[1] J. Grover and P. Handler, preceding paper, Phys. Rev. B 6, 3145 (1972).

[2] P. Handler, S. Jasperson, and S. Koeppen, Phys. Rev. Letters 23, 1387 (1969).

[3] F. C. Weinstein, J. D. Dow, and B. Y. Lao, Phys. Rev. B 4, 3502 (1971).

[4] F. C. Weinstein, J. D. Dow, and B. Y. Lao, Phys. Status Solidi 43b, K105 (1971); 43b, K177 (1971).

[5] As emphasized in Ref. 3, the WDL procedure was limited to rather crude fitting of theory to experiment (by eye), due to computational limitations.

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[8] G. G. Macfarlane, T. P. McLean, J. E. Quarrington, and V. Roberts, Proc. Phys. Soc. (London) 71, 863 (1958); Q. H. F. Vrehen, Phys. Rev. 145, 675 (1966); M. Cardona and F. Pollak, *ibid.* 142, 530 (1966); D. E. Aspnes and A. Frova, Phys. Rev. B 2, 1037 (1970).

[9] Once the Weston interpolator becomes available, it will actually be easier to perform the exciton analysis than the one-electron analysis [H. Weston and J. D. Dow (unpublished)].

[10] Factors presently limiting the extraction of precise information from line shapes are the following: Experimental spectra should exhibit many oscillations, taken under uniform field conditions on pure high-resistivity samples, for a wide variety of (measurable) applied fields and temperatures. Theories are presently limited to the weak-broadening limit and are unable to go beyond the independent-band, isotropic effective-mass approximation. Theoretical work should concentrate on determining effects independent of adjustable parameters; while experiments should attempt ideal conditions. Still, the experimental problems are sufficiently difficult that theories of nonuniform fields [D. E. Aspnes and A. Frova, Solid State Commun. 7, 155 (1969)], and screening [W. A. Albers, Jr., Phys. Rev. Letters 23, 410 (1969)], and broadening should be refined to the point where precise information can be confidently extracted from electroreflectance line shapes.

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Comment on "Observation of Nonextremal Fermi-Surface Orbits in Bulk Bismuth"

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This comment on a recent letter by Henrich points out that that author misinterpreted an earlier paper by Dooley and Tepley. Thus, Henrich's work is not the first measurement of nonextremal Fermi-surface orbits in bismuth.

In a footnote to a recent paper,[1] Henrich refers to an earlier paper by Dooley and Tepley.[2] He correctly reports that some quantum oscillation periods reported in that paper did not agree with theory, as the authors had already pointed out. However, Henrich also discussed the primary effect reported in that paper. He alleges that the effect (which he chose to refer to as "small deviations") shown in Fig. 2, Ref. 2, which he erroneously identified as Fig. 1 of Ref. 2, "could be due to the effective masses they used ...." The point is, however, that effective masses in no way enter into the crucial part of that figure, which plots Bhargava's experimental extremal Fermi-surface cross sections[3] as measured by the de Haas–van Alphen effect, and Dooley and Tepley's experimental nonextremal cross sections as measured by the Landau-level peaks in the tilt effect. Bhargava's effective masses were only used to plot a theoretical nonextremal cross section which agrees quite well with the experimental points. Thus, Henrich's claim, that "there have been no unambiguous measurements on nonextremal Fermi-surface areas," is in error. Furthermore, we will publish in due course additional Landau-level peaks which give data where the theoretical nonextremal curve referred to above bends over.

After reading Henrich's reply[4] to this comment, additional comments should be made as follows:

(i) The bismuth sample orientation for the observations of nonextremal Landau peaks in the tilt effect[2] was known, in fact, well within 1°, much smaller than the uncertainty Henrich inferred. This was accomplished by x raying the sample bonded to a quartz delay rod which was clamped in the goniometer, parallel to the goniometer axis. The goniometer was rotated to find the x axis. By means of laser-beam reflection techniques, the sample could be mounted in the rotating sample holder with this orientation retained and the position of the rotation counter noted. The sample holder is quite rigid and moreover fit snugly in a Dewar which was very accurately vertical. The rotating-base Varian magnet was precisely leveled. Using these techniques sample orientations known to well within 1° could be readily achieved. Thus, the error in the nonextremal areas measured is estimated to be about 2%, generally outside the range of Bharagava's uncertainties in extremal areas.[3]

(ii) The theoretical arguments based upon the theory of Gurevich, Skobov, and Firsov[5] for giant quantum oscillations does not appear to be relevant. As one would predict, we did not observe giant quantum oscillations in our samples nor did we report them. The relevant theory seems to be that of Spector[6] for the tilt effect. The peaks we reported are in agreement with the predictions of this theory.

Finally, it should be pointed out that the purpose of the original comment was *not* to contest the fact that Henrich first observed nonextremal Fermi-surface cross sections *by means of* quantum oscillations, but rather to point out his erroneous statement concerning the lack of previous unambiguous nonextremal measurements. We have no desire to fault his excellent data.

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[6] H. N. Spector, Phys. Rev. 120, 1261 (1960); 125, 1192 (1962).

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Comment on "Observation of Nonextremal Fermi-Surface Orbits in Bulk Bismuth"-Author's Reply*

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This paper points out the uncertainties inherent in comparing Dooley and Tepley's ultrasonic Fermi-surface measurements with separate de Haas–van Alphen data and in trying to interpret the differences as due to nonextremal Fermi-surface orbits. Calculations based on the theory of Gurevich *et al.* are not consistent with Dooley and Tepley's interpretation.

In the preceding paper,[1] Tepley calls attention to an erroneous statement in a footnote to a previous paper by Henrich.[2] He correctly points out that, when the experimental quantum oscillation periods measured ultrasonically by Dooley and Tepley[3] are compared directly to the de Haas–van Alphen periods of Bhargava,[4] the electron and hole effective masses do not enter explicitly; they are derived at a later stage in the de Haas–van Alphen analysis.[4] This does not, however, prove that the differences between the two data are due to nonextremal Fermi-surface areas. We do not believe that the departures observed[3] are sufficiently outside of the experimental uncertainty inherent in comparing the two data to warrant identification as nonextremal areas. If they are, however, and if the electron relaxation time they quote is correct, then the theory of giant quantum oscillations[5] requires revision.

The largest difference observed by Dooley and Tepley[3] between the Fermi-surface areas measured using the ultrasonic tilt effect and the extremal ones from the de Haas–van Alphen data of Bhargava[4] is about 7% (see Fig. 2 of Ref. 3). Unfortunately, the orientation used there—sound wave vector $\vec{q}$ 8° from the binary axis in the binary-trigonal plane, and magnetic field $\vec{H}$ in the binary-trigonal plane—is very sensitive to misalignment of either $\vec{q}$ or $\vec{H}$. When $\vec{H}$ is rotated in the binary-trigonal plane near the normal to $\vec{q}$, the extremal Fermi-surface area for the ellipsoid they consider changes by 5.5% per degree of misorientation. If $\vec{H}$ is rotated toward the bisectrix axis, the rate of change is 3.2% per degree. While the relative orientation of $\vec{q}$ and $\vec{H}$ can be determined to within a few tenths of a degree by means of the tilt effect, the orientation of $\vec{q}$ or $\vec{H}$ relative to the crystal axes—very important if two different experiments are to be compared—is usually somewhat less accurate (of the order of 1°). It should also be noted that any rotation of $\vec{H}$ in the plane normal to $\vec{q}$ (toward the bisectrix axis in this configuration) is difficult to detect. It cannot be seen by the tilt effect and can only be determined if enough runs are made with the crystal orientation changed by known amounts to see the Fermi-surface symmetry in that direction. Dooley and Tepley report no such measurements,[3,6] and they only claim to be within 2° of the binary-trigonal plane in Ref. 6. Thus, their areas could be more than 6% different than Bhargava's near $\nu = 0°$. To these sources of (systematic) error must be added the uncertainties in the de Haas–van Alphen periods quoted by Bhargava. They range from 0.6