scientists are mere tinkerers who have no responsibility for the ultimate uses of their ever more devastating weapons of war. Is it necessary to remind you again of the collective guilt conscience possessed by many developers of the nuclear bomb?

Yes, let us continue the development of a superior armament. But let us also accept the responsibility for its application. And, finally, let us not be deceived by a "detente" that compromises both these premises.

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After paging through the special November issue with considerable interest, I was astonished to realize that in only one place was there even an indirect hint of the existence of a problem that must surely be exercising the consciences of many of the authors who so praised the scientific and humanistic benefits of US–Soviet scientific cooperation. I refer to the continuing, and even intensifying, harrassment by the Soviet authorities of large numbers of Jewish scientists who have been dismissed from their posts, refused access to scientific institutions, conferences and symposia and reduced to the status of non-persons because they have requested (and been refused) permission to emigrate. In several of these cases, far from protesting or attempting to ameliorate the condition of the victims, the Soviet Academy of Sciences has been in the forefront of the harrassment. These facts have been widely documented, on a number of occasions in the pages of PHYSICS TODAY, and it is both surprising and regrettable that you should have failed to take advantage of the opportunity to remind our scientific colleagues in the Soviet Union that science, as a humanistic enterprise, cannot ignore blatant violations of elementary human rights, and that a scientific exchange program built on turning a blind eye to the vindictive persecution of a segment of the Soviet scientific community is perhaps not worth the moral price paid.

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Mirrors: past and future

The October issue (page 20) had an interesting "Search and Discovery note" by MSR regarding significant results from the 2XIIB mirror device at Lawrence Livermore Laboratory ($T_i \approx 12$ keV, $n \approx 4 \times 10^{13}$, $\beta \approx 0.4$, $\tau \approx 0.005$ sec).

Other mirror experiments in the past years have shown comparably important technical achievements. DCX-1 at Oak Ridge demonstrated 150-sec confinement time for its 300-keV protons, but at very low densities ($\approx 10^7$ cm^{-3}); however, unlike the storage rings (which confine relativistic particles for days) the DCX-1 protons were embedded in a realistic plasma with the Debye polarization length less than the plasma size. DCX-2 showed somewhat higher densities ($\approx 10^{10}$ cm^{-3}) for its energetic protons but had shorter confinement times (≈ 1 sec). After beam turn-off both DCX-1 and DCX-2 plasmas were relatively quiescent with charge exchange the dominant loss process. The ELMO mirror device at Oak Ridge generated MeV electron temperatures produced by resonance microwave heating of the electrons at or near the electron gyrofrequency. ELMO densities were 10^{11}–10^{12} cm^{-3} but the ions were quite cool; the mean confinement time for the relativistic electrons exceeded 1 sec and the plasma $\beta = 8\pi nkT_e/B^2$ exceeded 0.5 on occasion.

Phoenix II at Culham, UK, demonstrated an incipient onset of exponential build-up (plasma trapping rate greater than the charge-exchange loss rate) and Baseball II at Livermore exhibited an actual exponential build-up of the proton density; however, the exponential condition obtained in both cases only for the very small population of highly excited, quasi-metastable atomic states of H^{0*}, which population was rapidly depleted as the plasma density increased. Trapping by Lorentz force ($\mathbf{q}v \times B$) ionization of excited atoms or dissociation of excited molecules led to important physics studies in Phoenix II, Baseball II and DCX-1. All these mirror-confined plasmas were remarkably stable after beam turn-off. Some of the mirror systems were operated steady state.

In view of the significant range of physical parameters (T, n, β, τ, exponentiation, Lorentz trapping, microwave heating of electrons, steady-state operation, plasma quiescence) it would appear eminently desirable to look at the future prospects for mirror devices. An important scaling law for Coulomb-scattering-limited, mirror confined plasmas ($n\tau \propto 1/\sigma v \propto E_0^{3/2}$) was demonstrated at Livermore in 2XIIB. This scaling law suggests that more than a 500-fold gain in $n\tau$ might be attained by injecting at $E_0 \gtrsim 1$ Mev instead of 15 keV, provided one could utilize the exponential or Lorentz trapping principles, together with electron cyclotron heating to reduce stopping power losses on cold electrons and with low background pressure to reduce charge-exchange losses.[1] Nicholas Christofilos's dreams of producing a relativistic electron E-layer to close a magnetic mirror was eventually achieved at Cornell—perhaps another of Christofilos's dreams, the relativistic proton E-layer, might be achieved somewhat more gently than he had envisaged and with the important result that mirror-fusion reactors might become a reality in the not too distant future.[2] The high-β, high-temperature mirror confined plasmas

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may also lend themselves to utilization of the advanced fusion fuels, DD and D^6Li in a radiologically cleaner, structurally simple, steady-state fusion reactor.[3]

It would appear that more emphasis should be given to the theoretical and experimental study of magnetic-mirror devices, especially at MeV and GeV injection energies for start-up. The solution to CTR will not be easy but it's worth the candle!

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1. J. Rand McNally, Jr, ORNL-TM-4967 (1975).
2. J. Rand McNally, Jr, ORNL-TM-4965 (1975).
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Monopole in 1927

In connection with your report of the possible discovery of the magnetic monopole by the Berkeley team of P. Buford Price, Edward K. Shirk, W. Zack Osborne and Lawrence S. Pinsky (October 1975, page 17) it is of interest that Chester Snow was able to manipulate the Maxwell–Lorentz equations to show that if every electric charge in the world be given a charge of true magnetism, their ratio being constant, the change could not be detected. The references is "Indeterminateness of Electric Charge," Bureau of Standards Scientific Paper No. 566, Washington, D.C., November 1927.

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Unemployment problem

The statements and evaluations of M. L. Perl and R. H. Good in their letter, "The Permanent Career Problem and Some Partial Solutions" (June 1975, page 15) describing the purpose and results of the Conference on Tradition and Change in Physics Graduate Education, bid a reply. Perl and Good state that the purpose of the Conference was "to study the relation of physics graduate education to changes" occuring in the physicist's environment. One change is "the end to two decades of rapid growth in the size of the profession" and the other is the "increased interest" of the physicist in socially oriented research such as environmental and energy problems.

The establishment of the purpose of the conference on the premises stated is far from adequate to form a basis for the physicist's career problem. A study of the change in the physics profession over the past 20 years cannot result in solutions of the physicist's unemployment problem, for the problem has been present long before this. The funding of physics R&D projects increased during the "Sputnik" era and decreased during the Nixon cutbacks. The physicist's unemployment dilemma is not based on what occured to physicists during the past two decades but is based on the economic principle of supply and demand. The conference would have been far more fruitful if its purpose was to study the methods of increasing the demand for physicists.

The second stated purpose of the conference, that is, to study the relation of physics graduate education to the increased interest of physicists in environmental, energy, arms race and policy problems, is not a problem basic enough to supply practical reasons for the physicist's unemployment enigma. For, by virtue of economics, physicists are being forced to enter fields that the government is interested in. The conference would have been more enlightening if, as an additional purpose, it studied why the physicist is limited in economic or work opportunities.

The use, by Perl and Good, of the terms "traditional" and "nontraditional" to define a difference between careers such as elementary-particle physics, solid-state physics and nuclear physics as opposed to careers such as biophysics, medical physics, and geophysics is misleading and incorrect. The statement that the latter fields are "nontraditional" leads to the conclusion that these fields are not handed down from previous physicists. In reality these careers involve physics as much as the former ones do. The science of chemistry has been developed to its present advanced stage because of the great advances of the physicist in electric field theory and atomic theory. The statement that a physicist working on studies of electrochemical potentials is in a "nontraditional" career is forgetting the idea that a physicist is fundamentally a philosopher, that is, a lover of wisdom, and that knowledge is not limited to certain fields of study to the exclusion of other fields of study. Medical doctors are delighted to do "nontraditional" work like using the electronic analytical instruments and radiation therapy machines developed by physicists. The true distinction of the caliber of a physicist's work is in how original his contributions are, and not whether he works on solid state in the form of transistor or a liver cell wall. Most physicists find contentment in their "nontraditional" fields when they see the "real" money that they receive from working in them.

A far more meaningful conference would have resulted if, in addition to the above considerations, a purpose of the conference was to study the methods by which an American Physics Association could be formed and how the Association could advance the economic situation of the physicist by means of public relations men operating to promote federal legislation favoring physicists.

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Highly excited atoms

I read the article on highly excited atoms in the November issue (page 17). I observed the ionization by electron impact of Rydberg atoms during the work for my PhD thesis. This was the measurement of the ionization cross-section of H_2S metastable atoms by electron impact. The experimental setup consisted of a beam of ground-state hydrogen atoms, some of which were excited to the 2S metastable state in a first electron gun and then selectively ionized in a second electron gun. The measurements were troubled by a rather strong background signal, which we finally identified as the ionization of highly excited atoms by electron impact. We were able to observe reasonably strong signals at ionizing electron energies down to 1 eV despite the fact that the electron current in the gun was very low. We concluded that the cross section for ionization had to be very large, and estimated it to be around 10^{-12} cm^2.

We reported our results at the meeting of the Swiss Physical Society in 1969. Since I am not working in this field anymore, I would be very much interested to know if anybody measured ionization of Rydberg atoms by electron impact.

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More on waste disposal

In your January issue you published an article-length letter from Bernard Cohen entitled "Environmental Hazards in Radioactive Waste Disposal" (page 9). This letter is an abridged version of a preprint being circulated by Cohen which, as stated in the footnote to his letter, has also been submitted for publication in *Reviews of Modern Physics*. I share Cohen's hope that high level radioactive waste, after being solidified in a form with low leachability and buried in an appropriate deep geological site, may well represent an acceptably low hazard to society. After studying his detailed arguments, however, I find that they contain so many major