script in *Physical Review Letters* in 1965 (*Phys. Rev. Lett.* **14**, 380, 1965), at which time the velocity-dependent terms and our regularized meson fields that enabled us to treat *S* waves were still novel features among the then-evolving one-boson exchange models. In subsequent *N–N* studies [*Rev. Mod. Phys.* **39**, 594 (1967)] we worked towards implementing a fully relativistic treatment and finally succeeded [*Phys. Rev. D* **3**, 2076 (1971)], but found results in the 0–300-MeV region very similar to those obtained with a Schrödinger–Pauli treatment. However, in Dudley's PhD thesis on the *N*-nuclear problem using the Dirac equation, with the same scalar-vector meson fields, we could fit many observed nuclear properties with a minimal adjustment of parameters.

It might be noted that the cancellation of the major terms in a scalar-vector model of *N–N* or *N*-nuclear interactions suggests that at low energies the nuclear force should be characterized as a "moderate" force rather than a strong force. The strong force is manifested in the relativistic terms in scalar-vector theory as $v^2/c^2 \to 1$ and also in N–$\bar{N}$ studies, where the scalar and vector static terms add rather than cancel.

I hope these comments and this background add to your informative news item.

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Nonrelativistic spin

The news story "Relativistic treatment of low-energy nuclear phenomena," which appeared in March (page 20), contains a misstatement concerning the relationship between spin and relativity. We see there the assertion that "Spin is, after all, an intrinsically relativistic phenomenon." That this is not the case was clearly demonstrated[1] by Lévy-Leblond in 1967.

In his paper, Lévy-Leblond treats the Schrödinger equation the way Dirac treated the Klein–Gordon equation, seeking an equivalent differential equation that is first-order in the derivatives. As in the relativistic case, a four-component wavefunction emerges and transforms under rotations according to the direct sum of two spin-½ representations of the rotation group. (In the nonrelativistic case, the negative energy solutions are absent: Two of the four components are no longer independent.) Furthermore, Lévy-Leblond shows that when electromagnetic interactions are introduced, the Landé *g*-factor is equal to 2, just as in the relativistic case. Thus spin-½ particles emerge just as naturally in Galilei-invariant quantum mechanics as they do in the Poincaré-invariant case. Spin is not "an intrinsically relativistic phenomenon."

I hope that these results will become better known throughout the physics community, not only because of their fundamental interest but also because of the insights that they might yield in understanding phenomena that lie in the grey areas between the so-called "nonrelativistic" and "relativistic" domains.

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Long-time tails

In your interesting issue on Nonequilibrium Fluids (January) B. J. Alder and W. E. Alley and E. G. D. Cohen stated (or, at least, implied strongly) in their respective articles that the so-called "long-time tail" in the velocity autocorrelation function of an atom or a molecule in a liquid has not been observed experimentally. While this appears to be the case for atoms or molecules, the purpose of this letter is to point out that fairly unambiguous experimental evidence has been obtained recently for a long-time tail in the velocity autocorrelation function of spherical microscopic colloidal particles (diameter of order 1 micron) executing spontaneous Brownian motion in a liquid. These experiments were performed by dynamic light scattering, which measures the mean-square displacement of such a particle (in a suspension dilute enough that interactions between the particles can be neglected). Here the $t^{-3/2}$ long-time tail in the velocity autocorrelation function manifests itself as a $t^{1/2}$ term in the mean-square displacement.

The first such experiment was performed in 1976 by J. P. Boon and colleagues[1] who found indications of this $t^{1/2}$ term; however, their experimental error was not much smaller than the effect itself. Subsequently G. L. Paul and I studied[2] larger spheres (where the relative effect is large) and found clear evidence of the $t^{1/2}$ term. Here, however, the amplitude of the term was about $74 \pm 5\%$ of that predicted by the hydrodynamic theory outlined in the article of Alder and Alley. We emphasized that this discrepancy could well be caused by undetermined systematic errors in the experiments. Nevertheless, at about the same time, L. E. Reichl[3] suggested that consideration of rotational degrees of freedom in a suspending liquid composed of non-spherical molecules could modify the simple hydrodynamic theory. Most recently, however, K. Ohbayashi, T. Kohno and H. Utiyama have performed another dynamic light-scattering experiment[4] which again observed the $t^{1/2}$ term in the mean-square displacement having an amplitude that agreed with the simple theory within estimated experimental error (about 10%).

Thus, although the discrepancies mentioned above need to be resolved, I think one can claim that long-time tails have been observed experimentally. Of course a particle suspension is not a simple liquid, and unambiguous observation of a long-time tail in the latter remains an outstanding challenge to experimentalists. Nevertheless, the colloid experiments provide strong support for Alder and Alley's hydrodynamic picture of the long-time tail in a system where, because of the large difference in size between the particles and the liquid molecules, macroscopic hydrodynamics would be expected to apply.

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Interferometry: a bone to pick

Contemporary physicists are often so taken with their own accomplishments that the work of earlier researchers is sometimes ignored. Such is the case of a claim made in The article by James Underwood and David Attwood, "The renaissance of x-ray optics" (April, page 44). While the field of x-ray optics was generally well covered, the authors' discussion of x-ray interferometry was misleading. They claimed that Bonse and Hart "invented x-ray interferometry." This could not be farther from the truth and probably derives from similar claims made in articles by Bonse, and by Bonse and Hart, themselves.[1,2] Bonse and Hart did revolutionize x-ray interferometry with their invention of the monolithic Laue case device. However, the observation of x-ray interference patterns and the application of x-ray interferometry began more than fifty years ago.

Experiments to observe interference

with Cu Kα radiation (1.54 Å) were undertaken in 1912 by B. Walter and R. Pohl.[3] They repeated a Young's double-slit experiment attempted earlier[4] (also at 1.54 Å) and believed their work to be unsuccessful. However, a later study of their plates with improved densitometry revealed the predicted variation of optical density across the image. As the wave nature of *Röntgenstrahlung* became firmly established, researchers in Germany and Sweden began to sense the usefulness of short-wavelength interferometry. In 1931 H. Kiessing published[5] creditable interferograms obtained at wavelengths between 1.39 Å and 1.66 Å. He used x-ray interferometry to measure the thickness of films deposited upon polished surfaces, as well as to characterize the index of refraction of known film thicknesses.

In 1932 x-ray interferometry matured with the publication[6] of Gunnar Kellström's doctoral thesis, "Experimental investigations of interference and diffraction with long wavelength x-rays." Kellström fabricated a Lloyds mirror and a Fresnel mirror interferometer with which he measured the wavelengths of C Kα, Cu Lα, Al Kα, and Mo Lα characteristic radiation. One of his interference patterns can be seen in *Fundamentals of Optics* by Jenkins and White.[7] The fringe visibility was excellent, aided by the use of a 0.5-micron slit following his Coolidge-type x-ray tube. I am sure that Kellström would be surprised and amused by claims to the invention of x-ray interferometry in 1965.

Such inaccuracies poorly serve the needs of a creative scientific community. They inhibit further searches into work of the past, where one may often find long-unseen gems.

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THE AUTHORS COMMENT: In using the phrase "invented x-ray interferometry," we did not intend to imply that Bonse and Hart were the first to observe or to demonstrate the phenomenon of x-ray interference. Accounts of such demonstrations appear much earlier in the literature, as Paul Rockett has pointed out. A discussion of this work, much of which required exceptional experimental skill, would be appropriate in a comprehensive review article unlimited in permissible length or in the number of references. However, if "invention" in the scientific sense means the origination of a novel, versatile device or technique that enables important measurements to be made and new lines of research opened up, then Bonse and Hart may legitimately be referred to as the inventors of x-ray interferometry.

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Monopole skeptic

Thank you for April's excellent summary of the status of magnetic monopole search. As an old-time gauss-trap operator, I feel induced to make a few observations on the subject.

The concept of the isolated magnetic pole preceeds Dirac by many years, because Maxwell carefully pointed out that full symmetry of the magnetic field equations required magnetic charges, of *both* polarities. However, in the past hundred years no phenomena have been found that would be unequivocally explained by such charges. It is all very well to speculate concerning problems that might be explained by magnetic monopoles, if they exist. But such problems are as conjectural as magnetic monopoles. As you said, magnetic monopoles are not neutrinos. These had to exist to provide energy and momentum conservation in beta decay. Yet, as late as 1938 Arthur Eddington "did not believe in neutrinos."

For the above reasons I consider magnetic monopoles to be members of a null set. For the past seventeen years I have seen many megatons of various industrial products stored and retrieved by our automatic Hartman AS/RS machines, whose positioning was controlled by sensing heads detecting the magnetic field of "Null track," so the gradiometer trick is very "old hat" to me. (The Null Track/Sensing Head positioning system has been rendered obsolute by the development of optical readers for bar code, which enabled us to combine its bin center and height function with our Bin Location Indicating Device, so the gradiometer trick really is "old hat"; but the old systems still run well.)

Also, any self respecting monopole should have sufficient prescience to shear off from a good superconducting and *u*-metal shield. Bait the trap! Don't hide it!

Has anyone checked the Stanford