for society and may be a laudable goal for individuals to support, but what good will it do physics?"

There is an alternative assumption, the one I start from, to wit—that there *is* an enormous amount of untapped and wasted talent, and intellectual brilliance, within the minority community, and that not only would it be good for society as a whole if the best of these people could contribute to scientific research, but that ultimately it would be good for physics. We cannot have too many good physicists, but the profession now is undoubtedly missing a lot of potential contributors because of the lack of significant participation by minorities.

In this respect numbers are important. Talent and native intelligence are no doubt distributed among the minority population in about the proportions as among the majority population. Therefore, if we are to succeed in attracting the best minds and most creative persons from minority groups into physics so that they may make significant contributions, there must first be a sufficiently large pool of people who are interested enough in the subject to pursue it. Creating opportunities is only part of the challenge I feel the professional societies should meet. Generating sufficient numbers of people to take advantage of the opportunities is equally, if not more, important.

I submit then that the goal of increasing the number of minority physicists (and other scientists and engineers) *is* compatible with the goal of promoting the health of the discipline as a discipline, and would urge that the AAPT, but even more so the APS, recognize this as part of its mission.

One of the most significant accomplishments of the APS over the past few years was the creation and support of the Committee on Minorities in Physics. Carl Spight reported comprehensively on the activities of this Committee at the joint New York meeting in 1976, so I will not repeat all the details here.

Although the APS Committee on Minorities has engaged in a number of projects and activities, perhaps its most significant long-range effect will turn out to be that it provided a forum for minority physicists themselves (along with sympathetic whites) to discuss, debate, and propose remedies for the concerns of minorities in physics. These kinds of efforts have proved to be sufficiently exciting and potentially useful so that a group of black physicists have decided to formalize the process by organizing as a formal group. At a meeting of black physicists last April at Morgan State University, it was agreed to constitute an organization, which is tentatively called "The Society of Black Physicists." The broad purpose of the organization, which is still in a developing stage, is to promote the welfare of black physicists within the physics community and society at large, and to promote and support activities to increase opportunities for, *and* numbers of, blacks in physics.

Similar organizations have existed for some time among other scientific disciplines and other minority groups. There is SACNAS (Society for the Advancement of Chicano and Native Americans in Science); there is the Society of Black Chemists; black biologists and black engineers also have similar groups.

The goals of these groups are not in conflict with the goals of the mainstream majority associations, but the groups are seen as necessary to provide an opportunity for minorities themselves to mount programs and activities to address the issue this session is devoted to: increasing opportunities for minorities.

In closing I would like to call attention to a recommendation made by the National Board on Graduate Education in 1976. The recommendation addressed the role of professional organizations in creating opportunities for minorities, and I think it would be fitting if the APS and AAPT would endorse it.

> "We urge professional associations to draw upon the prestige and talents of members and to assign a *high priority* to promoting increased opportunities for minority men and women in graduate study and in the professions. Such efforts should be central, not peripheral, to the mainstream of association activities to ensure the *sustained* commitment essential to their mobility and success."

(Adapted from a talk given at the 1978 joint APS–AAPT Meeting)

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Reader compliment

I want to compliment you on the article, "Patents: another way to publish" in March (page 23). This article was especially interesting to me in light of my formal education and training in physics and my present involvement in applied science and technology. I'm sure others like myself, who are currently involved in development and applied physics work, would encourage more articles of this type.

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Cosmic rays in the 30's

The February issue (page 23) contains excerpts from Daniel J. Kevles's book *The Physicists*. This interesting article exposes "difficulties for physics in the

United States" and "the revolt against science in the 1930's." As an example of lack of public confidence in science, the Millikan–Compton dispute about the nature of cosmic rays is vividly described. The clash of two hypotheses, photons versus particles, with its philosophical–religious undertones, was an important phase in American physics.

While Kevles's book deals mainly with the "History of a Scientific Community in Modern America" (his subtitle), I would like to add to his valuable discussion by amplifying his statement that "other physicists had begun noticing the latitude effect" (which speaks for the corpuscular nature of the primary radiation). Indeed, cosmic-ray research was very exciting on a worldwide scale. The effect was first observed by the Dutch physicist J. Clay in 1927, although his data were not generally accepted. However, he confirmed his measurements in several subsequent trips between Holland and the Dutch Indies, and by 1932 he was fully convinced that the primary cosmic rays consisted of charged particles.[1] Clay was in close contact with Victor Hess (Innsbruck), A. Corlin (Sweden), W. Kohlhorster (Heidelberg), E. Regener (Stuttgart) and others. In 1930, W. Bothe and W. Kohlhorster[2] travelled from Heidelberg to the Arctic. For reasons now well known (the "Knee;" Størmer, Lemaitre and Vallarta), they found no variation in intensity, although their independent coincidence-counter experiments indicated that the cosmic rays were electrical particles. Similarly Corlin[3] observed only a small difference between northern Scandinavia and East Prussia. Also, Kerr Grant[4] measured constant intensity between Australia and Antarctica.

While A. H. Compton was conducting his worldwide survey, I travelled, in March 1932, from Bremen to Peru under the auspices of Erich Regener, Institute of Technology in Stuttgart. Regener[5] was leaning towards the photon theory of cosmic radiation on the basis of the analysis of his ingenious measurements from the bottom of Lake Constance to great heights in the stratosphere. I found a 9% variation between Bremen and the Panama Canal, where my high-pressure ionization chamber developed a leak. I reported back to Regener and Clay but refrained from publication in the expectation of confirming the variation on the return trip. In Peru, I conducted measurements 2 degrees north of the geomagnetic equator, mostly on glaciated mountain tops up to 6100 meters high, and obtained data that were compared later with my measurements made in the Alps and with balloon-based observations showing a strong increase of the latitude effect with altitude.

In December 1932, while waiting in Lima for my freighter to take me back to Europe via the Strait of Magellan, I met H. Victor Neher, Millikan's assistant, at the California Institute of Technology. Neher, who was on his way home from Arequipa (El Misti) in southern Peru via Mollendo, introduced me to P. B. Ledig of the Division of Terrestrial Magnetism, Carnegie Institute of Washington, D.C. Ledig was making measurements for Compton on the roof of the Hotel Bolivar in Lima. Thus, the three of us exchanged experiences in an impromptu cosmic-ray conference.

Neher, at that time, had found a large latitude effect at high altitudes during airplane flights over North America and Canada and his recent flights over Peru. As far as I can remember, Ledig had no full information on the results of Compton's seven other expeditions, which were published in Compton's classical paper in the 15 March 1933 issue of the *Physical Review*.[6]

The first account of my uninterrupted measurements from 42 deg south to 53 deg north, geomagnetic, appeared in *Nature* in July 1933[7] and was followed by several other papers in German scientific magazines. Later in 1933, the French physicists P. Auger and Leprince-Ringuet measured the latitude effect between Le Havre and Buenos Aires[8] with Geiger counters. There was little further doubt now about the corpuscular nature of the cosmic rays.

In the context of this letter, I refrain from referring in detail to studies of other geomagnetic effects (longitude, East–West symmetry) and pertinent laboratory experiments that were conducted during the same period. Review articles were written by many; those by A. H. Compton[9] and T. H. Johnson[10] are most easily accessible. Nevertheless, even today, the exact physical processes that lead to the birth of the radiation are still essentially unresolved.

In Europe, at that time, it was also difficult to obtain funds for basic research. The relationships between science, technology and jobs were discussed, but the public and the press paid little attention to scientific disputes. In scientific circles the arguments were less vehement than in the United States; they were overshadowed by the beginning of the incomprehensibly tragic, political events in Germany, which affected the course of science for several decades.

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continued on page 80

continued from page 15

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No universal equation

I read the letter by Gerald Lebau (March 1977, page 82) concerning the letter by David Lazarus with amused interest. In spite of Lebau's references to philosophers, it is not clear that Lebau is familiar with a rather famous result of the logician Kurt Gödel,[1] who in 1931 proved that every deductive system that can be represented within arithmetic is either incomplete or inconsistent. At the moment, no one has any idea of what a proof could be that is not arithmetically representable. Since this includes all the reasoning done in physics, I believe it is inconceivable that "it will not be long before a new and correct model of the physical universe will be found." In fact, Gödel's result has so well destroyed any hope for anything like "finding the universal equation" that this has not been "the goal of theoretical physics" for a rather long time now. On the contrary, it appears that enough remains to keep theoretical physicists employed forever!

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THE AUTHOR RESPONDS: Being aware that a mathematician once proved heavier-than-air machines can't fly, I'm not too concerned with Gödel's proof. However, Franklin Schroeck has raised precisely the targeted issue. Where I spoke of "a new and correct model of the physical universe" he cited the impossibility of "finding the universal equation." But mathematics and equations are insufficient. Until we understand the physical realities (the metaphysics) to which the symbols refer, equations don't model anything at all.

Absence of an accurate physical model allowed forbidden mathematical con-