Letters

Draft Lottery: Validity of Randomness

In "Randomization and social affairs: The 1970 draft lottery" (22 Jan., p. 255) Fienberg presents conclusions as to the unfairness of the lottery which seem to be supported by the evidence. But I am disturbed by his discussion of the problem of randomness. He states in his paper, "In statistics the word random is used to describe an idealized process by which numbers (or data) are generated." The key word in this definition is *process*, yet Fienberg consistently treats his discussion of randomness as if randomness is a result rather than a method of achieving that result.

Consider the results of a hypothetical draft lottery in which there is a perfect correlation between the days of the year and the numbers drawn (1 January is number 1, 2 January is number 2, and so forth). It is possible that this result could occur even if the lottery was performed by random selection techniques and the results appear systematic. This is what all of the statistical tests performed by Fienberg show, namely, the results obtained were *unlikely* to have occurred as a result of random procedures. They in no way, by themselves, demonstrate nonrandomness.

Fienberg states that previously "no formal statistical analysis was carried out to determine whether the selection procedure was indeed fair." This statement is a meaningless one since randomness (in a statistical sense) is a method and not a result. It is not possible to determine randomness by statistical analyses of the results obtained. All one can do is examine the procedures used in the lottery and then examine the data to see if biases which might be predicted from the procedures used do, in fact, occur in the series obtained. This point is one stated by Fienberg. "The methods that are used to achieve a random draft lottery sequence can indicate the nonrandomness that can then be found by post hoc examination." Even in this statement, however, there is an apparent confusion between randomness as a result and randomness as a procedure. Fienberg does state in his conclusion that randomization *procedures* were inadequate, thus softening the above criticism.

Further, it seems that Fienberg engages in statistical overkill. The data at hand are 366 dates and 366 corresponding ranks. The four analyses all utilize the same data. The first analysis is a rank order correlation on all the data, the second collapses this into a 12×3 table, the third compares the mean lottery numbers by month and the fourth considers the linear rank-order correlation for mean lottery numbers by months. The impression given is that each of these tests adds some certainty to the conclusions reached. Actually, it only indicates that the data have not changed between subsequent analyses.

In conclusion, Fienberg notes that the 1971 lottery was performed under fair conditions (computer randomization) but, in addition, a drawing was conducted in order to "give the lottery face validity and to appeal to the public's sense of what is random." I submit that this is what Fienberg has done in his article. He has attempted to use statistical arguments in order to add to the face validity of his argument and has consistently appealed to the "public" sense of what is random. This is unfortunate since I feel that the conclusion reached in the article is probably correct.

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Throughout my article on the 1970 draft lottery, I assume, as the statistician almost always assumes, that it is possible to draw valid inferences from a sample to the procedure by which the sample is generated. Thus, when I refer to the randomness or nonrandomness of a result (as I do only in the two instances cited by Scheirer), I am in fact drawing inferences, based on that result, about the data generating procedure. Indeed, in my analysis of the 1970 drawings, I am very careful to talk about the observations and tests as offering *strong evidence in support of the inference* that the method used to generate the lottery sequence was not capable of producing random permutations of the numbers from 1 to 366. Scheirer's claim, that I treat randomness as a result rather than a method of achieving that result, demonstrates a lack of understanding regarding the process of inductive inference upon which the discipline of statistics is based.

Scheirer's second complaint is that I engage in statistical overkill. As has been my experience with most real statistical problems, I was unable to decide upon a single most appropriate analysis for the draft lottery data. As a result, I carried out several different, yet closely related analyses, but was unable to report on all of them due to the limitation on the length of articles in *Science*. Each of the four analyses which I presented in the article explored the data in a somewhat different way, thus adding some certainty to the conclusions reached. In a sense, it would have been a statistical fraud had I chosen to report only that analysis yielding the "most significant" result, a practice quite common in the social science literature, and I strongly regret my failure to report on further analyses.

Several readers have requested information about analyses on the 1971 drawings, comparable to those for the 1970 drawings reported in my article. Using the data provided by Rosenblatt and Filliben [*Science* **171**, 306 (1971)] for the 1971 lottery, I have computed the following values. The Spearman rank correlation coefficient, relating birth date and lottery number assigned to that date, was 0.014. For the 3×12 cross-classification similar to my Table 2, the descriptive level of significance for either chi-square goodness-of-fit test was greater than .25. The Kruskal-Wallis test statistic dealing with the average lottery numbers for each of the 12 months took the value $H = 7.38$, which is smaller than the associated degrees of freedom. Finally the Spear-

man rank correlation coefficient, for the ranks of the average lottery numbers by month, was .021. These values offer strong positive evidence regarding the randomness of the procedures used to generate the 1971 lottery numbers and stand in marked contrast to the analyses reported in my article for the 1970 lottery.

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Standards

Boffey's article on "Radiation standards: Are the right people making decisions?" (26 Feb., p. 781) recounts the history of how this particular standard came about. Thirty years of dedicated work by the International Commission on Radiological Protection and the National Council on Radiation Protection and Measurements have produced formal AEC regulations. This historical effort can serve as an admirable example of how voluntary technical standards can be generated by consensus and technical competence on a national and even an international level.

Boffey also touches on the more basic aspects of the procedures by which technical standards are set—not only nuclear standards. There are many hazards affecting the quality of our life, of which the radiological hazard is probably one of the least significant ones at the present time. More important issues are the effect of additives and preservatives in the food industry, fertilizers and pesticides in agriculture, drugs, noise, solid wastes, and the whole area of safety factors in design—be it jet planes or suspension bridges. In all these issues some standards are used consciously or unconsciously, which affect the public at large. Basically, these decisions should be risk-benefit decisions, which are clearly not technical problems alone, but involve wider public issues.

In the past, the far-reaching effect of such technical standards was not generally appreciated. Only recently has a significant portion of the public begun to understand that radiation limits, or the 0.5 part per million limits for mercury, the use of drugs, or all safety factors on automobiles, for example, involve a value judgment. Bof-