Letters

Oral Contraception Dropout Rate

Because the Ryder-Westoff study of oral contraception usage ("Use of oral contraception in the United States, 1965," 9 Sept., p. 1199) is so widely quoted as an index of extensive pill acceptance, the following datum from their report—which for some reason is not commented on despite considerable interpretation of other data—deserves emphasis. One out of five women who have ever used the pill stated that they "will not use [it] again." The ratio runs as high as one out of 3.6 users in the age group of 30 to 34.

This is a strikingly high rejection rate for a drug. There is none that I know of which is comparable. It supports the contention of many that the untoward effects of this drug are extensive and that there is gross underreporting of these effects to surveillance agencies. The dropout is also consonant with the continuing reporting of the dangers of the drug in the medical literature, and the concern expressed in the Hellman report to the Food and Drug Administration (*1*).

The fact that this contraceptive is the most desirable to women psychologically (because it dissociates itself in time and place from coitus) and that its use is initiated in a state of health heightens the significance of this finding.

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Reference

1. "Food and Drug Administration Report on Oral Contraceptives" by the Advisory Committee on Obstetrics and Gynecology, FDA, 1 August 1966. Available from the U.S. Government Printing Office, Washington, D.C. 20402.

We did not comment on the admittedly high proportion of women who have used the pill but who have stopped using, and report that they will not use it again, because this measure is totally inadequate as an index of the dropout rate, not to mention the "untoward effects of this drug." The reasons for its inadequacy are:

1) The ratio calculated by Ratner does not take into account the length of time the pill was used; it includes women who have used it for less than a month as well as those who have used it for 5 years. The implications of termination obviously differ by length of use.

2) Our analysis of the use of the pill included women who used it for reasons other than contraception. Some of these women have now stopped using it because it had satisfied (or had proven ineffective in satisfying) the original medical complaint—such as menstrual discomfort.

3) Some women who had used the pill stopped because they no longer needed it; their contraceptive needs vanished with the onset of menopause or sterility.

4) A small group of women used the pill in order to promote fertility, and accomplished this purpose.

5) Some women stopped using the pill because of problems unassociated with side effects—such as questions of morality, or cost, or difficulties in remembering to take the pill.

6) Some women reported stopping because of "doctor's orders." Although part of this may be attributable to the occurrence of undesirable symptoms, it is likely that much of the category represents the doctor's precaution without specific indications.

The remaining women who do not intend to resume can be classified as interrupting use because of reported undesirable reactions. Admittedly they constitute a majority of the total group referred to by Ratner, but it is evident that the symptoms they reported cover a wide range from real to imaginary, and from significant to trivial. We are currently in process of trying to estimate the dropout rate over time by type of reason, in order to achieve refined estimates appropriate to the question Ratner has raised.

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Wine-Making: An Eternal Art

Thanks to Maynard A. Amerine for a brief but thorough account of the history and present status of viticulture and enology ("The search for good wine," 30 Dec., p. 1621). One can only marvel at the recent progress made in wine production through the use of chromatography, automated ion-exchange equipment, programmed photometric analyses, and computerized blending systems.

Even more remarkable, at a time when world wine output has reached 7 billion gallons (26.5×10^9 liters) based on 25 million acres or 10^7 hectares of grape cultivation), is the minuscule production of those tiny vineyards which cling to the more archaic methods of wine-making. Certain French holdings, for example, located on Jurassic oolitic soils on the banks of the Saône, are a mere 5 acres (2 hectares) in size and yet continually produce the world's most honored wines. That this incomparable quality should be achieved without benefit of solid-state controls or computer circuitry but rather through adherence to such naive and intangible standards as finesse, body, bouquet, and roundness, supports the belief that the making of great wine is still more an art than a science.

There are many examples showing how the tools and methods of science have been successfully used to formulate artistic processes. Hutchins (*1*), using modern acoustical techniques, has discovered the basic physical principles responsible for certain sound qualities in the construction of the violin family of stringed instruments. This is not to say that science has replaced the art of violin-making or wine-making, but there is danger in thinking that it has.

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Reference

1. C. M. Hutchins, *Sci. Amer.* **207**, No. 32, 78 (1962).

Methods in Parapsychology

Boring commended (Letters, 4 Nov., p. 589) Churchman's review of C. E. M. Hansel's book, *ESP: A Scientific Evaluation* (Book Reviews, 2 Sept., p. 1088) and gave scientists a choice of three positions to take on ESP.

There is, I think, a fourth choice, comprised in the proposition that the

systems of relationships known as science are far from complete, and that there are many things which are patently "impossible" from existing standpoints, which become reconceptualized and assimilated into science as time goes on.

Experimental evidence for extrasensory perception, as for many other functions attributed to man, can ask only to be judged in empirical and logical terms. The problem is to review closely and critically the full structure of reported methods and findings. With a clear analysis of the factual material, one may plan better experiments. If, however, I understand Boring correctly, he is saying that unless present-day physical models are applied, the whole field of parapsychological research is a waste of time. This might well mean that it is not worth while to embark on *any* study at *any* time which might call for new models.

To be concrete, the experimental work in progress under Montague Ullman at the Maimonides Hospital of Brooklyn, in a standard sleep-dreams laboratory, studies the impact of experimentally controlled percepts of a distant person upon the dream content of an experimental subject. This study has been going on for some years (*1*). Further findings and analysis appear in "Experimentally-induced telepathic dreams: Two studies using EEG-REM monitoring technique" (*2*). This study is not ready for physical models, but when they can be contrived they will be used. Other studies (*3*) find modern physical models useful up to a point, but not very far. Several other elaborately safeguarded studies of the telepathic process are going on in the United States, in England, and elsewhere. Annual conventions of the Parapsychology Association report much experimental work (*4*), and most of this shortly gets into publication where it is open to the full and normal critical reading and analysis of all interested scientists.

Hansel's book does contain some useful passages of critical analysis, along with some extraordinary misconceptions and errors. The primary thesis of this type of critique is that since the experiments surely could not have been carried out as described and obtained the reported results, the experimenter is responsible either for bad reporting or for trickery; consequently, he concludes that it is patently not worth the trouble to deal systematically with the data as presented.

The animus involved in this method is easily understood, for new models are indeed threatening. The struggle to get close observation and serious thought devoted to the forms of knowing that we do not at present understand was a part of the earnest labor of William James and Henry Sidgwick who, simply because they found phenomena which would broaden understanding of human cognitive powers, devoted a great deal of time to the mediumship of the staid, gentle, solemn, and always cooperative Mrs. L. E. Piper.

Regarding their studies, Churchman has this to say: "Consider the scene in which distinguished academic professors like Henry Sidgwick and William James sit around seriously watching the antics of clever biddies and brattish youngsters going through their tricks." If this has any relation at all to any discoverable facts, it would be well to have them. The impression of an experimenter working in this field is that a larger ratio of facts to opinions would be salutary.

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References

1. *Arch. Gen. Psychiat.* **14**, 605 (1966).
2. M. Ullman, S. Krippner, S. Feldstein, *Intern. J. Neuropsychiat.* **2**, 420 (1966).
3. L. L. Vasiliev, *Experiments in Mental Suggestion,* Trans. (Gally Hill Press, Church Crookham, Hampshire, England, 1963).
4. *J. Parapsychol.* **29**, 278 (1965).

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SYMPOSIA IN

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NOMINATIVE GERMAN
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ZUKUNFT OF THE FUTURE
EUPHORIA

The Editor
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Sir:

The Senatus of this Institution views with alarm the bias evident in the article by Greenberg (Science, 154, 1424,(1966)) on the work of Dr. Grant Swinger, and his calm acceptance of Swinger as the father of both NASA and UPCHUK despite the lack of authoritative documentation therefor. Again we have an instance in which recognition for basic achievement is lacking until some "name" institution suddenly becomes interested in projects already well advanced by academic groups less well situated with respect to publicity.

We seek no publicity, but desire to place upon the record the true history of both these projects.

NASA is the child of the affiliates of this university, most notably the Camelopardophile Society which, by the autumn of 1934, had so far succeeded as to present not merely one, but a full half dozen giraffes to the thousands assembled that year to witness the Yale-Princeton game. Intensive work in this field has, since, been carried on under the aegis of our department of Scatological Eschatology, but has recently been suspended in consequence of the translation of the chairman to Chapel Hill.

UPCHUK is, in fact, no more than an extension of the Central Oconomowoc United Group Helping Unlikely Projects, (COUGHUP) which, by 1954 had formulated basic rules for the solicitation of financing for less evidently anguished projects (LEAP).

It is to be hoped that, in the future, Greenberg may be able to better check his sources, and avoid playing into the hands of a self-seeker like Swinger.

C. N. Barnum
Writer to the Senatus

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In my review I did not mean to imply that those who seek a broader base for the explanation of phenomena are therefore ill-advised; on the contrary I was myself suggesting that the scientific community clearly needs a broader conception of deception. But of course, I also think that anyone who tries to move away from accepted patterns of research is apt to appear ridiculous; this in itself is a fascinating aspect of the humor of science. However, there is nothing "wrong" in appearing ridiculous, just as there is nothing really "wrong" in being deceived.

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Studies of Nonrandom Groups

In his letter (21 Oct.) Walberg does not make clear that hypothesis testing in studies involving nonrandomly chosen "grab groups" is feasible. If it were not, the results of many comparative experiments in the behavioral sciences would be difficult to interpret statistically, because any differences whatsoever might be attributed to chance fluctuations. The great Sir Ronald Fisher's work on statistical inference must be supplemented by that of Wilk and Kempthorne, Cornfield and Tukey, Collier and Baker, and many other statisticians whose extensions of randomization and permutation theory Fisher himself anticipated.

Generalization of the conclusions of such an experiment to units other than those in the grab group cannot be made probabilistically, however. This generalization depends on empirical evidence, secured outside the experiment, that no characteristics of the nonrandomly obtained units interact with the treatments so that the effects would probably be different in another group.

For example, one might grab the first 100 students passing one's door, assign 50 of them randomly to one treatment and the remaining 50 to the other, and conduct a controlled experiment. Suppose the "treatments" consist of having 50 students read a short story printed in German Gothic type and the other 50 read the same story in ordinary typewriter type of the same size, one might find comprehension better for the Gothic type if the grab group consisted chiefly of students majoring in German, but better for ordinary typewriter type if one had happened upon a sight-saving class.

Thus, if the difference between the two styles depends greatly on erudition, visual acuity, or other characteristics of the experimental subjects, the particular grab group used may lead to an ungeneralizable conclusion. Note, though, that such interactions may be infrequent or small enough not to restrict greatly the generalization of conclusions about *differences* obtained from experiments conducted with nonrandomly chosen groups (*1*). On the other hand, errors in estimating group parameters (such as, in the foregoing example, the mean reading-comprehension score) from the scores of a nonrandom sample of persons subjected to only one of the treatments seem likely to be great.

For example, if the height of a student does not interact with the style of printing type, it will make no difference whether one's grab group consists chiefly of atypically tall (or short) persons if one is trying to estimate the differential effect of the two styles on reading comprehension. If, however, one wishes to estimate the average height of male students on the campus from a sample of such men, bias will occur unless the sample is representative—and drawing a sample in a probabilistic fashion is the usual way to secure representatives within the known limits of sampling fluctuations. Walberg stressed the latter point. The former needs emphasis, too.

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Reference

1. For further discussion of the internal and external validity of experiments, see D. T. Campbell and J. C. Stanley, *Experimental and Quasi-experimental Designs for Research* (Rand McNally, Chicago, 1966).

Astronomy's Crucial Requirements

If United States astronomers were asked to draw up a list of the most exciting problems today in astronomy, we would include the following: (i) the nature of quasars; (ii) the identification and interpretation of x-ray, γ-ray, and nonthermal sources and the nature of the far ultraviolet and infrared emissions from various celestial sources; (iii) the establishment of distance scales and the large-scale distribution of matter in the universe; (iv) the origin and evolution of the stars and solar system and eventually that of the stellar system itself; and (v) the solar magnetic cycle and phenomenology-solar-terrestrial relationships.

Many of these problems require observations secured from above the earth's atmosphere. Therefore we should impress upon NASA that astronomy has a greater stake in the space effort than any other science. As with radio and x-ray astronomy, we can anticipate important results when space and ground-based observations are combined. The complementarity of optical radio, rocket, and satellite observations cannot be overemphasized. Equally indispensable is the large ground-based optical telescope located at a suitable dark-sky site. These are in grievously short supply.

All of us will agree that astronomy is a unique science. One of its unique features is that as late as A.D. 1967, its most productive research tool, the 200-inch (508-centimeter) telescope was provided by private funds made available 40 years ago! The second largest instrument, the 120-inch (305-centimeter) telescope was supplied by the California taxpayers to the University of California. Some 3 years ago, the federal government, through the National Science Foundation, initiated preliminary work on a 150-inch (381-centimeter) telescope for the Kitt Peak National Observatory.

The solution of the quasar problem may be delayed because it requires extensive use of large telescope time. As for stellar evolution and cosmological problems, nature has provided us with a celestial Rosetta stone in the Magellanic Clouds. The most urgent requirement for astronomy today is a number of large instruments in the southern hemisphere. This urgency was emphasized in the Whitford report (*1*) and reemphasized in the Physics Survey committee report (*2*). Whether American scientific policy-makers also appreciate this great need is not clear. Three well-documented proposals for telescopes with apertures of 150 to 200 inches for the Southern Hemisphere have been presented to various agencies by responsible and experienced groups of United States astronomers. The most recent one included an offer of equal cost-sharing by a southern hemisphere country and involved only the very moderate expenditure of $5