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have strong programs to support science; they have the gross domestic product to fund it; the smaller countries don't. Moreover, the average physics professor has to hold down two teaching jobs: one in the state university in which she or he has tenure, and the other in a private university or high school. Some physics professors also look for temporary consulting jobs.

Science and technology indicators must be used cautiously in predicting what will happen to Latin American physics. Often, officials do not want their country to look bad in front of its neighbors, so they may report figures that are not well supported by reality.

What can be done? In the same way investment capital is put into Internet ventures without investors' seeking immediate return, associations like APS could take action by tapping funding and resources that could be channeled to the least-favored countries through society members living there, specifically to Honduras, Nicaragua, Panama (don't be fooled by economic figures), Belize, and Guatemala. Costa Rica epitomizes the goal the others need to reach: a general public respect for science and technology, including physics. Countries with few physicists (Panama only has about 15 PhDs) need the support of strong and continuing research-exchange programs; they also need funds, equipment, and updated reference works.

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I read with interest the article by José Luis Morán-López. He states that it is not complete; still, the newly released results of a study by Colciencias, the Colombian Science Foundation (http://www.colciencias.gov.co), necessitate a revision of Colombia's entry in the table on page 40.

Almost 750 research groups in all disciplines (natural, social, and applied sciences, and humanities) participated in the study. Sixty-nine of them were ranked in the highest category based on publications in international journals. Of these groups, 17 (one-quarter) work in physics or related areas. Four universities—Antioquia, Valle, the Andes, and National—have at least three top-ranked physics groups each. Three others—Cauca, the Industrial University of Santander, and Quindio—have one group each.

Many of these research groups contribute to the early training of young scientists, who often publish their first papers while working with them, and many groups have long-term collaborations with major institutes or universities throughout the world. While the international effect of Colombian physics may be modest, the few hundred physics PhDs in this country have a significant impact on the local research community of about 4000 individuals.

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I enjoyed reading the article by José Luis Morán-López. However I do not agree with his starting assertion that "poor economic conditions in most Latin American countries have seriously restricted their capability to invest in science and technology." His whole article seems to suggest the opposite: Poor investments in science and technology in most Latin American countries have seriously restricted the capability of those countries to improve their economic conditions.

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Morán-López replies: I thank Azael Barrera for pointing out that the Physical Society of Panama (PSP) was founded in 1980, not 1998. He also explains why the PSP was inactive for the intervening period. The PSP began operations in 1998, the date that I mistook as the founding date. At that time, I was acting president of the Federation of Latin American Physical Societies when the PSP requested affiliation. The PSP is the most recent society to become affiliated with FELASOFI.

Barrera suggests that political arguments and distractions, and the desire of many scientists to look better than their colleagues, are two reasons why the Latin American Physics Network (RELAFI) is not "working as well as expected." RELAFI is one of five Latin American science networks that are funded primarily through UNESCO. Unfortunately, UNESCO has restricted its financial support, and the networks' activities have been limited accordingly.

I appreciate Edgar G. Pavía's point of view since it raises an interesting question: Does the lack of investment in science and technology in some Latin American countries cause poor economic conditions, or is it the other way around? To promote higher levels of education and thus improve overall economic conditions, Latin American countries must first educate a seed group of scientists, who in turn will contribute to a general improvement in education. Such a seed group needs special conditions to flourish, perhaps the most important being strong initial funding of science and technology programs. However, that initial investment has been hindered by the poor economic conditions in some Latin American countries.

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Global Science Teams Will Have to Share Resources and Glory

The recent announcement of an increase in government spending to allow the UK to join the European Southern Observatory (ESO; Physics Today, January 2001, page 25) may well have sent a shudder through the US affiliates of the Association of Universities for Research in Astronomy. Is this announcement, following as it does proposals for a non-NATO European "rapid reaction" military force, another instance of growing isolationism between Europe and the US? Will it threaten the future of joint projects like the 8-meter Gemini telescopes, a collaborative effort of the US, UK, Canada, Australia, and South America? Actually, it underscores the need for greater cooperation, and is a clear signal that science is a global, rather than a national, pursuit.

During science funding difficulties, the cry is often raised of national pride and the need to spend money on research to beat the competition. The UK has long realized that it cannot afford to fund major astronomy projects by itself, and has worked in various partnerships. That experience has been, on the whole, very positive. It has provided excellent observational facilities and has shown that added value can come from the resulting interaction among partners. This success is no news for particle physics, a field in which cost has essentially driven the most complex experiments to a single global site.

It is the conflict between shared facilities and "national" science that may itself generate a problem. Does each nation try to use its shared facilities to steal a scientific advance on its partners? As the sheer scale of frontier experiment and observation increases, and time on such facilities becomes ever more expensive, we need a new approach. We must share the science, too.

In astronomy, the next generation of large telescopes—the Atacama Large Millimeter Array and a proposed extremely large 50- to 100-meter optical/infrared telescope—must be global projects. Both the US and ESO are heavily involved, and that involvement necessitates stronger ties. But to capitalize on the great investment involved, will we be able to share the glory of the inevitable discoveries? Will the *New York Times* headlines declare "World Science Team Discovers . . . " or will it be "US Worried As UK/European 'Boffins' Scoop Discovery"?

Achieving a global aspect to science programs may well be difficult, even in good international partnerships. Satisfying reasonable national aspirations for observation time and still running major joint programs is not trivial. The US has experienced such problems in the rather uneasy atmosphere generated by variable community access to national and privately-funded observatories. Large joint international observing programs have a reputation for inefficient use of telescope time, and innovative thinking is needed to allocate this scarce resource effectively and equitably.

Perhaps it's just a shift of credit that will be needed. We scientists (and the funding governments) need to be prepared to acknowledge and accept success for the global project itself, an accolade for all the partners rather than individuals. In big projects we will need to share, rather than grab, success. Science itself knows no regional or national barriers. Neither should our pursuit of it.

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Renormalized Relations in Condensed Matter

In his article "Brainwashed by Feynman?" (PHYSICS TODAY, February 2000, page 11), Philip W. Anderson says that a generation of "field theoretically trained young theorists" now performs essentially irrelevant studies of the interacting fermions in condensed matter: "The obvious assumption is that if one is able, by dint of very hard work, to sum up all the Feynman diagrams, one must arrive at the right answer. The problem is that no one has ever been able, over four decades, even to arrive at the right interaction that way." Anderson invites these theorists to imitate their particle physics colleagues, who "have long abandoned straightforward diagrams, in favor of a much more varied toolkit of concepts and techniques."

This view, from a leader of theoretical condensed matter physics, calls for a debate. As a field theoretically trained (no longer young) theorist, I attack the problem of interacting fermions by resumming "all the diagrams." I agree with Anderson that the phenomenology of the interactions in "borderline materials" is still poorly understood, and that the description of many phenomena in condensed matter may require tools other than straightforward perturbation theory. However, I would not dismiss perturbation theory, for instance, by believing that it is limited to the analysis of essentially boring weakly coupled theories.

The study of interacting fermions in condensed matter by resumming diagrams has at least two scientific godfathers: Richard Feynman and Kenneth Wilson. In the late 1980s, a group of mathematical physicists, Giuseppe Benfatto, Joel Feldman, Giovanni Gallavotti, and Eugene Trubowitz, also made important conceptual progress: They realized that Wilson's renormalization group should be adapted in a nontrivial way to condensed matter. Indeed the long-range behavior of a system of interacting fermions is governed by an extended singularity, the Fermi surface. As a result, the corresponding scaling analysis and the underlying dynamical flow of effective interactions is much richer than in the ordinary Wilsonian case. No simple analog exists of the "block spin" and rescaling concepts. More important, there is an infinite set of relevant operators, or of coupling constants. Feynman diagrams are essential to analyze the corresponding flows.

Organizing perturbation theory based on the renormalization group around the Fermi surface is therefore not only conceptual progress, it is probably (even numerically[1]) the best tool to understand which among all these couplings diverges first and dominates the long-range physics. I also do not think that the analogy with the confinement problem for hadrons is relevant; we know that, because of the extended nature of the Fermi surface in more than one dimension, nonperturbative phenomena in condensed matter physics (particularly the formation of bound states such as Cooper pairs) can actually be controlled by analytic methods because of their similarity to large-component vector models.[2] Despite many efforts, this is not yet the case for hadrons, because matrix models are involved in their formation.

But is it the use of Feynman graphs or the pretension to "sum them all" that Anderson considers irrelevant?

If the latter is the case, here is a brief defense of traditional, perhaps old-fashioned,[3] mathematical physics, which consists in proving mathematical theorems about idealized systems inspired by physics. I view it as an indispensable complement to theoretical physics in the long run.

When Lars Onsager proved that the two-dimensional Ising model has a phase transition, or when John Imbrie solved a controversy about the nature of the ground state of the random-field Ising model in three dimensions,[4] they certainly did not believe any real material to be an exact Ising model. Nevertheless, each of their results acquires a particular value because it is mathematically rigorous. Even more than diamonds, mathematical theorems are forever; they are precious strongholds among all the uncertainties of an ever-changing scientific landscape.

The question of whether perturbation theory can be summed up or not goes back at least to Henri Poincaré. Even a negative result, such as his famous observation that the Lindstedt perturbation series of classical mechanics diverges, is a scientific landmark, a starting point for advances such as the convergence theorems of the Kolmogorov-Arnold-Moser type and the investigation of chaos. Therefore, a mathematical