HOW CAN IT ALL BE STABLE?

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My contribution to this Joint Discussion can hardly be termed very novel or original. Rather, I just want to call your attention here to two headaches – one perhaps only a hangover, but the other a real migraine. Sooner or later these two headaches beset anyone who persists in asking how the random motions of stars in the only vicinity where we tolerably observe them can possibly jibe with the sensible presumption that not only our neighborhood but this entire Galaxy should by now be reasonably stable.

1. Local Stability

There is of course nothing strictly 'local' about any problem involving the far-reaching gravity. Yet if some conceivable instability of our collection of nearby stars comes even close to deserving such a label, it is surely the Jeans instability or tendency toward gravitational collapse. After all it is not difficult to estimate (e.g., Toomre, 1964) that the most troublesome of such incipient clumpings ought to have significant dimensions (such as half-wavelengths) of the order of 3 or 4 kpc. Even the latter scale is probably not too large a fraction of our distance from the galactic center for 'local' analyses in the WKBJ spirit to remain coarsely trustworthy.

It seems just as clear, however, that any Jeans instability here can really be honored only *in absentia*. The reason is that any actual clumping tendencies in our disk of stars would exhibit *e*-fold growth times about as short as 10^8 yr. Hence any such troubles now could scarcely have been the first. Yet if similar instabilities indeed did arise in the past, it seems a safe bet that already they would rapidly have increased the random motions of the stars. And that in turn should soon have cured the problem – for there seems little question that all simple Jeans instabilities are avoided once the typical random speeds of stars exceed some relatively modest minima.

With much of this already in mind when I first considered such matters quantitatively in my 1964 paper, I remember being very struck that our locally observed random velocities seemed barely adequate even for that humble purpose. This is not to say that there seemed any real danger of contradiction, but only that the observed and required motions appeared identical within their uncertainties. However, in retrospect I have often wished that I had not been so captivated by that seeming agreement. For one thing it caused me to overlook one significant correction (to be discussed below) which tends to increase our margin of local stability. Far more important, it also lulled me – and perhaps others – into a false sense of security that various other kinds of collective instability of stars in this Galaxy were probably not much harder to suppress. We know today that isn't so. But let me not race ahead yet to that second and more serious headache. What I want to stress first is simply the

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frustration that despite the lapse of a decade, the uncertainties in the observed data remain such as to leave our actual factor of safety against even the local clumpings still quite poorly known.

To review those numbers, let us recall my old local criterion for the stability of a supposedly razor-thin disk of stars with a Schwarzschild distribution of horizontal velocities: It asserted that no short axisymmetric disturbances remain unstable if the rms radial speed σ_u exceeds

$$\sigma_{u,\min} = 3.36\, G\mu/\kappa\,, \tag{1}$$

where G is the gravitational constant, μ is the projected mass density, and $\kappa = [-4B(A-B)]^{1/2}$ is the so-called epicyclic frequency. Within its limited sphere of competence, that criterion itself seems to have fared well enough over the years. For instance, Goldreich and Lynden-Bell (1965) partly corroborated it through their analogous local findings for gaseous disks. Graham (1967) verified for star disks with several other sensible velocity distributions that the stability condition is likewise very similar. Julian and Toomre (1966) even reckoned that the criterion is not ruined by *short* non-axisymmetric disturbances. And Hohl (1971) showed among other things that when his full-disk n-body experiments were constrained to be axisymmetric, the speeds prescribed by Equation (1) were indeed about adequate for overall stability.

Yet some systematic correction for the finite thickness of a disk is clearly in order: Though I had guessed that such a reduction of $\sigma_{u,\min}$ might amount to perhaps 15 or 20%, first Shu (1968) and then Vandervoort (1970a) determined much more reliably that the numerical factor in Equation (1) had better be replaced by about 2.6 for an assumed ratio of vertical-to-radial motions $\sigma_w/\sigma_u \cong 0.6$ like observed. Finally, if some small fraction of the total projected density μ were to consist of cold gas instead of moving stars, its mere presence would *raise* the required stellar speeds by almost the same ratio; assuming 10% gas hereabouts, we thus arrive at the adjusted minimum

$$\sigma_{u,\min} \cong 2.9 G\mu/\kappa \tag{2}$$

to be used below.

Unfortunately this simple local formula remains distinctly more certain than the set of 'observed' quantities σ_u, μ and κ which it invites us to intercompare. The trouble is that none of those three quantities is obtained very directly. Probably best known is the epicyclic frequency $\kappa = 32$ km s^{-1} kpc^{-1} that is consistent with the conventional values $A = 15$, $B = -10$ for the local Oort constants; however, even it must be deemed uncertain by at least 10% and conceivably 20. As for the projected density of matter in our *disk* here, I prefer to think that $\mu = 75 \pm 15 M_\odot$ pc^{-2}. This estimate rests largely on the well-known deductions of the vertical force by Oort and others, most recently by Lacarrieu (1971). Though the end product of such studies has usually been a local volume density, the method in fact determines primarily a mean or projected density of all matter contained within modest heights z of the order of 200 or 500 pc above and below the Sun. Hence Oort's (1960) implied 62, 71 and $75 M_\odot$ pc^{-2} within $z = 400$, 600 and 800 pc, respectively, seem almost as significant as his $\varrho(z=0) \cong$

$\cong 0.15 M_\odot\ \mathrm{pc}^{-3}$ that is usually cited – and they are surely more accurate than if that volume density had simply been multiplied by some assumed equivalent thickness. As it happens, those particular values must now be increased to about 66, 77 and $83 M_\odot\ \mathrm{pc}^{-2}$ (cf. Vandervoort, 1970b) to account for non-local contributions to K_z implied by $A=15$, $B=-10$ instead of $A=19.5$, $B=-6.9$. The extrapolated total thus comes to perhaps $\mu=90$, but it seems fairest to reduce it slightly both to exclude various transient halo-like stars, and for one other reason: If indeed, as now seems likely from the work of Plaut, Clube and others, our measured galactocentric distance $R_0 \cong 10$ kpc is overdue for downward revision, such independent estimates as $\mu(R_0)= =114$ obtained from the 1965 Schmidt model seem themselves destined to shrink to roughly 90 if $R_0=9$ and to perhaps only $70 M_\odot\ \mathrm{pc}^{-2}$ if $R_0=8$ kpc (cf. Toomre, 1972).

If in fact we adopt $\kappa=32$ and $\mu=75$, Equation (2) claims $\sigma_{u,\min}=30\ \mathrm{km\ s}^{-1}$ to be the least rms radial speed that can be presumed adequate. However the alternatives $\mu=60$ or 90 would of course have yielded $\sigma_{u,\min}=24$ or $36\ \mathrm{km\ s}^{-1}$. And taking into account also the uncertainty involving κ, we observe that just the required random velocity can conceivably still range between extremes differing by a factor of two.

To be compared with this elusive target is the data on the actual random velocities of nearby stars. That topic has already been reviewed at this Joint Discussion by Wielen, and here I simply wish to reemphasize three things he told us.

The first is that it seems about as true as ever that not only do K and M dwarfs constitute the bulk of the *known* nearby stellar density, but also their own galactocentric radial motions *in this vicinity* seem best characterized by $\sigma_u=$mid-30 $\mathrm{km\ s}^{-1}$.

The second point concerns that oversight of mine to which I have already alluded. As first stressed by Vandervoort (1970b), any likely positive correlation between the vertical and horizontal motions of stars means that our $z \cong 0$ mid-layer sampling volume tends to be deficient even in radial velocities compared with stars found at various heights in this disk. Indeed Vandervoort has already suggested that the nearby σ_u's should for this reason be multiplied by $(\pi/2)^{1/2} \cong \frac{5}{4}$ to become truly representative – but his example was somewhat arbitrary and hence inconclusive. However Wielen has now come along and done us a related service that is both beautiful and convincing in its simplicity: He merely weights the observed radial motions of the McCormick selected-area K and M dwarfs with their likewise observed vertical speeds (though even this presumably still under-compensates for those 'polling errors'), and thereby he judges the correct rms speed to be no less than $48\ \mathrm{km\ s}^{-1}$. To be sure, to exclude the undue influence upon such statistics of a few passing halo stars, I am inclined to diminish Wielen's estimate slightly. Yet even so it seems hard to dislodge this new impression that the typical radial dispersion of the most common known disk stars is $\sigma_u=$low-to-mid 40 $\mathrm{km\ s}^{-1}$.

The third and last point is actually the most frustrating: As Wielen has already implied, even the inclusion of various earlier main-sequence stars, and of the giants, the known white dwarfs, and presumed dark companions still means that of the three-quarters or so of Oort's $0.15 M_\odot\ \mathrm{pc}^{-3}$ that cannot comfortably be attributed to the local volume density of interstellar material, roughly half remains to this day totally unidentified. Perhaps one should not even speculate on the motions of such 'missing' stars. However, whether or not that other half turns out to consist of many yet fainter M dwarfs and/or white dwarfs and/or some yet more exotic objects, it seems to me historically implausible that their present random motions could on the whole average less than those of the dK and dM stars just cited. On the contrary, their average motions (like their ages) seem apt to be even greater.

(In case anyone wonders, I remain skeptical of the reality of the dense local layer of faint, low-velocity M stars that has been suggested by the recent work of Weistrop (1972) and Murray and Sanduleak (1972)). At issue here is chiefly the mean distance of the stars detected by these workers: If one accepts fully the reasoning of Murray *et al.* based on transverse motions, the nearby mass density of such stars emerges as an impressive 0.05 $M_{\odot}\text{pc}^{-3}$, but the indicated less-than-10 km s^{-1} spatial motion in any one coordinate is even more startling. I find it incredible that so small a velocity dispersion could have survived random gravitational forces from gas concentrations, various spiral wave sloshings, and perhaps even Jeans instabilities for the presumed large age of those stars; moreover, even if one accepts that such stars provide all of Oort's 'missing' mass in the *volume* near the Sun, the small implied thickness of their disk means that they still cannot account for the bulk of the *projected* missing mass.)

To conclude, the above evidence suggests that the best single estimate possible nowadays for the ratio Q of the existing peculiar motions to the minimum required ones in the galactic disk near the Sun must be roughly 1.5. We have seen however that this simple factor of safety could easily range between 1.2 and 2.0, and it may possibly climb yet higher.

Thus the Jeans instabilities now seem certifiably impossible. But I would not have inflicted all this numerology upon you only to establish something so 'obvious'. I confess I had another motive as well: Though many of you may not have realized it, the ratio Q also plays a surprisingly sensitive role in the ability or willingness of disks of stars to carry density waves of the sort envisaged by Lin and Shu. With $Q=1.0$, the region of conceivable waves extends all the way from the so-called inner Lindblad radius to the outer. However, already when $Q=1.5$, the Lin-Shu-Kalnajs dispersion relation no longer admits any waves at all within a fairly extensive intermediate annulus where a certain relative frequency $|\nu|<0.6$ (cf. Figure 1 of Toomre, 1969). And when $Q\cong 2$, the remaining tightly-wrapped wave picture has become so cramped as to be practically valueless.

One may of course, if one wishes, reverse this reasoning and argue that just the likely existence of waves in this Galaxy implies that $Q\cong 1$. That is not my aim here. I merely wanted to caution explicitly that – contrary to a misimpression which I am afraid I helped begin – it is *not* the observations of σ_u or μ or κ which compel one to adopt $Q\cong 1.0$ for spiral waves or any other purpose.

2. Overall Stability

All these nitpicking details, however, pale by comparison with a near-scandal in our

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understanding of galactic structure that has surfaced unmistakably only during the past year or two. In its potential impact, this particular difficulty reminds me already of the solar neutrino embarrassment from another area of astronomy: It raises doubts even about fundamental assumptions, it seems unlikely to vanish overnight, and I can here do no more than describe it briefly.

The difficulty in short is that at least four independent analyses or numerical experiments have now converged to testify that disks of stars remain very susceptible to large-scale or 'global' instabilities of a non-axisymmetric sort, even when endowed with random velocities well in excess of those estimated to suppress the simple local clumpings. Though their exact nature is still unclear, these growing disturbances are not Jeans instabilities by any reasonable standard. Rather, they seem to represent a strong, if perhaps only transient tendency of the central regions of the model disks to develop bar-like structures, often accompanied by wide-open but temporary spiral structures or even waves farther out.

What is startling about these large-scale instabilities is not their occurrence as such (since local studies could logically neither predict nor refute them) or even their frequent bar-likeness (which is vaguely reassuring in view of the many barred spirals found in the sky). The real surprise is that such troubles seem unavoidable in all thin disks of stars examined, unless the kinetic energy of their random motions is more than two and a half times the kinetic energy of their systemic *rotation* itself!

To my knowledge, something like this alarm was first raised by Miller, Prendergast, and Quirk (1970), and by Miller (1971). Those authors remarked that their 10^5-body "calculations typically produce 'hot' systems that are largely pressure-supported", with "velocity dispersions... considerably greater than those needed to stabilize" in the local sense. Unfortunately no one knew at the time just how seriously that warning was to be taken: For one thing, Miller *et al.* had quickly added an inelastic or 'gas' component to their stellar disks. Though intended for greater realism, that addition made it unclear how much of the increase was to be blamed on the stars themselves and not on the evident Jeans instabilities and clumpings of the 'gas'. Likewise uncertain were the effects of possible numerical errors, inasmuch as both the integration steps and the potential mesh used by Miller *et al.* were purposely quite coarse. And then, too, an early report of related *n*-body calculations by Hockney and Hohl (1969) had claimed no such further instabilities.

The plot thickened, however, with the fine analytical study by Kalnajs (1972, and earlier) of the various linear modes of certain very thin disks of stars possessing a uniform angular velocity of rotation. Kalnajs again found strong indications that "disks which are hot enough to avoid axisymmetric instabilities can still evolve rapidly in a nonaxisymmetric manner". Yet a skeptic could have retained some mild reservations, this time notably about the strange velocity distributions required in those disks to begin with.

A third investigation deserved even fewer such quibbles: This one, by Hohl (1971), in a sense only continued the work of Miller *et al.* using roughly as many mass points; however, it distinctly excelled in that it consisted of numerous separate experiments, was much smoother in its numerical treatment, and above all dealt only with imagined stars. (It also retracted as premature the earlier contrary claims of Hockney and Hohl.) Again the bulk of the evidence was that "disks of stars are considerably more difficult to stabilize than indicated by local analyses". Hohl also guarded against the suspicion that the disks might have become excessively hot by having begun too unstable; he did so by 'cooling' one such stable final disk, and by finding that fresh instabilities soon reappeared. Finally, even possible relaxation effects as a cause of excessive 'heating' seem now to have been largely exonerated by Hohl (1973).

Most impressive is the fourth and latest chapter of this unfolding story. It is impressive not because the few-hundred-body calculations which Ostriker and Peebles (1973) undertook were themselves very remarkable. Rather it is so because it occurred to Ostriker (1973) to propose and to test numerically a new stability criterion that turned out to unify all four of the investigations: He wondered if it is as true of the thin galaxy models as it seems true of many models of differentially rotating and inhomogeneous stars (cf. Ostriker and Bodenheimer, 1973) that the criterion for the avoidance of bar-making practically matches that for the non-bifurcation of the Jacobi ellipsoids from the classical Maclaurin spheroids. In that classical setting, secular instability toward triaxial or bar-like forms occurs whenever the total kinetic energy of rotation, T_{rot}, exceeds a meager 13.8% of the absolute value of the potential energy W of the system. Practically the same critical value was found for the star models just cited. And now in a similar vein, Ostriker and Peebles report that the approximate criterion

$$T_{\mathrm{rot}}/|W| \cong 0.14 \pm \text{perhaps } 0.02 \tag{3}$$

characterizes not only their own n-body calculations but also the 'coolest' of the largely pressure-supported stable disks achieved in every one of the three previous investigations!

Though of course it is no proof that every conceivable model disk must be as hot to be fully stable, this astonishing numerical agreement means at the very least that the Ostriker-Peebles criterion is an excellent rule of thumb summarizing all the available evidence. Thanks to the virial theorem, an equivalent summary would have been that the portions T_{rand} and T_{rot} of the total kinetic energy associated with random and mean rotational motions, respectively, must satisfy

$$T_{\mathrm{rand}}/T_{\mathrm{rot}} \gtrsim 36/14 \cong 2.6 \tag{4}$$

for a completely stable equilibrium.

For various reasons including our locally-observed stellar motions, such a ratio seems about the reciprocal of what one might intuitively have expected to find in a real spiral galaxy. Hence it raises all sorts of questions and worries about the whereabouts of so many high-velocity stars and/or other mass points not only in our Galaxy but also in others which have heretofore been thought relatively cool and disk-like. Possibly the answer lies in major stellar halos, such as Ostriker and Peebles have already suggested tentatively, and within which relatively cool disks might still be embedded stably – but it is hard to believe, for instance, that any disk instabilities themselves would ever have propelled stars to great heights in the z-direction.

The only sure bet seems to be that, from now until it has been resolved, this second of the headaches that I wanted to complain about will seriously plague *all* large-scale galactic dynamics.

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DISCUSSION

Van Woerden: I believe there is no real problem in assuming two thirds of our Galaxy to have random motions of about 200 km s^{-1}. Halo objects have such velocities and can (I think) account for 70% of the mass of our Galaxy.

Oort: I do not find a real difficulty with the supposition that a large proportion of the mass of our Galaxy is contained in a halo and 'central bulge'. One has to admit only a small density of late-type subdwarfs in the vicinity of the Sun. In an article in Vol. 5 of *Stars and Stellar Systems* I estimated that a density of 0.005 solar masses per cubic pc near the Sun would produce a halo having a mass equal to the entire mass of the galactic system if one makes the reasonable assumption that the space density of the halo is proportional to the inverse third power of the distance from the centre. The density mentioned is only about 10% of the 'missing' mass density, and could in my opinion be easily furnished by subdwarfs which are intrinsically fainter than the faintest known subdwarfs.

I should also like to draw attention to the dynamical model of the Galaxy worked out several years ago by Woltjer and Ng. This was, I believe, the first model of the entire Galaxy in which full account was taken of Poisson's law. Ng obtained likewise the result that the velocity dispersion of the bulk of the mass must be very high.

Toomre: I largely agree with both of you. Yet it also seems to me that the really tough nut to crack is not whether a massive halo is plausible, but to establish that it indeed *must* exist. I am afraid that Ng's theoretical models offer no such proof whatever; his method of construction all but guaranteed that they would be very hot.

Freeman: We really have little reliable data yet on the local density of halo stars or on the distribution of density with radius in the galactic bulge, so it is difficult to decide whether or not the galactic bulge is capable of stabilizing the disk in the way Ostriker and Peebles suggest. On the other hand, there are plenty of disk galaxies (spirals and S0) with very weak bulge components (see the Hubble Atlas). The bulge components must be much less massive than the disks in these systems, unless their M/L is extremely large, so it seems unlikely that disk stabilization by the bulge is a general feature.

Toomre: That is certainly one frustration with postulating major halos. Those can indeed remain blissfully unknown to us if M/L is large enough, but like you I would be happier if the average edge-on spiral gave some decent positive evidence of such halos.

Innanen: Unless one makes the proposed massive halo with a radius smaller than R_0, it seems to me that placing $10^{11}M_{\odot}$ in the halo unavoidably contributes significantly to $\mu(R_0)$ and further aggravates Toomre's pleas for lower values of μ. Even in Innanen's 1966 model cited by Toomre in the discussion (*Astrophys. J.* **143**, 163) a halo of $0.4 \times 10^{11}M_{\odot}$ increased μ from 74 to $98M_{\odot}$ pc^{-2}. In more recent models (Innanen, 1973, *Astrophys. Space Sci.*, in press) a more modest halo of 1 to 2×10^{10} $M_{\odot}$ already contributes at least 10% to μ so it is not really easy to hide it.

Toomre: You are here talking of the total projected density, from the halo and disk together. In my discussion of the local stability I tried to include only the material from the most evident disk.

Miller: The Ostriker-Peebles and your earlier criterion both refer to initially axisymmetrical configurations. It may be possible to stabilize a non-axisymmetrical system more easily.

Toomre: I wish we could be sure that is so!

Mestel: Does the rapid heating of the disk occur whatever the details of the physical processes by which stellar energy is tapped by the gaseous component and radiated away? For example, if one pretended that random velocities are killed if they exceed a modest value (much less than the circular velocity), wouldn't the disk-like structure be maintained? Doesn't the Ostriker-Peebles result depend on the fact that star-gas interactions are much less effective for high relative motions?

Miller: Our model galaxies had two components, one of which was cooled and one of which was not. The cooled component would end up being cool enough, but the un-cooled component was very hot – as in the models that Toomre has talked about. We also tried some models in which the entire population was cooled – but these collapsed catastrophically.

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