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As well as suggesting priorities, the Bromley report sketches the drastic consequences of cuts such as those we face in nuclear sciences for FY 1977. But nuclear sciences is not alone. Government support of basic research has been declining steadily since 1967.[2] We firmly believe the time has come to promote aggressively public and official appreciation of basic research lest its accelerating strangulation continue.

We strongly urge that future funding for all basic research, and nuclear sciences in particular, not be allowed to fall below that of FY 1976, adjusted for inflation.

Feeling as we do on the above issues, we share the concerns expressed by C. S. Wu in her letter to the President.[3] Therefore, we ask you to join us in sending letters of this type to officers of The American Physical Society, Administrators in ERDA and NSF, and to members of Congress.

References

1. Physics Survey Committee, Part II (1973), page 370 (the "Bromley Report," chapter on "Consequences of a Declining Budget").
2. Editorial by C. S. Wu, PHYSICS TODAY, December 1975, page 88.
3. Letter by C. S. Wu, PHYSICS TODAY, January 1976, page 99.

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Limited nuclear war

In an obvious effort to make the limited-war doctrine more acceptable to the developing nations and to others who are convinced that a nuclear war threatens all human life on the planet, the US government requested the National Academy of Sciences to report on the "Long-Term Worldwide Effects of Multiple Nuclear Weapons Detonations" and a study bearing that title was released 4 October, 1975.

The Arms Control and Disarmament Agency, which financed the study, instructed the special committee formed to conduct the study to assume that the entire nuclear exchange would be limited to explosions in the northern hemisphere between latitudes of 30 and 60 degrees in the lower atmosphere over land and that no account be taken of the immediate loss of human life. Such questionable assumptions forced the committee to eliminate from its considerations any possibility of nuclear strikes at surface or underwater naval vessels or space satellites. Also excluded were all military bases or allied nations outside the above-mentioned latitudes. In other words, since the ground rules for the study were apparently based on the Schlesinger doctrine of limited nuclear war, even if they were not spelled out in those terms, it should be no surprise that the study concluded in the words of NAS President Philip Handler "in a decade or so after the event, in areas distant from the detonations, surviving humans and ecosystems would be subject to relatively minimal stress attributable to the exchange."

To minimize the impact of a nuclear exchange on food production, the report states that "Bell and Cole estimate it would require 11 years to rebuild beef and dairy cattle (in the US) in numbers to former levels following a 90% loss of female breeding stock. Similarly, sheep would take seven years, swine one and one-third years and poultry one-half year to rebuild after similar destruction" (page 96). Since the committee was not allowed to make any assumptions about the loss of human life in the nuclear detonations, it did not have to worry about who would organize the breeding. Perhaps the poultry, cattle, sheep, etc., would breed themselves like the Shmoos in the Li'l Abner comic strip.

The report can thus conclude in the paragraph following the one just cited: "It is reasonable to assume that mankind in combatant and noncombatant nations would recover from a nuclear war much as it recovered from other major disasters .... The most productive land would probably be occupied and tilled within a short time after a nuclear exchange."

It is shocking that a number of scientists, many of whom are distinguished in their fields, should find themselves taking part, even unwittingly, in the preparation of this pseudo-scientific, Strangelovian report.

It is the responsibility of scientists to alert the peoples of their countries to the dangers from continued escalation of the arms race, to the dangers from nuclear arms proliferation, and to the dangers of the Schlesinger limited-war concepts. The National Academy of Sciences should be devoting its efforts to meeting these dangers and not to selling the public on the safety of nuclear war.

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Monopole debate

I was dismayed to find a statement in your October issue to the effect that "... Julian Schwinger ... developed a consistent theory of monopoles." This statement completely disregards my proof of the noncovariance of the Dirac

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monopole,[1] and thus represents an editorial judgment that I believe to be inappropriate to the pages of PHYSICS TODAY. For ten years I have refrained from engaging in polemics concerning this issue in the naive belief that physicists working in this area would see the difficulties in Schwinger's theory without the need for possibly bitter debate. Having seen the error of my ways I wish to point out here three reasons for skepticism concerning Schwinger's result.

The operator that is to effect the change from one singularity line to another in Schwinger's theory is formally unitary but does not appear to exist as a well-defined operator. Unless such existence can be shown the covariance "proof" is no more than conjecture.

The limiting argument given on page 1091 of Schwinger's paper[2] is clever but not correct. Though it is true that Schwinger's observation that currents should be defined as limits rather than naive operator products had done much to increase our understanding of quantum field theory, one must realize that the new limit proposed by Schwinger goes far beyond the mere assertion that currents are to be defined as limits. In fact the limit proposed by Schwinger

$$-\bar{\psi}(x)\gamma\cdot\nabla\psi(x) = \lim \bar{\psi}\left(x+\frac{1}{2}\epsilon\right) \times \frac{3\gamma\cdot\epsilon}{\epsilon^2}\psi\left(x-\frac{1}{2}\epsilon\right) \quad (1)$$

(written here for $e = 0$) is valid only in the case that the bilinear $\bar{\psi}\psi$ is regular in the limit $\epsilon \to 0$. This is known not be be the case and indeed the general failure of equation 1 can be shown for a free field by elementary calculation.

Finally, mention should be made of a paper by Daniel Zwanziger,[3] which gives a set of conditions required for monopole covariance at the end of Section VI which he freely acknowledges to be contradictory. Inasmuch as the author claims to have demonstrated equivalence to Schwinger's result, one has here a further indication of difficulty in the allegedly consistent theory of monopoles.

To conclude this letter I will confess that I fully expect that my paper will continue to be conveniently ignored by many physicists who find this field a quick and easy source of publishable calculations of semiclassical monopole phenomenology. However, it might not be too much to hope that institutions such as the AIP will in the future seek to avoid editorial conclusions based on only a part of the available evidence.

References

1. C. R. Hagen, Phys. Rev. **140**, B804 (1965).

2. J. Schwinger, Phys. Rev. **144**, B1087 (1966).
3. D. Zwanziger, Phys. Rev. **D3**, 880 (1971).

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AUTHOR COMMENTS: It has been almost ten years since I abandoned the methods of operator field theory, for reasons not unrelated to the mathematical ambiguities and obscurities that so exercise Hagen. I believe there is a "consistent theory" of magnetically charged particles, but it requires the nonoperator, phenomenological language of Source Theory. That development of magnetic charge theory began in Phys. Rev. **173,** 1536 (1968) and has been reviewed recently in Phys. Rev., 15 November 1975; it seems to have escaped Hagen's attention.

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AUTHOR COMMENTS: As I am cited by Hagen at cross-purposes to my meaning, and because magnetic-monopole theory does have many subtleties, it may be helpful if I describe what is currently known about the consistency of the theory of magnetic monopoles.

That the classical nonrelativistic and relativistic theory of electric charges and magnetic monopoles is consistent does not appear to be in doubt. The issue is more difficult in quantum theory, where it is customary to introduce a vector potential A whose curl is the magnetic field $B = \nabla \times A$ and for which one would also like to have a source $\nabla \cdot B \neq 0$. In fact unless the electric and magnetic charges e_i, g_i satisfy a quantization condition $(4\pi)^{-1}(e_i g_j - g_i e_j) = n_{ij}$, the quantum theory cannot be reconciled with rotational invariance. However, if n_{ij} is integral or half-integral, then the nonrelativistic quantum theory has been shown to be consistent in the beautiful but often ignored work of C. A. Hurst[1] and there is no difficulty in calculating energy levels and cross sections.[2] In the case of the relativistic quantum field theory of magnetic monopoles, Hagen is quite right in asserting that no proof of consistency exists, and supposed proofs are only formal. Actually, even for ordinary quantum electrodynamics no proof of consistency exists, and further difficulties and paradoxes do accumulate if magnetic monopoles are added. (No perturbative argument may be used, for that would require consistency for all values of the magnetic coupling constant between zero and the first allowed nonvanishing quantized value, which is not expected.) However Hagen's asserted proof that monopole theory is inconsistent is in

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fact a proof that a theory of his own invention is inconsistent, but this theory does not have the correct classical limit for it to be regarded as the quantum field theoretic generalization of a classical magnetic-monopole theory. On the other hand it is true that the kinematics of relativistic scattering amplitudes for electric and magnetic charges is quite consistent if n_{ij} is integral or half integral although it has its own peculiarities.[3] For example the decay of particle 1 into particles 2 and 3 with spins s_1, s_2 and s_3 is forbidden unless $s_1 + s_2 + s_3 \geq (4\pi)^{-1}|e_2g_3 - g_2e_3|$. The usual connection of spin and statistics is lost unless n_{ij} is integral.[3]

References

1. C. A. Hurst, Ann. Phys. **50, 51** (1968).
2. D. Zwanziger, Phys. Rev. **176,** 1480 (1968).
3. D. Zwanziger, Phys. Rev. **D6,** 458 (1972).

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Space colony society

It may be of interest to the readers of PHYSICS TODAY, the magazine that first published G. K. O'Neill's description of how the L-5 point of the Earth-Moon system can be colonized (September, 1974, page 32), that an L-5 society has been formed. The expressed purpose of this society is to disband the society at a mass meeting on a space colony at L-5. Membership dues (regular $20.00 or student $10.00) support a monthly newsletter, L-5 News, and various other space-colony promoting activities. The L-5 society appears to be becoming an important clearing house for current information on space-colonization activities. I wish to encourage persons who have pertinent information they wish published or who wish to promote or receive information about space colonization, to communicate with the society (the L-5 Society, 1620 N. Park Avenue, Tucson, Arizona 85719).

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Physics comes of age

With quantum mechanics we enter a region in which the system of interest is perturbed by any measurement, with the result that there are inherent uncertainties in the extent of our knowledge of the system parameters. The profession of physics and the society of which it is a part now stand in a somewhat similar relationship. Any large undertaking in physics (large either in intellectual terms or in terms of dollars) will have its effect on society and be reflected back in a complicated and not easily predictable way into the profession of physics. In this regard physics now finds itself in the same position as many of the other major segments of society. It has joined the ranks of the armed forces, the churches, the federations of labor unions, and the institutes and *de facto* governing bodies of the major industries. In this sense physics and the other basic sciences have come of age.

Yet month by month we see in the pages of PHYSICS TODAY a wish to evade this irreversible situation. Some correspondents write in to say that too much attention is being paid by physicists to "society-related" problems and not enough attention is being paid to "basic" research. Others write in to say just the opposite. What is not recognized is the fact that these terms are no longer meaningful in the sense that held one hundred years ago before the results of physical research had so visibly altered the conditions and possibilities of human life. The confusion arises perhaps because the nature of the subject of physics has not changed but its practice by physicists in the aggregate has altered forever.

The leadership of The American Physical Society has been no more perceptive. It persists in using these outmoded terms and even in dividing jobs in physics into "traditional" and "nontraditional" categories. The truth is that there are no longer, nor will there be again, any traditional jobs in physics in the sense that applied even as late perhaps as the 1930's. Until these attitudes are changed and the leadership of the profession, and through it the ordinary physicist, comes to have a clear idea of the present situation we will continue to experience crises in physics. It is no longer enough to have a leadership for the profession which practices professional physics and amateur sociology and psychology. Such schizophrenia will only produce further examples of response after the fact, like those we have seen and continue to witness in the physics-employment crises. What is required is for the physics community to study and understand its situation in the real world and to begin to act in an integrated manner recognizing both the nature of its power and promise, and the extent of its responsibilities.

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Correction

February, page 70—The last paragraph on the page should begin: Bell Labs funds a wide spectrum of research, but unlike a university, the research must be relevant to the Lab's broad communications mission. □