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From the Research Notes of a Foreign Devil: Disability research in China Emma Stone

INTRODUCTION

In July 1995, the *British Journal of Sociology* accepted an article for publication. The authors: Emma Stone and Mark Priestley. The title: 'Parasites, pawns and partners: disability research and the role of non-disabled researchers'. The text: the emancipatory paradigm, its why, its what, its implications for us, our fieldwork and our PhDs. The sub-text (for myself at least): doubts regarding the practicability, the necessity, even the desirability of an all-or-nothing emancipatory research paradigm.

As I re-read the article, I am transported back to that time 'Before Fieldwork' when the motivated postgraduate is initiated into the inner circles of social science through ritual exposure to research methodology. Having come from Oriental Studies through Development Studies, I was unprepared for the delights which awaited me in a Sociology and Social Policy department. I drank in as much as I could of epistemology, ontology, triangulation, validation, evaluation ... until the fear of months away from my husband and home seemed nothing compared to the terror of being labelled parasite, of being put along with my research design and data in the methodological dock upon my return. Emancipatory research was hardest to imbibe ... not because I didn't like the look of it I like radical, I like uncompromising, I like principles which point at privilege and salute visions of a new world -why else do Chinese and Development Studies? -but because I could not see how I could square my research on disability in China within an emancipatory research framework.

Long before I came across the emancipatory paradigm, I determined to do participatory action research in line with current thinking on fieldwork in developing countries. But, to echo Zarb (1992) and Oliver (1992), emancipatory research is more, much more than those. Central to the emancipatory paradigm are a reversal of the social and (ultimately) the material relations of research production and a grounding of agenda,

analysis and action in the social model of disability. I took those ideals on board and in so doing I became increasingly anxious: the chances of realising the paradigm's ideals seemed so slim as to make me question the decision to proceed. Encouragement from Mark and from my supervisors Colin Barnes and Delia Davin ensured that my unease did not stop me researching. But nor did my unease stop. Thankfully, I found relief in a good friend and fellow postgraduate, also in China, for whom home-grown methodologies seemed similarly unreal. We were each other's partner in research crime, each other's reminder that all you can do is your best and what do people back home know anyway.

Eighteen months on and having spent half of that time in China, it is time to reconsider my research methodology with balanced hindsight rather than fearful anticipation. Give me another eighteen months and I might be less scathing of research methodologies -but at the moment my mind is in the reality of doing research in China. And it is something of that reality that I want to communicate here. The paper explores the three issues of researcher identity, the social relations of research production and the applicability of the social model in a cross-cultural context. My discussion of these topics is very much the product of my own fieldwork experiences, so I illustrate my points with reference to fieldwork I undertook in two rural counties in China: Heping County and Shanlin County (renamed to protect confidentiality).

HALF-DEVIL: RESEARCHER IDENTITY

'People in the field will also seek to place or locate the ethnographer within their experience' (Hammersley & Atkinson 1983: 77).

Chinese people have a name for foreigners: yang guizi, the foreign devils, predominantly white and western, most definitely Other. Such an apparently indiscriminate grouping does not render nationality obsolete, hence the almost inevitable opening line of a first encounter: what nationality are you? I am British. British Smog. British Empire. Hong Kong. Thatcher. Mad Cows. Documentaries.

My arrival in China followed hard upon the second and extended showing of a documentary on China's orphanages. This was British television filmed by young British journalists, undercover as charity workers. And there I was. Young, British and interested in disability - a matter for social welfare and therefore for the same Ministry that is

responsible for China's orphans. At national, municipal and county level, doors closed. There was a revived awareness of the dangers in associating with foreign devils. Association means responsibility and responsibility incurs risk: risk for the cadres that permit access; risk, too, for international organisations which have invested years in building relationships with those cadres. I had arrived in Beijing airport with a Government Warning invisibly tattooed on my forehead: Foreign Devils Can Seriously Damage. And it dawned on me that my preoccupations with doing disability research as a non-disabled researcher were of minor importance in China. Here it was nationality that mattered. Somehow I had to disentangle myself from the very negative images evoked by my British identity, lest my research should fail before it had begun.

Constructing an appropriate self, or selves, in order to facilitate data collection has been coined 'impression management' by Hammersley and Atkinson (1983) and it is largely unavoidable for any researcher engaging in cross-cultural research where colonial and neo-colonial ties operate. Those who wish to avoid being tarred with the colonialist brush naturally employ strategies to confound such perceptions (Hammersley and Atkinson 1983; Devereux and Hoddinott 1992; Francis 1992). Success in this endeavour depends on skill and luck - or a fortuitous combination of attitude and ancestry in my own case.

Attitude

'Researchers who are concerned about appearing judgmental or ethnocentric tend to present themselves as bland, pleasant individuals who never disagree with anyone' (Devereux & Hoddinott 1992: 19).

For a time, I dressed down, I looked down, and I toned down all my research questions. I was so concerned not to play the arrogant expatriate that I over-compensated.

As regards data collection, I was desperately anxious not to cause embarrassment through inappropriate questioning. When I first arrived in China, I hardly dared mention the words 'disability research' for fear that it would create tension or that I might find myself on the next plane back to Heathrow. It was some time before I realised that I was treading on eggshells where the ground was often hard as iron.¹ My images of China, doubtless distorted by the western media, were such that I

¹ See Lockwood (1992), Hoddinott (1992) and Francis (1992) for similar accounts of real and imagined sensitivities when researching in developing countries.

imagined - as maybe you do now - that it would be impossible to inquire about disability discrimination let alone eugenics. How wrong I was! This is not to argue a case for the insensitive researcher. There are times and places for asking questions and it is right that a researcher should respect those. But it is imperative that a researcher learn them first.

Excessive caution, based on my own prejudices rather than informed experience, placed similar inhibitions on my personality and behaviour. Initially, I took on a meek and mild persona. I wasn't comfortable in that role, but I thought it would be less threatening to Chinese research subjects and friends. That is as may be, but it was also far less interesting for them as my karaoke (or ka-la-OK! in Chinese) career illustrates: my debut was on the Beijing karaoke scene. Self-effacing apologies ('I am an awful singer and far too shy') which would work in Britain failed to impress in China. When I did take the microphone, I failed even more - my voice was so quiet that no-one could hear me and my hosts were most definitely disappointed. So once in Heping County, when I found myself in a factory's ballroom, I knew what I had to do. I took my place beneath the mirrored disco globe and forty sets of intrigued eyes fixed on me. Microphone in one hand and plastic flowers in the other, I gave it all had in an ear-blasting, lung-bursting rendition of ABBA's 'Dancing Queen'. It was a triumph and my research relationships soared! And so I discovered that when Robert Chambers (1983) writes of the need for humility in research and overseas development work, this should not be equated with bowing and scraping and staying in the background. True humility can entail performances of the most unlikely and flamboyant kind.

It seems that I became adept at the art of impression management. I learnt what went down well, what conversational gambits would amuse or relax or impress; I asked for advice and I was honest and direct in interactions. But what is particularly interesting about this process is that as I became more true to what I felt to be my everyday British self (karaoke excepted), my hosts believed that I was taking on more Chinese attributes. Therein lay the interface between my attitude and my ancestry.

Ancestry

Ancestors occupy an important place in Chinese lives. Through accident of birth (this is where luck rather than skill takes over) I have distant Chinese ancestry. My maternal grandmother was born in Hong Kong in 1904. She lived in Hong Kong until her husband's death in the late 1960s, whereupon she journeyed to England to live with my parents, my

sister and myself. My grandmother belonged to the Eurasian strata of Hong Kong society: a mix of Orient and Occident, a weaving together of the Middle East, Europe and China, wealthier than local Chinese but kept at arms length by the colonial elite. Her death, in October 1995, brought me back home. I have always loved to talk about my grandmother and never more so than when I returned to China to resume fieldwork in 1996.

I believe now that my ancestral claims go a long way towards explaining the ease with which I gained trust and support, the readiness with which I was introduced to others and brought into confidences. As someone with Chinese blood ties, my passport nationality proved less problematic. Indeed, my ancestry took on a wholly new significance. My ability to speak Mandarin, my interest in all things Chinese, the gusto with which I consumed Chinese food, even my sense of humour, opinions and values were frequently attributed to my ancestry. And, out of emotional need rather than cunning, I played along. Asserting the Chinese side of my identity enabled me to maintain a bond with my grandmother. I was a willing accomplice in a beautiful act. At the same time, my hosts were glad to find additional grounds for trusting me and to explain why it was that I didn't conform to their perceptions of white, western Other. Less flattering images of foreign devils remained intact.

In summary, I derived legitimacy from my Chinese ancestry in much the same way that I imagine disabled researchers doing disability research derive legitimacy from disability status or gay researchers doing research on gay issues derive legitimacy from their sexuality and so on. Yes, identity does make a difference but it is far from the last word on what makes a good and trustworthy researcher. Ancestry would have counted for little had I been that arrogant expatriate. It was ancestry and attitude together that enabled me to disentangle myself from some of the more negative aspects of my initial ascribed identity. Finally, it is important to stress that in drawing on, some might say exploiting, my heritage, I was not looking to go native. Impression management is not about living an Other's life. It is about giving face, showing respect, earning confidences, creating bonds. It is in this context that what might otherwise seem like small and silly things take on an inestimable importance.

DANGEROUS LIAISONS: THE SOCIAL RELATIONS OF MY RESEARCH PRODUCTION

'There is no independent haven or middle ground when researching oppression: academics and researchers can only be with the oppressors or with the oppressed' (Barnes 1996: 110).

Two assumptions underpin this statement. First, that it is possible to draw a clear dividing line between the oppressors and the oppressed. Secondly, that the nature of research on oppression is analogous to a zero-sum game, whereby the oppressors win and the oppressed lose or vice versa, depending on whose side the researcher plays. The extent to which a researcher has a choice in the matter is limited, as Barnes (1996), Bury (1996) and Shakespeare (1996) concur in their exchanges on disability research and independence. Most obviously, choice is restricted by fundholders and academic peers. Nonetheless, Bury and Shakespeare view independence, carefully defined, as a necessity, whereas for Barnes independence is at best mythical and at worst counter-emancipatory where the researcher holds onto control which might otherwise be devolved to disabled research subjects.

What would any of them make of my research? Although I could have taken advantage of the relative freedom afforded a PhD candidate, I chose instead to do research which would make it impossible to devolve full control to research subjects and equally impossible to exercise full control myself. In addition to the standard triangle of fundholders, academic peers and researcher, I added disabled people, parents of disabled children, grassroots project workers, local government cadres and two international organisations. If that wasn't enough, I elected to formalise local-level partnerships with government officials in using a team approach to conduct research. Thus, in deciding to do research in Heping and Shanlin counties, I entered a situation in which I did not know whether the inevitable compromises would be validated. I took the risk and placed others at risk in so doing.

The following is an outline of the main partnerships formed, compromises made in accordance with those partnerships and my justifications for straying from the paths of maintaining independence or devolving control to disabled research subjects.

International Organisations

In Heping and Shanlin, my first partnerships chronologically speaking were with an international non-governmental organisation and a multilateral aid agency. Prior to research taking place, the potential for channelling both the research process and outcomes into programme development was discussed and both organisations felt that process should be as, if not more, important than outcome. As regards process, we agreed on the primacy of participation and of using the research as a relationship- and capacity-building exercise with local government counterparts. As regards outcome, I agreed to write reports to include recommendations for programme development in the respective counties and to lead a workshop on participatory action research for national-level Chinese counterparts of the multilateral agency in Beijing. There was little need for compromise in the research process but it is understood that publications arising from my finished thesis should be cleared with the respective organisations first. Some of my biggest compromises might still be to come.

Government Cadres

The work of both organisations is at the invitation of and in partnership with local Civil Affairs bureaux. Inevitably, therefore, my research followed suit. In Heping, negotiations with local officials were straightforward since everything was arranged through an intermediary (the relevant Program Officer) and before my arrival in Heping. The specified time-frame was one week: any longer and local authorities would question our motives since standard evaluations lasted little more than a day. I submitted a research design. Heping officials faxed us their's. We proposed further revisions, some of which were taken up and others weren't. At the time, I was overjoyed at the extent to which some aspects of my initial design had been taken on board: a team of three local co-researchers under my direction; home-visits to meet with disabled adults and families with disabled children; visits to welfare factories incorporating focus groups with disabled workers. However, certain departures from my original schedule proved very problematic.

I had proposed to spend one day in each of two selected factories researching the situation of disabled factory workers in one-to-one interviews and a focus group discussion. Heping officials, by way of spreading the foreign guest and evaluation burden more thinly, made arrangements for visits to eight welfare factories. They were unwilling to renege on these arrangements. As a result, time was limited in each factory and divided between a management briefing and a focus group

discussion; one- to-one interviews could not be accommodated. This limited the extent to which focus group discussions could realize their potential. More difficult still was the highly skewed sampling which meant that the majority of disabled adults I encountered were welfare factory workers and male.

In Shanlin, negotiations and compromises seemed tougher still. At the end of my previous visit in July, local cadres agreed that I could return in the autumn to do collaborative research. Accordingly, I returned in November hoping that the three weeks allocated would be used in training and leading a local team in participatory research. On arrival, I presented my ideas. I would lead a team drawn from local disabled people, parents of disabled children and government cadres in conducting interviews in three sites in the county, each site exhibiting a different socio-economic level. We would spend five or six days in each site, with two focus groups per site in addition to one- to-one interviews. Interviews and discussions would be taped with participant consent. Content would relate to the needs and aspirations of local disabled children, adults, their families and their communities as well as general socio-economic data on the region and information on existing services.

Within hours of laying out my ideas, relevant phonecalls had been made, three sites and a team had been selected (which included a young disabled woman and might have included a young disabled doctor but for a case of mistaken identity) and I was invited to start training and teamwork the next morning. However, frequent interruptions from officials during the training sessions made it clear that higher permission would be required unless I drastically reduced the scope of my proposed research. I had neither the time nor faith to await provincial consent and I was unwilling to throw my hands in the air and cry 'forget this, I'm going back to Beijing' since such a gesture could have jeopardised relations between local counterparts and the involved international organisation. So I compromised, guided by more and less subtle hints from local officials. The outcome: two days in each of two sites (neither of which was a poor area although we did interview people from relatively poor households), interviews with disabled adults, parents of disabled children, local teachers and doctors but no interviews with local cadres and only one focus group per site, no tape recording and only supervised photography, and no questions pertaining to general socio-economic data.

Research Teams

In Heping County, the team was small and was eager to follow my lead in the formulation of questions and data collection methods. I had been brought in to lead the team and to train them in evaluation techniques so there were limits to the degree to which they were willing to suggest alternatives. In Shanlin County, on the other hand, the research team was made up of nine permanent members and three representatives from each of the original sites. The formulation of research questions and methods was a more participatory exercise which meant that reaching agreement sometimes proved very difficult. One of the most articulate members of the team (not, incidentally, a cadre) felt that nothing short of a closed questionnaire was good enough. 'Questionnaires are scientific and objective', he told me. His views were held by others in the team. In the end, we produced a guide-sheet and two activity sheets (uncomplicated tasks which required participants to circle sources of support and information and to draw comparisons between their own life-chances and those of other disabled people or families in the area). The method worked relatively well but would have been more successful had time been no object: a total of four days meant that a pilot was out of the question. Working with such a large team magnifies the problems involved with using research assistants, particularly where the nature of research is such that more than enumeration is required (Devereux & Hoddinott 1992b). Validity, reliability and comparability of data are all liable to suffer in the process.

Disabled Children, Disabled Adults, Their Families

Inevitably, perhaps, forming links with the more powerful members of a community inhibits relationships with less powerful members. Whilst I maintained what Truman and Humphries (1994:1) describe as 'a conscious partiality with those who are marginalised or invisible', the extent to which that partiality was visible and therefore meaningful to disabled research subjects and their families is open to debate. In the context of interviews and focus groups, I used every opportunity to demonstrate that partiality through words and attitude, but it is impossible to know whether or how far I succeeded. What is beyond doubt is that occasionally my desire to involve disabled people and families with disabled children in a meaningful way was frustrated by the unanticipated working-out of that desire.

My biggest regret is that I was not party to the selection of research subjects. In China, as Manion (1994) notes from her own research,

sampling for large-scale surveys let alone for one-to-one interviews is invariably biased. My concern, however, lies less in statistical bias than in the process of participant selection. The request for participants would have been made by local cadres or, in Heping, by employers and was not therefore a request that would be easy for anyone to refuse. While some participants (to judge from body language and vocal participation) relished the opportunity to voice aspirations and share life stories in the presence of a foreigner and entourage of cadres, others were clearly alienated by the whole process.² In my eagerness to practice participation-friendly methods and in my hosts' desire to fulfil my expectations, the wishes of research participants were relegated.

In Heping, I had assumed - wrongly and naively - that participants would participate only if they wanted to. It is good fortune alone that most seemed happy to be involved. I wonder if it is possible to balance the alienation of the few with the eager involvement of the many and come out with a half-way clear conscience? In Shanlin, I endeavoured to reduce risks of alienation by advising cadres that only people who were willing should participate, and by dividing the interviews between three teams thereby reducing the official entourage and the intrusive presence of a foreigner. Even so, there was still one occasion when a research participant was evidently tense and unsure as to what answers were acceptable, in spite of a village cadre's personal assurances that there would be no 'contradiction' and he could speak freely. (How far he believed that is impossible to say, although it is entirely possible that the cadre meant what he said.) I also tried to create more space for scrutiny by disabled participants or family members by structuring the two focus group discussions around analysis and planning rather than around individual lives (this was possible in Shanlin because focus groups built on one-to-one interviews). But these are small gestures compared to the ideals of emancipatory research.

Multiple partnerships: rights and wrongs

Quite a list of liaisons and compromises, then. With hindsight, there is much that I would have done differently but I would not change my decision to make multiple partnerships. I have good reasons.

First, working with government cadres enabled as well as inhibited the participation of disabled individuals and their families. It would have been impossible for me to knock on the doors of ordinary people in Heping or Shanlin and expect to be invited in without some form of

² See Thomas Gold (1989) on the pitfalls of officially arranged research in China

official introduction. Local officials were the principal gatekeepers and I could not have involved disabled people and families without them.³

Secondly, there would have been no possibility of research leading to action without linking with officials and international organisations. In certain contexts (and China is currently one of them) the replicability and sustainability, let alone implementation, of development projects depend on local government involvement. Co-operating with officials significantly increased the likelihood that research would make a difference in the lives of local disabled children, adults and their families. That was something all of us wanted to achieve.

Thirdly, given the role of civil affairs cadres in the lives of Chinese disabled people, conducting collaborative research was an excellent way to learn about local government responses to disability. There is a dearth of information on local responses to disability in China thereby encouraging the dangerous view that nothing goes on.

Fourthly, although social relations reflected real world hierarchies, there was still scope to challenge power relations and to highlight the importance of service provision with rather than for disabled people and families. Involving disabled individuals and their families at all was a significant departure from standard approaches to planning and evaluation in China.

Fifthly, in working with international organisations it has been possible to channel information to them on local conceptualisations of disability, family responses to disability and existing government services. International organisations need and want to know this information in order to make international intervention relevant.

Finally, for all the compromises involved in using a team approach to data collection, the rewards are unbeatable. It made infinitely more sense in terms of capacity-building and community-development to pass on evaluation and analysis skills rather than enter the field to gather data in haste and depart taking all the data and skills with me.

To sum up, having multiple partnerships is a messy business. Moreover, it cannot easily be squared within an emancipatory paradigm since the

³ Commonly, overt fieldwork in most developing countries requires working within an official framework (Devereux and Hodinott 1992) although some researchers avoid this (Gold 1989, Harriss 1992)

more powerful one partner, the less room there is for meaningful participation by less powerful partners. This can lead to a situation whereby ideologically significant partnerships are invisible outside of the interview bubble, research report or researcher's head. In this way, the view that research on oppression is a zero-sum game has some truth to it: research is not like the miracle of feeding five thousand with three loaves and two fishes. There is only a fixed amount of control to go round and the more partners you have at the picnic, the greater must be the host's skills in careful and ceaseless redistribution. This makes research more demanding and less emancipatory, but not less valid. As the reasons laid out above illustrate, the undeniable difficulties which arise from multiple partnerships do not confirm the universal necessity and desirability of seeking a single research relationship with disabled research subjects to whom all control is devolved. Nor is it necessarily helpful to categorise research relationships with more and less powerful partners as synonymous with siding with the oppressor or the oppressed.⁴ I do not doubt that I made dangerous liaisons but in the final analysis I believe that the risks proved worth taking.

COLONIAL MANTLES: THE SOCIAL MODEL AND CROSS-CULTURAL RESEARCH

'Disability. research, therefore, has reinforced the individual model of disability ... seeing the problems that disabled people face as being caused by their individual impairments. Hence they fail to accord with disabled people's own explanations of the problems of disability which argue that these are caused by society' (Oliver 1992: 108).

The emancipatory research paradigm is inseparable from the social (oppression) model of disablement. But what if 'disabled people's own explanations of the problems of disability' do not conform to the social model? In such circumstances, how should the researcher reconcile differences between disabled people who espouse the social model of disability and those who do not without compounding oppressive approaches to impairment on the one hand or compounding oppressive approaches to research subjects (by overriding their conceptualisations) on the other?

⁴ With regard to Mozambique, Hanlon criticises those outsiders who castigate fellow outsiders as 'supporters of the "wrong side" since governments are "never" interested in helping the poor' (Hanlon, 1992: 208)

We identified the potential for conflict between participants' analysis and the social model in 'Parasites, pawns and partners...' and questioned how far the researcher could or should act as an advocate for the social model without risking charges of proselytisation (Stone & Priestley 1996). At that time, we concluded that the researcher should defer to her or his theoretical and political standpoint:

'This need not run counter to the goals of emancipatory research, since taking the initial decision to adopt a social model of disablement as the theory which drives our research is in itself taking an important step in establishing our political commitment to the disability movement and transferring a degree of control to disabled people' (Stone & Priestley, 1996: 711).

Or more accurately, to western disabled activists. In the original version, that caveat was made in parentheses and is an indicator of my mindset at the time: I was more afraid to step out of line with the western disability movement than to be directly accountable to research participants who were still, in July 1995, a silent and faceless number. That position became untenable during nine months of work, research and life-sharing with Chinese people and in the light of my growing awareness of the criticisms levelled at western or western-trained intellectuals whose emancipatory theorising is deemed conceptual and methodological imperialism when transferred across cultures (see Humphries & Truman 1994). What 'need not run counter to the goals of emancipatory research' risks running counter to the goals of non-imperialistic research.

'Even accounts that at the time were aimed at championing the values and rights of oppressed people are now seen as fundamentally racist in their assumptions' (Wilson, 1992: 181).

Criticisms abound wherein western feminist representations of third world women assume that western feminisms can be transferred to non-western situations (Amos & Parmar 1984; Humphries & Truman 1994 inter alia). In the area of disability research, Miles has been highly critical of the transfer of western theory and praxis into developing countries. He attacks the: 'largely monocultural western or westernised disability evangelists' who have 'exported community slogans, muddled with the rhetoric of individual disability rights, to third world countries having minimal formal service structures' and he reminds potential western sociological imperialists like myself that the 'inutility to

developing countries of much western social science has been documented angrily by people who have tried it' (Miles, M., 1996: 488, 496).

Given my initial intentions (subsequently altered) to base my thesis on a critical exploration of the export of western rehabilitation concepts to China, I could hardly participate in the wholesale transfer of western social model concepts with impunity. And so I found myself steering between the Scylla of emancipatory research which would leave me open to charges of irrelevance and imperialism, and the Charybdis of jettisoning the western-evolved social model to calls of treachery by those who have fought long and hard to get due recognition for it.

In the end, I came to two conclusions. First, that it is insupportable to seek sanctuary in the social model of disability when engaging in cross-cultural research. Secondly, that the responsibility for balancing fidelity to respondent analysis with my own insights and perceptions as an outsider lies on my shoulders as the researcher. It is an unpassable buck, a burdensome obligation that might be associated with expertise or researcher independence. To understand my arrival at these conclusions (which seem to fly in the face of emancipatory research) a few more fieldwork illustrations are required.

Where the Social Model Falls Short

The social model of disability should inform and direct the formation of research agenda and specific research questions. That much is clear from Abberley's criticism of the OPCS surveys for failing to ask questions commensurate with the social model and thereby compounding the oppression of disabled people by focusing on individual impairment (Abberley, 1992). In Britain, I determined to avoid that mistake only to find that in China the vast majority of participants conceived disability exclusively as impairment, as the result of individual fate or bad luck, as a problem for themselves, for their families and communities. This conceptual barrier was made even more evident by the linguistic barrier which renders 'social model' untranslatable in its entirety (in spite of the translation of Oliver's 1983 text on social work with disabled people). Where a text in Chinese reads 'disability is a social problem', the meaning is more that disabled people constitute a problem for society, not the other way round. Certainly, there are Chinese disabled people and sociologists who identify social and physical environments as being the locus of some of the problems faced by Chinese disabled people, but

these individuals tend to be at the apex of the Chinese disability elite: those who have had a chance to participate in international events and thereby gain exposure to non-Chinese approaches (Ma 1993; Xi et al., 1993). Yet it is telling that even at this end of the disability hierarchy, language does not allow for the divisions on which the social model in the west is premised: the definitive divisions between 'impairment' and 'disability', between 'individual' and 'social'. Hardly surprising, then, that I found myself formulating research questions in Chinese which conformed to emancipatory research guidelines but made little sense to disabled participants.

If the social model proved culturally and linguistically untranslatable in research questions, it left me in a practical and ethical minefield in terms of recommendations for action. In Heping and Shanlin, I was in a position to help make a difference in disability-related provision. I had not fully appreciated the dilemmas which can ensue from a position of influence, dilemmas which again bring into question the relevance of aspects of the western-evolved social model to the daily struggles for survival experienced by many in the third world.

In Shanlin County, the needs and aspirations expressed by research participants who had disabled children centred almost exclusively on western-style medical intervention. The training of a team of rehabilitation medics, the establishment of a medical rehabilitation centre (if not hospital), the availability of remedial operations - these were on the lists of all families interviewed as well as of several disabled adults. But if, in accord with grassroots wishes, the disability programme in Shanlin took on a predominantly western medical approach, might that not compound the medicalisation of disability and encourage the rise of an oppressive rehabilitation business in China as it has elsewhere? Another example, this time from Heping County, revolves around employment. In Heping, the vast majority of disabled adults in non-agricultural work are employed by social welfare factories in which half the workforce is disabled. The possible extension of welfare factories is welcomed by all the disabled workers interviewed who greatly value the opportunities afforded them by factory work. However, the social welfare factory perpetuates the separation of disabled and non-disabled in society and economy: as long as the institution of social welfare factories persists, disabled adults are likely to be consigned to low-skilled work in marginalised factories. In light of this, should I go with or against the opinions of research participants, should I recommend or not recommend the extension of social welfare factories in Heping and beyond?

There are clear conflicts in both these examples between the researcher's allegiance to the social model and allegiance to research participants. Adopt the social model (which stands against medicalisation and against segregation) and you disempower research subjects, dismissing their opinions. Remain faithful to the analysis of your research participants (who want more medicalisation and view segregated employment better than none and, in some cases, better than non-segregated employment) and aspects of the social model must be set aside. Your choice is between oppression and oppression.

Navigating between Oppression and Oppression

'(I)t must be recognised that what we choose to observe, what we consider to be data, what we write about and how will always be affected by our personal and institutional values and the underlying assumptions absorbed through our training' (Wilson, 1992: 181).

My underlying assumptions are drawn from the social (oppression) model of disability. In view of this, I have had to make a conscious effort to examine and re-examine the nature and development of my research, to ensure that the enlightenment I have derived from the social model does not outweigh a personal and professional commitment to my research and research participants.

Research into indigenous concepts of and responses to disability is a vital but frequently neglected part of researching disability in developing countries (Murthy, 1991; Dalal, 1993; Prabhu, 1993; S. Miles, 1996; M. Miles, 1996 *inter alia*). To do this properly, the researcher should not be a slave to outsider theories or socio-political movements. Definitions need to begin with individuals, families and communities at the grassroots and not with outsiders. That said, there are times when the researcher must accept the weight of responsibility that comes with the job. Even Chambers, who has been the foremost advocate of listening to the voices of the marginalised rural poor, notes that the researcher's role is more than listening to, noting down and reporting the words of those interviewed:

'the rural poor are dispersed, isolated, uncommunicative, rarely asked their views, frequently masked by others, selectively

perceived, deferential... direct approaches distort impressions'
(Chambers 1983: 141).

The academic and the practitioner must make inferences from what is said and observed and only then report and recommend. A clear case for analytical detachment (independence? expertise?). I would add that where differences remain between researcher and research subjects (as they must where there is no shared epistemological or ontological base) then these need to be made explicit. The point is to make sense of difference not distort or disregard it.

In short, caution is vital when a researcher moves across cultures with a theoretical guidebook that was written by and for another country, another people, another set of social, cultural and economic structures. In my opinion, this means that any outside theoretical or practical approach, the social model included, must be critically explored rather than reified. I doubt that anyone would dispute this. Equally inadvisable is an unquestioning acceptance that the knowledge of research participants is beyond reworking or reinterpretation. Nothing is holy, nothing is beyond inquiry.

CONCLUSION

I have used my experiences of doing disability research in China to inform and to illustrate discussion on three issues: the status and identity of the researcher, the social relations of research production and the use of the social model of disability. It is clear that my initial fear of not being able to square my research within an emancipatory research paradigm was fully justified. The reasons for this centre on my decisions to form multiple partnerships in the field and to reject the social model as the sole referent in my research and recommendations. Thereby I forfeit claims to working to an emancipatory research paradigm which requires that disabled research subjects control research process and outcome and that agenda, analysis and action are premised exclusively on the social model of disability.

My decisions not to reverse the social relations of research production and not to defer to the social model as sole referent in theory and practice were taken with a view to increasing the relevance of my research to the lives of all those involved, whether disabled people, families with disabled children, local government cadres or international organisations. At times, these decisions have resulted in alienation for research

participants but this is balanced with the increased likelihood that my research can make a practical contribution to disability-related interventions and evaluation in China, thereby improving the material and social situations of at least some Chinese disabled children and adults.

I conclude that emancipatory research, narrowly defined (and I think it would be unwise to broaden definition) , is practicable and necessary only in certain contexts -notably where a unitary relationship can be formed with disabled research subjects and where researcher and researched share an epistemological and ontological framework grounded in the social model. Once research crosses cultures and the researcher forms multiple partnerships which reflect rather than reverse real world hierarchies, then scope for realising the ideals of emancipatory research is severely restricted.

If the emancipatory paradigm is of limited practicability, does that render it less necessary and less desirable as a framework for all disability research? Many say not: better that research which cannot conform to the paradigm be left undone. In my opinion, the emancipatory paradigm provides invaluable guidance in making the disability researcher think through all the implications of research but I disagree with those who would make the paradigm the sole measure of worthwhile disability research. The fact that I have not remained faithful to the emancipatory paradigm does not mean that I am parasite or oppressor (contrary to my initial beliefs). My work has made several significant steps towards participatory and action research in China and it should prove of practical and academic worth. For me, for the time being, that is justification enough.

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