Reply to G. W. Bruhn

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I show that Bruhn's criticism of my article is based on misunderstandings on his part. Most of the criticisms of the Evans articles were given first in my works and in private communications to them.

In ref. [1] G.W. Bruhn claims that he found errors in my articles [2,3]. For instance, the following statements are made by Bruhn: "Since $\mathbf{B}^{(3)}$ is merely the longitudinal component of a field $\mathbf{B}$ that has an additional transversal component $\mathbf{B}_{\perp}$ Dvoeglazov's result contradicts the well-known Lorentz transform of the electromagnetic field where the longitudinal component remains unchanged." "This result proves that V.V. Dvoeglazov Equ. (11b) cannot be true."

However, in actual fact, I frequently called $\mathbf{B}^{(3)}$ "the so-called magnetic field"—note the inverted commas in the words "magnetic field" (first line, page 228 of [3]). Next (see the 5th line of page 230 of [3], just before equation (11b), which Bruhn doubts) I state: "The 3-vector $\mathbf{B}^{(3)}$ (which is defined by (1) [by Evans indeed]) may *not* be the entry of the antisymmetric tensor field; it is ... the entry of some 4-vector provided that the Evans's definitions for circularly polarized

radiation are used." I hope that Bruhn knows that the Lorentz transformation laws are different for an antisymmetric tensor field and a 4-vector field. Equation (11b) of the cited paper is precisely the transformation law for the 3-part of a 4-vector. Its parity properties are discussed in the discussion with Comay and Evans, *cf*. ref. [4a].

Moreover, in [2] (published prior to [3]), see Eqs. (9), I proved the statement by explicit mathematical calculations. I again stated explicitly: "...$B^{(0)}$ transforms as zero-component of the 4-vector and $\mathbf{B}^{(3)}$ explicitly as the space components of the 4-vector..."

Finally, a quite inaccurate statement is made at the end of the Bruhn paper. The *SO*(3) group is the subgroup of the Lorentz group. So, it is obvious that *SO*(3) symmetry (but different from that given by M. W. Evans) is compatible with Lorentz covariance.

Thus, in my opinion, the Bruhn paper amounts to diffamation. [5] While I acknowledge the trivial errors in the papers by M.W. Evans, E. Comay and G.W. Bruhn, I continue to state that there are NO calculational errors in my papers [2-4].

Bibliography

[1] G.W. Bruhn, http://www.mathematik.tu-darmstadt.de/bruhn/EM-Lorentz-Transform.html. In similar articles published in the journals Bruhn has somehow removed the references to my works. However, it is still on his website. I was not aware of the existence of Bruhn's criticism for a long time.
[2] V.V. Dvoeglazov, *Found. Phys. Lett.*, 10, No. 4, pp.383-391 (1997), physics/9611009.
[3] V.V. Dvoeglazov, *Apeiron*, 6, No. 3-4, pp. 227-232 (1999), physics/9801024.
[4] V.V. Dvoeglazov, *Found. Phys. Lett.*, 13, No. 4, pp. 387-393 (2000), physics/9907048; V.V. Dvoeglazov and J.L. Quintanar Gonzalez, *Found. Phys. Lett.*, 19, No. 2, 195-200 (2006), physics/0410169.
[5] Unfortunately, similar transgressions can be found in the papers and correspondence by M.W. Evans and E. Comay. So I am not going to enter into discussions with them in the future.