On the Minimum Energy Configuration of a Rotating Barotropic Fluid: A Response to Narayan & Pringle astro-ph 0208161

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In a recent posting, Narayan & Pringle (2002, astro-ph/0208161) criticized a paper written by the current authors (Fromang & Balbus 2002, astro-ph/0207561 hereafter FB02). FB02 presented a second order variational calculation and concluded that supersonically rotating barotropic fluids had lower energy states neighboring that of equilibrium. (Note that this is not the same as claiming instability, a point noted explicitly in FB02.) Narayan & Pringle challenged this, presented a specific class of equilibrium solution, and claimed to prove that it represented a global energy minimum. They concluded that FB02 was incorrect.

The calculation presented in FB02 is indeed incorrect. The difficulty is that the second order changes in the energy cannot be calculated directly from the energy itself, even when the variations are restricted to satisfy the constraints of mass and angular momentum conservation. Rather, the constraints themselves change the effective functional form of the energy, by forcing additional second order correlations that would otherwise be absent. Narayan & Pringle (2002) is therefore correct in stating that our second order variational calculation is in error.

Nevertheless, we are compelled to respond to the critique of Narayan & Pringle (2002). Our reasons are twofold: (1) the critique unfairly misrepresents statements in FB02; and (2) the main discussion presented in the critique appears itself to have flaws. In particular, the conclusion of Narayan & Pringle that their chosen example is a global minimum with respect to arbitrary variations from equilibrium does not follow from the argument presented in their paper. We very briefly discuss these two points in turn.

1. Lower energy states and instability. The existence of lower energy states neighboring the equilibrium solution is not a sufficient criterion for instability. This point is stated explicitly in FB02:

"Note that our procedure does not ensure instability in the latter case [supersonic rotation], it simply shows that neighboring states of lower energy exist."

Unfortunately, Narayan & Pringle misrepresent FB02, stating, for example:

"Since FB02 claim that the only stable barotropic configurations are those that rotate uniformly and are subsonic at all radii, ..."

Throughout the critique, the existence of lower energy states is referred to as a “stability criterion.” No such claim was ever made in FB02. A stability analysis of a uniformly rotating polytrope in an external potential need not show any unstable modes, any more than a stability analysis of, say, a Keplerian disk is bound to show linear instability, even though such a profile is not an energy minimum. When a weak magnetic field is added to a Keplerian disk, no matter how small the field energy, pathways are opened to states
of lower disk energy and the MRI ensues, even for purely axisymmetric disturbances. In
the case of a rotating polytrope, the question is not whether the system itself is directly
unstable per se, it is whether subsequent evolution drives the initial state further away
from its original equilibrium when one makes energetically negligible modifications — say
a weak magnetic field and a tiny angular velocity gradient.

2. Global Energy Minima. Narayan & Pringle analyze rotating polytropes in the fixed
external potential

$$\Phi = \frac{\Omega_K^2}{2}(R^2 + z^2)$$

where $\Omega_K$ is a constant and $R$ and $z$ are cylindrical radius and vertical coordinate re-
spectively. For a given mass $M$ and angular momentum $J$, it is shown that there is a
unique equilibrium solution. Combining this result with the positive-definite character of
the energy $E$, Narayan & Pringle conclude that the energy extremum represented by their
solution must be a global minimum.

Clearly something more is required to establish a global minimum. This line of rea-
soning is insufficient to establish the existence of a local minimum, let alone a global one.
(A gaussian function is everywhere positive-definite and possesses a unique extremum,
which is, of course, a maximum.) We do not wish to belabor this issue or other technical
difficulties with the presentation, since the point of disputing the variational calculation
is moot. The point of physics, however, is not without inter-

Consider the gravitational potential arising from a constant density sphere of large but
finite radius. Within the sphere, the potential is exactly of the above form (perhaps with
an inconsequential additive constant), while outside the sphere the potential is Keplerian.
There will be values of $M$ and $J$ for which the unique equilibrium solution lies entirely
in the sphere, and is of the precise form considered by Narayan and Pringle. The total
energy is bounded from below (and can thus be made positive by adding a trivial constant
to the potential). But for any equilibrium solution lying entirely within the sphere, there
exists another state with the same $M$ and $J$ that has a lower energy: one in which the
mass distribution is essentially identical, and the angular momentum is removed to a ring
of arbitrarily small mass and arbitrarily small total energy at arbitrarily large distances.
(Supersonic disks do in fact avail themselves of these lower energy solutions.) Note that
if the potential rises sufficiently steeply at large distances, the argument need not hold.
Thus, the nature of the potential at large $r$ plays a role in establishing the existence of
global minima. If the solutions of Narayan & Pringle (2002) are in fact true global minima,
this is dependent upon the rapid growth of the harmonic potential at large distances, and
is not a simple consequence of the existence of a unique extremum and positivity of the
energy.