Letters

The Cable Car

In response to Lewis E. Walkup's plea that more attention be given to the psychological aspects of mass transportation (Letters, 17 Dec. 1971, p. 1184), perhaps designers should study the cable car in San Francisco. Its popularity is probably due as much to a multiplicity of exits and lack of confinement as to its novelty.

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Women Academics

In their article "Women in academia" (3 Sept., p. 892), Lewin and Duchan report a trend toward discrimination against women in the hiring decisions of chairmen of departments of physical science. A similar study has been published by Fidell (*1*), whose evidence clearly shows sex discrimination in the hiring attitudes of chairmen of departments of psychology. Fidell sent eight different, descriptive paragraphs to the department chairmen; the paragraphs emphasized different aspects that Fidell considered important in the hiring decision. She also varied the first names and the sex for each paragraph. The male candidates were offered hypothetical positions at higher ranks than those offered to the female candidates—even though the paragraphs describing the candidates were identical. In addition, more men than women were considered suitable for positions that lead to tenure.

The fact that these two studies of hiring attitudes were done somewhat differently and focused on different scientific disciplines should lead us to draw a stronger conclusion than the one arrived at by Lewin and Duchan. Sex discrimination in scientific academia is not just probable. It is a clear deterrent to the development of competence and productivity on the part of women in science. I doubt that we need further demonstration of the status of women academics in the 1970's. What we do need, as Lewin and Duchan suggest, is active federal support for equality in all phases of the training, hiring, and promotion processes.

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Reference

1. L. S. Fidell, *Amer. Psychol.* **25**, 1094 (1970).

Should scientists present their evidence? Lewin and Duchan hold forth for ten columns on "Women in academia" without once presenting their actual experimental results. The closest they come to giving their evidence is this: "The results, although not statistically significant, showed definite trends that confirm our hypotheses that discrimination against women does exist at the time of the hiring decision." Not a number, not a percentage, not a single shred of quantitative information pertaining to their own results is presented in the whole article.

Are the standards of *Science* the standards of science?

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Lewin and Duchan state in their conclusion that "although most of the individual tests did not yield statistically significant differences, the data consistently yielded a trend in the direction of the existence of discrimination against women in academia." Since their data were analyzed by means of chi-square tests, I do not understand how the data could not be statistically significant and still yield a consistent trend. It is my understanding that when a chi-square test indicates nonsignificance, one should accept the null hypothesis that there are no differences between groups.

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Inspired by Lewin and Duchan's article, I would like to propose a handbook of methods for sabotaging the women's rights movement. It would serve to make the many legitimate complaints and real issues in this movement appear to be fabrications of the extremists, or at least less believable. I suggest the following procedures:

1) Do a study designed to test the hypothesis that there is discrimination against women who apply for academic jobs, but guard against generality, in case you get positive results. That way, the results can be easily dismissed as specific to the individual résumés that were used or to the scientific discipline that was sampled.

2) Only broadcast your study if your results turn out to be nonsignificant statistically, but—and this is the real coup—argue that this constitutes evidence for your hypothesis. The subtle effectiveness of this procedure is that, for many readers who are not interested or willing to look at other evidence, you have left the impression that all claims of discrimination against women are only as valid as this one. You have also left the impression that women are a bit pigheaded and myopic when they try to prove they've been wronged, and that they are willing to misuse science to legitimitize their claims.

3) Tips for report writing: (i) In the introduction to your article, if you must include supportive evidence, make it as trivial as possible. For example, you could justifiably report that the failure of the National Science Foundation (NSF) to award senior postdoctoral fellowships to even one woman recently, when there were 14 women applicants, 381 men applicants, and 54 grants awarded, is a statistically unlikely event (given equal quality of the grants for men and women). But, since the expected value of women recipients, given these proportions, is less than 2, this is not one of the more important examples of discrimination. Another good gimmick is to make illogical statements—that sounds very feminine. You could argue that the awarding of research grants by NSF in a certain discipline from 1964–68 is evidence of discrimination because the percentage of grants to women is less than the percentage of women in the discipline. Be sure to make no mention of the number of women who actually applied to NSF for these grants. (ii) In reporting your results, the main thing to keep in mind is omission of any numbers—means, *P* values, and so forth. This is particularly

important if your reported trends approached statistical significance, because then it might be argued that there was at least slight justification for your article. Also, be sure to test, and then discuss at length, each of the comparisons—not just the overall one which you initially report as nonsignificant.

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The actual data-gathering phase of our study was carried out during the latter part of 1969. At that time, no behavioral data on discrimination against women at the time of hiring had been reported in the literature. Naturally, the ideal methodology would have been one permitting the direct observation and evaluation of the actual decision-making process of departments when female applicants were evaluated in competition with male applicants. Although this type of study was not feasible (and still is not feasible), we are pleased that our results, employing a less sensitive methodology, support the discrimination hypothesis and are in agreement with similar studies, such as Fidell (*1*), and

Table 1. Summary of questionnaire responses of department chairmen by item and classification of sample for average male, average female, and superior female job applicants. Question 1 concerns the general impression of the résumé. The response range is graded *a*, very impressive; *b*, average; *c*, unacceptable. Question 2 concerns the inclination to hire the applicant. The response range is graded *a*, hire the applicant; *b*, indifferent; *c*, reject the applicant. In question 3, an evaluation of the applicant's educational background is requested. The response range is indicated by *a*, excellent; *b*, average; *c*, unsatisfactory. Question 4 concerns the possible change in response if the applicant were not recommended by a colleague of the department chairman. The response range is indicated by *a*, yes or *c*, no; a higher percentage of "yes" responses implies a lower rating of the applicant. The direction of preference of an average male over an average female (M/F) and of an average male over a superior female (M/SF) is also evaluated.

Classification	Question	Response: Average male (%)			Average female (%)			Superior female (%)			Preference	
		a	*b*	*c*	*a*	*b*	*c*	*a*	*b*	*c*	M/F	M/SF
Overall comparison of applicants												
	1	72	28	0	68	32	0	89	7	4	+	–
	2	29	68	3	18	71	11	52	48	0	+	–
	3	93	7	0	89	11	0	100	0	0	+	–
	4	25		75	35		65	15		85	+	–
Geographical location of school												
East-West	1	10	60	30	0	66	34	22	78	0	+	–
	2	0	20	80	0	11	89	0	45	55	+	–
	3	30	60	10	22	56	22	78	22	0	+	–
	4	40		60	45		55	45		55	+	+
Midwest-South	1	30	45	25	30	45	25	67	27	9	0	–
	2	0	23	77	8	15	77	9	55	36	–	–
	3	69	8	0	62	38	0	100	0	0	+	–
	4	23		77	23		77	46		54	0	+
Quality ranking of school												
Above median	1	14	53	33	0	64	36	29	71	0	+	–
	2	0	14	86	0	0	100	0	29	71	+	–
	3	57	43	0	29	71	0	86	14	0	+	–
	4	29		71	50		50	43		57	+	+
Below median	1	25	38	37	43	43	14	53	38	9	–	–
	2	0	25	75	0	29	71	9	60	31	–	–
	3	50	38	12	71	0	29	92	8	0	–	–
	4	31		69	0		100	46		54	–	+
Age of department chairman												
Below median	1	25	42	33	9	64	27	100	0	0	+	–
	2	0	25	75	0	0	100	13	62	25	+	–
	3	58	42	0	36	55	9	100	0	0	+	–
	4	17		83	27		73	50		50	+	+
Above median	1	19	50	31	23	42	35	22	61	17	–	–
	2	0	31	69	0	29	71	6	35	59	+	–
	3	44	44	12	53	35	12	78	22	0	–	–
	4	31		69	41		59	39		61	+	+
Length of time as chairman												
Below median	1	25	33	42	0	70	30	50	25	25	+	–
	2	0	33	67	0	10	90	25	38	37	+	–
	3	50	42	8	40	40	20	75	25	0	+	–
	4	41		59	41		59	50		50	0	+
Above median	1	19	56	25	28	39	33	41	53	6	–	–
	2	0	25	75	0	22	78	0	44	56	+	–
	3	50	44	6	50	44	6	88	12	0	0	–
	4	13		87	34		66	41		59	+	+

with the many statistical surveys subsequently available regarding the hiring and promotion of women in universities. In Table 1, we have reproduced a summary of the statistical data underlying our analysis.

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Reference

1. L. S. Fidell, *Amer. Psychol.* **25**, 1094 (1970).

Cooperation

On the report "Human environment conference: search for a modus vivendi" (News and Comment, 18 Feb., p. 736), Nigel Hawkes states, "Throughout the preparations for the conference, U.N. sources have been complaining privately of the obstructive attitude the British have taken."

Quite to the contrary, the British representatives have been very cooperative and sources of constructive criticism and productive ideas. Whatever successes the Stockholm conference may have in June will be due in no small measure to the contributions of the United Kingdom. I make these observations after having recently completed a 6-month assignment with the conference secretariat.

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Testing for Teratogenicity

The eminent gentlemen who signed the protest (Letters, 5 Nov. 1971, p. 545) against *Science*'s treatment of the 2,4,5-T advisory committee report (News and Comment, 13 Aug., p. 610) appear to say that if a study *does* find a teratogenic effect in some species when doses of a chemical are given that are far in excess of any possible human exposure, it does not constitute scientific grounds for banning the chemical. Presumably if a study *does not* find a teratogenic effect in some species, it also does not constitute scientific grounds for banning the chemical. What then are the scientific grounds for banning a chemical because of its possible teratogenic effects? Obviously the answer is, "There are none," since studies are not made of the effects of doses that are below any possible human exposure. (Toxicological experiments of the type needed to permit the labeling of 2,4,5-T or similar substances are usually performed on a few animals that are exposed to high doses; little effort is made to tell what would happen to the animals—not to say anything about man—if they were given low doses.) In fact, Alvin M. Weinberg (Letters, 5 Nov., p. 546) makes it clear that the establishment of a teratogenic effect from low doses is believed to be "trans-scientific," since it would take too many animals to establish such an effect.

It is not clear whether the council of the Society of Toxicology speaks for all toxicologists, for a majority of toxicologists, or simply expresses the view of an establishment in toxicology. It is curious, nevertheless, that the main appeal in the letter is to respect the views of authority (that is, of the council of the society) and of the majority (that is, of toxicologists)—not very persuasive arguments for scientists to advance.

If the council believes it has a case, then the Society of Toxicology ought to sponsor an open examination of these issues. They are "fundamental" to all interests, and light, not heat, is needed to illuminate them.

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Sorensen and pH

John Walsh is safe in writing (News and Comment, 3 Mar., p. 973) that S. P. L. Sørensen "achieved the first really accurate method for the determination of *p*H," because nobody had ever before determined it. It was in fact Sørensen's brilliant achievement to perceive that the acidic intensity of an aqueous solution is best expressed as a logarithmic function of the concentration of H_3O^+. Today. pH is a vigorous near-septuagenarian, some premature obsequies in the recent clinical literature notwithstanding.

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