letters

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rienced, yet our voices are only weakly raised. If we cannot protect our brethren on Earth, how can we expect to protect them in outer space where the environment is alien and inimical to human existence from the outset?

Space colonization represents a monumental effort in technology to achieve the worthy end of peace—yet it is an expedient cop-out. The hardest task is not to move mountains, but to move human hearts. And that is the task before us.

M. J. DUNN
McGhan Medical Corporation
5/16/78 *Santa Barbara, California*

THE AUTHOR COMMENTS: Mike Dunn has expressed our dilemma only too clearly. Simply because it is more difficult to change human nature on Earth than to establish a space colony capable of sustaining and defending itself in the far reaches of the solar system, we should try to give ourselves the other option. Although interpersonal violence will certainly travel with the space colonies, it is unlikely that terrorism or the mass annihilation madness will prevail within their confines. These can only be perpetuated by politicians in a world where many different languages and cultures prevail and where an increasing population competes for diminishing resources.

MALCOLM THACKRAY
SRI International
3/28/78 *Menlo Park, California*

Membrane phase transitions

The article by John Nagle and Hugh Scott entitled "Biomembrane phase transitions" contains a discussion concerning the effect of cholesterol on lipid bilayers which is, unfortunately, incorrect. The authors have chosen to ignore the initial Raman work of Joseph Lippert and myself (Proc. Nat. Acad. Sci. **68**, 1572–1576 (1971)) which showed clearly that the effect of cholesterol on membrane bilayers is not to "kill" the phase transition but in fact to greatly broaden it. The thermodynamic data which they cite are experimental artifacts induced by a finite calorimetric scanning rate in a calorimeter of insufficient sensitivity to observe such a greatly broadened transition. Recent measurements with the new Privalov calorimeter in several laboratories including our own have now shown that the original thermodynamic results must be revised. The improved thermodynamic measurements show that the effect of cholesterol is to broaden the melting transitions, thus substantiating the Raman work.

It is regrettable that these authors did not discuss the many advantages which the Raman effect has in determining

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
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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 apoligize 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 tecuniques 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
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Genoa, Italy
1/6/78

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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

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3. A. M. Hillas, T. E. Cranshaw, Nature **184**, 892 (1959).
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WILLIAM M. FAIRBANK
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5/4/78

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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