Calculation of Vertical Stability in an Inverse Aspect-Ratio Expanded Tokamak Plasma Equilibrium

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Reply to Referees' Comments

Let me thank the referees for their helpful and insightful comments on my paper. Here are my responses to their comments.

1. As suggested, I have relegated some of the more tedious sections of analysis to Appendices. I hope that this will make the paper more easily readable.

2. I have added some language to p. 3 of the Introduction to point out that my simplified model allows the plasma internal inductance to be varied, which the simple model of Freidberg et al. does not. This is significant because, according to Ref. 15, vertical stability has a strong dependence on l_i.

3. I have explained what I mean by the L/R time of the wall (in Appendix A.1), and have referred to this explanation whenever the L/R time is introduced.

4. I have modified Sect. II to give a fuller explanation of the r, θ, ϕ coordinate system. This explanation now mentions that the coordinate system is 'straight field-line' system. I have also mentioned that r is the mean minor radius of magnetic flux-surfaces (or the mean inverse aspect-ratio in its normalized form), and that a is the mean minor radius of the plasma boundary. I have also made clear that the type of equilibria considered in the paper are "diffuse" in the sense that both the equilibrium magnetic field and current are continuous across the plasma-vacuum interface.

5. I have introduced the $\exp(\gamma\, t)$ time dependence of perturbed quantities as soon as possible (in Sect. III.A). For the sake of consistency, I have now replaced ω by $i\,\gamma$ throughout the paper (because we are dealing with purely growing or decaying modes, rather than rotating modes.

6. I have added a section (Sect. III.D) in which I discuss plasma incompressibility. As mentioned, we know, from general principles, that the most unstable mode has $\nabla\cdot\boldsymbol{\xi} = 0$. However, the question is whether or not we can find a physical $\boldsymbol{\xi}$ field that allows for this. For up-down symmetric plasmas it is possible to divide axisymmetric instabilities into two separate groups. 'Vertical instabilities' give rise to predominantly vertical plasma displacements, whereas 'horizontal instabilities' give rise to predominately horizontal motion. My criterion for setting $\nabla\boldsymbol{\xi} = 0$ is automatically satisfied by vertical instabilities, but not by horizontal instabilities. It is important to realize that vertical instabilities are not merely rigid shifts of the plasma. In principle, they can have complicated harmonic content (i.e., they can include poloidal harmonics other than $m = 1$.)

7. I have added a section (Sect. IV.A) in which I explain why it is necessary to use the toroidal coordinate system in the vacuum. I have made it clear that the fact that μ is not a flux-surface label is not a problem. I have also added a comment at the end of Sect. IV.G that explains that the effectiveness of the matching between the r, θ, ϕ and the μ, η, ϕ coordinates systems can be tested by checking whether the symmetry requirements (64)–(68) are actually satisfied in the region immediately outside the plasma boundary. (It turns out that they are.) This section also explains that in order to calculate the stability of the resistive wall mode you need to do two separate ideal calculations, one in which the wall is treated as a perfect conductor, and one in which the wall is completely absent. The contribution of the resistive wall to the perturbed energy is taken into account in the Haney-Freidberg formula, (101). (See Refs. 24 and 25.)

8. I have added a section (Sect. III.I) in which I discuss the contribution of the plasma surface to the perturbed ideal potential energy. In this section, I explain why this contribution is zero for the type of diffuse equilibria under consideration.

9. I have rephrased the comment after Eq. (47) to make it clear that Eq. (46) applies to $m \neq 0$.
10. In Sect. VI, I have tried to explain the physical significance of the "wall parameter" α_w.
11. In Sect. VII.B, I have explained that ω is a geometric poloidal angle that is different from the straight poloidal angle, θ. ω is only introduced as a means of parameterizing the shapes of magnetic flux-surfaces in an intuitive manner.
12. In Sect. VIII, I have added more language to make it clear exactly what needs to be continuous across the plasma-vacuum interface. I have also renamed the pressure peaking parameter, μ, since this was causing confusion with the toroidal coordinate μ.
13. In Fig. 7, I have added an insert to make it clear that pressure does influence δW_{pw}. The shifts in the no-wall and perfect-wall δW values with pressure are about the same. However, the relative shift in δW_{pw} is much smaller than that in δW_{nw} simply because the former is a lot smaller than the latter.
14. In Sect.VIII.F, I have improved the discussion of the l_i scans to more accurately describe the data shown in Fig. 7.
15. I have made it clear at a number of points in the paper that the reason that the TJ toroidal tearing mode code can perform the calculation is because the same basis solutions used to construct tearing mode eigenfunctions can be rearranged in a different manner to construct ideal eigenfunctions.
16. I have modified Sect. A.5.a to make it clear that Eq. (A75) really does reproduce $\hat{r}$.
17. I have rephrased the discussion of the Shafranov shift on p. 48 to make it clearer.
18. I have corrected the various typographic errors that the referees pointed out.