

membrane structure over the techniques that they did discuss. The Raman effect does not require the insertion of bulky molecular probes in the membranes as does the spin labelling technique, and the Raman effect is independent of the tumbling time of the bilayer, which may vary depending on the nature of the sample and its preparation—a fact that sometimes complicates the interpretation of the nmr data. Anyone interested in the physics of biomembranes must read the work in the Raman field as well as in the other areas which the authors have covered. A recent contribution from our laboratory (B. P. Gaber, P. Yager, W. L. Peticolas, *Biophys. Journal* **21**, 161 (1978)) gives recent references to important Raman work from many other laboratories.

WARNER L. PETICOLAS
University of Oregon
Eugene, Oregon

3/7/78

THE AUTHORS RESPOND: The 1971 paper of Lippert and Peticolas states that "the effect of cholesterol on dipalmitoyl lecithin multilayers is to change the sharp, cooperative gel-liquid crystal transition to a diffuse, noncooperative event." To most physicists this means that cholesterol kills the phase transition. The letter of Peticolas highlights one of the difficulties of interdisciplinary research because workers with different backgrounds sometimes use the same words to mean different things.

Concerning our rather brief mention of Raman work, space and reference limitations required us to be selective rather than comprehensive in our coverage. We apologize to the many workers whose contributions could not be acknowledged in this kind of article.

JOHN F. NAGLE
H. L. SCOTT
Oklahoma State University
Stillwater, Oklahoma

4/6/78

More on search for quarks

I was surprised by the reaction of William Fairbank to my letter in December (page 11) where I attempted to show the difficulties encountered in this kind of experiment. I would much prefer to avoid any further comment, but, obviously, I am compelled at least to reject statements such as: "Morpurgo suggests, without proof, that phase-sensitive ac measuring techniques are more affected by the noise than dc measurements": I never made such an absurd statement, as any reader can verify.

On the contrary, the phase-sensitive lock-in technique used by our group was clearly mentioned in our paper [G. Gallinaro, M. Marinelli and G. Morpurgo, *Phys. Rev. Lett.* **38**, 1255 (1977)]; this technique as well as the many precautions used to discriminate between spurious

and real residual charges will be illustrated in detail in a paper to be published by our group at the end of our set of measurements.

At this point I intentionally omit a more detailed analysis of Fairbank's comments to my letter; a new Ehrenhaft-Millikan dispute appears out of place.

GIACOMO MORPURGO
University of Genoa
Genoa, Italy

1/6/78

I regret any misunderstanding resulting from Giacomo Morpurgo's comments about our experiment or from our reply. Both experiments were carefully done and are significant contributions to an important problem in physics. We look forward to the complete publication of Morpurgo's results and will soon prepare a more complete article on our experiment.

It is important to note that the published results^{1,2} of the two experiments are not inconsistent with each other and are not necessarily inconsistent with any other published experiment. Together they show that there are very few fractional charges on 10^{21} nucleons. Morpurgo found 0 fractional charges on 10^{-3} grams of iron. We found 2 fractional charges on 0.6×10^{-3} grams of niobium. No other experiments have looked at the total charge on a larger amount of material without first removing all charged particles with a trap.³ All other experiments that have claimed more sensitivity depend on enrichment procedures and detection methods that require making assumptions about the properties of the fractional charges to guarantee detection. These assumed properties cannot all be known with certainty.⁴

We are working hard to take more data, and we urge Morpurgo to continue his important experiments.

References

1. G. S. LaRue, W. M. Fairbank, A. F. Hebard, *Phys. Rev. Lett.* **38**, 1011 (1977).
2. G. Gallinaro, M. Marinelli, G. Morpurgo, *Phys. Rev. Lett.* **38**, 1255 (1977).
3. A. M. Hillas, T. E. Cranshaw, *Nature* **184**, 892 (1959).
4. L. W. Jones, *Rev. Mod. Phys.* **40**, 717 (1977).

WILLIAM M. FAIRBANK
Stanford University
Stanford, California

5/4/78

I fully agree with the spirit of Fairbank's comment above and share his regret for any misunderstanding that may have intervened; we are continuing the measurements and I am pleased to hear that Fairbank is doing the same.

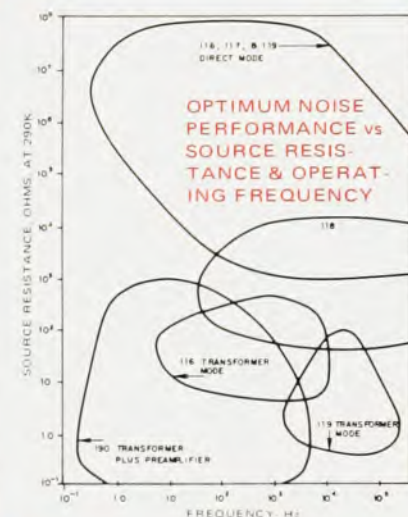
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5/25/78

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