## LETTERS

### AAAS Elections and Equal Rights

In studying the outcome of the 1979 AAAS annual elections (24 Aug. 1979, p. 783; 7 Dec. 1979, p. 1170), we were struck by an apparent inequity in the electorate's treatment of the sexes. In 73 elections involving 2 to 16 candidates, we counted 224 candidates for 112 offices. We classified them as male (M) or female (F) and as elected (E) or not elected (N). Thus for all candidates:

| Sex | E | N | Total |
|---|---|---|---|
| M | 84 | 100 | 184 |
| F | 28 | 12 | 40 |
| Total | 112 | 112 | 224 |

If we eliminate from the classification all those contests involving only male candidates (there were no all-female contests), the discrepancy is even more striking.

| Sex | E | N | Total |
|---|---|---|---|
| M | 28 | 44 | 72 |
| F | 28 | 12 | 40 |
| Total | 56 | 56 | 112 |

We see that in elections where both sexes were represented, 39 percent of the male candidates won, while in contrast 70 percent of the female candidates won.

If we reject as untenable the theory that the electorate is swayed by such an irrelevant characteristic as sex, this suggests that the nominating committees are applying more exacting standards to female candidates.

Or vice versa.

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### Is the Paranormal "Normal"?

Nicholas Wade (News and Comment, 4 Jan., p. 41) writes of Wagner and Monnett's conclusion that American college professors have much more positive attitudes toward extrasensory perception (ESP) than does the American population in general. They report that 15 percent think ESP is scientifically established and 50 percent think it is a likely possibility. It is more likely, however, that college professors are simply typical Americans. In a representative survey of the American population, conducted in 1973, Greeley (*1*) found that 58 percent of the population believed they had *personally experienced* at least one form of ESP (mind-to-mind contact with someone at a distance). Greeley further found that people who believe they have personally experienced ESP are significantly better educated and more liberal than those who do not believe they have personally experienced ESP. For those who believe that the characteristics of the majority or the more educated determine what is "normal," one may draw the interesting conclusion that people who do not think they have had a psychic experience are "abnormal!"

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**References**

1. A. Greeley. *The Sociology of the Paranormal: A Reconnaissance* (Sage, Beverley Hills, 1975).

### Social Science: Ethics of Research

I am writing to correct some of the more blatant errors in Constance Holden's article "Ethics in social science research" (News and Comment, 2 Nov. 1979, p. 537) and to raise an issue or two for your readers.

Many items in the first two paragraphs of Holden's article are not accurate. In addition to those mentioned in the letter from Horowitz (30 Nov. 1979, p. 1022) and in the erratum in the same issue, I note the following errors. My research on impersonal sex acts in public restrooms was conducted not "in 1969" but from 1965 to 1968. Observations of these acts were made in dozens of public rest facilities rather than "in a public bathroom." My study was not "designed to cast light on society's treatment of homosexuals," noble as such a purpose might be, but to investigate a covert and highly stigmatized form of behavior engaged in by men who claim a *range* of sexual orientations and life-styles.

I did fill the role of a "watchqueen" to alert the participants to approaching intruders, although the purpose of performing those duties was to facilitate observing sexual behavior in a natural setting in the least obtrusive way possible. Rather than "lying" to the Department of Motor Vehicles (as the article's indictment reads) in order to trace license numbers of potential respondents, I misrepresented myself to the local campus police as engaging in "market research" for the purpose of protecting my respondents. The implication that I "then joined a public health survey team" in order to deceive my "subjects" is a distortion.

Holden may be correct in noting that *Tearoom Trade* "has become a classic in the fast-growing field of ethics in social science research, where it is commonly cited as a crass violation of subjects' rights." What she does not report, however, is that the book won the C. Wright Mills Award of the Society for the Study of Social Problems in 1970 as "the outstanding work on a social issue."

It is disturbing to learn for the first time, from a journal article, of a 2-day conference that apparently featured discussion of my work. Are the distortions of fact perhaps the result of misrepresentations made in materials presented at that conference, to which I had no access?

Innumerable commentators have arraigned my dissertation research on charges comparable to those cited by Holden. Who, on the other hand, has mentioned the following points about the tearoom study?

1) Not one of my 100 respondents has ever reported suffering harm as a result of my research.

2) Many of my respondents (both those who were fully informed about the research and other, "deceived" men who later read the book), not to mention hundreds of other "tearoom participants," have expressed appreciation for ways they have benefited from the study.

3) The moral entrepreneurs fail to note that, in the mid-1960's, the sociologists had no Code of Ethics mentioning the need for "informed consent." The issue was never raised in my graduate studies, and I had never thought about that as a matter of concern. As Holden grants, I "was scrupulous about guarding the confidentiality" of my respondents. That was all anyone knew about the ethics of research in those days.

4) In the revised (1975) edition of *Tearoom Trade*, I added a 57-page "Retrospect" on ethical issues, including a complete reprinting of the major critical publications available at that time.

Do my critics offer me a comparable platform? Have those affluent foundations that foster the work and careers of the Ayatollahs of Research Ethics ever considered the possibility of allowing those of us assumed to be guilty to enter into dialogue with our accusers?


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The letter from Horowitz, aside from its self-serving bromides, objects to the fact that an author who was "mentioned repeatedly" at a social science confer-

ence was not invited to attend. Well, who was to know in advance that the author would be mentioned repeatedly?

Nowhere does this well-known gatekeeper-editor, Horowitz, commend Holden for her brilliant article, which makes clear what is generally obscured by the social science jargon of our time.

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## Power-Line Radiation

Thorne and Tsurutani (Reports, 25 May 1979, p. 839) address some important questions regarding power-line radiation (PLR) in the magnetosphere, a subject that has become a topic of intense research in recent years (*1-3*). Although Thorne and Tsurutani accept the existence of PLR in the magnetosphere, they conclude that it is unimportant in comparison to naturally occurring waves. We believe that their conclusion, based on analysis of a limited amount of data, is premature and that it is important to further investigate this man-made perturbation of the space environment out to a distance many times the radius of the earth.

It has been clearly demonstrated that PLR can trigger emissions that strongly interact with trapped energetic particles in the magnetosphere (*2*). However because of limitations in signal detection and processing techniques that have been employed to date, it is not *always* possible to determine conclusively whether a given emission is triggered or is generated spontaneously. For this reason, it is not possible at present to make a quantitative comparison between PLR effects and natural effects.

The main argument of Thorne and Tsurutani is based on the fact that their data on ELF (extremely low frequency) chorus collected by the Orbiting Geophysical Observatory (OGO) do not show significant variations at different longitudes that could be attributed to PLR. This is contrary to earlier results based on ELF and very low frequency data collected by OGO-3 (*3*). This discrepancy is not too surprising in view of the fact that the two relatively small data sets were acquired by different instruments covering different frequency ranges and were analyzed differently. Without getting into detailed technical arguments, we simply wish to make a fundamental point concerning their data —that the absence of evidence should not be used as evidence for absence.

There are undoubtedly waves of entirely natural origin that are important for magnetospheric dynamics. However, their importance cannot be used as evidence against the importance of PLR unless it is supported by a quantitative comparison. Such a comparison will require improved techniques for identifying PLR and PLR-triggered emissions. Fortunately progress can be and is being made in that direction. For example, since the report by Thorne and Tsurutani, direct evidence of PLR has been found in the deep magnetosphere in data from ISEE-1 and GEOS satellites (*4*). Theoretical and experimental studies of PLR sources are under way or are being planned by a number of research teams around the world (*5*).

It will be some time before we fully understand PLR effects and their importance, vis-à-vis natural phenomena, but there is certainly no basis at present to dismiss PLR as unimportant.

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### References and Notes

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2. C. G. Park and R. A. Helliwell, *Science* **200**, 727 (1978); K. Bullough, A. R. L. Tatnall, M. Danby, *Nature (London)* **260**, 401 (1976); J. P. Luette, C. G. Park, R. A. Helliwell, *J. Geophys. Res.* **84**, 2657 (1979); H. C. Koons, M. H. Dazey, B. C. Edgar, *ibid.* **83**, 3887 (1978); C. G. Park and T. R. Miller, *ibid.* **84**, 943 (1979); _____, D. C. D. Chang, *Geophys. Res. Lett.* **5**, 861 (1978).
3. J. P. Luette, C. G. Park, R. A. Helliwell, *Geophys. Res. Lett.* **4**, 275 (1977).
4. J. P. Luette and T. F. Bell, private communication; F. Lefeuvre, private communication.
5. The PLR intensities near power lines have been recently measured by R. Barr in New Zealand and T. Yoshino in Japan, both of whom used ground-based as well as balloon and rocketborne instruments. Yoshino and his colleagues plan to make more measurements using balloons, rockets, and satellites. The University of Manitoba, Stanford University, and the Sandia Corporation plan to make balloon measurements in Manitoba, Canada, in collaboration with the Manitoba Hydro.

Park and Helliwell disagree with portions of our *Science* report, particularly our conclusion that the stimulated generation of magnetospheric chorus by PLR is unimportant in comparison to naturally occurring waves. We argue that our conclusion remains valid. There is no conclusive evidence of any dependence of outer zone chorus on geographical longitude. Claims of such evidence (*1*) are based on erroneous statistical arguments (*2*). Furthermore, such a dependence would not be expected because chorus occurs on magnetic field lines that intercept the polar regions of the earth, well away from high-density population centers. As we stated in our report, PLR would also be strongly damp-