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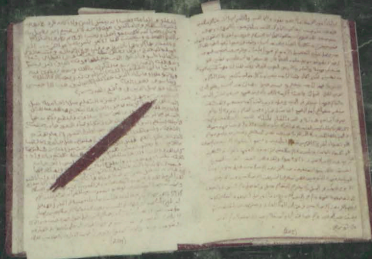
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Between Field and Text: Emerging Voices in Egyptian Social Science

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WHY IS ANTHROPOLOGY SO HARD IN EGYPT?

HANIA SHOLKAMY

Anthropology has been 'born again' in Egypt. National policy makers and international donors working in Egypt (and perhaps elsewhere) have an increasing awareness of the contribution that anthropology can make to social research and human understanding. In fields as diverse as health sciences and medicine, demography, and other population sciences, ecological and environmental research and advocacy, economics, rural development, agrarian reform, urban planning, and governance, anthropologists who had barely been humored previously are now sought and heard. But on closer inspection one finds that anthropology has been born again as a collection of qualitative methods. One needs only to contemplate 'the focus group' and its ardent admirers to realize that it is qualitative methods separated from theory, methodology, and intellectual context, content, or history that are gaining wide acceptance. Quasi-anthropological techniques are in demand, not anthropology with its precepts and concepts, or with the holism that many tout as the hallmark of the discipline (Greenhalgh 1995).

In this paper I would like to discuss the political difficulties of writing anthropology and ethnography in Egypt, which persist despite the newfound fame of some anthropological methods. I argue here that these difficulties are about readership and about the consumption, not just the production, of texts. These are the same kinds of concerns that effected a paradigmatic shift in social theory during the latter part of this century (Geertz 1974, 1983; Clifford and Marcus 1986). The positivist paradigm, which dominated research until the 1950s, offered 'valid,' objective interpretations of social reality and hinged on the existence of social facts (Durkheim 1933). This paradigm has now been contested and supplanted (Denzin and Lincoln 1994). We are now, according to Richardson, in the age of a theoretical sensibility in which there: "...is doubt that any discourse has a privileged place, [or] any method or theory a universal and general claim to authoritative knowledge" (Richardson in Denzin and Lincoln 1994: 2). This sensibility is generally

referred to as 'post-modernism.' The anxiety it expresses and the alternatives it offers center on power, representations, and interpretation. The questions it raises resonate with a central concern: What do things mean to whom? (Clifford and Marcus 1986; Rabinow 1991; Rosaldo 1989; Said 1991, 1989).

Missing from the 'universal' anxiety over power and representation are considerations of the anthropologist in her/his national setting when it is a non-Western one. Also missing is the problematization of audience and readership for the non-Western national working at home. This paper points to the consequences of such collegiate exclusion by examining the structures and considerations marking the borders of anthropological research written by locals working locally.

An implicit assumption in this recent post-modernist contemplation is that all researchers are writing for the same kind of audience. But how different is the problematic of power and representation in the absence of a Western readership and in an often less than sympathetic, sometimes oppressive national context? As an Egyptian anthropologist I have experienced the practical as well as the theoretical consequences of such a situation. I have been commended for the findings reached by the insightful methods I am supposed to have learned in my studies abroad (and am often asked about the blasted focus group about which I never heard a word in the department of anthropology in which I trained). However, I have also been witness to some significant, if anecdotal, reactions to the meaning, representations, discourses, and constructions of this discipline.

So, how does the possibility of an other-than-academic/Western audience condition the diversity of discourses that could emanate from ethnographic and anthropological inquiries in Egypt? This possibility holds a double challenge. It challenges current theories in anthropology on writing, representation, and power. It also challenges the acceptance of anthropology and its qualitative methods by policy makers, development researchers, and other players in Egypt.

To make the point here I discuss the experiences of qualitative researchers who seek an audience and presence in places other than the corridors of Western academia. Many of us would like to engage in a dialogue with peers, executives, projects, and publics in our local, national, or regional contexts. In the absence of the traditions of reading and writing

established in Western intellectual, political, and academic circles, from where can we derive security, support, and where can we engage in serious criticism?

Before proceeding, I would like to make a cautionary note. The following discussion is personal and political. The paper is neither based on an exhaustive study of social science production in Egypt nor on a survey of readers and readership. The discussion airs some of my concerns as an Egyptian anthropologist and shares my stories and those of others. The issues raised may concern indigenous and non-indigenous researchers who seek to be recognized, challenged, and/or stimulated by non-Western or non-Western trained academic audiences.

The paper will first detail the problems faced by some anthropologists/ethnographers in Egypt. I then consider briefly the 'international' debate over representation and meaning in anthropology to highlight the absence of issues of readership from this debate. This is followed by a few short examples of misunderstandings around qualitatively-informed representations to illustrate the problems of the consumption of qualitative knowledge. Finally I offer a conclusion of possible alternatives in the spirit of constructive optimism.

The Egyptian Setting

Focus groups, observation, and in-depth interviews have become the tools of choice for many research projects and their evaluation in Egypt. A whole library of manuals have come into being in which authors flaunt the 'quick and easy' way to use qualitative and quasi-anthropological methods for the assessment of almost anything (see Sholkamy 1989). While qualitative data collection is recognized as a relatively cheap and efficient way of gaining insight into human behavior, the concepts and theories from which these methods derive are still underemployed and viewed with some suspicion. The battle is over representation and meaning. Qualitative data, by its very nature, must reflect the eye of the viewer, which in turn shapes what is viewed. How can anthropological methods be so successful when the subjectivity of anthropology is still suspect, threatening, and regarded as not credible?

Denzin and Lincoln define qualitative research as being an approach that:

...is multi-method in focus, involving an interpretive, naturalistic approach to its subject matter...(whereby)...researchers study things in their natural settings, attempting to make sense of, or interpret, phenomena in terms of the meanings people bring to them (1994: 2).

As such, qualitative research is problem-oriented and context-specific. It is distinguished from other approaches to social, and other forms of, discovery not by its methods or techniques but by its focus on the meaning of things in their natural context.

A caricature of the common misunderstandings prevalent among circles of readerships in Egypt would be as follows. Many appreciate the verbatim quotes that interviews and observations supply, with their 'straight from the horse's mouth' color and freshness. Focus groups are favorites because they can cram many subjects into busy schedules and because they are supposed to capture conflict, decisionmaking processes, and the complexity of human interaction. But venturing into the naturalistic context is unpopular, as are questions of multiplicity and relativity of truth and meaning. These tools are used in a positivistic framework and are made to render the same kinds of enduring facts and information that numbers are made to do.

Leaving aside the misuse of methods and looking at the difficulties of ethnographic and anthropological research, we can easily trace three reasons why it is held suspect. The first is the reign of modernist 'scientific' thinking that finds strength and meaning in lots of numbers. This ideology of undisputed facts and streams of numbers is still prevalent in many academic and public discourses. This is in spite of, or perhaps because of, the way qualitative methods have inched their way to a degree of recognition as scientific, perhaps credible, and often useful.

The second reason for suspicion is a case of misconstrued intentions that politicize qualitative data collection and its use. Here the sensitivity lies in the details and the voices that are the flesh and blood of qualitative methods. Descriptions of poverty or of divergence from the norm are often seen as acts of denuding and exposition, challenges to structures of authority, such as the government or the family, or to idealized norms and customs. Moreover the words of the poor, the dispossessed, or the suffering are too much, too vulgar, too disturbing. But they are the research subjects with whom many of us work. After all, an interview with an urban slum

dweller is research; with a minister or another official it is a proclamation to be read in the daily papers.

While anthropologists have always been interested in both rich and poor and have studied the mundane as well as the profound, and the common along with the rare, they have always done so from critical perspectives that retain the potential to unsettle and question. Hence even the few studies that exist of the not-so-poor are still studies that question and, for some, are ones that expose.

The preference for 'scientific' research methods and the distaste for subjectivity and details are part of the third major problem, that of readership. Public consumption of social science research is very low for several reasons. The first is that reading is not a popular pastime among even the literate of the still largely illiterate public in Egypt. Another obstacle is that of the Arabic language and social science. Perhaps because of the practice of importing social science concepts or the lack of effort invested in using concepts in a reader-friendly manner, social science, anthropology included, makes for very unattractive reading in Arabic.

As long as readership is limited and specialized, and texts about daily life are distant, the current situation, in which anthropologists do not write in Arabic and when they do (through authorship or translation) they are judged by a powerful few, will continue. By 'powerful few' I mean people who have access to and or control of various public fora and media. I mean academics, politicians, journalists, and policy makers; people who can dismiss work as being subversive, slanderous, Orientalist, biased, or dangerous in some other way. This proxy readership is perhaps the most obstructive element to the publication of ethnographies in Egypt and perhaps elsewhere. I have in mind all the research that is written up in Egypt in English but that goes untranslated because it is too 'sensitive' or because it is liable to be 'misunderstood.' Some of the examples cited in this paper will illustrate the perils of powerful and limited readership. Here I should make a belated clarification. Anthropologists write for anthropologists, architects for architects, and each generally unto their own. This exclusivity, however, is compromised by interdisciplinary endeavors and by the collaboration of researchers in some ventures of mass concern or appeal such as public health campaigns and projects, urban and rural development and 'renewal,' and other such politicized projects. Because these initiatives touch

and change the daily lives of large groups of people, their documentation becomes a subject of public concern. These situations raise questions of authentication, understanding, and of accountability, censorship, and self-censorship. While anthropologists and others have been agonizing over these issues for much of the past three decades, the current intellectual setting in Egypt and the great potential for the practical use of qualitative research as a research alternative present added dimensions to the debate.

Qualitative Research and the Debate over Meaning

Cutting through the mix of methods that are most often used in qualitative research, including case studies, personal experience, life history, observation, interview, and visual texts (Denzin and Lincoln 1994) is the central concern for meaning in natural setting. This is in fact the 'quality' that anthropological research seeks to understand and relay. As such, qualitative research has become: "...an ongoing critique to the politics and methods of positivism" (Denzin and Lincoln 1994: 4). This however is a recent development. One can trace the progress (or some may prefer to see it as regress) from the strictures of positivism to a more diversified and critical notion of facts and truths. Attempts to formalize and standardize research procedures and outcomes have been superceded by a post-positivist phase. The post-positivist trend questions factual reporting and emphasizes that writing is a creative and personal process, one that creates texts and manufactures truths (Denzin and Lincoln 1994).

The cumulative effect of this trend has been to question the monolith of positivism and of a single truth and to replace monophony with polyphony and certainty with reflexivity. Researchers who sought meaning in its natural setting no longer report what that meaning is. They engage with it, reflect upon it, and try to translate or interpret it (Clifford and Marcus 1986).

The critique to "writing culture" posits the anxieties that its proponents have concerning truth(s) and representation in their tradition of Western individualism. As Sangren makes clear in his much criticized and venomous essay:

[T]he heady feeling that one can convey the 'experiences' of the exotic other by such rhetorical devices as 'foregrounding' the 'fieldwork experience'... seems to me to suggest not that such techniques are

necessarily superior ways of communicating understanding of exotic cultures or 'experiences' but that they may merely reinforce our own individualistic delusions. Individual experience must be dialectically related to its conditions of production and reproduction in society. In short, the privileging of 'experience' or the 'actor's point of view' reproduces a bourgeois, Western, individualistic ideology (1992: 295).

In considering the merits and problems of the post-modernists in informing my own work, I find that Sangren may have a point. While this theory of writing and the basic thrust of the theory are liberating, they assume too much about the background and circumstances of the researcher. They espouse a political correctness that may not be suitable for a researcher who is not politically and practically free to do what he or she likes. Often in our own national settings, we find that our responsibility is not only to our work and its merits. We find that our work is loaded with meaning and significance that implicate whole governments, peoples, and even historical eras. Such implications can have unforeseen and perhaps serious consequences.

The post-modernist critique has provided a much needed challenge to qualitative research and has brought this field to a new point of no return. It has broken the monopoly over truth and created a free market of ideas in which truths compete on equal grounds. It has also contested impartiality and forced researchers to 'position' themselves and their work, thus making gender, class, and personal history variables that are part of and not outside of the research problematic (Rosaldo 1988). But are these innovations pertinent to researchers who would like to engage in a national or regional, or for that matter international debate but want to be recognized in their own intellectual milieu as well as in an international one? I suggest not.

Lila Abu-Lughod, for example, attempts to break the hierarchy between the researcher as the interpreter and the researched as the interpreted by relaying lived situations and refraining from establishing a monopoly over truth and meaning. By "writing against culture" she works against generalizations. To achieve this unsettling effect she chooses to "tell stories" as she witnessed, and participated in, their creation and then refuses to supply a conclusion "to make sense of it all" (Abu-Lughod 1994:13, 1991; cf. Appadurai 1988).

But is this a realistic option for researchers of lesser renown? Does it, for example, help in establishing the worth of a qualitative input in a national survey of child morbidity or in reassuring a research community about the intentions of the resident researcher, or even to aid a researcher in getting a permit and defending the legitimacy of his/her project? To what extent have theoretical innovations addressed or helped those of us working in the confines of national structures? How can researchers defend their credibility and understand their accountability? All researchers should be held accountable for what they do but not be incriminated for simply having done what they are supposed to do.

Doing Fieldwork in Egypt

Ba' hth 'ala el-tabi' a is how qualitative fieldwork is quite accurately described in Arabic; it means research in a natural setting. When I first went to do participant observation in an Egyptian village I was not living in the village. I was introduced as someone who is coming to visit the houses (*bitlef 'ala el-beyot*). When I went to live with the people I was studying, my work was called *ba' hth 'ala el-tabi' a*. The degrees of submission and suspicion that researched communities show are a function of the perceived research purpose. 'Visiting the homes' is generally considered a futile activity whereby researchers come, are told what they want to know, always with a degree of discretion, and then leave. *Ba' hth 'ala el-tabi' a* on the other hand is an enigma for the researched communities.

The power of people over the research situation is confined to the exercise of their will to cooperate or not to cooperate. Admittedly the question-marks around extended qualitative research are usually short lived. As many anthropologists have experienced, the concerns of people become overshadowed by the nature of the relationship between researcher and researched. Once trust, mutual respect, and liking are engendered, members of the researched communities come to protect and aid the researchers and trust that the researchers' presence will not be to their detriment. In return, researchers bear the responsibility of this rapport that they have sought so keenly to generate.

As an Egyptian working in Egypt I have had to live up to my promises. I have moderated the extent to which I have presumed upon

peoples' hospitality knowing that my promises to reciprocate have to be forever. I live in Cairo and am accessible to the families in Upper Egypt with whom I lived and about whom I have written. Critical to our relationship is the responsibility I have toward them in the understanding of the community that I convey. I accept this responsibility and let it affect my work because I share with them much more than our research experiences. We share a country, a history, and a present that unites us and differentiates us in a variety of ways and moments. Researchers have begun to establish the right of people to get something in return. However, we still speak on behalf of people without them even knowing that their voices are being mimicked. Real feedback and sharing of results are unfortunately rare.

We still rely on textual dominance. We still feel better that no local will read the analysis in full or take issue with it. Textual dominance means that our work can only be critiqued or challenged by another text written in a similar academic or scholastic tradition. It is a well accepted fact that 'natives don't read.' But have we come to write on the basis of this assumption? How do we continue to work with, and not despite, our responsibilities toward the people we have researched?

Moreover, because 'natives don't read,' others may do so on their behalf. This proxy readership is often behind the misinterpretation and sometimes the censure and censorship of the anthropologist. The average student working on a degree or the researcher doing work for an academic or development institution can get away with an analysis written in a foreign language or with a publication that will have a limited and strictly academic readership and function (for example, to get a degree). The situation is different for researchers who want to share, compare, or discuss their work locally or nationally. This is the area in which misunderstandings can and do happen. To explain I would like to give some examples. This may read like an eclectic collection of anecdotes but they serve an analytical purpose.

Getting a Permit

To obtain a research permit in Egypt, a researcher is required to apply at the Center for Public Mobilization and Statistics (CAPMAS). Applications must include some basic documentation and a variable sum of money for processing. Those who can obtain a cover to do research, in the form of an

official or official-looking letter, generally avoid the procedure. However, doing fieldwork without the requisite license is a public security crime. Part of the documentation required is a written questionnaire. This document is given priority since it is supposed to reflect the intentions of the researcher and research. Obviously it is not an impossible request. Many, myself included, who have had no intention of using a questionnaire have made up a mock one to satisfy this requirement. Since my work involves children I was expected to hand in the children's questionnaire as well as the one designed for adults. No easy task since my interest was in pre-schoolers.

The futility of this request has yet to register with the authorities. What is more they actually reject questions on the questionnaire and make adjustments. But like other aspect of governance, they cannot enforce these questionnaires, which often remain works of fiction. In presenting my request at CAPMAS, I thought it honest to say that I did not intend to use the questionnaires. "How will you come up with results?" was the genuinely puzzled response. At the time, participant observation, open-ended interviews, and others qualitative methods were objectionable. "A research must have an objective and a hypothesis by which it can be measured," I was told. My request was denied and I am still not sure why!

There is another, more important, reason why questionnaires are required. In the event that a work is published that does not agree with CAPMAS or with the media, CAPMAS can claim that they did not approve the questionnaire. This semblance of control over that which is very difficult to control is one of the most obstructive aspects of doing qualitative work in Egypt.

Research does get done. If a researcher is part of a known organization, national or international, no one will interfere. But for students and for the 'unconnected,' qualitative work can be a criminal activity for no clear reason, even if the researcher has the best of intentions. Strangely, permits are in no way subject to the acceptance or the refusal of the community to be researched. Their good will is assumed if the authorities like the research plan and the questionnaire. If written-up in a foreign language the research may go by unnoticed. But to share it widely is to risk stepping on the toes of the authorities if it touches on 'sensitive' issues.

In this way, getting a permit puts researchers in a position in which they either knowingly devise fake questionnaires, if questionnaires are not a

central part of their research agenda, or feel more comfortable in avoiding public debate and discussion. This furthers the problem of disseminating qualitative work; the researched community never gets to learn about the findings that were investigated in its midst.

The process of getting a permit and the precepts at the foundation of it illustrate the dilemmas of ethnographic fieldwork. At the heart of the matter lie considerations of control and security. The secrets of societies are to remain locked within them and under control until an authority issues a permit. Moreover, the rights of the researched community are rarely considered, if at all. Uncontrolled research not bound by the strictures of a questionnaire is also inherently dangerous. While I see the wisdom in protecting people from the prying questions of not-so-well-intentioned individuals, I wonder why there is no trust in communities or individuals to protect and express themselves.

What is Fit to Read?

The qualitative researcher relies on contextualized details. The accuracy of description is at the same time the forte and the liability of the method. Ethnographers prove their worth and the fact that 'they were there' by providing details of life as it is lived and of rituals, procedures, events, or daily life and conversations as they took place. These snapshots of a 'reality' relay the texture of the researched community. However, ethnographers run the risk of offending when they relay the texture of observed events. An example may serve well here.

A major international organization commissioned a study of the sociocultural factors that influence diarrheal diseases in rural Egypt in order to devise meaningful and feasible interventions to reduce their prevalence. The researchers involved in the study were asked to describe, among other things, the personal hygiene habits of children and adults.

In the course of the study the four researchers involved (three anthropologists including myself and a nutritionist) could not agree on many things. One bone of contention was about graphic details concerning personal hygiene. These details are important but once written, they were seen to give 'the wrong impression.' They were not wrong because they

were inaccurate or applied to only a few cases, but because if someone read them, they would think that the writers were insulting the research subjects.

Some team members felt that if the data had been derived from quantitative material it would be beyond reproach. The offensive details included descriptions compiled from a time/activity chart drawn for a small number of the researched households. It was felt that recounting how children move around, play, eat, relieve themselves, nod off, and so forth was more offensive than saying: "Seventy-six percent of children under age five do not have their bottoms wiped after defecation." As one researcher stressed: "if this were ever translated into Arabic, then what would happen?!" referring to the potential scandal lying dormant in our work. Eventually the descriptions were edited with an eye to how they would be read. But to some extent this decision compromised the research. Details placed in the context of daily life relay a sense of why individuals do and do not do certain things. They convey the possibilities from which people choose and show the rationale employed in making these choices. They also do so in a way that is, in my opinion, more thought-provoking if only because they are less definitive than an accumulation of numbers.

Our National Pride!

In 1992 the BBC produced a film called *Marriage Egyptian Style* as part of its *Under the Sun* series. Dr. Reem Saad was the anthropologist who researched and narrated the film, which focuses on a *baladi* (popular) woman of Cairo. The film gives a glimpse into the life of this woman and that of her son, neighbors, unfaithful husbands, and her friends. After being shown in England, the researcher submitted the film to the Ismailia Documentary Film Festival in Egypt. Undoubtedly she was proud enough of her work that she wanted it to be shown in Egypt; no doubt she saw nothing wrong with the film and saw no reason for hiding or denying it.

The uproar that ensued after the showing in Ismailia was documented in the press and media for weeks to follow. Many film critics were offended by the woman in the film. "This is not Egypt," "We have female ministers and ambassadors, why choose such a vulgar woman to portray?" "The researcher has ulterior motives! This is the black propaganda that justified the Gulf war!" are just some of the quotations from newspapers that attacked the film.

This was not simply a matter of judging the quality of the film. Audiences of film critics, journalists, local politicians, and media responsables were offended by the **lack** of stereotypes in the film. Angry commentators insisted that poor women are kind and submissive. This woman was vulgar and used bad language. She had no kind word for the husband who left her. She was shown to be ambitious and to be doing rather well working as a domestic servant and doing other odds and ends. She had enough money to buy a piece of land and build a house. She was shown on an outing to the Suez Canal wading in the water and having a good time. All of these facts and activities turned out to be facets of popular life and of a not-so-poor poverty that middle-class audiences at the festival could not handle.

This particular audience may have wanted the complacency of stereotypes. In Egypt there is an 'official' interpretation of the cultures of poverty, which is idealized in public fora. A poor mother must also be an 'ideal' mother who practices self-denial, is a victim, speaks in a sanitized tongue, and accepts her lot but tries to overcome it by investing what she has to invest in her children. Even the women who are struggling to bring up their children in the midst of poverty and alone and who know that the ideal is just a nice picture that portrays little of their experience still uphold the stereotype and hold on to it dearly. Why the subjects about whom stereotypes are constructed do not reject such idealized nonsense is a question that requires serious answers. However what we do know is that even if *baladi* women wanted to give a different rendering of their experiences, they would be hard pressed to find a means or a voice with which to communicate their own views to a public or academic audience.

Kirin Narayan writes on the experience of facing stereotypes at home and abroad and reminds us that: "[A]nthropologists are not the only brokers of cultural representation nor is academe the only field of exchange of such representations" (Narayan 1993:477). Researchers cannot ignore stereotypical thinking that abounds around them; tourists, merchants, and advertisers all trade in stereotypes. Anthropological and other social research has an obligation to diversify such facile interpretations of 'types' of people. The anthropologist behind the film was simply doing what she is supposed to do. The fact that she was censured for attempting another representation is an

example of the problems of readership. It is also one that is quite common to visual representations.

Some years ago I wrote the script for a short film that was part of a series created for the Egyptian Family Planning Association. The film addressed the issue of early marriage and the hazards of repeated pregnancies. Technical advisors sent by the association rejected the actress chosen to play the role of the young girl to be married. It is true that she was older than her screen age but that was not the reason for the objections. She was said to be too robust and healthy. The two men said that she was not weak and skinny as poor rural girls were supposed to be. As one of them put it: "Well, if she is so healthy why shouldn't they marry her off even if she is supposed to be 14 years old!"

The advisors began to doubt the credibility and ability of the whole film crew. Discussions on the dangers of early marriage revealed a sharp difference of opinion. The association representatives felt that only a weak, sick, small girl could impersonate early marriage and its dangers. Big strong girls could marry as early as they wanted as they carried fewer health risks. The writer and others on the crew felt that early marriage was a harmful practice and that to convey this message, the film needed to be realistic rather than dramatic in its portrayal of the girl. The association representatives demanded the stereotypes, rejecting any divergence from or development of the familiar stereotype of wrongly-done-by womanhood.

Researchers challenging the taken-for-granted and the politically constructed run the risk of causing offense. Moreover in Egypt there is still 'strength in numbers,' so quantitative analysis can afford to be more abrasive, although it rarely is. For example, maternal mortality is easier to talk of in terms of its prevalence, even when high, than it is to imagine in terms of women who bleed to death sometimes needlessly and often because of poor access to and quality of health services. Qualitative images on the other hand can not only be abrasive. They can also be challenged in terms of their rules of authentication. How can you prove the validity and replicability of challenging findings? Sadly they may be easier to dismiss than to engage with.

Conclusions

It is futile to argue that different texts are specific to different readers. Perhaps the degree of specialization in natural and social sciences has meant that texts have become impregnable to the non-specialist. But this exclusivity of readership is problematic, particularly in the social sciences. Daily life and the actions and interactions of individuals and communities are fields in which the specialist and non-specialist can and should converse. Different interpretations of texts should be welcomed. As Moore argues, readings create text. The acts of reading and of re-reading are the final and definitive stages of the production of text (1996). In Egypt and in the social sciences in particular, as long as readership remains exclusive, the texts themselves will be problematic. The examples I have given above all indicate the dangers of this exclusivity. They illustrate that stereotypes will remain and prevail as long as challenges to them continue to be objectionable and threatening, to the degree that censorship and censure are employed to stifle discussion. Widening readership will create diversified interpretations and break the tyranny of the one official voice that currently reigns as spokesperson for how 'the majority,' who ever they may be, thinks and feels.

Sharing with studied people what has been written about them is a formidable challenge to a researcher and can be embarrassing and problematic. Nevertheless, it is a constructive endeavor. It can only make the researcher's work more sensitive, better informed, and will definitely challenge easy conclusions. When the readership is confined to designated authorities or intellectuals who are uninformed about the daily lives of those represented and who may therefore rely on commonly accepted stereotypes to read and interpret, and even to judge, then these proxy readerships come to act as censors on behalf of those they claim to represent.

In looking at the post-modernist critique, I have assumed that there are common interests and concerns shared by researchers. After all, if some have found solutions to their practical, political, and existential dilemmas in this school then why shouldn't I be able to find similar clarity and comfort? But the problems faced by many people besides myself are rarely given serious consideration by these lauded innovations in social thinking. As someone who lives and works in Egypt, like others who work in their own countries,

my 'field' and my home are one and the same place. I have other audiences, comprised of people with whom I have worked and those in different fields of Egyptian public life.

I have no wish to recreate the fallacies of the school that favors indigenous researchers over non-indigenous ones. This promotes a kind of nationalism that cannot further understanding per se. However, we must recognize a difference between working in familiar territory and being a stranger. Not in terms of skills, language capabilities, or other data collection proficiencies, but in terms of the nature of the relationship between researcher and field and between researcher and research subjects. We need to scrutinize the long-term responsibility entailed by working at home and the access that research subjects have to researchers and research texts.


Consequently anthropologists at home and abroad need to stay on shared intellectual territory. Ideally we should all work at home and work abroad and thus have a greater sensitivity to one another. Meanwhile, if the current theories fail or exclude researchers working at home, then it is incumbent upon them to comment and critique the theories. The absence of such critiques would indicate an acceptance of the inequalities between an Us and a Them. The situation would evolve such that we could collect the data because we can speak the language but we could not analyze or theorize from our findings. Questions concerning writing in our mother tongues, of being a 'native,' and of bearing responsibilities toward other natives are left to our devices.

My concrete recommendation is that we create a writing and reading tradition that encompasses both the specificity of our local situations and the universality of the problems of representation and reading. We need to evolve criteria for verification and authentication that take into account not only the multiplicity of truth and meaning, but also that of different researchers and their historical and political contexts. If we create diverse interpretations, report variations of experiences, and develop theories that are sensitive to history and geography, we can explode stereotypes. If we can create a discourse of qualitative research, we can establish it as a method with integrity and credibility. Building a culture of qualitative understanding will protect, improve, and challenge our own work.

Why is anthropology so hard in Egypt? My very personal answer is because I am yet unable, whether due to circumstances or capabilities, to

share my work with others in Arabic in Egypt without making changes and accommodations. If these changes were made to accommodate the privacy or sensibility of my studied community, that would be an advancement. But they have been made on behalf of a readership that presumes the right to control and censor qualitative work by virtue of power or position.

Referring back to the new-found fame of anthropology in Egypt, it is as though qualitative methods and insights are acceptable if they are constructive and complacent but not if they are unsettling or critical. Policy makers, journalists, senior and not-so-senior officials and development persons are interested in knowing that mothers-in-law influence decisions concerning female fertility, for example, but are less keen on facing facts concerning the political threats posed by street sub-cultures. To re-phrase once more, one could say that the observations of anthropologists are fine but their analysis is unwanted.

My argument has hinged on the centrality of readership. I believe that rendering readership problematic can first draw attention to the dangers of limited readership whereby the powerful few read and can censor on behalf of the many. I am not only suggesting that researched subjects should be able to give their own reflections on how they have been portrayed and analyzed. I believe that if more anthropology was written and read by specialists and non-specialists in Egypt, the sensationalism of intimate details and the impact of graphic renditions of daily life would lose their sting and become normalized in the democracy of interpretations. 

Secondly, contemplating readership can help those working at home contribute an as-yet missing dimension of social theories on representation, research, and writing. I have argued that it is this position, more than our color, language skills, or training that sets apart native anthropologists working at home.

The last point I want to raise concerns who 'we' are. 'We' are a critical mass of researchers who have trained in anthropology and who have ears in the West and eyes at home. I am referring to people who know what is going on in the corridors of Western academia, are familiar with that language and with its eccentricities, but who see themselves as apart from it by virtue of their immersion in local, national, or native realities and relationships. The contributors to this volume are typical of 'we.' They are the people hired to do focus groups and then censured if they do them too

well. They are also the people who have yet to be as prolific at home as they have been abroad through publications and analysis of that home.

It is probably due to these people that anthropology will find a space in Egypt and elsewhere. I hope it will be a meaningful and useful place and not just a kind of institutionalized voyeurism that sees much and says little.

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