

describe the wide applications of Casimir or van der Waals forces and their generalizations—for example, books by Milton,¹ by V. Adrian Parsegian,² and by Vladimir Mostepanenko and N. N. Trunov.³

As to the reported accuracy of various experiments, I prefer to not second-guess those authors, including myself. I have no illusions about the perfection of my own work, but I was careful and found the data internally consistent at the level of error I reported. I did find one calibration error and published an erratum. Given the attention I have paid to other corrections, the recent Drude-model finite-conductivity thermal correction appears incompatible with my experimental result.

A different analysis, done independently by me, Mostepanenko, Giuseppe Bimonte, and others, in which the metal plates are treated as a conducting waveguide, shows a relatively small correction and good agreement with my experimental result. Until the differences between the theoretical approaches are resolved, rejecting experimental results is premature.

As to the experiments that have yielded a 1% level of agreement with theory, the authors of those papers ap-

pear insistent that they have used no adjustable parameters. That work has gone largely unchallenged because the level of accuracy, at submicron plate separations, has not produced significant theoretical controversy; for example, the Drude-model thermal correction becomes very small for plate separations that are significantly below one micrometer.

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SSC price tag rose from \$3 billion in 1986 to to \$5.9 billion in 1989 to more than \$10 billion in 1993, it steadily lost political support—which was quite strong initially—and was finally terminated by Congress.

With such a track record lurking in its recent past, the US particle-physics community can ill afford to start down such a tortuous (and torturous) road again. We need to play the costing game straight this time and be honest about what the ILC will really cost, or it won't have a chance of getting off the blocks—at least not in this country.

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Randomness and strong disorder

The elegant experiments on photonic localization in randomized photonic crystals (PHYSICS TODAY, May 2007, page 22) raise an interesting question: What happens when an electronic system is strongly disordered but not random? Traditionally, “random” has been thought of as a convenient mathematical tool that gives a good approximation to any strongly disordered system, but a series of experimental studies, beginning in the 1980s with the metal-insulator transition in semiconductor impurity bands, have shown many properties that are inconsistent with random models. Thus the photonic experiments have confirmed idealized models of strong disorder and have simultaneously emphasized just how rare such idealized disorder really is.

Why shouldn't random models work just as well for electronic as for photonic systems? Electronic interactions are normally much stronger than photonic ones, but they often are also randomized by strong disorder. That is especially true in the presence of several kinds of incoherent disorder—for instance, mixed impurities, or impurity clustering, in the case of semiconductor impurity bands. For a long time it was thought that strong disorder is always incoherent, but modern materials science has become so sophisticated that it can produce not only rare examples of ideally randomized materials but also, and much more often, the opposite extreme: self-organized networks embedded in, and actually made possible by, strong disorder.

The classic examples of strongly disordered materials are found in network glasses, which William H. Zachariasen

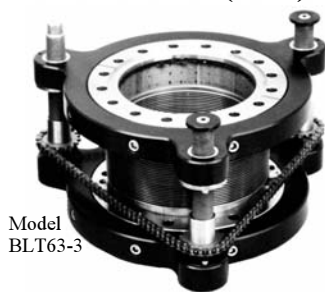
The true cost of the ILC

In his story on the proposed International Linear Collider (ILC), Bert Schwarzschild does his usual meticulous job of reporting the news on particle physics and cosmology (PHYSICS TODAY, April 2007, page 26). But behind the cost figures presented, there's a deeper story that he did not discuss.

The \$7.5 billion total estimate cited is what such a collider might cost according to European accounting practices, assuming it were located at an existing laboratory, like CERN, that could absorb much of the construction management, R&D, and other costs into its normal operating budget. Nor does it include the costs of experimental detectors, contingency, or inflation. Adding those costs would push the total well north of \$10 billion, by my calculations. If, as many of us hope, the ILC were to be built in the US, the Department of Energy would insist that all of the other costs be included, making it—as correctly reported in *Science*—a \$10 billion to \$15 billion project.

Advocates of the ILC are taking a risky path that resembles all too closely the one followed by promoters of the Superconducting Super Collider in its formative years. In that case the additional costs were ignored, and only that of the collider itself was given during the early going. Thus the project initially seemed much less expensive than it eventually turned out to be. But as the

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famously described in 1932 as “continuous random networks.”¹ If those networks were random, then increasing their connectivity—by replacing divalent atoms like calcium with tetravalent atoms like silicon—should lead to a stiffness transition, which was present in early data. Compositional proximity to a stiffness transition enhances the ability of the melt to develop longer-range order and avoid crystallization by nucleation of crystalline clusters.² More recently, purified samples have actually exhibited two stiffness transitions, usually spaced about 10% apart on a suitable connectivity scale. The two transitions bound a new kind of topological intermediate phase, not defined by any kind of conventional symmetry but only by its special non-random connectivity. The internal networks associated with this novel phase have properties of great technological importance: They are rigid but still free of internal stress.³ That combination of properties makes it possible to produce window glass on large scales without having it crack or crystallize.

The discovery of self-organized networks in intermediate phases has generated “constraint theory,” a new kind of topological theory of strongly disordered solids. Constraint theory has a strongly non-Newtonian flavor. In crystals, many quantum and statistical properties are described by power-law scaling, but network self-organization is an exponentially complex combinatorial problem. (Mathematicians call such problems “nonpolynomial complete.” They are best addressed by the variational methods developed in the late 1700s by Leonhard Euler and Joseph Louis de Lagrange, methods that are the basis of Lagrangian mechanics.) In any case, the identification of many such systems has made possible the development of a new linear algebra for describing them; their properties are at best only marginally accessible to computer simulations on even the largest scales.⁴

The PHYSICS TODAY story concludes on a positive note: our enhanced appetites for a deeper understanding of the transition between localized and delocalized states in materials. There are often two such transitions, and the intermediate phase between them is much more exciting than either transition alone.

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Publishing perils include single-blind review

The Letters section of the January 2007 issue of PHYSICS TODAY (page 10) contained a discussion about the exacting and often confusing electronic paper submission process as an obstacle to getting published. I think a much more important and corrosive impediment to publication is single-blind peer review.

Peer review is the cornerstone of scientific research and advancement. It recognizes the importance of the objective reality and is a manifestation of the scientific method. It also keeps crackpots and just plain bad science out of the journals.

However, the single-blind peer review system has a fundamental flaw. It allows reviewers to assess the author(s) of a paper along with the scientific content and thereby permits nonscientific considerations to creep in. As is often true in other aspects of professional life, in peer review who you know can be as important as what you know.

Single-blind peer review discourages scientists from publishing in new fields, suppresses research from unknown or unaffiliated scientists, and adds irrelevant considerations to the review of scientific content. It may, in fact, be robbing science of its greatest breakthroughs right now. More than 100 years after Einstein's miracle year, what are the chances that an unknown 24-year-old patent clerk could revolutionize physical law in a peer-reviewed journal today? It seems extremely unlikely.

The arXiv.org server suffers the same problem in that an author must have a personal reference in order to publish; in fact, there the personal relationship is paramount and is presumed to substitute scientific merit.

If journals are really about scientific truth and integrity, and if we believe in the objective reality, peer review must be double blind. The single-blind sys-

tem is intellectual laziness at best, cronyism at worst.

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Tuning in to dye laser origins

The historical feature article on “The Early Days of Precision Laser Spectroscopy” (PHYSICS TODAY, January 2007, page 49) misidentified the inventor of the tunable dye laser as Peter Sorokin.

B. B. McFarland and I, both then at Korad Corp, were the actual “inventors” who first demonstrated that device and published the findings.¹ The experimental design with a diffraction grating as one of the resonator cavity mirrors, as discussed in the PHYSICS TODAY article, was first described by us. Tunability was one original aspect of that discovery; another was the narrowband energy efficiency of the laser, funneling a large portion of the original broadband laser emission into the narrow band of a selected wavelength.

Sorokin himself, in a published interview,² gave full credit to that paper and its authors, adding that he had “missed the boat completely, . . . just hadn't thought of it,” and saying that “Soffer and McFarland's result was a tremendous surprise.” The reason for his and everybody's surprise was no doubt the conventional but mistaken belief that the xanthine dye system, and in particular rhodamine 6G, was primarily inhomogeneously spectrally broadened at the relevant time scales. I examined the literature that supposedly validated that belief and found it unconvincing. The same surprising homogeneity story that upset published notions also turned out to apply to the cyanine dye family and the xanthenes embodied in plastic,¹ despite their famously long fluorescent lifetimes in solid solution.

That was some 40 years ago. The lesson to be learned now is that we must read everything with considered skepticism.

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