is, for $r \approx 137 a_0$. In the case of the two H atoms, the $1/r^6$ van der Waals interaction becomes the c/r^7 Casimir–Polder interaction. In the case of e^- and He^+, a $1/cr^5$ term appears, as shown by E. J. Kelsey and me.[1] An improved theory by C. K. Au, G. Feinberg and J. Sucher,[2] valid down to smaller values of r, and supplemented by work by R. J. Drachman[3] and G. W. F. Drake,[4] is, after herculean efforts by S. R. Lundeen and collaborators,[5] within a laser's edge of providing the first high-precision confirmation of a Casimir interaction. (See my article in PHYSICS TODAY, November 1986, page 37, and references therein.)

The best known Casimir effect is the force per unit area between uncharged parallel ideal plates at a separation z. Retardation effects are less transparent for this case. One concludes dimensionally that $F/A = K\hbar c/z^4 \equiv (F/A)_{\rm Cas}$, with K a constant, for all z, with no change of form: Retardation is crucial for all separations. An ideal conductor adjusts, with period $P = 0$, to any electric field present, and $\tau = 2z/c \gg P$ for all z. In a real conductor, where the smallest period (or characteristic decay time) P is nonzero, $F/A \sim (F/A)_{\rm Cas}$ for $\tau \gg P$, that is, for $z \gg cP/2$, but for $z \ll cP/2$ retardation is irrelevant and F/A is independent of c and characteristically goes as $1/z^3$.

I would like to thank Kleppner for some useful conversations.

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KLEPPNER REPLIES: The term "Casimir effect" is often used loosely, and perhaps I used it too loosely in motivating my discussion of the van der Waals interaction. Larry Spruch's observation that retardation can never be ignored when considering the attraction of ideal conducting plates justifies his taking me to task for downplaying retardation or, alternatively, for not distinguishing between the Casimir force and the van der Waals force.

With respect to Peter Milonni's bewilderment at the excitement over cavity QED experiments, I can appreciate his point of view, for many of the basic physical principles were spelled out in his early work on atoms radiating between mirrors—work that was far ahead of experiment. Milonni correctly points out that physical effects of the vacuum are hardly news in physics, and that none of the recent generation of experiments can compete with, for instance, the drama of the Lamb shift. Although I did not labor the point, my comments on cavity quantum electrodynamics were in the context of macroscopic quantum mechanics. Whether or not the observation of the Jaynes–Cummings oscillations should be cause for excitement is, of course, a matter of taste. However, when an area becomes experimentally accessible—even one for which the theory is already beautifully developed—new phenomena are likely to be discovered. This has certainly been the case for cavity QED.

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Trends and Tactics in Science Funding

In their article on young physics faculty in 1990 (February, page 37) Roman Czujko, Daniel Kleppner and Stuart A. Rice report that there has been a dramatic drop between 1977 and 1990 in the fraction of young faculty who believe that research funding is adequate. Their report joins a rising tide of complaint about the plight of academic research in the US today. Leon Lederman, president of the AAAS, has reported on a survey of the views of 250 academic researchers; he too found a dismal state of morale among them.

The chart on page 136 shows the trend from 1973 through 1987 in support for research and development per doctoral degree holder in the physical sciences (mainly physics, chemistry and astronomy) employed in an academic institution. The curious fact is that young physics faculty felt better in 1977, after several years of diminishing support, than after the sustained growth in *per capita* support during the 1980s.

The explanation of this curious phenomenon is not obvious. Similar trends occurred in the support of academic life sciences and other natural sciences. These facts suggest that there may be underlying structural

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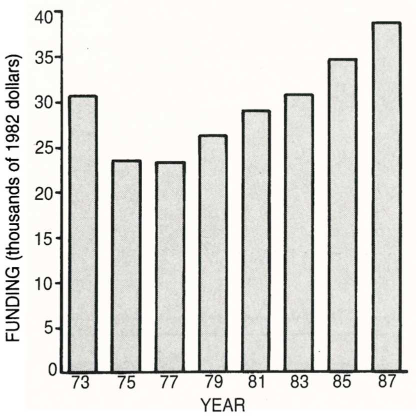
problems in the management and distribution of academic research funds. Just three possible problems of this sort that have occurred to me are a changing mix of the type and cost of research programs, marked inequalities in the distribution of funding among principal investigators, and diminished local flexibility in how research funds are spent. Such problems may be just as important as constraints on total funding in causing the present malaise among many academic researchers.

Even without understanding these facts, I am apprehensive of arguments for more funding that are based on inward and self-serving views. In the eyes of many Congressmen, it will



appear unseemly of us to base our pleas for more money on the poor state of our morale. There are too many homeless, returning veterans, AIDS victims and others waiting in that line, most with more appeal to our elected representatives than we have. We need to focus on the fundamental reasons why our society needs to invest in academic science, namely the tremendous economic, social and cultural benefits it brings. In short, we need to concentrate on what we can do for others, not what they should do for us.

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LEDERMAN REPLIES: Roland Schmitt takes issue with the APS survey reported on by Roman Czujko, Daniel Kleppner and Stuart A. Rice and with the AAAS inquiry over the question of choosing a strategy toward a common goal: improving the health of US science. His point of view is shared by a surprising number of good people who have commented on the AAAS report. Whereas letters from the bench scientists are in general supportive, the comments from the Washington-wise, a group we desperately want on our side, tend to emphasize the danger of self-serving actions. In our report we tried to stress that what should be of concern to policymakers and the public is the health of scientific research rather than the joy of scientists. The riposte that scientists are better off than the homeless may win a debating point but misses the crucial significance of what is going on in the laboratories. It is like responding to the fainting canary in the mine with "Who cares about canaries!"

Both the APS survey and the AAAS inquiry sound an early warning to the nation. If we are turning off young physics investigators, if the most successful researchers are in despair in full view of their graduate students, if (as my mail indicates) the same trauma exists in fields from anthropology to zoology, then someone must pay attention. Schmitt's strategy is to minimize these data in favor of stressing what wonders science can perform. Nevertheless our analysis makes us the messenger with the bad news: The costs of doing research have far outpaced the budget increases. How can we make this point if we do not use the morale of scientists as an important indicator? The criticism we have received, that scientists can always use more money, is superficial; the depth of the malaise is new and should be clear to anyone who studies the data or visits the laboratories. Ignore this early warning, we insist, and the nation runs the risk that US science will go the way of education and much of our once vaunted industry.

I can only hope that this lively debate will eventually result in a common strategy toward the noble end of restoring American science. For this we surely need the help of Schmitt and his Washington-wise friends.

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Cutting 'Big vs Little Science' Down to Size

Can we lay to rest the question of "big science versus little science" as a non-issue? True, this is a comfortable topic. It is like a joke whose punchline can be expected from the beginning and is guaranteed not to surprise the listener. But it is a code phrase for a concept that often bears little relation to how science is actually done.

The scales of "big" and "little" are not clearly defined in peoples' minds, except possibly relative to where they spent their early professional years. (For some scientists this is "a certain nostalgia for virtue," to quote Arthur Schnitzler in *La Ronde*.[1]) If pressed, many people would say that "little science" is done by a single faculty member together with one or two graduate students and possibly a postdoc, and with a small equipment budget; "large science" is a creaking, overadministered enterprise of unspecified size in which there is no room for individual initiative. Current folk beliefs can be summarized as follows:

▷ Little science is "good." The best science is done on this scale. It is cost effective. Students get the best training, on a one-on-one basis.

▷ Big science is "bad." No good science is done on this scale. It is a waste of money. Students get poor training.

Now consider the realities of how science is done. The optimum size of a group varies greatly. It depends on the problem studied and the nature of each scientist involved. Some scientists choose to work largely alone, with only a loose professional coupling to others in their field. Others work in small local teams on a common range of problems, with each contributing significantly. And finally, others simply prefer to work in larger groups that span departments or that make up institutes or laboratories in their own right. The larger groups require more internal administration, yet each scientist still is responsible for his or her own successes (or failures). The spectrum of group sizes is best determined by the spectrum of the most significant problems. Artificial limits on size will deny the exploration of key research problems.[2]

Even in the largest group, each student should always have a clearly defined mentor. Student training is critically dependent on the skill of the mentor, and on giving the student the correct level of independence and responsibility. This is true regardless of group size. If anything (everything else being equal), students can often get better training in the excitement and variety of a large group. After all, students get at least half of what they learn from other, more senior students.

"The best science is always done as little science" is far from a universal truth. Consider two examples:

▷ Hans Dehmelt, working on the traditional small scale, won the Nobel Prize for his precision measurements of the electron.

▷ Carlo Rubbia and Simon van der Meer, working on the largest scale yet, won the Nobel Prize for demon-