is as critical, where criticism is justified, as one can expect a lawyer in his field of practice to be. Perhaps a law school teacher, divorced from the necessity of pleasing the FCC, the networks, AFTRA, and all the other powers that be, could be more biting; but how would he master the vast amount of unreported data?

* Member of the Illinois Bar.

George Overton*


When the first volume of this series appeared, Dr. Kinsey received deserved praise for a monumental accomplishment in the face of great technical difficulties, and for having brought, for the first time, the scientific discussion of our sexual mores to the community itself. The reviewers took great care lest their technical criticism, which was severe at points, overshadow their respect for this basic achievement.1

But this is the second volume and it is many years later; it seems proper, therefore, to explore how much Kinsey has improved his apparatus of inquiry and thereby the significance of his findings. Normally such a question could be left to the statisticians and sociologists. But Kinsey's own aspirations do not permit such limitation. He has directed his book at the community at large, its opinion leaders and its law givers.2 This makes it desirable that they all, along with the findings, have some briefing on the reliability of the apparatus from which these findings emerged. The briefing, in this case, will entail some sharp criticism. But it would poorly serve its purpose were it to obscure this reviewer's deep respect for a scientific endeavor of the first magnitude.

Although only the next volume will deal specifically with the legal aspects of sexual behavior, Kinsey suggests that his present findings should lead to a reconsideration of our sex mores and sex laws.3 We will look, in turn, at the three types of data which could conceivably affect our notions as to what the sex mores and laws of the community ought to be: the sheer frequency of certain types of behavior in the community or in those of its strata which shape our mores; secondly, the evidence of specific effects of such behavior; and finally, data on the community's attitudes towards such behavior.

The present second volume, on the human female, follows essentially the structure of the first, dealing in turn with each of the sexual activities from pre-

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2 "It is for this reason... that our first volume... was taken out of the hands of those who claimed the exclusive right to knowledge in this area and made a part of the thinking of millions of persons.... [W]e are under obligations to make the results of our investigations available to all who can read and understand and utilize our data." P. 11.

3 C. I, especially pp. 8-21.
adolescence to widowhood. But this time the data from 5,940 interviews are even more deeply imbedded in important collateral facts gathered from many fields. This review will limit itself to the methods of collecting and analyzing the interview data and will concentrate on the relationship between these methodological aspects and the inferences which author and reader draw from these data.

It appears that Kinsey decided to disregard much of the criticism that became available to him after the first volume appeared. His female sample is, if anything, more oddly assorted than the male sample was. And in the analysis and presentation of his data, Kinsey has failed to take some of the care which data from such an irregular sample require. But also in other respects the overall impression emerges that Kinsey's methods of analysis of his interviewing data lag behind the powerful conceptual and theoretical framework which these data are designed to support.

The first major criticism of Kinsey's sample referred to the possibility that the sex histories of the people who submitted to interviewing might differ from those of the people who did not submit. One way of meeting this criticism would have been at least to try for a true random sample. But Kinsey states: "It should be obvious that a considerable proportion of the persons selected for study . . . would simply refuse. . . ." Perhaps, but the argument would have been neater were it based on some statistics showing this proportion on a trial run. This would also have permitted a comparison of the characteristics of those who did and those who did not cooperate. If this road was indeed closed, there was still another one open for dispelling the suspicion of bias. Kinsey's present sample consists of people who showed different degrees of "voluntary" cooperation. Some simply volunteered, others came around under the influence of loyalty to the group chosen for interviewing, and some became available only after considerable persuasion. It should be simple to compare within comparable groups the sex histories of these three types of interviewees. If the volunteers would not differ from the ones who needed persuasion, such evidence would go a long way to alleviate our fears that those who refused differed from those who cooperated. It is somewhat puzzling to find nowhere a reference to an experiment which Kinsey himself helped to design. A. H. Maslow and J. M. Sakoda, who initiated it [Volunteer-Error in the Kinsey Study, 47 J. of Abn. and Soc. Psych. 259–62 (1952)], found that the Kinsey volunteer interviewees differed significantly in self-esteem (measured by a standardized test score) from those who refused to volunteer; and an older study by Maslow (which Kinsey does not cite) indicated higher self-esteem to be related to less conventional sex behavior; thus a strong prima facie case has been established for the existence of a volunteer bias.

The second criticism of Kinsey's sample concerns its odd composition. The following synopsis compares the Kinsey sample in this second volume with the comparable figures from the U.S. Census (which Kinsey does not give us):

4 P. 25. 5 Pp. 32–34.
No statement derived from this total sample can claim to report accurately on the behavior of the human female. Take for example the following finding: "By age forty, 19 per cent of the females in the total sample had some [homosexual experience]."\(^6\) A re-computation of this percentage for the population of all American women (instead of only those represented in Kinsey's sample) puts it in the neighborhood of about 10 or 11 per cent, because those in the lower economic strata—which are hardly at all represented in the sample—have considerably less homosexual experience than the upper classes. It would appear that in judging the sex mores of the American female, it should make a difference, whether one out of five women have had some homosexual experience or only one out of ten.

One might object, of course, that the clause "in the total sample," carefully injected in all such statements, protected the reader against such misjudgment. To be sure, the statistically learned reader is protected. He knows the clause means: odd sample ahead! And by careful re-analysis of the sample he will avoid too distorted an impression. But even the statistician is unable to make a clear and simple statement as to what population this 19 per cent refers. Yet the average reader certainly remains unprotected, as is shown by the following significant episode: Simultaneously with the publication of Kinsey's book on the human female, a series of articles appeared in the popular magazines which

\(^6\)P. 453.
(this was Kinsey's condition) were checked by his staff as to the accuracy of the reported statistics. One of them was an article in the September 1953 issue of McCall's. There Kinsey's statement about the "19 per cent in the sample" is transformed into: "As for homosexuality, Dr. Kinsey found that 19% of the females had such an experience." The phrase "in the sample" had disappeared, and understandably so. To a good editor the phrase must have appeared redundant. But had the phrase been kept, it would still have been misleading, since elsewhere in this article Kinsey's sample had been described as "5,950 more or less representative with regard to age, education, marital status, geographical location, occupational background, and religion." And although this too is an inaccurate statement, one is inclined to excuse McCall's writer and Kinsey's own checkers: both succumbed to the psychologically correct, if factually wrong, interpretation.

The point is simply this: unless one is interested only in the 5,940 women in Kinsey's sample, which of course nobody is (or would anybody suggest, for instance, that the law be changed to conform with the experience of the 5,940 women?), then the reader must be able to project the found percentages against some kind of meaningful population. Indeed, what on earth is a measurement from a total sample good for, unless it is "more or less representative" of some meaningful population? The "in the sample" clause is therefore, at best, comparable to the small print in a contract. It fails to protect the general reader whose intelligence is guided not by the statistical vagaries of an oddly assorted sample, but by his general understanding of the situation.

But even as odd a sample as this can provide meaningful data on those population strata which are adequately represented in it. The data showing, for instance, that among the devoutly religious women only 45 per cent ever masturbate as compared with 58 per cent among the religiously less active women, that homosexual experience is more prevalent in higher economic brackets than in lower ones, and so forth, are important documentations. But the significance of such comparisons of subgroups with each other depends on the exact relationship of each classification with every other. An example will illustrate this: Kinsey reports, in addition to the figures cited above, that the accumulated incidence of masturbation among females who had been brought up on farms was 38 per cent as compared with 51 per cent for women who had been reared elsewhere. By comparing these figures with the ones from the two religious groups, the question must arise as to whether the low farm percentage is due to the fact that there are percentage-wise more religious people on farms or whether it is something about farm life which makes a girl forget about masturbation. Clearly quite different conclusions could emerge, depending on which is the correct interpretation. But without analyzing all four combinations separately (the religiously inactive women from farms, the religiously active women from farms, etc.), the correct answer cannot be derived.

7 From Table 35, p. 187.  
8 P. 459.  
9 P. 153.
Similar spurious relationships may exist between religion and education, between education and decade of birth (generation), etc. In other words, we need to know not only how many college-educated women there are in the total sample and in each age group, but also how many there are in each religious group, in each decade of birth, etc. In short, for a proper appraisal of most of these data, an exact picture of the complete interrelationship of all these variables in the sample is required. Strangely enough Kinsey made a start in this direction by showing the proportion of each subgroup for every age level of his sample; one wonders why he did not go on.

A special question is raised by one of Kinsey's most impressive and consistent findings to which we have already referred: among the devoutly religious women, of whatever denomination, fewer appear to have any of the "forbidden" experiences than among religiously less active women. That this is not due to a reduced overall sexual activity is suggested by the lack of such a difference in respect to marital coitus. But the difference holds true, interestingly enough, also for nocturnal dreams resulting in orgasms, a finding somewhat at variance with our conception of the relationship between sexual tabus and dreams. Strangely, however, once a devout woman does start to have such dreams, their frequency does not differ from that reported by less religious women. Kinsey himself is puzzled. "It is difficult to understand why a religious background which has kept a female from dreaming of sex for some period of years does not continue to influence her after she has begun to have sex dreams."10

For some reason, Kinsey never wonders whether religious women could have been more hesitant in reporting what, by their own standards, must appear as misdeeds. Yet such a relationship between tabu and truthfulness in reporting has been established in many other investigations. Even Kinsey himself ran into some of it: In one of his tests of reporting reliability, he found discrepancies between women and men, in respect to their pre-marital coitus experience: the women showing consistently lower figures. Kinsey thinks one of the factors responsible for this discrepancy was that "the females may have covered up in reporting their pre-marital experience, or the male may have exaggerated." And he adds "[B]ut it is our judgment that the female record is more often an understatement of . . . fact."11 The reason for this discrepancy in reporting is suggested by the difference in attitudes towards pre-marital coitus, which Kinsey summarizes in another section of the book as follows: "Intention to avoid coitus in female (among virgins): 80%; in male: almost none."12 Why should this same relationship between tabu and underreporting not hold true in the case of religiously devout women? Kinsey might, therefore, have cared to investigate whether the reporting reliability among devout and religiously inactive women is equal on those activities which are frowned upon by religious doctrine.

Among the most important of Kinsey's exploits are his investigations of

10 P. 205.
11 P. 79.
12 P. 332.
specific effects that result from certain sexual behavior. Here, Kinsey found valuable data, but he is not always prepared to pursue their full significance. Consider, for instance, his figures on the effect of pre-marital coitus. Kinsey’s records show “that there was a marked, positive correlation between experience in orgasm obtained from pre-marital coitus, and the capacity to reach orgasm after marriage.” The ambiguity of such correlations is properly noted: they “may have depended on selective factors, or ... on causal relationship. ... The females who had abstained before marriage may have been the psychologically less responsive individuals who, therefore, were the ones who had most often remained chaste, both before and after marriage.”

After citing some of the findings of learning theory, Kinsey concludes somewhat reluctantly at another point that “[i]n general, it seems probable that selective factors are more often responsible,” but that they “could not have accounted for the whole of these correlations.”

He warns that “where there are long years of abstinence and restraint before marriage, acquired inhibitions may do such damage to the capacity to respond, that it may take ... years to get rid of them after marriage, if indeed they are ever dissipated.” And in the summarizing appraisal of the social desirability of pre-marital coitus he suggests that we consider “the effects of abstinence or of pre-marital experience on the ultimate success of marriage” leaving little doubt as to which side of the ledger each ought to belong.

Again, this reviewer at least is inclined to share Kinsey’s convictions; but how do his own data support him? Following is a simplified version of Kinsey’s Table 109 which contains the pertinent data:

### Percentage of Married Women Reaching Orgasm in Marital Coitus at Least 9 out of 10 Times—Classified by Pre-Marital Sexual Experience and Length of Marriage

<table>
<thead>
<tr>
<th></th>
<th>Had No Pre-Marital Coitus</th>
<th>Had Pre-Marital Coitus</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>(1) No Orgasm from Any Source</td>
<td>(2) Had Orgasm from Other Sources</td>
</tr>
<tr>
<td>In 1st Year of Marriage</td>
<td>29</td>
<td>44</td>
</tr>
<tr>
<td>In 5th Year of Marriage</td>
<td>31</td>
<td>49</td>
</tr>
<tr>
<td>In 10th Year of Marriage</td>
<td>34</td>
<td>49</td>
</tr>
</tbody>
</table>

*This Table is to be read as follows: 29 per cent of the women who had no pre-marital coitus and no pre-marital orgasm from any source experienced orgasm in the 1st year of marriage at least 9 times out of 10 marital coitus, etc., the remaining 71 per cent experienced it less than 9 times out of 10. Hence, a higher per cent figure in this table indicates a higher success in marital coitus.

Comparing the first and third columns, we note that among the women who had no orgasm before marriage those who had had pre-marital coitus show, if anything, a lower rate of marital orgasm in any year of marriage than the women who had no such coitus. Comparing the second and fourth columns, we note that among the women who had pre-marital orgasm from other sources than coitus, again those who had had pre-marital coitus show a lower rate of marital orgasm in any year of marriage than those who had no such coitus. Thus, on the evidence of these data, it would appear that pre-marital coitus—unless orgasm is reached—has a negative effect on the success of marital coitus. Yet the chances for not reaching orgasm in pre-marital coitus, if the Kinsey sample may be taken as a rough indication, are only about 3 out of 10. On the other hand, the marital success rate of those who had no pre-marital coitus, but had orgasm from other sources (column 2), is only slightly below the rate of those women who had orgasm from pre-marital coitus (column 5). These data suggest, therefore, that if there be indeed a causal relationship between pre- and post-marital orgasm, it may be safer to reach orgasm before marriage through other sources than coitus.

But it seems that Kinsey's own data could carry the analysis one step further, if one were to divide the women in each column into those who had and those who had not tried one of the pre-marital experiences (other than coitus) that can lead to orgasm. We could then see whether the post-marital success rate is more dependent on not having tried to reach orgasm, or not having been able to reach it. But it is also possible that this entire relationship is a spurious one: that not orgasm itself is the true correlate to post-marital success, but rather other characteristics of pre-marital coitus, such as its frequency, its regularity and probably most important, its having or not having been part of a love relationship.

At this point, Kinsey's self-imposed limitation, to analyze only the physiological aspects of love, bares both its strength and its weakness. Its strength lies in the adoption of a basic, unambiguous and quantifiable criterion (orgasm) which permits rigorous investigations of the sort we have just discussed. This is distinct progress over the maze of unverified theorizing which dominates so much of the discussion of sexual behavior.

But unless this precise and quantifiable criterion is analyzed freely in relation to all factors which exert primary influence on its frequency—it is deprived of its main significance. It is quite probable, for instance, that the effect of pre-marital coitus on the success rate in marital coitus is primarily determined by factors which are excluded from Kinsey's investigation: the psychological context of the physiological act. Our argument, thus, does not only not object to the role assigned to the physiological criterion, but suggests that its very significance might be enhanced by a less rigid limitation of its analysis.

Somewhere else Kinsey suggests another effect of pre-marital coitus. In commenting on the co-existence of low pre-marital coitus and high homosexual ex-
perience in the higher educational brackets, Kinsey says: "We are inclined to believe that moral restraint on pre-marital heterosexual activity is the most important single factor contributing to the development of a homosexual history...."

Again, it would seem that one could do more than "be inclined to believe." Why not refine the correlation between pre-marital coitus and homosexual experience by looking at these correlations separately for women who showed and those who did not show such moral restraint?

Kinsey also fails to confront his hypothesis with another one of his findings, which appears to contradict it: although the generation born after 1900 shows an acceptance of pre-marital coitus that is two to three times as great as that of the generation born before 1900\(^2\)--there is no evidence that homosexuality has declined in the younger generation.\(^2\) Here again, Kinsey's interpretation is somewhat ahead (if this be the direction) of his data. But, at times, he simply disregards them.

When he asked women for their reasons against pre-marital coitus the following tabulation was obtained:\(^2\)

<table>
<thead>
<tr>
<th>FACTORS Restricting Pre-Marital Coitus</th>
<th>Percentages</th>
</tr>
</thead>
<tbody>
<tr>
<td>Moral objections</td>
<td>80%</td>
</tr>
<tr>
<td>Sexual unresponsiveness</td>
<td>32%</td>
</tr>
<tr>
<td>Fear of pregnancy</td>
<td>21%</td>
</tr>
<tr>
<td>Fear of public opinion</td>
<td>20%</td>
</tr>
<tr>
<td>Lack of opportunity</td>
<td>14%</td>
</tr>
<tr>
<td>Fear of venereal disease</td>
<td>5%</td>
</tr>
<tr>
<td>All Respondents</td>
<td>100%*</td>
</tr>
</tbody>
</table>

\(*\) Percentages add to over 100 because a respondent may give more than one reason.

One page later Kinsey summarizes: "Taking all of our experience into account, we are inclined to list, in order of importance, the following as the primary factors which had limited the pre-marital activity of the females in the sample: (1) The sexual unresponsiveness of many younger females, (2) The moral tradition of our American culture, (3) Lack of experience and the individual's fear of engaging in an unfamiliar activity." This reversal of the data rank order is explained by merely stating that "in some cases these [factors] appeared to be nothing more than rationalizations of the real reasons."\(^2\) This is skimpy evidence. But if we trust Kinsey's judgment, we must again conclude that the conceptual level and the analytical tools of his statistical investigation are not adequate to the complexities of his insights.

\(^{18}\)  P. 460.

\(^{19}\)  P. 331.

\(^{20}\)  P. 461.

\(^{21}\)  Fig. 58, p. 315.

\(^{22}\)  P. 316.
How, then, is one to appraise the power of Kinsey's argument with those who shape our mores and our laws? He will easily convince the people whose own notions are sympathetic to his findings and who trust his judgment. For most of Kinsey's conclusions are probably not too far off, and the intuitions and observations, not now supported by sufficient evidence, may all yet be confirmed. But in his overall working plans, Kinsey perhaps did not allow sufficient time for the perfection of the one tool which, in the long run, could also convince the skeptics: the rigorous and exhaustive quantitative analysis of his data.

HANS ZEISEL*

* Professor of Law and Sociology, University of Chicago.