Research with refugee communities:
Going around in circles with methodology

Pauline Guerin
Bernard Guerin
University of South Australia

Doing long-term participatory research with refugee communities has changed our views on methodology for community research. For this paper, we draw on our experiences, rather than the community psychology literature, to show ways in which community researchers could productively change or supplement their typical methods and gain flexibility. In particular, we have learned to expect new "stories" to appear at regular intervals and to not place any final value on early understandings. We have found that intensive, participatory methodologies should be used wherever and whenever possible. Finally, we have learned to not expect research 'topics' to divide neatly into compartments when in the field even if those categories are used for funding purposes. After discussing a few more specific methodological issues and problems, we discuss two examples of our research to illustrate how these issues and problems arose and were handled.

We began working with the Somali community in New Zealand in 1998 with an initial meeting between the 2nd author and a few male leaders of the Somali community about possibilities for research between the University of Waikato and the Somali community. Since that time, we conducted formal research projects with the community on health services (Guerin, Abdi, Guerin, 2005), physical activity (Guerin, Diiriye, Corrigan, & Guerin, 2003; Guerin, Elmi & Corrigan, 2007), mental health (Guerin, Elmi, Guerin, 2006; Guerin, Guerin, Diiriye & Yates, 2004; Ryan, Guerin, Guerin, & Elmi, 2005), employment (Guerin, Diiriye, & Guerin, 2004; Jelle, Guerin, & Dyer, 2007), female circumcision (Diiriye, Guerin & Guerin, 2004; Guerin & Elmi, 2004; Guerin, Allotey, Elmi & Baho, 2006), a census (Guerin & Diiriye, 2004; Guerin, Guerin & Elmi, 2006), youth (Guerin, Guerin, Abdi & Diiriye, 2003), and general resettlement (Guerin, Guerin, Diiriye, & Abdi, 2004). We volunteered extensively through running exercise groups, facilitating health sessions, teaching English classes, being a leader at a youth camp, volunteering with the Refugee and Migrant Service (RMS), and conducting orientation to New Zealand sessions. We also worked as advocates with people in the community, liaising with the New Zealand Immigration Service, Work and Income, housing, police, various health practices and hospitals, the United Nations High Commission for Refugees (UNHCR), and RMS.

Overall, we estimate that we have conducted over 900 formal interviews or survey data collection, have spent over 1000 hours facilitating various education sessions (including exercise, English, orientation, health), and spent over 500 hours in advocacy work. Although tertiary qualifications were uncommon in the Somali community when our projects began, there are now many in the community who have completed qualifications and many had been directly involved in projects with us, either as students or as employed research assistants or co-researchers. In September, 2004 we also organized the first Refugee Research Conference in Auckland, New Zealand, with 110 participants and over half of the presenters came from refugee backgrounds.

The purpose of mentioning all this as background is that from our training as social and experimental psychologists these experiences have led us to re-think our understanding of methodology. Many times we felt like our research was going around in circles, but more often, we found that the research was on a spiral, still going around in circles, but progressing for the better. In this paper, we draw on this experience to focus some of our methodological issues and solutions. Rather than relate this back to the extant literature extensively, we focus on

discussing our experiences listed above for others to compare to their own research experiences.

We first discuss briefly three imperative methodological changes for research with communities: what we came to understand as 'peeling the layers of an onion'; the importance of participation and the complexities associated with that; and the important issue of not 'compartmentalizing' research topics. We then discuss some more specific issues and problems. Finally, we work through two examples of our research showing how these issues and problems arose and were (hopefully) solved.

Three Methodological Imperatives for Studying a Somali Community

Making Multiple Cuts in Research: Peeling the Layers of an Onion

Our first imperative from experience working and researching with a Somali community is that finding out about the community, and how it functions as a whole, comes in layers. About every one or two years we came to realize that we had made another 'cut' into what we knew. In between, we had the illusion that we finally found out the 'truth' about particular issues or topics pertinent to the community. Over time we learned to live with those illusions (actually, stories) knowing that they would soon change once again. What this means, then, is that methodology should not be thought of as a procedure that will produce an immediate definitive answer to a research question.

While this might sound obvious to some, especially social anthropologists (Koser, Werner & Ang, 2004), it is not common for some other social scientists, particularly for us in psychology when we started. Even those using 'qualitative' methods tend to use one- or two-hour interviews and write that up as a thematic whole, rather than peeling away over time (see our next imperative) more and more of the context and dynamics of those themes. To help see this point, we will outline four ideas that have helped shaped our thinking along these lines.

First, we found it helpful to get away from the idea that there is an 'authentic' voice or story that can eventually be captured for either individuals or communities (cf. Guerin, 2005; Kusow, 2003). For example, Miller (2004) discusses how first responses ('frontstage'

responses) by participants are not always accurate, but then writes about the more 'authentic' backstage responses. Our experience is that there are many backstages that are not necessarily contradictory but can differ markedly and that backstage can become frontstage, with different scenery, and over again.

Second, one has to get away from the idea that what is found in early 'cuts' is 'in-authentic', wrong, or inaccurate, and therefore should be avoided or thrown out. We found that the early material is useful 'in general', but later cuts show many exceptions to the general rule and also show how the initial impression arose in the first place. This point also reflects the changing nature of communities and people over time. We would not want to discourage new researchers to this area by suggesting that the first five years of research will be worthless—it is certainly not. But we would like to discourage researchers— including ourselves—from making bold early assertions that can mislead new people in the area or impressions about people in the community. This in turn raises another important question we will address, of how to write diversity into research reports without losing the audience in a wealth of detail or, at the other extreme, making stereotyped and simplistic statements. But we argue against the idea that because the research stories or cuts are constructed that they are useless.

Our first example relates to parenting and involvement in schools for Somali. We first came to understand the 'problem' through teachers at schools concerned that Somali parents were not involved in their children's schooling. In our initial attempts to understand why, we learned that Somali parents might shy away from participating in schools because, in Somalia, parents only got involved in schooling when something was wrong and their child was in trouble (e.g., Abdi, 2003; Guerin et al., 2004). Over time, however, we found that many Somali parents were 'involved' in schooling in New Zealand as well as previously in Somalia or in other countries, but this involvement took different forms, such as arranging Koran lessons. Also, we found that 'involvement', in the sense that teachers were seeking (such as attending parent-teacher interviews, attending school events and going on camps), involved many

barriers, such as language, religious issues, and multiple family and resettlement demands (Iszatt & Price, 1995). This did not mean that the earlier idea was not true, but there was a greater diversity in parental involvement in school that was not initially evident.

The complexity of these issues was illustrated by a woman in a focus group discussing resettlement issues. She had six children in school and, partly from not speaking English well, and partly from not understanding the New Zealand education system, felt isolated from the education her children were receiving and was unable to engage with either her children or the school to get an understanding of what they were learning and how they were learning it. Her need to trust 'the system', because of the various limitations, was a scary and disempowering reality for her, limiting her ability to get involved in schooling at another level. While an English-speaking parent might read homework and talk to teachers before or after school and other parents, this mother had limited English, a small baby, and various resettlement concerns. However, she was involved in her children's schooling in that, when possible, she encouraged her children to teach her English and lessons that they had learned at school.

Third, the further 'cuts' were not always just adding more diversity and complexities to a generally true assertion, although this was sometimes the case, but they also involved changing the whole way of conceptualizing the question and therefore the 'generally true' answer. In the case just given above, our next cut led us to change the many ideas we had of schooling, of children going to a fixed school, of parents being the sole caregivers of children, and of parents being concerned about what others thought about their children being 'bad'. We also changed our conceptions of 'schools' or education to integrate ideas of private Koran lessons as another form of schooling, and gained new ideas of why parents might not get involved in schools. We not only discovered greater complexity to the issues, but also the framing of our questions changed.

Finally, a point we came to understand only after many years was that first 'cuts' can be insidious precisely because they usually make very good stories (hence our earlier word

'illusions'). To use the above example, a bold statement that "Somali parents avoid contact with schools because in Somalia that was indicative of their child having problems", makes a short, quite reasonable, interesting, probably novel, and highly repeatable story—all great rhetorical or conversational features (Guerin, 2004, p. 222). If initial cuts were vague and not easily comprehended there would be less of a problem, because others would almost certainly try to follow-up for more details and make sense of it all. However, most 'first cut' reports seem final and have that attraction of a good story that tells researchers and professionals they can stop there and not pursue the details and contexts further. Even communities can promulgate simple, attractive (or not, if that is the point) stories about themselves and can facilitate access to resources.

Another example we found was the connection between trauma, post-traumatic stress disorder (PTSD), and refugees. Most refugees have been through trauma (which is true) and most refugees in new countries are struggling (also generally true). Connecting these two makes a nice narrative package in which all sorts of issues are attributed to the trauma. While there are certainly real connections for many refugees between current problems and past traumas, we believe that this has been overemphasized or taken for granted and so other causes and contexts for struggling are overlooked and not researched. We are not saying that refugees have not been through traumatic experiences or that none of them 'have' PTSD, but we are questioning the easy story that all their woes are due to (i.e., caused by) the trauma they have experienced. Like others (Sonn & Fisher, 1998), we have looked for features in social contexts that might show resilience (Bracken & Petty, 1998; Guerin, 2001b; Ryan, Guerin, Guerin, & Elmi, 2005). For example, we have found that an important factor in the community's resilience was that everyone in the Somali community has experienced similar traumatic events in their lives. We believe there would be worse traumatic effects if only a small group had experienced them and the others in the community could not fully grasp these experiences.

What this first point means in practice for research is that we have become critical of our

own early work and summaries, but without discounting them entirely and 'throwing babies out with bathwater'. Further, research that has approached a community once, conducted 10 one-hour interviews and gone away to write up the themes and results, is limited. Once again, we do not completely discount such reports, but we know that if the same researchers went back and did even one extra hour of research interviews per participant, they would likely come away with different impressions and more interesting complexities. These limitations are often not identified and research findings can be overly ambitious and de-contextualised because of how the findings were obtained.

The Use of Participatory Methods

Our second methodological imperative from research with a Somali community is that intensive and participatory methods are necessary (e.g., see Maton, 1993) and that questionnaire and survey methods will not provide quality information, even on what may seem to be straightforward issues. One problem is that data from non-participatory methods can usually make a good story from whatever results there are, as we saw above for any first cut, even if untrue. But a second problem is that you do not have any way of gauging whether or not your results are close to or far from what is actually going on. So once again, the results (or the story) can appear to be definitive and mislead those not versed in being critical of methodology—especially policy makers.

One of our first projects (e.g., Guerin, Abdi, & Guerin, 2003) was 'collaborative' in that members of the community presented the initial idea for the project and were also employed as researchers collecting and interpreting data and writing reports and publications. However, that project was a survey using a questionnaire. Below is an example from that study (Guerin, Abdi & Guerin, 2003) on experiences of the health system, through a questionnaire-interview carried out by a Somali co-researcher:

Perceptions of Good and Poor Health
Most of the participants rated themselves as in good health, with an average of 5.2 on the 7-point scale (slightly above the label "good"). Correspondingly, most were not concerned about their health, with an average of 2.5 on the scale of how concerned they were about their health. Both these ratings correlated with English proficiency, with better health and less concern about health both associated with better English. (p. 29)

This makes a nice and typical summary of research results. A policy maker may have viewed the above results as indicating a lack of health problems within the community. Since that project, we have spent many hours talking about health informally in a wide variety of settings with many of the same people who would have been interviewed in that study and contributed to the results. We talked during exercise classes held for Somali women, we ran health information sessions, we accompanied many to health and medical appointments, and we talked extensively with health professionals with lots of experience with the major health problems of this group (Guerin, Diiriye, Corrigan, & Guerin, 2003).

Doing this provided a very different picture of the health of the Somali community, their perceived health, and their concerns about health. We found that there was a growing concern with diabetes in the community and a lack of knowledge about some health issues such as cancer (that was perceived to be a 'western' disease and believed to not affect Somali). We also learned that access to health care before coming to New Zealand was poor compared to the relatively easy access in New Zealand (e.g., while the density of physicians in New Zealand is 2.37 per 1000, it is only 0.04 in Somalia; WHO, 2006). This led to excessive use of general practitioners by some, which could very well lead to a perceived sense of good health. What we originally wrote was not untrue. The above quoted summary likely reflects the improved health services in New Zealand in contrast to those available prior to coming to New Zealand. But learning informally by participating in the community brought us a much more accurate (both for us and the Somali community) picture of these topics and a much better basis for intervention and policy in this area.

Overall, the second clear methodological imperative in this sort of research is that

participatory or intensive methods are necessary. This may seem easy to those not accustomed to such methods—you just go into the community and start chatting with anyone you meet—but the methods require extensive preparatory work gaining the trust, collaboration and permission from communities to work alongside them and can be time-consuming and costly in time and money. However, the point for us is not that researchers cannot afford participatory methodologies, but that we cannot afford to do without participatory methodologies.

Non-Compartmentalization of Research Topics

The final methodological imperative is that topics about refugee communities (and amongst many other communities) cannot be treated as separate or be 'compartmentalized' into distinct topics. We originally planned a series of separate studies with the Somali community on topics they reported to be of importance—health, mental health, religious beliefs, youth, discrimination, etc. We soon found that the topics all impinged on each other and were not distinct separate topics.

As an example, when we set up exercise classes after a group of Somali women expressed an interest, we found that many community and religious issues became a focus in a way we had not anticipated. For example, a rumour circulated in the community that an imam (i.e., religious leader) had said that the women should not do exercise classes because they should stay at home. This was promulgated by some of the men in the community and some women then felt they should not attend the classes (Guerin, Diiriye, Corrigan & Guerin, 2003). Resolving the issue required an understanding, not just of the benefits of exercise for women, but of the religious implications and communicating with the women about these issues. We also found it was necessary to discuss concerns about appropriate music (or no music at all sometimes), the kinds of exercise that we did, and we discussed and incorporated cultural dance into the sessions. Women later requested sports and walking groups and, surprisingly, requested the sessions be run after dark when people could not watch them and stare or make rude comments (Guerin, Diiriye, Corrigan & Guerin, 2003). This was an important consideration as another exercise group was stopped to change facilities,

partly because of the abuse the women encountered when going to the first facility. Discrimination and religion impacted on health issues, and we needed to consider these alongside the development and planning of exercise classes.

We are not suggesting that all research should be approached as a big amorphous whole, but that with groups who live in close communities it is unrealistic to question people and learn about one topic, without needing to know about other topics. We therefore only nominally divided our research into topics such as a census of Somali in Hamilton, households and mobility, employment, dealing with bureaucracies, health, Somali women's fitness and exercise, female genital cutting, mental health, children and schooling, discrimination and westernization. The broader argument, not given here in full, is that it is the nature of typical western social relationships that make the compartmentalization possible, but this does not apply to other communities that have different sorts of social relationships (Guerin, 2004)

Despite not being separable, the individual topics were useful for organizing themes and analysis, and also for funding purposes. Funding is more likely to be received for a series of projects on mental health that includes looking at community dynamics, health, discrimination and religion, than for a big amorphous project on 'studying everything about Somali communities'. There is a practical use to dividing up the material, but for analysis we found it was important to look at the whole context rather than discipline-defined topics.

The final point to make about lack of nicely compartmentalized topics for research is that this impacts on social policy and other interventions (Guerin, 2005). Social policy is often divided up into the same or similar topics as we have mentioned, such as housing or immigration policy, but in reality social policy in one area impacts on many others, and the interventions in one area of life impact on other areas. For example, changes in housing and government housing policy affected other areas of our research including ones that we considered remote. If our research methodologies can better reflect the interconnectedness of topics then we will be in a better position to write good policy

that is sensitive to all the trickle-down consequences even in seemingly unrelated areas. Governments are also attempting to tackle this same issue with "whole of government", cross-government and cross-departmental committees approaches (e.g., COAG, Council of Australian Governments, in Australia).

Other Methodological Issues in Studying Somali in Diaspora

Communities are Changing

Another methodological consideration we found is that communities are highly dynamic and researchers need to be careful about what is described as 'traditional'. It is easy to 'explain' results as deriving from essentialistic, traditional characteristics when there are many other factors in the social, economic and historical contexts that have been overlooked because of methodological assumptions about what is 'traditional' or not for those communities.

For example, we not only found that many lay people but also many professionals explained high mobility in Somali communities as deriving from their 'nomadic' nature. Somali themselves have even explained high mobility to us this way. However, there are many other factors in the situations for Somali that account for high mobility, and the same factors can be seen in other populations in similar situations who do not have a national stereotype as nomadic pastoralists.

Methodologies, therefore, need to be geared towards finding changes and flexibility rather than just a static, snapshot picture of a community. In our studies, under a general topic of housing mobility, we employed a variety of methods—surveys, case studies, repeated interviews, key informants—to help us understand why some Somali in New Zealand communities frequently moved houses within a city or moved to another city or country and then back again. By careful searching we found a plethora of reasons for moving frequently, none of which related to being 'nomadic'.

Suspicion towards Authorities (including Researchers)

Like others (Chile, Dunstan, & Dibley, 2003; Jacobsen & Landau, 2003; North, 1995; Pernice, 1994; Reichelt & Sveaass, 1994) we encountered suspicion about research and fear of revealing information. There are numerous reasons for suspicion of research that differs between communities. For example, research participants are usually in a position of lesser power (e.g., see Ramachandran, 2004) and may try to please the interviewer or researcher and give answers that do not reveal but provide what the researcher 'wants' to hear. Additionally, history of bad experience with researchers or a lack of tangible outcomes for research participation can influence hesitation to participate in research.

However, we found that when participants were suspicious of research, this related to concerns that providing information might impact on them. Participants could be particularly hesitant to answer questions when the researcher was from an immigration or other government department or assumed to be part of a government department. For some, there was no distinction between government and university, but, rather, all were seen as 'people with power and access to resources'. Although being independent researchers from a university helped, we also learned that refugees had to provide stories repeatedly about their activities all along the route from exiting a civil war to long after arriving in a new country. This often involved questioning from police and military personnel from both friendly and unfriendly countries that could have dire consequences. Immigration questioning is usually done in such as way as to 'catch people' telling a 'lie' or hiding something, thereby aggravating the whole process and creating, sometimes unnecessary, tensions and anxieties about interviews and questioning. Even when answering questions from immigration services, people are careful and suspicious about what is said, not because they are dishonest or hiding something (McKelvey, 1994), but because they know a misunderstood statement can lead to grave consequences, and this is true for anyone negotiating the immigration system.

Finally, a more simple reason for being suspicious and unforthcoming with information is because of limitations in ability with the host country language. Coupled with the problems of accidentally giving detrimental information, people will often err on the side of caution in revealing anything in case lack of language ability also leads them inadvertently astray.

A clear example of this was when we conducted a census of Somali. Some expressed hesitation in revealing how many people lived in a household or their ages in case information was passed along to housing or income benefit agencies. Despite following standard community-based research approaches and ethics protocols (i.e., consulting with leaders, holding community meetings, employing researchers in the community to collect data, ensuring anonymity and confidentiality) a few families agreed to provide only limited information. The project was conducted not long after the September 11th attacks and some members of the community believed that one of the authors, a US citizen, was an undercover CIA agent documenting Muslim residence.

This brings us back to two of our earlier points. First, that participative and longer-term methods are necessary, since these are the only way to overcome some problems. Building trust and exploring topics and issues from many sides are the ways to go. Second, the well-rehearsed stories are very seductive in their rhetoric and are believable and fit the facts. If the stories had glaring contradictions then researchers would seek out more information, but the stories are often so well-rehearsed that one can easily accept them and not pursue the contextual details (which needs participatory methods).

Political and Social Issues can Colour the Research

Suspicion is just one aspect of having a refugee background that can colour responses to researchers. There are political agendas that can also form the basis of responding to research. The problem is not that methodologies should prevent any such biases from creeping into research questioning, the problem is that there will always be biases and the methods need to inform us when there are biases. Until a community is known well, a researcher will not know if responses are influenced by social and political concerns. So once again, knowing communities in an intensive and participatory way must come first. This will not stop biases appearing in 'data' but will allow recognition or prediction of when they are likely to occur.

As an example, 'clan' divisions among Somali was a contentious issue with contradictory views. Some Somali were adamant that, although clan divisions were responsible for conflicts in Somalia, the refugee experience and resettlement in a new country ceased those clan divisions in New Zealand and that all Somali were Somali, regardless of clan. On the other hand, there were indications that clan divisions were still used to differentiate people and to gain access to resources. It was therefore possible that results in our research might have been influenced by clan membership. The influences of clans were not all bad, however. For example, in countering the reputation that clans divided the community, some community members made extra effort to ensure that people from all clan groups were included in anything that involved the Somali community, sometimes at the expense of their own clan, as an attempt to demonstrate lack of clan preferences. In one case, a new family were arriving who were from a minority clan with only one family from that clan living locally. To help the family feel welcomed, women from a variety of clans, including the minority clan, pulled together to set up house for the new family. New curtains were bought and the women stayed up late into the night cleaning and decorating the house and ensuring it was suitable for the new family.

After more years of participating in this community we now have a better understanding of the situations in which clan divisions count and those situations in which they do not. We cannot stop biases based on clan membership about what one person says of another but we now can anticipate and recognize when it occurs in many instances.

Methods are Social Relationships

We can summarise the main points of the paper by reframing methodology itself as a social relationship with all the properties implied (Guerin, 2001a, 2001b, 2004). All methodologies are a way of bridging or negotiating a social relationship so as to obtain responses. For example, useful information can be obtained through door-to-door surveys or questionnaires with populations in which 'stranger' or 'contractual' relationship are ubiquitous, such as most western societies (Guerin, 2004, 2005). But in social situations in which there are complexities, multiple strategies, or strong consequences, such relationships do not suffice. This is characteristic of the communities

with strong kin-based ties, and with these close communities and kinship groups, the building of 'rapport' seen with typically western populations transforms into community collaboration and participation (Austin, 2003; Barnes, 2000; Baum, Sanderson & Jolley, 1997; Davis & Whittington, 1998; Dunne, 2000; Holmes, Stewart, Garrow, Anderson & Thorpe, 2002; Kowalsky, Verhoef, Thurston, & Rutherford, 1996; Potvin, Cargo, McComber, Delormier, & Macaulay, 2003; Snell-Johns, Imm, Wandersman & Claypoole, 2003). The 'data' to be obtained does not 'reside' in one individual but is spread across the community, so the methods must change.

Instead of thinking in terms of the named categories of methods, as are usually outlined in methodology textbooks, surveys, interviews, control groups, etc., we think in terms of the social relationships involved in research between researchers and participants, and what methods are necessary to answer the research questions. To give some examples, running control and experimental groups who are given the same measurements requires a certain type of population with certain forms of control (social relationships). This is perfectly fine for some topics and some populations but for research questions with refugee populations we have found them unrealistic as it is usually not possible to get the necessary social influence to run control groups. Second, we can certainly conduct interviews but the short-term social relationships usually developed with interview methodologies, while fine for western populations who have a high percentage of stranger relationships in their lives (Guerin, 2004, 2005), are usually not enough to get the answers and complexities needed in refugee research. It is the limitations in forming quick social relationships that discourage us from using typical interview methodologies in our research with refugee or other kin-based groups. Repeated interviews can overcome some of the problems but participatory methods we find are usually necessary.

The Effects of Stress and Community Disruption on Methodologies

Even apart from the trauma that is part of the life experience of most refugees; many refugees are under stress from any number of issues arising from moving to a new and unfamiliar country, settling without many resources including language, and from having to face a usually semi-hostile and discriminatory set of strangers. Communities are usually disrupted from what they might have been previously, and do not quite work in the same way as they might have 'traditionally'. This means extra stress from the people in the community trying to build new processes, practices and procedures into their lives.

While these factors are topics in themselves for research, and suitable topics for developing interventions, they also impact on research methodologies. In researching with Somali refugees we are researching with a group of people under some considerable pressure. This means care must be taken in asking for time or other resources from the communities, in how topics are broached, in interpreting what is found, and especially in researching questions that will be of relevance, usefulness and interest to the community. If the people in communities were luxuriating in their new homes they might be open to very theoretical or abstract inquiries, but refugees are people under pressure and are focused on solving problems rather than finding out answers to theoretical questions.

For these reasons, we focus the methodologies as well as the topics on interventions and useful outputs (Guerin, 2005). One does not have to adopt a label of 'action researcher' to allow interventions to develop and work alongside the research methodologies. They can also be subsumed under the 'rapport building' part of methodology and be widened to include many other forms of advocacy (Guerin, 2005).

Distorting Effects of Writing

While not strictly methodological in a traditional sense, the writing-up of research is an essential part of the research method that has also raised issues for us. First, writing usually produces homogeneity or sharpening of results that can give a false impression of unity, single causes, simple contexts, and uniformity. Writing for journals, in particular, usually shapes a coherent and homogeneous story about a topic. Within editorial guidelines and page limits it is difficult to represent the diversity and uncertainties of researching with refugee communities (or any kin-based community).

We have already argued for methodologies (in the traditional sense) that allow longer-term understanding of complexities and seeming contradictions in the wealth of lived experience in communities. A necessary methodological extension to this that we are now exploring is how to present material without losing that complexity, and without being verbally inaccessible, even if it does not make a traditionally 'good story'. One project of ours, for example, is writing about the different aspects of Somali refugee life but layering the complexities into the different cuts—as described earlier in this paper. In this way, we hope to find new ways to 'write diversity' without producing an overly abstract or theoretical end-product, and without producing a conglomerate of contradictory statements that confuse readers more than help them understand.

Two Examples

A Census of the Somali Community

One might argue that there is a place for quantitative methodology in research with a community such as the Somali community. Indeed, medical research such as a study of dental age and chronological age in Somali and Caucasian children (Davidson & Rodd, 2001) is a good example of a straightforward quantitative project. Another good example is that of a study examining a scalp fungus among African (primarily Somali) children and treatments (Lamb & Rademaker, 2001). The results of radiographs of teeth and laboratory tests of fungi are not likely to create much dispute in their interpretation. Social science inquiries, on the other hand, are often fraught with differences in interpretation and definition, even in the best of situations.

Conducting a 'head count' or a simple census of the Somali population in New Zealand at first glance appeared a straightforward project to conduct, similar to the above medical projects. Political and trust issues aside, consider the question, "How old are you?" But, it is not uncommon for some Somali to not know their exact date of birth or their age, so such a question does not produce undeniable results. Similarly, take the question, "How many people live in this house?" Again, from a western-trained, social science perspective there should be no qualms about asking this question or getting

straightforward responses. However, in our census project, we found, through our community involvement, that these were very much problematic questions.

As an example, during the course of the project, one Somali family living close to the researchers had some family members move in and out at least three times between Australia and New Zealand, and the people moving in and out were different each time, making a census very difficult (cf. Morphy, 2004; Warchivker, Tjapanati & Wakerman, 2000). Did these other members live there or not? Where should they be counted? We opted to count them all as living in Hamilton because their presence, when they were present, and even their absence, impacted on the family and wider community. Other households had one or two people who lived with them sometimes, but not all the time, due to seasonal employment, university studies, or family issues. There were also a few families who, for a variety of reasons, reported less or more people in the household than were actually there. With a community of approximately 700 people at the time, it was not too difficult to check the accuracy of the counting simply by brainstorming between a few of the researchers and recalling friends, acquaintances and family and ensuring that they had been counted. We also confirmed these numbers with estimates from other agencies and groups to further validate the numbers. Overall, this project showed us that, even with seemingly straightforward questions, we needed to exercise caution in interpretation and draw on multiple ways to think about the collection and results.

The Mental Health of Somali Women

Another on-going project was to understand the 'mental health' of Somali and to find ways to incorporate traditional or religious treatments into western approaches (Guerin, Guerin, Diiriye & Yates, 2004; Guerin, Elmi & Guerin, 2006; Ryan, Guerin, Guerin, & Elmi, 2005). We do not wish to go over all the material here but point out the links to all the methodological issues raised in this paper.

First, this series of projects, more than any other, highlighted to us the point about compartmentalization of topics. Dealing with what are called 'mental health' issues closely involved every other topic, even housing,

discrimination and mobility. We also found that mental health issues were linked directly both to religious practices and beliefs and also to the everyday stresses of life as a refugee.

To illustrate peeling away the layers or cuts, and using intensive methods of research, in the early days of our research we asked people direct questions about mental health and received some good ideas and stories that made sense, but we gradually realized that there was much more to it that we were yet to understand. A breakthrough came one day when with a Somali friend who often came with us for family outings. We were talking in the car about her Mum when she happened to say that her Mum had been getting angry at them lately and said she had spirits in her. The descriptions of her mother's behaviour would, in a western assessment, suggest schizophrenia. We asked what she did to make things better and were told that she had Koran readings.

We followed this up and found that Koran readings were a regular part of anyone's 'mental health', as well as physical and spiritual health, and were almost always the 'first stop' when depressed or 'not feeling yourself'. We also learned more about 'spirit' possession' which is much more ordinary and everyday than the English translation suggests. But this peeling away of new layers came about through participatory methods and had been missed in previous 'interview' research we conducted. As mentioned above, this did not mean that anything we reported earlier was completely wrong, just limited and possibly over-simplified. We did not know to ask about Koran readings and interviewees did not mention them. But in terms of mental health and useful interventions, learning about Koran readings was very important.

Looking at the other methodological points made in this paper and how they relate to our metal health research, there is still much diversity within the community on this topic and it is constantly changing. For example, while most of the women did not find western therapies and approaches helpful, there were Somali women who had been through western mental health services (including Cognitive Behavioural treatments) and were satisfied and felt it helped them. Also, while many women believed in

spirit possession, others did not. Consideration of diversity in community research is certainly not new (e.g., O'Donnell, 2006; Trickett, 1996; Watts, 1992), but incorporating that understanding in practice and in interventions or policy is more challenging.

Conclusion

In conclusion, there are many methodological issues when working with refugee communities but we do not see an answer being more and more refining of methodologies to get rid of all the "biases and issues". The issues are part and parcel of any social relationships and we have argued briefly that methodologies are social relationships. It is like saying that families sometimes have conflicts and we must aim to build families with no conflicts. But conflicts have good sides and we are unlikely to be rid of them unless everyone gets everything they want or nothing.

Overall, we have found that balancing two facets of communities is important in understanding and researching with Somali. First, we need to show the **unique characteristics** of these communities—the specific practices that make them different and lead to some problems in resettlement. Somali have specific and sometimes exotic-looking ways that differ hugely from those of most other New Zealanders, especially on religion, social practices, language, and looks. This is similar to working with the homeless and first focusing on the major differences to other Australians. But second, we also need to show the **ordinariness** of the people who are, after all, just people like we are. Somali are just ordinary people who share most things with everyone else—love, conflicts, social relationships, closeness of families, humour, beliefs, opinions, emotions, moods, health and illness, need for employment, desire for education, gossip, parties, and spiritualities.

The trick is to find the methodologies—that is, the social relationships—that will allow both these to be discovered and that can maintain a good balance between the different and the ordinary. Our experience is reflected in this paper more than the rational arguments of ourselves and others through the research theories, and our experience promotes participatory methodologies for this balance, or

at least spending as much time as possible with participants of research. It also encourages us not to be so enamoured of words that we cannot be critical of the stories constructed by both our participants and ourselves around the subject matter, and to being open to regular new stories (or cuts) in what we know. Finally, our experience also tells us not to be so enamoured by words that we divide our subject matter up in ways that prevent us from seeing connections because our theories (words) do not point us in that direction.

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Acknowledgements

Address correspondence to
Bernard Guerin
School of Psychology
University of South Australia
North Terrace, Adelaide 5001
South Australia
Email: Bernard.guerin@unisa.edu.au