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Breeding with tokamaks

The articles on Soviet physics in your November issue are excellent. The authors and your staff merit congratulations for the informative, justifiably laudable and polite style in which the material is presented.

In view of worldwide emphasis on fusion as an ultimate and inexhaustible source of energy, the last paragraph in the fusion article merits the most serious attention. I believe it says that the Soviets are deferring their goal of constructing tokamak reactors because their technological feasibility is still not established and, even if such reactors were scientifically feasible, no one knows how to surmount the foreseeable materials and engineering difficulties. We have here a classic example of the fallacy of proceeding to engineering before the basic knowledge base has been built. As an interim goal the Soviets are now considering using tokamaks to produce neutrons for breeding Pu and/or U^{233}, because the difficulty of achieving the economic "break-even" point is thought to be less severe for this modality of operation.

Over the past several decades we have seen an interesting interplay between the US and USSR efforts in fusion. We invented stellarators and abandoned them only to see them assiduously pursued in the USSR. The Soviets invented tokamaks, are temporarily abandoning them as far as original goals are concerned, while we are pursuing them with great enthusiasm.

I think it might be a great loss were we to proceed into large-scale endeavors to breed fissionable material, with devices as uncertain and as difficult as tokamaks, if this occurred at the expense of performing the really basic research that is necessary at least to determine whether controlled fusion reactors are indeed feasible.

Before we follow the Soviet lead in the direction of tokamaks as "breeders," the following additional points are worth discussing.

Deuterium–tritium fusion reactors are attractive because they require much lower plasma temperatures than do D–D reactors, and because they can breed at least as much tritium as they consume.[1] However, you cannot use the same neutron for breeding Pu or U^{233}. You may have one or the other. A Pu atom is worth about 180 MeV, including two and one-half 1-MeV neutrons. (Also, 14-MeV neutrons interacting with U^{233} give rise to neutrons of average energy about 1 MeV.)[2] A tritium atom is worth about 18 MeV including one 14-MeV neutron. This looks like you can make a large profit by breeding Pu and then making up the tritium by irradiating Li in a reactor. However, there are some penalties. Fourteen-MeV neutrons can convert Li^7 into tritium[1] while 1-MeV neutrons cannot. They require, for tritium production, the much rarer isotope, Li^6.

If breeding fissionable material is to be the name of the game, would it not make sense to do this with D–D neutrons, which can be obtained (perhaps quite economically) from explosively driven fission–fusion devices? One thereby avoids both the high cost and high radioactivity associated with large tritium inventories. Our technology for underground nuclear explosions is far more advanced (it even works safely) than our CTR technology. Should we not at least be prepared to use that technology, to breed Pu and U^{233} in case all else fails?

Going one step further, since reactors will be required to utilize the fissionable material, however it is produced, how about building breeder reactors? We might even consider giving them French names, since those with French names appear to work.

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Improving peer review

Several years ago, on behalf of the Netherlands government, I studied review methods practiced by a number of US research agencies, including NSF, AEC, and NIH. Since NSF provided host facilities for me, it was inconvenient to concentrate most of my efforts on NSF itself. At the time I was impressed by the skill with which program officers especially in the physics section of NSF carried out their task. They were extremely well acquainted with their fields and had thorough methods of continuously finding new names to add to the list of future referees. If necessary, they made use of foreign specialists and in *their referee files* I hap-

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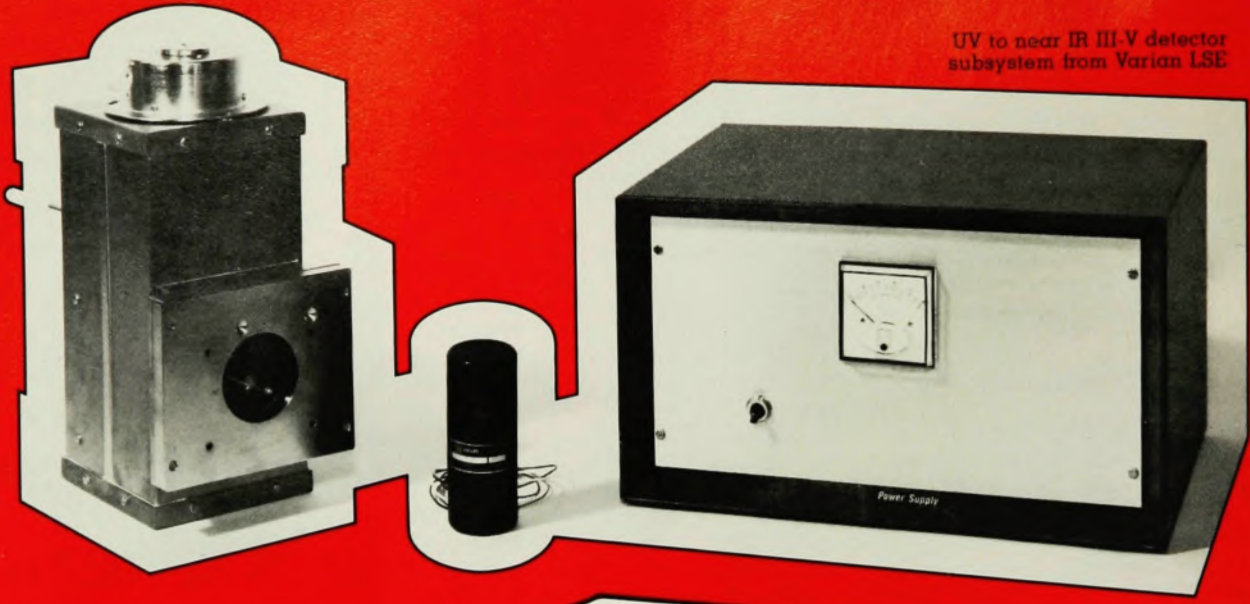

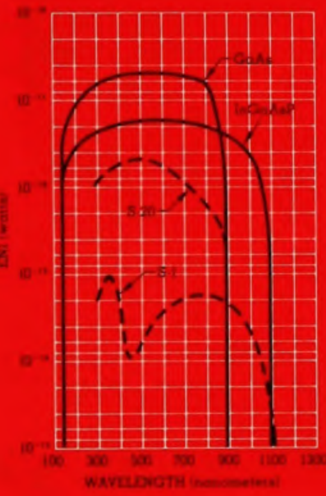

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pened to discover several names of my fellow countrymen we consider to be top physicists. I checked with numerous scientists in the field and found a general confirmation of my own first impressions of the Federal offices. The system was fair, it was skillfully administered, and the physics community seemed to have great confidence in it.

When I returned to Holland, where the review of grant proposals was done by committees with all those involved present, we tried to incorporate some NSF techniques in our system. (My study was triggered by the feeling in Holland that current techniques, fit as they were at times with rapidly growing funds, completely broke down when the budgets were frozen and extra support to Dr A would mean less support for Professor B.) We merely used the comments and criticism obtained through mail review to initiate debate in the committees where silence would have prevailed otherwise.

Since then much change has taken place. As in NSF not all our decision making is uniform for different programs. However, there is one characteristic that is very common to our review system, which I think is missing in US procedures. After we have collected all comments and criticism on a proposal through mail review and other means, we sum them up in a so-called "protocol", which may be a document of one or more pages. These protocols do not mention names of advisers. We keep these off the record just as NSF does. I believe names are unimportant in most cases, primarily the contents of the statements should be counted. Eventually the difficult job of weighing the advisers' judgment is done in the preparation of the protocol. To make sure this summary of advice does not contain mere mistakes or biased notions and so on, we allow the principal investigators to learn the contents of these papers. They have the opportunity to deny certain points or defend themselves as they may deem necessary. These comments are added to the protocol before it is sent to the committee which makes the final decisions.

After all, the common practice even before Justitianus authorized the Roman Code of Justice was to allow the accused to learn about the accusations and to defend himself before the sentence was passed. For this reason I feel NSF procedures could be improved and that is also why I do not understand NSF critics who believe the judgment by referees should be made public after a decision is made. What use is the knowledge of grounds for refusal after refusal?

Our experience in Holland with the new method is favorable. We do not have serious problems with either the loss of confidence in our advisers or a rise of tensions within our scientific community.

A last point I would like to make concerns the difficulties Stever mentioned (September 1975, page 77): "The quality of rejected proposals had improved significantly, because of the current squeeze on basic research" (stress also results from this situation). In our system final advisory bodies deal with the completed protocols and then hand out grade marks to the proposals. Although it does not help the proposer of a rejected request much in practice, since a "no" still remains a "no," he feels better on hearing that his grant is refused because it was "very good" (and not because it is considered lousy research) and that budget constraints allow only for those graded "excellent" to be supported.

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Physicists as engineers

I should like to endorse the idea put forward by Charles Riedel in your January issue (page 96) that physicists should consider employment in an engineering field. I myself began to make that transition many years ago soon after graduation, when I applied for membership in one of the engineering institutions. Passing the Engineer-In-Training and Professional Engineer examinations should not be difficult for a physicist after a review of the subject matter. (Though this can conveniently be done by attending a formal review course, it is possible to study the material at home.) Registration provides the degree of "respectability" that the engineering employer desires, though for most types of employment it is not a legal requirement. I have found that my physics background, which inclines me to work everything out from first principles, sometimes has given me an advantage over my colleagues when something out of the ordinary has to be done, though they are quicker when it comes to applying existing technology. Perhaps this is the principal difference between a physicist and an engineer; the former is brought up to believe in a quest for knowledge for its own sake, the latter to apply existing knowledge to everyday problems (including that of earning his own bread and butter).

Riedel does not mention classical physics and acoustics in his list of parallel subdisciplines. Acoustics, with its related fields of vibration and noise control, though sadly neglected by many physics departments, is providing opportunities for both physicists and engineers.

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