cunmin of XXX village’,
which is quite logical as the heart of the matter is legal: the villagers claim that the authorities have violated the Land Protection Law.6

The last sentence of the main text is particularly significant: ‘As long as there are officials (guan) who violate the law there will also be ordinary people (baixing) who are not afraid of dying.’ While the Ganbunese masses/government dichotomy in the rest of the text plays on the inability of the cadres to live up to the party-state’s own standards, this final sentence uses traditional Baixingese terms to indicate that the government’s confiscation of the villagers’ land is just one more instance of the age-old problem of officials abusing commoners.

The story later took an interesting turn when the former township Party secretary in July 2003 was convicted to 5 years in jail for accepting bribes from the entrepreneur who had won the bid for developing the land confiscated from the protesting villagers. When the Xinhua News Agency brought the story they described the Party secretary as a ‘township official’ using the term xiangguan in quotation marks. The quotation marks signaled that this was how local people saw him, and the pre-1949 term xiangguan indicated that he had the vices of the old regime rather than the modern, legal-rational mindset of a true CCP cadre. In this case the official media could use a term outside the standard vocabulary of Ganbunese to mark their distance to a corrupt official (Renminwang 2003.7.31).

In the cases discussed above the basic social classification was quite similar in Ganbunese and Baixingese, although different terms were chosen to describe the two groups: those who act on behalf of the state on the one hand and the rest of the population on the other. During other interviews, however, I realized that in most contexts the poorer villagers tended to divide their social world differently. To them the local hierarchy primarily consisted of ‘salary-takers’ (nagongzide) and peasants. This meant that schoolteachers, factory workers, and the staff of the local tobacco station moved to the upper stratum of the social model. The reason for this was obvious: cadre status and the political authority it involved was not of primary importance in an environment where there were few local economic resources to share, and where even village Party secretaries therefore found it hard to make ends meet. To be a salary-taker, on the other hand, was important, because it meant that enough cash to support a family would come in from the outside every month regardless of bad harvests and failing markets. The binary pair of ‘salary-taker’ and ‘peasant’ never turned up during my interviews with the cadres, but its use by especially the poorer villagers signaled that to them political capital was less important than economic capital, and this raised new questions about how the Xuanwei villagers perceived recent socio-political changes.
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With these examples I want to illustrate that people’s choice of words and expressions is important. The way language is used in a given situation to establish social identity and claim political legitimacy reflects ‘social moods’ and may signal conceptual change, like when categories of social classification are revised. People choose their words with great skill and care, and the way they switch codes and manipulate terms from both Ganbunese and Baixingese gives clues about how they think about and act in their society. This creates a need to design language strategies that can catch as many of these clues as possible.

ELEMENTS OF A LANGUAGE STRATEGY

A language strategy must be designed to the particular circumstances of the fieldwork situation and take into account such factors as the researcher’s own language abilities, the linguistic qualifications of his or her interpreter and/or academic co-operator(s), and the distance between Putonghua and the local dialect. I shall suggest two elements in a language strategy that is sensitive to some of the language issues I have mentioned above.

Use different kinds of interviews to produce different types of language material

As I have discussed above, the Chinese language can play a double role during fieldwork. On the one hand it is a practical tool of communication, which calls for a pragmatic approach where both parties adapt their language use and mental worlds to the unique and peculiar social occasion of the interview. On the other hand, the way things are said can inform us about local perceptions of the social world. To make use of this fact we need to know how our informants speak when they are in more ‘normal’ social and linguistic environments, to look at language in its natural habitat, so to speak, when it is disturbed as little as possible by the presence of a foreigner.

I clearly felt this dilemma in the early 1990s when I collected life histories in Zouping county in rural Shandong in order to learn how different generations of villagers had experienced training and education up through the twentieth century (Thøgersen 2002). My initial plan was to let people tell their stories without interruption. Some of them recollected events that had taken place as long as sixty years earlier, and I was afraid it would make things even more difficult to them if they constantly had to rephrase and explain for the benefit of my understanding. It turned out, however, that particularly the older interviewees were hard for me to understand once they really got going about experiences
in their childhood and youth, and when I got back and listened to the tapes in the evenings together with my Chinese assistant I realized that there were many ambiguous and contradictory points in their stories, which I ought to have clarified during the interviews. So if I used language only as a practical tool – with all the interruptions necessary to make me understand their story on the spot – I would miss much of the original flavor, but if I simply let my informants talk I would only catch part of what they were telling me. This made me turn to a more flexible strategy with different communicative approaches for different types of interviews.

With most informants I conducted what I would call dialogue-type interviews, where I looked at language primarily as a tool of communication. My ambition in such situations was to establish a shared definition of the interview as a process where I tried to learn more about education in rural China, a topic which the informant was knowledgeable about. I started by explaining who I was, what my research was about, and what the reason for this particular interview was (see Solinger’s chapter for more details about interviews). This kind of presentation does not overcome the basic problem of uneven power relations during the interview, of course, but if all involved parties agree that the purpose of the exchange is to make the interviewer understand a certain topic better, it makes it possible to enter a meaningful dialogue. In this way the interview becomes a clarification process with interruptions, repetitions and negotiations rather than a monologue or a series of questions and answers.

Such dialogue-type interviews deliver the stories, but they tend to ignore the way the stories are told, because the informant is forced to change his or her ways of expression in order to facilitate the communication. In order to study language use more closely I therefore conducted a limited number of what I would call *language sampling interviews* where the main purpose was to collect material for language analysis. In some of these interviews my Chinese assistant who had grown up in a neighboring county did all the talking, which would give the conversation a more natural flow. In the evening we would then listen to the tape-recordings and analyse and discuss issues of language use, metaphors, set phrases, etc. In Xuanwei I later had the opportunity to have several interviews videotaped and have transcripts made of some of them. The transcripts were written in standard Chinese, so by comparing them to the spoken words on the video I could get a more precise impression of terminology and rhetorical style. Analysing interviews in this way is very time consuming so I have had to limit it to a few cases, but it is a useful exercise in order to sharpen one’s linguistic sensitivity.
Make the best use of ‘the third party’ in the interview

During many interviews where foreign researchers are involved a third party is present, who tries to facilitate the communication either by translating between Chinese and a foreign language, or by fully or partly transforming local dialects into Putonghua. This person can be the researcher’s academic collaborator or assistant, or a professional interpreter, or a local student, school teacher or cadre who knows both Putonghua and the local dialect. In any case this will add an extra link to the communication process, and will very often serve as a filter that will standardize language use and obliterate exactly those phrases that could tell us most about local perceptions. Interviewees will often try to adapt to the more ‘educated’ and politically correct language of the mediator, and their words will be further normalized when translated into either English or Putonghua.

Based on my own experience both as a researcher in China and as an interpreter for Chinese social scientists visiting Denmark I think that the most crucial point about using this type of linguistic assistance successfully is that the interpreter is briefed about the research project in as much detail as possible. Because of the local dialect of my elderly Zouping informants I sometimes asked my Chinese research assistant to translate to and from Putonghua. He was already fully informed about the general aim of the research project but I soon realized how important it was that he also understood the purpose of each individual interview, and practically of each question. I read Chinese more often than I speak it, so he had to translate my probably quite bookish and awkward expressions into phrases that local people of all age groups could make sense of, and this process involved a series of language-related decisions that he could only make when he knew precisely what I wanted to know.

The briefing of the facilitator should include information about the language strategy that should be followed in each particular case. In all interviews interpreters should be given an active and legitimate role in the interview process, and they should be clearly informed that it will not be seen as a sign of incompetence if, for example, they ask the interviewer to explain a question further. But the facilitator should know that different purposes of the interviews call for different communication styles. In the dialogue-type interview he or she should be free to elaborate on and reformulate the interviewer’s questions, interrupt the interviewee to clarify points, etc. In contrast, in what I have called language sampling interviews it is essential that the interpreter understands that informants should be given room to talk as they please without unnecessary interruptions.

The significance of briefing one’s interpreter is even larger when a foreign language is involved. Anyone who has functioned as an interpreter will know
that being regarded as part of the communication process with the goal of establishing a common understanding is vastly different from being treated as a machine that can miraculously transform one language into another. Some people expect top-quality professional interpreters to function in the latter way, but such expectations are based on a naive understanding of language, and in any case most non-Chinese-speaking foreign researchers in China work through academic colleagues or teachers and students of English for whom interpretation is not a profession but a more or less well developed practical skill. These people – even more than the real professionals – need to know what is going on in order to do a good job.

So particularly in language sampling interviews it is of crucial importance to explain why it is necessary to find out how things are being said, and to ask the interpreter to take part in the hunt for metaphors, local expressions, and other interesting language phenomena. Such phrases can rarely be perfectly translated or explained on the spot, but it is possible to discuss them more thoroughly after the interview.

CONCLUSION

If we want to move beyond ‘official China’ we should try to look behind the official Chinese discourse and those constructions of Chinese social reality that are presented in the language of officials and the media. I am not arguing that interview data are valuable only if they are subjected to an analysis of language use, or that all fieldworkers should become amateur sociolinguists or linguistic anthropologists. I have personally never even come close to doing the sort of detailed conversation analysis where a few minutes of verbal exchange can provide data for several chapters in a book, but I believe that regardless of which discipline in the social sciences or humanities we work in, a close awareness of language use can add an extra dimension to fieldwork and make the ‘social moods’ and subjective realities of our informants stand out more clearly. There is a lot of creative and resourceful language use to be picked up, also for social scientists whose field of research is far away from linguistics.

Elin Sæther in her chapter noted how language skills are often used as ‘a marker separating insiders from outsiders’ in the field of China studies. I suggest that at least when we talk about oral proficiency it is more profitable to look at language skills as an element that must be taken into consideration in one’s design of a research strategy. It is, of course, a great advantage for someone doing
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fieldwork in China to speak Chinese, but language is actually a problem for even the most ‘fluent’ foreigner – and for most native speakers as well. It is mainly a matter of degree. The question of language skills is only one of several factors that determine what we learn through communicative interaction with the people we meet in the field.

AUTHOR’S NOTE

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NOTES

1 For a discussion of the CCP as a ‘discursive community’ in the 1930s, see Apter and Saich 1994.

2 I call Ganbunese and Baixingese language codes, but it should be noted that I use the term in a different, and much less ambitious, sense than those who work in the tradition of Basil Bernstein. The two codes do not permeate language use like Bernstein’s restricted and elaborate codes, and they are not to the same extent infused through early childhood socialization. The relationship between Ganbunese and Baixingese bears some resemblance to James Scott’s public and hidden transcripts, but one important difference is that Baixingese is not hidden “‘offstage’, beyond direct observation by power-holders’ (Scott 1990: 4). Baixingese can be used publicly for a number of purposes and in many different contexts, even in open challenges to the authorities.

3 In Chinese fangyan is normally used for ‘dialect’. It is a reasonably neutral term denoting that the speaker comes from a specific locality. Tuhua, ‘language of the soil’ has further connotations of low status and lack of culture.

4 Mishoe (1998) looks at code switching between ‘home style’ and the more formal ‘local standard’ among her informants in North Carolina and notices that ‘a strong indicator of a switch to local standard is that speakers speak more slowly than usual’.

5 Link and Zhou (2002) discuss several examples of shunkouliu. They were told by an informant that the government ‘circulates them in classified reports designed to let officials know what the people are really thinking’ (p. 89).

6 Kevin O’Brien (2001) discusses a parallel example where a farmer plays on his status and rights as a ‘citizen’ (gongmin) to hamstring corrupt local cadres.

7 For an example of how language use and particularly the use of different dialects reflects power relations among female workers in Shenzhen, see Ngai 1999
This kind of research can be fascinating reading also for social scientists when it is linked up with social (rather than purely linguistic) analysis. An example from Thailand can be found in Moerman 1988.

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Section III

DATA COLLECTION
Chapter 8

SURVEYS — THREE WAYS TO OBTAIN
HOUSEHOLD INCOME DATA

Björn Gustafsson and Li Shi

China is changing rapidly. The industrial sector is growing swiftly and the economy is in transition towards a market economy. Labor markets are developing in a situation with great differences in living standards between rural and urban areas. These processes occur at different speeds in various regions of this very large and populous country. While some households are improving their standards of living, others are left behind. Therefore economic inequality in China is on the rise.

To describe and analyse this development poses a considerable challenge to economists and other social scientists. Today researchers are much better equipped than previously to study poverty, income inequality, earnings inequality, income- and earnings-determination, the gender earnings gap and the economic situation of ethnic minorities. Present-day researchers into China are not constrained to begin their statistical analysis with what is available in official statistical publications. They can apply modern quantitative methods to analyse microdata on individuals and households.

This paper describes and discusses quantitative data which have been used to study research questions relating to household income in China in order to illuminate their advantages and disadvantages. We will draw on our own experience as well as on what others have written. As the literature is far from limited we do not claim that our coverage of the issue is complete.

In discussing strategies for obtaining data from households and individuals in China we find it meaningful to distinguish between different types of strategies. The first strategy is that the data are a product of a process initiated by researchers, who are at the centre of all stages of the process: formulating the
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research questions, designing and planning the data collection, monitoring the fieldwork, coding, cleaning and finally analysing the data.

This strategy is attractive for several reasons. It enables researchers to design questions to respondents matching the theoretical framework entailed in the research. If successful, this strategy has the advantage that researchers can be in full control of the entire process. Such a strategy thus has the potential of providing high quality data relevant to the research questions formulated by the researcher.

However, such a strategy requires long-term commitment from the researchers involved as well as long-term funding. This combination means that researcher-initiated data collections are typically limited in regard to the area and time period covered. From this it follows that the strategy is very difficult to adopt if one wants to analyse variations across areas and their changes over time. Further, there is always the issue of to what extent findings from one area studied can be carried over to other areas.

The second strategy is that researchers rely on the data collected by the Chinese statistical authorities in their regular surveys. After the end of the Cultural Revolution, the National Bureau of Statistics (NBS) revitalized its urban and rural household income surveys. These surveys provide the basic information on the distribution of household income in rural and urban China that is published annually in the Statistical Yearbook of China.

Having access to such data, researchers can concentrate on the analytical part of the research project. Collected in a harmonized way over large parts of China, such data make it possible to study spatial differences and their development; issues not possible to examine with the data gathered for smaller territories. Thus this strategy for obtaining data is widely adopted for the reasons mentioned above. However, its usefulness depends on what information is collected by the NBS, how it is collected and if and how it is made available to research.

Regarding these alternative strategies, there is a trade-off between the quality (in a broad sense) and the representativity of the data used by the researcher. However, we claim that much can be gained by combining the two strategies for collecting household data on income, that is, by using the experience of the NBS in conducting the fieldwork as well as in the process of data entry. The researcher’s role in the data collection phase of the research project is to formulate survey instruments applying to subsamples drawn from the broader sample frame of NBS. This approach requires the opportunity and ability of co-operating with the staff of the Statistical Bureau. Further, a research project using this strategy is not suitable for a single researcher, but requires a team in order to make the efforts pay off.
Here we need to say a word on the limitations of the territory covered in this paper. We concentrate on household data. This means we do not cover data used in many (but not all) studies of spatial differences in economic performance across China, as such studies typically work with data on GDP per capita (for recent contributions see Yao and Zhang, 2001, Bhalla et al, 2003 who base the analysis on GDP at the provincial level, while Song et al (2000) use city level data).

THE RESEARCHER-INITIATED STRATEGY

To illustrate the diversity of the researcher-initiated approach we have listed a number of empirical studies on China from which the results have been published in English in academic journals from 1998 to the summer of 2003.\footnote{In Table 8.1 we refer to 25 published academic articles. They are grouped by whether the topic is rural China (the largest group), urban China (the smallest group), or migration between the two parts. Within each group the articles are ranked by the year of the survey. In the table we also describe the coverage and location of the survey, the number of observations, the Chinese collaborating agency, and finally the main purpose of the study.

Table 8.1. A partial survey of researcher-initiated surveys on households in China used by articles published in academic journals 1997–2003

<table>
<thead>
<tr>
<th>Reference</th>
<th>Coverage of survey</th>
<th>Location of survey</th>
<th>Year of survey</th>
<th>Number of observations</th>
<th>Possible collaborating Chinese agency</th>
<th>Main purpose of survey</th>
</tr>
</thead>
<tbody>
<tr>
<td>a) Benjamin and Brandt (1995), (1997)</td>
<td>a) 21 / 22 villages</td>
<td>Manchuria</td>
<td>a) 1935</td>
<td>a) 1049 /1094 households</td>
<td>a) Ministry of Enterprises of the National Affairs Yuan of Manchukuo</td>
<td>a) The role of women in agriculture and the working of factor markets during the 1930s</td>
</tr>
<tr>
<td>b) Benjamin and Brandt (1999)</td>
<td>b) 30 villages</td>
<td>North East China</td>
<td>b) 1995</td>
<td>b) 780 households</td>
<td></td>
<td>b) Long run change in income inequality</td>
</tr>
</tbody>
</table>
### Doing Fieldwork in China

<table>
<thead>
<tr>
<th>Study</th>
<th>Sampling Details</th>
<th>Research Questions</th>
</tr>
</thead>
<tbody>
<tr>
<td>Li and Zhang (1998)</td>
<td>a) One Production team a) Wugong village Hebei province a) 1978 983 individuals in 232 households</td>
<td>The relation between education and compensation for work before and during reform</td>
</tr>
<tr>
<td></td>
<td>b) China’s Official Rural Household Survey b) Sichuan province b) 1988 – 1990 11,231 individuals in 2,734 households b) NBS</td>
<td></td>
</tr>
<tr>
<td>Matthews and Nee (2000)</td>
<td>a) 69 rural counties from which 138 towns- and 138 villages a) 25 provinces a) 1989 and 1990 7,950 households</td>
<td>Women’s opportunities for off-farm work and contribution to household income</td>
</tr>
<tr>
<td></td>
<td>b) 30 villages b) 2 counties in rural Fujian b) 1985 624 households</td>
<td></td>
</tr>
<tr>
<td>Hare (1999a)</td>
<td>Quingyan county Guangdong province 1989 249 households South China Agricultural University</td>
<td>Women’s economic status</td>
</tr>
<tr>
<td>Knight and Li (1997)</td>
<td>7 villages Handan county Hebei 1994 1,000 households Institute of Economics, CASS and Statistical Bureau of the Handan Government</td>
<td>Income differences among villages</td>
</tr>
</tbody>
</table>

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## Surveys – Three Ways to Obtain Household Income Data

<table>
<thead>
<tr>
<th>Researcher(s)</th>
<th>Study Areas</th>
<th>Year(s)</th>
<th>Data Details</th>
<th>Questions Studied</th>
</tr>
</thead>
<tbody>
<tr>
<td>Morduch and Sicular (2000)</td>
<td></td>
<td></td>
<td></td>
<td>Do cadres enjoy rents?</td>
</tr>
<tr>
<td>Morduch and Sicular (2002)</td>
<td></td>
<td></td>
<td></td>
<td>To understand patterns of income inequality</td>
</tr>
<tr>
<td>Bowlus and Sicular (2003)</td>
<td></td>
<td></td>
<td></td>
<td>Emergence of labor market</td>
</tr>
<tr>
<td>Brown and Park (2002)</td>
<td>6 counties located in 6 poor counties Shanxi, Gansu, Sichuan, Guizhou, Henan, Jiangxi</td>
<td>1997</td>
<td>446 households, 40 primary schools and 37 junior secondary schools</td>
<td>China Poverty Research Center</td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td></td>
<td>The relation between education and poverty</td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td></td>
<td>Choice of land tenure</td>
</tr>
<tr>
<td>De Brauw et al (2002)</td>
<td>60 villages Hebei, Liaoning, Shaanxi, Zhejiang, Hebei and Sichuan</td>
<td>2000 and retrospective questions</td>
<td>1,199 households</td>
<td>Renmin University, China Agricultural University</td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td></td>
<td>The evolution of labor markets</td>
</tr>
</tbody>
</table>
## Doing Fieldwork in China

### Urban China

<table>
<thead>
<tr>
<th>Reference</th>
<th>Coverage of survey</th>
<th>Location of survey</th>
<th>Year of survey</th>
<th>Number of observations</th>
<th>Possible collaborating Chinese agency</th>
<th>Main purpose of survey</th>
</tr>
</thead>
<tbody>
<tr>
<td>Maurer-Fazio and Hughes (2002)</td>
<td>26 cities</td>
<td>12 different provinces</td>
<td>1992</td>
<td>9,397 individuals working in 430 enterprises</td>
<td>Economic Institute of CASS &amp; Ministry of Labor</td>
<td>The gender wage gap</td>
</tr>
<tr>
<td>Raymo and Yu (2000)</td>
<td>Claimed to be nationally representative</td>
<td></td>
<td>1992</td>
<td>7,208 (retirees)</td>
<td>China Research Center of Aging</td>
<td>Income sources and income levels of elderly persons.</td>
</tr>
<tr>
<td>Wong and Lee (2000) and (2001)</td>
<td>Urban districts</td>
<td>Shanghai</td>
<td>1996</td>
<td>Slightly less than 1,000 respondents</td>
<td></td>
<td>Popular opinion towards the economic and social consequences of reform</td>
</tr>
</tbody>
</table>

### Migrants

<table>
<thead>
<tr>
<th>Reference</th>
<th>Coverage of survey</th>
<th>Location of survey</th>
<th>Year of survey</th>
<th>Number of observations</th>
<th>Possible collaborating Chinese agency</th>
<th>Main purpose of survey</th>
</tr>
</thead>
<tbody>
<tr>
<td>Hare (2002)</td>
<td>6 villages</td>
<td>Xiayi county, Henan province</td>
<td>1995</td>
<td>309 households</td>
<td>Development Research Center of the State Council</td>
<td>Determinants of job locations and their effects on migrant wages</td>
</tr>
<tr>
<td>Hare (1999b)</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Meng and Zhang (2001) (two surveys)</td>
<td>Not reported</td>
<td>Shanghai</td>
<td>1995 1996</td>
<td>a) 6,609 migrants b) 2,514 urban residents 486 rural migrants</td>
<td>Institute of Population Studies at Shanghai Academy of Social Sciences</td>
<td>The degree of segmentation between migrants and urban residents</td>
</tr>
<tr>
<td>Rozelle et al. (1999)</td>
<td>31 villages</td>
<td>Hebei and Liaoning</td>
<td>1995</td>
<td>787 households</td>
<td></td>
<td>The linkages between migrant remittances and agricultural productivity</td>
</tr>
</tbody>
</table>
In most of the cells in the table there is one paper based on one survey, but there are also exceptions. First there are papers using more than one survey. Sometimes this is due to the design of the study, which focuses on changes over time. In the table there are two such studies, both referring to rural China. The first of these is Benjamin and Brandt (1999) who took a very long-range perspective and compared the distribution of income in pre-Communist rural China and China in reform. The authors used the data collected by the Japanese occupation authority in 1935 (analysed in some separate articles, e.g., Benjamin and Brandt 1995, 1997) together with recent data from the same geographical area. The other example is Li and Zhang (1998) who compared the relation between education and income during the planning period and early reform. This study is based on data from different geographical areas, which means that the researchers have assumed that variation across area is of lesser importance than variation over time.

To use two surveys is the design chosen by researchers of rural migrants who are interested in comparing rural-to-urban migrants with urban residents (Knight et al. 1999). In other cases the researchers have chosen more than one survey referring to approximately the same point in time in order to investigate whether results are robust with respect to sample selected, or because different surveys complement each other when it comes to the information gathered.

In some cases one survey has been used for more than one article. Here one dataset on rural households in the county of Zouping stands out. Zouping is situated south of the Yellow River in central Shandong Province. Although its
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population makes up less than 0.01 percent of the total population in China as a whole, its rural population of 620,000 is of approximately the same size as the population of Cyprus or half the population of Estonia. The data on farm households come from a larger research project that brought together collaborating researchers from many disciplines and resulted in several publications (Walder 1998). One reason why it has been possible to base many studies on the data from less than 300 households in Zouping is that the survey provides detailed information. Probably more important, however, is the fact that the households were followed for several years, making the dataset one of the very few panel datasets available in China.

A panel makes it possible to address research questions for which cross-section data are not aimed. However, researchers can also obtain a time dimension in their data by asking retrospective questions (De Brauw et al. 2002). Notice that there are two limitations to this approach not shared by ‘true’ panel data. First, respondents may not recall all information accurately. While an event like the birth of a child is often remembered without error, this is not the case for many labor market events, not to mention the remuneration a worker received some time ago, or components of income. Second, recalled data cannot be obtained from people who have died or out-migrated. Still another strategy for obtaining a time-dimension in the data collected was used by Perkins (2003). She supplements interviewing with archive research. The latter of course requires that documents have been preserved, and are made available to the researcher.

Researcher-initiated surveys can cover one single territory, like a township (Perkins 2003), a county (Cook 1998, Hare 1999a, Morduch and Sicular 2000, 2002, Bowlus and Sicular 2003) or a major city (Wong and Lee, 2000, 2001 and Meng and Zhang 2001). However, the design can also consist of choosing households in different provinces. For example Brown and Pak (2002) worked with data from six counties located in various provinces. Concentrating a study on one area has advantages when it comes to the fieldwork, but choosing more sites makes it possible to investigate the importance of variations across sites for the phenomenon under study. This means that when designing a researcher-initiated survey there is a trade-off between the efficiency (or cost) of the fieldwork and possibility of investigating the importance of differences across geographical areas.

Turning to the roles of the researchers and the collaborating Chinese agency, we find they differ widely across the studies listed. One extreme is represented by Meng and Zhang (2001) as well as Raymo and Yu (2000). These studies are based on surveys made by Chinese agencies other than the NBS. The articles
give the impression that the researchers have not been involved in the data-collection. These studies also stand out from the rest listed in Table 1 as being based on the largest datasets. Datasets obtained with more involvement of the researchers themselves are typically smaller, or even much smaller, in terms of sample size. Perkins (2003) represents the polar opposite regarding involvement in the fieldwork, as she lived for one year in the area of Tianjin to collect the data.

Table 1 provides a diverse picture about which Chinese agencies have been involved in the data-gathering process. Upon reading the articles one feels that the involvement from the Chinese side can consist of very different activities; that is, from being responsible for all stages of managing the survey to providing the basis for recruiting staff for limited tasks in the survey.

An interesting and somewhat sensitive issue is to what extent the Chinese partners are involved in the analytical stage of the research project. In cases where a research project is set up in such a way that the Chinese side only has the role of providing data, this might not be optimal for the outcome. The knowledge of local experts is valuable – for example when drafting questionnaires, and if Chinese collaborators expect to be involved in analysing the data they have an incentive for keeping the quality of data high.

Academic papers typically provide information on the affiliation of authors of published articles. Few of the authors of the papers listed in Table 1 are affiliated with universities or other institutions in the People’s Republic of China. However, this does not mean that the authors lack a Chinese background. Actually several papers are authored or co-authored by researchers who were born, resided, and received much of their professional training in China, but then became affiliated with universities outside of the People’s Republic of China.

THE STRATEGY OF USING DATA COLLECTED BY NBS

Like almost all countries in the world, the People’s Republic of China has an official statistical system which produces data published in, for example, a statistical yearbook. In China the NBS (earlier known as the State Statistical Bureau, SSB) is responsible for country-wide reporting and it has counterparts at the provincial level. Ideally a country’s statistical bureau produces impartial, high quality and easily assessed information on the situation in the country it serves. How well is this done in present day People’s Republic of China? This is an important but also broad question which we can only address briefly.
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Starting with the issue of impartiality one can note that this provides the major motivation for creating a statistical bureau as a body clearly separated from central government ministries. The central government should not be able to manipulate statistical information. However, such a separation can never be complete as it is the government which defines the tasks for the statistical bureau and also provides it with funding. There is also the subtle issue of self-censorship among government employees who know that some results are more welcome than others. These are the conditions under which official statistics are produced in any country. We are not able to compare the degree of impartially of statistical bureaus in various countries and are thus not able to say how the Chinese NBS performs relative to its counterparts in other countries.

Turning to assessing the quality of the data, this is the outcome of all steps in data production, that is, sampling, construction of instruments, fieldwork, coding and editing the data. Our personal impression from having worked with officials at NBS is that the staff have the ambition of being professional at each step. Unfortunately, however, it is not possible to make a well-based and detailed quality evaluation as the NSB has not published documents describing the various details in the data gathering process. This means for example that the details in the sampling procedure are not documented, neither have researchers been given the opportunity to access the microdata in order to evaluate its quality. It would be valuable if the NBS were to change its routines in these respects.

Statistical bureaus collect and disseminate information for various spheres. When it comes to macroeconomic statistics such as the Gross Domestic Product there are international standards followed by most countries. China has now adopted the System of National Accounts (SNA) meaning that the information is internationally comparable to a large degree. When speaking with specialists in the field of national accounts we understand that China has a relatively high reputation regarding data quality in this area. The opinion has been expressed that the quality of national accounts data in the PRC is probably higher than in many other countries in transition from a planned to a market economy.

The topic for this paper is data on the household level. Although some international recommendations for this field exist, comparability of information across countries is lower than for national accounts data. The relatively low degree of international exchange in the field of assembling household data makes it difficult to compare quality in household data across countries. Based on our somewhat limited knowledge, we have the impression that household data collected by NBS for China compares favorably with household data collected for countries formerly belonging to the Soviet Union. Therefore our opinion is
that statistical information collected and/or published by NBS should be taken
seriously, but not uncritically.

The NBS regularly publishes information on household income in its statistical
yearbooks. For example in *China Statistical Yearbook 2002*, one finds several
tables on income and expenditures of urban and rural households (Chapter
10). Summaries of information on household income and poverty published by
NBS are often used by researchers. See for example Yao (2000) who assessed the
development of poverty in rural China taking the published figures as a starting
point only; he did not accept them uncritically. Sometimes researchers ask the
NBS to prepare tables for their research task, see Ravallion and Chen (2003).
In still other cases researchers have been given access to the microdata collected
from households surveyed by the NBS.

When seeking contact with the NBS the researcher has to consider the size
of the research budget at his or her disposal. Needless to say, the more services
the researcher wants from the NBS, the higher the price demanded. Some rules
of thumb are self-evident. For example, collecting new data is more costly than
gaining access to already collected data, and the price increases by the number of
units covered. Equally important, however, is that how the researcher approaches
the NBS can affect the price significantly. For cost saving it is advisable to
establish direct contact with the survey team responsible for the product, rather
than approaching the international department of the NBS.

In China there are the Rural Household Survey and the Urban Household
Survey, not one survey for all of the country. First to mention among users of
microdata from the rural household survey are researchers from the World Bank
who created a database from the information gathered by the NBS. It consists of
the complete microdata collected in Guangdong, Guangxi, Guizhou and Yunnan
and refers to the period 1985–1990, covering approximately 10,000 households
each year. Due to the sampling design, most of the households are in the sample
each year. Constituting a panel it is possible to use this data for addressing many
research questions and by now many analyses have been published based on the
data.2

Other analyses of Chinese households and their behavior are based on the
official data generated from smaller geographical areas. Starting with rural
China, there are studies using information from two provinces collected for
a number of years, such as Tsui (1998) who studied Guangdong and Sichuan
from 1986, 1988, 1992 and 1994. While these two studies limit their analysis
to the cross-section approach, McCulloch and Calandrino (2003) followed
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3,311 households over 1991–1995 in order to analyze poverty dynamics in rural Sichuan. Still another design is found in Kung and Lee (2001) who analyzed the data referring to 1993 obtained from 400 households located in two counties, one in Sichuan and one in Hunan.

We now turn to the studies using data from the Urban Household Survey. The design of choosing two provinces is represented by Aaberge and Li (1997) and Aaberge and Yu (2001); both use data from Sichuan and Liaoning in the period 1986–1990, the first focusing on income and the other on savings. Coady and Wang (2000) also used the data from Liaoning over 1986–1990 to study earnings. At the other extreme regarding the number of provinces covered is Fang et al (2002) which uses data from 28 provinces. Deriving a subsample by taking the data from one city in each province, these researchers worked with information from 3,600 households in 1992 and each of the years from 1994–1998 to study urban poverty.

The strategy of basing research on microdata collected by a statistical bureau is attractive from several perspectives. Statistical bureaus typically have a long-term commitment to their work leading them to be experts at all stages of the data gathering process: sampling, interviewing, coding, data entry and data cleaning. Therefore in these areas they have an advantage over individual researchers. On the other hand statistical bureaus are typically not equipped with departments that analyze the data and follow the research literature. This means that in many Western countries there is a sort of division of labor between statistical bureaus and researchers at universities; the statistical bureaus collect the data and the data analysis is usually conducted by people at the universities. Is such a division of labor something to strive for in the context of the People’s Republic of China?

Trying to answer this leads to further questions. There are the issues of data accuracy and quality. To have a view of this, one needs to be familiar with details of sampling, questionnaires, the data collecting process, data entry and data cleaning. We will discuss these issues below.

Starting with design, it should be mentioned that China has two systems for collecting data from households; one for its rural regions, another for its urban regions. This reflects the different realities faced by rural and urban households. Although highly understandable, such an approach means that straightforward comparisons across rural and urban households are not possible from the data collected. Therefore, it is difficult to assess the income gap between rural and urban China as well as income inequality in China as a whole.

When describing how sampling is designed for the Rural Household Survey and the Urban Household Survey, one problem (as discussed above) is lack of
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clear documentation. Another problem is that sampling procedures have changed over time, meaning that knowledge from one period need not necessarily be useful for another. China Statistical Yearbook 2002 provides a description of how the surveys were made on page 318. More information of the Rural Household Income survey can be found in Chen and Ravallion (1996: 26–31). The design of the Urban Household Survey is discussed by Gibson et al (2003), and Park and Wang (2001) discuss in detail how this information is used to obtain the official numbers on poverty in urban China. See also Bramall (2001) who assesses the quality of China’s household surveys. Our description here is also based on our interviews with officials as well as on our experience with cooperating with them.

Households sampled in the Urban Household Survey are drawn from a large sample frame consisting only of registered households in urban China. A large problem with the survey is that rural migrants without urban hukou (household registration) living in urban areas are not included in the survey. As many rural-to-urban migrants have incomes lower than households with urban hukou, income inequality as well as poverty in urban China would be underestimated if data were used from the Urban Household Income Survey. The importance of this bias depends on how large the population of rural-to-urban migrants is and on the income situation of migrants in relation to the registered population. Many observers seem to think that both of these circumstances are of substantial magnitude, but precise estimates are not available.

How are the respondents sampled in the recent surveys? Starting with the Urban Household Survey, a two-stage stratified systematic random sampling scheme is applied. At the first stage, cities and counties are classified into five categories by population size. Then they are grouped into six administrative regions. In each administrative region the cities and counties are arranged in the order of average wages of staff and workers in the urban areas. Based on this classification, cities and counties are selected by a systematic sampling scheme. At the second stage, sample households are selected. This results in 25,000 households in 226 cities and counties selected by the NBS. Local statistical authorities in these cities and counties select additional households creating an urban sample of 40,000 households.

Households selected for the Urban Household Survey remain for a period of three years after which they are rotated out. When included in the survey they are visited by enumerators who ask questions and assist in book-keeping. The household has to record all of its income and expenditures in a very detailed manner. For their trouble, household members receive a small compensation.
It is becoming increasingly difficult to recruit households for the urban survey. Rich households, with high opportunity costs of keeping records, are more likely to avoid being in the survey.

Households providing information for the Rural Household Survey are selected in a multistage procedure as well. Our understanding of the procedure is that it leads to the information being representative at the level of province, as for the Urban Household Survey. The sampling scheme means that counties are chosen after ordering all counties in a province according to average income, and choosing villages in each county follows the same method. In each village 10 households are typically chosen. At present 67,000 households from 7,100 villages are included in the survey. A household is included in the survey for a period of five years after which it is rotated out. The rural households are also asked to keep detailed records of their expenditures as well as to provide information on their income. A total of nearly 10,000 assistant enumerators are involved in helping the households keep good accounts and in checking the information.

Both the rural and urban household surveys collect very detailed information on the consumption expenditures of the household. For instance, expenditures for food, clothing, transportation, communication, medical care, housing, and schooling can be found in the data published by the NBS. There are a number of questions on various income components in both the urban and rural income surveys, ranging from wage income of individuals to property income of households. Traditionally some income components such as in-kind housing subsidies, in-kind income, and imputed rent of private housing, have not been accounted for in the surveys. If these income components were to be more equally distributed, their exclusion would lead to overestimation of income inequality.

Our understanding is that the income concept adopted by the NBS when preparing the published tables has been broadened over time; a fact which should be regarded as progress. However, the changed income concept has a side effect on comparisons over time. Some of the recorded increase in average income is not real but due to movement towards an improved definition of disposable income. Such a change also affects estimates of the development of poverty which means that to some extent a reported decrease in poverty is due to changed income definition, not what is happening in reality.

In the urban survey information on earnings is obtained for each household member while in the rural survey it is reported as a total. While the rural survey assembles information on the demographic characteristics and the education of each household member, it does not put earnings questions to each household
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member. The survey is thus not designed to study gender differences in economic activity.

Turning finally to the availability of data for researchers, this seems to have been handled differently from case to case. Researchers actually have many options when it comes to finding a data supplier. To name one, there are the central urban and rural survey teams. China has 31 provinces, autonomous regions and metropolises and each has its own rural and urban survey team. Therefore, there are an additional 60 authorities to negotiate with at the sub-central level. The description above shows that many researchers have been able to obtain data from one province, while some have gained access to data from two provinces. In the published articles there are few examples of studies based on data from the official system collected in more than two provinces. There are the studies based on the databank from four rural provinces in southern China constructed by researchers at the World Bank, and Fang et al (2002) covers almost all provinces in urban China – but only one city in each province.

THE COMBINED STRATEGY

The third strategy for obtaining data on household income and earnings of workers is to combine the two approaches mentioned above. This strategy enables researchers to be active in developing instruments for the survey while being able to sample households out of the larger sample used by the National Bureau of Statistics. The NBS carries out the fieldwork and data entry. The output from the NBS is a dataset which might need some additional cleaning. Such an approach is attractive as it allows for data covering large parts of China, while permitting researchers to obtain information which better corresponds to the theoretical concepts they work with.

There are now several publications on Chinese household data collected with this approach. It is interesting that several of these publications also have counterparts translated and published in Chinese. The background for this is co-operation between researchers in China and in other countries at all stages of the project.

There are now four surveys that have been conducted using the combined strategy. The surveys of 1988, 1995 and 2002 cover rural and urban China, while the 1999 survey concentrates on urban China and emphasizes unemployment and other issues related to the labor market. With the exception of Appleton et al (2002) and Xue and Wei (2003; forthcoming), who use the 1999 survey,
the publications listed above refer to the surveys of 1988 or 1995 or both. The fieldwork for the 2002 survey has just been finished and publications from it will appear in the future. The discussion here will concentrate on the surveys of 1988, 1995 and 2002.

One motivation for the 1988 survey was to work with more defendable income concepts than those used in the official statistics. Another motivation was to have access to microdata for China as a whole. All the surveys of 1988, 1995 and 2002 used subsamples drawn from the broader samples at the NBS. For the urban survey 10 provinces were drawn for the 1988 survey to represent the East, Central and West of China. The sample consists of 9,009 households and 31,827 individuals. For the urban survey of 1995 the same provinces were included, plus Sichuan. The sample consists of 6,931 households and 21,694 individuals, as the size was reduced in each province due to budget cuts. As Chongqing was added to the urban survey of 2002, the number of provinces is now 12. The survey includes a sample of 7,000 households with urban hukous and an additional sample of 2000 rural-to-urban migrant households.

The 1988 rural survey covers 28 provinces and consists of 10,258 households and 51,352 individuals. Due to budget cuts nine of these provinces were not retained in the 1995 survey which has 7,998 households and 34,739 individuals. The same provinces as in the 1995 survey appear in the 2002 survey, which also includes Guangxi, which was covered in 1988 but not in 1995, and Xinjiang which was missed in the previous two surveys. The 2002 survey has a sample size of 9,200 households. A novelty of the 2002 rural survey is that village questionnaires to officials in the villages were implemented. The questionnaire contains questions regarding geographical environment, infrastructure, population and labor force, economic conditions, taxation and public finance, village management and public services, village governance, economic and social network, and social and historical characteristics.

The combined strategy as it has been practiced has several attractive features. For example, by comparing the results from the multi-year surveys, changes over time can be described and analysed. However, such comparisons cannot give the full picture as data have not been collected for each year; when an increase in income inequality is observed we do not know if the increase appeared continuously between the years or not. Note also that while comparisons over time can be made with samples drawn in the same manner, they do not relate to the same households and cannot therefore be informative on over-time variation at the household level.
With the rapid growth of the Chinese economy the empirical literature on household income and behavior is growing at an astounding pace. In this paper we have surveyed data used by researchers, made groupings based on how data have been collected, and discussed the advantages and disadvantages of the different strategies. On the one hand there is the strategy of researchers initiating as well as monitoring all stages in the data-producing process. On the other hand there is the strategy of working with data already collected by the National Bureau of Statistics. While the first has the advantage of researchers being able to formulate questions and being in control of all stages in the data processing process, the second has the advantage of producing results applicable to a large territory.

We have further argued that combining the two strategies in collecting data has attractive features. Working with such a strategy, researchers can obtain data covering large parts of China; this enables them to address research questions not easily answered if choosing a strategy of collecting data without the cooperation of the National Bureau of Statistics. One can also claim that being involved in designing the questionnaires can provide information which can be more relevant compared to relying merely on that which is regularly collected by the NBS.

The combined strategy requires more interaction with people at the NBS than the other strategies discussed here. Effects may go beyond the research reports. If a dialogue is possible, it can affect the design of the official statistics and in turn the information published. Such influence might be rather valuable in that policymaking will be based on more accurate information. If we were to mention one area in which official household income statistics in China are in urgent need of improvement it would be the coverage of rural migrants living in urban areas. Although there seem to have been some improvements recently, rural migrants are not usually seen in the household statistics although their proportion of the urban population is great.

The advantages we see with the combined strategy do not mean to imply that the practice of researcher-initiated studies should end. On the contrary, the comparative advantage for such a strategy should be large in terms of asking questions on phenomena a public agency has difficulties in asking as well as asking questions on attitudes.
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NOTES

1 This is a selective survey. Not including studies published before 1998 means we leave out previous work such as Hussain et al (1994), Rozelle (1994), Cheng (1996), Meng and Miller (1995) and Meng and Kid (1997). We also leave out monographs such as Sato (2003).


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Chapter 9

INTERVIEWING CHINESE PEOPLE: FROM HIGH-LEVEL OFFICIALS TO THE UNEMPLOYED

Dorothy J. Solinger

I fondly recall my maiden venture in walking and talking ‘in the field.’ It was December 1978 and, having completed a dissertation some four years earlier that had relied entirely on old Chinese newspapers stashed in an American library, I had been hard at work on a book on Chinese socialist commerce for about two years. I was a member of a delegation of professors from the University of Pittsburgh, and we traveled to seven different cities, among them Beijing (where I entered a shop and carried out an informal interview), Changsha (where I eavesdropped on state commercial cadres and tiptoed around a tiny outdoor stack of vegetables then illicitly on sale) and Tianjin (where I saw an official wall poster describing forbidden trading activities, especially the illegal barter in ration coupons then quite prevalent in cities).

I knew the lingo of shops, retailing, and bureaucratic purchasing from interviews with former commercial workers that I had conducted in Hong Kong and from a reading of massive stacks of journal and newspaper articles from China. But I was not yet equipped to put together the intricate relationships among state sales agents and their bureaucratic superiors. I was aware that there were issues involved with rationing, but had not realized that at that present moment all sorts of connivances were enabling people who were not meant to have access to grain coupons to obtain them nonetheless. I had no idea precisely what the mechanics were when a middleman from a northern city arrived in the south to find that the structure of state companies varied with the nature of local output.

What was the hierarchy of authority and the internal organization in an ordinary state-owned hardware shop? How open were vegetable markets in the interior in the period just before the famous Eleventh Central Committee’s Third
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Plenum of 1978 that began to hint at economic liberalization? All these tidbits seemed delicious, forbidden secrets to me, but I was uneasy, not knowing how much trouble I could cause for anyone by asking about them. In the end I got my answers, but by no means did I acquire all the information by asking directly. I peered along city walls and noted circulars, listened in in stores to talk among cadres, and snuck around open markets to observe the paltry bits of produce on display, all the while noting the guarded, suspicious looks on the faces of the sellers. On that trip I just could not pluck up the courage to put potential interlocutors squarely on the spot. Still, I left the country with some morsels of data, and, even more, with a sense of the excitement one can feel just knowing that real people existed who could bring my research to life.

Except for a very brief tourist trip in 1979, it was nearly another five years before I tried again. By May 1983, when I first set foot on the streets of Wuhan – the town that was quickly to become my Chinese laojia (old home) – the scene had changed immensely. The people were friendly and open, the markets lush with produce, and no signs against wrong marketing practices preached menacingly from the city’s signboards. By then the only warning posted consisted of rules for out-of-towners who needed licenses for doing their business in town. That time and during my following three visits (May 1984, Autumn 1984, May 1985) my subject was industrial policy, however, and it would not be enough just to stroll about and scrutinize covertly.

Instead, I needed to speak with officials in the city – in the comprehensive economic organs, in the sectoral bureaus and also in the enterprises – in order to find out how central policy was interpreted and implemented. Ideally, I needed too to understand how different bureaus each adopted their own distinctive strategies in accord with their sector’s place in the national plan of the time. It helped, I should note, that the policy I was there to study – the regime’s effort at ‘economic readjustment’ from 1979 to 1982, aimed at shifting much of industrial investment from the heavy to the light sectors – was one that was already finished. That the initiative was no longer in progress meant that, for the most part, this program was not overly politically sensitive nor was it particularly controversial by the time of my interviews. By 1984, that is, there was not too much reticence about discussing failed tactics, though by no means was there total frankness, either. Still, it definitely helped to be able to relate my queries to the past and not the present.

Thereafter, having tasted the treat of informal exchanges with local leaders, in subsequent years throughout the 1980s I took an interest in many aspects of the then evolving local economic policies of reform and their execution. I wanted to
explore everything from government-managed bankruptcies and state-arranged mergers to Wuhan’s quarrels with Hubei province (of which it is the capital) and onto the larger-scale inter-provincial regional planning along the Yangzi. Again, only local bureaucrats and factory managers could fill me in, though I also got to meet with scholars in their capacity as members of the official policy consultation committee that had just been created by Wuhan’s mayor, then (now-Politburo member) Wu Guanzheng.³

My next project in some ways broke very new ground for me. In investigating the treatment and plight of the ‘floating population’ (liudong renkou, the nickname bestowed on farmers migrating into the municipalities) it was necessary for me to converse with local officials again, but this would be insufficient. This time I would have to confront and win the confidence of the newly urbanizing mobile farmers themselves, those who had left their country homes and were sojourning in cities to make their livelihood. This, of course, would require the acquisition and development of a new set of contacts and skills, and was in many ways the most exciting and involving of the interviews I had conducted up to that time.⁴

Finally, over the past six years my current project has concerned the ongoing process of laying off state-employed workers. This is a program that has been advocated by the central state since 1996 in the course of its effort to modernize China’s enterprise system by winnowing out the weaker workers and plants. Again, as in my last project, relevant informants have had to include both those responsible for carrying out the basic contours of this undertaking in the localities, but also the targets themselves, the xiagang (literally, off-post) workers and staff. I even had occasion during this piece of work to meet with leaders in the Ministry of Labour and Social Security in Beijing, along with many top scholars, also based in the national capital. In each of my last four researches Wuhan was always my most comfortable, accessible base, but I made forays into Harbin, Shenyang, Tianjin, Guangzhou, Nanjing, Beijing, Lanzhou, and Shanghai on various occasions to check out the variations.

A number of questions attend this array of disparate settings, topics, and types of interview subjects. In what follows I discuss the larger issues of preparation and access (chiefly, getting permission to perform the work); finding and connecting with subjects; and gaining assistance. I go on to describe how I have teased out data and attitude from my conversation partners when I sense the speakers are unlikely to provide such material voluntarily or readily. I also recount some of the strategies I have used when problems in these areas became insurmountable; and, finally, address matters of sensitivity and secrecy, including how I have figured out that such ethical issues are present, and how I have attempted to handle them.

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Two institutions in particular were absolutely essential to my research over the years. The first of these was the foreign office of Wuhan city (shi waiban). My ability to appeal for its staff’s assistance began in a special way. In 1982, at the suggestion of Jimmy Hsieh, the Chinese geographer at the University of Pittsburgh, my then-home institution, the University approached Wuhan to institute ‘sister city’ relations between the two cities and their two home universities. A delegation that included the sister-in-law of one of China’s very highest state/Party officials was then the head of Wuhan’s foreign office and led the group, a person with a keen interest in anything ‘abroad.’ With the way paved by this bond, I found a hearty and genial reception in the city for my early research in China, and the tie has almost always been available for me to draw upon ever since.

Preceding every one of my visits from 1983 through 1992 (of which there were eight), I simply announced that I was coming to China, the nature of the research I hoped to conduct, the bureaus at which I wished to meet with officials, and the questions I intended to put to them, and the deed was granted. In the years since 1992 the personnel at the city’s foreign office has been altered several times, but in my eight subsequent visits I continued to be assisted there more times than not. In 1994 I was engaged in a consultancy with the World Bank to study the contribution of urban governments to economic growth. For a reason never explained to me, the Wuhan foreign office, after first agreeing to help, later declined to be of service. I quickly realized, however, that the Bank’s bureaucracy within China surely had a resident office in Wuhan, and indeed that organ was able to arrange all the interviews I required.

The other institution that paved my way was the organization set up to intermediate scholarly exchanges between China and the U.S., then called the Committee on Scholarly Communication with the People’s Republic of China (CSCPRC). Both in 1984 and in 1992 this association granted me fellowships, along with which came valuable connections and permissions from responsible bureaucracies in cities where I had no contacts of my own that allowed me to carry out my work. Other introductions came, twice, from the U.S. government’s consular officials in Shenyang, once from the consulate in Guangzhou, and once from two China studies colleagues who had previously conducted research in Tianjin and who had retained friendships with scholars at the city Social Science Academy there. Over the past decade, however, most of my trips have been preceded, less formally, not by linking with official organs but by entering into

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communications with Chinese scholars working on topics close to my own, or by getting help from personnel at the Ford Foundation’s central China office in Beijing who had contacts in the cities I planned to visit. As a general statement, I have always preceded any research journey by first contacting people who can talk with me or who can introduce me to others more deeply involved in the issues of my concern. In a few cases in the past few years this simply meant asking friends in Wuhan to provide access either to private informants or to their own schoolmates who were then working in the city government.

Even with the assistance of the CSCPRC in paving the way, however, I had one very difficult experience, in Harbin in July 1991. Although that Committee had carefully negotiated a placement for me with the city’s foreign office and its social science academy, the timing was exceedingly poor. Harbin was then a particularly conservative place, hostile to Americans in the wake of China’s post-Tiananmen campaign to stymie U.S. efforts, as the Chinese leadership perceived them, to trigger a ‘peaceful evolution’ of China. Bureaucrats in these units were loath to impart what the city political elite considered classified or covert information of any kind. While I was scheduled to spend a month in the city, at least half of that time had to be given over to complex negotiations over my questionnaire, my purposes, and the identity of the bureaus I would be allowed to visit. Throughout this period of haggling – and, indeed, after it too, for all efforts at discussion failed totally – I availed myself daily of the city library, where I discovered and perused a wealth of journals I had never seen or heard about before.

Similarly, in Guangzhou in 1998, when university professors whose names I had been given by a colleague failed upon my arrival to make good on their earlier promises of help, I turned instead to the giant ‘book city’ (shucheng), or bookstore, the city then boasted and spent my days reading at my hotel desk instead of putting questions to cadres. The moral here is to be as prepared as possible, but to be able to adapt quickly, if need be, once on the scene, in order to lose as little time while in the field as possible.

Another maxim is to try to draw upon any relationship one might have with any person willing to be of help in one’s ploy to meet potential subjects. A former undergraduate student of mine spent two years at Nanjing University when I was working on the ‘floating population’. Because she was there, I decided to do field work on the migrants in that city, knowing she could ask her classmates, many of whom were familiar with local dialects, to accompany me on my street interviews and interpret the generally non-Mandarin speaking peasants for me. Four young college students provided invaluable help for me that way.
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A similar stroke of luck befell me in February 1992, as I prepared for my time in the field to study the migrants. At that point a professor I had met while working on Wuhan’s market reform policies in the mid-1980s had agreed to select two graduate students to act as my assistants in Wuhan. A few months later, when conservative politicians were blocking Deng Xiaoping’s program of opening up and marketization, the professor changed his mind and declined to see me. One of the students, however, met me secretly at my hotel upon my arrival and we carried out the work. I have never revealed his name.

Retaining old contacts, however trivial they may seem, has worked to my advantage. In 1999 when I wanted to meet with recently discharged former workers, I made contact with all the people with whom I had developed any sort of personal connection in Wuhan over a period of, by then, more than a decade and a half. Through these people I was able to meet the discharged wife of a colleague of a colleague, the laid-off co-workers of my former hotel waitress, the poverty-stricken wards being helped by the work unit of a local urban chronicler I had met in 1990, the neighbors of a friend of a UC Irvine graduate student, and the housemaid of a novelist I knew, among others. A UCI graduate student’s former teacher’s daughter acted as my informal interpreter in dealing with people who spoke in the Wuhan dialect.

There was a trick many foreign scholars employed in the 1980s and early 1990s when they set about seeking permission for undertaking their research projects. This was to describe one’s projects and one’s interests in the most benign terms possible – without, of course, thereby shutting off the opportunity to meet the people most in a position to provide the material one needed. Thus, when commencing my inquiry into industrial policy I explained that my then-hometown, Pittsburgh, had many similar problems of industrial restructuring as did industrial cities in China, and that I wanted to learn about China’s positive experiences. When I set out to examine the situation of the movement of rural people into the municipalities, I praised the Chinese for, as of the late 1980s when I started the project, having so few homeless people living on the streets, in contrast to many American cities. My purposes on both these occasions must have seemed to the Chinese who had to approve my project to be harmless and even flattering; perhaps for reasons such as this I never had trouble obtaining permission to engage in any of my research projects.
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THE INTERVIEW

I have never gone into the ‘field’ without having done a great deal of prior reading. There are a number of reasons for this. In the first place, reading of all sorts helps one put the issue at hand into a larger context, whether historical, comparative, or theoretical. It also helps me to acquaint myself with the jargon, practices and purposes current and germane to the topic I hoped to investigate. If one knows, for instance, that a particular meeting was held at which critical decisions were taken, that specific bureaucracies or geographical entities or echelons of administration have experienced tensions or controversies among themselves in the past, or that city officials believe that certain tasks are especially irksome, one is then in a position to uncover more information by revealing what one already knows than if one is forced to put merely vague, generalized inquiries to one’s subject.

Secondly, most of the times when I went to China to do research through formal channels a city waiban would instruct me ahead of time to present him or her with a set of the questions I planned to ask. In order to organize such a list I needed to know a great many of the relevant details of my research subject, especially its bureaucratic intricacies, the identity of the particular units directly involved, and any potential pitfalls. Were I not able to convince my future informants that I knew some of what they knew, they would probably have brought fewer pieces of data to our meeting.

Here I recall, during my study of industrial policy, having read about the Wuhan machinery bureau’s quandary in the early 1980’s campaign to ‘readjust the economy’ away from its habitual emphasis on heavy industry. The factories under the management of this bureau had been told to cut back on their production and were given greatly reduced state investment. Many plants, workers and technicians had even to learn how to turn out products entirely different from those they had been fashioning for several decades. In my interview with the bureau chiefs, however, no one dared to bring up the topic of recalcitrance. The ice broke rapidly when I went through my notes and referred to a specific conference called by the city’s party committee at which machine-building bureau heads were chastised for their evasive tactics. So warm became the temperature that I almost imagined one or two of my informants might break out in tears, as they explained to me how very difficult it had been to corral their subordinate firms’ cadres into compliance. They also bemoaned how much they all had suffered under pressures from the bureaucrats over their heads fundamentally to transform their production techniques.
On another, later occasion when I was studying the migrants, I spent nearly two hours feigning patience, while I heard of all of the proper, official procedures and bureaucratic agencies involved in dealing with issues of birth control in the city. Obviously, it seemed, the members of this bureau sent to talk with me had no intention of discussing my subject, which was the problems entailed in handling the city’s migrant population. Just at the very end of the interview, I remember saying, with compassion if out of the blue, ‘If only it weren’t for the floating population, your work would be much easier.’ That released the floodgates that had blocked off the anger and frustration that were attending my informants’ daily work. And with that, everyone present chimed in together to relate one annoying incident or obstacle after another that got in the way of their job. So I used the understanding about the difficulties of this agency’s responsibilities (information that I had already acquired from reading Chinese newspapers and journals) to offer a sympathetic comment that led to an opening.

In 1987, I conducted interviews aimed at finding out from the city’s reform commission about Wuhan’s behavior as the newly significant marketing and transport ‘central city of Central China’ once economic reforms had restored its old, pre-takeover historical role as an entrepôt. Before arriving, I was already cognizant of the stresses that characterized its competitive, often even insubordinate, liaison with Hubei, the province of which it was the capital city, and also of its rivalrous relations with another major metropolis in the interior, Chongqing. These bits of information were at the forefront of my mind as I queried local scholars and officials about how Wuhan’s post-1984 status as a ‘jihua danlie’ (separate line item within the central plan) city was working out. My prior knowledge about the difficulties – rivalrous relations among relevant cities and provinces – of establishing a central Yangzi cooperation zone likewise informed my conversations. Officials seemed to feel they were talking to an insider and became remarkably frank.

One last example illustrating this technique comes from my 1998 interview in Beijing with four officials at the Ministry of Labour and Social Security (an interview arranged for me by an applicant to our graduate program at UC Irvine who had offered in his application to assist me in any way he could3). As per usual, I was treated to platitudinous accounts of the work of the Ministry and how it was proceeding to address all the problems of China’s working people. Suddenly I commented on how arduous the task of building up a brand-new social security framework must be. ‘We can’t even hope to solve that one before the year 2020,’ one of them admitted in reply. ‘And that’s only if Zhu Rongji
could remain our Premier,’ he went on to allow. And that was when the interview finally got down to business.

In these and many similar incidents people felt much freer to speak frankly to me so long as I appeared already to be privy to their private matters. Informants could see that I knew enough to ask the telling questions and in such instances were more often than not willing to give at least part of the answer. Thus, one can clearly enhance the interview by being well informed ahead of time; such prior knowledge forms a springboard for diving much deeper.

On the other hand, when further details were what I needed, I suddenly shifted my stance: then I became ignorant, naive and obtuse, putting query after query to my interviewees, until all the trifles of the matter, the methods, and the numbers involved were clearly laid out before me. Indeed, during my open-ended interview sessions, it often has seemed as if every response I am given leads me to inquire more intensely about additional behind-the-scenes mechanisms and strategies. In this way I often leave an interview having discovered data I had never known existed when I began the meeting. In sum, as a shorthand formula, my tactic has always been what one might describe as a Daoist-type dictum: appear at once knowledgeable but ignorant, knowing and not knowing.

FINDING AND CONNECTING WITH SUBJECTS – AND WHAT TO DO WHEN YOU CAN’T

When I need to locate less well-placed, more anonymous informants, such as urban migrants and laid-off workers, naturally my strategies have to switch. For these types of subjects, both my current and my previous projects have employed a sort of selection process once termed ‘guerrilla interviewing’ by the sociologist Thomas Gold (1989). This is a form of picking persons for research interviews by spontaneously engaging in seemingly idle, friendly conversation people at work on the streets. It was easy to identify the rural migrants on the city sidewalks, and most of them, grateful to be resident in an urban area, were proud of their new lives and happy to explain them. In several months in 1992 and 1994 I spent many hours along the thoroughfares of a number of cities speaking to people I recognized – either by their occupations, their accents, or their clothing – to be from the countryside.

Later, in my yearly visits to Chinese cities beginning in 1998 while at work on my project on unemployment, I sought out middle-aged, generally downcast labourers along the road who appeared to be urban residents and encouraged
them to explain how they had landed on the pavement. Occasionally, especially after the U.S. bombing of the Belgrade embassy in 1999, some of these manual workers were afraid or unwilling to talk to a foreign face. But on the whole, what seemed to be the key was my show of empathy and support for their position. This display of understanding almost invariably softened their reserve. On one occasion, two jobless workers thanked me most profusely for caring enough about them to want to know their stories, since, as they felt, ‘The Party and the government and the trade union didn’t ask us about our situation.’

An obvious question that might arise in the reader’s mind at this point concerns getting such people to talk openly with me. In the current project, when I have met my subjects through mutual acquaintances reserve melts quickly. Even more, when our site for communication is someone’s home or the restaurant of my hotel, the feeling of relative safety rapidly loosens the tongue. I usually begin such interviews by referring to our common friend or associate, and what I had learned about the subject ahead of time. When the setting is such as to permit a longish talk, I start off by ‘shooting the breeze’ for a while, bringing up topics such as how I know their friend or relative, or how I learned about the person I am addressing. If I already knew that the informant had a special situation of some sort, I took the conversation around to that.

If on the other hand (and, as was much more frequently the case) I encountered the person or persons at work on the street, either selling trinkets or doing repairs, driving in a taxicab or in the course of mounting a pedicab, or hawking at a market stall, the point of entry into the conversation had to be different. On the occasions when I intervened in a person’s work, I would generally begin with a query such as, ‘May I speak with you?’ or ‘Excuse me, do you have a few minutes to talk now?’ I then would explain what my interest was and, ideally, present the person with my name card to demonstrate that I was a scholar. Following such an establishment of trust, it then became possible to ask simple questions, such as ‘Are you from the countryside?’ or ‘Are you a laid-off worker?’, ‘Where are you from?’ ‘How long have you been here?’, etc. Soon the words flowed more or less naturally, as I appeared unassuming, sympathetic and genuinely interested and supportive. When in Wuhan, I would usually let the interviewee know how often I had been in Wuhan, over how many years and how much I like the city.

I also make certain those speaking with me understand the nature of my interest in learning from them – i.e., that I am a professor with a special interest in the problems attending urban/market reform, and that my research project is about such-and-such that directly pertains to their own daily life problems and experiences. Thus it becomes immediately clear to them just why I have targeted
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them for talk. At the same time, by identifying myself and my profession, I manage quickly to dispel any fears they could entertain that I might be planning to write news articles on their plight or, worse yet, inform on them to either their or my own government (indeed, such were the fears of some of my subjects). On those occasions when I can calculate ahead of time that it is likely that informants (especially those from the rural areas) will be using a local dialect, I have engaged local, educated Chinese to accompany me. I then pose my questions in Mandarin to my partner, who then translates the query into local dialect. Following the interview, my assistant and I go off somewhere and quickly share what we both recall from the conversation and I write the words into my notes.

Speaking with scholars and officials is, of course, a different matter. But at the same time, there is a commonality: every interlocutor – whether a high-ranking official in the central government or a migrant meandering along the alleyways – needs to be shown that I am not only very aware of his/her situation, but that I commiserate with its complications and hardships. It appears critical to display the appropriate emotion no matter who one’s subject may be. But for people with specific positions in bureaucracies or with scholarly expertise, the first issue is to find out the nature of their own work, while quickly establishing my own credentials and credibility. Exchanging name cards is de rigueur. In addition, I bring along any recently published articles of my own to distribute, especially if I have any that might be of interest to them: time has long passed since the days of the early 1980s when ball point pens were the ideal gift. But there can be awkward side-effects to the offer of a piece of writing. Once, my distribution to an older cadre of something I had written led the recipient vainly to attempt to determine which way to hold up the thing!

Once our mutual authority has been thus cemented, I proceed to describe the nature of my current research, as soon as possible letting them know how knowledgeable I already am on the topic so that they understand what the starting point in their own speech should be. As they speak, I frequently stop them, raising every small question that comes to mind, for two reasons. First, there are many things they will take for granted that are unknown to me unless I ask; and second, I try to keep in mind the inevitable backward looks I will inevitably be taking later on in the day, as I review my notes after the meeting. For at that point there will no longer be any chance to fill in the blanks.

During all of my interviews, I never used tape recorders, but only wrote notes. When the speaker used a word I did not know, I would ask that s/he write it down for me. I often ask for repetition if I think I may have missed something important. I believe strongly that I must understand and catch every word and
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thought on the spot. Listening to a transcript later on will not help me if I have not understood the first time something was spoken, I reason. I have consequently always asked my subjects from the start if they would mind my taking notes. No one has refused to date, especially when the people on the streets remain nameless. In the case of scholars, I inquire as to whether or not they would mind my quoting them directly, and whether or not I may use their name if I do.

SENSITIVITY AND SECRECY: HANDLING ETHICAL ISSUES AND LISTENING BETWEEN THE SOUNDS

One of my largest concerns when interviewing is that I do not make my informants feel uncomfortable. For this reason I do not try to compel them to give me information that runs directly contrary to Party policy or government directive, nor do I bluntly ask questions that would put them on the spot. Instead, I push, but only so far as it appears they are willing to go. When I am convinced that the subject is truly out of bounds for that speaker (or more generally out of line), I let further inquiry pass. Sometimes the very discovery that the answer will not be given can alert one to the sensitivity of the issue, and, later and in a different setting with a new informant, the initial lack of reply might form the basis for further inquiries. Perhaps this stance has won confidence for me, such that I have never had trouble staying on in or returning to a place, nor has anyone I have spoken with in the past ever refrained from meeting again with me.

Even without pushing the interlocutor one may glean concealed fragments of information through careful watching and listening. During my study of industrial policy, I noticed that several different speakers alluded to one plant in particular where, in the course of shifting its output from boilers to electroplating bicycles, there had been some sort of awful outcome, resulting in zero production for a full two years. I only learned of the extent of the disaster by following tiny leads and particles of the picture from one interview to another, expanding my knowledge of the events bit by bit as I let each later subject see how much I already knew (Solinger 1991: 82–83).

On another occasion, in 2000 I was intent upon figuring out the total number of laid-off workers in Wuhan, and also in determining the extent to which functionaries on the ground trusted the totals being spewed out by official statisticians. Paying close attention when a trade union cadre answered my queries as to how many people had lost their jobs, I observed him quietly but emphatically qualify his answer: When he carefully preceded his reply with
the words ‘According to government statistics,’ and a pregnant glance, I decided that he probably had reason to wonder if the count were fully accurate. Tours of ‘re-employment models’ (successful post-layoff ventures) also have afforded me many a chance to pry into the intricate inside stories about how the heroes were selected and by whom, and about the types of assistance they received – and from where – in order to achieve their marvels.

Ethical concerns enter especially in holding conversations with people who have been the subjects of sorry tales the content of which they are plainly hesitant to discuss with foreigners. Examples of such stories are those of migrants operating their micro businesses without licenses or laid-off workers complaining of the corrupt or arbitrary behavior of their former factory managers who had engineered their dismissals. In such cases I have met with the individuals in neutral, unidentifiable settings, such as a coffee shop or a park, and have assured them that I do not know nor wish to know their names. Ironically enough, one of my frankest interviews ever was held at night in a seemingly highly sensitive arena: it took place in the empty building of the local party committee’s editorial offices, where a friend of mine worked by day. Perhaps in such a seemingly protected setting the men being questioned abandoned their fears and poured out their feelings and their experiences quite frankly. Indeed, they even felt free to criticize the authoritarian form of government under which China is ruled, and to pine for a two-party, competitive electoral system.

Interviewing scholars is often one of the best ways to discover popular reactions and sticking points in the implementation of difficult programs. But here especially one must ascertain whether or not the scholar is willing to have his/her name in the notes. Bringing along papers of one’s own frequently leads to an exchange of research output, and, having acquired a Chinese scholar’s work, further correspondence about its content can lead to new opportunities for asking questions and gaining data. One more way of breaking through the invisible walls of secrecy that surround much Chinese material is to tease out gradually and circumspectly what the speaker feels s/he can reveal by tiptoeing, little by little, closer and closer to some crucial bit of information, testing the waters with each separate query.

Rather like scholars, but unlike migrants and discharged workers, officials are particularly in possession of information whose exposure could be dangerous to them. On several occasions when I knew that certain types of data were neibu (internal) and thus perhaps not to be disclosed, I found that subjects felt uncomfortable orally reciting the pertinent figures. They were, however, willing to have me copy down information they had in their own notebooks. Whether
this was because the room in which we were speaking was bugged was never explained to me. It is best to back away from an inquiry that the subject is quite purposely evading.

I have also learned, from my own error, that some city officials, no matter how well disposed or how much of like mind with one they may seem to be, will not conduct a formal interview in which policy issues are discussed unless the city foreign office has arranged it. It was clearly for this reason that informally inviting friendly, sympathetic trade union officials of this type (with whom I had previously spoken officially and exchanged Christmas cards) into the privacy of my hotel room did not yield any information of value.

CONCLUSION

Some may wonder about the research value of such random chats, in which the subjects are somewhat haphazardly selected; others may ask how many such interviews are sufficient to amount to a project completed. My own feeling is that in qualitatively-conducted research one ought to employ as many different sorts of research material and data as one can, including statistical data from yearbooks and specialized volumes, as well as that found in survey research by others reported in Chinese journals; official documents; internal journals; daily newspapers from China in both Chinese and English, whether official, semi-official, or the product of ‘reportage literature’; and Western media reports, in addition to secondary sources and interviews in China.

As for the number of such conversations to seek, the more the better! On numerous occasions I have gained insights and a sense of how things operate from talking informally to dozens of people of all strata in China; almost always these insights have informed my reading of other kinds of data and/or have been confirmed as I review material of different sorts. Most importantly, the nuances I have obtained from such encounters have helped me to identify and assess subterfuge and euphemisms on the printed page.

Speaking directly with individuals whose daily business carries with it the knowledge we seek but can find in no other place must be one of the truly exhilarating aspects of doing research on China. But to get the maximum payoff from the venture, it has been my own experience that four fundamental rules should be followed: (1) arrange appropriate approval for one’s project – and the necessary access – if officials are to be your subjects, before entering the country, but stay flexible once on the spot; (2) keep track of all the friendly people you
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meet, for one never knows in advance who might later be able to introduce suitable subjects; (3) in the course of the interview, seem to be both aware and uninitiated, first to appear to be an insider but then to make certain of all the fine points; (4) keep the subject’s safety at the center of your consciousness.

NOTES

1  The book was Solinger 1984.
2  The book that came out of this project was Solinger 1991.
3  The resulting book was Solinger 1993.
4  My book from that project was Solinger 1999.
5  He was not admitted to the program, but was nonetheless willing to be of help, and I took him up on that offer.
6  Interview in Wuhan, 18 August 2002.
7  My interviews began at the machine-building bureau, then at the first bureau of light industry in Wuhan in November 1984. The city’s Economic Commission filled in further details, including the hitherto unrevealed name of the hapless firm. It was not until a return visit the following May, when I requested to talk to people at the plant in question, that I was given the whole story. Immediately thereafter, however, all further opportunities for interviews with factory managers in other plants were cancelled and I had no choice but to terminate my stay in town on that occasion.

REFERENCES


Chapter 10

CONSULTANCY: A DIFFERENT GATE TO THE FIELD

Baogang He

Consultancy has emerged in Chinese studies in recent years as a new gate into the field. The aim of consultancy fieldwork is to work and engage with local actors, train local officials, and provide knowledge and resources for local projects. This type of fieldwork involves frequent trips to villages, towns and townships, or urban residential committees, and scholars and experts are required to attend work meetings on concrete policy and planning, which are quite different from academic workshops or conferences.

Consultancy is an interesting and exciting gate into the field and constitutes a form of fieldwork at least in the following three senses. First, consultancy provides opportunities to collect materials more quickly and efficiently. In conventional fieldwork, provision of data which researchers seek is seen as a favor offered by the informant. In consultancy fieldwork, scholars have a legitimate right to demand relevant material which is otherwise inaccessible. In the last few years, in my official capacity as a consultant I was able to collect more materials than I did in pure academic fieldwork. Second, benefiting from work meetings and the whole process of consultancy in the field, scholars will gain an insider’s knowledge of the policy-making process, and such knowledge can contribute to institutional design studies and democracy-promotion studies. Third, the consultancy fieldwork provides scholars with an opportunity to carry out quasi experiments in social sciences; that is, applying social sciences to local issues, and testing and developing social and political theories through the consultancy fieldwork.

This chapter aims to provide an insider’s perspective of consultancy fieldwork and reflects personal experience. It will consider how consultancy fieldwork benefits scholars’ research and how it sheds new light on some academic debates. It also critically examines the approaches and strategies of consultancy fieldwork, and provides tips for others. The chapter presents a scholar’s perspective, keeping
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...a critical distance from the official line of both the Chinese government and international donors.¹

The consultancy fieldwork I shall discuss in this paper is related to democracy-promotion projects. Since the middle of the 1990s, international organizations such as the United Nations Development Program (UNDP), the European Union (EU), the Ford Foundation, the National Endowment for Democracy (NED), and foreign countries including the United States, Denmark, Japan, Canada, Finland, Australia (Carothers 2000; Pastor 2000), and many others have developed a number of projects devoted to improving good governance and promoting local democracy in China.² These projects have become a new fast-growing ‘industry’ and made a significant contribution to good governance and local democratization (Shelley 2000). Despite the success and achievements of these projects, a lot of money was wasted, some funding failed to achieve its potential, inappropriate strategies met opposition from local governments, and misunderstandings between Western project managers and Chinese officials delayed the implementation of projects. There are many lessons to be learnt.

Invited by the Ministry of Civic Affairs (MoCA) of China, the UNDP, the Danish Ministry of Foreign Affairs, and a number of local governments in China I have made more than ten fieldwork trips in recent years to participate in several projects relating to good governance and local democracy in China, and I have played different roles including designer, reviewer, commentator and advisor. This involvement gives me an opportunity to reflect on consultancy fieldwork, and to consider a number of approaches and strategies to overcome the obstacles that projects often confront in China.

THE POLITICAL FRAMEWORK OF CONSULTANCY

As in conventional fieldwork, scholars and experts who engage in consultancy fieldwork need to pay special attention to a constantly changing political atmosphere as well as political regulations. China’s political development is moving very fast. Few projects escape the need for some revision and flexibility along the way. Back in 1993-4, an internal classified document warned against any funding for local political reform from foreign organizations that had a hidden agenda of ‘peaceful evolution’ to end the communist rule. At the same time, some foreign funding organizations were opposed to this sort of project in a Communist China. As Anne Thurston reviewed in 1999, ‘working in China may necessitate more compromise than some proponents of democratization might be willing...
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to make. Slow, incremental change is not enough, some would argue. China is too large and too complex for efforts by outsiders to make a difference. Political reform is a sham. Foreigners are being deceived. The party is illegitimate, beyond reform. Only the death of party rule and the establishment of a new democratic government is acceptable, some would say’ (Thurston 1999: 26).

A breakthrough was made in 1996 when the UNDP initiated a project to support the Chinese government’s efforts to promote grassroots self-governance and democratic elections in rural areas. The UNDP project had its own unique comparative advantages. Because of the UN’s neutrality, it met less criticism and opposition than the United States, and its funding for training was not interpreted as a plot of ‘peaceful evolution’. That was why the UNDP was the first to be allowed to conduct this kind of project. Because the UN is widely seen as ‘world government’, the UNDP project has enjoyed higher authority among the Chinese populace, thus constituting a powerful supporting force for Chinese political reforms.

For the Chinese government, the UNDP project opened a new channel for overcoming the problem of a funding shortage for training. From the successful example of the cooperation between the UNDP and the MoCA, some provincial officials have also successfully won foreign funding to set up democratic training programs at the various levels. Even some NGOs gain funding from foreign countries directly; Unirule Institute of Economics (Tianze), for example, received a grant entitled ‘Grassroots Democratic Training for Villagers’ from the Canadian Embassy in Beijing in September 2000.

The Chinese government takes necessary measures to control foreign funding. All political reform projects funded by foreign organizations must be approved by Chinese governments, and are likely to be investigated by the department of public security. Each project is required to obtain a formal approval by the department or office of foreign affairs. Officials in the department of foreign affairs often receive official notices and documents about regulations and rules. Wang Zhengyao, the former officer in charge of the UNDP program, explained this well: ‘Foreign donors should not be allowed to play a leading role or operate independently. They are not allowed to bypass the state to provide funding for local actors. The State should play a leading role in implementing foreign funded projects and control the process of foreign funding. The state is therefore able to ensure foreign funding to promote local stability and democracy.’

To discipline and monitor foreign organizations, the Chinese government closed down the office of one foundation in Beijing, when it supported a project which was sympathetic towards Tibetan independence in the 1990s. In 2003
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an internal document ordered Chinese governments and universities not to develop any collaboration with one American funding agency. In 2004 any visiting member from Taiwan should be accompanied by a host from a Chinese institution. Any foreign sponsored project in Tibet must be examined and approved by a joint working committee composed of officials from the party organization, the department of foreign affairs, and relevant host institutions.

HOST ORGANIZATION AND SITE

As in conventional fieldwork, researchers have to choose a site, select an issue to be studied and find a host institution. Sometimes there is scope for choice, sometimes little choice is available. For example when selecting townships as the site of experiment, one scholar suggested random sampling and a more rigid scientific study of the effect of election, but local politicians rejected random sampling, and selected the townships they believed would ensure that the experiment would be successful.

Going to the countryside, and particularly to the poor western regions, is the best option because there are now many projects in Beijing and other cosmopolitan cities. Nevertheless, there is an irony. Local governments in poor areas are keen to seek funding from international donors, and are eager to cooperate with them to develop some projects. However, these projects are likely to be a product of political mobilization rather than of a natural process in which interest structure is matured. As one county deputy Party secretary said, ‘It is easy to carry out a democratic experiment in a poor area, because people are poor, simple-minded and willing to listen.’

Many foundations and donors support a wide range of projects. For example, the National Democratic Institute in the United States focused on improving the Chinese people’s congress system, the International Republican Institute on political reform at the village level, the National People’s Congress, legal aid reform, judicial training, and village elections. A few big European grants focused on training. Apart from training, donors now are considering support for other projects, such as monitoring teams during village elections, post-election training programs, taxation systems and village self-governance, institutional management systems (specifically dealing with the tension between the party secretary and the elected village chief), and trial direct township elections.

The issues in which international donors are interested are not necessarily a local priority. Donors tend to look for projects that are making breakthroughs in
politic and applicable to other regions. In contrast, local officials are concerned with local benefits and local stability without political risk.

It is essential to find a Chinese host institution and persons who are committed to democratic values and good governance. The ministry of foreign affairs and the MoCA have promoted good governance and local democracy in China. While the former is interested in improving the international image of China’s record in human rights, the latter has a stake in maintaining social order through democratic participation in rural China.

To choose a host institution, one has to consider the advantages and disadvantages of the organization. Often there is a trade-off. Working with Chinese governments, one has the advantages of accessibility, maximal impact, and organizational support. But the disadvantages are bureaucratic machinations that can impair the donor’s effort, lack of autonomy, bureaucratic control over projects, the absence of the formation of an open opposition, the compromise of democratic principle, and having to work under the Party’s official definition of democracy. Some Chinese NGOs are more independent, friendly and cooperative, free from bureaucratic procedures, and willing to do things as requested by donors, and some of them may be able to maintain autonomy and create truly alternative types of political reform. Nevertheless, NGOs are less effective than Chinese governments when they implement training projects, and have limited access to the decision-making process. Indeed, some officials refused to attend meetings on local governance projects organized by one NGO in order to avoid any political risk. Both bureaucratic organizations and NGOs are at risk of wasting money. Government officials may use funding for other purposes, and members of NGOs have to make a living. Irregularities and money-grabbing tendencies exist amongst some NGOs.

The Chinese Communist Party (CCP) organization provides a new opportunity for foreign cooperation because the Party has recently introduced intra-party democracy reform. But foreign donors are reluctant to provide any funding to work with the Party. At the same time, the CCP itself is cautious not to develop close cooperation with foreign donors except on a party-to-party basis.

The reluctance of international donors to work with the CCP comes from ideological concerns. ‘How can we use tax-paid money to support the Chinese Communist Party!’, one senior program officer told me. One project document wrote clearly that the cooperative agency is the department of civic affairs, but the project manager knew well that the Party organization was in charge of the project. It seems that the policy-makers of international donors are not adaptive enough to Chinese reality.
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Realistically speaking, one has to work with the Party to carry out projects and to achieve a maximal impact. Today, the Party controls all governmental organizations. In recent years, it has moved away from the 1980s political reform principle that the party and government should be separated, and local party secretaries now tend to be appointed chairpersons of local People’s Congresses. Crucially, some local party members are indeed agents for local democracy, and an intra-party democracy, however limited, is one path towards Chinese local democracy. Indeed, when the author and Professor James Fishkin in 2004-5 negotiated an experiment with deliberative polling techniques in Wenling city, it was the town Party secretary who supported it while the town mayor was against it. The numerous encounters I have had with various Party secretaries at different levels reveal that the CCP has multiple facets. If we take an agent’s perspective, rather than a structural perception of the conservative and anti-democratic nature of the CCP, we will find that in some local districts in Shanghai, local Party secretaries want to transform the Party organization into a ‘charity’ organization which provides welfare to the poor. In some rural area, the rich are admitted into the local Party organization, which looks like a ‘corporate’ party. And in some villages, all adult villagers elect the village Party secretary and the organization seems to become a ‘villagers’ party’.

THE RELATION BETWEEN CONSULTANCY AND RESEARCH

Although aided by modern social sciences the consultancy fieldworkers have brought to their projects many untested and unhelpful assumptions about democratic transitions and political change. Consultancy fieldworkers may discover how little they really know about politics and political change in China and the fieldwork provides them with an opportunity to challenge their own presuppositions and ideas. The result of the consultancy fieldwork can be used to test and revise existing social science theories and to improve existing institutional design and democratization strategy. Below I would like to reflect on the dynamics between my consultancy and my ordinary academic research, and to show how consultancy related to democracy projects can improve our understanding of Chinese democratization and/or politics.

Rethinking path dependence

Effective democracy assistance requires an insight from social sciences and contemporary scholarship on democracy to design an institution that is not only
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democratic but also appropriate to local conditions. The democratic transition literature has generated a body of knowledge of the patterns, sequences and strategies of democratization, and is extremely valuable in that it enables Chinese to think about Chinese paths towards democracy. The use of institutionalism to work out a wise strategy is where the application of social sciences has been most successful.

A number of American organizations have acted as background advisers adopting a strategic posture in their democracy-promotion projects. Rather than challenging the CCP, they have chosen to focus on technical questions concerning electoral procedures. This was an incremental strategy, making minor and technical suggestions that reduce opposition and that do not challenge the CCP directly. It focused on procedures, helping to draft or improve procedures for nomination and screening. Assuming an institutional path-dependence approach, these organizations hope that an initial introduction of some procedural initiatives will lead to demands for more extensive changes.

This approach has contributed towards a significant improvement in procedures. Since 1993, the International Republican Institute (IRI) has assisted local electoral institutions to conduct legitimate, competitive elections at the village level. The IRI sponsored election workshops for officials emphasize the importance of secret ballots, multi-candidate elections, and transparent vote tabulation. Eighteen recommendations made by the IRI in 1994 were implemented by May 1997, eleven were partly implemented, while another eleven were not implemented (International Republican Institute 1997).

The Carter Center has ‘continuously looked for procedures to best implement’ the goal of full, free and fair elections in villages (Carter Center 1998: 10). In 1997, the Center, under the directorship of Robert Pastor, made fourteen suggestions, including standardizing and synchronizing voting dates among villages inside a county or province, adopting a direct primary system and secret ballots, limiting the number of proxy ballots, and institutionalizing campaigns (Carter Center 1997: 13-16). In 2000, it discussed the problems associated with village elections and made seven recommendations, including proposals that incumbent village committee members and the local Party secretary not be eligible to the village election committee, and sufficient time be set aside for voters to question the candidates at election time (Carter Center 2000: 26-33).

The success of the consultancy fieldwork described above can be explained by the path dependence of complementary configurations of organizations and institutions (Katzenelson 1997, Pierson 2000). The presence of one democratic institution leads to increasing returns, demands institutional integration, and
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constitutes an institutional need. To build a coherent system, one procedure already in place requires another compatible and complementary procedure. This process becomes progressively self-reinforcing. The logic of democratic institutions lies in their demand for autonomy and coherence. Institutional coherence requires the reduction of conflicts between institutions, and compatibility between old and new institutions. Tensions between institutions require coherent treatment and the spread of democratic institutions backed by democratic forces.  

During my numerous field trips, local officials have told me of various examples concerning the notion of institutional coherence. The need to overcome practical problems and ensure institutional coherence has become a driving force in the reform of electoral procedures. For example, serious problems of corruption by village leaders have in the past led to complaints, petitions, and resistance by villagers, who realize that it is dangerous for them to elect village committee members, while having little or no control over the committee’s powers. In turn, the right to dismiss a corrupt committee member has been institutionalized as an integral part of their rights in village democracy.

Consultancy fieldwork has also provided me with challenging cases and events that have forced me to reflect, revise and modify the institutional coherence thesis. To put it concretely the thesis must take into account different levels of institutions. Micro-institutions at the same level strongly require maximum coherence. In particular, the conflict between village committees and Party organization needs to be reduced. This is why villages have introduced election of Party secretaries and why procedures to determine the composition of village committees by township leaders was deemed incompatible with well-established procedures of open nomination by villagers.

By contrast, institutions at different levels may require only minimal coherence. For example, the logic of democratic development from village to township elections is not strong, as village and township institutions are at different levels. In general, institutional conflicts exist between micro-institutions at the village level and the national level regime. There is a space for such contradictions as there is no urgent pressure to resolve them. This is partly because local actors refuse to fix them and attempt to benefit from resulting institutional contradictions. The ban on direct township elections is an example. The incoherence between central and local electoral laws is another. The center takes account of and allows for local variations, while some local actors use this opportunity to inhibit democratic development.

The increasing return thesis, which is understood as a self-reinforcing process, needs to be modified in the case of Chinese village elections. Village electoral
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Institutions are embedded in the political structure where the Party dominates the whole election process. This creates a ‘lagging effect’ and sets limits on the effects of increasing returns from specific reforms. Village democratic institutions and the national authoritarian system constrain and contradict each other. In order to strengthen the compatibility of existing institutions, democratic forces attempt to reform the Party system by promoting intra-Party democracy. In opposing them, the anti-democratic forces deliberately use institutional contradictions to frustrate further democratization and, for example, ban direct township elections. An evolitional model of Chinese transition must therefore be understood by analysing interactions between self-reinforcing democratic forces and anti-democratic institutions.

Rethinking the civil society strategy for Chinese democratization

The development of civil society and its implication for Chinese democratization has been one of my main research interests (He 1992, 1995, 1997, 2002, 2003). Consultancy fieldwork has helped me to revise my view of the role of civil society in Chinese democratization, and reinforce my conceptualization of semi-civil society.

The nurturing of civil society is a main strategy of democracy-promotion assistance. As Thomas Carothers has noted, there has been a shift among three categories of democracy-promotion programs – elections, state institutions, and civil society. Aid to state institutions invited criticism and a certain disillusionment. Electoral aid has now declined. Much more prominent is aid to civil society.

International donors have made great efforts to cultivate the development of civil society in China. The main contested issue between the Chinese government and international donors is the one concerning the political participation of NGOs in Chinese local democracy. Let me discuss this through two cases which I met during my consultancy fieldwork in 2000.

Take the first case of the UNDP project. The original plan for this project did not mention the role of NGOs in a training program. However, UNDP required a review of the partnership between the Chinese government and civil society in the final evaluation stage when the project was completed in 2000. This requirement not only made Chinese officials very puzzled but also made them think of a strategy for meeting the demand. As a response, a senior Chinese executive stressed that the Chinese style of NGOs included university research institutions and women’s associations. Thus, it was claimed that the MoCA as an implementing agency of the UNDP project had successfully developed a partnership with the Huazhong University, the Women’s Federation of Qianxi.
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and other social organizations. The personnel and resources of university research centers such as the rural research institute at Huazhong University were used in training.

The second case was the EU-China Training Program on Village Governance. The early draft of the project set up as targets that ‘non-governmental organizations serve as watchdogs, demanding performance and accountability of elected officials. At least one non-governmental organization exists in each of the project areas and engages in monitoring the work of village committee. Representatives of at least 90% of relevant NGOs are able to point to at least two instances where their interventions have led to better performance and/or greater accountability of decision-makers.’ The proposal was rejected by Chinese officials. One senior official claimed, ‘Chinese election monitoring mechanisms comprise local people’s congresses, local governments, media and public opinion, non-governmental organizations, and villagers themselves. Chinese NGOs include associations of women, youth groups, and older people. There is no need to establish a new independent organization.’ In reality, the NGOs referred to in the proposal are likely to be urban-based organizations that have established links with government, and it would be too costly to send one NGO to one village where an election is being held.

Both international donors and Chinese NGOs face a dilemma. On the one hand, support for the role of NGOs in promoting Chinese local democracy is needed. Politically oriented Chinese NGOs are suppressed by the government; if they want to survive and develop, international support is extremely vital to help them gain independence and autonomy. On the other hand, the international funding for Chinese NGOs complicates the development of a genuine Chinese civil society. These NGOs will not be regarded as autonomous if their operational funding comes from the governments of the United States and Europe. Moreover, international funding for political opposition increases Chinese officials’ suspicion of ‘peaceful evolution’.

The result of the contestation is mixed. While the idea and practice of NGOs has been accepted and has penetrated into Chinese governments and society, the Chinese state still effectively controls political NGOs and prevents them from forming an open opposition. The involvement in local democracy by NGOs is limited, and the main actors for local democracy are local governments. In the political environment of China, NGOs have to work with Chinese governments at national and local levels. Currently, the only feasible and workable model is a partnership between the Chinese government and NGOs, with the latter largely dependent upon the former. Interestingly, due to the nature of Chinese politics, the
MoCA did not develop direct cooperation with fully autonomous organizations. It has, however, developed an *implicit, loose, and indirect cooperation* with fully autonomous organizations such as Tianze. Tianze has reproduced some items from the MoCA website, used some of MoCA’s textbooks for its training, and even invited MoCA officials to be their advisers. Such a pattern of cooperation between the Chinese government and NGOs has a uniquely Chinese character: by maintaining a loose contact with the state, the NGO reduces political risk (He 2003).

The above cases illustrate an important lesson about the strategy and institutional design of Chinese democratization. The Polish ‘Solidarity model’, in which a strong opposition from civil society forced the government to come to the negotiating table, does not fit China now. As Bruce J. Dickson convincingly argues, ‘Incorporating them [the economic and professional elites] into the CCP is likely to have the intended effect of stabilizing the political order, at least in the short run. If so, the emergence and strengthening of civil society will not produce an organized opposition, as it did in Eastern Europe, but a cooperative set of groups focused on economic rather than political issues. This would frustrate those who see civil society as an inherently democratizing force.’ (Dickson 2003a: 24; see also Dickson 2003b: 140).

The above cases have also reinforced my view that China scholars should give up a misleading assumption about the complete autonomy of NGOs (He 1997). They confirmed the usefulness of a notion of semi-civil society appropriate to an understanding of Chinese local conditions. We need a broader notion of civil society and/or the public spheres that is able to incorporate many local initiatives by both citizens and local governments.12

**Rethinking economic conditions for democratization**

There has been a debate over the relations between the economy and democracy.13 In the 1950s, Lipset hypothesized about the connections between economic development (a high level of wealth or capitalist development and a market economy) and democratization. However, the breakdown of new democratic institutions in the 1960s challenged Lipset’s hypothesis. Rustow (1970), for example, argues that the lack of preconditions such as economic development and political culture does not prevent a society from embarking on democratization.

Consultancy fieldwork offered me an opportunity to rethink the debate from the perspective of a practitioner and to ask a further deepening question. Obviously, democratization can take place in both poor and rich economic conditions – even in poor Cambodia. Nevertheless, a practical and substantial
question is how economic conditions influence the quality of democratization and the model of introducing democratization (for a detailed discussion on the topic, see He 2001).

Two contrasting examples can inspire our thought over the matter. In one village in Hubei Province, poor economic conditions made the villagers more concerned with how to earn more income for their families than with participating in making decisions on the budget; and the agricultural economic structure did not produce the plural economic interests or the new rich class that could have demanded participation in budgeting. But a large amount of money was given to the villagers who were asked to develop democratic and participatory budgeting procedures. As a result, the outsider’s money caused division among villagers, and different factions in the village were soon fighting for a portion of the money. The plan did not work because the money came from outside and the democratic procedure was imposed on villagers by outsiders.

By contrast, in the rich Zeguo town, Wenling city, Zhejiang province, the rapid economic development had created a situation in which the revenue of the town government depended upon the taxes paid by the private sector, and the social conflicts over diverse interests had increased. The newly rich and the private sector demanded that their voices be heard and that they got a right to participate in the decision-making process. Under this pressure, the local government in Zeguo town needed deliberative and consultative meetings to reduce interest conflicts, to provide a channel for citizens to express their concerns, and to increase the legitimacy of local official policies. With this background, local officials spent more than 100,000 yuan in April 2005 to conduct an experiment of participatory budgeting in which 257 participants, randomly selected from the population of the township, debated and deliberated over 30 construction projects and made a preference choice. The top 2 choices were then submitted to the Zeguo people’s congress. Among 92 deputies who attended the meeting, 84 voted for the top 2 projects; seven voted against, and one abstained. The experiment was extremely successful in articulating people’s preferences and collecting people’s opinions, in encouraging citizen’s participation, and in providing the policy legitimacy the local government wanted (Jakes 2005). The experiment was not imposed by outsiders (the author as an initiator and organizer only provided technical advice). It was the natural product of the economic development. The conclusion is that one of the best ways of introducing democracy is to let people follow democratic procedure to make a democratic collective decision on specific issues which impact local life. The success of democratic institutions must be generated from within and be based on certain interest structures.
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RESPECTING LOCAL AUTONOMY

The aim of consultancy fieldwork for the promotion of democracy is to provide knowledge and expertise to local actors, thus the fieldworker easily assumes the role of teacher. It is better, however, to take the roles of helper and facilitator. Rather than thinking ‘we do things for them’, the appropriate attitude is ‘local people do things themselves with our help’. Project implementers, coordinators and managers have to assume student status because they have to learn about the local environment, conditions and culture in order to implement projects, and they must be keen to learn whether their ideas and projects are workable in a particular locality. Thomas Carothers put this very nicely, ‘Democracy promoters have failed in many cases to seek a sophisticated understanding of the societies in which they work, resting on the misguided idea that their knowledge of democracy alone is a sufficient guide to foster democracy wherever they go. Too often they have taken upon themselves the role of agents of political change in transitional societies, treating local partners as mere assistants. Countless projects have floundered for lack of real ownership in recipient countries.’

Scholars and experts doing consultancy fieldwork are required to respect local actors as equal partners, and to value the autonomy of the Chinese host organization which is able to make decisions on how to implement projects. This respect is based on the following reasons. The first is a strategic one. In order to get things done one has to take account of local conditions and demands and consult with local actors. Moreover, in Chinese history and popular culture, those who work for foreigners are called ‘lackeys and compradors in the service of foreign bosses’ or ‘running dogs’. To respect local autonomy is one way to help these people to dismiss or reduce the suspicion from local people so that projects will run smoothly. The second reason is the philosophical belief that an equal concern and respect for other cultures is required for transnational activities. The third reason is that local autonomy is one principle of democracy. To promote local democracy is to respect local autonomy. There are pluralist paths towards democracy, and if we follow the recommendation of Michael Walzer (1994: 59–61), foreigners should respect the local Chinese interpretation of and quest for democracy.

A successful story of respecting local autonomy is provided by a Danish project. The coordinator at the beginning explained to Chinese host institutions and officials why they funded the project. Members of the project adopted a cautious attitude and never used the term ‘you must do this or that’. They frequently used phrases such as: ‘We only make suggestions and provide adequate funding for training. We are advisors only. You have a final authority to make a decision.’
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on whether you will adopt our suggestion. We come here to help you. We do something for your program, not vice versa.’

There are a few counter examples. One big project required that ‘Government officials of PRC learn specific values, skills and practices in Europe and Asia that they can reasonably apply to the advancement of local democracy at home. Participants returning from study tours are able to cite not less than eight democratic practices and not less than four social values that they consider potentially useful in the Chinese context.’ Chinese participants, however, rejected these quantitative criteria. They discarded the concept of social value, instead favoring the concept of ‘advanced experience’. This is because, as they argued, ‘We should not accept the values of other countries, nor do we export our own value system to others.’ Chinese participants were also dissatisfied with the requirement that ‘national project personnel are able to follow and apply EU procedures for procurement and project management. The Chinese Co-Director, Project Administrator and at least one official from MoCA achieve scores of not less than 90% on a simple test of familiarity with EU procedures for project management and procurement.’

Foreigners’ blunders can cause resistance and resentment from Chinese officials. A senior official told me that ‘X thought that they should decide finance matters because of funding from Europe, and they wanted to be independent. If they don’t cooperate with us, the training book they produce is likely to go in a rubbish bin. We will redo it!’ In another case, one project executive told local officials: ‘We come here to draft an electoral law for you; and I have to illustrate some important issues for the sake of our mission!’ In responding to this remark, one local officer said in private, ‘Americans come to draft a law for us? No way! I won’t even bother to meet you in the first place!’ Another administrator said, ‘OK, if you draft a law for us, you must provide funding to make it happen. But I will use your money to do something else!’

Respecting local autonomy will result in some undesirable outcomes. Overriding concern about equal respect may lead to little pressure; too much respect may mean a lost opportunity of fighting for new initiatives; and respecting autonomy is in fact to respect local officials, not necessarily grass-root people who may demand radical political reform. Take the example of secret voting. It was argued by some local officials that ‘you [foreigners] do not understand Chinese reality, and there is no point in having secret voting.’ If we had accepted this kind of reality in the name of respecting local autonomy, we would have compromised too much. Indeed, local officials changed their views of ‘reality’ and discovered the benefits of secret voting.
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In consultancy fieldwork scholars and experts have sometimes failed to strike the balance between respecting local autonomy and defending democratic principle. Here are a few examples. One scholar who received funding from Japan forced village cadres and villagers to learn democratic budgeting, saying, ‘If you don’t do it according to democratic procedure, I won’t give you money.’ This tone of language and coercive method didn’t respect local autonomy and invited criticism from villagers. In another case, one project manager learnt some lessons from previous projects, and attempted to control finance so as to avoid the waste of funding. For example, he stressed that the recipients of training should be villagers. He also set several high targets whose realization will lead to substantial progress in Chinese political reform. For example, he attempted to introduce independent NGOs to monitor village elections in all selected sites. However, his defense of civil society and his control of funding met opposition and criticism from Chinese officials, and few of his aims were realized. From these cases, it is clear that one needs to balance democratic principle and local autonomy and must develop a skill and strategy to do so.

NUTS AND BOLTS OF STRATEGIES

Scholars and experts engaged in consultancy fieldwork have to think about nuts and bolts of strategies for maximal impact and minimal costs. Below is a discussion of strategies for successful democracy promotion in consultancy fieldwork, and my suggestions for scholars who enter China as consultants.

1. Balance academic and public policy functions. In this kind of fieldwork scholars and experts are often regarded as ‘too academic’ by governments and NGOs, but ‘too political’ by their own colleagues. They have to live in this tension, and treat it as a source of creation. If they tailor their research to support a particular government policy, their reputation will suffer. They should not guess what governments want to do. They are advising local governments but not following government policies. Maintenance of independent scholarship is important for the success of projects.

2. Skillfully exert pressure on local leaders. Instead of imposing certain ideas and projects on local actors, one successful tactic is to ask scholarly, innocent questions that would be tolerated by local officials and to ‘seduce’ them into promoting political reform. A typical question would be: ‘Your plan has already been carried out somewhere else, what is your new initiative, policy or method?’ The
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tactic is to push local officials gently and skillfully and to encourage them to take local political reform initiatives.

3. Respect local officials, make friends with them, and be patient. The IRI, for example, did not strongly insist on a secret vote. Their tactic was to let Chinese local officials work it out for themselves. After a few experiments, local officials realized that secret voting can reduce conflicts and create an image of fair elections.

4. Do not confront local officials directly in public. While giving a necessary and moderate praise one needs to make constructive and friendly criticism in private through informal channels. It is desirable to solve difficult issues in an informal way. It is unwise to make things rigid, leaving no space for change and adjustment.

5. Keep a low profile. One should not claim oneself as the first to carry out important projects; nor should one go to the media too quickly and frequently. In this respect, both the Ford Foundation and the NED should be praised because they have funded numerous good projects, but they have never indulged in the honor and have seldom been to the media for public propaganda. By contrast, one NGO organized an experiment of direct township election and announced it before the election date. As a result, the Party secretary of the township was dismissed from his post by the higher level Party organization. A rush for public media attention killed this experiment.

6. Know different languages. Thøgersen’s chapter has discussed the importance of understanding and speaking different languages and local dialects in fieldwork. To achieve the goals of the project efficiently, foreign experts should have a command of both English and Chinese and be familiar with both Western democratic electoral systems and Chinese village election and self-governing practices. Such skills and knowledge are essential for effective communication and the avoidance of political risk. A good understanding of Chinese political language is required in order to design a project suitable to Chinese political conditions, to implement the project quickly and efficiently, and to avoid unnecessary delays or rejections. There are few international experts with all the criteria and skills required.

In consultancy fieldwork, it is essential to understand and use different political languages like those of Chinese liberal-minded intellectuals, official political propaganda, and international discourse. Despite the fact that one Party secretary revealed a very liberal view of democracy in our private conversation, she used the Party language to justify the fairness of election procedure by saying ‘the fairness
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of election procedure will strengthen the Party’s reputation and legitimacy.’ It is important for us to understand that this local leader must persuade her staff members in the official language. Stressing the leadership of the Party in the process of democratization made local leaders feel safe. The deliberate use of the official language results in an easy understanding by local officials, and provides effective justification for a project. At the same time, saying ‘this is an international standard or trend’ provides an extremely powerful argument.

CONCLUSION

Consultancy fieldwork is an effective way for academics to interact with the policy-making world so as to contribute academic knowledge and expertise to community and society. In return, consultancy fieldwork is an important channel for improving and generating knowledge. Consultancy fieldwork offers us opportunities to gain practical insights that can benefit theoretical and academic debates, to access particular types of data that one could not have collected through conventional fieldwork, and, importantly, to access the political decision-making process that would be very hard to get into through normal fieldwork channels. It also provides opportunities for us to rethink purely theoretical positions. For example, it has made me think differently about certain issues such as path dependence, civil society strategy, and economic conditions for democratization.

While the advantages of consultancy fieldwork are obvious and indisputable, the possible disadvantages and inbuilt bias of consultancy work should be noted critically. Consultancy deals with officials who often take academics to the best ‘demonstration’ site. This process has a built-in bias. For example, in 2005 I was taken to one residential community in Beijing and was extremely impressed with the social order and welfare program there. Later in the night, however, I went alone to a ‘shangfangcun’ where those who come to Beijing from the rest of China to launch their complaints or appeal to the superior authorities stay. I saw a completely different picture: more than 2,000 people stayed in this small village and more than twenty people were squeezed into a room smaller than ten square meters. They had been ignored for a long time and no officials cared about their welfare. And when I as a scholar showed my interest in their complaints and my concern with their individual cases, I was suddenly surrounded by two dozen of them eager to talk to me and seeing me as their ‘saviour’ or ‘helper’. They gave me many letters and supporting documents, and provided me with materials and
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interpretations very different from the official ones. This casual and informal ‘fieldwork’ helped me to see what I didn’t see in the official pre-arranged consultancy fieldwork. It supplements the consultancy fieldwork. Indeed, when doing fieldwork in China, it is better to have both official and private interviews and/or fieldwork. If we endeavor to combine both, we are in a better position to approach a fuller picture of social and political change in China.

AUTHOR’S NOTE

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NOTES

1 It should be noted that some persons and official organizations are kept anonymous as some of these people are still in charge of projects, and some references are omitted because some projects are secretive in nature and still unfolding.

2 By the end of the 1990s the U.S. government was spending over $700 million a year on democracy aid in approximately 100 countries. See http://www.ceip.org/files/Publications/aidingdemsum.asp?p=1&from=pubdate, accessed on 5 October 2004.

3 At the same time, the European Union had also discussed a similar project, but made very slow progress. Also the Ford Foundation had supported a few international conferences on village elections organized by MoCA.

4 The complex arrangement and political regulation around this project were interesting. While the donor was the UNDP, China International Center for Economic and Technical Exchanges under the Ministry of Foreign Trade and Economic Cooperation was the executing agency, while MoCA was the recipient. It should be noted that the MoCA was in full charge of the UNDP project. A review revealed ‘the limited resources available to UNDP to provide monitoring’ and recommended that ‘UNDP’s capacity to provide monitoring and backstopping of training projects should be strengthened’ (Falcon and Hong 1994: 13–14).

5 Author’s interview with Wang in 1996 in Beijing.

6 Author’s conversation with him on 26 Nov 2003.

7 Tianjian Shi (1999) has applied an institutional approach to the study of village elections.

8 For this idea, I benefited from my talks with Dr. Gu Xin in 2003 in Singapore.

9 Another battle is about overseas opposition groups. The Chinese government failed to control the support for overseas Chinese organization by the US.

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11 The author’s interview with a senior official in Beijing in 2000.

12 This broader conceptualization of civil society is also called for in the context of the participatory budget in the city of Porto Alegre (Baiocchi 2003) and of international support for grassroots organization in Russia where Henderson (2003: 153–4) sharply comments: ‘it was unclear for me why one unfunded group’s ability to attract 2,000 people off the streets to a health screening clinic sponsored by the local administration was not, in donor’s eyes, building civil society, but yet another cookie cutter round table for a smattering of NGO leaders on “working with local government” was.’


15 Elsewhere (2005) I have discussed the importance of cultural equality and the strategies for achieving it.


17 The minute of a working meeting held in the Ministry of Civic Affairs on 29 June 2001.

REFERENCES


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Chapter 11

APPROACHING THE FIELD
THROUGH WRITTEN SOURCES

Stig Thøgersen

Collecting written sources is a considerable part of the fieldwork experience, and researchers who go to China often spend much of their time ‘in the field’ trawling through bookstores and pestering officials for documents. In most projects it will be highly relevant to dive into the huge Chinese social science literature published in books and periodicals, to search the national media for information, and to look at the often very interesting debate taking place on the Internet, but I shall limit this discussion to locally produced written materials relevant to case studies of a specific locality such as a county or a township. Even within these limits there is an almost overwhelming number of texts to be found. In this chapter I shall discuss different types of written sources that I have personally found useful in connection with two different projects, one mainly historical, the other dealing with the contemporary period. Across the two projects the sources fall into three main types.

One type of texts addresses a relatively wide audience interested in local affairs, and even foreign researchers can normally get copies of them without too much hassle. These publications are produced by local cadres and intellectuals with close ties to the administration and include local gazetteers (difangzhi), yearbooks, Materials on Culture and History (wenshi ziliao), and products of other official publication projects; books and periodicals dealing with local history, culture, and social and economic affairs; local newspapers and periodicals; and newsletters published by local organizations. Since the 1980s China has seen an enormous growth in this type of publications as local governments have been competing for tourists and investments, and as it has become more legitimate to talk about local identity and uniqueness (within certain limits).
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Other texts are documents in the more narrow and bureaucratic sense of the term such as political guidelines, reports, statistics, and regulations. They are also produced by officials but are meant to be consumed mainly inside the party-state itself. Most of them are either received from or addressed to higher level authorities, and every branch of the local administration handles dozens of them every day. They are in principle always classified and therefore harder to get access to, but they are a main source for knowledge about the worries, problems, plans, and visions of the party-state. Chinese officials live and work in a steady flow of these documents, and interviews with cadres often get more interesting if the interviewer has had the opportunity to read the relevant documents.

A third group consists of texts produced outside the world of the bureaucracy. They can be written for private consumption (diaries, account books, letters), or they can be published officially through a publisher (memoirs, poems), illegally (posters of protest, some commercial hand-outs), or in a grey private/public zone (genealogies, some religious texts). Whether one finds them or not often depends on one’s personal relations to the author/distributor, or on pure luck, but they are clearly growing in quantity and in social importance as more and more people, even in remote villages, express themselves through writing. This group is particularly important as an alternative to the official worldview found in the other two types of texts.

Jointly, these written materials offer many valuable insights that supplement and interact with the information gained from interviews and observation. They allow us to look into the discursive worlds of the authors without being under the time pressure and the social and linguistic constraints that exist in the interview situation. They also provide us with the terminology and background knowledge we need for planning interviews and surveys, and offer clues about where to look for interviewees. Finally, locally produced texts offer factual information that simply cannot be gleaned from other sources with the same degree of detail and precision. It should be kept in mind, however, that local documentary sources in most cases reflect the views of what we in earlier chapters have called ‘official China’, and they cannot stand alone if we want to understand the mental and social world of ordinary Chinese citizens. For this purpose one needs to combine them with other sources, and my main focus here will therefore be on the interaction between documentary research and interviews.¹
The first project I shall discuss was mainly historical and looked at the history of education in Zouping county, Shandong province, from the last few decades of the Qing dynasty (1644–1911) to the 1990s (Thøgersen 2002). A large component of this study was to interview students of different generations about their personal experiences, but before I could start interviewing I needed an outline of the institutional framework of the education sector during the different historical periods, and for this purpose written sources were essential. They also turned out to give very good clues about whom it would be useful for me to talk to.

My first contacts were a group of local historians who were publishing very actively in the field I wanted to learn more about. In every county and city in China, and very often at the township level as well, there are former cadres and con amore historians who collect materials on local history. They often have a wide net of contacts with people with special experiences and interesting stories to tell. In Zouping, as in many other places, the local historians published through two main channels, local gazetteers and Historical Text Materials.

Local gazetteers and yearbooks

Local gazetteers were a distinct genre of history writing already during the Song dynasty (960–1279), and China has a continuous tradition for compiling information on all aspects of local natural and human conditions in this form. The gazetteers were revived with great force in the 1980s through a large-scale publication effort where all local governments from the county level up were told to set up special local gazetteer offices. The result of the joint efforts of local academics, retired cadres from all branches of the administration, and specially assigned officials were published in literally thousands of volumes all over China up through the 1980s and 1990s (Thogersen and Clausen 1992).

In Zouping I was able to buy the Zouping County Gazetteer, which had been published in 1992. More than 300 people had been involved in the writing of this volume over a period of close to ten years. With its almost 1,000 pages it provided much useful information on many aspects of Zouping’s development since the mid-nineteenth century, but there was relatively little to be found on education. This problem was solved, however, when I received a copy of the Zouping County Education Gazetteer, a book of 448 pages which had been printed in 1990. It was classified as ‘internal’ (neibu), which is quite common for sector and township gazetteers, but this classification is normally not taken very seriously by local editors who are happy that outsiders take an interest in their work.
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Most branches of the administration have produced gazetteers for their own field (industry, transportation, health, population, etc.) and besides being huge reservoirs of factual information such as dates, names of places and persons, statistics, and documents they are also very useful for identifying key informants, particularly inside the official circles. The sector gazetteers are often edited by retired high-level cadres who have been active in the relevant field for decades. In my case the list of editors of the Zouping education gazetteer led me to four elderly gentlemen who all had had personal experiences as students, teachers, and cadres in Zouping since the early 1930s. It turned out that during the more than four years they had been preparing this publication they had interviewed Zouping emigrants all over China and traveled to libraries and archives around Shandong as well as in Beijing and other major cities. Not all of the information they had found fitted the dry and matter-of-fact style of the gazetteer, but during the interviews they were happy to fill in more information. As an extra benefit their own life histories were highly illustrative of successful upward social mobility through the school system.

The county gazetteer offered some information about the township level but very little about the villages. It turned out, however, that many townships in Zouping had produced their own gazetteers, sometimes only in the form of a mimeographed manuscript, but still very useful for getting information about the history of village schools, local model teachers, and families with a tradition of learning. In Shandong, where civil unrest, occupation and war have been hard on archives and private collections, there are generally few pre-1949 documents to be found at the sub-county level, and the compilers of the township gazetteers therefore partly based their work on interviews. I met one editor of the section on education in a township gazetteer who explained to me how he and his three colleagues had divided the district into four sections with one person responsible for each. In every village they had called a meeting of people who had been involved in education as cadres or teachers. After they had heard their accounts they asked their informants to write down as much as they could recall about their experiences in the local school system. If they found unclear points in the texts or contradictions between texts and interviews they went over the topic again with the informants until they felt they had a consistent picture of the different periods. In the end only a fraction of all the information that was collected in this way was included in the township gazetteer, but the compiler had taken careful notes and remembered many details when I interviewed him. One of his suggestions was that I see a group of former students of an anti-Japanese school from the early 1940s who still met occasionally in one of the villages. These people eventually became my key informants for this period.
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The gazetteer publication project is followed up by yearbooks (nianjian) with updated information and statistics. Provinces and most larger cities publish them regularly, while the lower administrative levels are less consistent. In 1998 Zouping published an education yearbook covering the period from 1986 to 1995, which among other things included full text versions of a large number of official documents including government work reports and directives. Although useful in many ways this publication was not produced with the same personal enthusiasm as the gazetteer, and it did not guide me to significant new informants.

It almost goes without saying that the gazetteer project, like official Chinese history writing in general, is a highly politicized affair, and that one has to read and use these publications with a critical mind. In the relatively few cases where I could check documents reprinted in the gazetteers and the yearbook against the original version I found no discrepancies, and it was my general impression that the editors were careful to be as accurate as possible. They could do little about the inherent problems in the original data, however, as when official statistics were unreliable because local cadres had reported inflated figures for school attendance, as I shall return to below. Much relevant information had also been left out for political reasons. It was obvious, for example, that schools which were considered progressive and patriotic during the Japanese occupation of the area from 1937 to 1945 were given much more coverage than ‘reactionary’ or ‘puppet’ (i.e. pro-Nationalist or pro-Japanese) schools. Another example of political bias was the treatment of the Cultural Revolution decade (1966-1976), which is still so politically charged that it was either completely ignored or dismissed with a few negative sentences, although interviews made it quite clear that enrolment in village primary and secondary schools rose dramatically during that time. So while gazetteers are reliable sources in the sense that the editors, in my experience, rarely tamper with official documents and data, they are, of course, influenced by the political climate in which they are produced. They are of use primarily for topics that can be placed clearly within the logic of the party-state, while there is little information on all those aspects of local life that fall outside or between bureaucratic boundaries. So while you will get minute details on topics like infrastructure, enterprises, and the health system, you will find little if any information on prostitution, corruption, or even migrant workers.

As I wanted to use the gazetteers as a starting point for identifying informants it was also problematic that the biography section mainly mentioned persons and institutions that had a positive attitude towards the CCP while omitting ‘reactionaries’ that may have been of equal importance for local education.
I tried to find more of the suppressed stories from those who had supported the losing side in China’s successive political struggles. I realized that those who had later been officially rehabilitated – such as ‘rightists’ from the 1950s – were reasonably easy to identify and quite happy to tell their stories, while others whose past was still seen as problematic – like students of pro-Japanese schools and some children of landlord families – were much less willing to talk to me about their experiences. But except for these problems the gazetteers proved to be a good starting point for finding out which persons it could be interesting to talk to.

**Historical Text Materials**

The other main outlet for Zouping historians was a publication series called Historical Text Materials published by a small office with just a few employees. Just like the gazetteers, the Historical Text Materials (HTM) were the outcome of a state initiative during the 1980s, and similar publications can be found all over China at each administrative level from the county up. The HTM consists mainly of the personal memoirs of people from different walks of life who have either played a significant historical role themselves or who have witnessed important events. Some informants are asked to write down their stories while others are interviewed by the editors. In Zouping six volumes of HTM were published up to 1996, all dealing mainly with the pre-1949 period. The office in charge took a special interest in the time from 1931 to 1937 when Liang Shuming and the Rural Reconstruction Movement made Zouping their headquarters. They saw this as a unique period when the county played a crucial role in national history, and HTM work is often concentrated on such glorious moments in local history, particularly when the events can be constructed as part of the history of the Chinese revolution. There was also a quite extensive coverage of other periods, however, and many accounts were relevant to my topic because the informants often offered a brief record of their careers in the education system even when they were interviewed about other issues.

The quality of the historical text materials depends heavily on the memory of the informants. I found that their strength was the quite vivid picture they often gave of daily school life and living conditions in Zouping before 1949. Some of the accounts were internally inconsistent, however, and they sometimes contradicted the gazetteers on concrete data such as dates, names, and numbers. It was also obvious that they suffered from the same weaknesses as historical interviews: it is hard to recollect what happened forty or fifty years ago, and considering that some informants had probably told their stories over and over again during
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political campaigns and earlier exercises in collecting revolutionary history their accounts should be taken with more than a grain of salt. Furthermore, unlike interviews it was impossible to ask informants to clarify certain points or give more details. The HTM were still useful, however, because they mainly featured prominent persons with dramatic experiences and exceptional careers, while most of my own interviewees, mainly found through the snowball sampling method, were ordinary villagers who had lived ‘normal’ lives.

As in the gazetteers there was a clear bias in favor of ‘positive’ historical characters among the informants selected for interviews by the HTM, and former party secretaries and guerrilla fighters figured prominently on the list. As clues to identifying informants the HTM were somewhat less useful than the gazetteers. When the editors started collecting these stories in the 1980s they wisely chose to start with the oldest and frailest citizens, and many of them were long gone when I turned up ten years later. Still, I found valuable references to institutions and events which I could later ask for more information about in interviews.

Archives

After I had got a general picture of Zouping’s educational development through gazetteers, statistics, and a first round of interviews, I tried to find more documentary sources in the Zouping County Archives. This institution started modestly in 1958 and was reorganized after the Cultural Revolution in 1978, when materials from the period 1938–1978 were put in order and catalogued. All local materials dating from the period before the Japanese occupation had apparently been lost. When I visited the archives in the mid-1990s I was told that they had very limited holdings from the period before 1963, and that most materials related to education were actually stored at the county education bureau. I was later told that I could not get access to the education bureau files because they could contain material about individuals whose privacy should be respected.

Archives of this type can be found all over China, and researchers have mixed experiences with them. I have heard that some places hold significant collections that they are willing to share with foreign researchers, but judging from my own experience in Zouping and a few other places county archives are not exactly characterized by great accessibility. It is my impression that in order to get anything out of them you either have to know exactly which documents you are looking for, or simply be lucky. The only collection in the Zouping archives that turned out to be useful to me was its copies of the Xiangcun jianshe, the main periodical of the Rural Reconstruction Movement of the 1930s. It should be noted, however that provincial and national archives are generally in a much
better shape, and normally also more service minded. In any case it would be a
good idea to check available information on local archives before going to the
field, and pay them a visit to see what they have (and want) to offer.³

MATERIALS ON CONTEMPORARY SOCIETY

In the more contemporary project I was looking at the role of the local state and
cadre-peasant relations in Xuanwei municipality, Yunnan province. In the history
project my main problem had been to identify and get access to the rather limited
number of existing sources. In Xuanwei I was in a significantly different situation,
because the place was flowing with all types of publications that seemed highly
relevant to my topic. The question here was rather how to pick the most relevant
sources, and how to use them for research purposes.

News media and local periodicals

When I first came to Xuanwei around the year 2000 the municipality had its
own newspaper, the Xuanweibao, and a daily TV program with local news. The
paper could not survive under market conditions and ceased publication
in 2003, but the local TV channel still exists. At first impression the local news
media were quite disappointing. While some national and provincial papers
and TV programs have experimented with investigative journalism and other
attempts at satisfying the customers’ demands, the local media in Xuanwei –
and probably in most other counties and municipalities – were stuck in their
traditional role as ‘mouthpiece of the Party’. While this made them quite boring
to the readers, and thereby contributed to the Xuanweibao’s eventual closure,
it made them a good source for topics like official ideology, campaigns, and
leadership changes. The Xuanweibao could be bought in a very handy form with
six months’ papers bound in one volume. The relatively easy access to local news
is clearly an improvement compared to earlier periods when the paranoid Maoist
state classified even provincial level newspapers as ‘internal’ so that they could
only be accessed in a few libraries outside the control of PRC authorities.

One example of useful information found in the Xuanweibao was its quite
extensive coverage of exams held to recruit new government officials. This was an
experiment intended to introduce a more meritocratic and competitive approach
to cadre recruitment, and in order to present the exams as transparent and fair
every step of the process was announced in the paper in advance and followed up
by reports. Around the same time, and for similar reasons, the paper published
short biographies of all cadres appointed to township mayors and deputies with information on their age, level of education, and most recent post. Information on cadre careers is normally zealously guarded by the Organization Department (zuzhibu), which is one of the least open branches of the Chinese administration. The newspaper articles on exams and careers therefore provided a good and much needed starting point for later interviews with local officials.

Newspapers are also a practical guide to persons who can embody the official image of the locality. Local Party and government leaders figure prominently on the pages, including township leaders and prominent village heads. During the time of village committee elections the Xuanweibao carried many reports about individual candidates, which clearly demonstrated which type of people the municipal leadership wanted to see in charge of the villages. Major local enterprises and successful entrepreneurs presented themselves in commercials dressed up as news reports, and model institutions and citizens in different fields were praised. Although such models are not representative it can often be useful to study them because they show how the system is supposed to work, and how it is legitimized and sold to the citizens.

Besides the news media some local periodicals were quite interesting in spite of their limited circulation. Two of them turned out to be particularly useful. One of the towns distributed a monthly in-house journal to all its cadres. Here they could read work plans from the different branches of the town administration, speeches held at meetings for cadres, advice about how to handle problems with the citizens, pep talks intended to boost their morale, and more freewheeling meditations on cadre life. Besides giving very concrete insights into the workings of the bureaucracy it was also a good source on the self-perception of the cadres and the kind of professional ethos that the leadership were trying to build.

One of my fields of interest was the way cultural control was exercised under market conditions (Thøgersen 2000), and I was therefore very happy to find a cultural magazine that published short stories, poems, and plays by local authors, as well as articles on cultural issues. I learned about this magazine during an interview, and through the list of contributors I got in touch with a group of local intellectuals who were active writers, painters, or musicians, but at the same time worked in the cultural administration. These people turned out to be very good informants about official cultural life in Xuanwei.

Official documents

While the local authorities offered me newspapers and journals without any reservations, even though they were officially classified as internal, it was more...
Difficult to get access to regular official documents. The term ‘official document’ covers a wide range of written communication. Local cadres ‘ask for instructions’ (qingshi) and write reports (baogao) to the higher levels on problems and issues within their field of responsibility, while several types of orders, directions, and information flow from the higher to the lower levels of the Party and government organization in the form of ‘red-head documents’ (hongtou wenjian), whose red letterheads signify their official status. All these documents float around in a highly structured universe where detailed rules control how they should be designed and handled. The regulations about Party documents (Zhongguo Gongchandang 2004), for example, run into 19 pages and 40 paragraphs and stipulate, among many other things, the correct names for 14 different categories of written communication ranging from the serious ‘resolutions’ (jueyi) on important issues to the much less consequential ‘minutes of meetings’ (huiyi jiyao).\(^4\)

The first problem with such documents is, of course, how to get hold of them. The official Chinese system of classification of official documents has three degrees of secrecy (in descending order: ‘top secret’ (juemi), ‘highly secret’ (jimi), and ‘secret’ (mimi)), but even documents without such a classification are to be considered ‘internal’ (neibu), which means that they cannot be taken out of the country, and that access to them can be restricted in a number of ways (Zhongguo Gongchandang 2004: 18). This system is a serious obstacle to research, as much information on sensitive issues is systematically classified. A recent report from Human Rights in China and China Labour Bulletin shows how the authorities through the regulatory framework for ‘state secrets’ control information on the labor market in such a way that certain types of reports and statistics on issues like child labor, industrial accidents, unemployment rates, and labor unrest are consistently suppressed (HRIC 2004).

It is my experience from Xuanwei and elsewhere that Party and government offices in most cases handle the classification system in an open and flexible but also somewhat unpredictable way. With a research permit and a letter of introduction from a higher level authority it has not been difficult for me to get access to ordinary ‘internal’ documents. Some cadres stuck to the rules and preferred to read the document aloud rather than handing out a copy, but most agreed that this was a waste of everybody’s time. In some cases officials also readily gave me copies of documents classified as ‘secret’, especially when they had been asked directly by their superiors to be helpful to me, but this happened less often. Very few cadres actively directed me towards such documents, so unless I knew the titles of specific reports and regulations and asked for them...
directly I did not receive them. In this context the local newspaper was useful, because it occasionally referred to important new documents.

The next problem that arises is how openly you can use classified documents in research publications. I think the main issue here is to be careful not to put your informants in a difficult position. I never received documents under the table or through private connections, they were always handed over to me quite officially during interview sessions when I was asking questions that could be answered more precisely by giving me access to the relevant written text. For this reason I have not hesitated to refer to or quote internal material. However, some researchers have privately reported that they have run into trouble with the Chinese authorities when they have published information based on classified documents. As in many other fields the Chinese state formally maintains strict rules about access to documents while in reality being much more flexible. This leaves open the possibility of striking against individuals whose research the authorities particularly dislike, while keeping the rest in a state of insecurity, thus placing the researcher in a dilemma of which at present there seems to be no easy way out.

Official documents can be highly instructive about the party-state’s policies, plans, and modes of thought. Even after a lengthy interview with a member of a so-called ‘cultural inspection team’ that was dispatched to markets to confiscate illegal cultural products I was still uncertain about what precisely these teams were looking for. An official instruction from the provincial level with the exact titles of publications they should watch out for made this much clearer. Similarly, a written report from a township to the municipality about how they perceived the ideological situation in the villages was much more concrete and informative than my interview with the author of the report.

However, documents are not produced to make foreign researchers better informed but to achieve certain goals that we often do not fully understand. A cadre can write a report about significant local achievements in order to make a favorable impression on his superiors, or exaggerate a particular social problem in order to receive more resources. In both cases the author will give the document a twist that serves his or her purpose. We may be tempted to read a local report on serious poverty as a reflection of the ‘real conditions’, particularly if it is written in an alarming tone, but the motives for producing such a report may be to maintain a status as ‘poverty township’ with the economic benefits this brings along.

It is also clear that documents with instructions from the higher levels should not be read as descriptions of what actually happens. They are interpreted and transformed several times before they are turned into social action at the local
level, and in some cases they are not implemented at all but live a shadow life somewhere in the files. I also found that official documents on social change in the villages of Xuanwei generally tended to exaggerate the role played by the party-state. Many local activities, not least in the economic field, were presented as the result of specific Party policies or interventions from the local leadership, although the initiative evidently came from private entrepreneurs. In this way official documents construct a world in which the Party is in full control, and they should therefore be supplemented by other sources of information, such as interviews with people outside the official circles and observation of social practice.

Statistics

In order to decide which townships in Xuanwei I should go to I needed comparable information on their economic and social conditions, so it was very useful that the municipal statistical department produced a small yearbook with statistical materials, which contained data down to the township level.\(^5\) In the 1990s these booklets were classified as ‘highly secret’ and each copy had a serial number so that it would be possible to trace any leak back to its source. From the year 2000 the classification was only ‘internal’, which is considerably milder. However, the materials were not that hard to get even from the 1990s as the local officials felt that it was much more convenient to hand us the booklet than to answer our questions during interviews.

The weaknesses of statistics from the county and township levels are quite obvious, and it is understandable that the statistical bureaus at the provincial and national levels sometimes conduct their own surveys to collect data directly from the grassroots (see Gustafsson and Li’s chapter). It should be remembered, however, that in many cases local statistics are the stuff that central statistics are made of so that errors made at the bottom are simply aggregated at the higher levels. Like documents, statistics can be compiled for a number of purposes, and the figures they contain reflect their purpose. This was quite obvious already in my project in Zouping, where I once visited a township with a primary school attendance rate close to 99%. This was surprisingly high, as there were at the same time alarming reports in the national media about parents who let their children drop out of school to look after younger siblings or work in the fields. It turned out that the optimistic figures were reported by the local school headmasters after a head count on the first day of the new school year. Prior to this day teachers and village officials had put pressure on the parents to register their children, so they practically all turned up on the opening day. The
headmaster reported the number of students to the education department, which aggregated the data from several schools and turned them into township and county level statistics. Everyone from the headmaster to the county cadres were happy, because they could prove to their superiors that they were doing a great job and successfully implementing the law on compulsory education. The figures were not even formally wrong, but if they had counted the students a month or two later the result would have been quite different.

In Xuanwei I was told by a village leader that the per capita GDP figures he had reported to the township were much exaggerated. He had received certain standards from above that he had to base income figures on. If someone in the village owned a truck, for example, he should report a specific standard income from this truck even if the owner had made no money on transportation at all. The township leaders could then claim that they had lifted this village out of poverty, and in this way add to their political capital. Yongshun Cai, who did fieldwork specifically on the issue of rural statistics reports how cadres were ‘over-reporting their achievements, under-reporting their failures and even making up statistics’, and he found these problems to be ‘rooted in the institutional arrangement of the administrative system in China’ (Cai 2000: 784–85).

So why bother about collecting local statistics at all? First of all, statistics are problematic in all countries and always have to be handled with care, and I am not sure the quality of the Chinese data is much worse than in other places. While there are very good reasons for always interpreting statistical data in the light of the institutional framework that produces and consumes them, the information I got from the statistical compilations probably gave fairly accurate indications of important aspects of social reality. For example, when I compared statistics from different townships on population, most important crops, degree of urbanization, number of cadres, industrial enterprises, schools, and health clinics these figures all contributed to forming a rather consistent picture of economic differentiation within the municipality. When I later visited some of these townships, the places that stood out as relatively well-off in the statistics actually looked richer in real life as well. Frequent inspections from above probably also put some limits to statistical exaggerations of local achievements.

Written sources produced outside official circles

Although the Chinese authorities to a large extent still control publication and the media there is a rapidly growing stream of written materials created by people outside the party-state, and such materials present exciting new opportunities for hearing alternative voices. A new publication trend in Xuanwei is that local
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lineages produce their own genealogies (jiapu), which are often impressive hardback publications with illustrations in many colors.

Just like the gazetteers the genealogies represent a genre in local history writing with long historical roots, which has been revived during the post-Mao period. In other respects, however, they differ from the gazetteers. They construct the history of a local lineage, a surname group with a common forefather. In Xuanwei most Han lineages claim that they can trace their ancestry back to settlers who came to the area during the Ming dynasty (1368-1644). The structuring principle of the genealogy is the family tree in male lines. The genealogies contain stories from the pre-Xuanwei past of the lineage, lineage rules, etc., but the bulk of the work is the family tree with its many branches containing the names of all male members and, for the latest generation, also the names of wives and daughters. An important section contains biographies of prominent lineage members. The compilation and publication of genealogies take place without public support and is funded through donations from lineage members.\(^6\)

The CCP has a highly ambivalent attitude toward the revival of lineages, but in Xuanwei the authorities apparently followed a policy of non-interference towards the compilation of genealogies. The editors I met were in fact mostly elderly Party and government cadres, who invested much energy and money in this work. They saw genealogies as ‘part of the glorious historical heritage of the Chinese nation’, and they had even found a 1957 quotation from Mao Zedong in which he praised the value of genealogies as source material for historical research.\(^7\) But there is still a limit to how ‘official’ the new published genealogies were. They did not receive an ISBN number, so although they were often beautifully produced by a state-of-the-art publishing house they were still semi-private and distributed through the lineage rather than through bookstores.

The genealogies were particularly interesting because of their explicit focus on topics such as individual careers, inter-generational social mobility, and marriage patterns that are evidently important but receive little attention in official sources. It was not just in relation to subject-matter that they differed from official publications, however. A comparison between biographies in the local gazetteers and in the genealogies showed clear differences in their ways of representing individuals and careers. The gazetteers had biographies of revolutionary martyrs, important historical figures, moral-political models, and officials, professionals, and academics above certain ranks. Moreover, all of these people were already deceased. The genealogies not only included the living, they also expanded the number of people whose life history was put in print, and covered other categories, particularly people with economic success. While the
gazetteers created an image of the CCP as the driving force in people’s careers the genealogies to a much larger extent represented life histories as the result of individual efforts, so wealthy entrepreneurs and businessmen appeared in great numbers, and their biographies offered detailed information on how they had made their fortunes. This meant that a researcher working on rural entrepreneurs would be able to identify different types of informants, find representatives from different age groups and industries, and proceed to interview them, as they were all still living in the area.

The genealogies were not the only examples of the boom in non-official written sources. On the streets of Xuanwei people handed out illegally printed advertisements for health products and unauthorized healers, which contained a wealth of implicit information on perceptions of illness and health, sex, family life, child care, and old age. In front of the local memorial hall for Deng Xiaoping’s father-in-law old men were selling religious texts and booklets with stories about demons and fairies. On lamp-posts there were job advertisements with wage information and occasional posters with political protests such as the one I discussed in an earlier chapter. It would thus be possible to find interesting unofficial texts on many research topics and search them for promising links to informants. If we extend the scope to private written sources there is, of course, much more to find. Diaries, private letters, account books, and gift lists are among the types of documents that have been used in research on contemporary Chinese society, but it obviously demands a high degree of trust between researcher and informant to get access to such sources.

THE DYNAMICS BETWEEN DOCUMENTARY SOURCES AND INTERVIEWS

In a highly literate – some would say text-obsessed – society such as China, documentary sources are essential to most social science studies, as several contributors to this volume have already mentioned. At the same time it is evident that many types of social activities never reach the written media. Fields of social action that fall outside the scope of the bureaucratic organs tend to disappear from public view, and the same goes for aspects of social life that the authorities deliberately want to hide from the domestic and foreign public. So if we look at China exclusively through written sources we obviously get a quite distorted picture, even if we include the expanding field of non-official documents. This is, of course, where interviews, direct observation of social practice, and surveys come in.
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In this chapter I have looked at some of the potentials and limitations of locally produced documents and particularly at how such documentary sources can be used in a dynamic interplay with interviews. The strategy that I have found most fruitful has been to combine the study of written sources with interviews in a zig-zag movement, where I have identified the names of potential informants from newspapers, journals, or books, interviewed them and asked them for more written materials, returned to them with the questions that emerged from the documents they had given me, and moved on to other informants mentioned in interviews or written sources. This approach works best with the social strata that are most visible in written sources, primarily officials and intellectuals. With the growing number of texts produced outside official and intellectual circles, however, there are reasons to believe that other groups will gradually become accessible through written sources.

NOTES

1 For a much more comprehensive and very interesting discussion of the use of documents in social science research see Prior 2003.

2 The section on Zouping builds on Thøgersen 2002: 251-256.

3 See Ye and Esherick 1996 for a guide to Chinese archives. Several archives now present themselves on the Internet. For an excellent example see the Beijing Municipal Archives at the address http://www.bjma.gov.cn/

4 The regulations on Party documents are from 1996. Similar regulations for government documents came into effect in 2001 under the title Guowuyuan xingzheng jiguan gongwen chuli banfa. Both sets of regulations can be easily accessed from PRC web sites.

5 It was impossible for me to get statistical information from administrative villages in printed form, but it could be obtained through interviews with village cadres.

6 The cultural meaning of genealogies in Xuanwei is discussed in Pieke 2003.

7 Genealogies should not be seen as ‘subversive’ publications, in most respects they probably support state-building efforts. As Frank Pieke (2003: 118) has said, the ‘genealogical mentality’ that they help to reproduce ‘can be a powerful tool for the localization of the state contributing to … the strengthening of a vertically integrated state apparatus’.

REFERENCES

Approaching the Field through Written Sources


Section IV

INSIDERS AND OUTSIDERS
Before I went to Sichuan to do fieldwork in 2002 I had done research on local people’s media use in other places, but I had never paid any particular attention to the term: ‘the insider’s perspective’. I had focused on collecting data according to my interview guide and writing the results into reports, but I had not been concerned with what the media looked like from the informants’ point of view, or with how they reacted to our questionnaires. To be more precise: I had not paid attention to the difference in perspective between them and us. China is a quite complicated society with a high degree of social differentiation, so there will always be large differences in perspective between the researcher and the informants, which means that even a Chinese researcher doing fieldwork in her own country has to face the insider/outsider question. In this paper I shall discuss how a native researcher discovered the difference in perspective between herself and women who were in danger of being trafficked, how this discovery broke up the way the research question was originally framed, and how the questions were rephrased in a more correct and fruitful way based on the perspective of the informants. Before looking at my personal experiences, however, I shall briefly discuss the position of qualitative research in Chinese media studies.

ON QUALITATIVE MEDIA RESEARCH IN CHINA

In the early 1980s, academic disciplines such as sociology and psychology, which had been forced to close in the 1950s were rapidly revived in China as a result of ideological liberalization and the policies of economic reform and opening up.
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The social sciences entered a blooming period, and in order to satisfy the demands of a growing number of social scientists many Western books on social science research were translated into Chinese, including works on research methodology such as American sociologist Earl Babbie’s *The Practice of Social Research* (Babie 2000). These books introduced the concepts of ‘scientific’ and ‘objective’ social research, and the strongly ideological propaganda works so characteristic of the Cultural Revolution era were gradually excluded from the field of social science.

It was quantitative research methods that first caught the attention of Chinese scholars, at least in the field of media studies where researchers began their study of scientific research methods by imitating Western social research. Because the large majority of the researchers had not gone through systematic methodological training or even given the issue of methods much thought, a tendency emerged where only quantitative methods counted as research, while there was no systematic understanding of qualitative methods. The nationally recommended university textbook *Mass Communication* introduces the three methods of social surveys, content analysis, and experiments and presents them all as quantitative (Guo 1999: 277–89). Another textbook, *Principles of Mass Communication and Their Application* adds ‘case studies’ to the three methodologies mentioned by Guo, but places all four under the heading of quantitative methods (Dai 1988: 381-405). From this we can see that mainstream media scholars of the 1980s were unable to differentiate between quantitative and qualitative research. Still another textbook talks about ‘natural observations’ without differentiating such observations from social surveys, content analysis, and experiments (Zhang 1993: 384–92).

This neglect of qualitative methods was not accidental. During the 1980s and 1990s some social scientists had a much too broad conception of qualitative research, and they often placed all sorts of non-quantitative work in this category, including scattered reflections, thoughts, and positions, descriptions of individual experiences and impressions, and even simple notes on specific policies. They simply acquired the habit of calling anything that went through their brains the results of ‘qualitative research’ (Bu 1997: 51). On this basis some researchers claimed that ‘qualitative research provides the foundation for quantitative research, quantitative research gives accuracy to qualitative research’ (Chen 1989: 122). This is a far cry from real empirical qualitative research. In the words of Beijing University scholar Chen Xiangming, ‘most “qualitative research” basically does not demand systematic collection and analysis of original data, it is rather casual, habitual, and spontaneous, and it mainly plays a role in raising a discussion or forming public opinion’ (Chen 2000: 23), and it is therefore difficult to regard it as a scientific method. But this is only one side of the coin. The other aspect is
that to many researchers, often those who do not master quantitative methods, ‘sinified qualitative research’ applies exactly those methods they are used to, so they definitely regard it as ‘scientific’.

It should be emphasized here that there is a vast difference in China between how qualitative research is understood and applied in media studies and in sociology. Researchers in the sociological field, represented by Fei Xiaotong, have a fine tradition for using fieldwork methods. In sociology, feminist researchers in particular are considering issues like ‘the important role of the researcher in the creation of knowledge’ and how ‘we should examine with which features and through which channels the inclinations and fixed views of the researcher enter the process of constructing knowledge’ (Feng 2004: 27). In contrast, Chinese media research is rooted in traditional journalism studies, which for many years have been regarded as an integrated part of political propaganda work and therefore lacks a tradition for scientific research. When foreign media studies were introduced to China during the 1980s it was mainly in the form of quantitative studies. The ‘objective’ and ‘scientific’ character of quantitative research, and its commercial value, led some researchers to do quantitative surveys of media audiences, but generally speaking media studies, quantitative as well as qualitative, lacked the thorough methodological reflections that could be found in sociology.

In 1996 Chen Xiangming published a paper entitled ‘Qualitative Research Methods in the Social Sciences’ which led to some discussions and an increased awareness of qualitative methods. In media studies, academic papers on qualitative methods began to appear in the mid 1990s (Bu 1994), but the transition from the Chinese version of ‘qualitative methods’ to a real empirical qualitative approach is still incomplete, and there is some way to go before the large majority can identify with this new understanding. One sign of this is that the number of articles employing qualitative methods is still very small, although China in fact needs much research of this type.

In China, regional and social differentiation has created a complicated situation where large-scale standardized questionnaire surveys often lead to conclusions that are far from reality, particularly when the surveys cover rural areas. For example, one question that often pops up in media surveys is ‘What sort of programs do you like to watch on TV?’ In China’s villages, the large majority of households only have one TV set with a limited number of channels, and it is maybe the male head of the family who decides what to watch. In a case I shall discuss below, I interviewed a woman who had been abducted, and who watched kongfu movies every day without understanding much of the plot. This choice of program, however, was her husband’s, not her own. If you asked her what sort of program...
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she likes to watch, she might say ‘kongfu movies’ or she might just smile, but none of these answers would give you any reliable data. Standardized questionnaires are the main tool of quantitative research, but if you want to use them in media research the following conditions should be fulfilled: (1) the interviewee must have personal independence and be able to use the media in an independent way; and (2) the interviewee must fully understand the questions in the survey, which means that there should not be too much cultural and linguistic difference between the interviewer and the interviewee. It may be easy to comply with these conditions in the US, but this is certainly not the case in the large majority of China’s villages. As we have already seen, people of this woman’s generation often consume the media as a household rather than individually, and they often cannot see the point of a questionnaire worked out by an urban intellectual.

In qualitative research we must often discuss the question of ‘insiders’ and ‘outsiders’ and always stay alert to this issue, because it affects the reliability and validity of the research, and because the whole research process requires that the researcher constantly approaches and tries to understand the insiders. Quantitative research apparently does not have to pay attention to the insider/outside dichotomy, but in reality the standardized questionnaires transform the remarkably diverse questions of insiders from different social strata and translate them into the uniform code of the outsider. Particularly in media market research concerned with viewing rates and circulation the unified standards tend to conceal the many different positions and perspectives of the insiders and subordinate them to the outsider’s perspective.

This forces us to examine the foundation of the standardized questionnaires, indicators, and scales that have come from the West, and consider whether the same foundation is present in China, particularly in the rural areas. If the foundation is not the same, the best starting point will be to engage in qualitative rather than quantitative research. My personal decision to start to experiment with qualitative research was based on critical reflections on quantitative methods. This certainly does not mean that I have discarded quantitative methods, but only that I choose my research methods and tools more carefully based on the nature of different research questions.

One of the most controversial issues in qualitative research is how to treat the relationship between insiders and outsiders (Chen 2000: 134). As an outsider the researcher must often make a choice between either learning to become an insider and gain access to the cultural group she wants to study in this way, or try to approach the insiders in the capacity of an outsider. Both alternatives have their advantages and drawbacks (Babi 2000: 363). In my experience, presenting
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oneself as an insider or as an outsider are simply different tools. The aim is always to get a deep understanding of the research question through studying and imitating the perspective of the insiders. A perspective is a method of observation, which is based on a certain experience and knowledge. Because insiders and outsiders have different experiences and different knowledge, their perspective on the same issue or event will also be different. Research is always about getting access to the insiders’ experience and understanding, so looking for the insiders’ perspective is extremely important.

The remaining part of this paper is a case study of the research project Information, Education, and Communication Strategies Against the Trafficking of Women In Sichuan Province, and it tries to explore experiences and lessons to be learned about looking for the insider’s perspective during fieldwork.

BACKGROUND AND METHOD OF THE PROJECT

Since the 1970s the criminal trafficking of women and children has been on the rise in China due to several factors such as uneven economic development in different regions; the influence of feudal phenomena such as arranged marriages, bride price, and the preference of boys over girls; and the lack of protection and education for weaker groups of migrants. First there was large-scale migration of women from poor and remote areas who married far away from home in order to get a better life. Then followed abductions of women, who were sold as wives, and in the 1990s increasingly serious cases of human trafficking appeared where women were sold to perform sexual services. In the mid 1980s the Chinese police started to strike against criminals who abducted and sold women and children, and in 1999 UNICEF and the Chinese Ministry of Public Security initiated a joint project to ‘strike against criminals trafficking in women and children’, which boosted the Chinese fight against this type of crime. The aim of the research project I shall discuss here was to provide basic information for UNICEF to be used in their efforts to develop a media, communication, and education strategy aimed at wiping out the trafficking of women in China and particularly in Sichuan province. I worked on this project together with Qiu Linchuan, a PhD student from the Annenberg School for Communication, University of Southern California.

We actually started on the project without any predetermined methodological approach and had not even decided whether we would conduct interviews, or to whom we would talk. When we arrived in Sichuan we first went to some relevant
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sites to observe what was going on. We made observations and talked to people in the counties of Jintang and Renshou (which is a national level poverty county\(^1\)), where we went to the homes of trafficked women, to police departments, to the local media, to the labor markets, and to the living quarters of migrant workers. According to what local people told us, labor markets were among the main centers for human trafficking, and UNICEF, the Sichuan Provincial Women’s Federation, and the Sichuan Public Security Department were already running joint propaganda and information activities in labor markets in the provincial capital of Chengdu. Based on my earlier experience I realized that this research project had to involve the young women who came to the city from the villages to look for jobs, as they were the target group of the future information program. What were their experiences and viewpoints, and how did they react to trafficking and to the propaganda and education activities that were conducted to prevent it?

We then started looking for informants among the female job-seekers. Later developments showed that such exploratory interviews were highly necessary, and they later led to a number of crucial tactical interventions through the media. In this research project, my co-researcher and I were always ‘outsiders’, and we faced the challenge of identifying the perspective of the ‘insiders’ in order to understand their experiences and viewpoints and their attitudes to anti-trafficking information. This paper will analyse what we experienced and learned through this case study.

LESSONS LEARNED

Insiders and outsiders have different perceptions of the same event

We first of all found out that insiders and outsiders often understood the same event or fact very differently. A village cadre took us to the home of a woman from the Yi nationality who had been trafficked. This woman told us:

I really feel embarrassed to talk about this trafficking business. When I lived in Liangshan [an autonomous area for the Yi nationality] I had never heard of ‘trafficking women’, but it was quite normal for a woman to be taken from her home and married off to somebody. Don’t even think of taking me away from here. It is much better here than in my old home in Liangshan. I get enough to eat, and I don’t have to work in the fields, I just do the housework.
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When I later got to Liangshan I realized that most Yi women had a really tough life doing all the housework and working in the fields as well. My own original perception would be that the Yi woman had been sold illegally to this place and that somebody ought to rescue her, but seen from her own viewpoint it was not so important whether she had been ‘illegally trafficked’ or not. Her life had improved since she came here, so she did not want anybody to come and set her free. From our point of view she was poverty-stricken and her husband was ill, which was why it had been necessary for his family to buy a wife for him. Still, in her own understanding this was a better destiny than any of the alternatives. Or you might say, if she wanted to survive there were no alternatives. It was evident that we and this trafficked woman had very different understandings of ‘trafficking’ as a social event.

I had come across this phenomenon often before, but it made me extra aware that in this particular communication project it was essential to discover all such differences in understanding between us and the members of the target group we were studying. When I saw a large slogan in the street saying ‘Resolutely Fight the Criminal Activity of Trafficking Women’, I started to think about whether this was what a destitute young woman who came to the city to find a job would really need in order to gain a better life. What would she think about such a slogan when she heard the voice of a trader in human beings saying ‘I can sell you to a rich family. You will be able to see Tiananmen Square in Beijing from your own doorstep’? Would the anti-trafficking slogan affect her decision? I realized that making propaganda that could prevent trafficking would be much more complicated than we had anticipated.

As the project concerned the use of media we focused our research on our informants’ attitudes to TV. For several years TV has been considered an effective media for propaganda and the large majority of the urban population knows about trafficking from television. But what did television and TV information mean for village women who were in danger of being trafficked? The Yi woman who had been sold told us that back in her home in Liangshan there was electricity but nobody in the village had a TV set, there were no newspapers, and people could not afford to buy books. In her present home they had cable TV for which they paid 0 yuan a month. Her husband’s younger brother, who was a migrant worker, had paid for the TV set just as he had paid for the house they lived in. Her husband suffered from epilepsy, so they had no money. When we visited her home we saw an adapter next to her old TV. The electricity supply was insufficient so they had had to spend over 100 yuan on an adapter in order to be able to watch TV. The woman next door told us that it was not everybody in
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the village who had cable TV, but that year the harvest had been good, so many families had installed it. In another interview a migrant worker from Jianyang said that in her home there was a black and white TV more than ten years old. It had cost 100 yuan, and it could receive two channels, but the signal was weak and they were too poor to install cable TV. They could pick up a signal now and then but the quality was never good.

So ‘watching TV’ was also different from what we had expected. The Yi woman told us that during the day she was too busy to watch TV, but she watched it in the evening. Her family liked to watch kungfu movies, and she had also learned to understand them. Her favorite series reflected family life, but she did not remember its title because she could not read. They could watch CCTV, but she had never heard of *Focus*, which is the most popular CCTV documentary program on social and political issues. Our talk with her demonstrated that not everyone in China is used to watching TV from childhood; that she did not watch TV during the day because she was too busy; that when she watched TV with her family in the evening she herself could not select which channels and programs they would watch; and that even when she could choose the programs herself she preferred programs with a limited content over the mainstream programs because of her low educational level.

So the kind of television our informants were thinking of was far away from the color TVs in our minds, which could receive dozens of channels in high quality. And, more importantly, because they had received limited education they found it difficult to relate to the serious ‘mainstream’ news programs and documentaries which the authorities normally use to conduct propaganda against human trafficking. If we wanted to reach them through the mainstream media with our ‘mainstream culture’ messages about trafficking we would have to take these differences in perceptions into serious consideration.

Insiders and outsiders ask different questions

Since labor markets are major venues for trafficking we visited the Jiuyanqiao Labor Market and the Huangwa Street Domestic Labor Market in Chengdu. Jiuyanqiao is the largest labor market in Southwest China, and it is visited by thousands of job-seekers every day. As soon as we entered we saw anti-trafficking posters produced jointly by UNICEF, Sichuan Public Security Bureau, Sichuan Provincial Women’s Federation, and Sichuan Labor Bureau. Inside the market we crossed the section for former state-employed workers who had been laid off before we got to the women’s section. Unlike the male workers, the women did not have a piece of paper in front of them describing their skills and the sort
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of jobs they were looking for. They explained to us that they were too modest to attract employers in this way. They were sitting or standing, all waiting for someone to come and pick them up.

On one of the walls surrounding the female workers’ section there was an anti-trafficking bulletin board set up by UNICEF, and a small crowd was looking at it. As soon as I entered this section I was surrounded by women who took me for a potential employer. When I told them I was a researcher they quickly disappeared. I walked over to a group of young women who were standing close to the bulletin board and found out that they came from Jianyang district and were all in their teens. I asked them if they liked the content of the bulletin board and whether they found the information useful. They did not reply.

I stood around feeling uncomfortable for a long time, then I walked over to another group and asked them the same questions but they did not take any interest in the bulletin board either. Some of them said it was useful, others said it was not, and I could feel that they were not taking me very seriously. I soon realized that there was something wrong with my questions, they were all the questions of an outsider. They aroused no interest among the insiders, and they did not know how to reply to them.

When I went outside the market together with my co-researcher I saw that even here there were many rural women waiting by the side of the street, so I sat down and started chatting to them. After about an hour a kind of mutual trust had developed, and I asked them whether they were afraid of being abducted when they contacted employers outside the official market. A 19 year old girl replied: ‘No, I am not afraid, I can always tell a trafficker.’ I immediately felt that I had discovered a topic worth looking into, so I asked her how she could tell them. All the women now started describing to us what a human trafficker at the labor market might look like: ‘Most of them are men, but there are some women as well’; ‘they never carry a bag’; ‘they never have an identification card or any other ID, or if they have one it is probably fake’; ‘they look for waitresses and promise high wages’; ‘they are only interested in pretty girls’; ‘they have a small truck waiting at the roadside, one of them starts talking to you and the others push you into the truck, and then it is too late to get out’; ‘they often turn up at the labor market’; ‘when he says he has got a job for you, you should look into his face, if he is a swindler he will lower his voice and look over his shoulder all the time. Those who are really looking for workers behave much more naturally’. Based on their descriptions I also learned to spot the traffickers. I saw a tall, good-looking woman turn down the suggestion of a trafficker, and I understood that the communication between the female job-seekers had much more effect.
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than the poster saying ‘Resolutely Fight the Criminal Activity of Trafficking Women’. They had to persevere in looking for jobs, but knowing how to spot a trafficker had become a basic skill of their trade. I now felt that the time was ripe to invite them to a restaurant. They first politely declined, but in the end six of them joined us.

At the dinner table I asked them: ‘Except from knowing how to recognize a human dealer, what other measures do you use to protect yourselves against being abducted?’ Then two of the women started telling us about how they had been abducted. A 38-year-old woman recalled:

I was kidnapped 18 years ago, but I managed to escape. I met this woman at the labor market who said she had a job for me out of town, so I went with her. It was somewhere near Shantou in Guangdong province, and when we got there she said I should stay with one of her old schoolmates. She was talking to the people there in Cantonese, so I did not understand a word, but when I pretended to sleep I saw through the mosquito net how that guy was paying her money. The man who had bought me was short and ugly, and he called me ‘pretty’. I first stayed with his mom, and on the third day a crowd of people came to the house to drink. When they began to leave his mom also disappeared, and I knew something was wrong, so I raised hell and said I wanted to go. They couldn’t really do anything about it, and some nice people in the village helped me buy a ticket back to Guangzhou, so I managed to escape.

This woman said that at the time she was kidnapped she had never heard about trafficking in women, she only got suspicious during the event.

A 28-year-old high school graduate said:

I was also in danger once at the Jiuyanqiao Labor Market. A man came up to me and said I looked familiar, and that he had a job for me. He said he definitely would not sell me off. When we got to the highway I got suspicious so I said my feet hurt, and that I would not go with him. He threatened me and said that he would settle with me if he ever saw me again, but I told him I was not afraid of him. When I met him at the market the next day he did not dare to do anything.

One job-seeker said: ‘I have a little niece who was kidnapped and brought to Shanxi, but she is doing OK and she does not come back very often.’ Another woman said that eight girls had been kidnapped from her village. Their conclusion was that if someone said to a girl that she looked familiar, or offered her a job in a far-away place there was reason to be wary. The 38-year-old woman said: ‘They prefer little kids who are new here. They are easy to cheat, and they fetch a good price.’
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During the meal they told us how they had learned to prevent trafficking. They would, for example, never tell their real name and address to a stranger. They also said that the names and addresses they had given to us at the start of the interview were all false; they always carried their ID card and a contact phone number in their bra; they would often look for a job together with a mate; some girls were accompanied by their mother or another relative who would not leave until they knew where they would be working, and so on.

I felt this was a very fruitful evening because now I had some information about how village girls protected themselves. I had abandoned the question ‘Do you find the bulletin board useful’ and started to find out what sort of questions made sense to the job-seekers. ‘How do you spot a trafficker?’ was their own question, and it was essential for their survival that they knew the answer. When they answered this question their experiences and strategies for preventing kidnapping came to the light of day.

To understand the insiders’ knowledge and strategies

This conversation made me realize that when rural female job seekers get to the city they are soon hardened by their experiences and acquire their own strategies for finding a job. How to tell a trafficker and how to keep one’s real identity secret is extremely useful knowledge. To fully grasp such knowledge and strategies is often a big challenge for a researcher solidly planted in mainstream society. In order to learn more I went to the Huangwa Street Domestic Labor Market.

This market is a famous center in Chengdu for hiring ‘amahs’. Young girls from the countryside come here looking for jobs as nannies and housemaids, and we soon saw around one hundred female job seekers sitting in the courtyard waiting to be picked up. But outside, in a street opposite the market place, there were also scores of women sitting at the roadside waiting for employers – this was the strictly illegal unofficial labor market. The risk of trafficking is normally considered to be much higher outside than inside the official market, so why were all these female job seekers willing to run the risk of placing themselves outside?

Just like the day before we invited some of them to have lunch with us. They told us that it was too expensive to be in the official market, and that there was no real protection there anyway. Then they started telling us the story of their ID cards. I had actually seen a pile of these cards at the entrance to the official labor market, but I had not realized how important they were for women who came drifting into an unfamiliar city looking for jobs.
If they wanted to offer their services inside the market they had to hand over their ID cards in addition to paying a fee. When they found a job the card would be given directly to their employer, and they themselves would not even be able to touch it. This was unacceptable to some of the more independent-minded women. One of them told us that once when she found a job in the official market her card had been given to her boss.

I had a valid contract with my employer, but after a month he did not pay my salary. There was nothing I could do about it, because he held my ID card. So I made a phone call to the local Labor Bureau, and they told me that it was illegal for my boss to confiscate my card. They were of no use, however, because they asked me if my boss had a business license, and when I told them he hadn’t, they said it was hard for them to do anything. Then I made an emergency call to the police, and when they came he had to give me back the card, but the police made me apologize to them because making an emergency call for this reason was a ‘shameful thing’, as they said. But what could I do, the Labor Bureau wouldn’t do anything about it.

Why did the women have to hand in their ID cards, and why were the cards handed over to their employers and not to themselves? From our conversations it turned out that the authorities wanted to guard the employers against the women. One woman said:

Sometimes an employer hires me and really trusts me, but the official will tell him ‘you’d better take your precautions and get her ID card so you can report her to the police if she steals some of your stuff.’ We know that it is a violation of the law when they confiscate our ID cards, but that’s what they do, and we can’t stop them.

The women outside the market could not tolerate this behavior. ‘This card is more precious to me than my life’, one of them said firmly. Another job seeker said that sometimes the officials even scolded them:

They treat you like some ignorant kid. One day one of the female officials told those job seekers who still had not found a job that they should start considering why nobody wanted them, when so many others had found employment. ‘Isn’t it because you are...’ and then she said some dirty words that I will not repeat. So I won’t go to the official market, I have too much self-respect to be humiliated like that.

The officials were often rude to those who were standing outside the official market, because they were seen as ‘disturbing the social order’, and sometimes they would call in the police to arrest or fine people.
From the perspective of the female job seekers, the market authorities, the employers, and the police may want to ‘maintain public order’, but they start out from the assumption that the women ‘may steal’ and ‘need to be strictly supervised’, so the price for their ‘order’ is that the female job seekers loose their freedom and dignity. This is why officials meet resistance from the women who feel isolated and cut off from help. To operate from outside the official market is a way of resistance they choose when they are facing social stigmatization. Their actions are called illegal, but they are just ‘the weapons of the weak’ (Scott 1985; Guo 2002). As part of their silent resistance the female job seekers have developed their knowledge about how to defend their ID cards and their strategies for operating outside the official market. The fact that they asked for my ID also shows their vigilance and the active role they are able to play in protecting themselves.

CONCLUSIONS AND CHALLENGES

This project first of all taught me the importance of going to the field. This may sound like a very evident point, but every time I went to the field to observe and conduct interviews I realized just how necessary it was. Through my fieldwork I learned that earlier propaganda contradicted the knowledge and strategies that the rural job seekers had gained when they came to the city. Even the ‘protection’ of the authorities was harmful to them. The project also made me reflect on my role as an outsider. It is still a challenge for me to see things from the insiders’ point of view. In the course of the project I discovered many facts, and after analysing these facts we revised the earlier information strategy, but I am sure that much remained hidden from me, I only saw the tip of the iceberg.

I also learned that in order to see things from the insiders’ perspective we must first discover our different ways of perceiving the same facts. We must uncover what sort of questions the insiders find it important to ask, and then thoroughly discuss these questions. Only when our research questions and hypotheses interact with those of the insiders will we be able to get useful information.

Before I started on this project I knew about trafficked women mainly from documents, statistics, and media reports, which often described them as a weak, passive, and ignorant group waiting to be rescued, but I realized that they had their own knowledge and their own strategies for protecting themselves against trafficking, and that through their resistance they wanted to take control over their own destinies within the limited space available to them. I am sure that any
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weak and marginalized group has its own survival strategy, which the researcher can discover and understand if she momentarily drops her preconceived ideas.

Although we got much valuable information from this project, winning the trust of our informants remains a difficult issue. Female job seekers are in a marginal and dangerous position, and they do not easily trust outsiders. In my report I described them as birds that start at the mere twang of a bow-string. Because I am a woman and because I wear glasses I was, in their eyes, a reasonably reliable ‘intellectual’. Once they had checked my ID card and name card and believed that I was not a trafficker myself they agreed to eat with me, although there have been cases where women have been kidnapped after traffickers have put tranquilizers in their food or drinks. They still found it hard to trust us completely, however, and they kept some personal data hidden from us in order to protect themselves. They even corrected information about their mailing address several times during the interviews, and when I sent some photos to the addresses they had ended up giving me, three out of six letters came back with a note saying ‘person unknown at this address’.

Another problem concerns identification with the informants. In this project I had no problem identifying with the female job seekers, their knowledge and strategies. Although I claimed to myself that I held an objective view, I felt deep in my heart that they were being treated unfairly, and that they deserved understanding and efficient help. If I came across informants whom I did not trust and whose basic values I was unable to identify with I would not know how to proceed with my research.

A final challenge is how to find a way to further develop this project. The project has showed that the female rural job seekers are not passive and weak but have their own strategies to prevent trafficking, but how do we cooperate with them in order to discover more facts and further improve their environment and anti-trafficking strategies, this is the question we have to consider next. My cooperator Qiu Linchuan has trained a small survey team that consists of former rural job seekers in Guangdong, and they have produced their first results. In this way the rural job seekers’ participation in the research process is developing into a process of empowerment.

Because of the researcher’s own background, he or she can never be equal with the insiders, but if we want to ask the right questions to the insiders we must, through arduous efforts, find their perspective. Even when we believe we have found such a perspective it is hard to prove that we are completely objective and not just constructing their viewpoints based on our own established truths and experiences. The researcher will often, based on his or her accumulated professional knowledge, place the questions that she discovers during fieldwork inside a
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preconceived framework. Maintaining a sensibility to the insiders’ perspective can help us understand their actions and strategies. Even if we do not discover any absolute truths we can, at least, get somewhat closer to the realities. The essence of qualitative research is that it can construct and interpret a part of reality based on what grows out of the fieldwork – rather than on the researcher’s a priori theories and knowledge. I understand qualitative research as a process of pursuing the construction of reality based on the results of fieldwork. The large majority of researchers, both foreign and Chinese, must go through this process.

Translated from Chinese by Stig Thøgersen

NOTE

1 In 1986 and 1994 China designated 331 and 592 counties respectively as national level poverty counties based on their annual average per capita income.

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The benevolent see it and call it benevolence. The wise see it and call it wisdom. – *The Book of Changes: The Great Appendix*

**INSIDERS AND OUTSIDERS**

Discussing fieldwork, positionalities are often simplified into the mutually excluding categories of insiders and outsiders. Discussing fieldwork in China, this simplification is sometimes in addition conflated with a Chinese-foreign dichotomy that leaves little room for the sophistication and ambiguity we have come to expect from the methodological literature of other fields of the social sciences. Indeed, these twin dichotomies allow little room for most researchers of flesh and blood since few if any fit the racial and cultural stereotypes that come together with them, and needless to say they leave even less room for variation among the people on the other side of the notebook. In reality, any researcher will keep, or will be kept at, a certain distance in the field, whether working in what one considers to be one’s own society or in a foreign. On the other hand, it is usually as impossible to be an outsider in fieldwork as it is to be an insider.

The tendency to simplification into outsiders and insiders, and into natives and non-natives, has as solid a history in China as it has elsewhere. Moreover, the latter distinction is a not easily dismissed social fact well entrenched in popular
Thinking and fortified by regulations governing most international research in China as well as by official discourses on ethnicity. This dichotomy is also nurtured within the frame of indigenous social science, a movement whose rationale is highly compatible with, and already partly tangled up with, a kind of cultural nationalism often found in post-Mao China (cf. Guo 2004). I have discussed problems and potential dangers of this thinking elsewhere and suggested that an understanding of ‘nativeness’ in terms of disciplinary and organizational belonging rather than in primordial terms of blood and culture would lead to a more satisfactory understanding of the researcher’s relation to the people he or she studies, to his or her research, and to other researchers (Kjellgren 2003).

Widening the angle, stepping back from the confined disciplinary spaces within which we acknowledge the importance of the individual mind rather than the institutional framework we more often see structuring our subjects’ behaviour than our own, I believe we can understand how the institutional belonging of the researcher is by far the most important of the factors that condition a researcher’s study and set the parameters for the final product by furnishing him or her with the choice of certain theories, methods, terminologies, research agendas and publishing strategies. However, zooming in on the details of the fieldwork, where our data is created, the idiosyncrasies of the individual researcher seem all the more important.

In a recent publication, Mao Zedong’s rural investigations carried out from 1927 to 1934 are held up as still valid models for anthropological fieldwork (Qin 2003). One major strength of these investigations, the author points out, was Mao’s decision to use local research assistants from different social backgrounds, which ensured that no vital facts remained unnoticed. Applying conventional terminology, it would seem Mao was checking for measurement error caused by instrumentation in order to strengthen his study’s validity. The logic builds on a commonsensical way of thinking about research as fact-finding, and of individual researchers as biased instruments. Mao was primarily interested in land distribution, but the significance of this understanding is no less salient when we are looking for intangible things like personal knowledge, feelings, memories and attitudes, i.e., the kinds of things we often want to explore in ethnographic interviews. The first character in the Chinese word for interviewing, caifang, means ‘to collect,’ and interview training, increasingly detailed interview manuals, and methods such as computer assisted telephone interviewing all strive to cancel whatever bias the individual interviewer may introduce into this process of collecting.

This is of course only one way of thinking about the interview and even if few of us are willing to inflate our egos to more than a certain degree many researchers
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would probably feel more at ease with a characterization of the interview as more of a dynamic creational process. Either way, although for different reasons, the positionality of the individual researcher remains crucial and potentially problematic in most ethnographic studies. In this chapter, I shall reflect on the impact of the individual researcher’s social standing in the fieldwork situation drawing on personal experiences from Shenzhen’s Special Economic Zone.

IN AND OUT OF THE SPECIAL ZONE

My fieldwork was part of a study of the production of locality focusing on the everyday ideational landscape of the Special Economic Zone of Shenzhen, for many years one of the localities most intimately associated with China’s reform programme, and geopolitically as well as culturally often seen as being positioned between China and the world, or between Maoist traditions and Chinese modernity. However, in ways that I guess are familiar to many social scientists, my choices of topic and fieldwork were not so much theoretically informed as they were the results of coincidences, stray itineraries, unintended encounters, personal desires and perhaps yuanfen, destiny. But, I shall try to be coherent and fast-forward to the point of entry. Suffice it to point out that it was a stray visit to Shenzhen that first made me interested in the Special Economic Zone. Staying a few weeks in Shekou – ‘the special zone’s special zone’ – in the winter of 1991–92, I realised that many of my ideas of China stemming from books as well as from living there, did not fit Shenzhen reality very well.

The mere fact of how I stayed there contrasted in a refreshing way with the enclosed and well-guarded compound for foreign students – known on campus as the ‘panda zoo’ – where I was staying in Chengdu, studying Classical Chinese. In Shekou I was residing with a part Hong Kong, part Guangdong family in an ordinary housing complex, a foreign male living with a Sichuanese female lodger, something at that time unthinkable in Chengdu for moral and social as well as legal reasons. Furthermore, people in Shenzhen told me things about life that I had not expected; this was especially true with regard to their accounts of social relations with neighbours, workmates and business contacts, often described as, respectively, lacking altogether, socially distant, and conducted without guanxi, i.e., being ‘business-like.’ Although relations such as these – often accusingly characterized as ‘lacking in renqing’ – had long been seen as archetypical for urban life in other parts of the world, they deviated significantly from the then still dominant social pattern in urban China. In Shenzhen, they represented attractive alternatives to
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this prevailing mode of life, and their partial realisation explained, justified and helped sustain the social reality of the Special Zone (Kjellgren 1999).

Back in Chengdu, where more traditional moral and legal standards still applied, my Sichuanese girlfriend and I later on got married, landing me amongst many other things in the admittedly ridiculous-sounding social position of Zhongguo nüxu, ‘son-in-law to China.’ A position that in no way makes me appear less white in the eyes of the world, nor by itself contributes many cultural skills or knowledge, but which has certainly informed my self-perception as well as my emotional understanding of many things Chinese and non-Chinese.

Those informal chats in 1992 about social life and special zone behaviour gave me a much-needed reminder of the complexity of Chinese reality but they also brought forth an interest in this special corner of the country. Although at the time the reasons to me seemed only indirectly connected to the ‘grand architect of reforms,’ I was soon to be caught up in the same Special Zone Fever as the rest of the country following Deng Xiaoping’s Southern Tour of 1992.

In the summer of 1994 I was back in Shenzhen with what I deemed to be a reasonably well-designed plan for fieldwork. Access was not an issue, and asking for official approval or a host danwei did not even enter my mind as a possibility. This choice had much more to do with my academic affiliation than with my lack of respect for official research regulations, fear of red tape or other ‘routine difficulties’ reported on by foreign researchers in China. After June 4 1989, departmental ties and attitudes to Chinese authorities were all but frozen in Sweden for many years (indeed until September 11 2003, the date of the Mid Autumn Festival that year, an auspicious day for official reconciliation), and coming as I did from a largely text-based discipline, sinology, does not foster awareness of formal fieldwork procedures anyway. My institutional ancestor Bernhard Karlgren wrote somewhere that it was doubtful whether there existed a field larger than the sinological. That was perhaps only a piece of rhetoric, but when you are there, in the sinological field, his words do make sense, and are also mirrored in, for example, Tan Chee Beng’s suggestion that Chinese anthropology is best conducted in a world-wide ‘Chinese ethnological field’ (2004: 9-29). Indeed, although I have all those stamps in my passport to tell me differently, I still do not think of China primarily as a country that I can enter and leave at will given the good grace of the authorities. It would be more true to say that I entered the Chinese reality slowly in the mid-80s and never came back again. Shenzhen was for me much more part of both the world at large and of my own personal history than it was a ‘field.’ However, in conventional terms, the warm summer of 1994 was my first spell as a ‘fieldworker.’ It was spent living with the Sichuanese–Cantonese Qian family.
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My research, like that of so many Chinese and non-Chinese scholars before and after me, was in the methodological tradition of ‘opportunistic research’ (pace Riemer 1977). The household I stayed with was headed by a childhood friend of my wife, a Ms Qian from Chengdu, and consisted besides her of her Hong Kong boyfriend (born and raised in Guangzhou, married with children in Hong Kong as seemed to be the typical case), her elder sister who was in-between marriages and jobs at the time, her eccentric old mother, two recently hired girls from a little township in Anhui, and a seasoned domestic worker, baomu, from a mountain village in Sichuan, not far from the city of Renshou.

Funding my research took longer than expected, but working as a tour guide travelling in China in 1993–95 gave me unplanned opportunities to continue collecting stories, impressions and images of Shenzhen. Far from Shenzhen, I had the fortune to interview some of the many who have been to Shenzhen and who have left for various reasons. This large and geographically widespread group was one I had not considered when initially planning my research, but whose insights in important ways complemented my own understanding of Shenzhen.

The thought of multi-sited fieldwork, or more specifically of travelling fieldwork, clearly tempting when working with migrants, had early on to be rejected for financial reasons. In fact, at least in the beginning, free lodging while in Shenzhen also seemed a prerequisite for research, and throughout my research I had to keep a rather low economic profile, which I think over-all served me well in getting rapport with all but the very rich, framing me as a relatively harmless figure despite my otherwise obvious foreignness.

In 1995 I once more worked in Shenzhen staying first with Ms Qian and later with a Sichuanese friend of hers whose household structure was fairly similar. This sojourn was supposed to be primarily spent getting feedback about what I had already figured about the zone by then, but new issues emerged – change is at the heart of any capitalist society – and I also started more systematically to collect printed sources to complement or corroborate my interviews and observations. In a complex and highly self-reflexive society like the Chinese, media both carry and create much of what I wanted to catch on tape; furthermore, paper has a more consistent memory than men.

My initial idea of collecting data only through interviews was partly guided by the naïve thought that while many researchers could read the printed stuff, I was myself in a very good position to collect the ‘thoughts of ordinary people.’ Little did I then understand that there are no ‘ordinary people.’ Little did I understand then that in order to assess the character of the thoughts and memories as I got them in interviews, I had to know roughly to what extent they were public, maybe
even official ones, and if so, to what extent and in which ways they differed from this/these public discourse/s.

In the end, although there were interesting non-overlapping areas, I re-discovered most of my interview topics in official documents, local papers and books, academic articles, popular literature and songs, and TV dramas. Given the limited freedom of Chinese media and the restrictions imposed on all public discourses, my informants’ narratives naturally often deviated from these in terms of attitudes and conclusions. However, the tales they told me were often found to be fashioned in narrative moulds similar to those of the printed and broadcasted ones.

It is impossible to say that a factory girl’s tale as told to me was only a copy of one given in a popular TV-series about factory girls, written by a former factory girl. We all use other people’s language, and my interviewees tended to speak different languages, from the formal lingo of the People’s Daily to the sentimental lyrics of Taiwanese singer Lo Tayu, from the lines of the local TV soap opera and Hong Kong slang to classical quotations, all by way of glocalized Wall Streetese or institutionalised sukū, ‘speaking bitterness.’

As impossible as it is to say that any of these languages had more authenticity than the rest, I do think it was important to recognise them, because understanding the linguistic references brought my interviewees accounts into relief, and guided me to the complex of meanings of which their tales were parts. In my case, however, the desire to see if I could find written (or sung) accounts of issues discussed and not discussed in interviews, gave me much more than I had really bargained for and even if I do believe in complementing, or triangulating, sources and methods of analyses, I would like to put up a large sign of warning for data overload.

My next stay in Shenzhen was planned to coincide with Hong Kong’s return to China on July 1 1997. To me and most Shenzheners during the officially ecstatic months leading up to the event this seemed – perhaps mistakenly – to be an even more important date than when Deng Xiaoping had visited the zone in 1992.

Once more I stayed in Shekou, not far from where I had stayed on my first visit five years earlier, but with another family, the Chen’s. This was a modelsized Sichuanese–Shenzhenese household (two parents with Shenzhen household registration, a four-year old daughter) where the woman’s father in his second marriage had married a relative to my in-laws. Not too close, but close enough to get me somewhere to stay in a very unrestricted way. Compared with earlier spells in the zone, I spent little time together with this family, since the daughter attended kindergarten and the parents both worked long hours outside the home. Still, being related gave to our everyday interaction an unstrained character that I much appreciated.
Destined by the Swedish school system in which I taught to return to Shenzhen only when the city was at its hottest and wettest, I made another trip to Shenzhen in the summer of 1999 to find out what had happened in the zone since the return of Hong Kong to China two years before, and what impact the so-called Asian Financial Crisis had had. Printed data were also collected in 1999 at the libraries of Sichuan University and Peking University. My last visit, a brief stay where I tried as best I could to avoid generating more material, took place in the autumn of 2000 by which time the Internet had long made daily news from Shenzhen available to me regardless of where I happened to be.

Amongst Potentially Important Strangers

It is conventional wisdom as well as the teaching of our standard manuals that social commonality in terms of gender, ancestry, class, occupation etc., is the best way to gain access, establish rapport and thus eventually gain the ‘insider’ understanding we usually are looking for in ethnographic fieldwork. Granted this and the widely popular Chinese-foreigner dichotomy, it would seem difficult for most foreigners to conduct research in China. Indeed, given the complexity of the country it could be challenging work for many Chinese researchers as well. Luckily, there are many ways of finding common ground.

From the Chinese psychologist’s perspective, David Ho (1998: 6-7) offers us a mixed list of interpersonal relationships that gives a hint of the wide variety of factors that can serve as this factor of commonality. His list starts with relations based on blood and continues with connections based on birth and social in-groups, nationality, political power, force, social class, occupational belonging, residential belonging, institutional affiliation, social belonging based on ascription or achievement, tutelage etc., professional contacts, companionship, and ends with casual encounters. In short, almost anything will go. This is consistent with my own experiences in Shenzhen and elsewhere. Besides the more usual ways, I have established initial rapport with people also on account of things like my Chinese surname being the same as someone’s wife’s sister’s husband’s (jokingly: ‘four hundred years ago we were one family’), or on the fact that I eat pork (very seriously: ‘so then you are a Han after all’ [as opposed to being Hui]).

Side-lining official regulations, and utilizing my qundai, i.e. working through my wife’s relations, to gain initial access and somewhere to live, of course gave me more substantial platforms of commonality in the Qian household, but closeness is not uncomplicated. The household members became my first interviewees in
the summer of 1994. As could have been expected, these were slightly problematic interviews. As a guest of the household head, and as the husband of one of her best friends, I had some sort of credentials, but I also occupied a peculiar intermediate position that could not but influence my conversations and interviews. I could for example not ask the family head or her Hong Kong boyfriend too personal questions on account of my wife’s close relation to Ms Qian (either I should know the answers to those questions already, or I should not ask them at all), and the three employees could not refrain from using me as a potential messenger in close contact with the same household head. Neutrality in fieldwork, as Liu and Cheung states (1998: 230), ‘is not a simple position to keep.’ I would say that it often is an impossibility to start with.

Later I used two ways of finding less socially entangled interviewees. One was simply to strike up conversations with strangers. This was effortless but time-consuming, and it was trial and error as far as their status as potential contributors was impossible to judge beforehand, but it was often rewarding and almost always interesting. The more frequently used way to locate people, especially when I knew better what kinds of persons I was looking for, was to ask if someone I had already talked to knew anyone meeting my criteria who perhaps could be asked to help me out with an interview? Shenzhen is a young city with an amazing social turbulence where weak social ties make all the difference, and although it did not always work, this theoretically informed snowball sampling proved effective. Moreover, already by the first link, and certainly by the second, there would be a distance between my interviewees that insured a secure and reasonably well-defined interviewer–interviewee relationship without more than the lightest of renqing indebtedness involved.

In total, I conducted in-depth interviews with more than 60 individuals, some of which were recorded while most were documented in notes and/or mind-maps. ‘You definitely want to avoid carrying a tape-recorder if you want people to talk’ was one piece of sincere advice I got from a senior Chinese-American scholar in 1994. But, working in Shenzhen proved to be working very far from the ‘culture of fear’ that Mayfair Yang (1994: 15–25) described as a major obstacle to research and even as a threat to the mental health of the researcher. The worn saying ‘the sky is high and the emperor far away’ seemed to apply when Shenzheneros told their stories to me, even when discussing politically sensitive topics. Embarrassingly, technical failure and not always carrying the tape-recorder were the main reasons I did not tape more than I did, something I truly regret since no notes in the world can compare to taped or, better still, video-taped interviews – not least as material for educative self-reflection.
In conducting my interviews, a problem connected to my foreign complexion was that my interviewees as a rule felt the need to assess my level of understanding (some of them did not, or at least acted very coolly indicating that they either did not care, or that Chinese-speaking foreigners were not new to them). First, did I understand enough Chinese to understand them? Second, did I understand enough about Chinese society and history to grasp what they were saying? And third, did I know enough about Shenzhen to be able to understand what place, institution, person or event they were talking about? This phase of uncertainty as a rule formed only a brief part of any interview, and I did not feel then that it hindered my work. Quite the opposite, some sort of trust must of course be present in an interview situation, and since my complexion made these uncertainties obvious, they could also be dealt with more explicitly than would perhaps otherwise have been the case.

Once past this state, my interviews as a rule worked without problems. But, of course, these introductory remarks, innocent and essential at the same time, could not but help set the agenda in different ways during the years I was working in Shenzhen. Not only did my Chinese improve (I was fluent when I started, but fluency is only part of what there is to language command) but my knowledge of China in general and Shenzhen in particular increased. While in 1992 I knew exceedingly little about Shenzhen, in 1999 I usually knew more about Shenzhen than my informants. Displaying this knowledge or lack of knowledge to some extend triggered different stories, with the role of ‘local expert’ being in a sense shifted from them to me, a shift of course only I could be aware of. When I found my later informants who had recently arrived in Shenzhen to be much less aware of anything being special about the special zone than my earlier informants had been, I took this as confirming the then old complaint ‘the Special Zone has become like the Inland, and the Inland has become like the Special Zone.’ While this popular summation of local history must be basically true given the trajectory of China’s reforms, I now wonder to what extent it really was me who had normalized the zone over the years, no doubt generating a somewhat jaded outlook that I perhaps unconsciously managed to get across to my informants together with my increasingly narrow curiosity?

The basic curiosity of the researcher naturally begs the interviewee to take on the role of teacher or informant, but unless you conceal your knowledge, which anyway would be very hard to do in a natural way during a long interview, your local knowledge can sometimes be a hindrance. Then again, I early on understood that the only real gratification I could offer my interviewees was to lend my ears and let them tell their stories the way they wanted to. This
utilitarian and respectful attitude, I think, made very few of my interviewees feel intimidated, and doubtlessly being an alien helped a lot. Even on their first day in the city, my interviewees were still closer to home than I was, and at best saw in me more a ‘fellow outsider’ than a ‘local.’

Working in the officially ‘international’ city of Shenzhen, my blue eyes were among the first things my interviewees saw in me even when I got to them through referral, and from this coloured understanding they had to form their impressions about my research and me. I believe this served me well, attracting positive attention and expectations since I could be thought to provide anything from a good laugh to a chance to a better life through business deals or marriage, or both. The positive stereotypes associated with white foreigners of course only got me that far, but that was often far enough. Once initial contact had been established, I do not think it mattered too much that I was not the unmarried rich American business opportunity some would have loved to meet. With a few notable exceptions of deep disappointment in me, I thought the ways in which my marital, economic, and national status deviated from the ‘ideal’ laowai worked in my favour since it was harder but also less necessary for my interviewees to fit me into their pre-understandings of what a ‘foreigner’ should be like, and how to interact with one. Moreover, Europeans and Chinese can often find common ground in discussing our complex emotional, cultural and material relations to America.

Perhaps more importantly though, for all but a few interviewees, my foreignness served as a sign of security, signalling that I would take their personal stories far away from Shenzhen. As a contrast, I have later found it harder to interview Chinese persons in Sweden about details of their lives and doings. In Sweden, my informants’ stories will stay geographically close, and despite my insurance of anonymity and scholarly purposes, many interviewees obviously fear that their stories, if too frank, may bounce on them. In Sweden, I may be as nice a guy as in Shenzhen, but my personal qualities are there often overshadowed by real and imagined connections to ‘the establishment.’ As fieldworkers, we are representing far more than ourselves.

However, there are few coins with only one side to them, and I guess one example of the negative aspect of not having a local institutional basis was that my informants saw little point in giving me detailed recommendations of things that could be improved in Shenzhen. Had I been working for or under the auspices of the local government, I would probably have been utilized more as a potential messenger to the people in power, in much the same way as the hired hands in the Qian household tried to use me, or indeed in the way many Swedish Chinese
want me to forward to the public the ‘reassuring’ image of a model minority. This may perhaps not sound like too tempting a scenario, and it would certainly have meant less relaxed interviews, but when people try hard to get certain things across they generally convey much more than they intend to. In Shenzhen, as luck would have it, I had second-hand access to some of what was said to the government’s representatives through reports by local researchers, but there was no making up for not having local colleagues to discuss with – and perhaps be stalled by. As always it would have been sweet to be ‘both and,’ to be able to have both fish and bear paw.

Over the years, I met a few of my informants several times, but unfortunately most of the people I had talked to proved impossible to locate on later visits to the zone, or even later during the same stay. The turn-over rate of Shenzheners is high, and with few exceptions, addresses, telephone numbers and e-mail addresses seemed to expire after a few months or at best lead me to someone else who could only very vaguely tell me in what general direction my old interviewee had taken off.

As far as sampling is concerned, I adopted another research strategy to facilitate understanding. My ‘core group’ of informants were people from cities or townships in Inland China that were or were striving to become middle-class (in a loose sense of the term, a few of them were rather wealthy even by Shenzhen standards while others were quite poorly paid) and belonging to the ‘cultural cohort’ born in the period between the Great Leap in 1958 and the death of Mao Zedong in 1976.

They belonged to the biggest age group in Shenzhen, and I assumed that people from this group would be more acutely aware of differences than people grown up in Guangdong, and also more aware of them than older and younger Shenzheners. I further assumed that this group would be more attracted to, and less disapproving of, aspects of life in Shenzhen than would many of the more temporarily sojourning members of the lower-paid labour force.

Behind the rationale was of course also the fact that this was an analytical group to which I had instant access by way of friends and relatives, and to which I could more easily relate, belonging not only to the same group in my own Swedish way, born as I am in 1966 into a middle class family with working class roots, but also as them looking for a future somewhere far from home.

As it turned out – surprise, surprise – the main group of interviewees did come from Sichuan. This easily rationalized fact – Sichuan was held to be the biggest supplier of Shenzhen immigrants after Guangdong province itself (Shi 1999) – gave me an additional advantage since I could also relate to them through my own experiences from Chengdu, and work without any linguistic problems as far
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as dialects went. Although a few of them had no secondary or tertiary education, most of them did, which also made my status as student seem less distant. Not that all of this made me feel as ‘we’ with most people, but it certainly narrowed the gaps.

That my snowball sampling tended to keep me within the ranks of the ‘Sichuanese army’ was in itself somewhat enlightening. Although not strange in itself – ‘the reform politics put wings on us’ as Chen (1999: 237) proudly writes about his fellow Sichuanese – the dialect-based networks that were invoked were interestingly juxtaposed to my core informants’ solid statements about how little, if at all, geographical origin meant for social interaction in Shenzhen.

The relative ease with which I conducted fieldwork had, as noted above, a lot to do with Shenzhen being a city of migrants. In a locality where success or failure are seen as the responsibilities of the individual, in a place presumably packed with opportunities, it is important to be constantly watchful in order to not miss these as they come; or to create them whenever possible. This no doubt facilitated meetings, but the fact that the interview situation many times was the only situation in which I met a particular individual introduced another facet of Shenzhen social interaction. In a ‘city of strangers’ the individual is, to a degree not possible either in the village or in the traditional danwei, free to re-create herself, or at least to re-present herself. Whereas who and what you are often are established through communal understanding – ascribing an identity to you over which you do not necessarily have much influence – newcomers to the zone were both free and obliged to make fresh starts or reveal only selected sides of themselves (Kjellgren forthcoming). The conscious discontinuation of a person’s identity, or her ‘selfsameness’ to use Erikson’s (1982) term, helped many migrants overcome potential problems of adaptation to the new sociocultural standards of Shenzhen, and in my own way I was doing something analogous when shifting from classical sinology to sociology. However, this certainly introduced a fair amount of uncertainty since I had absolutely no way of contextualizing my informants’ narratives the way it could have been done in a village or any other more bounded social setting. I could only hope that I got the story my informant wanted me to have there and then, but then again this is of course often the best and most attractive deal we can come by (Arras 1997).

It is hard and maybe not necessary to draw a solid line here between the interviews I did solely for my investigation and the many conversations that formed a constant part of my stays in Shenzhen. Interesting topics could well be discussed with several people at the dinner table functioning somewhat like a focus group interview in disguise. The difference between what was said in the
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interviews and what was more ‘publicly’ discussed also helped me corroborate ideas mentioned in the interviews, where as a rule I always promised my interviewees anonymity – something they themselves only very rarely seemed to consider an issue.

These everyday conversations were also complementary in another way. Whereas my interviews usually had a life history approach focusing on their migration to Shenzhen, dinner table conversations were much more here and now. Gossip, a fundamental moral force in every society as well as the primary medium for informal information, together with general remarks about today’s news, gave me more of an understanding about everyday topics and everyday morals (gossip in Cantonese is literally ‘right-wrong’), and helped me bring some of my interview data out in relief. To a certain degree, these everyday conversations also made up for some of what I think I had lost by not having a local institutional affiliation, or a more permanent life in the zone.

To complement the views of my core group from the Inland, I conducted several interviews with individuals born and raised in Guangdong and Hong Kong, with poorer ‘migrant workers’ (mostly from Sichuan and Hunan), and with people older and younger than the core group.

The fact that I did not possess more than the most rudimentary skills in Cantonese did not influence my ‘formal’ interviews since all of my Cantonese-speaking interviewees spoke Standard Chinese (putonghua) well enough (unlike some of my Sichuanese interviewees). However, it was still a real hindrance for me since Cantonese – the language of modernity and the hallmark of the seasoned Shenzhen as well as the numerically biggest dialect in the Special Economic Zone – was often the dialect of choice in linguistically mixed company. Indeed, many conversations started out in Mandarin only to shift into Cantonese as they got more intense. Leaving me the choice of cooling them by calling attention to my ineptitude, or waiting for them to return to Mandarin, at which time I could jokingly ask them what conclusions had been reached, and on what topic. Cantonese-speaking Hong Kong TV was also much preferred in the evenings, especially for news reports. The only positive side to all of this was that I could identify very easily with the frustration of newly arrived migrants, but this was of course scant comfort.

I have already said that I generally found communication with people from my analytical core group to be the least problematic, as indeed it should be according to received wisdom. There was, however, danger lurking in this effortlessness. According to theory, self-disclosure, if done in a sensitive and progressive way ‘mimicking real friendship,’ leads to self-disclosure, and, supposedly, to better

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interviews and richer data. But, there is a strong tendency in an informal interview situation, as in most friendly conversations, for the parties involved to accommodate each other, for example by stressing points that are believed to be shared values, experiences or interests. Thus, the more self-disclosure, the less likely we are to get stories that fundamentally differ from those we are prepared to accept, or ready to hear. The more we feel to be on the ‘inside,’ the more likely we are to take understanding for granted, assuming that we know what our informants mean instead of probing this with questions. In formal interviews the impact of our own personalities is restricted by the questionnaire and balanced by the code of conduct laid down in the manual, but in most qualitative interviews the opposite is true. The more relaxed the situation, the better the rapport, the more likely we are to hear exactly that which we want to hear, which of course is often the opposite of what we really want to hear.

Common manners, interview etiquette, as well as gratefulness for the time people set aside for our meetings in this place where ‘time is money’ had been an official slogan already in 1982 all contributed to make the interviews highly civilized affairs. In contrast, conversations defined as between friends can more easily derange, revealing unflattering but informative sides of people and their ideas. Memorably, I was thrown out on the drizzling Shenzhen streets in the middle of the night by a well-known writer after an argument over race and racism. (I had increasingly impolitely argued against the existence of a natural race hierarchy with ‘White’ on top, ‘Yellow’ in the middle, and ‘Black’ at the bottom). Upsetting as it was at the time, this incident gave me an opportunity not only to reflect on racism, but more importantly on how even disagreements on very basic topics could easily be neglected, avoided and unnoticed in many social encounters, not least in interview situations with people we feel relatively close to. In fieldwork, we frequently develop a sympathetic understanding for our informants – this is indeed often one of our explicit goals – but how often, and to what degree, does this understanding build on a mutual avoidance of more awkward topics and ways of thinking?

In interviews with people from the countryside, I found myself asking more detailed questions about the organisation of labour, ownership and financial questions, and the meanings of words, than I did with people from my core group. With my core group I felt I understood more precisely the situations and properties people were referring to. By signalling that I understood references to people and events mentioned I enforced the impression that we had a common understanding, and I do hope we had. But then perhaps we didn’t. It’s easy enough to see if your clothes are in the same price range and style, if you both
like the same music or are into the same kind of food, but given the limited time granted for most interviews, commonality must largely remain imagined, at best hoped for, and falsely believing one understands the minds of others is as common as it is consequential. I have nothing tangible to prove or disprove my belief, and while I remembered probing a younger female interviewee from a village in Hunan about what specifically she referred to when she said that something was ‘hard’ (xinku), I also remember not doing the same when listening to a local male researcher my own age. Was it then the case that social distance bred detailed data? Yes, I think so. To a certain extent. Even though the difference shouldn’t be overly stressed, the more social distance, the more my interview notes look like those of a one-shot investigating journalist, and the less the interviews had the form of a conversation about a topic of mutual interest. On the other hand, I do believe that this was more than made up for by the fact that I could go faster and deeper the more confident I was that I was understanding, in a Diltheyian fashion, what my interviewees were talking about.

Interestingly, with older informants, most of them men, I found that another praised quality of the researcher, ‘cultural sensitivity,’ i.e., knowing how to behave in a culturally appropriate manner, tended to render me powerless, often getting me no more than private versions of official stories. Trained with some success in the house of my mother-in-law to behave according to my position as young and respectful in the company of elders, I found it difficult to lead these conversations in the directions of my choice. My professional curiosity usually triggered a traditional teacher-pupil routine, made all the more uninteresting by the fact that few of my interviewees seemed ready to treat me as someone with potential knowledge of the topics they were relating.

Although I believe I made a good impression, I felt I accomplished little more than confirming a less personal explanation taking into account the very different culture these people had been brought up in: a noticeable legacy of the Maoist era, when the distance between ideals and reality reached an all-time high, is the ease with which many people choose not to disclose their feelings and thoughts even when these are not of a political or otherwise sensitive nature (cf. Li 1994).

Looking back, I think I could have done better if I had managed in the first minutes of my interviews (before I was transformed into powerless listener) to shift attention from my age to my education, thus by compensation creating a more evenly balanced situation. Even though old people constituted a small part of the Special Zone population, I think it was a pity that I did not get more out of my few interviews. It is common anthropological knowledge that socially
marginal persons, seeing things from the side as it were, often make very good informants, and Shenzhen was in the 1990s probably one of the most agistic localities in China, a side of local culture about which I wrote very little, and then not from the perspective of these aged men, less still from the perspective of the elderly women I found so few of. Perhaps it was just another sign of myself going ‘locally native’? Jing er yuan zhi, respectfully leaving the elders to themselves while concentrating on the young and beautiful?

Social categories are discursive constructions that usually come across as much more internally coherent, naturally given and decisive than they are, and the biological basis for distinctions of race, age and gender are not effortlessly negotiable in an interview situation. This does not always constitute a practical problem so much as it is a methodological concern. Seemingly counter-intuitively given the categorical gap, I generally found conversations with women easier and more rewarding than with men. The difference was not clear-cut, differences never are, but often I felt that women shared more personal stories than men, constructing their narratives around important moments and feelings pertaining to specific situations, whereas men’s narrative strategies often led them to make generalisations that many times seemed to be grounded not so much in personal experience as in socially shared understandings of ‘how the world works.’ It would seem that the ‘masculine’ interview situation Oakley (1981) criticised had become inverted, with me allowing for more disclosure and dialogue than my male informants were prepared to provide. To be sure, there were topics that my female interviewees never volunteered information on, and which I often found it hard to ask about, such as extramarital affairs (big topic in same sex conversations), abortions, and factory regulations on PMS. Gendered topics such as these, often avoided as gracefully by myself as by my informants, remained terra incognita, but on most topics the narratives were animated and full of witty detailed observations.

Using Harris’s (1989) distinction between a self as a locus of experience, and a person as an agent who takes a particular position in society and develops a series of actions to reach personal goals, female narratives seemed to be reflections of particular selves, whereas male narratives seemed to be reflections of persons. These two narrative styles easily lend themselves to different forms of analysis.

Now, were these observations more than just prompted by my own gender stereotyping and enactment? Was my particular interview format fitting a particularly mode of cross-gender conversation? Perhaps my marriage has brought me closer to a more ‘feminine’ style of conversation (‘unfortunately not,’ I can hear my wife sigh), or perhaps I had that from my childhood in a Sweden where
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A muscle-flexing macho mentality was often portrayed as a sign of stupidity rather than of strength? Maybe it was only common sexual attraction that made us all give a little bit more of ourselves? To some extent it was obvious that the perceived gender difference intersected with social power since locally well established, richer, female informants more often used the ‘male voice’ to tell me their stories. Nevertheless, can it simply be that what I experienced as the ‘female voice’ lends itself more easily to the kind of analytical work we are supposed to do as social scientists? The ‘male voice’ in a way pre-empted my theorizing and fictionalization, and I often found myself forced to ask my male interviewees to go back to their personal circumstances, when they of course were telling me these, only in other ways than I wanted.

I should add that far from all narrative strategies used by female interviewees appeared equally ‘spontaneous’ and grounded in personal experience. Female informants from the countryside, for example, sometimes evoked images of themselves as ‘traditional’ women from ‘traditional’ villages striving ultimately to return to these unaltered, even as the situation of our conversations made these romanticized tales of life appear very clearly as dreams of a past that never had been and that had few chances of becoming. These tales contrasted not in realism but thematically with those in which their male fellow villagers gendered themselves with grand plans for the future, often based on sometimes enacted but never successful schemes to get rich on the Shenzhen stock exchange, ‘the big goldfish bowl.’

BEING WHO YOU ARE, SEEING WHAT YOU SEE

The Chinese saying ‘the benevolent see benevolence, the wise wisdom’ is usually just a laconic way of stating that different people hold different views on matters of taste, but taken more by the letter it reminds us that what we see largely depends on who we are. I have tried to exemplify some of the ways in which ascribed and self-imagined belonging to different categories such as age, race, nationality, gender, organisational background and culture had an impact on my own fieldwork in Shenzhen. Doubtless, there were numerous other ways, many of which I most likely remain oblivious of, and they shifted through the years as I changed and as the field changed. As with most other researchers I could many times play the role of the ignorant outsider, the colour of my eyes excusing questions and behaviour that would perhaps have caused problems to a Chinese-looking researcher. Coming from abroad, yet being familiar with China
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and Chinese, made people relax in ways they maybe would not have if I had been sent from a local research institute. Coming from a family, and indeed a whole nation, with strong social-democratic traditions predisposed me to see things in a certain ethical light as well as to favour certain theories over others when analysing what I had seen. Not yet a father at the time of my research made parenthood less salient than it would have been had I done the study today, and so on and so forth.

The somewhat distressing conclusion of this enquiry is of course that as lone field researchers we can, at best, only speculate about the impact of our own personality and social standing in the field. Hypothesize we may and should, but never know. Then again, we do carry out most of our work not in the field, but in the safety of our studies. There, the impact of our organisational belonging is usually far more important in shaping the actual outcome of our work than many of the positional qualities that seemed, and in all probability were, important in the field.

Recognizing how my own understanding of life is formed more from living in large cities than from staying in remote mountain villages, and partly rest on my own ‘sinicization’ since the mid-80s as well as on a desire to contribute to a discourse of familiarity while guarding against a generalist approach that excludes the Chinese experience, I do believe the methodological strategies we adopt elsewhere, ‘at home,’ by and large are as useful in China as anywhere else. Ubiquitous lists of cross-cultural dos and don’ts should at best be taken with a grain of salt, resting as they usually are on old and poorly informed generalisations, as well as upon a simplistic view of culture. Working in China it is understandable that we, regardless of skin colour and nationality, easily perceive of many phenomena as ‘Chinese’ – and of course they are Chinese. But, human perception, focusing on contrast rather than on similarity, can easily lead us to think of people and situations as more distinct from each other than they really are. Distinctiveness does not, as I hope this chapter has demonstrated, necessarily imply uniqueness.

Salzmann, referring to anthropological studies in general, remarked a decade ago that ‘the credibility of our field reports rests mainly on their uniqueness, that is, the absence of any other reports that might present contrary “findings”, that is, test their reliability’ (1994: 35). If one believes in the interpretive function of the field researcher, and takes the notion of the researcher as co-creator of his or her object of study seriously, then it seems highly desirable to get as many different scholars as possible to engage in research, jointly in mixed company if possible. Not in order to get the whole truth, as was the object of Mao Zedong’s
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rural investigations, but in order to contribute more voices to the polyphonic dialogue that in the end perhaps is the best, some would say the only, way of assessing and enhancing ethnographic validity.

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For more than twenty years the People’s Republic of China (PRC) has been unbolted not only for financial investments, but also for non-Chinese researchers trying to gain insights and to make sense of contemporary Chinese society. Prior to the 1980s, the political restrictions imposed on foreign research after the establishment of the PRC led many scholars to withdraw to libraries to study contemporary China from written sources or through surrogate fieldwork of Chinese societies in Taiwan, Hong Kong or Singapore. Currently some limitations to field research are still maintained, but fortunately increasing political openness in China now permits even ethnographic research through long-term fieldwork by non-Chinese researchers. Similarly, former limitations on Chinese scholars caused by the extreme politicisation of Chinese academia after 1949 have been somewhat relaxed and the re-emerging at Chinese universities of the social sciences as academic disciplines has reinstated the ethnographic training of students (Guldin 1994; Harrell 1988). Now more academic freedom also offers non-Chinese researchers new opportunities for close collaboration with Chinese colleagues who share an interest in research based on ethnographic methods.

Based on experiences derived from two diverse fieldwork projects undertaken between 1999 and 2003 in the PRC, this essay will address some of the methodological, epistemological and political issues in relation to conducting collaborative fieldwork with Chinese researchers. Both research projects were related to contemporary issues in Chinese society – transnational migration and pariah group constructions – that required ethnographic research methods of participant observation and interviews at field sites in southern China.
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The reflections presented below on collaborative research and the implications for ethnographic fieldwork methods and knowledge production are evidently shaped by my own entrenchment as an area studies academic trained in Chinese language and culture. The academic discipline of anthropology has traditionally been defined by ethnographic methods of displaced ‘Western’ researchers’ dwelling long and alone in remote, confined geographic areas of the world. Likewise, sinology has also relied primarily on ‘natives’ as chief-informants, rather than regarding local Chinese researchers as colleagues. Although the dichotomy between the ‘foreign’ and ‘native’ researchers has been debated within the discipline of anthropology (Aguilar 1981; Messerschmidt 1981; Narayan 1993), collaborative fieldwork in which local scholars are more than just ‘chief informants’ does not seem to be a major issue of academic discussion. Nor is the collective production of knowledge based on this type of collaboration between non-Chinese and Chinese scholars a topic drawing much attention in the field of China studies.

Within both the academic traditions of anthropology and China studies, fieldwork and the analysis of research results still seems to be first and foremost conceived as an individual task, although this ‘lone rider’ practice is disputable in terms of research methodology. My personal experiences as a non-Chinese working in collaboration with Chinese scholars imply that during fieldwork as well as in the process of analysis teamwork across national and academic identities is a promising undertaking, but not without dilemmas of its own.

I do not claim that this type of collaboration is the key to more ‘authentic’ empirical field data, nor is it a solution to major epistemological problems of representation, power and authority. On the contrary, collaborative knowledge production across academic and national divides highlights differences of academic discourses and unequal power constellations. In contrast to individual fieldwork research, however, collaboration with colleagues from the areas of concern may produce a wider and richer array of sources, voices and narratives and thus challenge ethnocentric views and partial interpretations of empirical data.

CONSTRAINTS ON POLITICALLY SENSITIVE RESEARCH TOPICS INSTIGATED AND CHALLENGED THE FIRST COLLABORATIVE RESEARCH PROJECT

As a research method, ‘fieldwork’ has been adopted by various academic disciplines within the human and social sciences. Consequently, it has become a concept indicating merely some indistinct type and quality of direct human contact with
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the objects of investigation. This vagueness of the term may in particular be
descriptive of research conducted on contemporary Chinese society in the PRC,
because Chinese state regulations curbing and controlling especially non-Chinese
research continue to be issued.

Officially, foreign research projects are to be conducted in co-operation with
Chinese institutions and sanctioned at the ministerial level, but it is well known
that for geographically limited investigations it suffices to be approved by local
government institutions (see e.g. Hansen’s chapter). It is also possible to evade
central approval procedures by becoming part of larger Chinese survey teams at
either local or national levels, and fieldwork conducted with a limited number of
sporadic interviews is also increasingly accepted by local authorities, if reported
to them at all (see e.g. Sæther’s chapter). Since the mid-1990s, even long-term
ethnographic fieldwork in villages has again become a possibility in the PRC,
although these cases are still more limited in number (Stafford 1995; Mueggler
2001).

Despite official restrictions, the Chinese security apparatus only has the
capability of or genuine interest in monitoring social research on politically
sensitive topics. Since the late 1990s (and to a somewhat lesser degree at present)
my research topic of irregular overseas migration constitutes such a contentious
topic, since it has direct bearings on China’s relations to the outside world. Even
today, finding local partners or obtaining central authorization for a research
project on irregular out-migration from southern China is highly unlikely.
Recently, a dozen European researchers who independently of each other have
been investigating the pertinent topic of human smuggling to Europe have all
studied this social phenomenon without proper Chinese authorization. Almost all
of us attempting to do research related to regular or irregular overseas migration
from Zhejiang and Fujian provinces – both notorious for snakehead organised
migration – have been either detained, had research data destroyed by local
Public Security Bureaus (gonganju), or have in a couple of cases been expelled
from the PRC.1 In my own case, Chinese research colleagues have been forced to
report to the local Public Security Bureau on the questions I had asked and the
type of information they had handed over to me. Additionally, a novel way (at
least to me) of punishing unruly non-Chinese researchers was demonstrated in
May, 2000, when two PRC researchers invited to attend a workshop on Chinese
global migration in Budapest (Nyíri and Saveliev 2002) were denied exit from
the PRC by Chinese authorities who showed up in Beijing Capital Airport, due
to one of the European convenors’ previous unauthorized investigations in Fujian
province.
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Such political constraints endorsed by the Chinese state on non-Chinese as well as Chinese researchers obviously hold important implications for the research design, fieldwork methods, and the final research results. This became more than obvious in my project on overseas migration from Fujian province to Europe. The project necessitated ethnographic fieldwork and participant observation in migration villages (qiaoxiang), but no local academic institution was interested in supporting it given the sensitivity of the topic at that time. Instead, fieldwork in selected migration villages was commissioned to a Chinese researcher while my own presence was reduced to observations from a distance. When the empirical data had been collected my Chinese research partner and I intended the subsequent analytical phase of the project to be carried out by both of us. In this way, my first collaborative research project was instigated by Chinese authorities’ curtailment of research freedom.

NON-CHINESE AND CHINESE RESEARCHERS’ ACCESS TO THE FIELD: A MATTER OF DEGREE

The main objective of the larger study of which the migration village project was part included investigating the processes and patterns of transnational migration from China to Europe. A key concern to us was to study the sending communities in Fujian and to analyse the circumstances in these villages from where thousands of young Fujianese embark on frightful and highly risky journeys to Europe and the rest of the world.

I had previously studied Chinese migration in both receiving countries in Europe and sending communities in the PRC, and my participation in international conferences and guest lectures in Fujian and Zhejiang provinces had already provided me with a large network of academic contacts within the field of Chinese migration. When I was forced to solicit a Chinese researcher to undertake the more sensitive interviews and surveys in the migrant villages, I selected an academic committed to Chinese migration studies and with historical and political knowledge of northern Fujian. Although he was not an anthropologist and had no previous ethnographic fieldwork experience, his open-mindedness and training as a historian meant that I had no second thoughts about involving him in this project. As he was equally interested in the unexplored topic of Fujianese migration (that became all too relevant in the summer of 2000 with the tragic death of 58 Chinese migrants who suffocated in a tomato truck on its way to Dover, England) he agreed to become part of our project team on an equal basis and with all fieldwork expenses covered.

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Initially, he found it problematic to base his research entirely on ethnographic interviews and observations, but realising that the topic required information that could only be obtained through queries and observations in the villages characterised by a high degree of migration, he accepted the challenge. To prepare him for doing fieldwork, I first trained him in ethnographic methods and interview techniques during a one-month stay in Denmark.

When he started his fieldwork in Fujian, we decided that I should monitor the findings from the provincial capital Fuzhou, or from my home base in Copenhagen through daily e-mail exchanges. Several visits to Fujian also enabled us to go together on one-day excursions under the auspices of the local ‘mass organisation’ officially in charge of returned Chinese migrants (Zhonghua Quanguo Guiguo Huagiao Lianhehui) to other migration villages rather than the two finally selected for the research project. In this manner, we were able to make comparative studies and I gained some general sense of the different types of villages in which international mobility had become a dominant social, economic, and cultural phenomenon.

The methods applied by my Chinese partner in the two villages specializing in migration to Europe were broadly ethnographic, combining informal conversations and participant observation with formal interviews and a household survey. Although not living for longer periods of time in the two selected villages, he conducted a total of 106 interviews with officials and villagers over a period of more than one year. In the field, he wrote detailed reports on his observations, wrote interview summaries or transcribed interviews to keep me informed. When I wanted additional information, I requested him to ask the informants again. Constraints in access to the villages, however, also became a challenge to this project despite my reliance on a local scholar.

In the process of considering the pros and cons of a collaborative research approach, I anticipated that the study at least would benefit from a local scholar’s easier access to the field and from less government influence. This anticipation was not fulfilled. Chinese scholars have to rely on the same government institutions to permit them access to conduct systematic fieldwork and only the use of personal connections seems to be able to alleviate this process. In cases where the topic is politically sensitive even Chinese scholars are met with suspicion and restrictions from local officials.

The migration village project illustrates this point well. Contact with the first migration village was only made available by the political gatekeepers when my research partner exploited a connection held by one of his students to the local county government. Having done this, he easily gained entry to the village and
freedom to move around to conduct as many interviews as he wished, though still under local political surveillance.

In the second migrant village, he gained the necessary permission from the township government prior to entering. At the township level problems arose that were similar to the difficulties non-Chinese scholars typically face in the PRC. Village investigations are typically conducted by way of authorisation through the administrative hierarchy down to the village level. At the township level my research partner was requested by a government official to present a letter from his institution authorising his project. Unless this letter was produced, government permission would not be granted to conduct studies in the village. Since such a letter requires a person of authority to take responsibility for one’s actions, it is not always easy to produce. In fact, we had to wait a couple of weeks before it was signed. Even with a permit to go to the village, he was still totally dependent on the local government for arranging interviews.

Following this incident, the village head in the selected migration village became the next crucial link for gaining access to informants, since without his approval interviewees were reluctant to participate. As the village head started to become uncomfortable with the interview questions, he denied us contact to the people in the village by not returning phone calls or simply not showing up to appointments. We can only guess his reasons for not wishing to be of any assistance to us, but international migration from the area where the village is located is known to make use of elaborate, highly diversified and long-standing trade and human smuggling networks.

The major barriers of governmental approval at all levels of Chinese society thus apply for non-Chinese and Chinese researchers alike. Local researchers may have connections mitigating the ‘gatekeeper problems’ that non-Chinese researchers often helplessly face, but the presence and surveillance of the Chinese government is a fact for both non-Chinese and Chinese researchers. Having entered the field, the presence of local political authorities such as village heads or retired cadres may be either beneficial or a liability in the course of doing fieldwork. They may assist in locating key informants, but in the case of politically sensitive research projects, their supervision may also constitute a political threat to interviewees and researchers. Self-censorship during fieldwork is thus a factor also eminent in collaborative research projects.

By primarily relying on a local scholar to collect empirical data during short-term visits, the kind of research collaboration applied in this project compromised the archetypal ethnographic fieldwork method of gaining social experiences and exploring agency through the ethnographer’s own long-term dwelling and
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personal immersion experiences in the field (see e.g. Hastrup and Hervik 994: 3, 9). This ideal immersion into the field potentially allowing for access to lived experience was neither feasible for me nor for my Chinese research partner. In the political context of the PRC (and in many other countries as well) deviating from the ‘ideal’ fieldwork situation was necessary to realise the study, but by working closely together in a team and making numerous short term trips and follow-up interviews and observations we found an acceptable method of acquiring empirical information.

TRYING TO COPE WITH CONTENTIOUS FINDINGS

As the interpretation of the findings from the fieldwork approached, it quickly became evident that the epistemological problems of representing the ‘other’ (Clifford 1983; Clifford and Marcus 1986) was not to become elucidated or solved by any collective interpretations of meaning related to the project in question. In the interpretation of our research findings, we had originally planned to include our diverse understandings of Chinese migration, culture and history. In this project as in all other research projects, however, both of us were simultaneously academics and social agents affected by external forces such as academic institutions and political agendas that came to seriously obstruct the intended joint interpretation of our fieldwork results.

As a picture emerged of distinctly divergent migration processes in the two villages, my research partner gradually became wary of further analysis of the empirical data. It appears to be a decision he made as it became evident that many agents of the state and Chinese Communist Party apparatus at the local level were involved in different ways in the migration decisions of villagers. If our results had shown that migration from these villages primarily was market-driven by opportunities on overseas job markets and the commercialisation of legal and illegal migration services, our findings would have conformed to conventional PRC explanations of irregular Chinese global migration. From the empirical findings it appeared, however, that clandestine migration was tacitly permitted or even actively supported by local authorities, and most likely, as elsewhere in Fujian, helped by corrupt government officials (Chin 1999: 42-46; Kwong 1997: 60-67).

This discovery of an active Chinese state at the local level profiting from human smuggling and soliciting revenues from ‘illicit’ overseas remittances (Thunø and Pieke 2005) was clearly incommensurate with official Chinese claims
on the international political scene about actively fighting irregular migration and human smuggling. As a result of these local findings my partner became unwilling to disclose his name in the book that was to be published in English with a non-Chinese publisher (Pieke, Nyeri, Thunø, Ceccagno 2004), although this was his original intention and wish when he joined the research project.

In our discussions, we largely agreed on the causes and processes of migration from the selected villages, but we were unprepared to cope with the interpretation that local government agents were more or less involved in the lucrative business of human smuggling. The dilemma of either reducing my partner to an anonymous research assistant or not publishing our findings at all was difficult to resolve without having made any decisions in advance on how to proceed in such a situation.

In this project, collaboration across national and academic fields evidently failed with the withdrawal of my partner from the final process of the project. He has so far also refrained from publishing any of his own fieldwork data in Chinese academic journals, although according to the conditions for participating in the project researchers were permitted to use their ethnographic data for publishing on their own. This disconcerting outcome of our partnership calls attention to the need for planning for all eventualities before setting up collaborative research projects, especially on politically sensitive topics.

The failure to co-publish our results discloses the importance of considering the construction of research subjectivities (also) in collaborative projects. Research projects including both local and non-local researchers potentially reduce the risk of excessive simplifications and emotional or cultural biases, since the local researcher may be politically, culturally and linguistically more familiar with the field site while the non-local researcher may potentially be emotionally, politically and culturally less attached. From the fieldwork example above, our subjectivities as researchers became apparent in relation to the process of interpretation of the empirical findings, but especially in relation to the question of publishing the results, where our identities as subjects embedded in different larger socio-political contexts became obvious (Rabinow 1986). The influence of politics, university practices, and local academic cultures on the construction of knowledge is often a silenced topic, but in our endeavour it became unpleasantly evident.

As Bourdieu has shown for France (1988) and Chun for Taiwan (2000), academia in both Europe and Asia operates within institutional structures and highly competitive promotional systems that appear to have a powerful impact on knowledge production. In the PRC, higher educational institutions
increasingly demand that researchers solicit their own funding. By becoming part of the research project mentioned above, my research partner had achieved just this criterion of success, but the knowledge to be derived from the empirical findings developed into something that was not in conformity with the official interpretation of reasons for migration in the PRC.

Collaborative research projects intending to overcome some of the epistemological problems of authority and subjectivity of the ‘outside’ researcher have to take into account that subjectivities of researchers are decidedly related to the institutional embedding of science and academia. In the PRC, our research findings could easily be perceived as implying a direct critique of the dominant trend in Chinese migration studies, or worse, it could be considered unscientific, treacherous and unpatriotic. For my research partner this was too precarious and he felt he had to withdraw from the project, while my own position in a more liberal academic tradition enabled me to carry out the project to the end and publish the results.

In this case, the problem of interpretation and publication of the findings was ultimately a problem of power. Inequality of power between us did not, as it is frequently the case in collaborative studies, pertain primarily to financial inequity, but to difference in academic freedom. What initially started as a dialogue on the interpretation of research findings became an unequal discussion and in the end a monologue that did not as intended involve locally informed perceptions.

MY SECOND COLLABORATIVE RESEARCH PROJECT: INVESTIGATING THE ‘FALLEN PEOPLE’ OF ZHEJIANG

The distressing ending to the project on migration demonstrated the dissimilar political, institutional and discursive academic systems under which we are working as researchers in different parts of the world. After this experience I was, however, still committed to conduct collaborative research with local Chinese researchers, if the topic would enable fieldwork to be carried out by both researchers and would not again block my Chinese colleague’s possibilities of academic publishing. Nothing seemed more politically safe than to investigate a group of internal migrants in Zhejiang who formally do not even exist anymore: persons who during the Ming dynasty (1368–1644) possibly migrated to the area around Shaoxing and Ningbo in Zhejiang province and as a result became socially ostracized and categorized as the ‘fallen people’ (duomin). After 1949, this social stigmatisation was officially abandoned and the social category of the fallen
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people ceased to exist. Today, the fallen people of Shaoxing are thus regarded as a historical phenomenon and investigated more as a regional curiosum, although such outcaste groups (jianmin) were well-known parts of Chinese social structure during the Qing dynasty (1644-1911) and the Republican period (1911–1949) (Thunø 2004).

My first visit to Zhejiang province in 2000 went via an invitation from the local branch of the Chinese Academy of Social Sciences (CASS) in Hangzhou. In Shaoxing I first held workshops with local scholars selected by the local Lu Xun Museum and the editorial board of the local gazetteer of Shaoxing city. When I had read the publications of these local scholars and participated in discussions with them, I realised that their dominant research approach excluded information derived from interviews with persons who prior to 1949 had been categorized as fallen people. Only one younger scholar from Shaoxing College had investigated the phenomenon of this hereditary pariah group by combining historical documents and fieldwork, but she had not been invited to the workshops.

Later, I understood that the scholarship of this young female assistant professor in Chinese history was regarded as substandard by many of her superiors. Her fieldwork methods and her understanding of the value of fieldwork data was not in line with local perceptions of scholarship as descriptive folklore accounts, but it was fully in accordance with my intended approach to this topic. She welcomed collaboration on the topic, and subsequently a joint research project was drawn up that did not include promises of any funding of her activities, since fieldwork expenses were limited given the location of the field in the city of Shaoxing where she resides.\(^3\) Being permanently at the field site, she became responsible for the primary part of the fieldwork, but I joined her on several occasions (we conducted fieldwork together for ten to fourteen days in each of the years 2001, 2002, and 2003). The research project involved collective writing of at least one academic article, possibly more depending on the findings. Apparently, the topic was politically uncontested and our joint research proposal was welcomed and supported without any hesitation by Shaoxing College.

THE ‘INSIDER’ AND ‘OUTSIDER’ IN COLLABORATIVE FIELDWORK:
COMPLEX IDENTITIES AND POSITIVE FIELDWORK FINDINGS

Conducting fieldwork in a team with two researchers of the same age, sex, and academic status, albeit coming from very different universities and countries, enabled research on more equal terms than in the project described above.
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The typical circumstances causing inequality in collaborative research such as academic positions and uneven access to research funding and fieldwork sites did not affect this project. We both had easy access to informants, since local authorities were surprisingly indifferent to our interviews. Usually, we just hired a taxi, dropped by at the township or village government to tell them of our intentions, registered their bewilderment and went on from there to the villages where we conducted interviews without the presence of any village officials. The fieldwork carried out in this way produced a variety of empirical findings and the collection of historical documents was more successful than expected.

Some anthropologists doubt the value of combined local and non-local research. ‘I wonder what outside scholars bring to the research encounter. Are they the “expert” in theory who teams up with the specialist in local culture? Collaboration seems to be a way of hiding weaknesses as researchers. Is the native scholar not just a glorified assistant?’ (Bosco 1998: 28). Joseph Bosco assumes that a foreign scholar cannot positively contribute to the fieldwork process, but my experience shows that fieldwork benefits from the synergy of diverse identities, biographical backgrounds, personal interests, and professional concepts and techniques. The outcome of the fieldwork in Shaoxing in terms of both written and oral sources probably could not have been reached by either one of us working independently.

As the non-Chinese researcher, my initial arrival via CASS in Hangzhou was an entry from above in the local political power hierarchy, which proved useful for both of us. An essential local government report on the fallen people from the early 1920s had been cited in a few local publications, but despite several attempts my research partner had been unable to obtain it from the local library. The gatekeepers to this source only released it on my request via CASS in Hangzhou. In this manner, our collaboration made it possible for the local researcher finally to obtain this important historical source.

In the field, local and non-local identities also proved valuable. My Chinese colleague applied her local knowledge, local dialect and social networks to locate informants in the villages where those formerly categorised as fallen people still reside. On my own, this task would have been impossible. When we had located the informants who were mostly elderly people, we found that they were usually more than willing to account for their past, especially in the presence of a non-Chinese researcher. Prior to our project my partner had experienced some difficulties conducting fieldwork in her own locality and was surprised how open the interviewees became in the presence of an outsider. Naturally, the identities that researchers bring to the field have a profound impact on the fieldwork process. My position as ‘der Fremde’ (the stranger) permitted
me, following Georg Simmel, to become more ‘intimate’ with our informants (Simmel 1908 [1908]). Old women who generally were ashamed of their former social status as fallen people had nothing to hide when talking to a foreigner with limited relations to Chinese society. Consequently, personal histories of shame, humiliation, suppression and tragedies were disclosed and many tears were shed during these interviews.

Fieldwork conducted with a local researcher also raised the question about distance and partiality when studying one’s own society. In contrast to Euro-American anthropologists, who sometimes are criticised when conducting research in their own societies for making presumptions based on cultural sameness, ‘native’ anthropologists are now often assumed to have privileged access to their own societies. As probably most local or ‘native’ researchers (see the discussion in Narayan 1993), my partner was not really working ‘at home’ in the sense that she belonged to a family that had been labelled as fallen. Her local knowledge of conceptual categories and historical events proved highly valuable, but these categories were also constantly challenged. In some cases conventional local knowledge created blind spots, as when local perceptions about the fallen people as victims of feudal society forced to live on the fringes of majority society prevented her from probing into their cultural history as a group with a history of achievements of their own and with internal organizational power structures. She was surprised when I asked questions about their identity constructions in opposition to majority society and social structures prior to 1949. The discovery of elaborate cultural traits and an internal social hierarchy and social organization soon led her to appreciate these questions posed from my perspective as the outsider.

My research partner’s knowledge of local history, language, society and culture also repeatedly forced me to broaden my partial perspective. One particular instance is worth mentioning here: my Chinese colleague’s constant reminder that the fallen people had not only been located in the two streets in the centre of today’s Shaoxing where the authorities for centuries had confined them to live. Local Shaoxing history writing focuses almost exclusively on the city dwelling fallen people, but my local colleague broadened our research to include possible enclaves in villages far away from Shaoxing. At that moment, when she persuasively extended our fieldwork location from a single enclave of two streets to several groups distributed geographically in many locations, I was reminded how easy it is in the fieldwork process to privilege a single culturally defined location.
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My collaborative fieldwork experiences in Shaoxing city and neighbouring villages reflect that the relationships in the field between informants and ‘inside’ and ‘outside’ researchers may be more complex than just being the non-Chinese researcher teaming up with a specialist in local culture to hide academic weaknesses, or the local researcher being used as an assistant (Bosco 1998: 28). Undoubtedly, both of us have academic weaknesses, but by working in a team these possible weaknesses were turned into strengths by mutual acknowledgement of diverse approaches and personal and academic backgrounds. In the end, the empirical findings indicate that joint fieldwork and collaboration stimulate questions that can significantly raise the quality of fieldwork methods and empirical results.

NEGOTIATING THE SOCIAL CONSTRUCTION OF THE CATEGORY OF FALLEN PEOPLE: FAILING TO REACH COLLECTIVE KNOWLEDGE

Our subsequent negotiations of the history and social construction of the fallen people as expressed in historical sources and conveyed in interviews proved more complicated than working together in the field. Our joint fieldwork based on our subjective diversities had provided us with rich and varied fieldwork results, but now the very same differences and academic backgrounds became a discord in the interpretation of our findings. Both of us acknowledged that being a local scholar embedded in local culture and discourses did not warrant any authority in itself, but in the negotiation of the meanings of our findings it became apparent that combining different theoretical approaches from diverse academic traditions to produce joint knowledge is contentious and very time consuming.

Divergence in academic tradition, ideologies, and scientific paradigms still blocks us from reaching mutual understandings and perceptions of the social world of the former pariah group in Shaoxing and surroundings. One major issue of dispute has been how to explain the development of such a pariah group in Chinese society. Unfortunately, historical sources are limited on the topic of the origin of the ‘fallen people’. My Chinese colleague focused primarily on providing evidence that members of this particular group were originally prisoners of war, ethnic minorities, or slaves who upon migrating to the Shaoxing area became labelled as outcastes. I preferred to explain the categorisation of these people into a hereditary outcaste group as the product of official discourse by analysing different discursive means of social categorisation in the historical records (Thunø 2004).
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To use historical records either as sources for information or as sources for discourse analysis are two methods reflecting different academic disciplines and training, just as could happen in any team of researchers attempting to work collectively. In this Chinese–European collaborative project, however, discrepancies in regard to method of analysis and theory became more prominent given the different academic environments and language constraints that each of us were operating within. In a province such as Zhejiang, the exposure to non-Chinese research in English is limited and my Chinese colleague’s English abilities did not enable her to read books and articles on theories and methods as applied outside the PRC. She was of course familiar with academic theories developed outside China through the large amount of translations into Chinese, but I still found academic discussions arduous due to the lack of a common academic research and theory framework.

Just as in the migration project above, interpretation of the collective findings also became a problem of power in terms of uneven access to and knowledge of non-Chinese research methods and theories initially resulting in unproductive discussions. As articles have been produced individually by us both (see e.g. Yu 200a; Yu 200b), we have gradually regained the aspiration to present our collective interpretations including both local and non-local perceptions of the Shaoxing ‘fallen people’.

CONCLUSION

The PRC is, just like most other countries, a challenging place to do fieldwork. In my case accessing the field and fighting political obstacles became the very reason to commence collaborative research with Chinese colleagues. As interpretations of ethnographic data are no longer seriously being perceived as objective knowledge, but rather as negotiated realities in the encounter between researchers and informants, these negotiations may benefit from the collaboration in the field of both ‘insider’ and ‘outsider’ researchers. By probing into the dynamics of two collaborative research projects, I have attempted to demonstrate that joint fieldwork affected by different research subjectivities was the key to obtaining richer and more varied findings than any of us could have reached alone.

The political and institutional framework within which academia is situated in the PRC and in this case Europe, however, caused profound problems when it came to joint interpretation of the collective research findings. Fieldwork practitioners irrespective of their national or academic backgrounds are to be construed also as
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active social agents. Hence, the advantages of collaborative fieldwork are evident, but co-authorship based on collective fieldwork results may be unsuccessful due to impediments such as political considerations or different theoretical approaches. In this sense, collaborative research projects can be perceived as a viable alternative to the fieldworker working solo, but they are not necessarily a solution to epistemological problems of authority and representation, or a road to more original knowledge.

AUTHOR’S NOTE

Thanks to the anonymous reviewers and the editors for their thoughtful comments.

NOTES

1 Private information from the researchers involved.

2 This research project was part of a three-year project (1999-2002) on Fujianese immigration to Europe funded by the Economic and Social Research Council of England and Wales under its programme on Transnational Communities located at Oxford University. The project was led by Frank Pieke (Oxford University) and included Pál Nyíri (European University, Budapest), Antonella Ceccagno (University of Bologna) and myself. The research results have been published in Pieke, Nyíri, Thunø, Ceccagno 2004.

3 A year later, a grant of EURO 650 was transferred to her to alleviate expenses in relation to fieldwork in villages in the vicinity of Shaoxing.

4 Anthropology and space is discussed in details in Gupta and Ferguson 1997.

5 Viewpoints reflecting that ‘natives’ or ‘native’ scholars are associated with ‘authenticity’ and are able to communicate genuine positions are discussed in Appadurai 1988: 37; Narayan 1993: 672-73; 676–677.

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Chapter 15

ETHICAL DILEMMAS: BALANCING DISTANCE WITH INVOLVEMENT

Marina Svensson

FROM THE DESK TO THE FIELD

My doctoral research and much of my later work have been confined to collecting and analysing various historical and contemporary texts and documents. This was fairly simple and straightforward work, even though the topic of my doctoral research – the conceptual and political history of human rights in China – could be seen as rather sensitive and problematic (Svensson 2002). Texts are of course read and deconstructed in many different ways, and there is a risk that one misreads them or reads them out of context. But, on the other hand, historical and textual analysis is rather impersonal in character and one is therefore able to avoid the many personal and ethical dilemmas and conflicts that inevitably confront a fieldworker who conducts interviews or engages in participatory observation. My new research on the social construction of cultural heritage, and the way it is socially, legally, and economically contested, has forced me to step out of the safe haven of my study. I still use written documents – including laws, official policy papers, academic work and media reports – but I also do interviews with journalists, experts, officials, and local citizens, and I have conducted visits and interviews in different cities and villages. The new approach is exciting but my use of interviews and on site visits has sensitized me to a whole range of ethical dilemmas and potential conflicts that I was earlier happily unaware of.

Textual and historical research easily gives an illusion of definiteness and completeness, an illusion that one soon is taken out of when trying to compare
Ethical Dilemmas: Balancing Distance with Involvement

textual accounts with oral sources. I have thus discovered that the ‘reality’ I meet in fieldwork is quite messy and contradictory. This feeling of complexity is at times very confusing when one’s views and understandings are challenged and modified with each interview. As a fieldworker one is also inevitably made aware of the complex, ambiguous, and often unequal relationship between the researcher and people in the field. Even though identity, gender and status also influence historical work and textual analysis, these issues are not so immediately or painfully obvious to the researcher. When engaging in historical and textual analysis and studying the motives and values that inform authors or policy makers it was very easy, maybe too easy, to be critical, as I had no personal relationship with any of these individuals. But when conducting fieldwork I have found that I am much more timid and circumspect and eager to ‘get along’ with my interviewees, and worry more about potential dilemmas and conflicts.

This paper focuses on some ethical dilemmas and potential conflicts that may arise during fieldwork. The dilemmas and conflicts discussed relate both to the relationship between the fieldworker and people in the field as well as to the relationship between different groups of people in the field, and how one as a fieldworker deals with these situations. I believe that my experiences are relevant for many fieldwork situations, particularly when the research concerns pending and sensitive legal cases, social and economic conflicts, or political issues, or when interviewees either feel that the researcher can help their case or fear his/her motives.

This paper will also discuss the important question of how, and indeed if, it is possible or even necessary for the researcher to remain neutral when studying a hotly debated issue, such as a pending legal case or Chinese citizens’ attempts to influence policies and decisions. When conducting research on current legal and political issues one is often met with both implicit expectations and explicit appeals for help. Chinese citizens involved in a case can interpret the interest of a (foreign) scholar as being as much based on sympathy as on academic interest, or they, and the researcher, may have difficulties in drawing a line between the two. Many officials, on the other hand, are skeptical about the motives of the researcher, especially when the focus is on problems in Chinese society rather than on positive changes. While these ethical dilemmas and problems are certainly not unique to China, they take on a special salience in a country where official policy places many constraints on research, and where research very often is based on informal contacts.1

In China many, or maybe all, research topics dealing with social and political issues can be regarded as sensitive, depending on the timing and framing of the
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research. Topics that once were regarded as highly sensitive or taboo, including human rights and the death penalty, are today much less so, although they are still very problematic areas of research. On the other hand, issues that on the face of it would not seem to be sensitive at all, for example cultural heritage protection, can become quite sensitive if touching upon issues of political decision-making, economic interests, and corruption, or if they have led to open protests. In recent years much destruction of old buildings and neighborhoods has taken place in China as a result of city re-developments. This has led to protests as people are dissatisfied with the compensations offered or are reluctant to move from the city centre to areas in the suburbs far away from schools and work places. There have also been charges of corruption in connection with these real estate developments. While not all of the residential areas that have been demolished necessarily would be regarded as culturally and historically important, it is beyond doubt that many significant and valuable buildings have been destroyed and that old communities have been broken up. In the countryside we find conflicts over land and ownership issues and economic compensations related to cultural properties. Villagers sometimes lose control over their own cultural heritage to higher authorities or tourism companies without receiving any benefits from the revenues that rural tourism brings.

When research on cultural heritage touches upon issues of ownership, economic benefits, and forced evictions, the topic thus becomes quite sensitive. But sensitivity is not a given. Sensitive issues can become less so over time and with policy changes, as the Chinese leaders’ recent acknowledgment of the problem of demolitions and forced evictions also shows. These issues might however remain sensitive to local governments and individual officials who now become legitimate targets for criticism. Furthermore, research on pending and controversial legal cases and/or political issues is much more problematic than research on cases or issues that have already been settled.

CULTURAL HERITAGE AS A FIELD OF CONTESTATIONS

To exemplify my points I will focus on three case studies carried out in three different cities and also draw on my more recent experiences at the village level. I shall describe the three cases in some detail to give an impression of the complicated constellations of personal and institutional interests involved. In all three cases old buildings were slated for demolition, or were to be moved to another site, and the owners protested against these decisions. All cases drew a
lot of media attention and provoked criticism from Chinese experts. The context and official attitudes as well as the outcomes differed somewhat in the three cases, however. My own interest was triggered by reading reports in the Chinese media. I first contacted some of the journalists and experts who had spoken out on the cases, but later also conducted interviews with some of the individuals and officials involved.

In the first case, which is also the most publicized, officials at the local Cultural Relics Bureau and delegates to the local People’s Congress and the Political Consultative Conference early on protested against the planned demolitions. The owners of the houses also wrote letters of protest to the authorities. After several houses already had been demolished, eleven families, whose houses were next slated for demolition, decided to go to court and sue the government for violating the cultural relics law. The families lost in the first instance trial but appealed the verdict. It was obvious that the local government had put pressure on the court and that the Cultural Relics Bureau was under strong pressure not to testify in favor of saving the houses. The legal case was covered intensively by the media and the demolitions drew criticism from both provincial and national authorities and cultural relics experts. Both the Ministry of Construction and the provincial authorities sent delegations to investigate the matter. Despite this massive critique and attention, the local authorities at first did not change their mind, which exemplifies the difficulties for central authorities in implementing policies and decisions at the local level. In the end a compromise of sorts came about as seven houses, which were covered by the protection plan but nonetheless had been slated for demolition, were saved before the second instance trial took place. Four other families, however, whose houses were outside the protection plan, lost the second instance trial, and their houses were quickly demolished. The local government later issued many policy documents promising more money and better protection of the cultural heritage, although not much concrete has happened as yet.

I first visited the city less than half a year after the demolitions and talked to some of the local residents. My interviews have mainly been confined to the two most prominent and active individuals, one a local businessman whose house eventually was saved, and the other a journalist not residing in the city, whose ancestor’s house was demolished. I also talked with journalists, legal experts, and cultural relics experts and officials at the provincial and national levels. I made a rather weak attempt to contact the local officials at the Cultural Heritage Bureau on one of my first visits but at that point the time was obviously not yet ripe. Some time has now elapsed since the story hit the headlines so it could
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be worth making a new attempt with the Cultural Relics Bureau. Due to the bad publicity, however, the local government would probably still find the issue much too sensitive to permit any interviews. In fact, even some provincial level officials were remarkably embarrassed over the case as it reflected badly on the whole province.

Another case that I have studied is still pending and concerns a house in another city where the authorities have shown a very ambivalent attitude. I first read about the case in a national newspaper and then contacted the author of the article, a well-known expert on cultural heritage issues who himself had been approached by the family in question. I later found out that local and provincial newspapers as well as national and provincial television stations also had reported on the case. But generally speaking, the media attention was not so great in this case as in the other two. The expert had interestingly enough avoided meeting with the family because he didn't want to be influenced by them in his judgment of the case. Other national and provincial experts who had spoken out in favor of saving the house at the original site had however visited the family. In contrast to the expert, I wanted to meet the family as I felt that a mere reading of newspaper articles and official documentation was not enough to understand the case and the family's views. I have also discussed the case with some provincial and local cultural relics experts.

The house is possibly one of the oldest in the city, dating from the Ming dynasty, and was mentioned as an important historical building when the city appealed for status as national historic city. It is not currently listed as a protected building, however, and therefore its legal protection is virtually non-existent. The local Cultural Relics Bureau seems to have played a more sinister role in this case and has been accused of some underhand dealings with a developer who was promised the site in return for some monetary contribution, ostensibly towards moving the house to a scenic spot outside the city. The historical environment of the house – it is close to the old city wall – has seen much destruction in recent years. Modern buildings now engulf the house and more are currently being planned; this was used as an argument for moving it. The agreement between the Cultural Relics Bureau and the real estate developer was kept secret from the family which owns the house, a brother and his two sisters, none of whom live there anymore. The building now houses a small Chinese pharmacy run by the brother whereas other parts are rented out as housing to poorer families. The house is in quite bad shape and needs significant repair. When the family found out about the agreement they protested against the plan to move the house and argued that it should be saved at its original site. They contacted a well-
known national expert, who then wrote the article mentioned above. The family also sued the local Cultural Relics Bureau. Before the trial opened the bureau withdrew its decision to move the house and it then looked as if the house would be saved. The authorities however later changed their mind and decided that the house should be moved to a street in the city centre that is currently undergoing some renovations. In recent years some repairs of the old city wall and other heritage sites have also been made.

I first met with the family when they had recently decided to once more sue the authorities in protest at the decision to move the house. The family somewhat surprisingly later won the first instance trial, but the authorities appealed the verdict and won in the second instance trial. The family appealed, and the case was tried by the provincial people’s high court, which ruled that the local authorities should conduct a thorough investigation into the age and historical value of the house. The fate of the house is thus still undecided.

I now turn to my third case, which was also brought to my attention through my reading the Chinese press. I was able to read quite a lot about the case and also talk to some officials and experts before actually meeting the old man who owns parts of the house. I met with him after it had been decided that the house would be saved but when there was still much debate on the historical value of the house and in particular on how it should be preserved and used. As with many other big houses and mansions, the greater part of the house was confiscated by the state in the 1950s and then allocated to a number of families; in the end 25 different families lived there. After the house was slated for demolition the other residents soon began to be resettled, but the old man, son of the original owner, refused to move. He demanded that the house should be preserved because of its historical value and because a famous historical figure, a poet, had once lived in the house. The local Cultural Relics Bureau has constantly denied, quite correctly I believe, that the poet ever lived in the house, although historical facts show that he did stay in the neighborhood on one of his visits to the city. They have also challenged, and again I would side with them, that the house is as old as claimed by the owner and some other experts. The owner was supported on the age of the house by a famous national expert on historical preservation planning, who was later criticized by local cultural relics experts for being ignorant about basic archaeological and architectural facts. More problematic in my eyes, however, is the claim by some of the local cultural heritage officials that the house lacks any historical value, as it, despite probably only dating from the late nineteenth or early twentieth century and being in a delapidated state, is a very rare sight in the city after years of redevelopment schemes. This case also illustrates the
fact that personal attachment to old houses and historic environments and place myths often are more important to ordinary people than are architectural and historical facts.

Interestingly enough it was not the old man who sued the local authorities in this case but the school that had been promised the land that sued the old man in order to get a court decision that he had to move out. Despite massive media attention and a lot of support from ordinary citizens – more than 10,000 people have visited the house since the media began to write about it – the school won the case and the man was ordered to move out within ten days. Local scholars sympathetic to the case now called a well-known national expert whose visit to the site was given good coverage in the local press. The old man appealed the verdict and at the same time a member of the municipal peoples’ congress standing committee wrote an extraordinary motion calling for a reversal of the demolition order. When this order was soon after annulled it was, however, most probably as a result of the intervention of the municipal party secretary who spoke out in support of protecting the house. In any event, the second instance court later overturned the first instance court verdict and the house was thus saved from demolition. It was later, together with more than one hundred other historical buildings in the city, listed as a protected site. The city has later adopted a municipal protection plan and announced several protected areas. Although the court case had been settled when I began my interviews, the age of the house and how to preserve it was still a hotly debated issue. The owner, some local experts and intellectuals, as well as some delegates to the municipal people’s congress want to turn the house into a memorial museum for the poet. The district government, which has promised to shoulder the cost of the renovations, does not agree and has discussed plans to turn the house into a shop or alternatively into an activity house for elderly people. In any event the old man will have to be resettled somewhere else.

INFORMANTS’ EXPECTATIONS AND FRIENDSHIP IN THE FIELD

In several of my cases I have been alerted to the expectations of the interviewees regarding the motives, role, and ability of the researcher. Ordinary citizens are normally not used to foreigners or to academic research and often expect that I can help solve their problems or redress injustices. I have found that many Chinese perceive me as not much different from a (Chinese) journalist and therefore tend to have rather high expectations that I could have some impact by making my
voice heard. It is necessary to be clear from the outset about one’s own limitations and purpose as a researcher. In order to prevent giving rise to high and unrealistic expectations and/or negative prejudices among the interviewees, the researcher has to think about a whole range of issues: how to present his/her research to different audiences; how to balance involvement and distance; how to avoid being caught up in conflicts among the interviewees, as well as underlying cultural and political assumptions about friendship and research.

In this context one needs to give a thought to the issue of empathy and friendship in fieldwork. It is easy to say that one should not confuse striving for empathy with friendship, but the boundaries are not completely clear. I would agree with de Laine (2000: 119) that the fieldworker has to try to ‘balance involvement with detachment … and closeness with distance,’ but that is not much of a guideline either. A certain amount of rapport and empathy is necessary for a good interview but there is always a risk that one’s sympathy and interest can give rise to unrealistic expectations that one could help the case. It is natural during interviews to be sympathetic and attentive to the interviewees’ concerns. But it can feel very unsatisfying to leave an interviewee/field site after a long and sympathetic interview without offering any help. Another complication of the closeness/distance dilemma is that acts and behavior that in one culture would be seen as tokens of closeness and friendship, or carry some expectations of reciprocity, may be perceived differently in another culture. In China, information and research contacts are after all often gained through contacts and human relationships built up by acts and expectations of reciprocity.

The same concerns about empathy and detachment apply to interviews with officials but with a different twist. I have discovered that it is much easier to be critical when reading documents whereas I find that I shy away from critical questions during interviews. On a recent fieldtrip, where I had very good and cordial interviews with some village party secretaries, I found myself worrying whether I was not being too sympathetic and uncritical. There is also the question of whether the show of empathy during interviews is quite genuine, or if it is not contrived and false if it becomes a method to make people relax and open up. In ordinary life empathy is after all usually the beginning of friendship. Be that as it may, in fieldwork one will feel more sympathy towards some people and in some situations than in others.

It is quite natural that friend-like relationships develop in the field, especially if one stays long enough in one place. But how does one deal with it and when does it have a negative impact on one’s work? Unfortunately there doesn’t seem to be any easy and clear-cut answer to that question. One should however bear
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in mind that the relationship between the fieldworker and people in the field is basically quite unequal in character and usually ends when the researcher leaves the field. I recall a senior researcher who talked about the friendship that had formed and continued between him and some of the people he interviewed more than twenty years ago, and the response it elicited from a PhD candidate. The student had recently returned home from her first fieldwork with a much more complex and difficult experience of interpersonal relationships in the field and questioned his unproblematic description of personal relationships in the field. I would not question the senior scholar’s friendship or the possibility of friendship forming during fieldwork, but I do think that he glossed over and ignored fundamental questions and problems as well as the danger of becoming involved in conflicts between different individuals.

ON NEUTRALITY AND ACTIVISM

The above discussion on expectations and friendships leads me to my second question. Is the detached researcher really the only and the morally best or most ‘scientific’ role model? Can and should one be completely neutral, or is this just an illusion? What is the appropriate relationship or line between research and activism? This is potentially a more difficult question in China than elsewhere, especially if the topic is seen as sensitive. I don’t believe that one is a less serious researcher because one takes a position, and by choosing one’s topics, whether that is human rights or cultural heritage protection, one has already indicated that one finds these issues important. Taking a position is not wrong provided one doesn’t fabricate or twist facts and as long as one’s activism doesn’t get in the way of one’s research. Many people who are engaged in development studies or environmental research seem to be able to combine research and activism in a very productive way. In several of the cases discussed above I have found myself not being the completely detached researcher but being quite sympathetic to peoples’ efforts to save their houses. Many foreigners have also spoken out against the demolition of courtyard houses and alleys in Beijing, including researchers and architects, whereas international bodies such as UNESCO have been actively engaged in protecting historical buildings and drawing up protection plans.

As hinted at above, it is sometimes quite difficult to uphold the line between mere sympathy and more concrete support during interviews and fieldwork. I suppose one has to make decisions about one’s degree of involvement from case to case, but be clear about the fact that one’s main interest and responsibility as
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a researcher is to try to understand and analyse. Perhaps some people will feel disappointed by such an attitude since academic research, especially if published outside of China, naturally would seem to be of little significance to them in their present struggle. In Beijing, an activist thus turned down an interview with me because, as she openly stated, she would much rather spend her valuable time with a journalist than with a scholar as the former has a better chance to influence policy-making. Although I felt somewhat annoyed by her attitude, I cannot really blame her and would probably have felt the same way had I been in her shoes. Nonetheless, my impression is that people on the whole are more than happy with merely the interest of the researcher and are impressed by the fact that one is willing to take time and travel far to hear their stories and visit their houses. However, I think one has to seriously consider whether one’s own presence could perhaps reinforce people’s insistence to continue with their protests/legal cases, something that might prove harmful to them. This is a real danger in China where contacts with a foreigner could per se create difficulties for people. But, on the other hand, one must trust that people know what they are doing and one must respect their choices.

When I first met the old man in the third case he expressed a wish that I might help or encourage foreign companies to invest in the renovation of his house, something which I was unable to do, as I also told him. First of all, I didn’t posses such contacts, and secondly I doubted that a foreign company would be interested. In recent years, however, the successful renovation and redevelopment of some historical properties in Shanghai has turned out to be good business and has been met with interest by foreign investors. So it is not implausible that both foreign and domestic investors might find such an investment interesting although I don’t have either the contacts or the interest to become involve in affairs like this. In later meetings the old man’s main concern has been the authorities’ proposed protection plan. He and some local scholars and other people, including some delegates to the municipal people’s congress, are dissatisfied with the plan since it fails to recognize what they claim are the Song dynasty features of the house and the connections with the poet. The old man has my admiration for having persisted for all these years, although perhaps on the wrong historical premises, because without his perseverance the house would be long gone. Although I’m not an expert on Chinese architectural history, I am critical of his and others’ insistence that the house be restored based on the alleged Song connection. I have expressed some skepticism on this point but not wanted, or needed, to dwell too much on it, as it is more of a technical issue, although of high importance to him. The question of saving the house had
already been solved when I met him, but on this issue I strongly supported him and have also voiced my opinion in contacts with the local authorities. In other words, cases are often quite complex, and one has to decide which aspects one is sympathetic to or where one can offer some support, albeit perhaps only moral.

One should also realize that people are not passive but sometimes make ample use of the visit of the researcher/foreigner to promote their case, something that might be difficult to stop or control. The old man in my third case has thus made good use of the many visits to his house. He keeps a book where all visitors – 10,000 to date – are asked to write down their name and a few words. He also has several blackboards with press clippings and photos of more prominent visitors to the house. On my latest visit I discovered that a photo of me now had been pasted on one of the blackboards! In recent visits to some villages I have found myself the target of Chinese TV crews. They not only want to include the exotic foreigner in their program and hear her impression of the village, but also want to have her ‘expert’ advice on cultural heritage protection, including criticism of mistakes. The tourist guides in one of the villages also came to include me as part of the ‘view’ for the tourists. ‘This is a foreign scholar who studies the protection of the cultural heritage in the village.’ This elicited questions from some tourists who wanted to hear my opinions and also know something about protection issues in Sweden, whereas other tourists admonished me to speak out for stronger protection in China. Complete neutrality is thus not possible nor is it expected, or even accepted. I have thus increasingly found myself taking on the role of a spokesperson and have now even come to contemplate engaging more concretely in cultural heritage protection through workshops and direct support.

I understand the cultural heritage as a social construction, a complex process in which historical facts and political and economic motives interact and sometimes clash with ordinary citizens’ memories and visions regarding the past and daily uses of heritage sites. I thus aim to problematize cultural heritage production and focus on conflicts over the interpretation and management of the cultural heritage among different actors and stakeholders. But I am also, in my own way, taking part in constructing and consuming the Chinese cultural heritage. In this latter context I have to take off my academic hat and engage in debates with Chinese officials and citizens regarding what one should protect, and how. On many issues and research topics it is inevitable that one is drawn into the discussion and asked to, or feels the need to, offer one’s own views on a topic. This requires some diplomatic skills when the topic is sensitive and, for example, relates to official policies and ideology.10
Since I study the contestations surrounding cultural heritage issues, conflicts between different actors and stakeholders naturally become a major focus of interest, although these conflicts also pose problems for me as a fieldworker. Conflicts not only occur between citizens and officials but also among different authorities and among ordinary citizens. In most cases it is possible to avoid being drawn into these conflicts by stressing that I want to hear all sides of the stories, but sometimes people ask me to take sides, which can create problems. In my first case, I have been very dependent on the plaintiff residing in the city for information. This worked well until I discovered that he had a dispute with both some officials and a local resident. I am still unclear about the content of these conflicts, and this is part of the problem. The conflicts seem to concern both complaints from officials regarding the renovations the man had made on his house, as well as his own disagreement with the nomination of another resident’s house to a protected site. This new development was a bit surprising to me as the local residents earlier seemed to have been in complete agreement and the man had taken an active role in protecting the houses of his neighbors. But, on the other hand, it should perhaps not have come as a surprise. The issue of renovation/restoration is complex and relates both to issues of authenticity, skills, and economic means. In some places people with money have engaged in renovations and restorations without the formal approval of cultural relics experts, restorations that the latter have found inauthentic or poorly done. In other cases renovations have however taken place before the house became a protected site surrounded by strict regulations. This part of the conflict was more of a technical matter where I did not have to take a stand. The conflict with the neighbor was probably in part related to the fact that the neighbor had bought the historical property as an investment rather than inheriting it from his ancestors, as the man himself had done. When historical properties become regarded as valuable assets it is natural that some people see them as profitable investments, which leads to new conflicts. The justification for the nomination is not clear to me but it may have been fair enough.

This case sensitized me to the danger of becoming too dependent on one informant or gatekeeper. In this case I took the easy way out and refrained from seeing the neighbor as the conflict was not important for my research or for the larger picture. However, even small conflicts where one is not forced to take a stand can make one feel uncomfortable and put a strain on one’s relationship with informants. Conflicts between interviewees or dependence on one gatekeeper or informant can also obstruct one’s research and subsequent contacts. One could
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come to be seen as an ally of the first informant and have difficulties making contacts with others, or one might come to feel that one should refrain from seeing other informants, as seeking them out would put one in a bad light in the eyes of the first. In some cases one may become so contaminated in the eyes of others due to association with a certain person or official body that one’s research becomes threatened. But it is not impossible to overcome such initial hostility as long as one is aware of the problem, as clearly shown in Hansen’s chapter.

In some respect the dilemma of proximity and distance can be more acute in a closely-knit community such as a work place or a village. A village is by nature a rather small and confined place. The researcher’s interactions with local people are especially strong in this case and his/her activities and meetings noticed by everybody. Having myself recently begun to do research at the village level, I realize that my doings are known to everybody, whom I meet, when, how often, etc. I have to spread my graces, including where I take my meals, so as not to be seen to be too close to some individuals.

CONFLICTS AMONG AUTHORITIES

It should be remembered that the Chinese state is not monolithic and that local cultural relics departments, or individual officials within these and other departments, also have protested against demolitions and have spoken out for stronger and more effective cultural heritage protection. In fact, individual officials can sometimes be very critical of other departments and local governments. It is quite possible to have very frank and open interviews with many individual officials. In most cases it is useful to find some gongtongdian (points in common) and mention similar problems in the West in order not to be seen as a harsh and insensitive critic. I thus often refer to the fact that Sweden made some of the same mistakes as China is making now when we demolished old urban districts in the 1950s and 1960s. The existence of conflicts and different interests among different authorities can make officials more circumscribed in relations with foreign researchers but generally speaking this pluralism is positive and makes critical and frank discussions much easier. Needless to say, it is often easier to get in touch with and interview those authorities and individual officials who take a critical position on a specific case or issue. Although officials can be hostile or suspicious in contacts with researchers/foreigners when they worry about losing face if they admit or reveal problems, they can in other cases be quite forthcoming, especially if they have not been personally involved, or if there has
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been a policy change and they are trying to score a political point by being seen as protecting the cultural heritage. It was relatively easy to interview officials at the cultural relics bureaus, especially if they had not been directly involved in any sensitive cases, since they would feel that I was on their side against other more powerful departments. It is also relatively easy to interview higher officials and officials from other departments who have taken a critical attitude to the local government whereas it is much more difficult to approach the targets of criticism.

CONFLICTS BETWEEN AUTHORITIES AND ‘ORDINARY’ CITIZENS

Conflicts between individual citizens and authorities are a common, maybe inevitable, feature in fields like mine, and in the Chinese case it is further complicated by the lack of well-functioning democratic and legal channels. However, I think it is important to realize that as a foreign researcher one may have a tendency to idealize Chinese citizens who speak up against the regime, whatever the issue or method used. It is easy to paint citizens’ activities and motivations in a very romantic and heroic light, while being more critical of officials representing the Chinese party-state. The authorities have sometimes alleged that dingzihu (nail-like i.e. resisting households, as those who refuse to move are called), in fact are more interested in getting a higher economic compensation than trying to protect the cultural heritage. In two of the cases discussed above there were allegations from the authorities’ side that the owners of the houses were motivated by economic gains rather than concern for the cultural heritage. As far as I can judge the allegations were spiteful and unwarranted, and besides, trying to protect one’s economic interests is also a legitimate concern. Still, one should perhaps be somewhat cautious before getting involved or taking sides in particular cases as they are often more complex than they seem to be on the surface.

On a recent trip to some villages I found my prejudices towards officials challenged as I came to feel quite sympathetic towards one village party secretary who was trying his best to protect the rural heritage, help the village develop economically, and keep the higher authorities at bay, who wanted to control and take over tourism in the village. There were some disgruntled people in this village who felt they were not benefiting much from tourism, but as far as I can judge the complaints were not well grounded and their expectations highly unrealistic. But this experience also sensitized me to the fact that although I need to meet and interview party secretaries and local officials it is better not to
be seen too much with them, but to conduct my interviews in the confines of their offices. It is problematic, although often unavoidable, to walk around in the village, or in a city district, together with officials and expect people to be forthcoming about problems. Being perceived as on too good a footing with local officials might deter villagers from engaging in frank discussions about problems in the village. The fact that one is a foreigner can however be an advantage since one is not identified with officials or the state.

In one of my cases discussed above, the individual concerned had talked very negatively about one official with whom I then scheduled a meeting with some trepidation. But this official turned out to give, in my view, very reasonable and historically correct arguments regarding the house, as well as being very forthcoming on problems and mistakes in cultural heritage protection in the city more generally. He also expected and wanted me to write an academic piece on the topic, and obviously regarded me as a fellow scholar with a shared concern for historical facts. I think that taking the position that I just wanted to hear the arguments of all sides was justifiable in this case, particularly since the house had already been saved. In this case I also found myself at times taking a somewhat mediating and conciliatory position. Some officials talked too despairingly about the old man and the expert who had supported him, and were also too critical of what they saw as biased and ignorant reports in the media. In my talks with the old man I also came to take a more conciliatory position and to talk more positively of the planned renovations. It may not always be so easy to take such a neutral and conciliatory approach when one researches a pending case where conflicts are more severe and acute and emotions run high.

It can be more difficult to stay neutral or keep out of conflicts in a small village where people are much more thrust upon each other than in a big city. In one village I recently visited, some people I met were dissatisfied and very critical of their local leaders, characterizing the party secretary as a wenmang, an uncultured type. The local authorities in cooperation with the cultural relics bureau had decided to move people from one old house, justifying the move as necessary in order to protect the house. The villagers resented this move and also felt that they generally did not benefit from the growing tourism and that they were left out of the decision-making process. Their resentment was also closely linked to the loss of farmland as new housing and other facilities (such as shops and new government building, etc.) required them to give up some of their land. In an effort to influence this decision several villagers had gone to the provincial capital to protest against the requisition of land only to be rounded up by the police and sent back on the instruction of the county government. At my first
visit I found it acceptable to simply hang around, interview different people, and try to get a better picture of the political and economic power structure in the village. If plans go ahead to move people from other houses, resulting in more discontent and protests, it may not be possible to stay neutral and silent, or get access for that matter, and I may have to abandon this field site. Paradoxically, the new attention to and appreciation of old houses and efforts to preserve them give rise to new conflicts over ownership and management that become particularly acute if the sites also are perceived as having commercial potential and/or become tourist attractions.

THE PRIVILEGED POSITION AND RESPONSIBILITY OF THE FIELDWORKER

When doing research in China one is inevitably reminded of one’s privileged position and of the fact that one is only in the field for a limited period of time and will soon be gone. I often wonder whether people do not feel puzzled by the seemingly affluent scholar from abroad who suddenly appears on their doorstep to do research. One’s ability to travel is an indication of privilege, albeit less so in today’s China. It is difficult to avoid questions concerning salary and other monetary issues from interviewees. One’s economic affluence, at least in comparison with most Chinese people, can create feelings of unease and maybe even guilt for the researcher, especially when the people one meets are quite poor or if one studies issues related to poverty or inequality. I remember a woman in one of the villages I was staying in who asked me how much I had paid for my shoes. Although I gave a much lower price than the actual one, the figure was still much higher than what she made in a month, to my great distress. Chinese people’s hospitality is remarkable and one is often invited for dinners. But it makes me feel very uncomfortable when people spend their meager incomes, or officials use public funds, on entertaining me with sumptuous meals that I don’t really want, and sometimes with too much liquor. I try to schedule meetings well ahead of mealtimes but it is difficult to fake dinner appointments when one is so obviously a stranger in a village or a town. These feelings of guilt or unease admittedly say more about me than about the Chinese people.

In fieldwork in China as elsewhere one has to think about how to balance the responsibilities to and interests of different parties, especially if they are very unequal in terms of social and political power. An important ethical question is to whom the fieldworker owes loyalty, whether that primarily is to the research
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agenda, the gatekeeper or official sponsor, or the interviewees, or maybe to one’s own moral and ethical values and standards (de Laine 2000: 120). In a context such as the Chinese, I would argue that one’s loyalty and responsibility foremost is to individuals in the field, whether officials or ordinary citizens. It may thus be necessary to drop or revise one’s research agenda or change field site if the situation becomes problematic or new issues arise.\textsuperscript{13}

CONCLUDING REMARKS

In all the cases discussed above I became acutely aware of the many small and big dilemmas and conflicts that confront one in the field. My illusion of understanding the cases through reading media reports and official documentation was immediately dispelled when I began discussing the cases with people, and my understanding and sympathies were furthermore also changed and challenged from interview to interview. What has emerged is a fragmented and somewhat contradictory picture that is composed of the little pieces I have been able to put together from different actors who have told me their own stories and who have their own agendas. I believe that without the interviews my understanding of the cases and the social and political dimension of cultural heritage protection would have been much too simplistic but it would have been much easier to just stick to written documentation. I am still wondering how much, and what, I have got wrong and misunderstood in my interviews, and am open to the need to re-evaluate and modify my views. I also wonder how my interviewees perceive my questions and visits.

It is virtually impossible to fully anticipate the problems one will meet during fieldwork, or which aspects of one’s research will be regarded as problematic, and by whom, as well as how one then will deal with the situation. Regardless of how well one knows Chinese society, and who can say he or she really does, circumstances change rapidly, as do relationships between individuals. No matter how well one prepares before going into the field, or before beginning to interview people, one is bound to find oneself on occasion confronted with dilemmas and problems of various kinds. When studying contemporary social and political issues it is probably inevitable and maybe a good sign that one sometimes questions one’s role and the boundaries between detached research and involvement, or between researcher-self and ordinary self shaped by specific moral and ethical values, and that one worries about doing the right thing in a conflict situation. I believe that only a very insensitive researcher comes back from the field unperturbed by the experience and
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willing to attest that the fieldtrip was unproblematic and a complete success. It is furthermore an illusion to think that emotions and personal relations play no role in fieldwork or do not influence the final product, although it is difficult to know exactly how and to what extent they will. The only really good piece of advice is to tread carefully, be sensitive and question one’s own preconceived ideas and research questions as the research develops.

As for neutrality versus involvement, one simply has to decide on a case-by-case basis how much one wants to and can be involved. I’m rather bemused by the fact that most Chinese people I meet have already made up my mind for me. They take it for granted that I am all for protecting the Chinese cultural heritage, and they are not far from right either.

NOTES

1 I have found Marlene de Laine’s (2000) book very useful and stimulating for thinking of these issues.

2 In much of the work dealing with fieldwork the definition of sensitive issues is rather narrow and has been formulated with Western societies in mind. Sensitive issues are thus primarily defined as concerning matters of privacy, including sexual behaviour, deviance and social control (Lee 1993).

3 For information on city redevelopments and preservation in Beijing, see Merle and Peng 2003: 37-41. For information on protests regarding resettlements, see Human Rights Watch 2004.

4 Since cultural heritage issues also fall within the field of ideological and propaganda work it can furthermore be regarded as sensitive when one wants to deconstruct the ideological and political motives behind cultural heritage work.

5 In 2004, the Chinese government acknowledged the problem of forced evictions and the brutal methods used and issued several policy documents to control resettlement and eviction.

6 Others have also met expectations that they might solve problems or use their influence to alert national authorities to local problems. See for example Xin Liu (2000: 19).

7 de Laine (2000) pays attention to the dilemma of closeness and distance in relations with informants

8 For one example of a researcher who became actively involved with an NGO while doing research on foreign workers in Japan, see Roth 2003.

9 In my book (Svensson 2002) I made a point of mentioning that my interest in human rights began not as theoretical musings but in a concern about real human rights problems. One of the reviewers of the book interestingly enough commanded me for this position, and argued
Doing Fieldwork in China


Xin Liu (2000) discusses how a conflict with his first informant in the village forced him to move house.

It is not always necessary to take sides but sometimes one’s choice of interviewees or attempt to hear all sides of a story can be seen as such. Helen Hardcare (2003), who was researching a religious group in Japan, thus faced a rift with the first branch of the group when she moved on to study a rival branch.

Mayfair Mei-Hui Yang (2004: 744) decided to avoid looking for and visiting temples that had been destroyed during a campaign to dismantle temples and churches that took place when she conducted fieldwork in 2000 because she was concerned about her acquaintances and her own research.

REFERENCES


APPENDICES
INTRODUCTION

This is a bibliography of methodological approaches and discussions concerning fieldwork in China, and it is to our knowledge the first of its kind. It does not claim to be universally inclusive or fully representative, nor do we claim that any of the references listed below is more representative or has a more solid foundation than those not included. The bibliography should be seen as a tool for students and researchers interested in doing fieldwork in China, and for those who have encountered problems during the fieldwork process.

The bibliography focuses solely on fieldwork in China and does not contain references to more theoretical discussions of fieldwork in general. Only material published since 1990 is included.

The emphasis is on the social sciences and humanities, and the references are roughly categorized according to the following academic disciplines:

- Social sciences and humanities in general
- Anthropology and ethnology
- Sociology
- Political Science
- Economy
- History

For each reference the following information – if available and relevant – has been listed:
Doing Fieldwork in China

Relevant passage
Topic of research (if not apparent from the title)
Method
Location
Period
Duration
Setting (rural/urban/residential area/factory/etc.)

I. SOCIAL SCIENCES AND HUMANITIES IN GENERAL

Bruun, Ole, Soren Poulsen, Hatla Thelle, eds (1991) Modern China Research: Danish Experiences, Copenhagen Discussion Papers, Special Issue, University of Copenhagen.

Topic: 11 papers on fieldwork methods in various academic disciplines.


Relevant passage: Chapter II: Primary Sources (pp. 41–68)
Method: Recording during public performance or under private conditions
Location: Yangzhou


Topic: Discussion on research design and methodology
Method: Surveys
Location: Tibet and Hebei
Period: 1989


Topic: Results from a survey in 1991 of 582 social scientists who had recently returned from fieldwork in China.


Topic: Possibilities and caveats in relation to the use of the Internet as research source in China
Method: Internet research
An Annotated Bibliography


- **Topic:** The status of population research in China
- **Method:** Interviews at selected Chinese universities; survey research by mail
- **Location:** Several large university cities, including Chengdu and Xi’an
- **Period:** 1985, 1986, 1987, 1988


- **Relevant passage:** pp. 39–47 in particular
- **Topic:** Theoretical and methodological discussion on how to approach and study *guanxi* in China


- **Topic:** Discussion on how to get started with fieldwork. Based on own experiences
- **Method:** Interviews and field observation
- **Location:** Yunnan
- **Period:** 1997
- **Duration:** 10 weeks
- **Setting:** Yunnan Normal University


- **Topic:** Short comments on the research environment in China after June 4th, 1989.


- **Relevant passage(s):** Appendix: A Bibliography of Essays on the State of the China Studies Field (pp. 373–392)
- **Topic:** Multi-themed bibliography of the China studies field
- **Method:** Several approaches and methods

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Topic: Advice on what to pack and how to handle food, housing and transportation


Relevant passage: pp. 23–28 in particular
Topic: Collecting folk songs
Method: Observation, interviews, recording performances
Location: Jiangsu
Duration: 7 months
Setting: Peasant communities


Topic: Discussion on structural and practical impediments to gathering qualitative and quantitative data, with examples from research on environmental protection.


Topic: Discussion on different methods of introducing oneself when conducting fieldwork


Relevant passage: Sources (pp. 251–260)
Topic: Rural education in the 20th century
Method: interviews, documentary research
Location: Zouping, Shandong
Duration: each visit three weeks


Topic: Introduction to local gazetteers as a historical information source
Method: Interviews
Location: Harbin
Period: 1989
Duration: One month
An Annotated Bibliography


Relevant passages: Chapter 1: Research, study, and teaching in China (pp. 1–7); Chapter 4: Research (pp. 83–114)
Topic: General introduction to research (archival research, fieldwork, survey research, and laboratory research)

Vallaster, Christine (2000) ‘Conducting Field Research in Asia: Fundamental Differences as Compared to Western Societies’, *Culture and Psychology*, vol. 6, no. 4, pp. 461–476.

Topic: culturally related barriers when entering the research field
Method: Interviews
Location: Hong Kong and China
Duration: 9 months


Relevant passage: Preface (pp. xiii–xy)
Location: Zouping, Shandong
Period: 1988–1993
Setting: Rural


Relevant passage: How to find laws in China (pp. 1–18)
Topic: Discussion on categorizing and finding laws in China.

II. ANTHROPOLOGY AND ETHNOGRAPHY


Relevant passage: Chapter 1: Fieldwork in Kunming: cognitive and linguistic anthropological approaches (pp. 17–48)
Method: Interviews, observation
Location: Kunming, Yunnan
Setting: dormitory, classroom, street, travel, etc.
Doing Fieldwork in China


Relevant passage: Chapter 1: Methodology and fieldwork (pp. 1–16).
Method: Interviews
Location: Chengdu, Sichuan
Duration: 12 months
Setting: Business households


Relevant passages: Methodology (pp. 2–9); Down to the countryside (pp. 9–12)
Method: Interviews with PRC emigrants in Hong Kong, interviews in Chen Village, observation
Location: Hong Kong and Chen Village, Guangdong


Relevant passages: Introduction. Imaging heaven: collective dreams (pp. 3–14); Appendix 2: Field methods (pp. 292–298)
Method: Household interviews
Location: Beijing, Shanxi, Sichuan, Yunnan, Henan, Shandong, Guangxi, Jiangsu
Duration: Several trips over a period of 10 years
Setting: Villages


Relevant passages: Fieldwork sites (pp. 12–17); Project development and methods (pp. 17–23)
Method: Archival research, in-depth interviews, participant observation, household surveys
Location: Lancang Lahu autonomous county, Yunnan
Duration: 18 months of ethnographic fieldwork
Setting: Rural

An Annotated Bibliography

Relevant passage: Chapter 1: Of maps, metaphors, perils and precautions (pp. 1–9)
Method: Interviews, participant observation
Location: Southwest China
Setting: University compound


Duration: 18 months of teaching and fieldwork
Setting: Urban/University


Relevant passages: Preface (pp. ix–xv); Introduction (pp. 1–5)
Method: Interviews
Location: Meifa village, Fujian
Period: 1992, 1995


Relevant passage: The research: in search of the Hui (pp. 98–111)
Method: Interviews, participant observation
Location: Beijing, Ningxia and also several fieldwork trips to dispersed Hui communities in China
Period: 1983–86 plus three shorter trips afterwards
Duration: 27 months


Topic: National identity and minorities, Hui
Method: Interviews, observation
Location: Beijing, and field trips to many locations in China
Period: Several trips from 1983 onwards


Relevant passage: Social change in Shanxi (pp. 21–24 in particular)
Method: Interviews, survey
Location: Shanxi
Period: 1996–98

Relevant passage: Chapter 1. Memories and numbers: sources of migrant information (pp. 13–41)
Method: Group interviews
Location: Nine counties in Shandong
Period: 1984, 1989


Relevant passage: Chapter 1. On binational collaboration (pp. 8–10)
Method: Cooperation with Chinese colleagues, observation, interviews, local data collection
Location: A dozen field investigation sites in Yunnan, Hunan, Fujian, and Guangdong.
Period: 1992


Relevant passage: Research (pp. xvii–xxi)
Method: Interviews, observation, participation
Location: Sipsong Panna, Lijiang, Kunming, all in Yunnan
Duration: 3 years extensively, 14 months intensively
Setting: Middle schools


Method: Participant observation
Location: Yanyuan county, Sichuan
Period: 1993
Setting: A trip together with a group of communist cadres


Method: Focus group interviews
Location: Hubei
Period: October 1995
Duration: 1 month
Setting: Urban/ rural: two towns and two villages
An Annotated Bibliography


Method: semi-structured interviews
Location: Caohai Nature Reserve, Guizhou
Duration: 5 months
Setting: Rural


Topic: Discussion on the use of anthropological methods in China


Method: Interviews and video recording
Location: Hangzhou, Zhejiang
Period: October 1995
Duration: 1 month
Setting: Rural women’s workplaces and guesthouses


Relevant passages: Appendix A. Methodology (pp. 307–309); Appendix B: Prestige ranking study (pp. 310–312)
Method: Guided and open-ended interviews, external and participant observation, collection of life stories. 300 households examined focusing on 75 key families.
Location: Hohhot, Inner Mongolia


Relevant passages: Introduction. The book: researchers and methodologies (pp. 6–10); Appendix III: Questioning Hui and Han women and men on quality of life – survey (pp. 315–319)
Method: Interviews, textual analysis, historical research
Location: Henan
Period: 1994–97
Duration: 3 years, extensively

Topic: Collaborative research across cultures
Doing Fieldwork in China


Relevant passage: The Study (pp. 13–22)
Method: Interviews
Location: Three villages in Shandong
Duration: Trips in three weeks stints
Setting: Rural


Relevant passage: Methodology in Data Collection (pp. 11–17)
Method: Interviews (unstructured and structured), questionnaires, participant observation, literature survey
Location: Hong Kong and Beijing
Period: 1990–1991
Duration: 4 months
Setting: Four factories


Relevant passage: Introduction (pp. 1–20)
Method: Interviews, participant observation
Location: Fengjia, Zouping county, Shandong
Duration: 7 months total
Setting: Rural village


Topic: Anthropological research of a lineage separated by history and geography
Location: Singapore and Fujian


Relevant passage: Methodological Appendix: the ethnographic labyrinth (pp. 171–177)
Method: Interviews and observation
Location: Shenzhen and Hong Kong
Period: 1992, 1993
Duration: Each fieldtrip 2–3 months
Setting: Factory
An Annotated Bibliography


Relevant passages: Host and Hostage (pp. 16–20); Everyday Practice (pp. 20–27)
Method: Participant observation.
Location: Zhaojiahe, Shaanxi
Setting: Rural


Relevant passage: Chapter 2: Outline of the research project (pp. 9–24)
Method: Interviews, surveys, literature review
Location: Inner Mongolia, Gansu, Xinjiang
Setting: Pastoral regions


Relevant passage: Methodology (pp. 153–155 in particular)
Method: Participant observation over the Internet
Period: October 1995–May 1997
Duration: 16 months


Relevant passages: Chinese mass communication research (pp. 34–38); The research project (pp. 40–47)
Method: interviews
Location: Shanghai, Beijing, Guangzhou and Xi’an
Period: 1986


Relevant passage: Catholics are nothing but trouble: Fieldwork in China (pp. 15–21)
Method: Interviews and observation
Location: Tianjin and its suburban counties; Cangzhou, Hebei
Period: 1992, 1993
Duration: 3–5 months.
Setting: Church communities
Doing Fieldwork in China


Relevant passage: Appendix: Methodology (pp. 200–218)
Method: Grounded theory, survey, interviews, documentary sources
Location: Beijing and Shenyang
Duration: 7 months
Setting: One factory and one residential area in each city

Topic: Discussion on the importance of serendipity during fieldwork in China


Relevant passage: The construction of a fieldwork project in the People’s Republic of China (pp. 1–25)
Method: Interviews, observation
Location: Beijing
Duration: 8 months
Setting: Beijing


Relevant passage: Preface (pp. xi–xiii)
Method: Interviews and observation
Location: Chashan district, Dongguan, Guangdong


Relevant passage: Preface (pp. xi–xvii)
Method: Formally structured interviews, informal conversations, individual life stories, household surveys, village mapping, and genealogical reconstructions
Location: Qiaolou village, Sichuan
Period: 1990–91
Duration: 8 months
An Annotated Bibliography


Method: Interviews and observation
Location: Quanzhou region, Fujian; Taiwan
Period: 1986–1995
Duration: Several visits of various lengths


Relevant passages: Introduction (pp. xi–xvii); Chapter 1: The Chinese family (pp. 1–11)
Method: Interviews and statistics
Location: China and Taiwan
Duration: Extensively, various trips and contacts
Setting: Families


Relevant passage: Peasant memory and oral history: A note on methodology (pp. xiii–xix)
Method: Interviews; group oriented interview method
Location: Three counties and three towns on the North China plain
Period: 1985–1993
Duration: 8 years extensively
Setting: Peasant communities


Relevant passage: Survey of women in Beijing and Shanghai (pp. 45–48)
Method: Interviews
Location: Beijing and Shanghai
Period: September 2001
Duration: 1 month
Setting: Female managers and employees in private-sector enterprises


Relevant passage: Introduction: Fieldwork, politics, and modernity in China, (pp. 1–45)
Method: Interviews, observation, participation
Location: Beijing, Shandong, Hubei, Zhejiang, Shaanxi
Period: Extensively from 1980 to 1993
Doing Fieldwork in China


Relevant passage: Appendix: Notes on the conditions and politics of fieldwork (pp. 213–216)
Method:Semi-structured, open-ended interviews, observation
Location: Zhejiangcun village in Beijing
Period: 1994-1996
Duration: 16 months
Setting: Urban

III. SOCIOLOGY


Method: Interviews, observation
Location: Sipsongpanna, Yunnan
Period: 1990
Duration: 3 months
Setting: The Tai Lui people of Sipsongpanna


Topic: Overview of different aspects of sociological research in China, includes a 10 page bibliography


Relevant passage: Introduction (pp. 1–33)
Topic: Methodological and theoretical discussions concerning ‘eating’ and ‘desire’ as approaches to the study of post-socialist China.


Topic: Introduction to survey research in China

An Annotated Bibliography


Topic: Brief discussion on the cultural difficulties a researcher can encounter when gathering qualitative or quantitative data in Asia


Topic: Discussion on methodological issues in relation to sociological research in China


Relevant passage: Research design and data collection (pp. 33–37)
Method: Interviews and observation
Location: Wuhan, Hubei
Period: 1979–1989
Duration: 10 years, extensively
Setting: Four different work units


Topic: Survey research in urban China.


Topic: Discussion on approaches and methods for sociological studies of China.


Relevant passage: Part 2: Methodology (pp. 39–49)
Method: Interviews, participant observation
Location: Hongkou and Sahe villages, Sichuan
Duration: 1 year
Setting: Rural village


Relevant passage: Chapter 4: Research design and methodology (pp. 94–127)
Method: Survey interviews, experimentation
Doing Fieldwork in China


Relevant passage: Chapter 3: Research methodology (pp. 19–39)
Method: Focus groups, interviews, participant observation
Location: Beijing
Duration: 1 year
Setting: Residential neighbourhood

IV. POLITICAL SCIENCE


Relevant passage: Chapter 1: Studying government and economy in Shulu: goals and methods (pp. 1–12)
Method: Five-member research team. Interviews with government officials and others, participant observation
Location: Shulu county, Hebei
Period and duration: 1979, 1986, and 1990, duration of each visit: from 12 days to two months


Relevant passage: Data and method (pp. 10–11)
Method: Written sources


Topic: Methodological discussion of a collaborative research project on China’s TVP sector (Township, Village and Private enterprises) conducted by CASS and the World Bank.
Method: Survey, interviews


Relevant passage: Methodology and organization of this book (pp. 17–21)
Method: Interviews
Location: Wuxi and Xishan, Jiangsu
Duration: 11 months
An Annotated Bibliography

Setting: Scholars, government officials, party cadres, enterprise managers, people’s congress deputies


Relevant passage: Research Design (pp. 16–21)
Method: Interviews, documentary sources, statistics
Location: Shanghai, Guangdong, Zhejiang, Shaanxi, Shandong, Fujian, Hainan, and Sichuan


Relevant passage: Methodology (and inherent bias) of our research (pp. 17–18)
Method: Interviews
Duration: 1 year
Setting: Urban


Relevant passages: Data collection (pp. 18–20); Appendix: Position of the interviewees (pp. 181–182)
Method: Documentary research, interviews.
Location: Beijing, Anhui, Heilongjiang and Shandong
Period: 1992, 1994


Relevant passage: Appendix: Survey design and implementation (pp. 86–88)
Method: Survey
Location: Two counties in each of the following provinces: Zhejiang, Shandong, Hebei, and Hunan.
Period and duration: Fall 1997; spring 1999.


Relevant passages: Methodology (pp. 15–17); Appendix A: Methodological notes including list of interviewees (pp. 177–179)
Doing Fieldwork in China

Method: Interviews
Location: Tianjin
Period: 1991–92
Duration: 13 months
Setting: urban, municipal departments


Relevant passages: Discussion on methodology (pp. 8–18); List of interviews (pp. 200–207)
Method: Interviews with local cadres
Location: 12 counties in Jiangsu, Shandong and Zhejiang.
Period: 1996–1999
Duration: 22 months


Topic: Discussion on adequacy, reliability, evaluation and access to Chinese data on military spending


Topic: Discussion on studying China, including fieldwork


Relevant passages: Data and methodology (pp. 10–16); Methodological conclusion and limitations of the study (pp. 262–266)
Method: Written sources, interviews
Location: Beijing, Wanzhou, Chongqing, Yichang, Nanjing, Wuhan
Setting: Multiple settings from government offices to resettlements.


Relevant passage: Appendix. Sources of Chinese data (pp. 208–210)
Method: Provincial newspapers and almanacs; interviews
Location: Jiangsu, Guangdong, and Beijing
An Annotated Bibliography

Relevant passage: pp. 258–259 in particular
Topic: Access to documents about China’s foreign relations

Relevant passage: Basis for the present study (pp. 8–12)
Method: Survey, discussion of Chinese official data

Relevant passage: Sources of Data and Methodology (pp. 14–20)
Method: Quantitative analysis of biographical data; case studies of political networks; qualitative examination of values and policies of leaders

Relevant passage: Data (pp. 27–30); Chapter 3: Methodological problems of Chinese opinion polls (pp. 74–120)
Method: Collection of 329 opinion polls conducted 1979–1992

Relevant passages: 1.2 The Institutional approach and research methodology (pp. 17–23); 1.3 Research stages (pp. 23–24)
Method: Survey and census data, interviews
Location: Beijing and other cities
Period: 1997–1999
Duration: 5 months
Settings: Ministries, research institutes, municipal governments, state enterprises.

Relevant passage: The scope and methods of research (pp. 6–7)
Method: Mainly textual sources, interviews
Duration: Several field trips
Doing Fieldwork in China


Topic: Methodological discussion on how to evaluate and overcome difficulties in survey and interview research in China


Relevant passages: A note on sources and methods (pp. 19–21); Appendix: Survey methods (pp. 165–177)

Topic: Discussion on survey research methods, and on the combination and sequencing of different methods
Method: Survey, interviews, documentary sources
Location: Exploratory interviews in Beijing, first survey in a city in Northeast China, second set of interviews in Beijing with cadres from different provinces


Relevant passage: Interpretation and Analysis (pp. 49–65)
Method: Interviews
Location, Nanjing, Jiangsu
Period: 1994
Duration: 2–4 months


Topic: Approaches, sources, studies on the development of the Chinese military.


Relevant passage: Appendix A: Research and documentation (pp. 205–210)
Method: Interviews
Duration: 1–3 weeks in each place
An Annotated Bibliography


Relevant passage: Research location and methodology (pp. 932–938 in particular)
Method: Self-administered questionnaires, survey, interviews
Location: 4 villages in Zhejiang
Period: Summer 2000


Topic: Discussion and list of Chinese language written materials; discussion of interview sources


Topic: Discussion and overview of sources, including interview sources


Relevant passage: Questions, problems, and methodological limitations (pp.1–8)
Method: Interviews, participant observation
Location: Beijing
Setting: Chinese Academy of Social Sciences.


Relevant passage: Chapter 4: Sources, research techniques and the problem of understanding (pp. 80–90)
Method: Documentary sources and interviews
Location: Shenzhen and six cities in Guangdong
Duration: 9 months


Relevant passage: Appendix 3.1: The data (pp. 223–226)
Topic: Discussion on using Chinese official statistics
Doing Fieldwork in China


Relevant passages: Character of the research (pp. 12–17); Appendix B: Fieldwork in Xiamen’s business community. A methodological essay (pp. 244–251)

Method: Interviews, socializing and conversations, limited participant observation, brief visits to other localities

Location: Xiamen, Fujian


Duration: 18 months


Relevant passages: Design of the study (pp. 29–37); Appendix: List of informants (pp. 300–313)

Method: Interviews, archival research

Location: Songjian county, Shanghai; Wuxi city, Jiangsu; Yueqing county, Zhejiang. Archival research also in Hong Kong


Duration: 21 months of fieldwork


Relevant passage: Chapter 1: The project and its methodology (pp. 1–15)

Method: Interviews

Location: One county in eastern Shandong and one county in southern Jiangsu

Period: 1992

Duration: 4 months


Topic: How to categorize and classify documents from the central government


Relevant passage: pp. 137–142 in particular

Method: Surveys, in-depth interviews

Period of research: 1996

Duration: 4 months
An Annotated Bibliography


  Relevant passage: The local and regional context of my research (pp. 7–12)
  Method: Local data collection
  Location: Jiangsu (several counties and cities)

V. ECONOMY


  Relevant passage: Chapter 2. Methodology: Commodity chains and interviews (pp. 18–24)
  Method: Commodity chains analysis, interviews
  Location: Shanghai
  Period: 1997–2000
  Setting: Different industrial sectors


  Topic: Discussion of prefecture-level statistics, introduction of the ICCE Database. The database and further discussions can be found on www.on-China.de


  Relevant passages: Research methodology (pp. 14–17); Appendix A: Questionnaire (pp. 189–190)
  Method: Interviews
  Location: Tianjin
  Period: 1991
  Duration: 5 months
  Setting: Urban


  Relevant passage: Appendix 2: The research sample (pp. 169–174)
  Method: Interviews
  Location: Multiple locations, including Beijing, Shanghai, Guangzhou and Hong Kong
Doing Fieldwork in China


Relevant passage: Chapter 3: Research method and design (pp. 46–123)
Method: Survey, observation, interviews, census data from statistical yearbooks, photographs of retail outlets
Location: Guilin
Period: 1998
Duration: 5 weeks


Topic: Methodological discussions on how to undertake research on managerial issues in China
Method: Surveys


Topic: Cross-national research
Method: Interviews
Location: China and Singapore
Setting: Enterprises, managers

VI. HISTORY (ARCHIVES)


Method: Archival research, history
Location: Liaoning


Topic: Discussion on the type of materials accessible through fieldwork at publishing sites
Method: Tape recording, collection of local artifacts, local documents
Location: Western Fujian, eastern Jiangxi, southern Zhejiang, northern Guangdong and Guangxi.
Setting: Sites for block-carving, publishing and book selling
An Annotated Bibliography


Method: Archival research
Location: Fuzhou, Fujian
Duration: 1 year
Setting: Fujian provincial archives, Fuzhou municipal archives


Method: Archival research
Location: Beijing
Period: 1990


Method: Archival research
Location: Gansu
Period: 1990


Topic: Introduction to the use of Chinese archives
Method: Archival research
Location: Chinese central and local archives
Period: Several visits since 1982


Method: Archival research
Location: Beijing
Period: 1994
Duration: 2 months
Setting: Beijing University Library


Topic: National and local archives. In relation to this book a web-based supplement is available at the ‘Chinese History Research Site’ at University of California, San Diego: http://orpheus.ucsd.edu/chinesehistory/chinese_archives.htm

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Method: Archival research
Location: Shanghai
CHINESE GLOSSARY

baixing  百姓
baogao  报告
baomu  保姆
caisfang  采访
changwai jiaoyi  场外交易
chidiaole  吃掉了
cunmin  村民
cunzhang  村长
daguai  打拐
dangguande  当官的
danwei  单位
diaomin  刁民
difangzhi  地方志
dingzihu  钉子户
duomin  堕民
fangyan  方言
fu diar zeren  负点儿责任
fulao xiangqin  父老乡亲
ganbu  干部
ganbu gangwei mubiao  干部岗位目标责任制
gonganju  公安局
gongmin  公民
gongtongdian  共同点
Doing Fieldwork in China

gongwuyuan 公务员
han 官
Guanhua 官话
guanxi 关系
guanyuan 官员

Han 汉
hen pusu 很朴素
hongtou wenjian 红头文件
huayu tixi 话语体系
Hui 回
huiyi jiyao 会议纪要
hukou 户口

jihua danlie 计划单列
jiangli 讲理
jianmin 贱民
jiapu 家谱
jihua shengyu weiyuanhui 计划生育委员会
jimi 机密
jing er yuan zhi 敬而远之
juemi 绝密
jueyi 决议
junei ren 局内人

kaocha 考察

laobaixing 老百姓
laojia 老家
laowai 老外
lianxi 联系
lingdao guanxi 领导关系
liudong renkou 流动人口

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luohou 落后
mimi 秘密
nagongzide 拿工资的
Nanfang Zhoumo 南方周末
neibu 内部
ni mei you renshe 你没有人生追求吗?
nianjian 年鉴
nongmin 农民
peitong 陪同
Putonghua 普通话
qiaoxiang 侨乡
qing ju yige lizi 请举一个例子
qingshi 请示
que deshi 缺德事
qundai 裙带
qunzhong 群众
qunzhong luxian 群众路线
renfanzi 人贩子
renqing 人情
renquan 人权
shangfangcun 上访村
shehui qingxu 社会情绪
shenfenzheng 身分证
shi waiban 市外办
shucheng 书城
shunkouliu 顺口溜
suku 诉苦

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suzhi bijiao di 素质比较底
tuhua 土话
waishiban 外事办
wenmang 文盲
wenshi ziliao 文史资料
wenyanwen 文言文
wode nongmin 我的农民
xiagang 下岗
Xiangcun jianshe 乡村建设
xiangguan 乡官
xing 姓
xingzhengcun 行政村
xinku 辛苦
Xuanweibao 宣威报
yuanfen 缘分
zhanban 展板
zheng yizhi yan bi yizhi yan 睁一只眼闭一只眼
zhidao 指导
Zhongguo nüxu 中国女婿
Zhonghua Quanguo Guiguo Huaqiao Lianhehui 中华全国归国华侨联合会
zougou 走狗
zuotanhui 座谈会
zuzhibu 组织部
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