lection for irregularity very weak and perhaps nonexistent should be pointed out.

First, only in that portion of the population using rhythm would there be any selection at all and only in the female half of that population. Also, time of ovulation is probably a polygenic trait and the phenotype which we are examining rather distant from primary gene action and thus highly susceptible to environmental factors. These conditions would slow down selection. Nonetheless, use of the rhythm method would increase the selective value of the genes that cause irregular cycles.

On the other hand, selection factors exist which have been operating through evolution to give us a reasonably regular cycle in present-day women. These factors, however strong or weak they may be, should continue to operate in the population and perhaps cancel out entirely the selection caused by the use of rhythm. (It is interesting, and in a sense amusing, to note that the use of rhythm now, with its possible selection against regularity, may be several thousand years too soon. Perhaps we were evolving toward perfect regularity when natural methods of conception control would have been foolproof!)

Further, one may even postulate that selection for regular ovulatory cycles may increase as the use of rhythm grows. For example, that portion of the male population who are ready to practice rhythm are intelligent, responsible and self-sacrificing people, otherwise they would not attempt such a method. It seems reasonable (at least as reasonable as many of Hardin's conjectures) that they might in the future practice a rather rigid selective influence by choosing as mates only those women who have regular cycles. Thus selection may soon turn in favor of regular ovulatory cycles and the natural method of conception control become even more effective.

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Scientists and Civil Defense

I hope that scientists do not heed D. S. Greenberg's advice, on other matters so sound, that scientists transfer their attention from "bomb" problems to such home problems as traffic control (27 Dec. 1963, p. 1635). . . . Certainly their efforts and influence are needed to supplement the efforts of the Arms Control and Disarmament Agency, and civil defense is even more critical because it is the primary concern of no agency.

I also take issue with Greenberg's implication that the verdict of informed, thinking men is against civil defense. I don't believe that any scientific group has rocked or "could rock the U.S. government with a well-drawn and well-publicized brief against civil defense." And it was *not* "against its better judgment" that Congress granted an early Kennedy request for civil defense expansion (6 Dec. 1963, p. 1277). Contrary evidence is in the recent civil defense hearings before the Hebert subcommittee of the House (*1*). In these open hearings, 88 witnesses testified in favor of the bill for incorporating fallout protection into new public buildings, and 15 against. Of the 30 with claim to some scientific competence, including scientists, engineers, M.D.'s, and architects, 25 were for and 5 against the bill. However, as I interpret the testimony, only one, a psychiatrist, was against civil defense, his grounds being the possible psychic damage to children from civil defense preparations. The other four were against the bill because it was not strong enough, their general contention being that an effective civil defense must also afford protection from fire, blast, chemical, and biological hazards—comprehensive protection of the type which Russia and Sweden, according to other testimony, have already supplied to an important fraction of their populations. . . . the subcommittee, and then the House, by wide margins, voted for this bill authorizing all of the little the Department of Defense had asked for. If the Senate informs itself as well as did the House, it should follow suit.

Of course the remaining question is how far we should go beyond this rudimentary step, involving 0.5 percent of our defense budget, toward the ultimate of comprehensive protection, involving up to 10 percent of our defense budget for 5 to 10 years (*2*). Unfortunately, the Department of Defense may not supply a good answer to this question despite the competence of its OCD, if its thinking is reflected in the statement by General LeMay, Chief of Staff of the Air Force, that the expenditure for comprehensive protection "would be unwise, ill advised, and more importantly, would inevitably become competitive with requirements for active defense." Assistant Secretary Pittman candidly stated, at the more recent Senate hearings, that the program of the bill "has the support of the military services because it has been carefully designed as a modest and manageable undertaking. If it threatened to grow into a vast and expensive system, it would not have the support of the Secretary of Defense and the Joint Chiefs which it has today" (*3*).

We cannot leave to the military alone the development of policies on which, should war come, hinge the fate of each civilian and each segment of civilian society and culture. . . .

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Cigarettes and Polonium-210

Our report dealing with polonium in cigarette smoke (*Science*, 17 Jan., p. 247) was necessarily brief, and it is evident from Irving Michelson's letter (28 Feb., p. 917) that some aspects of our observations were not clear. With respect to the relative importance of polonium in genesis of lung cancer, we have emphasized that the anticipated low radiation dose would act primarily as a cancer *initiator*. The known chemical carcinogens are apparently not present in sufficient amounts in smoke to account for lung cancer rates ascribed to cigarette smoking. Although ionizing radiation is an initiator par excellence, only time will tell the importance of polonium's alpha radiation among the possible initiators of bronchial cancer in smoking. As we pointed out, the cocarcinogens in cigarette smoke probably are important also as causal factors. In addition, radiation from this source could act in association with viruses.

We believe our estimates of local radiation doses to certain regions of the bronchial epithelium are low principally because of variations within the samples of bronchial epithelium we

studied. The measurements have been done on samples as small as 0.5 cm^2 of area, and we calculated the *mean* dose over this area (about 165 rem in 25 years, in the example alluded to by Michelson). The higher estimate of 1000 rem or more in 25 years for particular areas was based on the supposition that local regions within the samples we measured could have polonium concentrations three or more times the average value. We believe that this guess of the degree of nonuniformity is probably conservative, because of the fact that the highest mean concentrations have been found at segmental bronchial bifurcations. On anatomical and physiological grounds we expect that chronic retention of smoke particles in the bronchial epithelium would probably be quite sharply localized to the region of the bifurcation itself, in an area of perhaps only a few square millimeters.

Another point should be mentioned with regard to these calculated doses. It is probable that the biological half-life of polonium in the epithelium is approximately 30 days, and therefore the physical half-life (138 days) is unimportant in determining the mean residence time. To make the dose estimates we used a mean residence time of 50 days (half-time 35 days). This figure is based on measurements of lung retention of a polonium-labeled aerosol in dogs [F. A. Smith *et al.*, *Am. Ind. Hyg. Assoc. J.* **22**, 201, (1961)]. Better data are needed to determine bronchial retention times, particularly because the doses localized to small regions are probably much more important than the doses delivered from smoke in transit over the epithelium.

We agree with Michelson's comments about the possible effect of filters in lowering exposure of the smoker to polonium or any other component of smoke, and indeed it is no trick to remove mainstream smoke completely. Whether a relatively "smokeless" cigarette will ever be acceptable to the public is questionable. To standardize comparisons of smoke obtained artificially from various brands of cigarettes, it is important that either the amount of tobacco consumed be the same or the length of butt remaining be constant. Both criteria have been proposed for testing purposes, and we are grateful to Michelson for sending us information concerning cigarette-testing methods. Neither of the above standard criteria was met in our studies, which were designed merely to show that polonium did get into the smoke and that we could account reasonably well for the polonium lost from the cigarettes during smoking. For this reason we knew comparison of results from the four brands would be meaningless. It may be that some kinds of cigarette filters may prove somewhat effective, but we do not believe our results should be used as evidence concerning filter efficiency.

Michelson's letter and the preceding comment raise an important point, we feel, for the scientific community as a whole. The cigarette industry is a multibillion-dollar one, and the stakes are high in terms of the relative sales of different brands of cigarettes. As a result of the Surgeon General's report and the rules for advertising cigarettes proposed by the Federal Trade Commission, it appears likely that increasing emphasis in cigarette sales promotion will be placed on alleged differences in composition of the smoke from the different brands and possible biological consequences of these differences. It is also likely that scientists will be asked by the press to give their opinions concerning the merits of these claims. In the past, evaluating such claims scientifically has been difficult enough, even though they have generally been based simply on amounts of "total tars" or total smoke condensate attributed to cigarettes of the particular brand. As an example of one difficulty, if it is true, as we suspect, that generally a greater amount of tobacco is consumed and a shorter butt results from smoking filter cigarettes than from smoking nonfilter cigarettes, what then is the validity of claims made on the basis of a constant amount of tobacco consumed in tests? When claims are made for *selective* removal of a particular component in smoke or combustion gases from a cigarette, moreover, the scientist should have an especially firm basis on which to judge the claims. In our opinion, the most important basis of judging is quantitative chemical analysis of smoke and other components of the particular brand compared with all other major brands, especially those most closely similar to the brand under discussion; these analyses should preferably have been confirmed in a laboratory independent of the particular company manufacturing the cigarette. If a specific biological effect is claimed for removal of the material from smoke, the theoretical or experimental basis of the effect should be in the field of competence of the scientist.

Finally, because of the great economic implications of remarks made by scientists concerning the relative merits of one cigarette brand or another, we suggest that anyone making such a statement identify himself more completely than is usually necessary. For his own protection, as well as for the good name of the scientific community as a whole, the scientist should, we believe, give at the same time his past or present affiliations with any of the tobacco companies or with the tobacco industry as a whole. Our support has been primarily from the Atomic Energy Commission, the U.S. Public Health Service, and Harvard University. We wish to make part of the public record the fact that we do not now have any financial support from the tobacco industry or a particular cigarette company, nor have we had such support in the past; neither have we acted at any time as consultants to any tobacco company or to the tobacco industry.

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Legislation for Humane Treatment of Laboratory Animals

Elinor Langer's story on humane laws (24 Jan., p. 339) misrepresents the Clark-Neuberger bill on two points. It did not indicate that this bill resembles the proposals of Congressmen Fogarty and Roberts in that it would affect all federal agencies and all recipients of federal grants or contracts. And the Clark-Neuberger bill is not the "strongest" of the many bills pending. Surely, this position is held by the Randall bill, which defines stress and pain and specifies who can administer an anesthetic to animals. The terms of the Clark-Neuberger bill are broader, as is the British legislation upon which it is based.

If the British experience affords a guide, the provision in the Clark-Neuberger bill for unannounced inspection of animal quarters should help considerably to establish reasonable standards of housing. Under the British system, institutions are visited by inspectors (all of whom are M.D.'s) an average of three times a year, although, in fact, reputable laboratories may be visited infrequently