much that one author's definition is better than another's as that the existence of different definitions itself can cause both error and confusion. The direction of precessing electrons certainly has relevance to the direction of an applied field; and the direction of circular polarization of a plane wave is similarly normally referred to its direction of propagation. The confusion arises, in part, when, say, a right-handed circular polarization of a propagating wave is referred to an applied field direction which could be parallel or antiparallel to the propagation direction. When the applied field changes direction, the polarization remains unaltered in the first case but becomes reversed in the second.

When reading a paper on the subject, one needs to be aware of which definition has been used, particularly since it may not have been explicitly stated. Mr. Eid's preference for defining circular polarization with respect to the applied field rather than the direction of propagation is exemplified by his statement, "....the parameters are better referred to the direction of magnetization in order to avoid unnecessary confusion." This may be contrasted with our penultimate paragraph which concludes with "Defining the sense of circular polarization with respect to the applied field, as is sometimes done, introduces yet a further source of sign confusion to the subject." I feel that the confusion really comes from the existence of differing definitions (which is something that cannot be expected to go away), together with an author's failure to clearly state which definition is being used, rather than because one particular definition may be *inherently* confusing. Mr. Eid's point about the difficulty in referring the polarization to the propagation direction when the wave is at cutoff is well taken, but the difficulty persists in the unmagnetized case, in which only the coordinate axis survives as a reference direction. (This is a yet further possible source of confusion that we hadn't come up with in our earlier letter!) Since, in the absence of an applied field, the latter cannot be used as a reference direction, we had preferred the propagation direction for the definition. The very last thing we want is to switch definitions according to the presence or absence of an applied field, the difficulty of the cutoff case notwithstanding. I think the lesson of all this remains as set out previously, namely, that one should a) specify the definition one is using, and b) when quoting from the literature, make sure that an unwitting error in presuming, incorrectly, the use of a certain definition, is not made.

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Corrections to "A Short History of Microwave Acoustics"

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In regard to Section III of the above paper,[1] John Eshbach has brought to my attention that the original experiments on microwave magnetoelastic YIG delay lines were performed by him on disc-shaped geometries and published in 1962 [1]. His 1963 paper [2] gave the first details of the electromagnetic → spin → acoustic conversion process for a YIG disc, which is shown in Fig. 5 of the above paper[1] for a YIG rod. Premium in space precluded a full description of the entire conversion process for an axially magnetically biased YIG rod which is electromagnetic → magnetostatic → spin (exchange) → acoustic. This was originally proposed by B. Yazgan in her 1966 Ph.D. thesis submitted to Glasgow University and subsequently developed by J. H. Collins [3] and experimentally verified by B. A. Auld *et al.* [4]. Reference to the caption in Fig. 5 of the above paper[1] allows references [3] and [4] to be traced.

Also, on p. 1135 of the article,[1] it was stated that Graham Marshall and Ted Paige were awarded the Microwave Prize in 1974. They were, in fact, awarded the 1973 Best Paper Award of the IEEE Group on Sonics and Ultrasonics, along with their co-author Cleland Newton, for their research on multistrip couplers.

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Manuscript received November 13, 1984.
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