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Female Genital Cutting: A Harmless Practice?

A recent article in Medical Anthropology Quarterly (Obermeyer 1999) argues that the “facts” about the “harmful effects” of female genital cutting (FGC) are “not sufficiently supported by the evidence” (p. 79). The article suggests three further hypotheses, among others: (1) FGC may be of minimal harm because the more educated continue the practice just as much as the less educated; (2) FGC may be of minimal harm because it is so widespread and persistent; (3) FGC may be of minimal harm because the supposed link between the clitoris and female sexual pleasure is a social construction rather than a physiological reality. I challenge these hypotheses. I say that by appropriate standards of evaluation, FGC is harmful. Finally, I submit that most FGC is a proper matter of concern because it is the irreversible reduction of a human capacity in the absence of meaningful consent. [female genital cutting, harm evaluation, critical epidemiology, harmful traditional practices]

The stated purpose of a recent article in this journal (Obermeyer 1999) is to obtain a relative reduction of funding for research intended to assist affected peoples to abandon the practice of female genital cutting (FGC), labeled advocacy, and thereby obtain a relative increase of funding for the mass survey research preferred by the author, labeled science (p. 85). The article has entered public debate as warrant for the claim that FGC is of minimal harm, and, thus, that it should be **legalized** for minors in the United States (it is presently legal for **consenting adults**) and other immigrant-receiving countries; that claims for asylum based on FGC are spurious; and that international agencies such as WHO and UNICEF should cease supporting programs for abandonment of FGC in practicing areas of Africa (Shweder 2000, 2001).

The article speaks in the voice of science as the authoritative survey of the epidemiological, demographic, and anthropological evidence on the question. It was awarded the Polgar Prize by the Society for Medical Anthropology, for the best article published in volume 13 (1999) of the society's journal, *Medical Anthropology Quarterly*. The article is correct that some people have undifferentiated and exaggerated views about the forms, meanings, and risks of FGC, and that it

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would be better to have more research on the topic. Otherwise, many of its remarkable claims are mistaken, or so I shall argue. Because of the article's vigorous claim to authority, and because of its actual and possible effects on public policy and private health decisions, I call attention to ambiguities, inconsistencies, and errors that one ordinarily might leave unmentioned.

This article mostly parallels the Obermeyer article. The following section examines its conclusions. Next, I consider estimates of prevalence of FGC. Then I contrast the article's idiographic theory of FGC to the convention theory of FGC. The section on trends and variations in the practice argues that the convention theory better explains the observations. Next, I ask whether the article's generalizations about the so-called advocates are appropriate. A section on frequency of health complications looks at neglected evidence.¹ I consider alternative explanations for the alleged scarcity of evidence pertaining to FGC. And I apply results from field research to show unsuspected limitations to the survey research recommended in the article. I then examine its assessments of the harms and benefits of FGC. Then I discuss the issue of effects on sexuality and the article's relativist undercurrents. A summary and evaluation conclude my article.

Scientific Conclusions

The article presents itself as the scientific approach toward FGC, in opposition to the "moral advocacy" (p. 78) approach of policy makers, activists, and professionals in various fields concerned to end the practice. The article claims to be within the purely empirical scientific framework (p. 97), based on the sciences of epidemiology and demography, "two disciplines where the evaluation of empirical evidence is a central preoccupation" (p. 81). The moral advocacy position is said to lack nuance (p. 79) and moderation (p. 98).

According to the article, the moral advocacy position has aroused an international consensus and mobilization to end FGC based on two premises: (1) that the practices are widespread, and (2) that the practices have "extremely harmful consequences for those who undergo them" (p. 80). An unstated implication of such an argument is that if one of the premises were wrong, then the international consensus on advocacy would be wrong; the author states that the second premise is wrong. I say, however, that the international consensus and mobilization on FGC is based on two alternative premises: (1) that in general there is an absence of meaningful consent to the irreversible act of FGC, and (2) that complications are nontrivial (e.g., Nussbaum 1999:118–129). Liberals are reluctant to intervene against activities freely consented to by adults, or of trivial consequence. Prevalence is not morally relevant: If only one person suffers nontrivial complications in the absence of consent, then that is a matter for concern. Complications are relevant only if nontrivial, but such complications need not be precisely measured, especially frequent, or especially severe.

The article's method is to search standard academic databases for literature, as of April 1996, relating to FGC, retrieve that literature, and review it from a scientific standpoint with respect to four topics of interest: (1) prevalence of FGC, (2) variations and trends, (3) health complications, and (4) effect on sexuality. Its conclusions (emphasis added) on the state of the evidence are as follows.

This article . . . examines the extent to which available research supports commonly accepted “facts” about the prevalence and harmful effects of these practices. [p. 79]

The assertion that female genital surgeries *unequivocally* function to *destroy* sexual pleasure *appears* to be scientifically inaccurate. [p. 96]

The powerful discourse that depicts female genital surgeries as *inevitably* causing death and *serious* ill health is *not sufficiently* supported by the evidence, which includes no *incontrovertible* data on mortality, and *suggests* that *severe* complications are *relatively infrequent*. [p. 97]

The conclusions quoted depend on ten qualifications (which I mark with italics). The quoted passages state only that a *caricatured* proposition—that incontrovertible evidence shows that almost every instance of FGC results in severe complications or death—is false (which is uncontroversial, as no serious student of the topic holds such a view).

To speak in the voice of science on matters that effect public and private health decisions and practice demands responsibly phrased expression. The article’s provocatively stated conclusions have been misunderstood by some readers and may be misunderstood by others. How might they read if we removed the ten qualifiers and readjusted phrasing?

Systematic demographic and health surveys support earlier informal estimates of country and global prevalence. The hypotheses that female genital cutting contributes to death and ill health, limits sexual capacity, and that the severity of harm is related to severity of the cutting, are supported by the evidence at hand, which includes anatomical inference, supplemented by anecdotal evidence and some formal studies. In regard to sexual capacity, occasional anecdotal reports and a few limited studies suggest that there are unexpected individual and local variations that deserve further investigation. Given the scale and complexity of the problem, more demographic and health survey research is desirable, especially on the severity and frequency of negative effects and how they relate to variations in the practice.

My reconstruction is not as dramatic as the article’s statement, but is, I believe, closer to scientific norms of expression. I would add to considerations in the article the following: Given the inevitable budgetary limits of any research effort, and the fact that FGC is a social practice rather than a pathogen or toxin, it is more important to investigate the sociology of the practice, including experiments in abandonment, and less important to replicate survey data on the practice. I would also add that the consent objection to FGC requires that research ultimately be oriented to effective abandonment of the practice.

The article’s exclusive reliance on epidemiology to establish harm is not apt.

Traditional epidemiology studies the distribution of a disease, and the factors that influence this distribution, in order to explain the etiology, and to provide preventive, public health, and clinical practices. [Brown 1995:92]

In recent decades epidemiology has been shaped on a laboratory science model, often more concerned with protecting the increasingly rigid standards of scientific procedures than with safeguarding public health. [Brown 1995:91]

The genius of epidemiology is to detect a causal relationship between an exposure, often subtle, and outcomes that are obscure, rare, or latent. There is no causal question, however, about the connection of the traumatic injury of FGC to immediate complications such as bleeding and infection, nor to many of the delayed complications cited in the literature. Nor is prevention obscure: Refrain from doing the FGC and the complications do not ensue. Effective strategies of prevention are a sociological question, not a medical one. Secondly, epidemiology might establish frequencies and dose–response relationships. Such information might be very useful in crafting strategies of prevention, but it need not be, and in any case we would also want insights from many fields of inquiry, most of all from people involved in the practice and attempts to end it.

Suppose that one is concerned about domestic violence. Major aspects of the problem are absence of meaningful consent and nontrivial complications. It does not take any epidemiology to establish either. Suppose there is a movement to provide shelters for battered women. What would its advocates say to an academic who writes that there is no incontrovertible evidence that domestic violence causes severe harm in almost every instance, and that science demands that such experiments in prevention be reduced to increase research about the frequency and severity of harm? More and better numerical data would always help, but would not be central to an effort to end domestic violence.

The article says that the concerns of the FGC activists guide the research agenda; thus, resources are directed toward “‘intervention studies’ aimed at abolishing the practice, rather than toward scientific inquiry” (p. 85). Why call intervention studies advocacy rather than science? A well-done abandonment project is an experiment in social practice, and either success or failure in attaining abandonment advances knowledge about the *sociological* causation of the practice. For example, a functionalist theory of FGC holds that it is maintained by benefits gained by the cutters. Failure of compensate-the-cutter abandonment projects counts against both that strategy and the functionalist theory behind it.

Traditional epidemiology does not provide an appropriate standard of evaluation for public policy. Over the last 30 years, participants in the environmental justice movement have heard again and again, from economic interests and sometimes from governments at their behest, confused or misleading claims of insufficient epidemiological evidence as supposedly scientific warrant to expose populations to unjustified harms.² In response, a more critical approach has arisen: environmentalist epidemiology (Tesh 2000), popular epidemiology (Brown 1995; Brown and Mikkelsen 1990), alternatives assessment (O’Brien 2000). There are parallel calls for a feminist epidemiology (Inhorn and Whittle 2001). Traditional epidemiology emulates laboratory science in preferring a large number of false negatives—erroneous findings that an exposure is safe—when, for purposes of public policy, it should emulate ordinary prudence in preferring a large number of false positives—erroneous findings that an exposure is harmful (see Cranor 1993 for a rigorous treatment of this and other aspects of the morality of risk assessment).

The article demands evidence that is *incontrovertible*; further, it states that those who claim nontrivial complications must present *irrefutable* (p. 91) and *indisputable* (p. 97) evidence for their case. An epidemiology textbook explains that since Hume it has been understood that induction fails to provide a foundation for conclusive causal inference (Rothman and Greenland 1998:22). Perhaps the author

means that the claim must be beyond any reasonable doubt. But there is no reasonable doubt that FGC is a traumatic injury, nor, from an anatomical standpoint, that there are associated complications of nontrivial quality. Frequencies of complications are more vaguely apprehended, but, for purposes of public policy, these assessments should be made by the public-policy standard rather than by the laboratory standard of evidence. By either standard, the author has no warrant to claim as “fact that the most severe complications are actually rare events” (p. 93).

Similarly, the article’s anticlimactic claim that FGC is “probably” not a matter of “relative safety,” but that further studies are needed to attain greater certainty (p. 97), wrongly assumes that harm is harder to establish than safety and it commits an error of misplaced precision. There is insufficient evidence that FGC is safe. There is no reasonable doubt that FGC is harmful.

Prevalence

The article suggests that the evidence on prevalence was unscientific and motivated by advocacy (p. 85). However, in the earlier 1990s, as the article was being composed, systematic surveys of nationally representative samples were carried out in the Central African Republic, Egypt, Eritrea, Ivory Coast, Mali, and the Sudan by the Demographic and Health Surveys (DHS) Program, many of the results since summarized in Dara Carr (1997). Keep in mind that the article’s bibliography indicates that at the time of writing, the author possessed those DHS results (pp. 99, 102) and, after writing, but around two years before publication, possessed Carr’s summary (p. 98). The article accepts the DHS data on prevalence (p. 98). It criticizes almost all prior data on prevalence as being based on samples of convenience or on anecdotal evidence; it even rejects some of the sample studies completely because “they did not report how the data were collected, had a very small sample size . . . had a low response rate, or because of the use of indirect reporting.”

The article rejects the pioneer estimates of American FGC activist Fran P. Hosken (1993) because of the “poor quality of the evidence they use, and the methodological shortcomings of their estimations” (p. 85). It says that Hosken “frequently does not cite sources, nor does she indicate whether they came from anecdotal evidence, primary case reports, or population-based studies,” etc. (p. 99). Hosken’s harsh and sometimes ethnocentric advocacy journalism does not satisfy academic norms, but it was more informative than the silence that preceded her efforts. Nahid Toubia (1995) estimated prevalence based on best reports, with citation to sources; the author is able to remark that some of Toubia’s reports are anecdotal because Toubia labels them so.

Moreover, according to the DHS survey data that the article accepts, the advocates were about right; indeed, they underestimated prevalence a bit. For Mali, Hosken (1993:44) estimated 75 percent prevalence; Toubia (1995) estimated 80 percent, and there were no acceptable samples of convenience. The proper DHS survey found a prevalence of 94 percent (Carr 1997:72; Obermeyer 1999:83). Thus, the “advocates” *underestimated* prevalence in Mali.

For Ethiopia and Eritrea, Hosken and Toubia estimate 90 percent, DHS data (Carr 1997:70) on Eritrea (which has separated from Ethiopia since Hosken’s report) indicate 95 percent. Dos Santos reported infibulation in Somalia in 1609

(Freeman-Grenville 1962), and there have been abundant anecdotal reports over the past 400 years about the near universality of the practice there.

For Somalia, Hosken (1993:44) estimates 98 percent, Toubia (1995) 98 percent; six are local samples or samples of convenience: 93, 99, 100, 100, 100, and 100 percent, and the DHS survey indicates 100 percent. For Somalia, the “advocates” *underestimated* prevalence.

For Egypt, Hosken (1993:44) estimated 50 percent, Toubia (1995) 80 percent, and local samples 85 and 91 percent. The DHS survey found 97 percent, which surprised some Egyptian observers, according to the article. A representative follow-up study was done based on clinical examinations, which estimated prevalence of 93 percent. The “advocates” *underestimated* prevalence in Egypt.

Hosken (1993:44) *underestimated* prevalence in the Sudan, 80 percent compared to representative surveys showing 89 (accepted by Toubia 1995), 96, and 99 percent and *overestimated* in the Central African Republic (Hosken 50 percent, DHS 43 percent) and in the Ivory Coast (Hosken 60 percent, DHS 43 percent). There are no DHS data yet for the remaining countries estimated by Hosken, or by Toubia and Susan Izett (1998) and published by the World Health Organization.³

The formal DHS data are more reliable and precise, but otherwise resemble the informal estimates of the advocates, at both country and global levels. The advocates did not irresponsibly exaggerate prevalence. Similarly, there is no reason to expect that more and better numerical data on complications would overturn the international consensus that they are sufficiently nontrivial to merit support for the abandonment of FGC.

Theory: Idiographic or Comparative?

According to the article, “female genital surgeries” are practices that are ambiguous, variable, changing (p. 84); sociodemographic factors equivocally relate to them (p. 88); indeed, “it may not be possible to fully understand the complex forces that account for the persistence or decline of these practices” (p. 97). It welcomes evidence from demographic surveys and ethnographies but cautions against comparative sociology and generalizing theorization about the practice (p. 89). Comparative studies are “simplistic. . . . They conflate a heterogeneous set of practices into a single category, for which explanations are then sought at the global level” (p. 89). Further, the comparative approach is part of a process “whereby the Other is reduced to a physical attribute or social characteristic, thus justifying reactions of rejection and contempt” (pp. 89–90), although the demographic and health survey research that the article champions is somehow exempt from the same charge (p. 97). The author believes that the only commonality among the practices is that they involve the cutting of women’s genital areas (p. 89). But just as it is possible for the ethnographer to generalize despite the range of circumstances, motivations, and meanings across individual respondents, so is it possible for the comparativist to generalize across groups.

A commonality of major importance overlooked in the article is that FGC is found more or less contiguously within a zone of distribution, but is not found outside that zone (with a few exceptions that can be traced to diffusions in the historical era). Such a distribution demands theoretical attention. Also, adherents give different but overlapping reasons for the practice. When we look for patterns

across lists, we notice that marriageability and tradition are offered as reasons in almost all groups. This commonality suggests that these two reasons usually play the causal role and that the remainder of reasons are either explained by marriageability and tradition or express noncausal associationist responses to the practice.

The Schelling-convention hypothesis (Mackie 1996, 2000) begins from the link, in most practicing groups, between FGC (or, in traditional China, footbinding) and marriageability of the daughter. We need not know how such a practice originated to understand that, once in place, a convention associating the practice with marriageability will be self-enforcing. It will be nearly universal within an intramarrying group, because individuals who ignore the convention go unmarried, and it will be extremely persistent, again because individuals acting alone cannot escape from the convention. If FGC is such a convention, then the tradition explanation frequently offered by insiders is entirely rational, although possibly opaque to outsiders. Also, when intramarrying groups overlap, the practice will diffuse contagiously from higher-status groups to often contiguous lower-status groups. Further, even if every family in an intramarrying group were to come to believe it would be better to abandon the practice, no one family stopping on its own would succeed, because unilateral abandonment would ruin the futures of its daughters. The great importance of the daughter's marriageability outweighs the importance of health, bodily integrity, and consent, even among those who endorse the latter concerns. Thus, *we would expect to observe that the practice is carried on even by those who oppose it*. Footbinding was carried on even by those who opposed it, as is FGC (Mackie 1996). We would also expect to observe that were such a convention to end, it would end suddenly within an intramarrying group.

The only way to abandon such a convention is if a critical mass of the families in an intramarrying group agrees to stop together at the same time, which preserves the marriageability of their daughters. This method worked to end the convention of footbinding in China (Mackie 1996). In Senegal, people in rural villages with almost no formal education were exposed in a basic education program to nondirective health, human rights, and other information that, among many other items, included information about FGC. They were further given information about how one village (later many) stopped FGC by means of collective deliberation culminating in public declaration that all would stop FGC at the same time. As predicted by the convention hypothesis, people in exposed villages decide to abandon FGC by the same process (Mackie 2000; Melching 1999), and these are by far the largest abandonments in the FGC zone. Further abandonments now proceed through hundreds of villages by way of organized contagion. The convention hypothesis is intended as a comparative generalization (of course, there are complications and exceptions), the goal of much social science, and not as a universal law.

Trends and Variations

The article reports that in some places there is a (modest) decline in prevalence and shift to less severe forms. Elsewhere, persistence is robust (p. 87). There is an association of FGC with region and with ethnic group, but an equivocal association with urbanization, it says, and an equivocal association with education (p. 88). The equivocal association with education, it argues, "undermines a key presumption" in the literature, "that the spread of formal education, mass media, and 'modern'

health care entails a convergence in worldviews towards biomedicine and the particular ways in which 'universal' values are defined . . . in international human rights documents" (p. 89). This discrepancy poses a puzzle to the intellect and imagination, it says (p. 89); FGC is perhaps even "unknowable" (pp. 97, 98).

The absence of an education effect on attitudes toward FGC is the second most important claim in the article (the first is the minimization of complications); the finding is now repeated in policy debates (Shweder 2001). The claim contributes to further inferences: that the understanding of advocates and generalizers is shallow and ethnocentric (p. 90), that FGC continues because it is of minimal harm (pp. 91, 92, 94), and that the association of genital organs with sexual enjoyment is a social construction peculiar to the Western worldview (p. 96).

The article reports three studies that do find an education effect, that is, an association between more education and less prevalence of FGC (p. 89). It claims that newer and better DHS data show no such association. It appeals, for example, to Fatma El Zanaty et al. (1996; called DHS, *Egypt, 1995* in the article's bibliography) to establish that 90 percent of women with secondary or higher education in Egypt are cut. The article does not report, however, that El Zanaty et al. (1996:173) also found that "Women with a secondary or higher education are the least likely to have or to consider having their daughter(s) circumcised."⁴ It helps to display further DHS results in tabular and more complete form, as in Table 1. Looking at the columns labeled *Beh* (Behavior), indicating percentage cut, it is plain that in five of the six countries there is an education effect (Sudan is the exception, and I surmise that ethnicity confounds education in this case).⁵

The author might reply that there is an education effect on *behavior*, but that it is a weak one, and I would agree. The article underplays (pp. 99–100 n. 19, with only partial data from only two countries), however, important *attitudinal* data from the DHS. Looking at the columns labeled *Att* (Attitude) in Table 1, it is plain that large proportions of the more educated do *not* support continuation of FGC, and in larger proportions than the less educated. In Eritrea, for example, there is 92–95 percent prevalence of FGC, but of women respondents with secondary education or better, 80 percent *oppose* continuation of the practice, as compared to 61 percent opposition by those with primary education and 24 percent by those with no education.⁶

Table 1

Beh = Behavior = Prevalence, percent respondents cut;
Att = Attitude = percent respondents support continuation of FGC.

Country:	CAR		CDI		Egypt		Eritrea		Mali		Sudan	
Education:	Beh	Att	Beh	Att	Beh	Att	Beh	Att	Beh	Att	Beh	Att
Sec'ary+	23	11	23	na	91	61	92	18	90	48	98	55
Primary	45	29	25	na	100	87	93	34	94	76	98	84
None	48	36	55	na	100	93	95	71	94	78	83	82

Sec'ary+ = Secondary education or better

CAR = Central African Republic, CDI = Ivory Coast

Source: Carr 1997:69–74

According to Carr, “In nearly all of the countries surveyed, urban and educated women are more likely to oppose continuation of genital cutting” (1997:7). The article does not cite this observation, even though it is listed as a special finding in the summary of the DHS surveys and presented as a category in their tabular displays (Carr 1997:9, 69–75). Attitudinal data show a strong education effect, and, thus, confirm the standard convergence hypothesis.

There are many who oppose the practice of FGC but nevertheless intend it for their daughters. If behavior matched attitude, then, among women who oppose continuation of cutting, 100 percent in each country would intend not to have their daughters cut. Further DHS data show that among women who oppose continuation of cutting, 71 percent in Egypt, 32 percent in Mali, and 66 percent in the Sudan do not intend to have their daughters cut (Carr 1997:89). “The findings suggest that *these traditions can prevail even among the offspring of mothers who say that they disapprove of cutting*” (Carr 1997:54, emphasis added), just as predicted by the convention hypothesis. The DHS findings suggest that the mothers are caught in a convention trap, and the sudden ends within intramarrying groups in Senegal suggest that the practice is a convention.

The survey research advocated in the article almost never measures by intramarrying group, thus it fails to detect conventional features of FGC, such as its nearly all-or-nothing presence in most such groups. The survey data do notice an association between ethnic group and presence of FGC, but otherwise measure a national sample of individuals scattered across almost as many intramarrying groups. Correlation of researcher-defined variables from national surveys is a clumsy tool of explanatory inference (and would be pointless if the author were correct that FGC is radically heterogeneous from group to group). Ethnographies, however, are often only descriptive, or at best propose idiographic explanations. The idiographic approach leads to the conclusion that FGC might be impossible to understand (p. 97). The comparative approach, however, explains observations that the idiographic approach finds puzzling.

Empirical Science versus Moral Advocacy

The article discourages generalizations about FGC (except to the extent permitted by the scientific demographic and health surveys) and encourages attention to the heterogeneity disclosed by ethnographic reports. It recommends that we investigate and understand the motivations of people who do FGC, “without dismissing others as ignorant, irrational and cruel” (p. 93). Yet, in its discourse about *advocates*, the article makes broad and extremely negative generalizations. Their discourse is not “supported by the evidence” (p. 79), which suggests that the advocates are ignorant. The advocates fail “to gain an insider’s view of those societies that practice such surgeries” (p. 89), which suggests that they are irrational, as does their “false sense of knowledge” and “sensationalizing accounts” (p. 90). Their “reactions of rejection and contempt,” their “neocolonial thinking,” and their domineering role in “ongoing political struggles about legitimacy and authority, at both the local and the global levels” (p. 90), suggest that they are cruel.

The article demands the highest standards of evidence for the uncontroversial proposition that FGC entails nontrivial complications but offers no such evidence

for its controversial propositions about the advocates. The negative characterization of them violates two further canons of science.

First, other than an assertion that “many” (pp. 89–90) sources, publications, and discussions of FGC deserve such characterization, the article presents no data of any kind concerning the distribution of views among those who prefer FGC to end. Shouldn’t we appreciate the heterogeneity of views among advocates? The term includes everyone in the world who would like for FGC to end and contains everyone from African individuals who have worked a lifetime for reform to high school students writing term papers in Peoria.

Second, there is no controlled comparison of advocates’ attitudes with respect to matched issues. To make the point that advocates are unusually in the grip of illegitimate motivations, the article would have to measure, say, American advocates’ attitudes with respect to FGC in Africa, and American advocates’ attitudes with respect to an American issue, perhaps abortion. Do all pro-choice advocates have a thorough and detailed grasp of the relevant evidence? Do they all sympathetically understand the motives of pro-lifers, and discuss them in tones of reserve and respect?

To conclude, is the article’s claim merely that public opinion has a shallow understanding of a topic distant from respondents’ experiences? Or does it intend to lodge the serious charge that the most informed and most practically involved advocates are factually and morally mistaken?

Frequency of Health Complications

The article excludes from evidence of health complications all anatomical inference, all history, all journalism, all ethnography, all policy forums, all public health reports, all clinical observations, and all personal experiences. Only academic articles in certain databases are potential evidence. Of those, only 30 are on health complications; of the 30, only eight merit consideration; of the eight studies to survive all sieves of exclusion, the rates for various complications range from zero to 22 percent, and all of those eight have problems, it says. Reasons for excluding the 22 articles include: sampling or methods not described, sample too small, single case only with no information on population, unclear whether woman had FGC, information on complications was vague, different sample sizes in study.

The eight studies include hospital cases, which cannot infer general rates of complication, surveys based on women’s reports that are limited by the accuracy of women’s reports, and controlled studies limited by the fact that they measure only hospital populations, it says. It continues that the evidence is scarce and limited because definitions and measurements are not uniform, there are few data on mortality, and an indirect measurement of mortality by demographic methods is flawed in design.⁷

The article concludes, then, that health complications “are the exception rather than the rule” (p. 92). Death from malaria is the exception rather than the rule, but no physician would put it that way to a patient about to travel to a malarial zone. A more responsible statement for the public would be that “complications are common, and can be serious.” Summarizing the DHS research, Carr states that “women commonly report cutting-related health problems” (1997:6). The DHS surveys that the article considers authoritative measured self-reported complications

in the Central African Republic, Egypt, and Eritrea. The DHS analysis extrapolated from the sample to estimate that one million women in those countries would self-report complications (Carr 1997:41), and in its summary reported that “medical problems related to genital cutting are a public health issue of some magnitude” (Carr 1997:6). For those one million women, complications would be the rule not the exception.

The article uses DHS data on self-reported complications in Egypt of 5 percent to bolster its “exception not the rule” remark (p. 92), but fails to inform the reader that the DHS (El Zanaty et al. 1996:177) warned that, “Caution must also be used in interpreting these data as representing the actual level of complications from circumcision both because it is based on subjective assessments by the women themselves rather than clinical assessments and because it is retrospective in nature,” suggesting possible undermeasurement. In a footnote, the article uses the 5 percent figure again in order to cast doubt on a more local Egyptian survey’s findings of complication rates of up to 22 percent (p. 100 n. 26). It also does not report DHS data on 19 percent self-report of complications in Eritrea (available at time of writing; p. 99) and 27 percent self-report of complications in Central African Republic (source cited in its bibliography; p. 102), also available in a document known to the author (Carr 1997:41).

The article demands more methodical evidence on the frequency of complications; here is methodical evidence demonstrably in the author’s possession that is contrary to its minimizing position, yet it is not reported. Finally, the variation in self-report of complications may be an unrecognized artifact of the limitations of the survey approach, as I shall explain below.

Inconsistent Standards

The article says that it is “telling” that merely a tenth of the works it found in its literature review contain (presumably numerical) data on prevalence or health effects (p. 81), and it thoroughly catalogs the shortcomings perceived in those few articles containing numerical data. The allegedly poor state of the evidence is blamed vaguely on the advocates at one point (p. 97), and directly on them at another:

That the evidence has been until now long on advocacy and short on empirically based research reflects the prominence of the concerns of activists in guiding the research agenda, and the disproportional allocation of resources toward “intervention studies” aimed at abolishing the practice, rather than toward scientific inquiry. [p. 85]

There is much to unpack here. First, there is an important complications objection to FGC and a more important consent objection to FGC. Given the consent objection, naturally much of the literature will dwell on normative questions and strategies of abandonment. Elsewhere, the article recognizes that “discussions of the reasons for, or significance of, the practice, and . . . ethical debates . . . occupy a major place in the literature” (p. 81).

Second, I do not defend all intervention studies (see Mackie 2000 for criticisms) and I do not disapprove of demographic surveys, but surely the question of the budgeting and allocation of research effort is an evaluative one. It is not as though there is one allocation of effort that is improperly based on values and another

allocation of effort that is properly based on facts. One person doesn't win the budget argument by calling him or herself the scientist and others the advocates; arguments are won by justifications, not by labels.

Third, the article's evaluation of FGC research priorities is idiosyncratic and opposite to that of other competent investigators in the field, whom it labels advocates (pp. 90, 98). The overview of FGC published by the World Health Organization (Toubia and Izett 1998:37–49), written about the same time as the Obermeyer article, assesses the matter differently. Its chapter on research begins with the observation that the amount of research is small compared to the scale and complexity of the problem. The state of research to date is briefly and scrupulously evaluated, and recommendations for further research are provided in detail. The most neglected area, according to the overview, is research to assist individuals and communities to abandon the practice (Toubia and Izett 1998:37), and the authors recommend that future research focus on behavioral and programmatic aspects. As for health effects, research is recommended on the contribution of the practice and its complications to the overall morbidity and mortality of women. The overview states, correctly in my view, that "although such studies are no longer necessary to justify action" (Toubia and Izett 1998:40), they may assist the decisions of policy-makers in the future.

Furthermore, if we were to use consistent standards of evidence, then it would be unscientific for the article to claim that evidence on FGC is peculiarly wanting, because, again, there are no controlled comparisons. For example, evidence on male circumcision (and on many other questions) in the United States is no more ideal than evidence on female circumcision in Africa. The American Academy of Pediatrics states that the data on prevalence of male circumcision in the United States are based on an "inadequate sample" and otherwise merely on local and clinical data, that the "true incidence of complications after newborn circumcision is unknown" (American Academy of Pediatrics 1999:686, 688). It also cites anecdotal reports of decreased sexual satisfaction. Those familiar with philosophical controversies know how fragile our concepts are to skeptical inquiry, and specialists in any empirical study know how fragile are our facts. I have proposed one relevant comparison, and I believe that similar comparisons would show that the evidence pertaining to health complications of FGC is not peculiarly wanting.

The article explains the alleged scarcity of data as due to "political, economic, and ethical factors at both the local and international levels" (p. 97). Are there alternative hypotheses, however, that would explain the shortage of *ideal* evidence on the FGC question? There are surely many. African demography developed only recently, and, of course, there are considerable fiscal and logistical barriers to carrying out research in Africa. The existence of the practice was little known to outsiders until the last 20 years; in many places, there was and is a norm of silence surrounding the topic. Even for insiders, prevalence in other ethnic groups and even in one's own ethnic group outside the local marriage market may not be well understood (the article notes that the best-informed Egyptians did not suspect that prevalence was as high as 97 percent; p. 96).

The local and international invisibility of the problem meant that few researchers were interested in the topic and there was little justification for donors to allocate funds for such research. FGC is usually carried out in the absence of meaningful consent; anatomical considerations establish that nontrivial complications are

entailed. Thus, compared to other research questions competing for resources, there is less urgency to measuring rigorously the frequency and severity of complications.

Furthermore, in practicing populations, as I shall detail in the discussion below on the limitations of survey research, there are problems in the causal attribution of harmful outcomes to FGC. Additionally, carrying out research on the form of female genitals, gynecological and obstetric complications, and sexual experience is difficult, even in liberal circumstances.⁸ There had been popular reactions to interventions in Kenya in the 1930s and the Sudan in the 1950s. There are strong norms of female modesty in some FGC areas, and natural reticence about such discussions everywhere. The article acknowledges that there is a “general change in the feasibility of carrying out research on a topic that some had believed to be unacceptable” (p. 86). It is considerations such as these that explain limitations to research on FGC, rather than political activity by advocates to discourage inquiry.

Perils of Survey Research

The article warns that respondent women may not be able to accurately distinguish among complications and may be subject to recall bias with respect to both the operation and potential complications. I want to add that there are other major problems of causal attribution that would bias toward serious underreporting of FGC complications, and these must be considered in research design.

If FGC is nearly universal within an intramarrying group, then respondents themselves have no comparison group. In the worst case, they would mistakenly consider even immediate complications as normal background, and this would bias against both recognition and recall of complications. Respondents without close knowledge of untreated women may not suspect a causal relationship between FGC and complications. Writing about complications of infibulation in Islamic Northeast Africa, Hicks observes:

Women do not even correlate subsequent physical discomfort, pain, and related gynecological and obstetric problems with having been circumcised. Such physical problems are perceived as being the common lot of women. That is because the problems are, to one degree or other, prevalent among the majority of infibulated women, they are not viewed as unusual. Logically then, neither the act of infibulation nor related sequelae (unless requiring emergency treatment) are high priority issues for women in these societies. [1996:73]

These issues were further made clear to me in an interview with Lala Baldé, president of the women’s association in the Pulaar village of Medina Cherif, region of Kolda, Senegal, in December 1998. Baldé was a leader in organizing Medina Cherif and surrounding villages to abandon FGC. She told me that the women never suspected a causal relationship between FGC and complications. When girls died, bled, or got infected at cutting, they attributed it to evil spirits; they did not know, for example, that heavy blood loss might retard child development, and gynecological complications were considered the lot of women.⁹

On hearing of the causal relationship from a source they considered credible, Baldé told me, it took them 30 minutes of discussion to decide that the causal claim was correct. They reviewed local history and suddenly realized that incidents of death, hemorrhaging, and infection were immediately associated with FGC, and they broke down and wept. One woman told me that she had her daughter who had

hemorrhaged seriously stand next to a girl of the same age who was taller by about a foot. "She's never been the same since the cutting," I was told. "Before she ran around all day and played and since she's been quiet and dull."

A survey among such women would show very different results before and after the discovery of a causal relationship (the article, however, considers schooling or contact with women's organizations a biasing factor; p. 100). An excisors' future work depends on the repute of her skills in carrying out the cutting. I learned in Senegal that excisors, those who perform FGC, often for compensation, will, sincerely or opportunistically, minimize or misattribute immediate complications, which further contributes to causal misattribution in the population.

This brings us back to variations in self-report of all FGC complications between Egypt and the Central African Republic (Carr 1997:41). In Egypt, 97 percent of women are cut (mostly Type II, excision) and 5 percent of those self-report complications, and in the Central African Republic (type unknown) 43 percent of women are cut and 27 percent of those self-report complications. I do not intend this as a piece of evidence but, rather, as an illustration of a possibility. That five times as many women report complications in the CAR than in Egypt may (or may not) be due to the circumstance that women in the Central African Republic have nonpracticing neighbors and, thus, encounter a comparison group, while women in Egypt do not have nonpracticing neighbors and, thus, no comparison group that permits causal attribution of consequences. Allam et al. (1999) interviewed about a thousand randomly selected university students in Cairo, Egypt, medical and non-medical, men and women. "Although medical students were significantly more knowledgeable than nonmedical students, less than half of them knew that hemorrhage and infection could follow female circumcision."¹⁰ If many medical students are unaware of the causal connection between FGC and immediate complications, it is likely that many more in the general population are unaware of the connection.

There seems to be a universal human reticence about discussing matters relating to human sexuality. This is exacerbated in some localities by norms of female modesty and further in some localities by powerful norms of secrecy concerning FGC and its meanings. According to Daouda Ndiaye (in Melching 1999; I have discussed this with him myself), master of the "Juju," traditional healer, and specialist in local knowledge:

In the Mandinka ethnic group Ngir-Ngiro is a "rab," a spirit which attacks women who dare to break through the silence and speak of any suffering or negative effects related to their experience with female circumcision. In fact, women are told never to discuss anything related to what they have learned or undergone during this ceremony, the consequences being quite serious: for example, mental illness, paralysis, or even death. This is why women have never dared speak about Female Circumcision, especially to someone who is not initiated. . . . The discussions in class and the public declarations helped to show women that Ngir-Ngiro doesn't exist any more and will not harm them as they once thought. Then breaking the silence allowed the women to understand the importance of ending this practice, a practice which, they learned to their surprise is the source of suffering, psychological problems and death. [Melching 1999:7]

Here, prior to sensitization, a causal relationship was not suspected by those involved in the practice and would never have been discovered, because women did not disclose to one another, let alone to local men or to outsiders, information

about the FGC and associated (though unrecognized) complications; complete silence was the powerful norm. After sensitization, and the suggestion of a causal relationship, women began to share experiences about complications, and discovered, to their horror, that what they thought were unassociated or idiosyncratic harms were associated and unacceptably prevalent. Demba Diawara, imam of Keur Simbara, a Bambara village in western Senegal, visited all the ten villages that intramarried with his to discuss the delicate issue; only someone of his high moral stature and reputation would be able to broach the subject. According to Melching (1999; I have also discussed this and related issues in depth with him), Diawara told her on his return:

I had no idea, no idea! I have heard unbelievable stories in the ten villages we are visiting—of girls suffering and women's pain during sexual relations and childbirth. The women never talked about this, never showed pain, never told us about the problems in Keur Simbara—most didn't realize this pain was linked to the practice of genital cutting. They thought all women have the same pain. [Melching 1999:53]

Diawara then helped organize a public declaration among them to abandon FGC.¹¹

Survey results would drastically undermeasure complications in locations where there is a strong norm of secrecy—the norm of secrecy is itself a secret—and would mislead, compared to other approaches. It would not do to have flawed surveys paraded as science alongside the dismissal of anatomical inferences, clinical reports, personal experiences, and other data as worthless information. Poorly designed survey research would also suffer from extreme selection bias on the question of severe complications. The survey would undercount all complications sufficiently severe to remove the woman by death from the measured population. Such selection bias also probably obscures causal attribution within practicing populations. If the survey measures only ever-married women, then those who survive but are unmarriageable due to complications also go unmeasured. Interviewing sisters or mothers about family fatalities would fail if respondents were unaware of a causal relationship between FGC and complications, which is no imaginary problem. And even if respondents were aware of causal connection, perceived culpabilities for the loss may inhibit reporting.

Survey research is not equivalent to facts.

Harms and Benefits

Next, the article maintains that assessments of benefits and harms are culturally variable and socially constructed. It cites the benefits of beauty, marriageability, sexual alteration, and group conformity. What is the evidence for such benefits? Notice again an inconsistent standard: citations to five ethnographers. If, on the question of *benefits*, we demanded the same standard of evidence that the article demands on the question of *complications*—that ideally designed and executed demographic and health surveys carried out widely across space and time confirm that FGC *inevitably* provides *incontrovertible* benefits—then we would have to conclude that its critical discourse about benefits is insufficiently supported. Ethnographers' reports contain descriptions of perceived benefits, harms, and complications; thus, if ethnographers' reports are our standard of evidence, then nontrivial complications are well established.

The article deplores generalizing theories of FGC but, inconsistently, proposes one of its own: “the ill health and the death that these practices are thought to cause are difficult to reconcile with the reality of their persistence in so many societies” (p. 91, reiterated on pp. 92, 94). That a practice would persist because it is only minimally harmful seems to be a plausible hypothesis, but on reflection it is not. Do smoking and drinking persist because they are harmless? Does war? Did footbinding persist for a millennium because it was harmless? The convention hypothesis explains how certain harmful practices like FGC and footbinding stubbornly persist, and how they suddenly end. No other evidence is offered in the article to support the minimal-harm hypothesis.

A few simple observations will indicate the implausibility of the minimal-harm explanation for the persistence of FGC. The article stresses the heterogeneity of FGC, less severe in one group, more severe in another. The minimal-harm hypothesis, then, would predict less persistence in groups with more severe FGC and more persistence in groups with less severe FGC. With the recent exception of organized abandonments in Senegal, where FGC has ended in groups regardless of severity of cutting, FGC is unusually persistent, for example, the severe practice of infibulation for 2,200 years among some Beja (Mackie 1996).

Inspection of DHS data (Carr 1997:69–74) does not suggest any relationship between severity of practice and proportion opposed to continuation of the practice. Notice that the minimal-harm hypothesis is bolstered by the author’s mistaken hypothesis that education has no effect on attitudes toward continuation of FGC (p. 89): if complications were trivial, then education would not change attitudes. We have seen already, though, that more education means more opposition in attitudes toward continuation. Finally, “women commonly report cutting-related health problems” (Carr 1997:6), contrary to the minimal-harm hypothesis.

The article suggests that in the societies where FGC is practiced, “discomfort” is more expected and more thought to be unavoidable than in societies where FGC is not practiced (p. 94). What is the evidence—demographic, ethnographic, *indisputable*—for this controversial assertion? The summary of the DHS data otherwise recommended to us in the article reports that, “A number of Egyptian, Eritrean, and Sudanese women who oppose these practices also mention that cutting is a painful personal experience” (Carr 1997:29). Anke Van der Kwaak (1992), an ethnographic source cited in the article, states that,

Many girls look forward to the day of their infibulation. Often they feel shocked afterwards, not prepared for experiencing so much pain. The girls do not easily talk about these experiences: they are brought up with the knowledge that reticence is one of the greatest virtues for a girl. [Van der Kwaak 1992:782]

The article suggests that education makes no difference in approval for the continuation of FGC, and from this it infers that the educated do not share the biomedical view of harm and the universalist view of human rights. The inference is confused: If one cannot escape from the tragic circumstance of having to suffer a harm in order to obtain a greater benefit, that does not mean that one believes there is no harm. Mothers worry and sorrow when daughters go under the knife. Waris Dirie explains why mothers consider FGC a benefit to their daughters. Dirie writes that men:

demand their wives be circumcised. The mothers comply by circumcising their daughters, for fear their daughters will have no husbands. An uncircumcised woman is regarded as dirty, oversexed, and unmarriageable. In a nomadic culture like the one I was raised in, there is no place for an unmarried woman, so mothers feel it is their duty to make sure their daughters have the best possible opportunity—much as a Western family might feel it's their duty to send their daughter to good schools. [Dirie 1998:232]

For a girl to avoid the harm of FGC brings about for her the worse harm of unmarriageability.

The article's overall case is based, it says, on "in-depth insights derived from the anthropological literature on the subject" (p. 81), but it sometimes misinterprets that literature. For example, it claims that ethnographic studies link FGC to "rites of passage and to the marking of membership in a social group such as a tribe or secret society" (p. 88) and cites Rose Oldfield Hayes (1975), John G. Kennedy (1970), and Robert A. Myers et al. (1985) as support. None of the three sources says that FGC is associated with secret societies.¹² None says that FGC is an ethnic marker (Kennedy says that it functions to promote group solidarity and identity, but that is what functionalists say about all social practices). The *major* point of Kennedy's paper is that, contrary to expectation, FGC is *not* a rite of passage, and Hayes emphasizes, confirms, and expands on Kennedy's finding that FGC is not a rite of passage in the groups they studied.¹³ Myers et al. (1985) claim that it is a rite of passage for some of the ethnic groups they surveyed, but from their description, what they mean by a rite of passage is not an initiation rite but rather a practice that is entirely oriented to marriageability.

A physician at a public health clinic in Senegal emphasized to me that the negative psychological consequences of FGC (for some individuals, not all) were not sufficiently appreciated. Toubia (1995:40) reports that there is a great deal of anecdotal evidence in support of the claim that FGC has a profound psychological impact on women and girls. Toubia and Izett (1998:31–33) say that the "cumulative evidence suggests that the [cutting] event is remembered as extremely traumatic and leaves a life-long emotional scar." The article mentions, but does not at all discuss, psychological complications.

Sexual Limitations

The assertion that FGC limits sexual experience is based on anatomical considerations and on case reports, according to the article. It is curious that the article mentions anatomical considerations with respect to sexuality, but not with respect to other health complications. Toubia, a physician originally from the Sudan, explains that, "by altering the normal anatomy of the sexual organs, FGM reduces the ease with which sexual fulfillment is achieved, or makes it extremely difficult" (1995:17).

The article (pp. 96, 101) proposes, however, that Western anatomy's finding that the genital organs are related to sexual enjoyment is merely a social construction, as is female orgasm. The case reports on sexuality, it says, are from "unusual" women who have had contact with schooling or women's organizations. Further, case reports tend to be from groups with more severe cutting, and negative effects are improperly generalized to all forms of cutting, it continues. Systematic studies

are rare, but the scant information that is available calls into question, it says, the assertion that FGC is “fundamentally antithetical to women’s sexuality and incompatible with sexual enjoyment” (p. 95), a carefully qualified statement, as we shall see.

The article relates a limited study by Lightfoot-Klein of infibulated women in the Sudan, which, if valid, indicates that 90 percent of respondents at least one time in their lives experienced an orgasmic response. This intriguing report has long been known and discussed, and the article mentions a possible explanation offered by Gruenbaum: That infibulators fearing a cut to the clitoral artery leave much of the clitoral tissue intact. A British physician who treats Somali women reports an intact clitoris under many infibulations (Shell-Duncan and Hernlund 2000:17), and there are now several other such reports.

Otherwise, the argument depends on equivocation: We are told of studies that report that a proportion of cut women “enjoy” sexual relations. Enjoyment is not equivalent to orgasmic response. Waris Dirie writes, “If you ask me today, ‘Do you enjoy sex?’ I would say not in the traditional way. I simply enjoy being physically close to Dana because I love him” (1998:227). Analogize hands to sexual organs, and dexterity to sexuality. Removal of a joint barely limits dexterity loss of a few fingers damages it, chopping off both hands severely damages it; such amputations are not *fundamentally* antithetical to dexterity but are *variably* antithetical to it. True, humans make the best of the limitations they face, a person who loses both hands finds alternatives to manipulation, and total loss of dexterity does not make life meaningless. These considerations do not justify the irreversible limitation of a human capacity without meaningful consent, however.

Finally, numerous sources indicate that FGC is widely *intended* to limit female sexual capacity, often to encourage chastity or fidelity (see also Gruenbaum 2001:133). The article, however, suggests that FGC is “not designed to obliterate sexual enjoyment,” that humans in FGC cultures have a “very different conception of the link between an intact clitoris and orgasm,” and that such a linkage is not a “physiological reality” but rather is “socially constructed” (p. 96). It offers three pieces of “ethnographic evidence” to establish these claims, but again it misinterprets the literature.

First, it recounts a story from Egypt that reduction of the clitoris balances the progress of partners in intercourse and, thereby, achieves harmonious sexual relations. This story assumes, however, that women possess greater sexual capacity than men, that such capacity can and should be reduced by cutting of the clitoris, and thus that the more intact the clitoris the greater the pleasure. “The main reason given by women is that the practice ensures attenuation of sexual desire, thus protecting the woman against her oversexed nature” (Assaad 1980, summarizing Egyptian and Sudanese ethnographies). An Egyptian man who supports the continuation of FGC states that, “Female circumcision . . . is meant to safeguard the dignity of women by checking their sexual drive and thus preserving their chastity” (Ezzat 1998).

Second, the article relates an anecdote in which an excisor in Egypt is admonished by attending women not to cut too much clitoral tissue. This anecdote again shows a belief in a connection between clitoral extent and sexual capacity, and nonanecdotal DHS data from a source listed in the article’s bibliography shows that 29 percent of Egyptian women respondents agree that cutting can lessen sexual satisfaction.¹⁴

Third, the article reports that in Nubia during infibulation there are chants of “Come, now you are a woman,” “You became a bride,” “Bring her the groom now,” and “Bring her a penis, she is ready for intercourse.” The Nubian ceremony establishes marriageability, however, not sexual capacity. The article does not report that the cited ethnography goes on to explain that the chants “serve partially to drown out the screams of the child” (Kennedy 1970:180), and that the Nubians intend infibulation to control women’s “inherently wanton character which is physiological centred on the clitoris” (Kennedy 1970:181).

Thus, the three ethnographic reports claimed as support for the social-construction hypothesis in fact contradict it, and instead support the physiological-reality hypothesis. If in some places the intent is not to *obliterate* female sexual capacity, nevertheless the intent is to *reduce* it; and if in fewer places there is no such *intent*, nevertheless the *consequence* may be such reduction.

Powerful Discourse

Amid the scientific demands for numerical data are contradictory allusions to various relativisms. The advocates are said to be engaged in a “powerful discourse” (pp. 79, 97), conventionally a phrase referring to the work of Foucault, who is cited in the article against the allegedly objectivizing gaze of the advocates (p. 90). For Foucault, there is no truth or falsehood, no right or wrong, except as arbitrarily constructed by power via discourse (Habermas 1987:266–293).

The article commends cultural relativism (pp. 79–80). Further, the “evidence . . . is not simply a collection of objective facts. It is part of ongoing political struggles about legitimacy and authority” (p. 90). Scientific assessments are inseparable from subjective judgments (p. 94), it says. The link between genital organs and sexual enjoyment may be a social construction rather than a physiological reality (p. 96). The advocates’ assessment of FGC as harmful is attributable to “political, economic, and ethical factors at both the local and international levels” (p. 97). With all this in mind, it is worrisome to read that,

From the biomedical point of view, which *powerfully* shapes the evaluation we make of interventions on the body, no matter what the exact numbers are, any pain and suffering that accompany or follow operations that are not medically prescribed are too high to justify their persistence, and even the lowest rates of complications are unacceptable. But this assessment is not universally shared. [92–93, emphasis added]

The article is scrupulous to never endorse the biomedical view, and FGC practices are said to “appear to be violations of values such as bodily integrity, health, and informed consent” (p. 89)—they appear to be violations, which is not to say that they are violations. Further, the article claims (wrongly) that education does not lead to a convergence in worldviews toward biomedicine or universal values (p. 89), suggesting that each is the arbitrary product of powerful discourse.

The frequent relativization of terms by use of quotation marks—“facts” (p. 80), “‘normal’ bodies” (p. 87), “‘healthy’ sexuality” (pp. 87, 96), “modernization” (p. 88), “harmful practices” (p. 89), “‘modern’ health care” (p. 89), “‘universal’ values” (p. 89), “knowledge” (pp. 91, 98), “‘unnecessary’ surgeries” (p. 94)—also casts doubt on the validity of the biomedical view and of universal values.

The author may well hold that facts and some moral values are capable of universal validity, but from the article it cannot be discerned how deep the relativism claims are intended to go.

Conclusion

In sum, the article shifts standards of evidence without justification, and its hypotheses are falsified by evidence from sources it otherwise accepts. Its central conclusions are stated boldly, but dissolve with simple linguistic analysis. The conclusive proof it seeks of complications is not possible in any empirical investigation, and is not appropriate for evaluation of a health and human-rights issue such as FGC.

The claim that the advocates exaggerated estimates of prevalence is contradicted by evidence from a source otherwise relied on and considered ideal, as is the claim that more educated respondents do not adopt new attitudes on harm, bodily integrity, health, and informed consent. The article's extremely negative generalizations about the advocates are unjustified. Its claim of low frequency of health complications is unsupported by the laboratory standard of evidence demanded in the article, and is contradicted by the DHS data that it otherwise accepts. Its belief that evidence on health complications of FGC is peculiarly wanting is not tested against controlled comparisons, and it fails to consider more plausible alternative hypotheses to its proposal that the allegedly poor state of the evidence should be blamed on the advocates and on unnamed forces.

It is wrong to equate survey research to the facts: Survey research on FGC may underestimate harms because of norms of secrecy and of modesty, because of causal misattributions among respondents due to the local universality of the practice, and because of selection bias. It wants to explain FGC as a benefit, not grasping that to avoid the harm of FGC brings about the worse harm of unmarriageability. It minimizes the "discomfort" (p. 94) of "female genital surgeries" without reference to any source, a claim directly contradicted by a demographic study and by an ethnographic study cited elsewhere in its text. It suggests that FGC does not limit sexual capacity, but the very sources it cites in support, on examination, demand the opposite conclusion. It may take a relativist view toward biomedicine and morality, which would not cohere with its scientism and evaluative stance.

The article argues that the "international consensus" (p. 80) to support the abandonment of female genital cuttings is based on "the conviction that they have extremely harmful consequences" (p. 80). The article concludes that the evidence that they have extremely harmful consequences is insufficient (p. 97). Thus, the author is "motivated" (p. 80) to shift "resources" (p. 85) away from "groups working against the continuation of these practices" (p. 80).

I propose an alternative account: That the international consensus and mobilization to which the article objects is based primarily on something like the understanding that FGC is an irreversible limitation of a human capacity carried out in the absence of meaningful consent, that opposition is motivated by human-rights concerns. I argue that by appropriate standards of evaluation, FGC entails nontrivial complications. The foregoing debate over complications must not distract us, however, from the more important human-rights issues pertaining to FGC and the need to orient research toward the goal of abandonment.

EDITOR'S NOTE:

The next issue of *MAQ* (17:3) will feature a rejoinder to Gerry Mackie by Carla Makhoul Obermeyer, entitled "The Health Effects of Female Circumcision: Science, Advocacy, and Standards of Evidence." The article also includes an update on recent evidence concerning health effects of female circumcision.

NOTES

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1. I use the term *complications* to describe damage to body or mind from FGC, *health complications* damage to the body except for the sexual function, *sexual complications* damage to the sexual function, and *psychological complications* damage to the mind or spirit.

2. I suffered from occupational exposures to thiram and to 2,4,5-T in the 1970s, and was active in struggles to regulate those substances, which were eventually banned.

3. Prevalence data are periodically updated at http://www.who.int/frh-whd/FGM/prevalence_rates_for_fgm.htm.

4. Conversely, "rural women, women with less than a primary education, and women who were not working for cash were more likely than other women to have at least one circumcised daughter or to plan to have their daughter(s) circumcised in the future" (El Zanaty et al. 1996:173).

5. Using the "StatCompiler" at <http://www.measuredhs.com/>, website of Macro International, publisher of the DHS data, I was able to cross-tabulate region by education for Sudan 1990. The lowest prevalence of FGC (65 percent) and the highest proportion of population with no education (79.4 percent) are found in the region Darfur, the far west frontier of Sudan. According to Gruenbaum (2001:104–105), the Fur and the Kinin in the far west do not traditionally circumcise and are considered lower-status groups by Arab-Sudanese more recently settled there.

6. To simplify exposition, I do not report in Table 1 the percentage that oppose continuation and the percentage of don't know/unsure/missing; this in no way changes qualitative conclusions. Nonresponses in Eritrea are 2 percent for secondary or better, 5 percent for elementary, and 5 percent for no education. Respondents are all female, except in Eritrea, for which I report only female responses.

7. It would be desirable to obtain the 435 articles examined by the author's team, and operational definitions of their criteria of evaluation, in order to independently replicate the exercise, but I lack the resources for such an effort. For an independent and systematic review of evidence of complications of FGC, see the study by the World Health Organization (2000).

8. In the United States, politicians have sought to block domestic survey research on sexual behavior; see Gardner and Wilcox 1993 and Miller 1995.

9. On the other side of Africa, Kennedy (1970) for Nubia and van der Kwaak (1992) for Somalia report a belief that a woman is subject to attack by evil spirits for 40 days after the cutting.

10. The authors also believe there is an education effect:

Of the 1,020 students interviewed, 734 (72 percent) supported the abolishment of female circumcision. The percentages of women and medical students who were against the custom were even higher. . . . Through further education, the beliefs about female circumcision can be changed and thus will help bring an end to this custom in Egypt and other countries. [Allam et al. 1999:1552]

11. The Bambara in Senegal say that the practice among them includes some proportion who are sealed, similar to infibulation but accomplished by vulval adhesion rather than stitching.
12. FGC is associated with secret societies in parts of Coastal Western Africa, especially Sierra Leone (Murdock 1959).
13. FGC is associated with initiation rites in parts of Coastal Western Africa, and in parts of Highland East Africa (Murdock 1959) but is not so associated elsewhere in its range across 28 countries. Furthermore, the association is not automatically an explanation.
14. El Zanaty et al. (1996:183), cited as DHS, *Egypt, 1995* by Obermeyer. There is also an education effect on this finding: no education: 18.3 percent; some primary: 25.9 percent; primary through secondary: 35.4 percent; completed secondary/higher: 48.2 percent.

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