

Forum: On the limited utility of KAP-style survey data in the practical epidemiology of AIDS



This issue of Forum presents a debate on Herbert L. Smith's, 'On the limited utility of KAP-style survey data in the practical epidemiology of AIDS, with reference to the AIDS epidemic in Chile', *Health Transition Review* 3,1,1993. His response to issues raised in this debate will appear in *Health Transition Review* 4,1,1994.

How do we secure adequate data on sexual networks and the probability of HIV transmission?

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Smith's (1993) paper is very welcome in that it stimulates a much needed debate on raising research standards in the study of sexual networks and the transmission of HIV/AIDS. Nevertheless, his paper is difficult to grapple with in that he has several targets, including perhaps population-based epidemiology itself. At least one would derive that impression from his insistence that the focus should be on the person truly at risk, defined as the one whose partner is seropositive, in contrast to advocating estimates of what fraction of persons are likely to become infected if their partners practise a certain sexual lifestyle. In contrast, we are told that what should be employed is 'practical epidemiology', which seems little more than traditional (and praiseworthy) public health and medical practice devoted to patients, those they might infect, and other who might be infected by those from whom they received the disease.

This comment will focus on three issues: the possibility of undertaking better population-based epidemiology in order to understand the epidemic; the extent to which the approach advocated by Smith can be said to lead to an understanding of the epidemic; and the public-health value of population-based research.

(1) The criticism of many KAP-style HIV/AIDS studies is justified but Smith does not make it clear whether he is attacking poor-quality research or maintaining that population-based inquiries in this area will never secure adequate data or will secure the wrong data.

Attacking the national AIDS surveys funded by WHO's Global Programme in AIDS (GPA) at this stage is setting up a strawman to fight. These first surveys were carried out rather quickly, often by national statistical offices or health departments, with for instance, little of the intense experimentation and supervision that laid the foundations for the World Fertility Surveys (WFS) and on matters more sensitive and without the decades of prior study that fertility enjoyed, and hence had no chance of success. The Togo data on men's numbers of extramarital sexual partners were implausible in a society where men are believed not to be able to go for long without sex and where long post-partum female

sexual abstinence and spousal residential separation was traditionally practised (Kumekpor 1975), and were not compatible with the findings of more intense studies in the same region (Caldwell 1993). The findings of the Lesotho survey were greeted, when first released, with scepticism by local medical practitioners and social scientists who were present at the meeting.

But intense person-to-person discussions, with some similarities to the 44 interviews in Chile that Smith found satisfactory, have almost certainly succeeded in securing reasonably accurate estimates of the numbers of different sexual partners of respondents in the Ekiti District of Nigeria (Orubuloye, Caldwell and Caldwell 1991) and elsewhere in Nigeria and Ghana (Caldwell 1993). As the respondents were mostly health persons, the substitution of nurses for social scientists would probably have jeopardized the work. Going one step further, a study in Ondo Town, Nigeria, probably achieved a reasonably accurate description of men's sexual partners (Orubuloye, Caldwell and Caldwell 1993). West Africa is one of the least difficult parts of the world to secure such information, but the GPA is also now funding successful small-scale projects in other parts of sub-Saharan Africa.

These data are needed for epidemiological modelling of the epidemic, but they are not sufficient in themselves. To model the sexual networks it is necessary to be able to identify partners' partners, at least to know their number and preferably some of their characteristics. Even the Ondo Town study failed to elicit high quality information of this type from the male respondents, not because of resistance but because most did not know with any certainty about the sexual activities of their partners. One could seek out the respondents' partners and treat them in turn as respondents (noting carefully how many cite the original respondents as being among their partners). This process could be repeated several times, but once would probably suffice for model-building provided that there was an accurate identification of the number of partners (perhaps adjusted by some factor derived from whether the first respondent was mentioned or not) and of whether the respondent belonged to a group with a high risk of AIDS: in Africa, commercial-sex workers, long-distance truck drivers, those suffering from other sexually-transmitted diseases (STDs), or those living in areas with a high proportion of HIV-infected persons. This would reveal the dangerous links in the chains and perhaps the extent to which networks overlap or have common links. The real problems are ethical (Caldwell, Orubuloye and Caldwell forthcoming 1994). Even in fairly permissive societies, there are real problems in locating a person – perhaps married or perhaps single and living with their parents – and proceeding to interview them on the basis that they have been named as a sexual contact by someone else (or alternatively giving misleading information about how they were selected). Furthermore, where a high proportion are commercial-sex workers, then there are difficulties because it will probably prove to be impossible for them to identify many of their partners by names and addresses.

(2) Smith (1993) advocates the use by HIV/AIDS research of 'traditional methods of control of sexually-transmitted diseases, such as contact tracing and partner notification, with their focus on actual social and sexual networks ...' (page 13). Are these really traditional and continuing methods in Chile and elsewhere? Should one, or can one, turn up at the door of every named sexual contact and demand to know their contacts? Does one also enforce an HIV test and then proceed to find the next wave of partners only for those persons seropositive? If the chain of infection is close to being fortuitous – as is implied by Smith's comment that the chance of infection is only one per thousand sexual acts (true only for normal heterosexual activity and even then when other STDs are not present and when the reference is to the man's chance of being infected) – then we may not get a very good picture of sexual networks or of the level of sexual activity in the society. Furthermore, the trail may become cold every time it reaches a commercial-sex worker – probably a frequent occurrence – who does not know the names and addresses of most of her or his clients. But how well does contact tracing work? In Australia it works well in the case of diseases with no implications of socially-unacceptable behaviour and with a short

latency period before manifest signs are visible, as was historically the case with smallpox. But the usual approach to STDs now is to suggest to the infected person that he or she tell all their partners or send them to the health personnel involved to be told. Some African programs report that such advice brings them into contact with any partners of infected persons in only about five per cent of cases, and doubtless contact with all partners is much below this level. Smith states that, 'most infected patients interviewed claimed to be in favour of notifying partners, but generally preferred to notify their stable partners themselves; notwithstanding the fact they had heretofore failed to do so' (page 11). In many cases this is probably a politeness response. In many cases, too, this form of tracing is more likely to reveal the possible future path of infection than the past one. We are told that 'many had information on casual partners that would presumably facilitate their contact by health officials' (page 11). The fact that neither the researchers nor the health officials have employed this method is probably testimony to both a reluctance to carry out such tracing (where interviewing would present difficulties of an utterly different order than the clinic-based interviewing of persons who had voluntarily submitted themselves for HIV testing), and a belief that the information supplied would allow only a fraction of each network to be identified. This approach's greatest value has been underscored by Smith. It allows the identification of high-risk geographical areas where people indulge in risky behaviour rather than the geographical areas where they live, and it provides a more certain identification of the afflicted person's characteristics and sexual preferences than forms often cursorily filled in by health staff (an aim of all good research interviewing). It has generally been concluded that the long latency period of HIV/AIDS, with its implication that the seropositive person may have been infected years before by some now forgotten partner precludes contact tracing for this disease. The only successful attempts reported in the United States were in two states previously unaffected where the infecting contact was more easily identified. There is also another problem. When carrying out contact tracing for STDs, US contact tracers arrived with the message that they had reason to believe that the persons contacted might have been infected and that a test and treatment could be arranged. The emphasis was all on concern for the person's health and then finding other people who could be helped. This is very different from contacting someone to announce that they might have a fatal and untreatable disease.

(3) Population-based survey *cum* anthropological research is needed to build models of the epidemic, to allow public-health personnel to gauge the potential danger, and to provide data for informational services and the media which may help to bring about behavioural changes. What is important here is sexual behaviour: numbers of partners over time, types of partner (how many in the high-risk groups), type of sexuality (basically heterosexuality, homosexuality or bisexuality), extent to which condoms or other forms of safe sex are used, STD status and history, and intravenous drug use. In order to model sexual networks, we also need to map them out to at least second-order partners; to know how many sexual partners a person has and how many partners each partner has. Some research on higher order partners would be social scientifically illuminating but the modellers need no more.

The fact that some surveys attempt to get their information by 'mirror samples' and others focus in on alcohol and viewing pornographic films are also strawmen – examples of bad research do not disprove the need for good research in the same area (although the level of alcohol consumption is in many societies a good indicator of the likelihood of men practising unsafe sex with unsafe partners). Certainly, any good research should go further and relate the level of sexual networking to the level of HIV/AIDS in the area, and our exploring of sexual networks probably should try to take into account – if respondents are truthful – where they 'hang out', especially when they are likely to indulge in high-risk sexual behaviour.

Only good data on sexual networking can show how fast an AIDS epidemic is likely to move through a population and whether it is likely to expand or contract, and, short of the mass use of an

effective vaccine, they are likely to prove the best weapon for containing the epidemic. In large areas of sub-Saharan Africa only good information on the extent of sexual networking, and on the frequency with which high-risk individuals are found in those networks, will provide governments with enough convincing information and the media with sufficiently newsworthy inputs, to disturb the population to the point where behaviour changes – where men have fewer partners and take fewer risks, where women regard the situation as so serious that they pressure their husbands and sons in this direction, and where, if they fail to change their husband's behaviour, they insist on safe sex within the relationship or break it up.

Surveys, surveillance, and the measurement of risk: a response to Herbert Smith

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In his recent paper Smith challenges the utility of surveys on knowledge, attitudes, and practices related to HIV transmission (Smith 1993). Smith has made two fundamental errors in his perception of the motivation for carrying out KAP surveys. First, he assumes that such surveys are meant to document the modes of transmission occurring in the surveyed population. Secondly, he assumes that epidemiologists use surveys as research tools to draw causal conclusions about risk factors and disease. Neither of these two assumptions is true.

Public-health surveillance is defined as the ongoing, systematic collection, analysis and interpretation of data on specific health events for use in the planning, implementation and evaluation of public-health programs (Thacker 1988). This concept has been broadened in recent years to watching over indicators of risk, as well as actual disease incidence (*Morbidity and Mortality Weekly Report* 1992). The continuity of the process is as important as the validity of the measures. Surveillance systems do not seek to identify individuals at risk, to provide aetiological data on the chain of causation, or precisely to measure disease dynamics. For instance, the surveillance of most infectious diseases captures information on only about ten to 30 per cent of the actual cases that occur. These are not selected by any formal sampling technique. The data are not representative, but a considerable body of data shows that the results of surveillance still provide good information about such important aspects of disease dynamics as age distribution, geographic distribution, and temporal trends (Langmuir 1963).

Smith's assumption about validity is curious to the epidemiologist and the health educator. He points out that 'The one-shot survey process is not well suited to obtaining valid measures of intimate behaviour' (Smith 1993:5). One cannot challenge this statement, but I am not aware that the proponents of KAP surveys have laid such a claim. KAP surveys may not be strictly valid, but when they are repeated with similar survey instruments and similar samples over time, they measure the changes in the population's knowledge and perceptions about HIV risk factors. If these trends are not in the desirable direction, then the political and technical leaders of HIV control programs must modify their messages. In the absence of such data, program managers are flying without instruments.

Surveillance is classically done not by 'investigators' but by governmental officials with responsibility for disease-control programs. They take their information about causal relationships from

the general research literature. They then decide what indices are practical to monitor over time. These indices are then placed under surveillance. Indices are selected more for the relative ease and low cost of measuring them than for their causal proximity to the disease under consideration.

Thus, for example, public-health officials follow tax revenues from tobacco sales because the data are readily accessible, whereas individuals' actual consumption of cigarettes is more difficult to measure. Actual determination of the population subgroups that are actively smoking might be still more difficult (Pierce et al. 1989; Lopez 1992). Monitoring changes in tax revenues in no way implies that tax revenues are the cause of lung cancer or coronary disease. Placing the population's knowledge, attitudes and practices related to HIV transmission under surveillance does not imply that the specific behaviours being monitored are either necessary or sufficient for transmission.

Smith's assumption that KAP surveys are meant to document the modes of transmission occurring in the surveyed population at the time of the survey, or accurately describe the epidemiology of the infection, shows a misunderstanding of the role of surveillance in epidemiology. Epidemiologists do not use KAP-style surveys to make inferences about the current prevalence of HIV infection, or to predict the future course of an epidemic, as Smith states. The World Health Organization advocates KAP surveys to help design the content of health-education programs in developing nations. If a high proportion of the population does not know what condoms are, then the health education message needs to explain them. If a high proportion recognizes what they are, but doesn't believe in their effectiveness, a different health education message is necessary. The main utility of KAP surveys is to guide these decisions about the content of HIV control efforts. Good KAP surveys thus play a central role in the planning and evaluation of health-education programs.

Epidemiologists do not expect KAP surveys, or indeed any cross-sectional survey, to have great use in understanding the epidemiology of HIV infection in a given population. This is because, as Smith eloquently points out, surveys are limited by sampling design techniques, and are not well adapted to ferreting out the actual behavioural practices that predispose to, let alone cause, any individual infection. Once transmission patterns in a given population have been identified, however, KAP surveys of the general population help monitor the prevalence of such practices in the general population. The exact prevalence estimate may be inaccurate, but increase or decreases in knowledge will be detected if similar instruments and similar sampling strategies are used. These changes can guide the evolution and content of health education measures.

Smith alleges that 'Most KAP-style operational constructions of risk behaviours posit an epidemiology of AIDS based on weak or irrelevant correlations' (Smith 1993:9). I believe that the public-health workers who manage HIV prevention programs and carry out KAP surveys do not posit any such relationship. KAP surveys are performed to help estimate the direction of changes in knowledge, attitudes, and behaviour over time. Whether or not these behaviours directly lead to HIV infection is not of paramount importance for use of the survey results. Smith, quoting Anderson and May, points out that, '[t]he most important factors in survey work are assessment of changes through time and at different spatial locations' (Anderson and May 1988:517 cited in Smith 1993:9). Note that there is no direct assumption of the importance of any particular behaviour in the causal chain.

Smith erroneously assumes that epidemiologists use surveys as research tools to draw causal conclusions about risk factors and disease. KAP surveys are not aetiologic research tools. They are valuable parts of a program of surveillance of behavioural practices in the general population. Perhaps Smith's fundamental problem lies with the concept of surveillance itself. Carefully conducted research studies, which can document the temporal course of events, are vital to define the importance of individual risk factors. Once these studies are done, surveillance of the prevalence of such behavioural practices becomes useful. Most epidemiologists insist upon well-performed cohort studies or case-

control studies to provide the causal link between a behavioural risk factor and a disease or condition (Rothman 1986:10-16). Fortunately such studies abound in the HIV literature, and show strong evidence of the now well-known behaviours that facilitate HIV transmission. KAP surveys did not become a major part of the HIV control agenda until after the major transmission modes for the virus were well documented in a large number of concordant cohort and case-control studies.

Smith is concerned about the 'failure of the investigators to think hard about the relationships between the behaviours they are measuring and the disease phenomenon they are studying' (Smith 1993:5-6). He confuses aetiological research with surveillance, and confuses the concept of risk with the concept of cause. Once he has done so, it is easy for him to criticize KAP surveys, since they certainly do not identify causes of transmission of HIV. Smith is concerned that measuring KAP in the general population does not identify the risk behaviours of people who are actually transmitting HIV. He points out that the HIV status of a sexual partner is much more important than the number of partners in determining risk. There is no practical or ethical way that any government can determine the HIV status of all members of its population, and then target educational messages to them alone. As long as educational messages are to be addressed to the whole population, then the whole population's general knowledge, attitudes, and practices should be monitored to provide guidance for and evaluation of the effort. Special efforts to target individuals such as in counselling sessions at an HIV clinic, may be tailored to the individual's needs. These efforts certainly should be guided by more sophisticated information than that gathered by a KAP survey.

International health, and particularly work on the health transition, attracts workers from a wide variety of fields. These workers may have distorted views of each others' disciplines and methods. Researchers and disease-control program managers may also have problems understanding each other. I believe that Smith has misinterpreted the methods of epidemiology, and the motivations of HIV prevention programs. *Health Transition Review* has rendered an important service by opening this debate to a wide audience of scholars and public-health activists.

Data collection strategies in the study of behaviours: limited use of simple solutions to a complex problem

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Several issues are of interest in considering the utility of various behavioural data collection methods in relation to HIV/AIDS. These can be formulated as follows:

- What behavioural research is useful for designing immediate interventions *targeted* at specific situations and/or to mobilize existing capacities to implement interventions?
- What behavioural research is useful for planning *wide-scale* public-health programs?
- What behavioural research and data are useful in making inferences on the current size and future course of the HIV epidemic?

- What behavioural research is useful in monitoring and evaluating progress and achievements either by small-scale interventions or large-scale programs
- In addition, what insights can be gained on the validity and reliability of various behavioural research methods and what are their implications for use in the pursuit of different objectives?

Smith touches upon some of these questions with what appears to be a random logic and attempts to provide some answers, but mixes up the issues. Smith's starting point relates to the first question. He focuses on situations where the epidemic is at an early stage and where, everyone would agree, the onus is to design and to implement interventions to slow the spread of HIV. The conclusion (p.13) that 'unfocused inquiry into diffuse behaviours in undifferentiated populations is not a productive line of research ... to design ... intervention' is fairly obvious in relation to such an objective, especially with the use of three disqualifying adjectives.

The discussion, however, is blurred on several accounts by the confusion of generic data-collection methods, namely the KABP surveys can be and have been used in different populations (not necessarily 'undifferentiated'). However, the choice of a method or a combination of methods depends on objectives; quantitative surveys usually complement qualitative studies without these being mutually exclusive.

A sense of history

In the early 1980s, in response to the Hepatitis B and AIDS epidemics, there was an upsurge of research on sexuality. However, the bulk of this work was devoted to populations such as homosexual men perceived to be at 'high risk' for contracting or transmitting infections. Missing from this research agenda were studies of the sexual behaviour of populations thought not to play a major role in HIV transmission. In response to that concern, and with the increasing evidence that heterosexual transmission of HIV was growing rapidly, the Global Programme on AIDS of WHO launched in 1988 the KABP and Partner Relations Surveys (WHO/GPA 1988) with the overall purpose of providing basic descriptive information about the knowledge, attitudes and the sexual behaviour of the general population as it relates to the risk of HIV/AIDS both within and across societies and cultures. More specific objectives of these surveys can be classified under three headings: a) to establish baselines for successive monitoring of reported behavioural changes and effectiveness of general information campaign in improving knowledge about transmission and prevention and in encouraging more tolerant attitudes towards those already infected; b) to assess ways in which the risks related to HIV infection are understood and perceived and assist in the design of general public prevention strategies; and c) to provide information which would be of value for advocacy to policy makers beyond the health sector, such as educational authorities.

These small-size surveys, generally around two to three thousands adult respondents, were *not* designed to obtain data on the frequency of, for example, homosexual behaviour or covert behaviours in the population at large; this would have required much larger sample sizes or a different method. Nor was it ever intended to use KABP data to predict the future course of the epidemic. As Smith appropriately points out, deterministic prediction of the future course of the AIDS epidemic would require not only estimates of the incidence and prevalence of specific types of risk behaviours in specific groups and estimation of their sizes but also more information about sexual networks and group mixing. As a parallel step in GPA research agenda, other survey instruments were developed, targeted at specific high-risk situations, such as homosexual and bisexual behaviour, prostitutes and clients and injecting drug users (WHO/GPA 1989–91). It was recognized that by their very nature

survey instruments are limited in their ability to conceptualize or situate research findings. Even the most carefully designed surveys offer only limited insights into the complex range of social and cultural meanings that may be associated with particular behaviours. In turning to the central goal of translating research findings into effective interventions, questions related to context and meaning are often centrally important.

In recognition of these limitations inherent to surveys, protocols for the qualitative study of both sexual culture and the culture of health, illness and disease were developed in 1990 (WHO/GPA 1990) and such work continues in the areas of evaluation and sociobehavioural determinants of HIV transmission.

Running behind the epidemic

Smith argues for a study of 'risk groups' derived from an indepth inquiry of people with AIDS/HIV, but his attempts to describe the characteristics and behaviours of persons with HIV/AIDS remain vague. Studies of homosexual men, injecting drug users and women prostitutes have shown that if the virus is present in some communities and the behaviours that transmit infection are sufficiently common and intense, HIV can become epidemic within specific populations and further spread into the 'wider general' population. For example, estimation of HIV seroprevalence among injecting drug users seeking treatment in Bangkok increased from zero per cent in 1985–86 to 16 per cent in 1988 and to 40–60 per cent in 1992 (WHO/GPA 1991 and WHO/GPA 1993). In Trinidad and Tobago, among persons with AIDS attributable to sexual transmission, the proportion linked to heterosexual contact increased from zero per cent in 1983–84 to 47 per cent in 1987 (WHO/GPA 1991).

Soon after the beginning of the AIDS epidemic, increasing concern was expressed about the accuracy and representativeness of the number of reported AIDS cases. AIDS patients having contracted HIV infection through stigmatized behaviours are likely to avoid notification. Moreover, the reported number of AIDS cases is only representative of infections contracted several years before and therefore merely reflects a picture of the past. In such a situation, 'snowball' investigations based on reported AIDS cases, mandatory HIV testing and contact tracing, as suggested by Smith (p.10), further increase the false impression that HIV is confined to particular groups of people, at a time when the epidemic may have spread to other populations. Some large urban areas of Brazil, for example, experienced epidemics that first affected homosexual and bisexual men, then emerged among injecting drug users and the female partners of these groups, and finally spread among non-injecting heterosexuals. Haiti, Honduras, Mexico and Thailand provide other examples of epidemiological patterns where the spread is rapid in urban areas, far beyond the first groups identified by 'case-reporting'. To rely on identified AIDS cases clearly bears the danger of looking backwards rather than addressing the current and, possibly, the future situation.

Biomedical risk and behaviour

Smith reserves some of his strongest criticisms of large surveys for their poor measurement of the risk of HIV infection. It is obviously true that sexual intercourse, of whatever nature and scale, carries no risk to an individual if all partners are free from infection. However, the purpose of surveys is not to assess the risk of HIV infection for individuals but to measure current or potential future risk at an aggregate level. There is abundant epidemiological evidence that, once the epidemic has spread beyond special groups in the ways outlined above, the rate of change of sexual partners and use of condoms are powerful indirect markers of HIV-infection rates. KABP-type surveys attempt to measure these and other similar aspects of behaviour. Such information is valuable for the design of public information campaigns directed towards different segments of the population and for the monitoring of subsequent change.

The relationship between diet, exercise and heart disease may be a helpful analogy. The statistical links are well established, but there is evidence that genetic factors are also critically important determinants of risk for an individual. In the future, it may be possible to identify individuals who are not at risk of heart disease, regardless of their dietary and other behaviours. In the meantime, it is entirely proper for public-health campaigns to advocate a balanced diet and exercise for all. Investigations of these behaviours among the general population play a valuable role in guiding and evaluating such campaigns.. Similarly, with sexual behaviour and HIV infection, it is entirely proper for public-information programs to attempt to reduce general-risk factors, even though the risk of infection to individuals with the same sexual behaviour will depend on a host of other factors and thus will vary greatly. An important priority in sexual-behaviour research is to assess the quality of data and to develop better measuring techniques, as has already been somewhat achieved for surveys of food intake and diet.

HIV prevention relies on non-discriminatory approaches

The importance of the ethical and public-health issues linked with the disclosure of identity of individuals infected with HIV is easily forgotten even if lip service is paid to such issues. The systematic survey of HIV-testing sites as proposed in Smith's paper (p.12) is based on records of all HIV-positive persons at the Public Health Institute in Santiago from a large network of hospitals, STD clinics etc. and also a penitentiary 'where testing is consequent on suspected homosexuality'. Documenting the spread of the epidemic over time and in different populations is easily achieved by using the unlinked anonymous testing method to estimate the HIV seroprevalence among 'sentinel' population groups that are selected for their accessibility over time (usually those who attend health services) and their potential vulnerability. For surveillance purposes, at an early stage of the epidemic it is therefore 'practical' to test blood from 'high-risk populations' attending various clinics, but those detected (anonymously!) as HIV infected represent only a small proportion of the total number of HIV infections and any strategy relying on their identification would be of very limited use. For effective prevention strategies to be implemented, those most at risk need to be accessed and not driven underground.

Indeed Smith's proposals on control measures are rather confusing. 'Traditional methods of control of STD, such as contact tracing and partner notification (p.13)' as advocated may only be of some value for curable diseases. The history of syphilis is illustrative of such a misguided approach. Before penicillin was available, the failure of syphilis control is attributable, at least partly, to the finger-pointing approach. Case finding and other militaristic approaches may succeed in disease control if there is a treatment for that disease. While there is certainly a role for voluntary testing and counselling for 'a reasonable intervention program to be targeted at these *individuals* (with AIDS who remain sexually active) and their partners' (p.13), this role is likely to be marginal in checking the general spread of HIV. Because there is a lifelong potential for transmission and because HIV is sexually transmitted and nevertheless a possibility of protection from infection, the main challenge for prevention at *population* level is to act *before* the HIV test becomes positive and not after.

Brief comments

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I would want to add only a footnote to Smith's excellent article. From my own experience as a consultant to the World Health Organization, Global Programme on AIDS, I think that a lot more could be said about why KAP-style surveys are used. WHO staff have recognized that such surveys tell only part of the story. Cara'l et al. (1991:S65) have written that 'By themselves, the survey data [i.e. WHO's KAP surveys], removed as they are from their social and cultural context, cannot furnish satisfactory explanations of patterns of sexual behaviour.' This, they continue, can only be supplied by mainly anthropological data which do not yet exist.

The problem is that WHO relies on such unsatisfactory data and frankly does not use what else there is. To explain this Alice in Wonderland situation we need, I think, to ask a number of searching questions about the sociology of those who prepare the KAP surveys. WHO is always poor mouthing, saying they could do more with more money. But this is only part of the story. In bodies like WHO, where there is still much suspicion of anthropology and similar soft qualitative disciplines, the method of data collection chosen, the data collected and their interpretation are determined to a large extent by the dominant medical quantitative subcultures.

The way forward may require a radically different approach in the handling of data. After all, our colleagues in the historical and legal disciplines, long ago devised rigorous methodologies to evaluate very dubious materials (Pitt 1972, 1982). In this way the researcher becomes rather like a judge in a court of law weighing up all the evidence from whatever source. Hopefully this might avoid some of the blunders that are likely from the KAP crowd until such time that we have the richer ethnography.

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