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On the Medicalization of Medical Anthropology

The following address was presented at the Annual Meeting of the Society for Medical Anthropology (SMA), November 21, 1997, Washington, D.C., by outgoing SMA president Carole Browner. It has been edited slightly for publication.

I would like to take this opportunity to reflect upon a tendency in our field that concerns me, a tendency I've come to call the medicalization of medical anthropology. By this I mean the trend among us to study entities as they are conceptualized by biomedicine—cancer, tuberculosis, or arthritis, for instance, or “stress,” “PMS,” or “postpartum depression.” At the same time, we are seeing growing numbers of medical anthropologists employed in the United States, not in basic research but in facilities that actually deliver medical care.

Neither of these developments is surprising. As funding for foreign research evaporates and opportunities for teaching in anthropology departments decline, it's only natural that we seek work where we can. Nationwide political pressure to address issues of cultural diversity in medical research, and the perception among mainstream health care providers that there is money to be made by courting ethnic minority patients and offering “alternative” medicine have also led to an increased demand for medical anthropologists.

Nor are these developments necessarily negative. For too long, anthropology has been associated in the public mind with the peripheral, the exotic, and the bizarre. It's time that we become more visible and that the full range of our expertise be brought to the fore. In addition, we have much to offer to the world of biomedical research. It's good to see more medical anthropologists studying infectious disease and chronic illness and a whole range of conditions we've neglected for far too long. My reason for addressing this now, however, is to urge that we not sacrifice what has made us unique as we inexorably move toward the more medicalized medical anthropology I've just described.

Some of the dangers I see in a more medicalized medical anthropology are associated with the risks of “going native.” One negative consequence is many more

medical anthropological studies that uncritically accept biomedical illness categories and explanations. This may be due to anthropologists' desire to find a common language with which to communicate with colleagues trained in the biomedical sciences. Or it may represent an effort to win acceptance by those allied with an immensely powerful institution. Or it may simply reflect the fact that biomedical categories unconsciously color our thinking to an extent to which we may be unaware. For most of us have already internalized the U.S. biomedical paradigm long before we become medical anthropologists. An example from my own work will illustrate this last point (Browner and Press 1995; Press and Browner 1994, 1997).

I have been studying the considerations pregnant women living in southern California take into account when deciding whether or not to have their fetuses screened for birth defects and other developmental disabilities. The screening is conducted through a maternal blood test administered during the second trimester of pregnancy. Since the mid-1980s, the screening has been offered to all pregnant women in California in the context of their routine prenatal care. The vast majority of the conditions detected through fetal screening have no treatment or cure. An induced abortion is all medicine can offer should a fetal defect be found. The participants in our studies are all women with no prior risk factors for bearing a child with a birth defect. Few, therefore, anticipate being offered fetal screening as part of routine prenatal care.

Nevertheless, in designing our research, my colleagues and I conceptualized what we were studying as a deliberate decision about whether or not to accept the offer of fetal screening. This conceptualization grew in part from our ethnographic observations in clinics and obstetricians' offices where women must indicate both verbally *and* in writing whether or not they want to be screened. This seemed to us to imply that pregnant women were facing a deliberate choice. After all, we knew of no other medical procedure patients are required to sign for when turning it down. The scientific literature on the subject made the same assumption that screening would be a deliberate decision.

However, we found that few of the pregnant women we interviewed retrospectively described their fetal screening decisions as having been made with much deliberation. The small proportion who did were either strongly in favor or adamantly opposed. They could readily recount the thought processes involved in the choice they had made. Most of the rest had no strong feelings one way or the other. They either accepted or declined screening based largely on how the offer was made.

Another unexpected finding was that women who screened positive (indicating the existence of a potential problem) did not necessarily intend to abort their pregnancies. While the literature assumes that the purpose of fetal screening is to avert the births of fetuses with defects, this was not at all what motivated the women we interviewed who agreed to be screened.

None of this may seem surprising in the case of a diagnostic procedure little known to the public. Yet clinicians and researchers assumed otherwise and as a result drew false conclusions about these women and others like them, conclusions about their willingness to comply with medical recommendations, their views on abortion, and even the kind of mothers they would become. Having read that clinical literature, we ourselves also assumed that we were studying something far more momentous than, in fact, was the case. Moreover, our efforts to understand

pregnant women's decisions about fetal screening by interviewing the women themselves were seriously misdirected.

To what extent, then, should we as ethnographers be aware of the biomedical thinking on a subject before beginning our study of it? I'm not sure there is a correct answer to this question. Two colleagues, both planning work on cancer, took opposite approaches when they began. One scrupulously avoided all biomedical materials, while the other read everything she could find. Each approach had its price: one put the ethnographer too close and led to a loss of critical distance, while the other left the ethnographer too far outside the very paradigm she sought to understand.

There are both substantive and methodological consequences that follow from an increasingly medicalized medical anthropology. When medical anthropologists for the most part engaged in cross-cultural community studies, their holistic analyses situated health-related phenomena in a broad sociocultural context. In contrast, studying a narrowly defined biomedical condition such as arthritis or asthma is inevitably a different sort of task. For one thing, the conditions may have limited or intermittent, rather than global, impact on the lives of those affected and others around them. For another, those affected by a given condition tend not to form any kind of natural community but instead are dispersed throughout a larger population. What, then, does a holistic study of a biomedical disorder entail?

Research by a growing number of ethnographers provides provocative models, for instance, Allan Young's work on post-traumatic stress syndrome (1995), Margaret Lock's on menopause (1993), Nancy Press's on breast cancer (Press et al. 1997), and Thomas McDade's on benign prostate disorder (1996), to name a few. Each of these studies provides detailed documentation of how medical conditions are as much cultural constructs as they are physiological processes. In another sense, Martha Balsham's ethnography of cancer in a Philadelphia neighborhood offers an excellent model for studying the impact of sickness on a community (1993), as does Merrill Singer's work on alcohol addiction (Singer et al. 1992) and Philippe Bourgois's on crack cocaine (1995). In still another sense, Lorna Rhodes's recent ethnography of a psychiatric emergency unit describes how external, community-based ideologies about mental illness influence treatment practices within the facility (1991). But there are still far too few such revealing and richly textured accounts.

The methods used in a more medicalized medical anthropology also differ fundamentally from the past. When most of our work was community based, and participant-observation our primary technique, we inevitably became immersed in the world we were investigating. This, in fact, was conventionally taken as the hallmark of a first-rate ethnography. But how does one become immersed in a circumscribed biomedical condition? Does it mean we can deeply understand it only if we experience it firsthand? Certainly, some of the most memorable medical anthropological accounts are by ethnographers who have written about their personal illness experiences. Robert Murphy (1990), Susan DiGiacomo (1992), Rayna Rapp (1984), and Linda Layne (1996) immediately come to mind. But those of us who work on conditions we have not personally experienced face a unique challenge, for our contact with "the field" is intermittent and we cannot internalize the illness experience.

A related problem concerns the kind of data we obtain when we focus on biomedical conditions. An important advantage of ethnographic approaches is that our data are not limited to what we are told. Our own observations and experiences are also central to our analyses. However, in order to establish credibility with biomedical colleagues or satisfy the requirements of funding agencies, we may find ourselves working with samples far larger than we might otherwise choose when we study biomedical phenomena.

Outside a hospital or other treatment facility, few conditions exist in large enough numbers within a circumscribed area for “community study” techniques to be applied. The alternative is formal interviewing—often of the one-shot variety—with all of the limitations this implies. Methodological experts have already written about some of the limitations of one-time interviews, such as the difficulty of establishing rapport. Relying on one-time interviews also means that we learn about a condition primarily through firsthand accounts rather than having such accounts supplemented by our own observations. As a result, we sacrifice the insight we otherwise would have gained into the gap between self-report and an outsider’s perspective. More importantly, perhaps, we sacrifice the ability to assess the impact of the condition on the larger life experiences and communities of those affected.

Of course, one-time interviews are not the only way to study biomedical conditions. Provocative ethnographies using traditional approaches have been produced by investigators such as Joan Ablon in her work with persons with neurofibromatosis (1996), Myra Bluebond-Langner in her accounts of families with terminally ill children (1978, 1996), and Sue Estroff in her research into the worlds of previously institutionalized persons with mental illness (1981). Their work provides provocative models for similar research.

I’ve just described some of the consequences of a more medicalized medical anthropology for the discipline. But what are the consequences for us, as medical anthropologists? For once the focus of our investigation shifts to biomedicine, we enter a world over which we have little control. While in some cases this has led to fruitful collaboration, often the results can be frustrating for all concerned.

To give one example, my colleague Linda Hunt spent three years working in a research center in a department of medicine on a number of projects focusing on the impact of chronic illness on ethnic minorities. Her efforts to introduce an anthropological perspective were often rejected by her physician-collaborators, who viewed such concerns as esoteric and beside the point. She has characterized her central experience at the research center as an ongoing clash of paradigms. As her tenure there was drawing to an end, she described a dream she had. She wrote,

In my dream I sat at a conference table, arguing with a group of frustrated physicians who were earnestly urging me to see their point: “If it looks like a duck, walks like a duck, and quacks like a duck, it’s a duck!” one shouted. Equally frustrated, I shouted back: “What do you mean by ‘a duck’?” [Hunt 1994:1]

The issue, then, is how medical anthropologists and biomedical researchers and clinicians can work together to arrive at mutually meaningful categories, concepts, and terms, and how the processes by which this occurs will be determined. Too often, we find that by the time we get to the table, such points are no longer subject to negotiation.

Some medical anthropologists have found the search for a common language with which to communicate with biomedical colleagues daunting. Others feel undervalued when asked to come up with a “quick” or “simple” ten- or 20-question research instrument so that their colleagues can quantify the “effects” of culture or measure the “strength” of an individual’s ethnic affiliation.

Others feel depreciated when they discover they have been included on a project only to satisfy a funding agency’s requirements. A colleague recently told me of being invited to participate in a group preparing a proposal to compare the effectiveness of different types of acupuncture anesthesia to control the pain of osteoarthritis of the knee. The P.I., a physician, introduced the anthropologist to the group as someone whose expertise in ethnicity would make their project competitive. When the anthropologist had the temerity to ask why ethnicity might be expected to affect a patient’s response to different types of acupuncture anesthesia or what such variation might mean if any were found, the physician replied, “Well, you’re the anthropologist, that’s for you to tell us!”

Conflicts, frustrations, and difficulties of these types will be inevitable as we intrude further into territory that historically has been biomedicine’s preserve. We haven’t defined the terms of engagement and are often seen as interlopers besides. Yet as long as Americans remain as obsessed with their health as they have become and health care administrators see our value in the marketing of their services, the demand for medical anthropologists will remain strong. How to respond to these opportunities while retaining the uniqueness of our anthropological perspective will be our challenge in the years to come.

NOTES

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