Comment on "The linear instability of magnetic Taylor-Couette flow with Hall effect"

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In the paper we comment on [1], the instability of the Taylor–Couette flow interacting with a homogeneous background field subject to Hall effect is studied. We correct a falsely generalizing interpretation of results presented there which could be taken to disprove the existence of the Hall–drift induced magnetic instability described in [2,3]. It is shown that in contrast to what could be concluded from [1], no additional shear flow is necessary to enable such an instability with an inhomogeneous magnetic background field, whereas for a homogeneous one it is. In the latter case, the instabilities found in [1] in situations where neither a hydrodynamic nor a magnetorotational instability exists are demonstrated to be most likely magnetic instead of magnetohydrodynamic. Further, some minor inaccuracies are clarified.

I.

The main purpose of this comment on the paper [1] is to prevent a wrong conclusion with respect to our work [2,3] which could be drawn from a wrong statement in the discussion section of [1]. There, at the end of the 3rd paragraph, the authors conclude from the invariance of their results with respect to simultaneous sign inversions of shear and Hall term that no instabilities are possible without shear. Besides the fact that this conclusion being considered isolatedly is not comprehensible, it is nevertheless true for the special case of a homogeneous background field $\boldsymbol{B}_0$, but not in general. As the scheme (40) of [1] is valid for inhomogeneous (axisymmetric) fields $\boldsymbol{B}_0$, too, and the quoted conclusion is drawn completely on its basis, the reader could be tempted to generalize it. He could then come to the end that the results on a Hall instability *without* shear reported in [2,3] are wrong. (Note, that the term 'shear' is used throughout [1] to refer to the macroscopic motion of a fluid.) Here, we will show that conclusions on necessary conditions for the existence of the instabilities in question can reliably be drawn on the basis of energy considerations. They support the possibility of a Hall instability without shear.

The linearized induction and Navier–Stokes equations describing the evolution of small perturbations $\boldsymbol{B}'$ and $\boldsymbol{u}'$ of the background field $\boldsymbol{B}_0$ and the shear flow (here: differential rotation) $\boldsymbol{u}_0$, respectively, read for a *homogeneous* $\boldsymbol{B}_0$

$$\frac{\partial \boldsymbol{B}'}{\partial t} = \mathrm{curl}(\boldsymbol{u}_0 \times \boldsymbol{B}' + \boldsymbol{u}' \times \boldsymbol{B}_0) + \eta\,\Delta\,\boldsymbol{B}' - \beta\,\mathrm{curl}(\mathrm{curl}\,\boldsymbol{B}' \times \boldsymbol{B}_0)$$

$$\frac{\partial \boldsymbol{u}'}{\partial t} + (\boldsymbol{u}'\nabla)\boldsymbol{u}_0 + (\boldsymbol{u}_0\nabla)\boldsymbol{u}' = -\nabla p'/\rho + \nu\,\Delta\,\boldsymbol{u}' + \mathrm{curl}\,\boldsymbol{B}' \times \boldsymbol{B}_0/(\mu_0\rho)$$

where we used the symbols introduced in [1]. Standard arguments yield the following evolution equation for the total energy E of the perturbations:

$$\frac{dE}{dt} = \frac{1}{2}\frac{d}{dt}\left(\int_{V'} \boldsymbol{B}'^2/\mu_0 dV + \int_V \rho \boldsymbol{u}'^2 dV\right) = -\int_V \left((\mathrm{curl}\,\boldsymbol{B}')^2/(\mu_0^2\sigma) + \rho\nu(\mathrm{curl}\,\boldsymbol{u}')^2\right)dV + \int_V \mathrm{curl}\,\boldsymbol{B}'\cdot(\boldsymbol{u}_0 \times \boldsymbol{B}')dV/\mu_0 - \rho\int_V \mathrm{curl}\,\boldsymbol{u}'\cdot(\boldsymbol{u}_0 \times \boldsymbol{u}')dV \quad (1)$$

with V' being the infinite space minus any volume with infinite conductivity and V the volume of the container. Of course, solutions with growing total energy are impossible, as long as $\boldsymbol{u}_0 = \boldsymbol{0}$. More generally, even if we would admit a rigid body motion for $\boldsymbol{u}_0$, growing solutions do not exist.

Situation changes qualitatively, if $\boldsymbol{B}_0$ is no longer homogeneous: The additional term $-\beta\,\mathrm{curl}(\mathrm{curl}\,\boldsymbol{B}_0 \times \boldsymbol{B}')$ occurring in the linearized induction equation results in the additional energy term

$$-\beta\int_V \mathrm{curl}\,\boldsymbol{B}' \cdot (\mathrm{curl}\,\boldsymbol{B}_0 \times \boldsymbol{B}')dV/\mu_0 \quad (2)$$

which quite analogously to the term $\int_V \mathrm{curl}\,\boldsymbol{B}' \cdot (\boldsymbol{u}_0 \times \boldsymbol{B}')dV/\mu_0$ is potentially capable of delivering energy. Hence, the argument concerning the necessity of shear for the occurrence of an instability in the model of [1] in fact supports our findings in [2,3], when the term 'shear' is no longer used to refer to macroscopic motions only, but is extended to the microscopic motions of the carriers creating the current $\mathrm{curl}\,\boldsymbol{B}_0/\mu$. If the latter should be capable of replacing the shear velocity $\boldsymbol{u}_0$ it must not be interpretable as a rigid body motion. Therefore, a background field exhibiting a *sufficiently curved* profile, is a necessary condition for the occurrence of the instability we reported on, as we stressed in all our papers on this issue. (A suitable profile for a plane slab $-1 \le z \le 1$ with its normal in z–direction is, for instance, $\boldsymbol{B}_0 = \hat{B}_0(1-z^2)\boldsymbol{e}_x$ as used in [2].)

As the energy term (2) contains only magnetic fields, the possibility exists that even in the absence of any macroscopic motions ($\boldsymbol{u}' = \boldsymbol{u}_0 = \boldsymbol{0}$), say in a crystallized neutron star crust, nevertheless an instability may occur. We demonstrated that this possibility is real.

With respect to the second additional energy term due to an inhomogeneous background field,

$$\int_V \boldsymbol{u}' \cdot (\mathrm{curl}\, \boldsymbol{B}_0 \times \boldsymbol{B}') dV/\mu_0 \qquad (3)$$

nothing more can be stated than that it is capable of delivering or consuming energy, too. Should it turn out that it can be responsible for an instability on its own, this instability had surely to be addressed as MHD, since (3) vanishes for $\boldsymbol{u}' = \boldsymbol{0}$ or $\boldsymbol{B}' = \boldsymbol{0}$.

II.

Another remark connected with the above seems to be appropriate. In the Results and Discussion sections of [1], the impression is given that the reported instability is primarily one of the flow. In our opinion, there are good reasons, and moreover even evidences provided by [1] itself to interpret it as a primarily magnetic (and not MHD) instability for conditions, in which the flow would be stable otherwise.

Considering the induction equation including differential rotation and Hall effect with the velocity perturbations suppressed (i.e., the kinematic case), one has formally the same equation as that describing mean–field dynamos with differential rotation and the so–called $\boldsymbol{\omega} \times \boldsymbol{j}$–effect, see [4,5]. From these calculations and from qualitative considerations, too, it follows that the sign relation between Hartmann number Ha and $d\Omega/dR$ reported in [1] (see, e.g., Sect. III) is just the one necessary for dynamo action, that is, a *magnetic* instability. (Note, that Cowling's theorem does not apply.)

In the Sect. III B of [1], a marginal curve in the $\mathrm{Re} - \mathrm{Ha}$–plane is given for the kinematic case $\boldsymbol{u}' = \boldsymbol{0}$. In that part of the plane where no hydrodynamic instability or magnetorotational instability (MRI) exists, it practically coincides with the marginal curve of the full system's instability. Thus, one may suppose that the velocity perturbations are simply "enslaved" by the magnetic ones in cases, in which the flow would be stable without Hall effect. Since $\boldsymbol{u}'$ gives rise to additional dissipation, the full system should exhibit smaller growth rates compared with those of the kinematic case. A hint on this is provided by Fig. 6 of [1] showing that for $1 \lesssim \mathrm{Ha} \lesssim 7$ in the full system a slightly stronger differential rotation is needed for marginal stability than in the kinematic case. In order to make checks of this supposition possible, we would like to encourage the authors to calculate and compare growth rates for the kinematic and the full MHD case. Moreover, the signs of those integrals in (1) - (3) resulting from the potentially energy–delivering terms could be inspected with the calculated eigensolutions inserted to judge the nature of the instability.

III.

In [1], the usage of the terms characterizing the conditions for magneto–rotational (MR) stability vs. instability is confusing: In all, three pairs of conditions are introduced, namely

- positive vs. negative shear
- $d\Omega/dR > 0$ vs. $d\Omega/dR < 0$
- $\hat{\mu} > 1$ vs. $\hat{\mu} < 1$, where $\hat{\mu} = \Omega_{\mathrm{out}}/\Omega_{\mathrm{in}}$,

but the reader is left with the impression that they were completely synonymous. Let us adopt the terms "positive vs. negative shear" as being equivalent to the condition of being MRly stable vs. unstable. That is, we equate them to the conditions $d\Omega^2/dR > 0$ vs. $d\Omega^2/dR < 0$, respectively. Of course, they are not identical with $d\Omega/dR > 0$ vs. $d\Omega/dR < 0$, respectively, since Ω is doubtlessly a signed quantity. For monotonous profiles $\Omega(R)$, 'positive shear' *is* equivalent to $\hat{\mu} > 1$, but 'negative shear' is not equivalent to $\hat{\mu} < 1$ except in cases where the sign of Ω doesn't change within the interval $[R_{\mathrm{in}}, R_{\mathrm{out}}]$. The background is that the MRI (and, of course, the purely hydrodynamic Taylor–Couette instability, too) cannot depend on the direction of the rotation with respect to an arbitrary co-ordinate system.

On the contrary and when confining ourselves to situations in which the reported instability can be considered as an essentially magnetic one as explained above, the instability condition depends on the *signed* quantity Ω, more precisely speaking, on its derivative $d\Omega/dR$ (and not on Ω itself). This follows from the linearized Hall–induction equation under the assumption of axisymmetry by virtue of

$$\mathrm{curl}\,(\,\Omega(R)R\,\boldsymbol{e}_\varphi \times \boldsymbol{B}'\,) = R\,\boldsymbol{B}' \cdot \nabla\Omega\,\boldsymbol{e}_\varphi\ .$$

According to the above discussion, the condition for the Hall instability reads (as perhaps meant in the abstract of [1]) $\mathrm{sgn}(d\Omega/dR)\,\mathrm{sgn}(\mathrm{Ha}) < 0$ for a positive Hall coefficient β. That is, for either sign of Ha there exists both a MRly stable and a MRly unstable situation admitting of the Hall instability (in agreement with (30) but not with (40) of [1]). This conclusion is missing in the discussion and, moreover, the abstract could be interpreted in the way that for positive Ha the Hall instability combines always with the MRI.

IV.

Within the discussion of Fig. 5 of [1] (Sect. III A), it is falsely stated that the existence of the current–free marginal solution $B'_R = B'_z = 0\,,\ B'_\phi \propto R^{-1}\,, \boldsymbol{u}' = \boldsymbol{0}$ requires *both* boundary conditions to be those of the perfect conductor. In fact, there is no reason why such a current–free (or vacuum) solution could not continue from the inner boundary $R = R_{\mathrm{in}}$ on to infinity what means nothing else than satisfying the corresponding vacuum condition at the outer rim $R = R_{\rm out}$. The necessary and sufficient condition for the existence of the vacuum solution everywhere outside the surface $R = R_{\rm in}$ is the existence of a net current in z–direction enclosed by this surface. Because an outer electromotive force is missing, a perfect conductor in the interior of the inner cylinder is needed. Then, e.g., an arbitrary surface current can flow without losses and therefore endlessly.

As a consequence, one would expect at first glance that the dashed and the dot–dashed lines in Fig. 5 of [1] looked similarly. But, in contrast to what a statement near the end of Sect. III A suggests, the vacuum solution satisfies the differential equations (16)–(25) of [1] for $m = \omega = k = 0$ and *arbitrary* Ha . Thus, an interpretation of the results presented in Fig. 5 for Ha $\to 0$ based on this solution becomes generally questionable since Ha $= 0$ is by no means special with respect to it.

V.

Considerations of the effect of turbulence on the Hall coefficient do exist [6,7] (see the last paragraph of [1]). Perhaps, it is appropriate to mention a recent revival of mean–field Hall–electrodynamics in [8] although there only the effect of the Hall–drift onto the α–coefficient is considered.

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