Who Should Teach Engineering?

Anyone interested in the undergraduate education of engineers cannot but be disturbed by the conflicts that complicate the nation's technical-manpower problems. In the face of the competition among engineering schools for faculty and the heavy demand for research talent by government agencies and private enterprise, educators are pressing for more Ph.D.'s to teach undergraduate courses in engineering. Some researcher ought to add up the number of engineering teaching jobs to be filled and compare it with the number of Ph.D.'s who are making a career of teaching engineering. There is nothing like pressing for the impossible.

Only about 25 percent of the Ph.D.'s in engineering take the examinations to qualify for the P.E. (professional engineer). There is little incentive for them to do so. Their future is in research, in graduate teaching, or in specialized administrative positions. Thus about 75 percent of the Ph.D.'s in engineering cannot engage in private engineering practice or approve engineering work for employers or clients. In other words, professionally and by law they are not considered to be responsible engineers. A Ph.D. in engineering considers himself to be more a scientist than an engineer. This raises the question, Who are better qualified to teach undergraduate engineering—scientists or engineers?

There is little or nothing in the acquisition of a Ph.D. that trains an engineer or a scientist to be a good teacher. The medical profession gives little or no weight to the Ph.D.; there are so few M.D.'s with Ph.D.'s that the AMA does not deem it worth while to keep a record. Medical teaching is done by those who have qualified or who have built a reputation by accomplishment in practice. This is the academic philosophy engineers feel should be applied to their profession.

The inducement to study engineering is to practice engineering, not to become a scientist. Enrollment in engineering has been practically static since 1956; we read in the papers that "The attrition rate of engineering students in American colleges is approximately 54 percent. In some state universities only 20 out of 100 engineering students get their degrees." A primary cause of dropouts, in my opinion, is the heavy dose of science courses—courses that are not well taught. (A Ph.D. teaching subjects at this level is bored to death.)

Perhaps we need to be reminded of the circumstances that led to the founding in 1932 of the Engineers' Council for Professional Development. It was instigated by young engineers who had taken their undergraduate training during the early 1920's, when the engineering curriculum was in its first cycle of intensive concentration on technical and science courses with a minimum of courses in the humanities. Graduates of this kind of curriculum found themselves on a dead-end street. They were not being given serious consideration for the kind of management and executive jobs they had believed themselves to be qualifying for by studying engineering. Now engineering education is again in a cycle of technical saturation. Yet the products of scientific research need to be developed, designed, and produced by a profession that is socially and politically well oriented. It is estimated that there are only 64 employers that can profitably use the science-oriented engineers that engineering schools are being pressed to produce. Many employers of engineering talent are becoming concerned about the present trend.

The engineering profession, if it is to solve such problems as this, needs a coordinated and unified leadership. This is not being supplied by the engineering societies, of which there are over 120. There is little direction or guidance in the professional publications. Perhaps the current investigation of the nation's research and development programs by Congress will help to produce it.

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Population Problems and Infectious Diseases

H. Frederiksen, in your issue of 17 January, comments on the National Academy of Science report "The Growth of World Population" (*1*), with particular reference to Ceylon. I recently analyzed and reported data (*2*) which I collected in Ceylon in 1956–58, reaching the following conclusions: (i) The rate of increase of population in Ceylon jumped markedly immediately after World War II. (ii) This increase can be explained by the decreases in deaths from the principal infectious diseases. (iii) These reductions in death rates were caused by the application of new methods of prevention and treatment of infectious diseases of all kinds, not only malaria. (iv) The incidence of malaria in Ceylon, as well as its effect upon the population size, has been grossly exaggerated. Probably half the illness ascribed to malaria in the days before DDT was in fact nonplasmodial. This illness still exists today after the malaria has practically vanished, only now it is called influenza. Studies in 1957 showed that much of this "influenza" was caused by arbor viruses, particularly dengue.

During 2 years in East Pakistan (1958–60), I concluded that if the health and environmental projects planned for that province were put into effect, especially that for the provision of a tube well for each village, the rate of population increase for that province would parallel that of Ceylon and would rise in 10 or so years from 1.8 percent per annum to 2.8 percent (*3*). In fact, if an extra 4 or 5 rupees per annum per head were made available in most countries in southeast Asia, and the money were spent on preventive medicine instead of hospitals, the rate of increase of population in the whole of this area could be raised rapidly to close to 3 percent per annum.

Population increases of this magnitude pose many problems. Since the rate of increase of productivity of this part of the world is unlikely for some time to exceed 2 percent, there is every prospect that the welfare of the people will go backward instead of forward. Birth control is not the answer at present; for the next decade or two its effect will be scarcely comparable to the opposing effects of preventive medicine. Some major scientific breakthrough in this field will be necessary before a deliberate major reduction of the birth rate will be practicable. Nor can the benefits of public health and medical care be denied to the people of any country for, as the example of Ceylon shows, when the people realize that scientific treatments can alleviate their sufferings, they demand them and the government has to supply them. No leader would dare tell his people that every year tens of thousands of them must die of leprosy, tuberculosis, and so on, just because productivity cannot increase fast enough.

Infectious disease is obviously one of the main factors limiting size of population, and indeed may be the controlling factor in areas where the food supplies are above the critical minimum. The various infections can usually be eliminated at very small cost, but the people who have been thus saved from dying will need food, clothing, homes, and work, which may not be available if there are too many of them. In many parts of the world today it is argued, "Why should a baby be saved from some infection soon after birth only to die of hunger 10 years later?" This kind of question determines the low priorities given to health programs in many countries and is reflected in much of the thinking of those allocating U.S. funds for assistance to foreign countries. The counterargument is that we cannot let die millions of people who could be saved at little expense, that progress at such a price is too expensive.

This is truly one of the great dilemmas of our time.

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2. T. A. Cockburn, *The Evolution and Eradication of Infectious Diseases* (Johns Hopkins Univ. Press, Baltimore, Md., 1963).
3. ——— "Infectious disease and the population of East Pakistan," *Seminar on Population Growth and Economic Development* (Institute of Development Economics, Karachi, 1959), pp. 297-302.

Cigarettes: Polonium-210

Radford and Hunt's report [*Science* **143**, 247 (1964)] that polonium-210 in cigarette smoke may be a significant factor in the genesis of bronchial cancer in smokers is a distinct contribution to this controversial subject. However, their conclusion that no significance can be attached to differences between filter and nonfilter cigarettes is open to question. Their data showed that filter cigarettes yield 28 percent less polonium in the mainstream smoke (that part of the smoke which goes into the smoker's mouth) than nonfilter cigarettes, but they explained away this difference as not being related to the action of the cigarette filters. However, it may be more than a coincidence that the yield of smoke particles from such cigarettes was found to be in a similar ratio by Consumers Union [*Consumer Reports* **26**, 207 (1961)]: filter cigarettes yielded 31 percent less smoke particles (by weight) than nonfilter cigarettes. The connection between the cigarette filters and the lower yields of both tar and polonium is supported by Radford and Hunt's own statements that (i) polonium is adsorbed on smoke particles and (ii) their smoke-collection filters collected both the smoke particles and the adsorbed polonium. Why should the cigarette filters act any differently from their collection filters, except for differences in efficiency?

The close agreement between the Radford and Hunt polonium data and the Consumers Union tar data may be fortuitous; a better comparison would have been afforded if Radford and Hunt had given the mainstream smoke yields for each brand they tested.

One might argue that the 28-percent reduction, even if actually due to the cigarette filters, is too small to show that cigarette filters may have value in reducing the hazards facing smokers. Such a conclusion is not warranted from the Radford and Hunt data, which compared only two filter cigarettes with two nonfilter cigarettes. Consumers Union's 1961 tests revealed that some king-size filter cigarettes yield as much as 70 percent less tars than several other popular brands. One newly introduced cigarette seems to afford a reduction of 85 percent on this basis. If the polonium-210 yields are similarly reduced, one might conclude that cigarette filters *can* effect a significant reduction in hazard. A large segment of the population will go on smoking no matter how much proof of hazard is presented. Many of these people would smoke less-hazardous cigarettes if they knew which these were. They should not be told on the basis of inadequate evidence that there are no differences among cigarettes when there may, in fact, be large differences. More testing to resolve this problem would be most appropriate.

The cumulated radiation dose from polonium-210 was calculated by Radford and Hunt to be about 36 rem in 25 years (for two-pack-a-day smokers) as the minimum, and 100 or more rem as the more realistic figure. But their further estimate of the possibility of 1000 rem or more in local hot spots in bronchial epithelium is hardly supported by the case they cite of a hot spot in one subject, who, they estimated, would have received a dose of 164 rem in 25 years from the hot spot they located. Their guess that they would have found a substantially higher concentration 10 days earlier, before the subject stopped smoking, is contradicted by the 138-day half-life of polonium-210 and the probable absence of ciliary action in an individual who had smoked heavily for many years and who was hospitalized for smoke inhalation.

Their concluding view that polonium-210 is only one of the many factors which play a part in the genesis of bronchial cancer in smokers seems much more reasonable than the earlier implication that polonium-210 may be the major factor.

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Freedom in Large Laboratories

The increasing concentration of scientific research in large laboratories may have a tendency to impede progress by stifling the creativity of the individual scientist, as Paul M. Gross suggests [*Science* **143**, 13 (3 Jan. 1964)]. How serious this danger is I do not know, but the example he uses on page 16 about the discovery of xenon tetrafluoride serves rather to indicate the opposite from what is implied. In the first place, the "young physicist" was not working alone but had for years been collaborating on a part-time basis with a group of fluorine chemists at Argonne, and it was with this group that the preparation and identification of the first simple noble-gas compound was accomplished. More to the point, at this large laboratory, the Chemistry Division of the Argonne National Laboratory, it is the policy to give freedom to the individual scientist or group. This particular group proceeded in this and many previous projects without being required to obtain approval by supervisors. Whether such freedom is infrequently allowed in other large laboratories, I am not sure, but in this case I am intimately familiar with the circumstances, as I was the "young physicist."

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