awards. When we moved into our three-story laboratory, we inherited a modern communications system—a pull cord attached to a bell which could be heard at all three levels. We now use it for ingenious approaches to the solution of problems, and, as yet, no one has achieved the ultimate—the five-bell award. However, when an original stupidity is perpetrated, we issue the no-bell award.

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Knowledge Factory

The following excerpt from Sinclair Lewis's novel *Arrowsmith* (*1*) is an interesting addition to Harold L. Enarson's editorial "University or knowledge factory?" (7 Sept., p. 897).

The University of Winnemac is at Mohalis, fifteen miles from Zenith. There are twelve thousand students; beside this prodigy Oxford is a tiny theological school and Harvard a select college for young gentlemen. The University has a baseball field under glass; its buildings are measured by the mile; it hires hundreds of young Doctors of Philosophy to give rapid instruction in Sanskrit, navigation, accountancy, spectacle-fitting, sanitary engineering, Provençal poetry, tariff schedules, rutabaga-growing, motor-car designing, the history of Voronezh, the style of Matthew Arnold, the diagnosis of myohypertrophia kymoparalytica, and department-store advertising. Its president is the best moneyraiser and the best after-dinner speaker in the United States; and Winnemac was the first school in the world to conduct its extension courses by radio.

It is not a snobbish rich-man's college, devoted to leisurely nonsense. It is the property of the people of the state, and what they want—or what they are told they want—is a mill to turn out men and women who will lead moral lives, play bridge, mention books, though they are not expected to have time to read them. It is a Ford Motor Factory, and if its products rattle, they are beautifully standardized, with perfectly interchangeable parts. Hourly the University of Winnemac grows in numbers and influence, and by 1950 one may expect it to have created an entirely new world-civilization, a civilization larger and brisker and purer.

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References

1. S. Lewis, *Arrowsmith* (Harcourt Brace, New York, 1949), chap. 2, sect. 1.

Marital Status and Mobility

Beverley R. Green (Letters, 10 Aug. p. 496) reports that only 5 percent of the applicants for a biochemistry position and 3 percent of the applicants for a botany position at the University of British Columbia were women. She implies that, since between 10 and 20 percent of those receiving Ph.D.'s in these fields are women, women are giving up without trying; that women make up a small proportion of faculty because they do not apply for jobs; and that men cannot be blamed for this.

We analyzed the applications received for five positions in the department of biology at Michigan Technological University as a result of an advertisement in the 2 March issue of *Science*. We found that the percentages of women applicants compared well with the percentages reported by Green. However, our interpretation of these results is quite different from Green's.

In addition to classifying applicants according to sex, we also noted marital status. We found that more than 90 percent of the male applicants were married, while all but one (93 percent) of the female applicants were single.

None of the female applicants had children, whereas the majority of the men were fathers. Females made up nearly 30 percent of the single applicants but less than 1 percent of the total number of married applicants.

It is not surprising that we did not receive applications from married women. The direct sexism we, as women, have experienced and witnessed in hiring and promotion practices is only the tip of the iceberg. Sexism has been institutionalized into all facets of our society including the family unit. As Ann C. Dean and Robert C. Dean (Letters, 14 Sept., p. 990) point out, it is not unusual for a woman to give up her job and move with her husband to the site of his new job, but the opposite is nearly unheard of. In fact, the one married woman applicant stated clearly she would not take a job unless her Ph.D. husband was also offered a job. On the other hand, two male applicants with Ph.D. wives expressed only the hope that their wives might also find a job in the geographical area where they found theirs.

For a woman to compete freely in the job market she must give up what many feel are basic human needs: marriage and a family. Men need not make this sacrifice. Although the men at a university cannot directly be blamed for the discrimination institutionalized into the family unit, the new understanding we have acquired of the sexism in our culture has come from women. Most university men, at least at our university, have little interest in expanding their consciousness in this area or accepting any new lifestyles or conditions which might offer alternatives to the present situation.

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Parapsychology

Nicholas Wade's otherwise excellent survey of contemporary parapsychology (News and Comment, 13 July, p. 138) perpetuates a long-standing myth that ought to be decently buried. Wade writes, "Although the parapsychologists have now amassed an impressive volume of apparently careful experimental literature . . . critics charge that the published work represents an artifact, in as far as it tends to be only the successful experiments that get reported, while the presumably more numerous null results go unremarked." Aside from the fact that this is true for all branches of science, dismissing ESP (extrasensory perception) results on the ground of selective publication is not statistically valid, as well as being empirically unsupported.

If we have only random variation in our experiments (no ESP), then we have to carry out about 20 experiments to get one (presumably publishable) which is significant at the .05 level, 1000 to get one significant at the .001 level, and so forth. There are hundreds of published, successful parapsychological experiments with the main analyses significant at the .05 level, and of these many have significance levels exceeding 10^{-6}. The selective publication hypothesis then predicts that there are trillions of unsuccessful, unpublished ESP experiments, an obviously ridiculous figure, unless one credits the handful of parapsychologists in the last half century with some phenomenal work abilities, paranormal in themselves.

If I apply the selective publication hypothesis to my own published, successful parapsychological studies, I seem to have misplaced the data for over 100,000 experiments: strange how I could be so productive and forgetful at the same time.

Burke Smith and I surveyed the membership of the Parapsychological Association on a variety of empirical matters in 1966, including the ratio of each investigator's unpublished to published studies. The ratio averaged about two to one, not hundreds or thousands to one.

May this myth rest in peace.

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Nicholas Wade's review of psychical research is timely. Many authors take the stand that parapsychology is now proven beyond a reasonable doubt. I believed them until I started reading the original papers, which are unconvincing, to me at least. I share Martin Gardner's view (*1*) that Rhine's experiments do no more than confirm the laws of probability.

If research workers wished seriously to investigate psychokinesis, they would not mess about with dice and pendulums, but would use the Boys radiometer (*2*). This instrument can be made sufficiently sensitive to detect the heat of a candle flame half a mile away, so it should respond to any psychokinetic effect. Furthermore, with the addition of a torque motor or revolution counter, the effect could be measured. Why have no parapsychologists done this?

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References and Notes

1. M. Gardner, *Fads and Fallacies in the Name of Science* (Dover, New York, 1957).
2. The Boys radiometer was devised by Charles V. Boys, an English physicist who is most famous for his publication *Soap Bubbles* (Dover, New York, ed. 3, 1959). The radiometer consists of four paddles in a cruciform arrangement, placed on a needlepoint. One side of the paddles is black (absorbing) and the other side is white and metallic (reflecting). The whole is enclosed in an evacuated glass case. It is obtainable from most novelty stores and scientific dealers.

Pesticide Regulation

The report by Luther Carter, "Pesticides: Environmentalists seek new victory in a frustrating war" (News and Comment, 13 July, p. 143), ignores the only full scientific review of the use of aldrin and dieldrin—that of the ad hoc advisory committee of outside scientists appointed by William Ruckelshaus, then administrator of the Environmental Protection Agency. The committee completed its study on 24 March 1972. The group was made up of R. L. Doutt, M. L. Fairchild, S. D. Faust, F. K. Kinoshita, R. A. Parker, S. S. Sternberg, and R. D. O'Brien (chairman). It represented very diverse fields of expertise and ideologies, yet its report was unanimous. Nevertheless, its recommendations were not accepted by Ruckelshaus. The report recommended that certain major uses of aldrin and dieldrin which we believed to be nonpolluting, should be retained, while others should be eliminated. By omitting mention of this step in the process, Carter paints a picture of morally pure and scientifically sound environmentalists struggling against wicked industry and an unhelpful Administration to abolish pesticide use. Our committee had hoped that we could inject an element of balance into the whole question of pesticide regulation and seek the elimination of those practices for which there was evidence of deleterious effects.

Carter refers to the "pronounced tumorigenic effect on test animals" of the persistent pesticides which are still available. Later he speaks of the "dispute . . . [about] whether all tumorigens are potential carcinogens and whether mice were an appropriate test animal." The fact is that malignant tumors de-