CONDENSED MATTER THEORY'S FRAGILE FUNDING

Has research in condensed matter theory really been dealt a worse funding hand than other branches of science? David Mermin raises the question by implication in his musicological dramatization (August, page 9). While it's not done to moan loudly (and publicly) when rattling one's begging bowl at the NSF, I believe there is an objective case for arguing that condensed matter theory has fared worse than other theoretical disciplines.

The major problem is not that the money has gone down but rather that the demand has gone up: First, the intellectual center of theoretical physics is moving over in the direction of condensed matter physics. By way of anecdotal illustration, in recent interviews of three eminent particle physics theory candidates for a senior faculty position at a prestigious university, every one of them gave a lecture on (would you have guessed?) condensed matter theory. As a result, graduate students who previously would have sought thesis research in particle theory are now keen to work in condensed matter.

Second, state governments around the country have been building up university faculties with the aim of stimulating local high-technology industries. Much of this buildup involves condensed matter physics and materials science and engineering. A recent study by Judy Franz and Neil Ashcroft showed a 32% increase in condensed matter theory faculty over the period 1982–88, compared with about 10% for particle theory.[1]

Third, increasing numbers of physicists are working outside "traditional" physics areas, moving into fields such as engineering materials research, geophysics research and biophysics[2]—all of which have condensed matter physics as one of a number of possible entry points. The greater diversity of career opportunities also attracts more graduate students to condensed matter physics.

These factors, taken together with more or less level Federal funding, have led to the kind of funding frustration experienced by Mermin's Professors "Mozart" and "Beethoven."

What is the reason that funding levels have not responded to this increased demand (and give little sign of doing so in the immediate future)? Part of the cause lies with the individualistic nature of condensed matter physics as compared with enterprises such as high-energy particle accelerators or space telescopes. The people who run large accelerators actually *need* theorists to tell them why they are doing what they are doing—and a small percentage of the high-energy physics budget can buy quite a few theorists. In contrast, the need for condensed matter theorists by people managing neutron or synchrotron light facilities comes at a much more removed level. The nature of the science research at these facilities is far more diverse. Some of it needs theories, but much of it is involved more with complexity at the structural or chemical level than with complexity of the underlying physics. So the pressures on the Federal government for funding condensed matter theory come from educational or industrial needs as opposed to programmatic needs, and are much less focused as a result.

Is there hope down the line for Professors Mozart and Beethoven? In the near term one can hope that efforts from within the profession to lobby the Administration and Congress will bear fruit. As for the longer term, I believe one should look at the current situation in the context of the natural evolution of science. Traditional theoretical physicists (including condensed matter theorists) are slowly going the way of the dinosaur. The smart young people are going to adapt and come out looking like materials scientists, molecular biologists or computer experts. The powdered wigs and romantic arias of the

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golden age of traditional theoretical physics will continue to be loved, but as memories of a bygone era.

References

1. J. R. Franz, in "Future of US Doctoral Programs in Physics," Top. Conf. Ser., Am. Assoc. Phys. Teachers, College Park, Md. (1989), p. 57.
2. Natl. Res. Council, "Physics Through the 1990s," Natl. Acad. P., Washington, D. C. (1986).

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David Mermin tells of Professor Mozart's difficulty in getting a grant because he couldn't describe in advance what he would find. If he could do that, "it wouldn't be research," according to Mozart.

Leo Szilard apparently shared Professor Mozart's opinion. Szilard, the story goes, would write grant proposals for work *he had already carried out.* With the money for the already completed work, Szilard would carry out new work, for which he would write a proposal in due season (that is, when it was complete).

The unwitting granting authorities were quite pleased. Szilard's research always was finished within the allotted time, and he always did what he had proposed to do.

This lasted until a referee or member of the granting authority objected that what Szilard was proposing could not be done. Finally Szilard pulled the completed work out of his pocket, slammed it on the table and said, "There, you idiot, it's *been* done!"

I can't vouch for the truth of this story, but perhaps it has some pointers for Professor Mozart. I think Szilard's scheme would work better for a theoretician than for an experimentalist.

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Mirror Aberration Communication

I read with interest the news story by Bertram Schwarzschild on the shape of the Hubble Space Telescope's primary mirror (August, page 17). The wrong shape of the mirror is presented as a gross error that happened by some misadventure nobody seems to understand. This may not be the case. There is evidence that current optical testing methods lack accuracy and that such an error was highly likely to happen.

At the University of Hawaii, we have recently been systematically checking the optical quality of telescopes with a new wavefront reconstruction technique using defocused stellar images. All the telescope primary mirrors we have tested have been found to suffer from spherical aberration; that is, the conical constant differed from the expected value. For telescopes on Mauna Kea, the wavefront spherical aberrations at $\lambda = 0.633\ \mu$m were found to be as follows:

Telescope	Peak-to-valley	rms
UH 88″	-0.3λ	0.1λ
CFHT	-1.0λ	0.3λ
IRTF	$+3.5\lambda$	1.1λ

The University of Hawaii 88″ telescope is a Ritchey–Chrétien telescope; that is, the primary mirror is hyperbolic. The spherical aberration given above is the residual value observed at the prime focus after correction for the expected aberration. The Canada–France–Hawaii Telescope and the NASA Infrared Telescope Facility both have parabolic mirrors, and therefore they each have a stigmatic prime focus. The IRTF value is a rough estimate obtained at the Cassegrain focus. Recent observations made at the prime focus indicate that the aberration is produced by the primary. In cooperation with the National Optical Astronomy Observatories, we have also tested the Smithsonian 60″ telescope on Mount Hopkins. Again a spherical aberration of 1.2λ peak-to-valley, or 0.4λ rms, was found at the Cassegrain focus, which may originate from the primary. The spherical aberration of the European Southern Observatory's New Technology Telescope quoted by Schwarzschild would have probably remained unnoticed in the absence of the Hartmann sensor used for active control.

The estimated 1.5λ peak-to-valley, or 0.5λ rms, wavefront spherical aberration of the Hubble Space Telescope is of the same order of magnitude. On ground-based telescopes such errors are hardly noticed owing to the image blur produed by the atmosphere. The IRTF's large error is hardly noticed because this telescope operates in the infrared. In space such errors become conspicuous and ruin the expected high-resolution images. Only recently has it been realized that on a good site such as Mauna Kea the image blur produced by the atmosphere occasionally drops down to the 0.2″–0.3″ range, whereas telescope aberrations limit the image width to about 0.4″–0.5″.

Because such errors have long remained unnoticed, people seem to have overestimated the accuracy with which the conical constants of large telescope mirrors are measured. It is indeed a difficult task, since measurements are made at the center of curvature and the deviation from a sphere must be estimated with a high absolute accuracy. The following table shows the deviation from a sphere at the mirror edge for the same telescopes as above, together with the relative error found in the measurement; I have added the Space Telescope error for comparison:

Telescope	Deviation	Error
UH 88″	160λ	0.8%
CFHT	100λ	3.7%
IRTF	300λ	4.9%
HST	300λ	2.0%

In all cases the error is of the order of a few percent. Absolute measurements with an accuracy better than 1% are known to be difficult. In the case of the Space Telescope, the difficulty has clearly been underestimated. Additional tests should have been made. Informed engineers know that most arguments given against these tests are wrong arguments. For instance, an 82″ quartz flat is available at NOAO for testing purposes and could have been used in autocollimation. This flat would not have had to be as optically perfect as the Space Telescope mirror as long as the errors were known, which they were. Contamination would not have been a problem, since all the tests could have been made with an uncoated mirror.

There are now plans to build 8-m telescopes with $f/1.8$ primaries. In this case the deviation from a sphere is more than 2000λ. A 0.06% accuracy is required in this measurement to insure that the rms wavefront error will be less than 0.1λ. Current testing methods clearly seem unsuited to meet this challenge. A research effort is urgently needed to develop more accurate testing methods. In addition, active control of the mirror figure during observations becomes mandatory.

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(*Editor's note:* For an update on the cause of the Hubble mirror's aberration, see the news story on page 19.)

Florida Un-sitely for Magnet Lab

Recently I learned that the National Science Board has decided to establish