Chapter 25

R.C. Lewontin[1]

SEX, LIES, AND SOCIAL SCIENCE
(a book review with subsequent correspondence)

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The Social Organization of Sexuality: Sexual Practices in the United States
by Edward O. Laumann, John H, Gagnon, Robert T. Michael, and Stuart Michaels.
University of Chicago Press, 718 pp. $49.95

. . .

THE FAMOUS STUDIES BY Alfred Kinsey and his collaborators in the 1940s and 1950s which have become part of everyday reference as 'The Kinsey Report,' the later research by Masters and Johnson, and the more popularly read work of Shere Hite,[2] are part of a long history of the science of 'sexology.' . . . The latest try at knowing who does what to whom, and how often, is the National Opinion Research Center's *The Social Organization of Sexuality* . . .

The problem for every sample survey is to know whether the answers are systematically untrue. Surveyed populations can lie in two ways. They can answer untruthfully, or they can fail to answer at all. This latter problem is known in the trade as 'non-response bias.' No matter how hard one tries, a significant portion of the sample that has been chosen will fail to respond, whether deliberately, through accident, lack of interest, or by force of circumstance.

1 Professor Lewontin notes that the material shown here is excerpted from a much longer article on methodological problems in sample surveys.

2 A.C. Kinsey, W.B. Pomeroy, and C.E. Martin, *Sexual Behavior in the Human Male* (Saunders, 1948); A.C. Kinsey, W.B. Pomeroy, C.E. Martin and P.H. Gebhard, *Sexual Behavior in the Human Female* (Saunders, 1953); W.H. Masters and V.E. Johnson, *Human Sexual Response* (Little Brown, 1966); S. Hite, *The Hite Report on Female Sexuality* (Knopf, 1979) and *The Hite Report on Male Sexuality* (Knopf, 1981).

It is almost always the case that those who do not respond are a non-random sample of those who are asked. Sometimes the problem is bad design. If you want to know how many women work outside the home you will not try to find out from a telephone survey that makes calls to people at home between nine AM and six PM. Much of the expertise of sample survey designers is precisely in knowing how to avoid such mistakes. The real problem is what to do about people who deliberately avoid answering the very questions you want to ask. Are people who refuse to cooperate with sex surveys more prudish than others, and therefore more conservative than the population at large in their practices? Or are they more outrageous, yet sensitive to social disapprobation? Because they do not answer, and self-report is the only tool available, one can never know how serious the nonresponse bias may be. The best that can be done is to try to minimize the size of the non-responding population by nagging, reasoning, and bribing. The NHSLS team tried all these approaches and finally got a response of 79 percent (3,432 households) after repeated visits, telephone calls, videotapes, and bribes ranging from $10 to an occasional $100. The result was that there were now three sample populations, those who were cooperative from the start, those who were reluctant but finally gave in, and those who refused to the end.

From an analysis of the eager and the reluctant it was concluded that for most questions there was no difference between the two, but that still leaves in the air the unanswerable question about the sex lives of those who found $100 an insufficient payment for their true confessions. If I can believe even half of what I read in *The Social Organization of Sexuality*, my own sex life is conventional to the point of being old-fashioned and I wouldn't have cooperated for any price the NORC was likely to find in its budget.

Finally, we cannot avoid the main question, whether those who did respond, reluctantly or eagerly, told the truth. Far from avoiding the issue, the study team came back to this central question over and over, but their mode of answering it threatens the claim of sociology to be a science. At the outset they give the game away.

> *In the absence of any means to validate directly the data collected in a survey of sexual behavior*, these analyses assess data quality by checking for bias in the realized sample that might result from potential respondents' unwillingness to participate because of the subject matter, as well as by comparing results with other surveys. In every case, the results have greatly exceeded our expectations of what would be possible. They have gone a long way toward allaying our own concerns and skepticism. . . . [emphasis added].

In other words, people must be telling the truth because other people have said it before and they say the same thing even if reluctant to answer. That many people at many times have independently claimed to have been present at Satanic rituals or seen Our Lady descend at Fatima, and that some of these witnesses have been reluctant to testify at first, will presumably convince Professor Laumann and his colleagues of the reality of those events.

Again and again the problems of how we elicit the truth when both conscious and unconscious distortions may be suspected are dealt with disingenuously. Men and women were interviewed by women and men indiscriminately, and there was no attempt to match race of interviewer and race of the respondent.

> Will men and women respondents be affected in similar or different ways [by this mixing of sexes of interviewer and respondent]? Will people who have engaged in socially disapproved activities (e.g., same-gender sex, anal sex, prostitution, or extramarital sex relations) be equally likely to tell this to a male as to a female interviewer? At present, these questions remain unresolved empirically. . . . Although this issue is certainly important, . . . we did not expect the effect of gender matching to be especially large or substantively noteworthy. The experience and belief among NORC survey research professionals was that the quality of the interviewer was important but that it was not necessarily linked to gender or race.

In other words, they don't know and hope the problem will go away. While sex and race are 'master status' variables, 'organizing the pattern of social relationships,' apparently being interviewed about your sex life is not part of social relationships. Instead of investigating the problem, the team 'concentrated our time and money on recruiting and training the best interviewers we could find.' That meant three days of a 'large-scale' training session in Chicago.

Anyway, why should anyone lie on a questionnaire that was answered in a face-to-face interview with a total stranger? After all, complete confidentiality was observed. It is frightening to think that social science is in the hands of professionals who are so deaf to human nuance that they believe that people do not lie to themselves about the most freighted aspects of their own lives and that they have no interest in manipulating the impression that strangers have of them. Only such deafness can account for their acceptance, without the academic equivalent of a snicker, of the result of a NORC survey reporting that 45 percent of men between the ages of eighty and eighty-four still have sex with a partner.

It is not that the research team is totally unaware of sensitivities. In addition to about a hundred face-to-face interview questions, respondents were asked to fill out four short printed forms that were placed by them in sealed 'privacy' envelopes for later evaluation by someone other than the interviewer. Many of the questions were repetitions of questions asked in the personal interviews, following the common practice of checking on accuracy by asking the same question twice in different ways. Two matters were asked about, however, that were considered so jarring to the American psyche that the information was elicited only on the written forms: masturbation and total household income. Laumann et al. are not so deaf to American anxieties as it seemed.

There is, in fact, one way that the truth of the answers on a sex survey can be checked for internal consistency. A moment's reflection makes it clear that, discounting homosexual partners, the average number of sex partners reported by men must be equal to the average number reported by women. This is a variant on the economist Robert Solow's observation that the only law in economics is that the number of sales must be equal to the number of purchases. Yet, in the NHSLS study; and other studies like it, men report many more partners than women, roughly 75 percent more during the most recent five years of their lives. The reaction of the authors to this discrepancy is startling. They list 'in no particular order' seven possible explanations including that American men are having lots of sex out of the country, or that a few women are having hundreds of partners (prostitutes are probably underrepresented in an address sample, but prostitution was not regarded as a 'master status' variable to be inquired about since presumably it is not a 'basic concept of self-identity'). Our authors then say,

> We have not attempted to reconcile how much of the discrepancy that we observe can be explained by each of these seven logical possibilities, but we conjecture that the largest portion of the discrepancy rests with explanation 6.

Explanation 6 is that 'Either men may exaggerate or women may understate.' So, in the single case where one can actually test the truth, the investigators themselves think it most likely that people are telling themselves and others enormous lies. If one takes the authors at their word, it would seem futile to take seriously the other results of the study. The report that 5.3 percent of conventional Protestants, 3.3 percent of fundamentalists, 2.8 percent of Catholics, and 10.7 percent of the non-religious have ever had a same-sex partner may show the effect of religion on practice or it may be nothing but hypocrisy. What is billed as a study of 'Sexual Practices in the United States' is, after all, a study of an indissoluble jumble of practices, attitudes, personal myths and posturing.

The social scientist is in a difficult, if not impossible position. On the one hand there is the temptation to see all of society as one's autobiography writ large, surely not the path to general truth. On the other, there is the attempt to be general and objective by pretending that one knows nothing about the experience of being human, forcing the investigator to pretend that people usually know and tell the truth about important issues, when we all know from our own lives how impossible that is. How, then, can there be a 'social science'? The answer, surely, is to be less ambitious and stop trying to make sociology into a natural science although it is, indeed, the study of natural objects. There are some things in the world that we will never know and many that we will never know exactly. Each domain of phenomena has its characteristic grain of knowability. Biology is not physics, because organisms are such complex physical objects, and sociology is not biology because human societies are made by self-conscious organisms. By pretending to a kind of knowledge that it cannot achieve, social science can only engender the scorn of natural scientists and the cynicism of humanists.

To the Editors:

We are puzzled by the review of our book, *The Social Organization of Sexuality* [*NYR*, April 20], because it is professionally incompetent and motivated by such an evident animus against the social sciences in general. . . .

The central premise of Lewontin's review is that people routinely and pervasively lie about sexual behavior – indeed, it would seem all aspects of their lives – and thus none of the data from our survey of 3,432 people can be taken seriously. But Lewontin relates no systematic empirical information to substantiate his claim. Rather, he relies on a set of rhetorical devices that tendentiously advance his assertions.

Lewontin opens the review with an argument based on a false analogy. He discusses at length the problems of credibility in autobiographical statements and then asserts the analogical equivalence of autobiography and the self-reports given in response to our questions. The reader by now is supposed to be thinking, 'I certainly would not tell anybody that I had sex with my spouse last night while clutching a yellow rubber ducky. I'd lie – at least about the rubber ducky.' But autobiography, by definition, involves the public disclosure of the identity of the person. This sets in train all the motivations to create a favorable self-image in the minds of others and perhaps some of the outcomes Lewontin asserts. In contrast, we went to great lengths to guarantee the privacy, confidentiality and anonymity of our respondents' answers as well as to provide a strong rationale for an individual to be candid and honest with us. We spent a great deal of time worrying about how we could check the reliability and honesty of our respondents' answers. While we readily admit that we were not always successful in securing full disclosure, his false analogy simply misses the point altogether.

Lewontin's next move is to provide an instance demonstrating the data's invalidity by discussing the large discrepancy between the average numbers of partners reported by men and women and the logical impossibility of such a situation assuming that they are recruiting their partners from a common pool. In the 52-page chapter devoted to the numbers of sex partners, we explicitly discuss (on p. 174) the undesirability of using averages (means) to summarize the central tendencies of distributions as skewed and narrowly concentrated (with long, unevenly distributed tails) as these are. In addition, we explore in considerable detail the reasons for this discrepancy. Lewontin argues that if we could not get this 'simple fact' right, it is evidence that all else is spurious. Error is a problem in all observations, how it is dealt with and its public recognition is the test of science. His decision to rest his case on this single issue without reference to its context forces us to conclude that he willfully misrepresented our analysis. . . .

Finally, we have Lewontin's discussion of our finding that 45 percent of men between the ages of 80 and 84 claim to have sex partners. He chuckles at our credulity in reporting such patent nonsense, being just one more instance of our hopeless gullibility of believing everything we are told by our respondents. Now this is a rather nice instance of his tendentious and misleading use of our data to support his central claim that everybody is lying about their sex lives. The survey in question, the General Social Survey (GSS), is a widely known, high-quality, regularly conducted survey that professionally knowledgeable people rely on for estimating social trends of various sorts. It is sponsored by the National Science Foundation and has been subjected to regular scientific peer review for some twenty years. To the professional social scientist, it is well known to be a household-based sample that excludes the *institutionalized* parts of the population. Any number of census and other highly regarded survey studies have also noted that, due to differential mortality and other factors, older women are progressively more likely to be living alone, By age 70, about 70 percent of women report, in the GSS, no sex partners in the past year. Older men, in contrast, are far more likely to be living with someone – the sex ratio is increasingly in their favor so far as the surplus of older women to older men is concerned. It is therefore not at all surprising that noninstitutionalized men in their eighties – presumably healthy enough to be living on their own – would have a fair chance of reporting that they have a sex partner. We discuss at length in the book the different meanings of sexuality across age, time and social circumstance. We believe the answers are hardly likely to be crazed lies by sex-starved octogenarians who are posturing like teenagers for the edification of credulous social scientists.

The review is a pastiche of ill-informed personal opinion that makes unfounded claims of relevant scientific authority and expertise. Readers of *The New York Review of Books* deserve better.

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To the Editors:
In the course of Richard Lewontin's brilliant essay 'Sex, Lies, and Social Science' he remarks that if the study he reviewed is typical of American scientific sociology, then this discipline must be in 'deep trouble.' That's putting it mildly, American sociology has become a refuge for the academically challenged. Some universities have closed their sociology departments; many have decided the discipline merits little new money.

Yet mere stupidity cannot explain the analytic weaknesses of studies like the NORC sexuality project; nor do social scientists so very gainfully employed in such shops simply misunderstand the scientific enterprise. The difficulties with this research, like the larger troubles of sociology, are political. . . .

However, if Lewontin's exposé is just, he uses a meat cleaver where a scalpel would have served him better. Is quantifying social phenomena an inherent evil, as at points in his essay he seems to suggest? Lewontin surely wouldn't deny that the Census Bureau provides useful and necessary information. In principle, survey research has its uses, in revealing how people think about themselves. (I found it both interesting and cheering that 45 percent of men between the ages of 80 and 84 in the NORC study reported still having sex with a partner, even if the aged have confused fantasy with fact.) Method *per se* isn't the issue.

I wish Lewontin had put his attack in a larger historical context. From its origins in Social Darwinism and the Progressive movement, American sociology has struggled with the contrary claims of those afflicted with physics envy and researchers – whether deploying numbers or words – more engaged in the dilemmas of society. In that struggle, midwestern Protestant mandarins of positivist science often came into conflict with East Coast Jews who in turn wrestled with their own Marxist commitments; great quantitative researchers from abroad, like Paul Lazarsfeld at

Columbia, sought to disrupt the complacency of native bean counters. In the last twenty years, more interesting 'hard' sociological research has been done in medical, planning, and law schools, and better research on culture and society in the humanities departments, than in sociology departments. The *intellectual* enterprise of sociology is hardly represented by the dumbed-down study Lewontin rips apart.

What places like NORC command, like other reactionary enterprises, is money. To defend themselves, the minions of these institutions will undoubtedly attack Lewontin for being anti-empirical, which will miss exactly his point, that their brand of science represses trenchant social evidence. My word is that this repression is more than an academic evil. Sociology in its dumbed-down condition is emblematic of a society that doesn't want to know too much about itself.

Richard Sennett

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R.C. Lewontin *replies*:

It should come as no surprise to the readers of *The New York Review* that the authors of *The Social Organization of Sexuality* did not like what I wrote. . . .

Our authors touch on the central methodological issue. It is their view that, although people may lie or exaggerate in autobiographies because they are trying to create a public persona, they will tell the truth in anonymous interviews; because there is no motivation to manipulate the impression that strangers have of us. Is it really true that quantitative sociologists are so divorced from introspection and so insensitive to social interactions that they take such a naive view of human behavior? . . .

First, Professor Laumann, people do not tell *themselves* the truth about their own lives. The need to create a satisfying narrative out of an inconsistent and often irrational and disappointing jumble of feelings and events leads each of us to write and rewrite our autobiographies inside our own heads, irrespective of whether anyone else is every privy to the story. Second, these stories, which we then mistake for the truth, become the basis for further conscious manipulation and manufacture when we have exchanges with other human beings. If the investigators at NORC really do not care what strangers think of them, then they are possessed of an insouciance and hauteur otherwise unknown in Western society. It is precisely in the interaction with strangers who are not part of their social network, and who will never intersect their lives again, that people feel most free to embroider their life stories, because they will never be caught out.

Laumann et al. try to minimize the impact of the observed discrepancy in the number of sexual partners reported by men and by women. There is an attempt at obfuscation in a remark by Laumann and his colleagues about averages not containing as much information as more detailed frequency descriptions. True, but irrelevant, because in their data men consistently report more partners across the entire frequency distribution. Anyway, Laumann et al. do not deny the discrepancy. Indeed it is they who brought it up and discussed it in the book, and it is they, not I, who offered as the most likely explanation that men 'exaggerate' and women 'minimize' their sexual promiscuity. Then they try to discount the impact of the discrepancy on the study as a whole. After all, it is just one false note, and we cannot expect perfection. People may lie or fantasize about how many sexual partners they have, but we can take everything else they say at face value.

But this neatly ignores the fact that this comparison provides the *only* internal check on consistency that the study allows. I nowhere claimed that 'all else is spurious,' but rather that we are left in the unfortunate position of not knowing what is true when our only test fails. . . .

While Laumann and his colleagues believe that men exaggerate while they are aged between eighteen and fifty-nine, they (backed by the peer review panels of the National Science Foundation) seem to have complete confidence in the frankness of octogenarians. Perhaps, as men contemplate their impending mortality, the dread of something after death makes lying about sex seem risky. We must, however, at least consider the alternative that affirming one's continued sexual prowess in great age is a form of whistling in the dark.

I have considerable sympathy for the position in which sociologists find themselves. They are asking about the most complex and difficult phenomena in the most complex and recalcitrant organisms, without that liberty to manipulate their objects of study which is enjoyed by natural scientists. . . .

Richard Sennett . . . is, of course, right when he insists that quantitative information is important in sociology. Data on birth, death, immigration, marriage, divorce, social class, neighborhood, causes of mortality and morbidity, occupations, wage rates, and many other variables are indispensable for sociological investigations. My 'meat cleaver' was never meant to sever those limbs from the body of knowledge. But it does not follow that collecting statistics, especially survey statistics with their utter ambiguity of interpretation, is sociology. . . . numbers can have no interpretation in themselves without a coherent narrative of social life. . . . Like it or not, there are a lot of questions that cannot be answered, and even more that cannot be answered exactly. There is nothing shameful in that admission.

To the Editors:

Professor Lewontin lobs grenades . . . with deadly effects on some of the fatter targets of social science method. Indeed, uncollaborated survey reports about sexual activity and other sensitive matters do deserve limited credence. Consequently our ignorance about private behavior is much greater than social scientists like to pretend. I do hope but do not expect that social scientists will add this book review to their reading lists in quantitative methods.

On a much smaller target, Professor Lewontin's aim is very slightly awry. Based on 1-to-1 mapping argument, he states that the average number of heterosexual partners of females should equal the average number for males. Well, actually not. One reason lies in the fact that members of the present cohort can have partners from earlier or later cohorts. As an artificial example, suppose there were equal numbers of males and females with equal life expectancy, each taking a single partner for life, but males mated with older females. Then because of young females not yet partnered, males would have a higher average ratio than females. (In fact, American males do report on average that they lose their virginity at lower ages than females.) Moreover, there are several other confounding influences: there are more females than males in the adult cohorts, females outlive males, and because of population growth newer cohorts are larger than older cohorts. Given that men claim 1.75 as many sex partners as women claim, Professor Lewontin and the books under review are probably correct to infer a severe reporting bias, but rigorous proof awaits a detailed quantitative argument.

And on a target of a middling size. I believe Professor Lewontin is too pessimistic about future possibilities for obtaining reasonably well-founded information about human behavior in private. There are many promising improvements in survey methods (admittedly, rather costly ones) that we have barely begun to try. Thus, while there is evidence that surveys of long term recollections are of limited value, diary and especially snapshot approaches are better (e.g. because they limit opportunities for self-deception). Also, we can sometimes gather data from multiple observers of a single private event (e.g. interviewing both sex partners separately). And we can set up experimental situations designed to bias responses one way or the other, (e.g. using an apparently opinionated interviewer) and see how far the responses can be manipulated.

At the same time we can develop more inside checks on survey data, like the sex partner ratio example discussed above. More usefully, we can develop outside checks, for example calibrated models that work back and forth between micro data about private behavior (e.g. unprotected intercourse) and observable data such as public consequences (e.g. births, abortions, and AIDS cases) or experimentally testable rates (e.g. conceptions per acts of unprotected intercourse). An existing practical example is the comparison of market survey data with eventual sales outcomes. . . . If and when we finally do find out how to ask the questions in ways that make the survey data consistent with the available public data, then I believe we will have a reasonable warrant to rely on the survey data.

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R.C. Lewontin *replies:*

. . . Dr. Burress offers some suggestions for checking on the validity of survey responses, but they do not seem to help us. The idea that diaries will somehow reflect the truth of peoples' lives is extraordinary. Are diaries not meant for other eyes? Remember the Tolstoys who left their diaries open on each other's bedside tables. Even when diaries are only a form of talking to oneself, one may engage in an elaborate composition of a self-justificatory autobiography, much of it unconscious. Can he really demonstrate that diaries or even snapshots 'limit opportunities for self-deception.' Who took the picture and why? To what extent are our family records of smiling children and indulgent parents in the Piazza San Marco part of our construction of a wished for life? Burress does not tell us how the records of births, abortions, and AIDS can do more than tell us that some claims of virginity are not to be credited. It is important to distinguish acts that are public or leave public traces from those for which nothing but self-report is available. So, we know that people over-report church attendance because one can actually count the house, and nutritional surveys are notorious for their unreliability because it has been possible to paw through garbage to find out what people really eat. But these examples raise the question of why it is worthwhile to do a sample survey in the first place, if the information can be obtained by direct observation. . . .

To the Editors:

Both Sennett and Lewontin focus on one particular piece of data reported by the NORC investigators as evidence for their accusations. This observation is that 45 percent of men between the ages of 80 and 85 report having sex with their partner. Both Sennett and Lewontin feel this is so obviously untrue that it calls into question the validity of the entire survey and the reports that were drawn from it.

Why do they think it is so obvious that this is a lie? Neither of them offers a single shred of empirical evidence that would support their doubt. They are in fact operating on the same unfortunate negative stereotype of aging that far too many Americans still hold – that aging is a period of sexlessness, silence and social irrelevance. In particular, sexuality in elderly men is viewed as either absent or, if present, with disgust as embodied in the phrase 'dirty old man.'

And yet part of the miracle of the dramatic increase in life expectancy that developed countries have witnessed in this century is that for many people old age is a much healthier condition than many of us could ever imagine. The 80- to 85-year-olds surveyed excluded those in institutions and therefore selected the healthiest of elderly men. Furthermore because of increased life expectancy of women compared to men at that age there are two to three times as many women as men, and active men are very much in demand. If Professors Lewontin or Sennett had chosen to heed their own admonitions and seek empirical support for their claims they might have checked the medical literature on sexual activity in the elderly. If they had done so they would have found that in one study of noninstitutionalized elderly men over 65 the prevalence of sexual activity was 73.8 percent in married men and 31.1 percent in unmarried men. Studies done at Duke University showed that 75 to 85 percent of men in their sixties and seventies maintained a continuing interest in sex. And an additional study of male veterans found, that even men in their nineties maintained sexual interest. Intercourse frequency declined from monthly in men in their sixties to less frequently but at least once a year for men in their seventies and older. And in up to 15 percent of elderly men followed longitudinally there was an increased level of sexual interest and activity at a certain point in old age such as after recovery from the grieving period of widowerhood. A recent study of 202 healthy upper middle-class men and women living in a residential retirement facility between the ages of 80 and 102 with a mean age of 86 found that 53 percent of the men had a sexual partner.

I hope that scholars who call for an alertness to problems of validity and accuracy in social science would consider their own biases before using unsupported stereotypes to criticize such a major piece of work as the NORC study.

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Richard Lewontin *replies:*

. . . My criticism of the NORC study certainly does not focus on the report of sexual activity by octogenarians, nor did I claim that it was sufficient to 'call into question the validity of the entire survey and the reports that were drawn from it.' [Dr. Cassel] has confused this issue with my discussion of the internal contradiction between the reports of men and women respondents. The data on old men was not part of the NORC study, but the result of one of their previous surveys, and it was mentioned because it illustrated the inconsistent standards of the NORC team, who claimed on the one hand that men between 18 and 65 exaggerated their sexual contacts, but, on the other, accepted the self-reports of 80 year olds.

Indeed, the empirical evidence that men between 80 and 85 lie about their sexual exploits is that younger men do. Or does Dr. Cassel share with Laumann et al. the view that only the young exaggerate? Nowhere in what I (or Sennett) wrote is there a single word that even suggests that aging is a 'period of silence and social irrelevance,' nor, for that matter, is the belief that old men exaggerate their sexual activity a claim for their 'sexlessness.' Dr. Cassel's citation of various studies from the 'medical literature' only illustrates again the lack of methodological care that characterizes the field. Unless, unknown to the rest of us, medical science has produced an electronic scanner or a blood test that will give an objective read-out of how many sex partners a man has had in the last year, studies like those cited by Cassel are just self-reports, offering nothing different than the NORC survey except smaller sample sizes, and less survey expertise. Calling it 'medical literature' is only a bit of propaganda meant to lend 'an air of verisimilitude to a bald and otherwise unconvincing narrative.' . . .