DENSITY-WAVE SPIRAL THEORIES IN THE 1960s. II.

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Introduction[2]

As it has been shown in the first publication under this title (Pasha 2002, hereinafter Paper I), by the 1960s understanding the spiral structure of galaxies entered a new stage of unusually vigorous activity, not always very united or monothematic, but broadly grouped under the umbrella marked "density-wave theory". Its foremost enthusiast and proponent was C.C. Lin. His papers with Frank Shu (Lin and Shu 1964, 1966) had a big and immediate impact upon astronomers, at least as a welcome sign that genuine understanding of the spiral phenomenon seemed in some sense to be just around the corner. Already at the time, however, Lin's optimism for spirals as *quasi-steady* waves was not entirely shared by other experts, and toward the 1960s it had become very clear to everyone that much hard work still remained to explain even the persistence, much less the dynamical origins, of the variety of spirals that we observe.

We start this second part of our narrative with the events that occurred and developed right in the period of Lin and Shu's initial semi-empirical explorations of 1963-66 on the alternative, dynamical front of *sheared*-wave research. It was those early analyses that first taught us, then in a local approximation, that massive shearing disks tend to be wonderful amplifiers and to respond strongly, though always in a trailing-spiral manner, to several quite plausible forms of forcing. After that, we will use Chapter II to describe most engaging topics like neutral tightly wound modes, spiral shocks and star migration that Lin and Shu plus several associates continued to explore from about 1966 onward, whereas in Chapter III we will turn to a fascinating and very serious difficulty with the group velocity that emerged only near the end of that decade. We will try to wrap it all up in Chapter IV which will focus mainly on a remarkable conference on spiral structure that took place in Basel, Switzerland in August 1969. Though its coverage may have been a little too slanted a priori toward praising mostly just the Lin-Shu ideas as major steps forward in this subject, that meeting also attracted nearly all of the other main players, and it appears interesting now to examine in retrospect which points they themselves chose to emphasize there.

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[2] Throughout the paper, the *italicized* names in parentheses refer to private communications as identified in the note to the list of references.

I. ORIGINS OF SWING AMPLIFICATION

> *W. Heisenberg*: How certain is it that the spirals are permanent structures? May it not rather be a process of continuous formation? Spiral structure might be very quickly washed out by rotation, but new spirals could be formed by fluctuation of density.
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> *J.H. Oort*: I agree. But it is difficult to conceive how spiral structures which extend over an entire galaxy could be formed entirely anew at intervals of one or two revolutions of the galaxy.
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> *Oort 1965, p.23*

> ...then a spiral arm is some sort of a wave. Once one says this, of course, one runs into an enormous number of possibilities.
>
> *Prendergast 1967, p.304*

1.1 Cambridge union

> Since Lord Rosse discovered spiral structure in M51 the explanation of this beautiful form has been one of the outstanding problems of cosmogony. The straightforward belief that this structure is a natural consequence of a swirling motion was probably held by many of the early observers and it is our hope that the present work goes some distance to establish that belief on a firm theoretical foundation.
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> *Goldreich & Lynden-Bell 1965b, p.125*

Donald Lynden-Bell and Peter Goldreich met in the fall of 1963 in Cambridge, UK. One of them had been back there from the USA for a year already, as a University lecturer in mathematics and director of mathematical studies in Clare College,[3] the other had just arrived on a one-year US National Research Council postdoctoral fellowship.[4] Goldreich had a concept of actual problems in galaxy dynamics and no plan to pursue them, but

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[3] Once he got his PhD degree in 1960, Lynden-Bell left Cambridge for California. Working at Caltech with Sandage, he solved problems on isolating integrals of motion (see Lynden-Bell 1962) and also made, with Sandage and Eggen, the classical work on high-velocity (old) stars (Eggen et al 1962) that proved the fact old-time contraction of our Galaxy. Besides, he visited Chandrasekhar at Yerkes Observatory for large-scale instability problems.

[4] Goldreich's 1963 thesis was on planetary dynamics.

Lynden-Bell got him captivated by the prospects of spiral regeneration.[5] Enthusiasm and youth – Lynden-Bell was 28, Goldreich was 24 – tempted the Cambridge researchers by the confidence that this trail would lead to the solution of the old great puzzle, and they started marching on the "spiral arms as sheared gravitational instabilities" (Goldreich & Lynden-Bell 1965b, hereinafter GLB) with a salvo of "requirements of any theory".

> "Any theory must be wide enough to contain the bewildering variety of galactic forms. The conventional picture of two spiral arms starting symmetrically from the nucleus and winding several times around like continuous threads is wrong in several aspects. In only about a third of all normal spirals can it be claimed that just two arms are dominant and although in these there is some tendency to symmetry it is not always very pronounced. [...] The remaining two-thirds of normal galaxies are multiple armed structures. In Sc's the arms often branch at unlikely angles and the whole structure is considerably more messy than the conventional picture. A swirling hotch-potch of pieces of spiral arms is a reasonably apt description. A correct theory must have room for neat symmetrical two-armed spirals, but it must not predict that most normal galaxies should be like that. The mechanism of spiral arm formation must be so universal that it can still work under the difficult messy conditions of a typical spiral galaxy" (GLB, p.126).

No less categorical was the authors' view of the acute 'winding problem' raised just a few years ago (Prendergast & Burbidge 1960; Oort 1962) to strengthen the evidence that "anything in the Galaxy is sheared at such a rate that at the end of perhaps one or two rotation periods it will be quite unrecognizable" (Prendergast 1967, p.304).

> "Unless the galaxies have conspired all to be spiral together for a very brief period we must deduce that either (1) the spiral structure rotates nearly uniformly although the material rotates differentially, or (2) the arms are short-lived but reform as open structures, or (3) that the observations are wrong and spirals rotate nearly uniformly" (GLB, p.127).

"To admit (3), is to say that the theorist is bankrupt of ideas", – GLB judged (p.127); definitely higher they favored "perhaps the most promising

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[5] "What I knew about spiral structure I learned from a course at Cornell entitled "Cosmology and Evolution" that I took in the winter of 1962 while still a graduate student. It was the only astronomy course I ever took. (After completing my thesis, I was appointed an instructor and taught the course the following year.) From this course I learned that young stars were concentrated in spiral arms and became aware of the winding problem. [...] My thesis advisor Thomas Gold mentioned Donald Lynden-Bell's name to me as someone who did interesting work on stellar dynamics. Otherwise I didn't know anything about him before arriving in Cambridge. Nor did I have any intention of working with him. I cannot recall how and why we started to collaborate, but probably it was due in large part to Donald's infectious enthusiasm for pretty much any topic in astronomy or related fields". (*Goldreich*)

"I think my enthusiasm was that the stability of a differentially rotating disk even one modeled as gas had not been worked out and understood and our mathematics should allow us to understand that problem". (*Lynden-Bell*) of the theories based on (1)" that was being made across the Atlantic (Lin and Shu 1964),[6] yet what they found even more consistent with the sheer complexity of actual galaxies was their own "second type of theory".

Spiral arms, the authors reasoned, are recognized above all by their brightness due to hot massive stars that are being formed there. For all that formative period, considerable compression of interstellar gas is needed. It logically calls for Jeans instability as occurring mostly in the spiral arms, and "this at once raises the question whether the arms themselves can be due to gravitational instability on a slightly grander scale" (GLB, p.126).

1.2 Swing amplification

> In the severe amplification, Goldreich and Lynden-Bell offered one real nugget of a discovery.[7]
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> *Toomre 1977, p.474*

Lynden-Bell (1960) had already tried to materialize his spiral-regeneration idea. He was then riveted to strict modal analyses of gas sheets which applied in the case of rigid rotation only, but he hoped their sensible modifications would nevertheless give him a correct view of the effects of shear. He found this way misleading, yet he retained his original interest.[8] Goldreich told Lynden-Bell when they met that Gold, his thesis advisor, had imagined some such concept, too, but could not work it out.[9] He also told that with Gold's influence his own reflections on the spiral-winding problem and the

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[6] It is not entirely clear when and how GLB had first learned about Lin's spiral interests and initial steps. Lynden-Bell does not think they had "any thoughts about Lin or about steady waves" when they worked in 1963 (*Lynden-Bell*). But soon afterwards they knew about the Lin & Shu 1964 paper from its preprint that Lin had sent to Lynden-Bell in mid-July 1964 to acknowledge his own receipt of the GLB preprints. "My reaction to that paper was that Lin and Shu had missed out the real problem by leaving out the pressure. While I read that paper my feeling was that had I been sent it to referee I would have rejected it. [...] I believe that if the paper of Lin and Shu had not been written we would have written essentially the same paper, and I think [one has] the information to deduce that from [...] my thesis along with our GLB paper, and one is a natural outcome from the other and the more detailed stability calculation." (*Lynden-Bell*)

[7] The now accepted term *swing amplification* had been introduced not in the original GLB and JT papers of the mid-1960s, but some 15 years later, in one of Toomre's conference talks (Toomre 1981).

[8] "I was already at work on the spiral problem in 1959-60, and an outline of the changing wavelength stabilizing modes as they get sheared is given in my thesis *with the deduction that probably this theory of spiral structure will not work.* One chapter was nevertheless entitled "Towards a regenerative theory of spiral arms." (Lynden-Bell 1964d)

[9] "I imagine we are but the present end of a long line of people who believed these ideas", Lynden-Bell reacted (1964d).

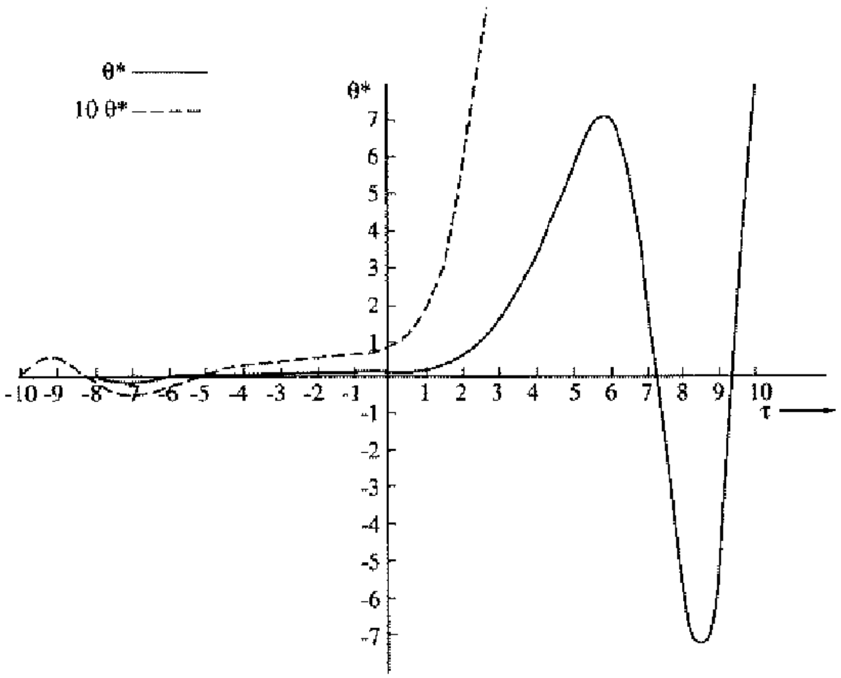

Figure 1: *Amplitude amplification of a wave in the course of its swinging from leading* ($\tau < 0$) *to trailing* ($\tau > 0$). (The figure is reproduced from Goldreich & Lynden-Bell 1965)

fact that young stars are concentrated in the arms had set him to thinking about *local* gravitational instability in differentially rotating disks. Lynden-Bell immediately appreciated this small-scale approach, and they took it up together.

Imagine a washboard-like sinusoidal disturbance with its parallel crests and troughs oriented initially at some arbitrary, perhaps even 'leading', angle with respect to the galactocentric direction. The basic differential rotation of that region of a galactic disk will slide (shear, swing) that density pattern as if it were painted material, except that the sinusoidal disturbance is itself a wave and its own amplitude will evolve amidst the shearing. To explore the actual character of this time evolution, GLB figured out a neat 2^{nd}-order differential equation (almost as if for a mass vibrating on a string, though now with a time-variable spring rate). In that way that they *discovered* that such special waves can get amplified very strongly indeed as the shear sweeps them around through the fully open orientation, especially in the case when their gaseous equivalent (Goldreich & Lynden-Bell 1965a) to Toomre's Q is as low as unity and the azimuthal wavelength λ_y matches Toomre's (1964a) axisymmetric critical λ_T (Fig.1).

Specifically, GLB considered a patch of a gravitating gas sheet, small and distant from the rotation axis to allow rectangular geometry and neglect radial variability of all its characteristics except angular speed $\Omega(r)$ defining the shear rate $A = -\frac{1}{2} r d\Omega/dr$. They attached co-moving axes x and y, oriented one radially and the other along the flow, to shearing material and explored in new axes $x' = x$, $y' = y + 2Axt$ wave harmonics of the form $\exp[i(k_x x' + k_y y')] = \exp[ik_y(y - \tau x)]$. Each of them knew its invariable azimuthal wavenumber k_y and turned by the shear 'clock hand' $\tau = 2At - k_x/k_y$ (pointing radially at $\tau = 0$) in the amplitude control of an inhomogeneous in τ differential equation.[10] In its structure, shown by GLB to be the same for infinite and finite thickness models,[11] they read the

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[10] "I remember Peter coming into my room saying he had an interest in spiral structure and that he did not know how to solve the problem but had figured out what coordinates to use. He then told me about his shearing coordinates which were the key to that work." (*Lynden-Bell*)

"I certainly didn't solve anything substantial, but I believe that I recognized that these coordinates exchanged homogeneity in time for that in x. This was probably the most important contribution I brought to my collaboration with Donald. [...] I don't know whether shearing coordinates had been used in fluid problems before GLB. However, they are such a normal choice that it would surprise me if that had not been." (*Goldreich*)

The sheared disturbances were adopted in the first papers on the non-axisymmetric local dynamics of galactic disks (GLB; Julian & Toomre 1966). More classical, separable forms $A(r)\exp[i\omega t - m\theta)]$ were recognized in the spiral-mode context just one or two years earlier (Lin & Shu 1964; Hunter 1963; Kalnajs 1963). (Lindblad had long ago been using them in his cumbersome bar-spiral theories. Contopoulos recalls (*Contopoulos*) that when in 1962 he told Lin about Lindblad's work in some detail, Lin took with him to MIT a batch of the latter's articles but then confessed that he did not understand them and preferred to start working from the scratch.) The interconnection of these two types of disturbances was for years questionable, and, for instance, Hunter who commented on it in his 1972 review criticized the "seemingly arbitrary decision" about using shearing axes and their related Fourier-analysis, pointing out that although it leads to solutions "that have certain desirable properties" it yet "does not show why these particular solutions should be especially significant" and, after all, "masks the possibility of steady waves" (Hunter 1972, p.234-35). Lynden-Bell admits that he "did not see how to translate our result into a real stability result on $\exp(i\omega t)$modes with ω real or complex", referring to Drury (1980) as one who first "showed how to do this" (*Lynden-Bell*). However that may have been, sheared techniques adequately captured a very powerful amplification process, and this alone was to sound an alert to the danger of underrating them.

[11] The reason why GLB neglected much simpler but sufficient "thin disk models with infinite density" is curious. They believed that those were "violently unstable since the growth rate of Jeans' gravitational instability is proportional to $(G\rho)^{1/2}$" (GLB, p.127). Lynden-Bell disclosed the misthought once he had submitted the paper. Yet he did not disavow it by inserting corrections in proof: that would indicate an obvious inelegance of the authors' original analyses, and to reduce that would have meant to redo the whole publication because quite a number of its key discussions leaned principally on the vertical, third dimension.

Lynden-Bell to Toomre: "I have now read properly your work (Toomre 1964a) and write to apologize for our tirades against infinitely thin disks. Earlier we held the belief that because of the form of Jeans' instability formula all bodies of infinite density must be unstable with infinitely rapid growth rates and that analyses that only found finite rates were not really treating true Jeans instability but rather the associated divergence-free or incompressible oscillations of a compressible fluid. [...] I now agree that sufficiently behavioral scheme of such waves. At initial stages of their leading orientations the inter-crest spacing λ is small and gas pressure ensures stability. As the waves are swept round, λ rises (right up to λ_y), the pressure loses its effect, and the net shear comes into play. It tends to feed genuinely 'well-organized' gas perturbations, and the waves get amplified.[12] But by the time of their considerable trailing there comes the renewed dominance of the pressure term and, with it, renewed oscillation, now at a largely enhanced amplitude.[13]

1.3 Spiral regeneration, take two

> In order to continue the problem you must then do something nonlinear, or you must simply publish the results. The authors mentioned did something non-linear.
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> *Prendergast, 1967, p.309*

The discovery of strong amplification of shearing formations in self-gravitating systems must have surprised most of the cosmogonists and dynamicists who used to think of galaxies as figures of basically uniform rotation. In essence, with this elementary and natural 'microprocess' Goldreich and

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anisotropic velocities can change the look of Jeans' criterion so that you are really discussing the same instabilities that we are." (Lynden-Bell 1964b)

"The centerpiece equation of GLB is more complicated than necessary because we did not clearly realize that it would have been adequate to study two-dimensional sheets. Thin disks faithfully capture all the horizontal dynamics of thick ones in the context of density wave theory and swing amplification. All of the regenerative spiral structure story could have been told in the context of two-dimensional disks because, aside from a minor correction for vertical thickness, all that matters for the dynamics is the horizontal velocity dispersion." (*Goldreich*)

[12] To better understand the amplifying mechanism, GLB first looked into the situation with an infinite medium subjected at some moment to a slight disturbance in the plane of rotation, its gravity and pressure being turned off. Each elementary 'fluid volume' there starts moving along its epicyclic orbit "with only one velocity at each point at any one time" (unlike its stellar counterpart with no oscillatory phase correlation of its stars), but as the epicyclic period changes with radius due to shear, the motions of 'perturbed elements' on each given azimuth are progressively more and more out of phase, the stronger the larger is their separation, because of which the density amplitude in places grows with time. In the non-axisymmetric case, azimuthal phase (and amplitude) dependence of initial perturbed motions becomes one more growth factor: the shear brings parts with different phase and slightly different radius to a common azimuth, which only adds to the proportional phase-difference dependence on the radial distance and makes the density growth still stronger. But the same shear also produces a counteraction as it shortens trailing wavelengths thus reducing amplifying capacities. Overall, consequently, "this interesting behavior is not directly related to spiral arm formation." (GLB, p.130)

[13] "I suspect we didn't expend much effort attempting to provide a physical as opposed to a mathematical explanation for the transient growth of sheared waves." (*Goldreich*)

Lynden-Bell struck upon a powerful engine for generating spiral structures, at least in a transient way. Still what they had dealt with so far was a wave-*propagation* problem that gave no closed dynamical picture, even in local setting. It left untouched the vital points of fresh-wave sources (one-time, periodic or permanent; external or internal; distributed or compact) and resulting responses. This engaged our authors throughout much of the remainder of their work where they tried to build a home by way of "reasonable speculation which [they] probably felt was justified by [their] solid result" (*Goldreich*).

Within this speculation, "the predicted return to oscillatory character need not occur". With isothermal gas, it gets "energetically advantageous" and "energetically possible for the nonlinear modes to continue to condense rather than to revert to oscillatory behavior" (GLB, pp.139, 150), because energy released during the gravitational collapse cannot be stored as thermal energy and is radiated away.[14] A thing to stop the growth and revert the system to its initial state (else it is no machine) is to break the energy replenishment of the gas layer. This done, it flattens and gets less stable. Closer to marginal stability, the swing amplifier is turned on, it applies to various existing perturbations,[15] and analysis has it best tuned on those with $\lambda_y \cong 8\pi h$, h being the layer's half-thickness. In the nonlinear stage, genuine trailing arms have been formed and hot stars are born in growing condensations. They stir up the interstellar gas, however, and it swells, recovering stability. The amplifier is turned off, the spiral arms break down, the star formation stops, the hot stars fade away, the gas layer thins, and the cycle repeats – local structures on a scale $\lambda_y \cong$ 1-2 kpc are periodically regenerated everywhere in the gas layer, the only responsive galactic ingredient.

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[14] One knew well the interstellar gas as being heated up by star-formation regions and particularly by supernovae, but also dissipative, tending to self-cooling and forming clumps at least slightly bounded by self-gravity. This invited a no less than two-component gas scheme with molecular clouds as its discrete, dense, cold and inelastic part. Such a mix badly approximates to an isothermal gas sheet, however, and it carries over no better to acoustic waves. "It is not clear at all how one may go about describing the collective behavior of such a medium – Kalnajs reasoned. – Clearly an application of the hydrodynamical equations (including magnetic effects), correct in principle though, leads to a problem of unmanageable proportions" (Kalnajs 1965, p.56). He thus "pretty much avoided gas dynamics." (*Kalnajs*) Toomre had taken some such action as if continuing his star-disk-stability study, and even submitted a special paper to ApJ (Toomre 1965), but then he dropped it suddenly and was never upset about having retreated (*Toomre*). In contrast, Goldreich and Lynden-Bell, who claimed priority to gas models, just had to be content with their simplest isothermal treatment, saying that it was "not a bad approximation" overall and, anyway, "not significant for the linear mathematics from which we obtained our main results." (*Goldreich*)

[15] "We felt that there were lots of disturbances in galaxies once one mode had become nonlinear and so there would be no difficulty in having a small component to amplify. We were not concerned with any feedback loop at that time and to this day I am less than sure of its existence in real unbarred galaxies as opposed to theorist's models." (*Lynden-Bell*)

Now what to do with the disk of stars, another licensed player in galaxy dynamics? Its natural length scale differs from that of the gas layer almost exactly in the ratio of their column (surface) densities, or typically roughly 10:1. At such a hostile difference their actual coupling cannot stop the interstellar gas, well able to cool itself, from tending to have severe gravitational instabilities of its own. Yet GLB reckoned that Jeans instability "occurs for stars in much the same way as it occurs for gas" so that the "spiral arm formation should [...] be regarded [...] as an instability of the whole star-gas mixture".[16] Thus they coopted the star disk into their basic gas-dynamical scheme and got a condominium with an essentially stellar 'effective' density and – clearly – λ_T-comparable characteristic scale $\lambda_y \cong 10$ kpc, "embarrassingly large for something deduced from a small-scale approximation". "From a local theory we cannot produce any preference for the formation of symmetrical two-arm spirals", GLB recognized, but found it "however [...] likely that the instability leading to them is a somewhat more organized form of the one discussed here" (GLB, p.151).

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[16] The GLB gas treatment of galactic disks reflected Lynden-Bell's earlier devotion to cosmogony and, in its frames, to Ledoux-oriented analytical tradition of treating flat systems (Ledoux 1951). GLB believed that even the largest-scale galaxy dynamics features the gas as the colder and more pliable dynamical component, and the idea of general 'equivalent stability' of gas and star models hit them on the fact that those obey the same Jeans-instability criterion $\pi G\rho \geq \Omega^2$ when infinite and in uniform rotation.

Lynden-Bell to Toomre: "We treated everything as gas not because we think the gas is dominant (except possibly as a triggering mechanism) but because in those cases where the transition from stability to instability can be worked out for both a star distribution function and an equivalent gas system they both become unstable at the same point. At present I only have a proof of this for star clusters whose distribution functions depend on energy only and I am not sure what equations of state the anisotropic pressure of a gas should obey if it is to go unstable in the same way as the stars in the disk of a galaxy. However I think this would probably make our basic philosophy clearer. This work on the equivalence of stability is almost all that is directly relevant that is happening here at present". (Lynden-Bell 1964a) ("I had already found very little difference between stars and gas in the Jeans instability criterion so had little compunction in solving the gas problem with the velocity dispersion of stars replaced by the sound velocity of the gas." (*Lynden-Bell*))

Toomre to Lynden-Bell: "I understood that your main *motive* was not so much to gather what a supposedly smooth gas disk would do by itself, as to mimic the likely behavior of a disk of *stars*. At least in a vague, intuitive sense I agree with you that the pressure should give neutral stability results that should at the worst be of the correct order of magnitude. [...Still] the evident gross unevenness in the way the interstellar matter appears to be distributed in most galaxies would have meant that such initially smooth analyses could not *directly* be relevant." (Toomre 1964c)

Lynden-Bell to Toomre: "I convinced myself that star and gas systems (apart from static) normally have different critical stability criteria. This floors my earlier hope though I did prove a nice theorem for static systems." (Lynden-Bell 1964c) "When I get a typist to do it I will also send you a lengthened version of the paper I delivered at IAU Symposium No 25 on stability of collisionless systems. This is great fun though not applied to spiral problems." (Lynden-Bell 1964d)

The GLB paper was closed with a “Note added in proof” whose reproduction here will allow us to turn conveniently to the subject of the remaining sections of this chapter.

> “We have heard from Dr Toomre and Mr Julian of further work on zero thickness stellar disks including a discussion of sheared modes. These behave very similarly to their gaseous counterparts discussed here. This work was independent of ours although the same sheared coordinates have been invented by them.” (GLB, p.157-158)

1.4 Transient growth and asymptotic stability

William Julian had, like Toomre, Kalnajs and Shu, been an undergraduate student at MIT. After receiving his bachelor’s degree in mathematics in 1961, he continued on as a graduate student and soon took a course on galactic astronomy from Woltjer when he visited MIT. That roused Julian’s interest in galaxy dynamics, and the time, personified by Lin and Toomre, magnified it. When the latter completed his axisymmetric-stability study of flat stellar galaxies (Toomre 1964a), he determined to encompass the asymmetric task, and this motif guided him and Julian into their work on “Non-axisymmetric responses of differentially rotating disks of stars” (Julian and Toomre 1966; hereinafter JT), which started in the spring of 1964. The news soon about parallel studies at the English Cambridge gave them still more incentive to struggle along, upon which Toomre promptly and in detail informed Lynden-Bell about the steps the MIT duet had done and planned to do.[17]

> “Since this May, a graduate student named W. Julian and I have been involved in much the same sort of an analysis as you describe in your Part II, but for the somewhat more complicated case of a thin sheet of stars with not insignificant random motions in the plane of the disk. [...] My interest in your problem dates back to the sheared non-axisymmetric disturbances for the case of negligible pressure, which were among the things I reported

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[17] Lynden-Bell and Toomre already knew each other. They first met briefly in June 1962 at Woods Hole Oceanographic Institute. Toomre’s cold axisymmetric modal calculations were being finished during his stay there, and he spoke of disk instabilities at a seminar with Lynden-Bell present. (The listener later recalled: “I fear that such are one’s subjective impressions that my memory of your talk at Woods Hole is solely an irrelevance which I will not burden you with” (Lynden-Bell 1964b). “I think your sentence is Churchillian”, then commented Toomre.) In June 1964 Mestel visited MIT, and he brought both Lin and Toomre preprints of two GLB papers from Lynden-Bell. “Figures 3-5 in their Paper (or really preprint) II resembled hugely what Bill Julian and I had managed both to *discover* and to plot all on our own just during the preceding 1-2 months – Toomre recalls. – We had at that point been doing our stellar dynamics only via truncated moment equations which were flawed in not including the strong (= vaguely Landau) damping toward short wavelengths that is very characteristic of the stellar rather than gaseous problem ... and it was for that slightly bogus reason that our results looked so similar.” (*Toomre*)

> in the recent ApJ. Even at the time I did those, I realized that any inclusion of pressure forces to remove the shortest instabilities would leave a typical situation that was at first stable, when the disturbance was still tightly wrapped in the 'unnatural' sense, then unstable for a while, and finally stable again. (In fact, if one were to choose the unwrapped wavelength long enough, and the pressure quite small, I felt one would even find two periods of temporary instability! Have you tried this admittedly unrealistic case on your computer?)[18] However, I felt then that the situation did not merit a detailed calculation, since it could not be terribly relevant to the spiral problem to discuss such disturbances to a supposedly uniform disk of *gas* in view of the observational evidence about the gross unevenness of the existing gas distributions in galaxies. [...]
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> Certainly, you arrive at a most worthwhile result in observing that under circumstances in which the axisymmetric instabilities (locally at least) would be avoided, there is still the distinct possibility of temporary non-axisymmetric instabilities, and that this could not help but provide a bias in any situation with a somewhat random excitation in favor of waves with the 'natural' wrapped-up orientation. [...] Where I would at present reserve my judgment is in your conclusion that your result is directly pertinent to the spiral problem. Julian and I had our own burst of enthusiasm on this when we obtained our very similar results, but lately it has become a little more difficult for us to envisage the exact connections. But surely it cannot be altogether irrelevant!" (Toomre 1964b)[19]

Using kinetic methods, Julian and Toomre described non-axisymmetric responses in a thin Cartesian model of a small stellar-disk region of a non-barred galaxy (JT model). In so doing, they actually managed to conquer a considerably more difficult technical problem via the collisionless Boltzmann equation than the one that GLB had needed to solve for their idealized gas.[20] Help from the Volterra-type integral equation to which the authors had converted the problem enabled them to track the evolution of an impulsively

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[18] "We did not try any of the double growth period solutions (where oscillations take place in between growths) because unless the radial modes are unstable the double growth ones never get a decent acceleration." (Lynden-Bell 1964a)

[19] "I agree with almost all you say, Lynden-Bell responded, even to some extent the doubtfulness of whether the theory as outlined by us is really the mechanism." (Lynden-Bell 1964a)

[20] "My main idea in spring 1964 had been to expand the perturbed phase density from the collisionless Boltzmann equation as a sum of products of Hermite polynomials in $u$and v multiplying the two-dimensional unperturbed Schwarzschild distribution. Closing them was not a big concern [... and] this was already some rather honest stellar dynamics. [...] But Bill and I were dismayed to learn during summer 1964 (or roughly a month or two after the GLB preprints had arrived) that such expansions looked as if they would need *thousands* (!) of terms to begin to capture reasonably accurately the later decay of vibrations due to what we realized eventually was just phase-mixing. It was this terrible inefficiency that prompted Bill to go looking extra hard at the alternative route of an integral equation. And it was definitely he who first realized that the ferocious kernel there could be integrated explicitly, an insight that suddenly made that route *much* more palatable than it had seemed at first" (*Toomre*).

"I guess Alar knew it also, but did not realize that the integrals could be worked out." (*Julian*) applied disturbance and to see the shared waves *damping*. They damped as well at a finite-time imposition of disturbances, while asymptotically, as $t \to \infty$.[21] "This means, JT concluded, that a collisionless star disk, if it strictly obeyed our model equations, could not even sustain self-consistent non-axisymmetric waves set up by previous gravitational disturbances, let alone admit modes that grow indefinitely" (JT, p.819). This plainly conflicted with the Lin-Shu self-sustained and tightly wrapped wave scenery, while still giving it formally, as the axisymmetric limit, a saving chance in an indefinitely slow damping.[22]

These results enabled Julian and Toomre to speak of the stability in the strict sense. Because the heat and shear parameters played no quantitative role (as well as k_y, they were only demanded not to be infinitesimally small), JT stated that the technically correct criterion for their model disk had to be the axisymmetric one, $Q > 1$. Yet "this curiously simple conclusion" is

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[21] That non-axisymmetric waves damp is due to phase mixing of the perturbed star distribution function in the course of its averaging. It does not imply energy dissipation as long as the system obeys the isentropic collisionless Boltzmann equation. In the gradient-free JT model, the mixing effect comes from the shear that breaks down phase alignment of the stars on their epicycles, induced by previous disturbances. Waves of lengths $\lambda_y >> 2\pi r_e$ (r_e being the epicyclic radius) are almost uninfluenced, while those of $\lambda_y \leq 2\pi r_e \approx \lambda_T$ damp severely.

"Agris Kalnajs started hammering away on my dense skull from roughly spring or summer 1963 onward about the *undamped* axisymmetric vibrations even in the presence of ample (especially $Q > 1$) random epicyclic motions, – since they followed very naturally (as he well knew) from the kinds of plasma-like math. [...] I remained suspicious for a long time especially about his claims that there should be such undamped vibrations no matter how short one chose their wavelengths – I had somehow become over-convinced that strong phase mixing or loosely speaking 'Landau damping' of any short waves in stellar sheets had to be the rule and could not be avoided! Of course on the latter point I was wrong, as even Julian and I had convinced ourselves [...], ... but intuition is a funny thing, and sometimes when wrong it takes a long time to get repaired." (*Toomre*)

[22] "Let me state again, a little more explicitly, why all that largely Russian 'grumbling' about folks in this business having been genuinely 'ignorant of plasma parallels' is beginning to get under my skin. In its linearized form, the collisionless Boltzmann equation is nothing more than a 1st-order quasi-linear PDE which almost any competent applied mathematician would (or should) recognize is solvable via very standard characteristic curves such as I did in my paper (Toomre 1964a). So I honestly still don't think that was any big deal, or something that C.C. or anyone else could not quickly rederive on their own. One most definitely did not need to go running to plasma physicists to see how they had handled something so 'obvious'. [...But] those characteristics surfaced again in JT, and there in a situation with a shear flow which even the kind plasma physicists had probably not met! I also assert that the Volterra integral equation (21) from JT – with its kernel figured out as compactly and explicitly as it appears in eqn (23) thanks to my clever student Julian – is distinctly *more* remarkable than anything Lin & Shu 1966 managed to do on their own, esp. since that formalism not only contains 'their' dispersion relation as a limiting case that we there only hinted at, but also because we unlike they went on to show right in JT that the shearing sheet 'could not even sustain self-consistent non-axisymmetric waves', plainly contrary to what Lin & Shu 1966 would have implied for this same situation." (*Toomre*)

but an asymptotic result, they stressed: "as such, it does not preclude the amplification of disturbances during a conceivable intermediate time period" (JT, p.819). And JT did compute "a remarkable transient growth of these wavelets while swinging" (p.821), very similar to that revealed by GLB in gas models.

1.5 Spiral stellar wakes

> The main inference to be drawn from these analyses is undoubtedly that even a seemingly stable, differentially rotating star disk ought to *respond* with a remarkable intensity, and in a distinctly spiral manner, to quite typical forms of non-axisymmetric forcing. [...] These intense trailing star responses obviously demand a physical explanation.
>
> *Julian & Toomre 1966, pp.829, 827*

> The response to a traveling point mass is a nice physical idea although again it can't be very large scale. However I am sure one can see results of it in galaxies and it could be important as an observational tool to tell the conditions in a galaxy from the shapes and angles of 'the tails of condensations'.
>
> *Lynden-Bell 1964d*

However nice the above results from the first part of the JT paper may have appeared, one cannot help noting that in the large they just confirmed the basic GLB picture of the strong transient amplification. The 'English signal' that had come to Julian and Toomre in June 1964 must have made them feel that they would not get out of the shadow cast by the GLB-planted spreading tree without an advance in strict description of local swing-amplification in its self-consistence and closure. And, it must be said, that signal did not take them unawares: they had already set themselves the task of answering the question: "How would a thin, differentially rotating, self-gravitating disk of stars respond to the presence of a single, particle-like concentration of interstellar material orbiting steadily within its plane?" (JT, p.810)

> "We have thus far mainly talked about putting these disturbances together to obtain among others the density patterns of the steady response of stars to a point mass representing a similarly orbiting gas concentration, for instance.[23] This task will only be messy, not difficult, [...] but we can already foresee that the response density will be in the form of an elongated hump, inclined roughly at your angle to the radius" (Toomre 1964b).

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[23] "It must have occurred to Bill and myself that the forcing by a point mass was a basic question to be answered, since it amounts essentially to a Green's function approach to this subject." (*Toomre*)

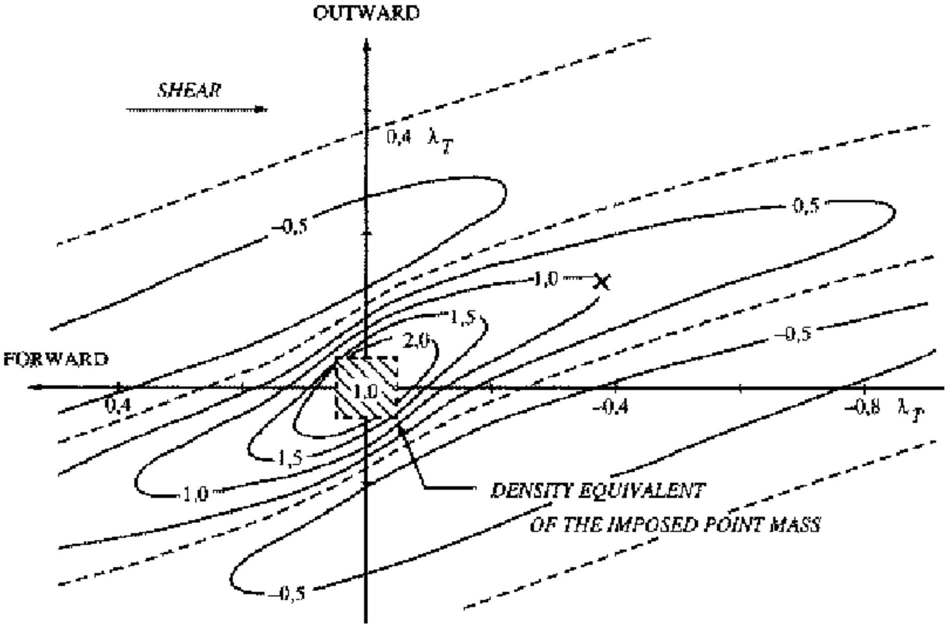

Figure 2: *A stationary density response of the JT model on the action of a local mass source.* $Q = 1.4$*,* $V(r) = const.$ (The figure is reproduced from Julian & Toomre 1966)

Volterra-equation methods allowed Julian and Toomre to calculate those responses, essentially by superposing lots of individually shearing waves to obtain a steady pattern of positive and negative disturbed densities in the vicinity of the imposed mass point. And precisely because many of those waves had been strongly swing-amplified, this sum of Fourier harmonics resulted in an awesome trailing stellar wake extending to both sides from the point perturber (Fig.2). As might be expected, the isodensity line inclination was sensitive to the shear rate and much less so to the stability parameter Q; the latter, in its turn, strongly influenced the wake's amplitude, especially at $Q \cong 1$. JT preferred $Q = 1.4$, however, as corresponding to our solar vicinity, and for this case they computed disk-thickness corrections. At the assumed thickness $2h \cong 0.1\lambda_T \cong 1$kpc, those reduced the perturbed gravity by no more than 20-30% but did not hurt the general characteristic picture of a steady trailing spiral-shaped wake that impressed one with its severe length scale and amplitude (Fig.3).

To clarify the dynamical substance, the authors separately considered what happens to a 'cold' test star, say, on a larger circular galactic orbit than the imposed point mass, as the differential rotation carries it past this force center, collective forces being ignored (Fig.4). As long as the time interval during which the star is close to the center is only a fraction of an epicyclic period, as in the shearing conditions near the Sun, the radial

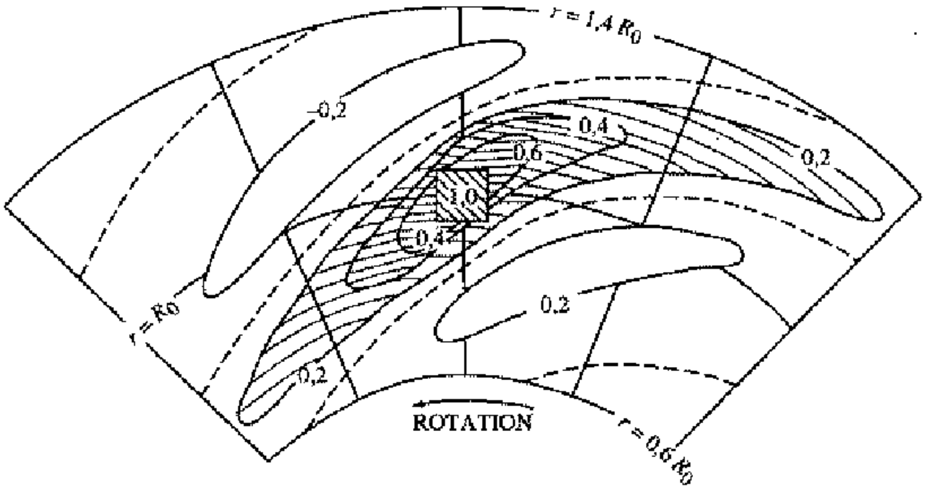

Figure 3: *A polar-coordinate view of the response in Fig.2 corrected for the disk thickness* $2h \simeq 0.1\lambda_T$. The galactocentric distance R_0 equals λ_T. (The figure is reproduced from Julian & Toomre 1966)

force component resembles an impulse occurring at the instant of closest passage. It sets the star in epicyclic motion by giving it a radially inward disturbance velocity at the abreast position. Because of this the relative speed of passage reaches a minimum approximately one-quarter epicyclic period later, or some 45^0 or so downstream of the perturbing mass point. This angle, which is considerably larger in the collective case – toward 70^0, as in Figs 3, 4 – shows the direction in which the passing stars are grouped most closely, forming a characteristic phase concentration called a wake.

What can one say about the JT work in summary? It stands on its own as a complete solution to a well-defined problem, an accurate and ample model, a neat and strict theory (though tedious to compute). Being self-contained, it has no need for subsidiary assumptions, hypotheses, speculations and evaluations. It must have been evident to many thinkers that a steady compact source might create nothing but a steady (what else, if any?) hump of trailing (what other in the face of shear?) orientation. Why had this idea not been worked out earlier? Because fresh physical intuition, mathematical excellence and advanced computing were needed, and all at once. But, all the same, the paper itself impeded general insight into its findings. Written with the feeling of intellectual and aesthetic pleasure of having solved a difficult but important problem, the article contains some unnecessary confusing details, and in other places – through scrupulous and otherwise brilliant style and wording – is too condensed to be accessible without a lot of work by the reader. So it was rather too terse and mathematical in the general climate

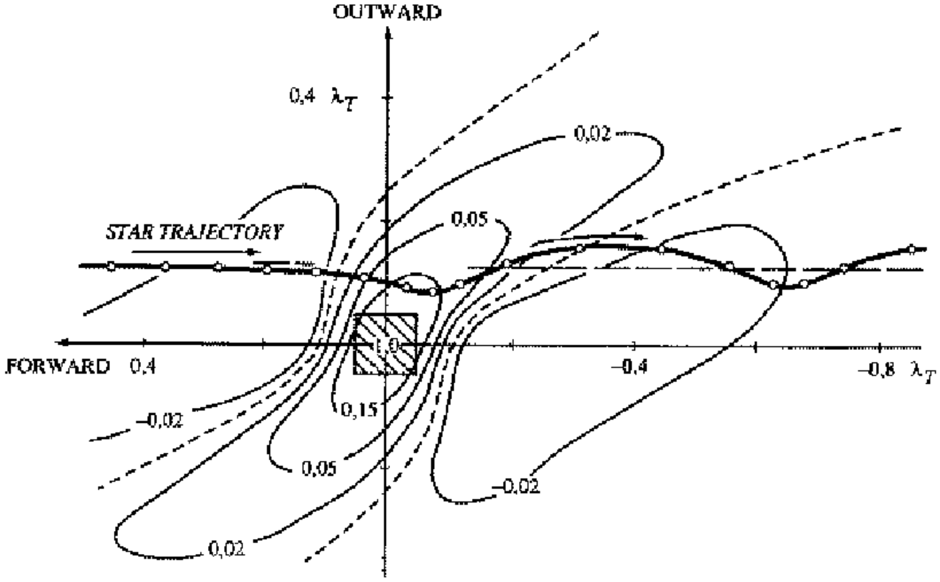

Figure 4: *Trajectory of a test star moving past a local mass source.* $Q = 1.4$ and $V(r) = const$ as in Fig.2, but the collective effects due to mutual attractions of the background stars are turned off. (The figure is reproduced from Julian & Toomre 1966)

of tastes and attitudes with which traditional astronomers had encountered the early steps of new, modern galaxy dynamics. It has been no wonder that even several serious dynamicists, let alone ordinary astronomers, have never bothered to read this important paper carefully.[24]

1.6 The roads part

Tempted by a wealth of interests and prospects, the fathers of the 'swirling hotch-potch' theory were not very resolute about raising their yet-unrisen-to-its-feet brainchild. Goldreich had no plans at all to continue working on spiral structure, and once he had moved from Cambridge to California in the summer of 1964 he did not pursue their studies. In the late 1970s only did he return to the subject, and then only when he began to study planetary ring dynamics with Tremaine (Goldreich & Tremaine 1978, 1979, 1980).

> "You must appreciate that I was not a major player in the story you are concerned with nor did I ever consider myself to be one. Moreover, I am not a particularly scholarly scientist, and am undoubtedly guilty of paying too

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[24] The JT paper had fewer than 20 citations (according to ADS) in the first 10 years after its publication.

> little attention to who deserves credit and for what even on topics to which I have contributed. My main pleasure comes from understanding things for myself. I like to get applause for my work, but that is a secondary benefit.” (*Goldreich*)

No sooner had Lynden-Bell submitted the GLB article (spring 1964) than he “just finished a paper (Lynden-Bell 1965a) on explaining the bending of the galactic plane by a precession of the galaxy” and then “temporarily left spiral research for a problem in general relativity” (Lynden-Bell 1964a) – apparently, not without a compunction on Goldreich’s departure and partial unsuccess of arranging things with Toomre so that they “can work complementarily rather than on the same topics” (Lynden-Bell 1964c).“I quite expect to stop or rather remained stopped for a bit” regarding galaxy-disk problems, Lynden-Bell wrote to the latter (Lynden-Bell 1964d), still in winter 1964-65 he collaborated with Ostriker on a general energy principle for differentially rotating bodies. In his 1960 thesis (Lynden-Bell 1960) he already had one for axisymmetric modes, and he was eager now about the non-axisymmetric case envisaging its relevance to spiral modes. The work was almost completed by the time Lynden-Bell left Cambridge, and his summer 1965 arrival at the Royal Greenwich Observatory in Herstmonceux returned him for a while to spiral regeneration channels. Impressed by the “formidable difficulties” that the leading spiral theories of the day met “in fitting precisely the observed phenomena in our Galaxy”, he announced that he was to show “a fundamental role for a small magnetic field in a basically gravitational theory” via “modification of the Goldreich–Lynden-Bell–Toomre approach”. That, he believed, “provides a more natural discrimination between old stars and gas, avoids the relaxation difficulties and provides condensations which do not spin too rapidly for star formation.” (Lynden-Bell 1966, p.57-58)

“I have gone over to being a magnetic man in part”, Lynden-Bell wrote to Toomre (Lynden-Bell 1965b) inviting him for a bigger joint effort, which struck the latter as a bizarre digression (*Toomre*). Something must have been disturbing him as to which spiral ideas to believe.[25] Possibly, this was partly due to his general motif of finding pleasure in formulating and trying original dynamical problems rather than in routinely clearing roads already laid. There Lynden-Bell was no doubt successful since despite his few miscarriages (like Lynden-Bell 1965a) he had won fame as a galaxy dynamicist already in the 1960s, especially after his important studies on violent relaxation of stellar systems (Lynden-Bell 1967) and on the nature of quasars (Lynden-Bell 1969). With all that he himself has admitted, as if implying the reverse of the medal:

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[25] “I still do not know whether the magnetism is an important catalyst for the modes we see or whether it is irrelevant. It is irrelevant for a purely stellar system but what we see has gas and star-formation.” (*Lynden-Bell*)

> "I have no claim to the theory of spiral structure. Of those who once worked on it I feel that I am one of those least well informed as to its current state and most skeptical that a full understanding has even yet been reached." (*Lynden-Bell*)

Working with Toomre on stellar wakes, Julian prepared his PhD thesis "On the Enhancement of the Random Velocities of Stars in Disk-like Galaxies", supervised officially by Lin and submitted in August 1965 (Julian 1965).[26] There and in his consequent paper (Julian 1967) he calculated the heating of orbiting stars such wakes cause. The simple truth of local differential rotation and triaxial residual-velocity ellipsoid had long argued partial relaxation of our Galaxy's star disk but – paradoxically – found no reasonable explanation in terms of two-star encounters (see Chandrasekhar 1942). In the early 1950s, Spitzer & Schwarzschild (1951, 1953) proposed and qualitatively estimated the heating by giant 'molecular complexes'. Now Julian included collective star interactions and found much higher growth rates and velocity-dispersion points: taking the 'complex' mass of an order of 10^6 – 10^7 suns, he had Toomre's $Q-$parameter grown to as large as 2.0 or so.[27] This disfavored the Lin-Shu wave picture ensured by the capabilities of marginally stable disks, yet no reasonable reduction of Julian's generous choice for a typical gas-cloud mass was seen to let Q go under 1.4.

As a PhD-degree holder, Julian worked at the University of Chicago until 1967 when he got a postdoc position at Caltech. Goldreich warmly met him there and had him running and swimming during lunch the second day already. Soon they went on to do their famous work on pulsar electrodynamics (Goldreich & Julian 1969). For a while, Julian kept a side interest in the continuing discussion between his distinguished former MIT colleagues, but when Toomre wrote to him in 1970 musing about possible "large-scale sequels to the JT paper", Julian – now in New Mexico – "seemed to be not at all interested" (*Toomre*).

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[26] "Bill throughout our mutual involvement remained C.C.'s student officially. [...] When I returned to MIT in fall 1963, C.C. himself had urged me to look after Bill, not on the grounds that he wasn't talented but because – to C.C.'s own taste, at least – he seemed too independent." (*Toomre*)

[27] Following Julian, Thorne (1968) solved an inverse problem of dynamical friction on a massive particle in a slightly eccentric orbit in a hot thin disk of stars. With JT techniques, he included collective stellar interactions whose neglect had been excused for pairwise stellar encounters in elliptical galaxies and galaxy clusters where Jeans' length is of the order of the whole system, but not in flat galaxies where it was co-ordered with their thickness, pointing at much more pronounced collective effects. Thorne found that this collective play could double the friction in magnitude.

II. THE LIN-SHU THEORY GOES ON

> If you believe that a spiral arm exists over a very large distance in the Galaxy, you would probably also like to believe that it exists over many rotation periods.
>
> *Prendergast 1967, p.304*

> In the beginning the immediate necessity was a consistent description of the spiral phenomenon, in sufficiently good agreement with the observational data.
>
> *Bertin 1980, p.10*

The Lin-Shu Milky-Way spiral diagram favorably met at the Noordwijk 1966 IAU Symposium (Lin & Shu 1967), its authors affirmed that their wave theory already "produced conclusions which appear satisfactory from a general point of view". It was declared "free from the kinematical difficulty of differential rotation"[28] and permitting "the existence of a [two-armed trailing] spiral pattern over the whole disk while allowing the individual spiral arms to be broken and fragmentary" (Lin 1967b, pp.459, 462; Lin 1968, p.47). This optimism gave Lin a feeling of confidence, correctness and leadership in the understanding of galactic spiral phenomenon, feeding his further initiative.

2.1 Neutral modes and marginally stable disk

> ... a number of major improvements and further extensions of the theory.
>
> *Shu 1968, p.5*

> Still, a posteriori, the behavior of the system is remarkably simple, and the use of asymptotics is a generous source of physical insight.
>
> *Bertin & Lin 1996, p.219*

To Shu, his historic early coauthor, Lin posed the important task of enriching the analytical attire of their 'theory of density waves', and Shu supplied some in his PhD thesis work "The Dynamics and Large-Scale Structure of Spiral Galaxies", presented at Harvard in early 1968 (Shu 1968, hereinafter

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[28] Woltjer, the key player who turned Lin to galaxy dynamics, stated in his spiral review that the density-wave *theory* as pictured by Lindblad already "resolves the kinematical difficulties, but of course a dynamic justification is needed" (Woltjer 1965, p.570). Lin (1967b, p.458) soon claimed that his and Shu's theory resolves the same *as well.*

S68).[29] He started with the derivation of a general integral equation for self-consistent responses in a thin star disk. Kalnajs (1965) already had one in an epicyclic approximation, attacking it for growing-mode solutions, but uninspired by those arduous efforts, Shu was not in the mood to vie with him in direct modal search, the more so as, following Lin, he targeted only tightly wrapped neutral modes. This converted his practical interest in the integral equation to analyzing its short-wavelength limit in the 2^{nd} WKBJ order. For the day, it was a rather worthy plan as for instance it seemed to allow access to the radial behavior of the supposedly long-lived modes.

> "We start with the hypothesis that a neutral spiral density wave exists. We then investigate the question whether such waves can be self-sustained in the presence of differential rotation and finite velocity dispersion. In this way, we are able to study, in a qualitative manner, the characteristics of such self-sustained waves. This deals with the question of *persistence*. [...] We investigate the question how such waves can be expected to attain finite amplitudes, and what mechanism is that allows them to take on a spiral rather than a barred form. This deals with the question of *origin*" (S68, p.6)

In his attempts of answering the so posed 'question of persistence', Shu resourcefully argued for adoption of the marginally stable galaxy-disk model, and turning then to the 'question of origin', he called for the idea of overstability with which to resolve the 'antispiral theorem' in favor of a trailing quasi-stationary spiral mode.

Lin and Shu initially ascribed a quasi-stationary spiral structure to an in-places-strongly-unstable star disk (Lin & Shu 1964), but soon they changed their mind (Lin and Shu 1966) for Toomre's early idea of the disk entirely evolving to a state of marginal stability Q =1 (Toomre 1964a). Toomre himself had already left it, having considered the role gas clouds must have on stars, which Julian's calculations soon supported (Julian 1967), however Lin and Shu remained skeptical of any need for Q to rise above unity.

> *Lin:* "Toomre (1964a) gave a criterion for the minimum dispersion velocity needed to prevent gravitational collapse. He and Julian (JT) are inclined

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[29] "Much of the credit for this investigation belongs to Professor C.C. Lin who asked the key question concerning the spiral structure of disk galaxies and then formulated the basic approach toward the resolution of the problem. [...] Professor M. Krook provided much generous help in his capacity as my faculty advisor and official thesis supervisor. Without his guidance and patience, my progress as a graduate student at Harvard would not have been as pleasant. Discussions with Dr. A. Kalnajs have cast light on several major and subtle points. Many of the more fruitful approaches were found only because of his well-raised criticisms of the form of the theory prevailing at one time. I have made use of some ideas of Professors A. Toomre and P. Vandervoort and am indebted to them on that account. Professor Toomre's helpful criticisms of various aspects of this research invariably proved to be illuminating. [...] The arrangement and style [of the final draft of the manuscript] were greatly improved by several suggestions made by Professor C.C. Lin. To all of these people, I am extremely grateful". (S68, p.i)

> to believe, however, that the mean square dispersion velocity might exceed this minimum by as large a factor as 1.8. On the other hand, Lin and Shu (1966) are inclined to believe that the value would not significantly exceed the minimum needed [...] Since observations show deviations from a Schwarzschild distribution, it is difficult to distinguish between these two opinions without a careful analysis of the observational data." (Lin 1968, p.49)
>
> *Shu:* "Whether the Galaxy is everywhere more than marginally stable is a point of some debate. Julian (1967) is of the opinion that the enhancement of cooperative effects of the irregular forces provided by massive objects (on the order of $10^6 - 10^7$ solar masses each) will inevitably drive Q to values substantially higher than unity. Observations in the plane of the Galaxy show only the 'spiral arms' to possess large mass concentrations." (Shu 1970c, p.111) "In the density-wave theory of spiral structure (Lin and Shu 1964, 1966), large aggregations of interstellar gas are the manifestation of a density wave and do not represent either a bound or a quasi-permanent body of matter. The interaction of stars with such a wave does not lead to appreciable relaxation." (Shu 1969, p.506)

This troublesome climate prompted Lin and Shu to reverse the logic of thinking and they put, accordingly, that their pioneer Noordwijk plot best attested its underlying $Q = 1$ star disk. Shu examined Lynden-Bell's mechanism of violent relaxation and claimed it not occurring in disk conditions. "The only relaxation mechanism operative for stars in the early life of such galaxies, he thus argued, is an axisymmetric form of the Jeans instability discussed by Toomre" (Shu 1969, p.505); it develops in the disk plane and affects neither vertical distribution of stars nor their angular momentum. Along the event sequence Shu proposed for this mechanism, our young, still gaseous Galaxy first attains a disk form. Via shear deformation, its mass distribution becomes axisymmetric, and turbulent gas motions get fixed at a permanent level c comparable to today's vertical stellar velocity dispersion. There comes a period of violent star formation. The baby stars, inheriting parental kinematics, gain an isotropic rms velocity c. The fresh cold disk they arrange is a fit subject for the operation of axisymmetric instability through which it heats up until a stage $Q \equiv 1$ is reached. The process cannot go beyond it, and losing the heat is also impossible owing to the lack of any plausible cooling of the stars (Shu 1968, 1969, 1970c).

In the adoption of $Q \equiv 1$ Lin and Shu found two attractive factors. One was that in this neutral-mode case the four dispersion-curve branches seemed to converge at corotation $\nu = 0$ (Fig.5). Shu conceived that two longer-wave branches, due mainly to differential rotation, are "more in the nature of pulsations", and two other, determined primarily by velocity dispersion, are "more in the nature of *local* oscillations" (S68, p.108). Still, well seeing that these two processes are present in varying degrees here and there in the disk, he found this "useful for conceptual purposes" insight "somewhat arbitrary" and credited realistic 'coherent' spirals "without a 'kink' at $\nu =$

0" to a proposed 'Mode-A' meant to couple the short-wave branch inside corotation with its long-wave counterpart outside the same.[30] This smooth and conscious selection, Shu noticed, had already served him and Lin in 1966 with their Noordwijk Milky-Way spiral understood as the inner half of Mode-A.

Lin and Shu hoped that "after a galaxy has been completely stabilized against Jeans' condensational instability, it is still susceptible to a mild overstability of two-armed waves" to which one owes actual spiral formation (S68, p.8).[31] Shu developed this theme in his thesis. In 1967 he learned from Toomre about his tentative group-velocity results and misused those to visualize how the individual *wave crests* move radially. He did not think then of genuine spiral-wave packets (see Sect. 3.2) and what he had was but a group of tightly wrapped two-armed waves somehow occurring to a galaxy and soon developing into an almost self-sustained mode, to get perfectly so via slight shearing and other modifications when it would gain and fix its amplitude. But if such a wave-crest group is not quite a mode yet, why not to apply to it group-velocity formulas? Shu did so and there he saw "another (and perhaps more important)" attraction due to the $Q = 1$ model (S68, p.111): his near-Mode-A got an inward radial group motion that "does not reverse sign somewhere in the principal range" between the inner and outer Lindblad resonances (ILR and OLR hereinafter).[32] In the inhomogeneous overstable disk such a motion "would lead to the growth of a 'group' of spiral waves to some finite amplitude, the growth being ultimately limited by non-linear effects [...of] the shearing effects of differential rotation (which is absent in the linear theory) [that] may be expected to enhance any preference for trailing patterns." (p.8)[33]

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[30] Shu's proposed 'Mode-B' combined the long waves inside and short waves outside corotation. "Formally, Mode-B spirals with $m = 2$ would present the appearance of a barred spiral" (S68, p.123).

[31] Overstability meant to Lin and Shu (Lin & Shu 1966; Lin 1967a) slow growth of waves traveling in an inhomogeneous $Q = 1$ disk.

[32] It is this mode, Lin and Shu believed, that manifests itself in the observed spiral structure, and only by superposing the identical 'near-mode' with opposite sense of winding and direction of motion that "we obtain pure standing waves which do not propagate. Such a wave, of course, does not have any spiral features." (S68, p.113)

[33] Lin and Shu knew well that the shear, which was absent in their wave-mode theory, was absolutely present in the alternative, sheared-wave theory (GLB, JT) and that it there supported nothing but trailing waves. At the time they (and not only they) thought, however, that there was no intrinsic connection between these two types of density-wave theories. From their own end, they were not very successful in the 1960s in explaining the trailing-spiral prevalence, though that had been a vital test for any spiral theory. As regards their repeated mentions of and hints at nonlinear effects (Lin & Shu 1966; Lin 1967b, 1968), Lin and Shu never went into it very seriously. Besides, their view of mild instability favored trailing waves only inside corotation, diagnosing that "there might be a preference of leading waves in the [Galaxy's] range 10-12 kpc". Lin (1967a, p.80-81) professed that this "cannot be taken on face value", and largely to avoid the trouble he

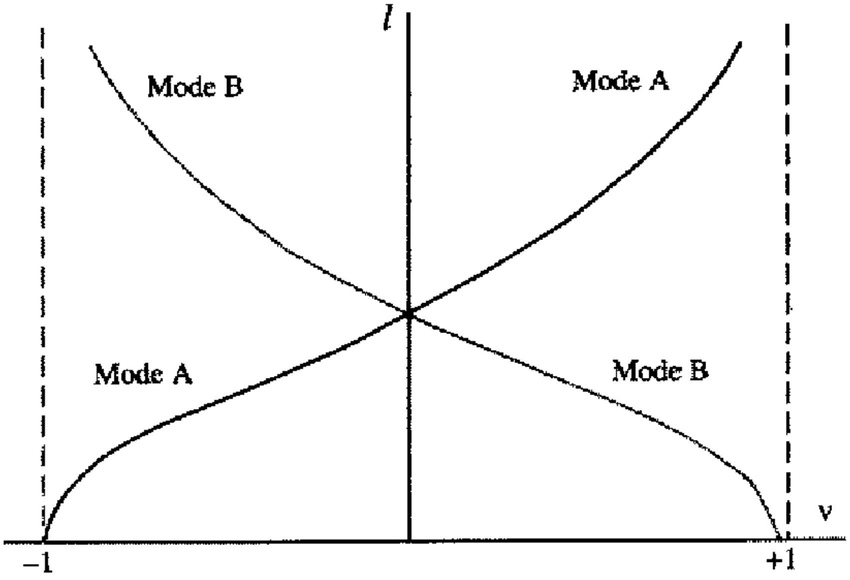

Figure 5: *Shu's Mode-A as proposed by him to account for the grand design in non-barred spiral galaxies.* (The figure combines two separate figures from Shu 1968)

These overstability ideas Shu directly associated with the *antispiral theorem* that "a number of us have sometimes been worried" about (Prendergast 1967, p.308).

2.2 Antispiral theorem

> After my paper on the stability of collisionless gravitating spheres was published (Antonov 1960), I approached the density-wave theory but did not believe it, mainly because of the antispiral theorem, anyway known to physicists.
>
> *Antonov 2003*

> In hindsight, I think Lin's judgment was accurate considering how quick people were to attack his point of view with proofs of 'antispiral theorems' and the like shortly after the publication of Lin & Shu 1964.
>
> *Shu 2001*

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left his original grand spiral plan over the entire 'principal region' between the $m = 2$ ILR and OLR for a conceptually different but yet space-preserving variant with corotation just transplanted to safer areas of disk outskirts or thereabouts.

Soon Contopoulos (1970a,b) calculated near-ILR stellar orbits subjected to a growing imposed Lin-Shu spiral gravity field of leading and trailing planforms. He got trailing responses in both cases and explained his result in terms of a specific character of misalignment of solutions inside and outside ILR, considering this the first strict demonstration of the Lin-Shu-wave trailing.

At the Noordwijk Symposium Prendergast explained to astronomers the general meaning of the theorem. If in linear theory there were to exist a nondissipative global mode of trailing planform that was content to rotate indefinitely without growing or decaying, then a similar mirror-image leading mode must exist as well. “This symmetry property of the equations means only one thing: the system is too simple. Whenever you see a symmetry property, all you have to do is mess up the system a little bit and give up the symmetry. There are a large number of things that will remove the symmetry, [...] there is non-conservation of everything” (Prendergast 1967, p.308-309).[34]

The antispiral theorem took on particular sounding after Lynden-Bell and Ostriker (1967) set it out as an application of general principles they worked out for differentially rotating bodies. Lynden-Bell, to whom we owe the idea of this explicit consideration, no doubt knew that it “had many let-outs” hence he “did not think it as restrictive of spiral theories as some others took it to be”. For one thing, the theorem could be strictly applied to exponential modes only, and Lynden-Bell hoped that “double modes that might grow as $t\exp(i\omega t)$ might well be the ones needed to transfer angular momentum outward” through corotation (*Lynden-Bell*).[35] Moreover, it did not oblige one at all to mix leading and trailing waves in equal proportion obtaining a cartwheel-type mode, that was no necessity imposed by the equal-frequency condition. Shu recalls that Lin from the beginning “felt sure that one should not do the naïve thing of superimposing equal trailing and leading parts” and that “he probably wanted to discover the reason why before publishing anything”, but the Toomre 1964a paper “triggered him into premature action” (*Shu*). One is to wonder what annoyance for Lin and Shu became Lynden-Bell and Ostriker’s antispiral address that appeared just when they thought they got the true mixing mechanism as due to disk overstability. It was imagined to cause slow growth of one of the components, the trailing one in Lin-Shu’s ‘nonlinear’ assumption, and then to break the full symmetry in the basically neutral-mode problem by ensuring different radial behavior for the components and, correspondingly and automatically, their unequal mixing.

In his 1968 thesis and, more pointedly, in his papers to follow (Shu 1970a,b) Shu demonstrated one more ‘let-out’ in the antispiral theorem. “The general formulation for the normal modes, he noticed, [...] shows that a

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[34] The feel of symmetry breaking “non-conservation of everything” then prompted Prendergast that there ought to be some way to determine that “the natural way to get the arms is trailing” and that “presumably that would be a direction that would be given [...] by an increase of entropy” (Prendergast 1967, p.309).

[35] “I always held the view that angular-momentum transfer is the driving force behind spiral structure. [...] In part the anti-spiral theorem was there because it seemed to point out that what Lin said was much less than the whole story.” (*Lynden-Bell*)

certain degree of spiral structure must be present in every mode of oscillation which contain stars in resonance" (S68, p.7). Stars, unlike gas, can resonate with the oscillating gravity field without any continual shattering due to collisions. Mathematically, this is answered by the integrand poles, and even at real frequencies those compel one to make integrations along contours going off the real axis, which provides the solutions with an imaginary part and ensures their general spiral form. The resonant technique of clearing the antispiral hurdle was to Lin and Shu one of the highest points to back up the QSSS as a neutral density wave (Shu 1970a,b; Lin & Shu 1971).[36] It seems curious, however, that they did not refer to any leading component either in 1966 on their short-wavelength spiral proposal for our Galaxy (Lin & Shu 1967), or in 1971 when Shu et al (1971) announced for M51 and M81, apart from their dominant short trailing waves, unmistakable traces of an extra 'mode', yet not mirror-reflected – short and leading – but *long* and again trailing.

2.3 Spiral shock waves and induced star formation

> Fujimoto, followed by Lin and Roberts, recognized that gaseous motions generated by a tightly wrapped density wave would be dominated by the appearance of tightly wrapped shock waves. Later work (Shu et al 1972) has fulfilled Lin's belief that the density wave itself might trigger star formation and it is the shock that seems to be the trigger.
>
> *Lynden-Bell 1974, p.117*

> It is not astonishing that one gets difficulties in making stars. I think nature has difficulties too, because otherwise no interstellar matter would be left.
>
> *Hoerner 1962, p.107*

"In the early 1960s, Prendergast often expressed the view that the intense, slightly curving dust lanes seen within the *bars* of such SB galaxies as NGC 1300 and 5383 are probably the result of shocks in their contained gas, which he believed to be circulating in very elongated orbits. In such 'geostrophic' flows of presumed interstellar clouds with random motions, Prendergast (1962, p.220) wrote that "it is not clear what is to be taken for an equation of state", but he knew that "we should expect a shock wave to intervene before the solution becomes multivalued". As regards the normal spirals, Lin and Shu (1964) stressed from the start that since the gas has relatively little pressure, its density contrast "may therefore be expected to be far larger than that in the stellar components" when exposed to a spiral force field such

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[36] Kalnajs already in 1963 had an idea of such a resonance 'resolution' of the antispiral theorem in the neutral-wave setting (Kalnajs 1963; see Paper I, Sect. 2.4).

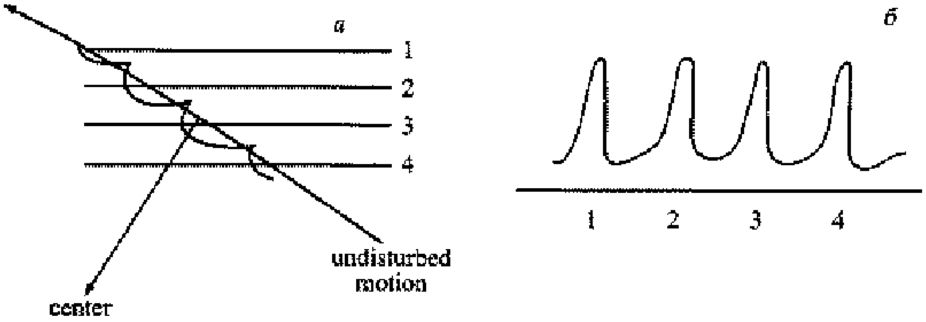

Figure 6: *The behavior of interstellar gas in the spiral gravitational field of a galaxy, according to Fujimoto: (a) – the gas motion across the "gravitational washboard", (b) – the ensuing gas density distribution.* (The figure is reproduced from Prendergast 1967)

as they had just postulated. That hint remained largely dormant, however, until Fujimoto [...] combined these last two lines of thought" (Toomre 1977, p.453).[37]

Fujimoto made his spiral-shock-wave report at the May 1966 IAU Symposium held in Burakan, Armenia, but it became widely known thanks to Prendergast.[38] He not only had urged Fujimoto to consider the problem and "helped much via fruitful discussion" (Fujimoto 1968, p.463), but also presented his results at Noordwijk, just three months after Burakan.

> "Let us suppose one has a rotating system and a *gravitational washboard*, that is, a disturbance in the gravitational potential of a sinusoidal form. [...] Then what happens to the gas? [...] The answer is that the streamlines of the gas – instead of being straight lines, in this model corresponding to a perfect circular orbit – become somewhat cusped. In every cusp there is a shock; and in that shock the density increases, even for a very modest gravitational washboard, by an enormous factor; let us say five or more" (Prendergast 1967, p.310).

Fujimoto confirmed this shock-wave picture (Fig.6) by computing two-dimensional nonlinear dynamics of perfect isothermal gas in a quasi-steady

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[37] As such, the idea of a shock in the interstellar gas was not novel in the mid-1960s as for years it had helped various small-scale problems. So the larger-scale shock-wave speculation did not come to be very striking. Goldreich and Lynden-Bell (1965a,b), for instance, had it quietly on their spiral-regeneration concept. In fall 1964 Lynden-Bell remarked in his letter to Toomre, musing on the topics for their intended complementary work: "The other thing that is interesting is the formation of shock waves as the disturbances get very violently sheared, but while the overall structure of spiral arm formation is not very clear I would rather leave such a secondary problem till later" (Lynden-Bell 1964c). "I profess no vested interest in the formation of shock waves in an initially smooth gas layer, much against my upbringing as a fluid dynamicist, Toomre responded. All yours." (Toomre 1964d) "I am not going to work in shock waves for a few years yet, came Lynden-Bell's upright reaction, so you are welcome to them too." (Lynden-Bell 1964d)

[38] The Symposium proceedings were published in Russian 2.5 years later.

field of galactic spiral potential. As the spiral angular speed “cannot be determined even by Lin’s method”, he decided to “take a priori some reasonable values” and chose, as Lindblad (1963) and Kalnajs (1965) did it, high speeds $\Omega_p \cong 45$ km/sec/kpc and close corotation $r_c \cong 5$ kpc. The answer he gave was that “both the high-density hydrogen gas contained in spiral arms and the dark lanes seen in external galaxies on the concave side of their bright arms can be due to the presence of the shock waves” (Fujimoto 1968, p.463). Lin reacted quickly and inspiringly. He assumed that a shock wave could cause and organize star formation in the spiral arms (Lin 1967b), and posed as William Roberts’ thesis theme the problem of modeling “the presence of large-scale ‘galactic shocks’ that would be capable of triggering star formation in such narrow spiral strips over the disk” (Roberts 1969, p.124].

Roberts considerably developed and expanded Fujimoto’s analyses. He corrected one mistake made by Fujimoto with his working equations (he had missed one of the full-value terms in the perturbed gas velocity equations), presented the star-disk potential description in the Lin-Shu asymptotic language, and focused on slowly rotating two-armed spirals with distant corotation. Roberts’ interest was in a “particular type of solution of the nonlinear gas flow equations” permitting gas to pass through the shock waves coincident with spiral equipotential curves and describing the gas flow along a nearly concentric closed streamtube band, to exclude net radial transfer of anything. And indeed he got desirable solutions whose family presented “the composite gas flow picture over the whole galactic disk” (Roberts 1969, p.129). To this he conjectured that the shock wave, unaided by large-scale magnetic fields (which were Lin’s initial candidate (Lin 1967b)), could trigger by itself the along-spiral formation of star associations.

> “One might imagine that the gas in turbulent motion has ‘clouds’ before the shock, which are on the verge of gravitational collapse; the sudden compression would then trigger off the collapse of the clouds, which would lead to star formation. After the gas left the shock region, it would again be decompressed, and the process of star formation would cease” (Lin et al 1969, p.737).

How could this sudden growth of the interstellar gas density and pressure trigger the desired gravitational collapse of the already existing dense clouds? Roberts did not know or show – he only said: conceivably (Roberts 1969, p.131) – but that hardly matters.[39] “The crucial point is that before the

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[39] “However, the previous discussions (Roberts 1969; Lin et al 1969) are incomplete – Shu and Roberts admitted. – There are severe difficulties in visualizing how this ‘effective pressure’ is transmitted on a small scale to trigger the gravitational collapse of clouds since cloud-cloud collisions provide compression essentially only in one direction”. Looking for “a clear physical basis for the mechanisms of the production of the shock and of the compression of the clouds”, they discussed a two-phase model of the interstellar gas (Shu et al 1972, pp. 558, 585).

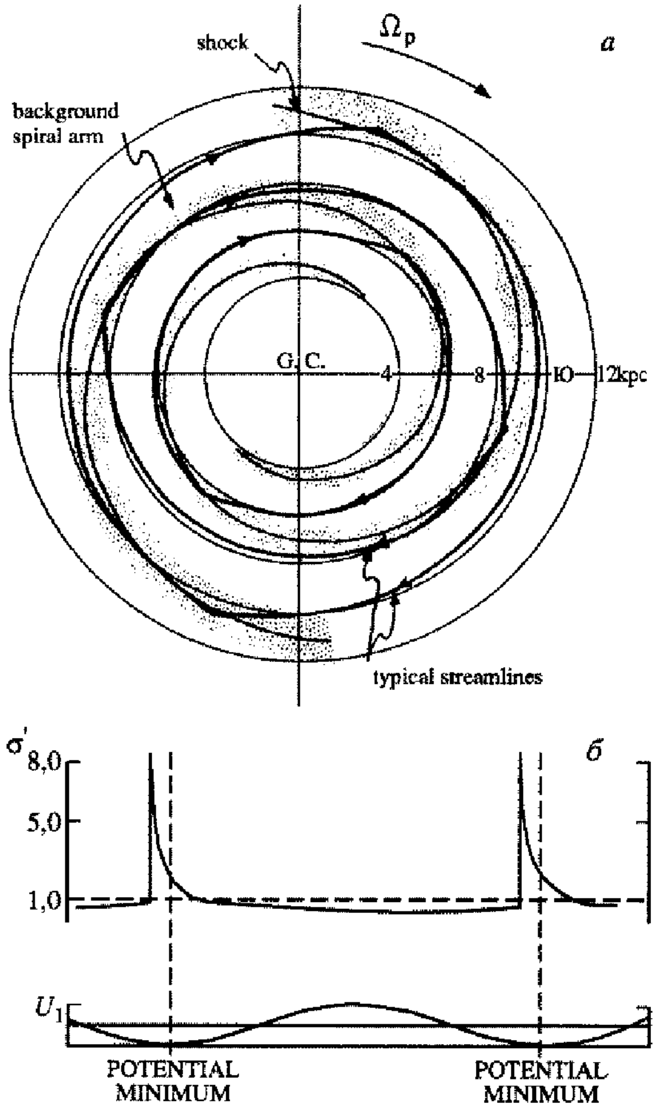

Figure 7: *The behavior of interstellar gas in the spiral gravitational field of a galaxy, according to Roberts: (a) – a shock in the gas as its reaction to the spiral gravitational field of the stellar disk component, (b) – azimuthal density distributions in the stellar disk and interstellar gas.* (The figures are reproduced from Roberts 1969)

shock idea there had been no defensible explanation at all for the striking geometrical fact, first noticed by Baade in the late 1940s, that the main HII regions in large spirals tend to define considerably crisper and narrower arms than the rest of the visible material [... and that] these highly luminous chains seem biased toward the inner edges of arms, though perhaps not quite as much" (Toomre 1977, p.453-54).[40]

Roberts' 1969 work was greatly appreciated as a basis for further studies of related problems in galaxy physics. Well familiar to astronomers in various contexts, the shock-wave idea in the new, spiral context appeared to many to be more attractive and soluble than the intricate collective effects from collisionless star-disk dynamics.

But there was some deserved criticism as well, largely in relation to Roberts' stress on closed gas streamlines. Pikelner (1970), who calculated energy loss of the gas as it crosses a spiral-wave arm and found it to be a few percent of its kinetic energy, concluded that in order that the gas might gradually come closer to center its streamlines had to be open.[41] He noticed also that "gas flows out of the arm slower than at the reversible compression, so that its mass center shifts behind the arm axis and pulls the stars ahead" (Pikelner 1970, p.758). Therefore, he inferred, angular momentum transfer from the shock-suffered gas to the spiral wave must amplify the latter, feeding it up with energy. Kalnajs corrected Pikelner in absentia (Simonson 1970; Kalnajs 1972).[42] Having established that the

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[40] "Two important assumptions underlie the above [Roberts'] gas dynamical results. The first is that the driving potential wave is tightly wound and the second that the interstellar medium can be adequately described as an isothermal gas. It is a great pity that these were introduced in the initial stages, since the results in many minds have been tightly associated with them. However, most of the results still stand if these assumptions are relaxed, e.g. it has been shown a number of times that a very open or even barred forcing can drive a similar response." (Athanassoula 1984, p.348)

[41] The latter factor, Pikelner noticed, "must have the cosmogonical consequence, [...] since in the lifetime of an Sc-galaxy gas must cross its [every] arm dozens of times. [...] Possibly, this explains why in spite of the intense star formation in galactic innermost parts there still remains a fair amount of gas." (Pikelner 1970, p.758).

[42] Kalnajs advanced his criticism in the course of a discussion at a special 'Spiral Seminar' held in 1970 at the University of Maryland in connection with the problems of spiral structure as followed from the findings of the recent IAU Symposium at Basel, 1969 (see Simonson 1970). After, he discussed the subject corresponding with Roberts and then set it out in his special note (Kalnajs 1972). Shu and Roberts rejoined (Shu et al 1972; Roberts & Shu 1972). They agreed that, strictly speaking, gas streamlines could not be taken closed, but emphasized that in the WKBJ limit the actual non-closure per cycle was a quantity proportional to small spiral pitch angle, which Roberts reasonably neglected in his original paper. It must be said, however, that originally he was rather straightforward about following Lin's directive on a *stationary* picture (Lin 1967b, p.463), and in that way he even created some photogenic theory of gas 'free modes' (Roberts 1969), which was really more than just asymptotic.

"Yup, these 'free modes' happily lose energy, and keep on doing it forever without

Lin-Shu slow waves carry over *negative* energy and momentum (Kalnajs 1971; Lynden-Bell & Kalnajs 1972), he recognized that their 'pulling ahead' should cause the *inverse* effect, i.e. wave damping. All in all, the conclusion was that in open spirals (and bars, providing the general mass distribution in the SB-galaxies is also roughly axisymmetric) the shock wave "still more increases density and non-reversible dissipation of energy" (Pikelner 1970, p.758). "It meant not only that Roberts was slightly mistaken [...]. Much more important, this [...] implies that even the neatest spiral structures can at best be only quasi-steady" (Toomre 1977, p.460).

2.4 Extremely satisfactory comparisons?

> *J.H. Oort* asks: What are Lin's further plans for numerical model computations [of spiral structure]?
>
> *Lin* answers: At present, we have no immediate plans to extend our work much further...In the meantime, we are collaborating with Stromgren on the problem of the migration of stars, to gain a more definite picture of the spiral structure within a few kpc of the Sun.
>
> *Discussion: Noordwijk 1966, p.334*

Being in 1966 on the wave of his first success in astronomy, Lin spoke of "three levels of discussion in dealing with the structure of the Theory of Spiral Structure" (Lin 1966b, p.6). Two of them – physical and mathematical – he saw already mastered to a degree,[43] so that more opportune and vital for the day he recognized the third level that discussed "the agreement of the theory with detailed checks with observations" (Lin 1966b, p.6).

> "In view of the difficulty of the theoretical problem, it is fortunate that, from the beginning, we have placed great emphasis on working out the *consequences* of the QSSS hypothesis" (Lin 1975, p.120). "In the absence of a complete theory for the mechanism of density waves, the need for observational support is urgent" (Lin et al 1969, p.722). "Indeed, our theory can be used as a tool to connect several seemingly unrelated observations. [...] Even without the discussion of the detailed mechanisms, the mere assertion of the existence of a density wave with a spiral structure, propagating around the galactic center, leads to implications which can be checked against observations." (Lin 1968, p.36, 49)

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suffering any damage. This discovery deserves to be commercialized. [...] What is the secret that makes the streamlines closed? According to Appendix V [in the PhD thesis by Roberts (1968)], you just assume that in the equilibrium state there are axisymmetric radial *and* tangential forces." (*Kalnajs*)

For more detail on this discussion see Toomre 1977.

[43] All the same, Lin conceded that "as one can see upon a little reflection, the problem of the origin of the spiral structure is mathematically more difficult", so that "these studies remain a challenge for future investigations" (Lin et al 1969, p.722).

Deliberately avoiding mathematical difficulties as "the challenge for future investigations", Lin did not seem embarrassed in the face of empirical difficulties due to apparent incompleteness and inaccuracy of observational data, and he went and led his associates along the way that was in fact no lesser challenge. There he saw an urgent interest in problems of systematic noncircular gas motions and star migration (Lin 1966b; 1967b; 1968). The famous 1969 paper by Lin, Yuan and Shu "On the structure of disk galaxies. III. Comparison with observations" (Lin et al 1969, hereinafter LYS) gave a summary of all of Lin's "levels of discussion".

Systematic noncircular gas motions in the Galaxy were under discussion already,[44] still – LYS noticed – no one spoke of their producing dynamical mechanism, although "it is easy to see this from considerations of angular momentum". Indeed, the spiral gravity causes additional along-arm gas motion which must be with the general rotation on the outside edge of the arm and against it on the inside edge, and which is "to maintain *conservation of matter*" (LYS, p.731). Starting from the well-known data on wavelike velocity variations in the Galaxy rotation curve,[45] LYS determined radial and azimuthal components of noncircular motion to be of the desired, for linear analysis, order of 10 km/sec, and assured (Lin 1966b; Yuan 1970) that the observed three-to-five-fold gas compression in the arms just corresponds to such a motion, so that "there is good agreement in major features" (LYS, p.732). But at their accepted pitch angle $i \cong 5^0$ WKBJ equations associated those motions with a rather strong, knowingly nonlinear density response that was the business of a theory yet not in the authors' hands. Thus more correct would be their simpler inference that the observed noncircular motions indeed "in major features" are due to a *certain* spiral density wave.

Mentioned by Lin at Noordwijk, the problem of young star migration arose in relation to Stromgren-initiated studies of star ages, claimed to be accurate within 15% (Crawford & Stromgren 1966; Stromgren 1966a; 1967)]. Contopoulos and Stromgren (1965) set this migration problem as based on the idea that time reversion of star motions and their countdown in the hold of the axisymmetric gravity field of the Galaxy could / would show the stars' birthplaces and check if they fall inside the arms. With their tables of plane galactic orbits, Stromgren traced back the migration history

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[44] Kerr (1962) was likely the first to point out systematic motions as a possible source for differences in the northern and southern observations. Considering such motions near the outer edge of the Sagittarius arm, Burton (1966) suggested that an along-arm hydrogen flow might explain the high-velocity stream. Shane and Bieger-Smith (1966) considerably contributed to the discussion.

[45] "These variations have long been observed, but they were thought to be possibly the consequence of missing gas over interarm regions. A detailed study by Yuan (1969a) has conclusively shown that the latter effect does not give significant contributions to the variation in velocity." (LYS, p.731)

of about sixty late B stars aged between 100 and 200 million years and placed within 200 pc from our Sun. Those "showed a definite separation into two [velocity] groups", and their birthplaces took a nearly tangent-to-circle extended area connecting those regions of two nearest to us outer arms where the 'points' were grouped more closely. Stromgren concluded that "the present location of the arms favors the picture formed by the theory of density waves, providing one takes the pattern frequency Ω_p to be about 20 km/sec/kpc" and that this "offers possibilities of testing the theory developed by C.C. Lin" and "forms a *definite test*" for it (Stromgren 1966b, pp.3- 4; Stromgren 1967, pp.325, 329).

In response, Lin executed some "preliminary explorations" accounting for the spiral component of galactic gravity, and found that "even a small spiral field [. . .] could be quite significant" (LYS, p.734). He then urged Chi Yuan to check "whether there exist a pattern speed and a strength of the spiral gravitational field (or a range for it) such that the stars considered are found to have been formed in the gaseous arm as expected" (Yuan 1969b, p.890). Experimenting with different choices of the parameters, Yuan preferred a pattern speed of about 13.5 km/sec/kpc and a spiral field strength of about 5%, with which he calculated time-reversed motions of 25 stars from the Stromgren sample, their ages being 'optimized', or arbitrarily shifted within 15 percent in the desired sense. With these (and several other) corrections Yuan succeeded in improving Stromgren's picture and taking out of the interarm space all his stars that fell there *but one*. LYS proclaimed this result as offering an "impressive agreement" and "also extremely satisfactory comparison between theory and observation" (LYS, p.736). That was an overestimate, as was soon shown to the authors (Contopoulos 1972; Kalnajs 1973) and as they conceded in turn.[46]

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[46] Contopoulos gave two reasons why he did "not consider this test as crucial". First, he calculated the uncertainty in the birthplaces, assuming an uncertainty in the ages of 10-15 percent, and found that that was large enough "so that most of the stars found by Yuan as born between the spiral arms may well have originated in a spiral arm, *without considering the attraction of the arm*". Secondly, he noticed, *in any case* and any spiral galaxy the stars spend on the average more time in the arms than between them. "Therefore, finding that the perturbed orbits give the places of origin in the spiral arms does not provide a good test for the particular model chosen. [. . .] Similar results were found by Kalnajs (private communication) after a more detailed analysis". (Contopoulos 1972, p.91)

Indeed, Kalnajs (1973) reproduced all of Yuan's calculations and determined their statistical significance. He found that even when correcting star ages following Yuan in a most advantageous manner, one to three stars from the latter's sample should anyway be expected to be 'bad' and not to leave the interarm territory. Yuan had one such star *at least*. (Stromgren's initial sample included 26, not 25 stars; the omitted 26th proved 'bad', too.) Therefore, Kalnajs concluded, Yuan's "calculated birthplaces of the stars, while in agreement with the expectations of the density wave theory, do not provide a test for the presence of the spiral field" (Kalnajs 1973, p.40). "Perhaps C.C. thought this was a stringent test of the theory, but as I discovered, the truth is quite the opposite: nothing really could have gone wrong, and what little did go wrong was hushed up by the omission

III. SHARPER FOCUS

> When a discovery is already done, it usually appears so evident that one cannot but wonder why nobody hit upon it before.
>
> *P.A.M. Dirac 1977*

3.1 A feel of group velocity

> And yet, though it may be premature to speak of spiral waves as true modes of oscillation, it seems entirely appropriate to ask how some postulated spiral wave pattern in a galactic disk would *evolve* with time.
>
> *Toomre 1969, p.899*

The WKBJ-style hot-disk dispersion relation admitted at least two different treatments. Lin and Shu's rested on its 'modal' form $\lambda(\nu)$. Looking for a particular two-armed spiral wave, they let it rotate with some angular speed Ω_p, converted it to its pure-note frequency $\omega = 2\Omega_p$, got it differentially 'Doppler-shifted', $\omega_* = \omega - 2\Omega(r)$, found a ratio $\nu \equiv \omega_*/\kappa$ and, upon substituting it into $\lambda(\nu)$, obtained and plotted the ready-to-serve interarm-spacing function $\lambda(r)$ and its pitch-angle cousin $i(r) = \lambda(r)/\pi r$.

More in Lindblad's spirit, though equivalent, was the treatment stressing that the dispersion relation specified the reduction of free oscillation frequency κ to some $|\nu|\kappa$ due to gravitational star coupling. This provided a deeper look at the so called 'dispersion orbits' – ovals composed of many

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of the errant star #26." (*Kalnajs*)

"I never responded to Kalnajs' article, Yuan comments. The reason was he stressed the point, if I am not mistaken, that we have not proven the density wave theory to be correct by star formation study. We did not want to challenge that point. In fact, we agree with it. We only demonstrated the consistency between the theory and the observations (not only star migration but all other studies, e.g., streaming motions, vertex deviation, etc). I believe that his Observatory article was written to respond to some of the strong claims of the density wave theory made by C.C. in early days. [...]

My early contribution to the density wave theory is to piece together all the relevant observations to show the consistency of the theory. One aspect in agreement is not enough, but the agreements with all observations are impressive. The most significant early work for me was the doubly periodic solution of the MHD density waves (Roberts and Yuan 1970; that paper was alphabetic order in authorship; I made the crucial assumption and formulated the problem and solved it in parallel to Roberts). That work was shortly confirmed by Mathewson in observation of synchrotron radiation of M51. It produced a strong support of the density wave theory. That MHD model is still the best model for the Milky Way." (*Yuan*) separate test stars at their judiciously phased gyrations about a mean circumferential radius but devoid of self-gravity. Such 'orbits' precess at a rate $\Omega_{pr}(r) = \Omega - \kappa/2$, and if general rotation did ensure an approximate radial independence of this combination, that alone would give practical prospects of plaiting the happily co-revolving ovals into a common quasi-steady two-armed pattern. Nature's choice proved slow variability of $\Omega - \kappa/2$, however, as if implying that there might be reason to try the possibility of reducing $\Omega_{pr}(r)$ to a common value by allowing for the as-yet-dormant star coupling. Indeed, that pointed at $\Omega_{pr} = \Omega - |\nu|\kappa/2 = \Omega_p = const$ with its now Lin-Shu tuning formula $\nu(\lambda)$ for selecting spiral geometries $\lambda(r)$, but to make this chance really work, it needed to be demonstrated that the desired tuning of the precession rates could actually be accomplished simultaneously, over a large radial span, and with plausible interarm spacings.

Yet some restrictions were to be placed on these considerations. One was that in a disk of stars the WKBJ waves could not abandon the territory fenced by their related ILR and OLR. But Lin and Shu, who had rightly fixed it in their 1964 patent, found this partial ban to be even a positive factor as they let it favor the prevalence of two-armed spirals, on the simple ground that only those might occupy the entire disk region between the best separable $m = 2$ Lindblad resonances. Still, for tentative disks reserving local stability there happened to be another type of basic restriction.

Despite several early cautions (Kalnajs 1965, Julian 1967), Lin and Shu kept on exploring their waves for the extra-helpful special case $Q = 1.0$ only. This persistence seemed to annoy Toomre until late 1967 when he "finally ground out for [him]self what their dispersion relation would imply" at $Q>1$ (*Toomre*). He plotted $\nu(k)$ for different Q's (Fig.8) and found that the case $Q = 1.0$ was in a sense degenerate: it did let the WKBJ waves reach the corotation circle from both sides, but just a minuscule addition to Q was enough to create their forbidden near-corotation zone that already for as not so very much as $Q = 1.2$ paralyzed quite a sizable portion of the disk.[47]

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[47] To be true, Shu was the first to discuss the $Q \neq 1$ disks publicly, he did it in his thesis (Shu 1968) when attempting to "finish cataloging the nature of the dispersion relation for *neutral* waves" (S68, p.113). Because this nature "changes somewhat when $Q \neq 1$", he "briefly summarize[d] [its] salient points" (p.114) using a special plot (Fig.9). For the $Q > 1$ that summary read: "There is a region about $|\nu| = 0$ for which spatially oscillatory waves cannot propagate. Toomre (private communication) has computed that for values of Q which are moderately greater than unity, the region of inaccessible $|\nu|$ can be quite substantial. [...] Such an effect is not too serious since for pattern frequencies of the range to be considered [...] the corresponding annular region where spatially oscillatory waves cannot exist is small in comparison with the range where they can exist. When Q is greater than unity, the reflection, refraction, and tunneling of propagating waves by and through such annular regions become a serious problem for investigation" (p.116). Shu, however, did not explain, nor did he even hint, why he let this serious problem miss the threshold case Q=1 where his proposed 'Mode-A' was so welcome to cross corotation smoothly, i.e. with no reflection, no refraction and no change in the sign of group velocity.

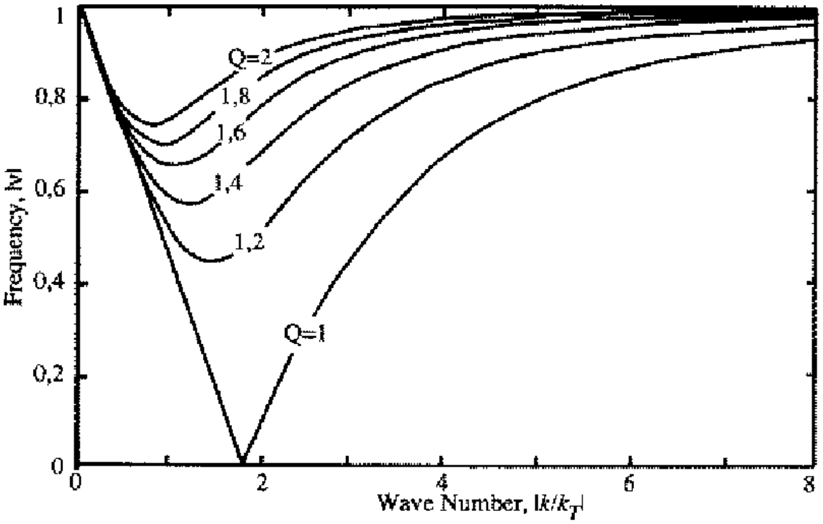

Figure 8: *The Lin-Shu-Kalnajs dispersion relation.* Wavenumbers are in units of $k_T = 2\pi/\lambda_T$. (The figure is reproduced from Toomre 1969)

Not even this, however, was the chief restriction to the envisaged frequency-tuning success. "Lin and Shu completely overlooked that in repairing one serious defect they had actually created another: An inevitable price for altering those speeds of precession in a wavelength-dependent manner via the (very sensible) radial forces is a *group* velocity, likewise directed radially" (Toomre 1977, p.449). Evaluation of this 'price' made the point of Toomre's work "Group velocity of spiral waves in galactic disks" (Toomre 1969, hereinafter T69). In his preliminary "Note on group velocity" that in late 1966 was privately circulated at MIT, Toomre had discussed the dispersive properties of the original cold disk by Lin and Shu and called upon extending his discussion to their newer and fairer hot model.[48] But Lin himself "never took it very seriously", and not only because there were "plenty of reasons not to brag about that old note" (*Toomre*). More generally, at the time he fell into a muse over the role of his introduced 'reduction factor', when the group aspect might well appear to him merely as an unnecessary tedious detour in the pursuit of his plain ideas, so that he got no particular intention to 'comb' the hairy and transcendental Bessel functions and the like in his dispersion relation for finding out some certain explicit function $\omega(k)$ just to take its trite derivative.[49]

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[48] As defined in the standard $d\omega/dk$ fashion, the 'group velocity' of a rotating cold disk grows infinite as one approaches the critical wavelength λ_T, below which the model gets unstable.

[49] "Besides, why in fact would anyone want to differentiate the frequency ω only with respect to the radial wavenumber k instead of also the circumferential wavenumber m, since 'everyone knows' that a group velocity is a vector quantity, with components in

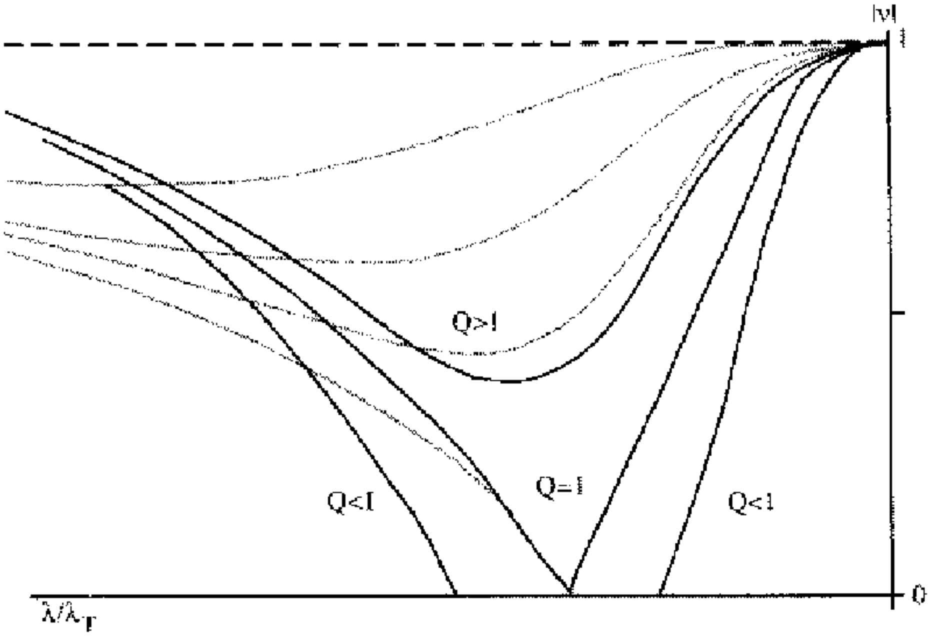

Figure 9: *The Lin-Shu-Kalnajs dispersion relation in the 'modal' form* $\lambda(\nu)$. Shu's original curves (reproduced from Shu 1968) are given against the exact (lighter) curves

> *Lin*: "The general theory of group velocity is a well-developed and much taught (e.g. in quantum mechanics) classical study valid for any dispersion relationship connecting that wave number with the frequency. Different people will feel differently whether it is even necessary to go beyond taking the derivative and develop it anew for any each specific application. I adopted the empirical approach. [...] That was directly related to the calculation (or derivation) of the dispersion relationship. For this calculation, Frank Shu did his share. The Lin & Shu 1964 paper showed that the crucial step is the calculation of the *reduction factor*. [...] As I worked out the dispersion relationship, I realized that the present problem is further complicated by the presence of resonances. Thus the hope of success in the calculation of modes depends on a very long-term effort (as it indeed turned out to be the case). Thus our strategy not to pursue the dynamical approach immediately turned out to be the right choice."[50] (*Lin*)

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both directions? This question sounds pretty silly in retrospect, but obstacles like that often seem a lot taller when they are first met." (*Toomre*)

[50] In 1968 Lin gave a course at the Brandeis University Summer Institute, with the purpose "to present the modern version of the *density wave* theory as developed over the past few years by myself and my collaborators Frank H. Shu, Chi Yuan, and William W. Roberts" (Lin & Shu 1971, p.239). Put on paper, that course appeared as Lin & Shu 1971. Speaking there of the spiral interest of prominent astronomers for many years, the authors emphasized that "until recently, however, there has always existed the *dilemma of differential rotation*", and claimed that "in this article, [they] shall present an essentially stellar dynamical theory for the *persistence of the spiral pattern in the presence of differential rotation*" (Lin & Shu 1971, p.239, 248). Then – as originally in Lin & Shu 1964 – they "venture[d] to suggest that there are indeed large-scale neutral (or nearly neutral)

> *Shu*: "I remember Lin telling me that he had group velocity well before the T69 paper on the subject; however, none of us then had any idea (a) what the group velocity carried, and (b) why the concept would be relevant to disturbances with a single value of the wave frequency." (*Shu*)

> *Toomre*: "The reasons why one does or does not choose to attack some scientific problem from a particular direction are rather 'artistic' in nature, and hard to make (or even hope to make) very sound and rational. [...] Surely group velocity may be terribly 'obvious' in retrospect to various learned scholars, but I believe it did not seem so to Shu at the time he struggled with his 2^{nd}-order WKBJ thesis at Harvard." (*Toomre*)

The main reason why Toomre took to the topic seriously in late 1967 only, about two years after his work with Julian had been completed and the Lin & Shu 1966 paper published, was that in the global-mode context the latter seemed to him to be no more than a trial exploration, and what he judged vitally important and necessary to prove or disprove those authors' 'asymptotic' hopes was a full-fledged disk analysis.[51]

> "In that climate of opinion it wasn't immediately evident to me, or to anyone else, that one would learn much from the group velocity of those short WKBJ waves that Lin and Shu (1966) were suddenly proposing. Of course I was wrong there, but at least I can boast that by 1969 I myself had repaired that oversight!" (*Toomre*)

Toomre became the first to take real action on the evident understanding that *if* indeed a quasi-steady spiral mode can form in a galaxy disk, then it does it via natural wave-packet evolution. And group velocity, he showed,

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waves of spiral form for most of the disk galaxies, and formulate[d] [their] ideas in the form of the [QSSS] hypothesis". They pointed out that accepting it and "following the line of reasoning that led to it" one infers, among other things, that in the coordinate system rotating with the pattern "all phenomena are stationary" and "both the stream lines and the magnetic field lines form closed nearly circular loops coinciding with each other" (Lin & Shu 1971, p.248). Then the authors gave a basic presentation of the WKBJ dispersion relation and of the ensuing comparison with observations, and concluded that their spiral theory "needs extension in several directions to complete the theoretical understanding of basic mechanisms and to develop its implications". The directions there envisaged were (1) the "Thickness effect", (2) the "Complete formulation of the theory", (3) the problem of the "Origin of galactic spirals" that "cannot be solved without using the complete formulation mentioned above", and (4) the "Nonlinear theory", becoming important as "one looks beyond the developments of the complete linear theory". In topic (3) the authors enthused on the promising "preliminary indications" and called for "much work [that still] remains to be done", mentioning in a footnote that "Toomre (private communication) has also carried out studies involving the propagation of a group of waves and their initiation by external agents" (Lin & Shu 1971, pp.287-289).

[51] "In essence, I was there just echoing what Agris Kalnajs actually wrote in his not-very-conclusive but nonetheless farsighted concluding chapter on "Instabilities and Spiral Structure" of his 1965 thesis. Yes, it seemed to both *him* and me at the time – plus probably Hunter, Lynden-Bell, Lebovitz, etc. – that there was a lot of hard but very promising work to be done on the 'global' behavior of full-scale disks." (*Toomre*)

describes at least qualitatively how different kinds of information from the packet are transmitted along the radius, being therefore directly related to the maintenance of *all* sorts of spiral patterns, even steady ones.

3.2 Group properties of tightly wrapped packets

> ...a shatteringly destructive article.
>
> *Lynden-Bell & Kalnajs 1972, p.1*

Various properties of certain types of waves are described in a unified way, regardless of the specific sort of the medium in which they propagate. Such, for instance, are nearly plane – weakly modulated – waves $\varphi(\mathbf{x},t) = A(\mathbf{x},t)\cos[S(\mathbf{x},t)]$ whose amplitude $A(\mathbf{x},t)$ is much less dependent of its arguments than the phase $S(\mathbf{x},t)$. Their wave vector $\mathbf{k} = -\nabla S$ and frequency $\omega = \partial S/\partial t$ get connected through a link $\partial\mathbf{k}/\partial t + \nabla\omega = 0$ meaning conservation of the wave crests in number, their being neither created nor annihilated. One more link is the common dispersion relation $\omega(\mathbf{x},t) = f[\mathbf{k}(\mathbf{x},t),\eta(\mathbf{x},t)]$ (with parametric η-dependence reflecting spatial inhomogeneity). Together, these two connections form equations

$$\frac{\partial\omega}{\partial t} + \frac{\partial f}{\partial\mathbf{k}}\nabla\omega = -\frac{\partial f}{\partial\eta}\frac{\partial\eta}{\partial t}, \quad \frac{\partial\mathbf{k}}{\partial t} + \frac{\partial f}{\partial\mathbf{k}}\nabla\mathbf{k} = -\frac{\partial f}{\partial\eta}\nabla\eta. \tag{1}$$

Their characteristic curves coincide with the solution $\mathbf{x}(t)$ of the equation

$$\frac{d\mathbf{x}}{dt} = \frac{\partial f}{\partial\mathbf{k}}; \tag{2}$$

they are understood as *rays*, in analogy with geometric optics. Determined by the left-hand side of (2), vector $\boldsymbol{c}_{gr}$plays as group velocity, with it information on ω and $\boldsymbol{k}$ is conveyed along the ray. If the medium is in general motion with a speed $\mathbf{U}(\boldsymbol{x},t)$, the waves are carried away. A co-moving observer finds their frequency shifted, $\omega = \omega_* + \boldsymbol{k}\mathbf{U}$ (asterisk marking the shifted quantity), and the equations (1), (2) preserving their form. In particular, for the Lin-Shu WKBJ waves they become

$$\frac{d\omega}{dt} = 0, \quad \frac{dk}{dt} = -\left(\frac{\partial f_*}{\partial r}\right)_k, \quad \frac{dr}{dt} = \left(\frac{\partial f_*}{\partial k}\right)_r = c_{gr}, \tag{3}$$

right how Toomre wrote them having k and m as radial and azimuthal wavenumbers, and $\omega = \omega_* + m\Omega(r)$.

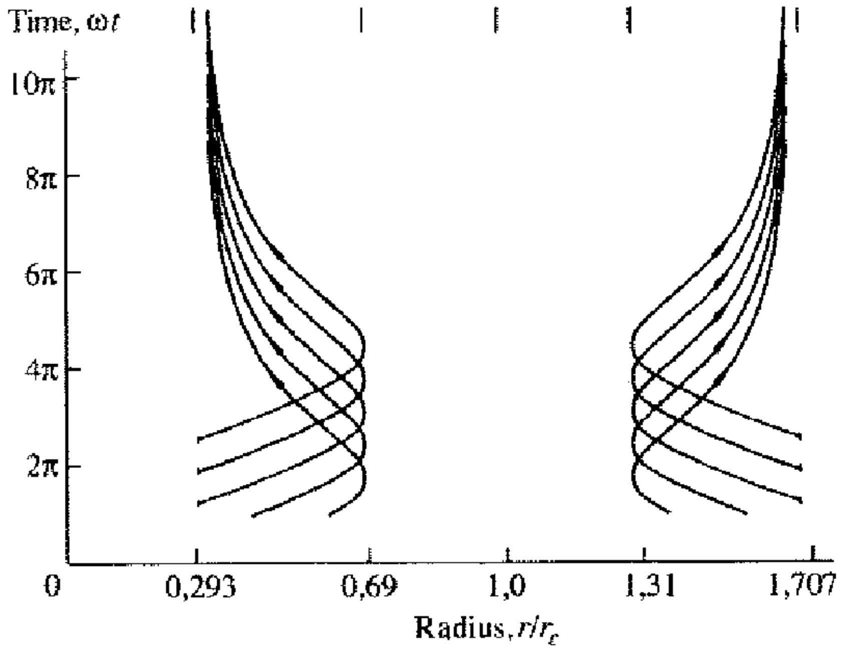

Figure 10: *Some $m = 2$ characteristic curves (rays) for a disk in which $Q = 1.2$* The x–axis is scaled in corotation radius units. (The figure is reproduced from Toomre 1969)

Equations (3) describe the radial transmission of the signals informing one about invariable wave frequency and knowingly changing wavenumber. Toomre computed them for an easy-to-use but realistic model with $Q = const$, $V = r\Omega(r) = const$ where the rays $r(t)$ just repeat, in relabeled axes, the form of the dispersion curve $\omega_*(k)$. They are followed always in the sense of growing k, because of which leading waves ($k < 0$) can do nothing but unwind while those trailing ($k > 0$) wind up more and more. Given by the local 'slope' dr/dt, c_{gr} changes its sign as the ray reflects from the near-corotation barrier, and the Lin-Shu adopted short-wave branch of the solutions has it negative inside corotation, or directed inwards. A value $c_{gr} \cong$-10 km/sec that Toomre found for the solar vicinity yields an estimate of few galactic years only for the signal to travel from corotation to the ILR. To an already existing tightly wound trailing pattern, an entire ray family may be compared in its part after the near-corotation turning point, giving exhaustive knowledge on the current and following dynamics of such a wave packet (Fig.10): its information will simply be conveyed inward and gather all at the ILR where the wave group velocity and pitch angle tend to zero.

To illustrate these 'information' results, Toomre computed the wave-packet evolution. There he relied on the program that had served him and Julian for local needs of their Cartesian model (JT), because that model luckily revealed an ability to mimic not only the corotation resonance $x = 0$ but also the Lindblad resonances, since stars placed at and moving along lines $x = \pm x_L = \pm\kappa/2Ak_y$ at the expense of shear were ascertained to feel a $\cos(k_y y)$ wave at their natural frequency κ.[52] Toomre placed a short-term emitter of such waves a little below $x = -x_L$ – this imitated a bar – and purposely chose rather long-wave situations $\lambda_y/\lambda_T \geq 4$ where the JT-exploited swing amplifier was all but shut off. His computations showed that the 'bar'-induced trailing-wave packet propagates outwards; that its envelope drifts in approximate conformity with the established by the ray methods characteristic curve; that indeed a larger part of energy flow is reflected somewhere near corotation; and that the packet drifts back to$x = -x_L$ where it eventually damps (Fig.11a).

The wave-packet evolution in the threshold $Q = 1.0$ disk was of particular interest. While the Lin-Shu theory allowed the tightly wrapped waves to reach and touch the corotation circle, it did not know if they could cross it. And these waves showed they really could: the packet readily invaded all the healthy tissue between the ILR and OLR, and even got amplified to a degree, but then the inevitable group drift constricted it like a sausage at $x = 0$, squeezed it out of that region and took the forming parts to their LR destinations, as in the common case of $Q > 1$ (Fig.11b).[53]

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[52] Of interest is the following record made by Toomre in January 1968. "The linearly shearing, constant surface density model of a star disk that was used by Bill Julian and myself admittedly lacks i) curvature, ii) boundaries, and iii) any gradients of unperturbed quantities such as c_r^2 or κ. Nevertheless it can be used in the following manner to illustrate to all desired numerical accuracy not only C.C.'s dispersion relation for tightly wrapped spiral waves, but also the related transient behavior and the transfer of energy. The point is that if one were for some reason to choose any specific circumferential wave number in the JT model, then as Agris correctly pointed out during Frank Shu's thesis exam yesterday, our model, too, would have various Lindblad resonance radii [. . . and the region between them] will then correspond to what C.C. and Frank call the 'principal range'." (*Toomre*)

[53] The question on the preferable sense of spiral winding was not discussed explicitly in T69. Toomre (as well as several others) held that its full solution might be obtained only in the global-wave setting of the spiral problem. At the same time, he was sure that several local findings already gave a *sufficient* understanding of the trailing-sense benefits. He meant, above all, the delayed character of cooperative star wakes of non-axisymmetric forcing from individual material clumps in a galaxy disk, and the group properties of the Lin-Shu spirals. Indeed, since we do not observe them at the stages of very loose winding and cross orientations, these stages either went already (or were altogether absent) or they still shall have to go. In the first case we have the trailing spirals whose old times are almost unknown to us but whose long-lived future is unambiguously associated with states pretty close to the today's one. In the second case, we would have the leading spirals, huddling up to their ILR and extremely tightly wrapped in order to avoid premature unwinding before too long. Besides, not to forget, the waves of the short-length limit get excited with almost no concern of self-gravity, so that only some 'pressure'-force mechanism can generate them. But what might be concretely any such *'elastic'* mechanism localized in a narrow circumcentral ILR region, and how would it manage to create a practically circular

Now what physically do the waves carry over the star disk and how do they do it? This question was not trivial at the time. Only by the mid-1960s Whitham had worked out a general variational principle for describing a wide class of wave fields with dispersion. For weakly modulated packets it led to the equation

$$\frac{\partial}{\partial t}\frac{E_*}{\omega_*} + \nabla\left(\mathbf{c}_{gr}\frac{E_*}{\omega_*}\right) = 0, \tag{4}$$

expressing conservation of the wave-action density E_*/ω_* and its along-ray transmission with group velocity (E_* being the mean volume density of low-amplitude waves, and $\mathbf{c}_{gr}E_*$ – its flow). Toomre conceived that this should be applicable to the Lin-Shu waves as well,[54] and felt that the 2^{nd}-order WKBJ theory, which Shu had already been developing to estimate the rates of change of wave amplitudes with radius, should also yield an accurate radial derivative of E_*. The dE_*/dr that he first inferred from Shu's analyses differed in two small but vital ways from that implied by equation (4). However Toomre suspected that some errors had crept into Shu's work. In due course he located them, and Shu soon concurred (Shu 1970b,c).[55] After these small repairs, as Toomre remarked (T69, p.910),

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wave running away ($c_{gr} > 0$) from a gently sloping (inelastic) 'beach' of the ILR instead of rushing on it just like an ocean wave? Only something akin to a Maxwell demon, Toomre guessed, could manufacture such short leading waves.

Yet he mentioned them once in T69 in the positive sense. Speaking in a footnote of plausible variants for either one or both m =2 Lindblad resonances to be absent from a galaxy disk, he remarked that "in such cases the given wave packet must in some sense be reflected either from the outer edge of the disk or from its center" and that "in the process the character will presumably change from trailing to leading, and the sign of the group velocity should also reverse" (T69, p.909). But, true, at that time Toomre did not think seriously about any such conversion.

[54] "I was glad enough to brag there that I could also figure out that energy density itself, [... but] I was yet prouder of noticing and pointing out that the main conserved density is not even that energy as such, but instead the *action* density [...] which Kalnajs in turn soon told me had to be 'the excess density of angular momentum associated with the wave'. [...] There was nothing very original about either accomplishment, though of course it could not have been entirely obvious a priori that Whitham's Lagrangian reasoning would apply here as well, with these collisionless stars rather than some more standard fluid." (*Toomre*)

[55] (*Toomre*): "Even in 1967 I was well aware that Frank Shu seemed to be progressing nicely with his thesis, and was still claiming to confirm and to expand upon the 'gradient instability' which he and C.C. had announced rather cryptically in Lin & Shu 1966. In detail, I did not pay much attention until he had finished, but then gave his analysis an exceptionally close going-over once he had been awarded his PhD. [...] Amidst his immense and rather impressive 2^{nd}-order WKBJ calculation I eventually located two small algebraic errors, once I had suspected because his inferred dE/dr did not quite match what I had hoped for in what became eqn (34) of T69. Frank soon agreed, and that was the end of those gradient instabilities!"

(*Kalnajs*): "As to the famous 'gradient instabilities' I went as far as to type up a short

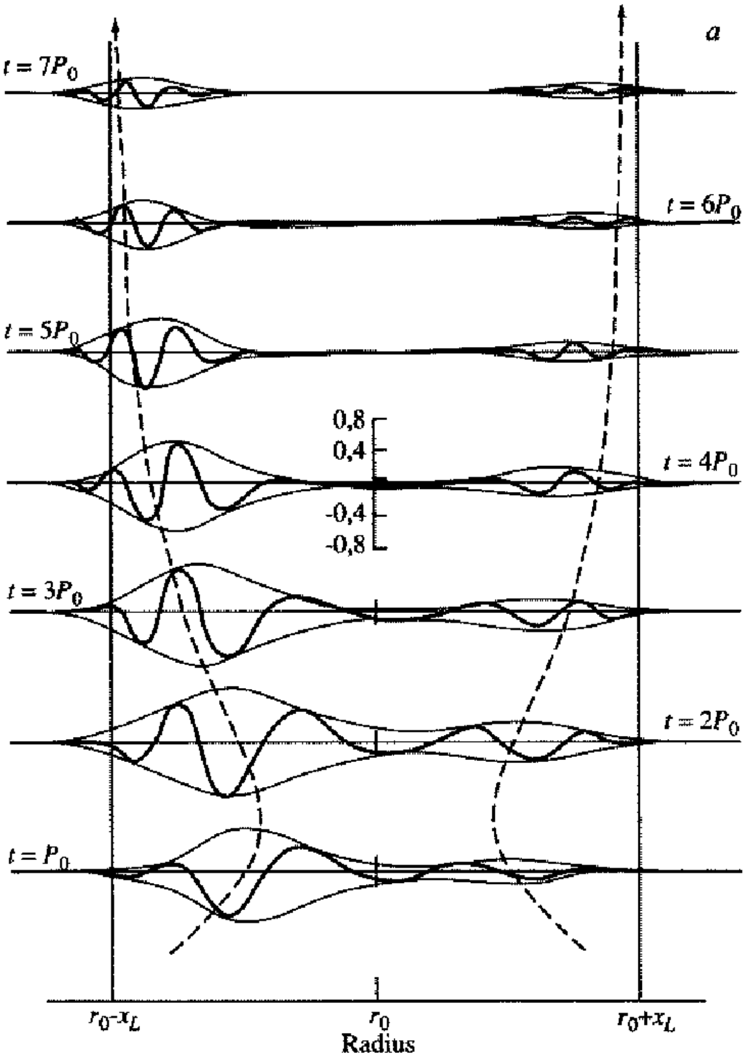

Figure 11: *A density wave evolving (a) in the $Q = 1.2$ and (b) $Q = 1.0$ local models.* r_0, r_0-x_L and r_0+x_L are the corotation, ILR and OLR radii, $x_L = \lambda_T$, $P_0 = 2\pi/\kappa_0$. (The figure is reproduced from Toomre 1969)

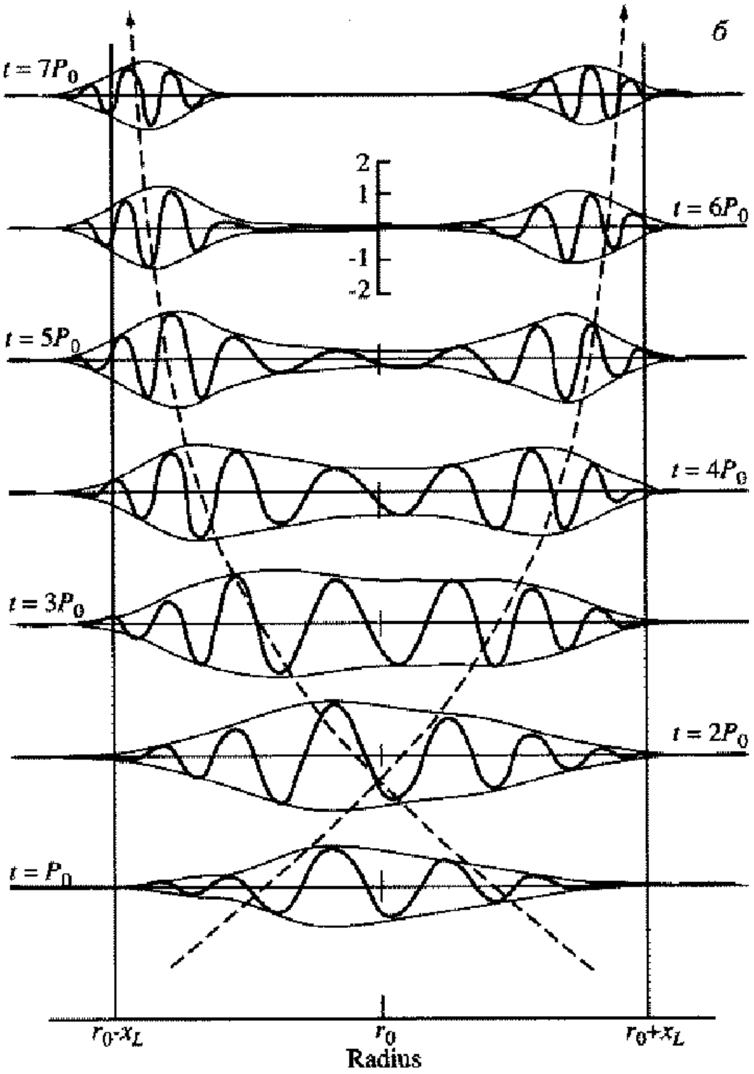

Fig. 11 (b)

Shu's work unwittingly closed the main logical gap of his own paper, and this concluded his expose of the serious *strategic* error by Lin and Shu – their oversight of the group velocity.

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paper, dated July 29, 1968. I used my integral equation to show that if you put corotation at the outer edge and made the same sort of tightly wound approximations as Lin and Shu, then there could not be any instabilities. But David Layzer thought that it would be far better if my first publication on density waves made a positive contribution. So this effort remained in a drawer. As it turned out, the 1969 Toomre paper made a positive contribution to the field and at the same time debunked the 'gradient instabilities'."

Indeed, in a year Shu found it "apparent that the growth (or decay) of the wave amplitude arises because the disturbance is propagated radially in an inhomogeneous medium and not because the disturbance is inherently overstable" (Shu 1970c, p.110).

3.3 Sources of spiral waves

For one thing, Toomre's work T69 logically debunked the very principle of the Lin-Shu theoretical construction, having shown that for all the profundity of their core QSSS hypothesis their selfsustained-wave claim did not immediately follow from their genuinely straightforward – azimuthal-force-free – 'asymptotic' dispersion relation. Yet it also made an important positive offer. The point was that wave-packet drifting and damping still did not exclude the possibility itself of really long-lived spirals, it only implied that "if such patterns are to persist, the above simply means that fresh waves (and wave energy) must somehow be created to take the place of older waves that drift away and disappear".

> "Where could such fresh and relatively open spiral waves conceivably originate? The only three logical sources seem to be: (a) Such waves might result from some relatively *local* instability of the disk itself. (b) They may be excited by tidal forces from *outside*, such as from a companion or satellite galaxy. (c) Or they might be a by-product of some truly large-scale (but not necessarily spiral) distortion or instability involving an entire galaxy" (T69, p.909).

Thus Toomre simply formulated the evident – but as yet unreleased – necessity of establishing real mechanisms for maintaining spiral structure in galaxies.

At the time, Toomre's particular interest lay in the tidal mechanism.[56] It arose after his and Hunter's work on bending oscillations (modes) of finite-radius thin disks of a single gravitating material (Hunter & Toomre 1969). Among other things, that study hypothesized that the bending of our Galaxy might be due to the vertical component of tidal force during a possible close passage of the Large Magellanic Cloud (LMC). The authors reckoned that their relatively slowly evolving $m = 1$ retrograde responses were the only plausible candidates for the observed distortion. This made them infer a

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[56] By the 1960s, the version of gravitational tides as mainly causing the observed variety of 'peculiar' forms of interacting galaxies had been discredited, and what was brought to the forefront were alternative considerations about magnetism, explosions, ejections, and just as-yet-unknown 'forces of repulsion', all kept at a level of hopes and suspicions (the topic has been nicely reviewed in Toomre & Toomre 1973). During the decade, the tidal ideas were being gradually rehabilitated, but, Toomre noticed (Toomre & Toomre 1972, p.623], "judging from the reservations admitted by Zwicky (1963, 1967) despite his former use of words like 'countertide' and 'tidal extensions' – and especially from the vehement doubts expressed by Vorontsov-Velyaminov (1962, 1964), Gold and Hoyle (1959)], Burbidge, Burbidge and Hoyle (1963), Pikel'ner (1963, 1965), Zasov (1967), and most recently by Arp (1966, 1969a,b, 1971) – it has usually seemed much less obvious that the basis of also such interactions could be simply the old-fashioned gravity." Only in the early 1970s did the tides find proper treatment (Tashpulatov 1969, 1970; Kozlov et al 1972); that was a period of general recovery and renewal of interests to galaxy dynamics.

very close passage at a perigalactic distance of 20-25 kpc and, to bring estimates into appreciable consistence, even claim to favor a solar galactocentric distance $R_O \cong 8$ kpc instead of a little too 'ineffective' 10 kpc sanctioned at the time by the IAU. But Hunter and Toomre "were blissfully unaware" of the work by Pfleiderer and Siedentopf (1961; 1963) and "also did not realize the undue sensitivity – which those German authors had already implied – of any such disk to the horizontal components of the same tidal force during a *direct* encounter of low inclination" (Toomre 1974, p.351).[57] In a sense, Pfleiderer became Toomre's eye-opener, and in the closing part of T69 he already proposed that much of any spiral density wave in our Galaxy might have evolved from vibrations set up during such a passage of the LMC. Providing its orbital eccentricity $e \geq 0.5$, it would have spent less than one galactic year traversing the nearest 90^0 of galactocentric longitude, and in the direct – not retrograde – case the implied angular speed Ω_s would have roughly matched the speed of advance, $\Omega - \kappa/2$, of the slow $m = 2$ 'dispersion orbit'. "And that, coupled with the dominant $m = 2$ character of the tidal force in the plane, means any direct close passage of the LMC should have been very effective in exciting $m = 2$ oscillations of the Galaxy" at a radius where $\Omega_s = \Omega - \kappa/2$. "It also suggests that, even with self-gravitation taken into account, the resulting 'pattern speed' should have been of the order of 10 km/sec/kpc" (T69, p.911).

Toomre (1969) supported his reasoning by computations of the perturber's action on the Galaxy disk test particles (Fig.12). Then he made a separate 'progress report' at the Basel Symposium (Toomre 1970), but soon turned his tidal interests to more spectacular and controversial forms, which resulted in the famous dynamical study of 'galactic bridges and tails'

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[57] Pfleiderer reasoned that tidal action should be much the strongest in the exposed and relatively slowly rotating outer parts of the galactic disks where the mass density is small and its self-gravity must be weak. He thus just neglected the latter and treated the disk particle dynamics as the restricted three-body problem, these three being the test particle and mass centers of the paired galaxies. Such an over-idealization greatly simplified his computer work (which still remained time-consuming since hundreds of trial encounters were required for an understanding of the effects of various mass ratios, orbital parameters and times and directions of viewing).

"These test-particle calculations can, of course, be criticized for their total neglect of any interactions between the various particles. However, this is not to say that the self-gravity of these relatively low-density parts of the disk should immediately have been of major importance, nor does it contradict our qualitative picture about the evolution of the waves: For one thing, the relatively sudden passage of the LMC should have induced roughly the same initial velocities regardless of the subsequent *disturbance* gravity forces from within this system. And also, it seems that the principal effect of that latter mutual attraction of the various disk particles should have been to enhance the shearing discussed above, since in effect it would have reduced the epicyclic frequency κ and thus caused the wave speeds $\Omega - \kappa/2$ at the various radii to become more disparate." (T69, p.912)

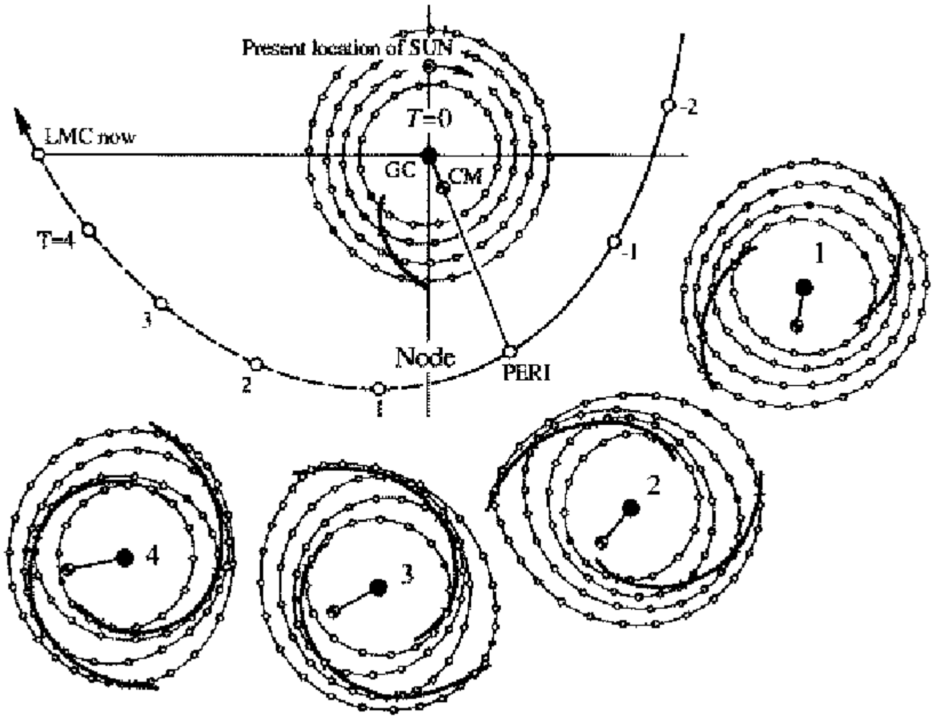

Figure 12: *A time history of the displacements of four rings of noninteracting test particles provoked by a simulated direct passage of the LMC.* The spiral curves connect points on each ring which are at maximum distance from the Galaxy center. Point ‘CM’ marks the location of the center of mass. Time in units of 10^8 years is reckoned from the perigalactic point. (The figure is reproduced from Toomre 1969)

done jointly with his brother (Toomre & Toomre 1972).[58]

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[58] “The hopes of Hunter and myself that an unusually close passage of the LMC caused the well-known warp of this Galaxy proved to be sadly in error. I worked on that topic quite intensely for another year or so, and even ‘predicted’ a long tidal stream to be torn loose from the LMC in turn [...] and probably inclined about 30 degrees to the plane of our Galaxy. I never published that, but it was well enough known hereabouts that one day in early 1972 I got a sudden phone call from Wannier or Wrixon at Bell Labs to ask whether a good chunk of what turned out to be the Magellanic Stream which they had just then spotted – about a year or two before Mathewson et al (1974) turned it into a big business from Australia – might possibly be the stream of gas that I had asked about among several of our radio astronomers. And I still remember with pride that it took me just the few minutes during that phone call, after learning this new Stream was located almost a right angles to this Galaxy, to reply sadly that such an orientation or orbit could not help Hunter and me at all, and that there apparently we had *lost*!

That Basel example led me soon to enlist the help of my own brother Juri, who was then affiliated with one NASA research institute in New York City that had much better computers than any that I had access to ... and that in turn eventually led to our joint paper Toomre & Toomre 1972. No, we did not even come close to explaining the warp of our Galaxy ... but we did end up explaining other nice things like NGC 4038/39 = ‘The Antennae’, and pointing out that the implied galaxy mergers probably explain why we

IV. GATHERING IN BASEL

4.1 Astronomers' applause

> Thus the credit goes to Lin who not only developed the theory of spiral waves in much more detail, but also presented it in a relatively simple form that made it acceptable to the rest of the astronomical world. The response of the work of Lin and his associates has been an ever-growing wave research in this area, that has produced many important new results.
>
> *Contopoulos 1970a, p.303*

The August 1969 Basel IAU Symposium "The Spiral Structure of Our Galaxy" was a significant event in the astronomical life, "the first international gathering ever of optical astronomers, of experts in galactic dynamics, and of the world's greatest radio *astrologers*".[59] Bok, Contopoulos, Kerr and Lin were the mainstay of its organization presided over by Woltjer who deserved "great credit for planning the symposium to reflect the current status of our knowledge in this field, and for the selection of speakers" (Lin 1971, p.35).

Opening the meeting, Oort conveyed his pleasure that Lindblad's spiral-wave ideas had in recent years been "further worked out by Lin, Shu and Yuan, who showed among other things how such a density wave causing a spiral pattern could be sustained by its own spiral gravitational field superposed on the general axisymmetrical field of the galaxy" (Oort 1970, p.1). This gracious view the speaker supplemented with a prudent, if not veiledly critical, comment.

> "The theory explains the maintenance but *not* the *origin* of spiral structure. I do not think this is an important shortcoming, for it is easy to conceive of processes which would *start* a spiral structure. [...] A more serious problem seems that of the *long*-term permanence of the spiral waves. Can they continue to run round during 50 revolutions without fatal damage to their regularity? Looking at the irregularities in the actual spiral galaxies one wonders whether the present spirals could *continue* to exist for such a large number of revolutions. [...]

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have the ellipticals. Quite a twist from where I began." (*Toomre*)

[59] An extract from *Baseler Nachrichten* quoted in the Symposium proceedings (Bok 1970).

During the IAU General Assembly in Prague, 1967, various theoretical and observational papers were presented at a special meeting of Commission 33 on Spiral Structure, most notably including "The density wave theory of galactic spirals" by Lin, "Magnetic approaches to spiral structure" by Pikelner and "Self-gravitating spiral models of the galaxy" by Fujimoto. The participants' interest was obvious, and Contopoulos proposed a special thematic symposium for 1969. It was agreed to hold it "in Basel, a center of galactic research in the center of Europe" (Becker & Contopoulos 1970, p.vii).

> Dr. Lin has sometime quoted me as having stated [...] that in so many cases spiral arms can be followed more or less continuously through the entire galaxy. I do not want to withdraw this statement, but I must point out that it should be supplemented by two essential additions. First, that in about half of the spirals the structure is either unclear, or there are more than two arms. Second, that even in the half that can be classed among the two-armed spirals there are invariably important additional features *between* the two principal arms, while the latter have often a number of secondary branches coming off their outer rims." (Oort 1970, p.2)

On its empirical side, the meeting revealed strong excitement and desire of astronomers about establishing the Galaxy's spiral structure, at least in general. Their demonstrations were a mixed collection, however. Even the cutting-edge radio data instilled a scanty unanimity at best. Kerr (1970) inferred the Perseus, Sagittarius, Norma-Scutum and Cygnus-Carina Arms as spiral fundamentals, all of pitch angles $i = 5^0 - 7^0$ (the latter having our Sun at its inside, and the Orion Spur emanating from it), but Weaver (1970) agreed on only the first two of them, and then with $i = 12^0 - 14^0$. Was it to be wondered at the scatter of opinions of 'ordinary' optical reporters? Metzger (1970) found no definite spiral pattern at all upon the distribution of HII regions. Courtes et al (1970) re-interpreted data on radial velocities for about 6000 HII regions and concluded an $i \cong 20^0$ four-armed spiral. Pavlovskaya and Sharov (1970) gathered a 14-armed (!) spiral from their studies of surface brightness distribution in the Milky Way plane. Vorontsov-Velyaminov (1970, p.17) reminded that the largely discussed tightly wrapped two-armed spiral proposal for our difficult Galaxy called for quite a number of full turns inconsonant to the views of other galaxies, and he advised "not to be in a haste to construct a model of our Galaxy, but to search the real patterns without bias". Vaucouleurs (1970) interpreted the remarkable '3-kpc arm' as a bar particularly oriented to the line of sight, for which he adduced even more bar-favoring 'statistical' arguments. Kerr (1970), to close this chain, supported an oval distortion of the galactic plane as a plausible cause for marked asymmetry of the observed rotation curve in the North and South quadrants. At the same time, he expressed general concern over a large uncertainty in the determining of galactic distances, which undermined the cogency of any 'large-scale' statements.

In this climate of empirical scatter and vagueness, Lin stated his "bird's eye view of theoretical developments", as enlightened by his focal-point QSSS hypothesis (Lin 1970). That view captured: "ten general observational features which one must consider in dealing with spiral features in galaxies" (p.377); "deep implications on the physical processes in the interstellar medium, and in particular on the formation of new stars" (p.379); "a better opportunity for the understanding of the physical processes, including such microscopic behavior as the formation of molecules and dust grains" (p.389); "a deep mystery of the 3-kpc arm" that on fact might be "a part of a reflected leading wave, of an evanescent type" (p.383); the necessity of recognition that the agreement with observations "should *not* be perfect, since the galactic disk is perhaps not perfectly circular and the actual structure may not be a pure mode in the theory" (p.381); "the success of the theory" as being expected "to embolden us to apply the theory to external galaxies" (p.379). But Lin's special emphasis was there and on "one important theme to be kept in mind" – *coexistence.*

> "The complicated spiral structure of the galaxies indicates the coexistence of material arms and density waves, – and indeed of the possible coexistence of several wave patterns. When conflicting results appear to be suggested by observations, the truth might indeed lie in the coexistence of several patterns. Before taking this 'easy way out', one should of course try to examine each interpretation of the observational data as critically as possible.
>
> There is also coexistence in the problem of origin of spiral structure. From our experience with plasma physics, we learned that there are many types of instabilities. Since a stellar system is basically a plasmoidal system, various types of instability can also occur in the problem of the galactic disk." (Lin 1970, p.379)

Under the auspices of the coexistence theme and in grudging admiration for Toomre's group-velocity work (as yet unpublished[60]) that "brings the problem even into sharper focus" (Lin 1970, p.383), Lin let the sheared waves *coexist* in his grand-design view in order to provide it with one of several possibilities of an instability mechanism. He presumed that such waves naturally occur on the outskirts of a galaxy disk where stars are sparse and the well-cooled gas is dominant; that this calls up "the Jeans instability of the galactic disk" and thus a random occurence of local condensations developing in the GLB fashion into trailing spiral-shaped segments; and that each such 'material arm' produces its own effect, now in the JT manner, and "eventually becomes a roughly self-sustained entity, somewhat like the self-sustained density waves (Lin & Shu 1964, 1966; Lin 1966a) with inherent frequency $\nu = 0$ (corotating waves)" (Lin 1970, p.384). But in fact such 'entities', established in local frames though, were on an intermediate scale at the least, and far enough from their producing source they could indeed appear "somewhat like the self-sustained density waves", but with *non-zero* frequency ν. Thus it was not entirely unreasonable to suspect that a really massive outside perturber might be capable of bringing to life some grand galactic spiral.[61] Lin, however, was "inclined to discount the roles of the satellite galaxies in creating spiral patterns". Believing instead that "indeed,

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[60] Contopoulos also mentioned it in Basel, reviewing theoretical spiral developments (Contopoulos 1970a).

[61] Commenting on the fact that "the M51 type spirals in Vorontsov-Velyaminov's catalogue all have the companion galaxy or galaxies on the arm", Lynden-Bell well admitted that there "the distortion gravity field of the companion is very important and something on the lines of [Toomre's] work with Julian ought to apply." (Lynden-Bell 1965b)

the corotation of wave pattern and material objects in the outer parts of external galaxies ha[d] been confirmed for M33, M51 and M81 by Shu and his associates (Shu et al 1971)", he favored a picture where M51-type spiral patterns well originated in remotely corotating local 'self-sustained entities' and one of the arms "would join naturally to the intergalactic bridge" (Lin 1971, p.36-37).

> "Owing to resonance, the two-armed structure will prevail as the disturbances propagate inwards as a group of waves, which extracts energy from the basic rotation of the galaxy. [...] The reflection of the waves from the central region then stabilizes the wave pattern into a quasi-stationary form by transmitting the signal, via long-range forces, back to the outer regions where the waves originated. Thus, there is necessarily the coexistence of a very loose spiral structure and a tight spiral structure. Population I objects stand out sharply in the tight pattern while stars with large dispersive motion would primarily participate in the very loose pattern." (Lin 1970, p.383)[62]

Lin's views of spiral structure, generously illustrated at the Basel symposium in the coordinated presentations of his associates (Roberts 1970; Shu 1970a; Yuan 1970), evoked in quite a few of the astronomers a sort of delight imparted so eloquently by Bok in his 'Summary and Outlook'.[63]

> "Until half of a decade ago, most of us in this field were of the opinion that the magnetic fields near the galactic plane [...] would probably have proved sufficiently strong to hold the spiral arms together as magnetic tubes. [...] Theory took a new turn about five years ago, when Lin and Shu entered the field with the density wave theory. [...] The magnificent work of the MIT group loosely headed by C.C. Lin has made the pendulum of interpretation swing toward Bertil Lindblad's gravitational approach, and this is wonderful indeed. [... It] is now in full bloom, but we must not fool ourselves and think that all is done except the mopping up. [...] There is controversy aplenty even within the MIT-Harvard family and this is all to the good.
>
> We are fortunate indeed that the theorists attended our Symposium in force. [...] The observational astronomer is especially pleased to learn about the interest our theoretical colleagues are showing in observations, and it is a source of regret to the observers, optical and radio alike, that we cannot agree as yet on the full outlines of spiral structure for our Galaxy. Give us a few more years, and we shall be able to tell you all right!" (Bok 1970, pp.457-462)[64]

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[62] In one year or so Lin's enthusiasm for this scenario will be tempered. He will concentrate on mechanisms of spiral persistence, and direct his associates' efforts to exploring the feedback cycles. Remote corotation will be as important there as before, but now without reference to the GLB and JT ideas. The main point will be the WKBJ-wave excursions to and from the center, and the associated role of a central bar.

[63] Very instructive is the view of the contemporary spiral progress by Goldreich who after leaving the subject by mid-1960s "remained an interested spectator to the battles between Alar Toomre and C.C. Lin". "Although I generally favored the arguments of the former – he recalls – the latter's campaign was more successful." (*Goldreich*)

[64] Possibly, such a generous support of Lin's initiative by several leading astronomers

4.2 Distinct cautions

> Lin's programme for developing Lindblad's idea into a full theory has up to now led to a theory of waves with neither a convincing dynamical purpose nor a certain cause.
>
> *Lynden-Bell & Kalnajs 1972, p.25*

> All things considered, only cumbersome 'global' mode analyses and/or numerical experiments seem to offer any real hope of completing the task of providing the wave idea of Lindblad and Lin with the kind of firm *deductive* basis that one like to associate with problems of dynamics.
>
> *Toomre 1977, p.452*

Public acknowledgment of the Lin school was quite natural. Its initiative greatly helped in re-orienting astronomers toward active recognition of and observational tests for the gravitational nature and density-wave embodiment of large-scale spiral structure. No sooner had Lin adopted the QSSS hypothesis, he set himself the urgent and essential task of giving it adequate empirical support. The thing demanded a practicable analytical tool, and by 1966 he got it in a facile and handy asymptotic dispersion relation. That it explained neither the origin of spiral structure, nor the cause and mechanism for its tentatively long maintenance may well have worried Lin, but in consort with his original plan he relegated these kinds of topics to the future and rushed straight into empirical testing, having added some heavy claims to his available basics as if adequately backing the grand and quasi-steady spirals. Conveyed by him with the weight of his authority, this played an important part in turning the tide of the battle in his favor... and it affected the intuition, taste and attitude of his audience toward more fundamental aspects of the spiral problem.

Nonetheless, there were presentations at the Basel meeting that alerted its participants to the fact that true understanding of global spiral-making lay far beyond the asymptotic theory they applauded and was bound to take quite a while longer. One of the cautions came from Kalnajs (1970) in connection with his long-term theme of coupled epicyclic oscillations of stars in a thin disk.

Lindblad had introduced and studied the test-star-studded narrow rings – 'dispersion orbits'. Kalnajs (1965) in his thesis examined their gravitational

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of the day partly reflected their desire to see in him a direct follower of Lindblad, their previous indifference to whose efforts might have evoked in them feelings of regret and some guilt. "I do not believe it – Contopoulos comments on this guess. – In particular Bok wanted a simple theory to explain star formation and migration. I remember that when I presented the work of Fujimoto in Prague (1967) and wrote down only two formulae he told me: "Very good George, but too mathematical". A few years later, Bok expressed his disappointment to me, because the density wave theory had become rather complicated. I do not think that Bok appreciated the more formal work of Lindblad." (*Contopoulos*) coupling, first in pairs[65] and then in the whole, already in a continuous disk setting. There he derived an integral equation for his disk's oscillatory dynamics,[66,67] and in Basel he demonstrated a variant of its numerical solution. That was a trailing bar-spiral mode $m = 2$ with an e-fold growth in about two rotational periods of the outer disk (Fig.13a). Kalnajs was pretty sure that his analysis already resolved much of the global spiral-mode problem, and he believed that at least qualitative confrontation with the evidence would prove successful. In this respect he attached particular importance to the fact that his analyzed gas-component reaction to the forming mode showed a tightly wrapped two-armed spiral (Fig.13b). It was, however, far from certain why his main unstable mode could not grow faster and how, even at rather moderate growth rates, it would help one for very long. But

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[65] Kalnajs considered a pair of rings separated by a corotation region. He found that each of them is corresponded by two basic oscillatory modes, one fast and the other slow, and that even in axisymmetrically stable situations the different-type mode coupling creates instability causing an outward angular momentum transfer. Yet on this fact "it would be premature to draw any conclusions about spiral arms of galaxies", he judged (Kalnajs 1965, p.81). "By that time I knew about the shearing sheet results. The two-ring example works even more accurately in this setting. But of course one knows that the sheet is stable and therefore the results inferred from two rings are not the same as that from $2N$ rings – of mass proportional to $1/N$ – when one lets N go to infinity." (*Kalnajs*)

[66] To reduce his complex integral equation, Kalnajs limited its frequency range by specifying angular momentum radial distribution. He took Lindblad's $\Omega - \kappa/2 \cong const$ for the main part of a flat galaxy and the Keplerian $\Omega \cong \kappa$ for its outer part, thus imitating (or implying) an 'edge' in his galactic system. As in the case of paired rings, two modes, slow and fast, grew prevalent, the first one contributing much more. This enabled Kalnajs to describe the modes separately and then account for their coupling by perturbation theory methods. The kernel of the slow-mode equation revealed no pole, it was symmetrical, and the mode stable and devoid of trailing or leading signs. But the kernel of the fast-mode equation had a pole at the OLR associated with the said 'edge'. This changed the qualitative situation: interacting with the OLR, the relatively slowly growing perturbations supported the *trailing* character of the fast mode and, therefore, of the entire spiral wave.

[67] (*Contopoulos*): "Kalnajs' thesis has a correct remark about trailing waves in a particular page. I copied it and asked Toomre whether he could find there the preference of trailing waves, but he couldn't. This convinced me that I should publish my own results."

(*Toomre*): "I have no such memory, but this is in no way to dispute George's own recollection. [...] He always strove to be very fair to Agris as a significant independent worker who had many good ideas and sound mathematics. And so it is entirely plausible that he asked me whether I thought that Agris – then still lacking any true global-mode results that his thesis had been struggling to develop – had really clinched that all realistic spirals must trail. Indeed, I remain pretty sure that Agris by then had not done so . . . but ask him yourself!"

(*Kalnajs*): "Unlike most people who would prefer a physical (or verbal) explanation, George was keen to see the mathematics behind the leading/trailing preference. Fortunately there is a simple enough approximation of the galactic parameters in the vicinity of an OLR whose contribution to the integral equation can be evaluated in closed form. The result is eqn (117) of my thesis. In the subsequent three pages I explained how that contribution to the kernel changes from one that in the absence of a resonance does not favor leading over trailing, to one that prefers trailing waves when a resonance is present. [...] Today I would use a simpler example, perhaps the shearing sheet."

anyway that cast no doubt on Kalnajs' principal result – the strong tendency of a star disk to develop a temporary open two-armed spiral structure, which in turn encourages bar-formation. Thus the Schmidt model of our Galaxy, which was more or less favored in the 1960s by various investigators and which Kalnajs now checked, was seriously unstable and unsatisfactory. If true, this alone would soon overwhelm any 'self-sustained modes' of Lin and Shu peacefully revolving in a disk of stars.

Another caution came in Basel from the evidence provided by numerical experiments. First computer simulations of the flat-galaxy dynamics as the $N-$body problem had been performed in the late 1950s by P.O. Lindblad (1962). He then took about 200 points only, because the early electronic computer was painfully slow on direct calculation of paired interactions for appreciably higher N's. This stimulated new approaches to numerical experiments, and by 1968 Kevin Prendergast and Richard Miller worked out a more effective scheme which, calculating forces in a limited number of cells, allowed rather quick and accurate dynamical description of as much as 10^5particles or so.[68] Inspired with the observation that "because the program is new, new results are coming rapidly" (Prendergast & Miller 1968, p.705), they and William Quirk prepared for Basel a motion picture of "the very interesting physical implications" of their experiments (Miller et al 1970a,b). Spectacular spiral patterns were found to "nearly always develop [already] in the early stages" of their model disks, yet – they argued – these "cannot be valid $N-$body analogues of the spiral patterns of actual galaxies" as most evidently reflecting violent reorganization of the artificially arranged initial state. More importantly, it was ascertained that "machine calculations typically produce 'hot' systems that are largely pressure-supported" (Miller et al 1970b, p.903-4), in contrast to the observed thin disks in galaxies.

The above experimenters found a simple but interesting way out – a 'manual' cooling "by appropriately modifying the systems already in the computer" (Miller et al 1970b, p.904). By integration steps (cycles) they cooled some 10% of their particles, preserving their orbital momentum to imitate their inelastic mutual collisions and make them dynamically akin to interstellar gas clouds. As before, the remaining 90% heated up to the circular-speed-comparable velocity dispersions, but with this a bar was formed and also a trailing pattern of moderate winding that, although chaotic and flexible as it might appear as a whole, contained a bar-bound $m = 2$ spiral wave component (Fig.14). Slowly revolving in the sense of general flow, the bar and the gradually tightening spiral faded from the sight in about three disk rotations.

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[68] Following Miller & Prendergast (1968), particles 'jumped' between discrete-valued locations and velocities under discrete forces. The fast finite Fourier-transform method was used for solving Poisson's equation at each integration step.

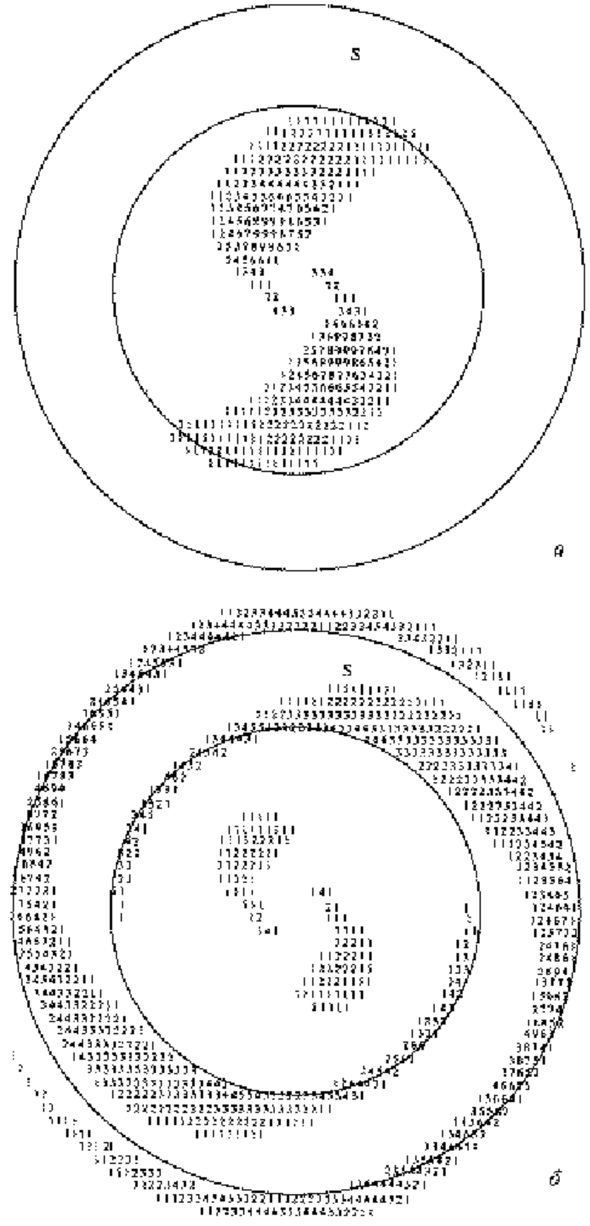

Figure 13: *Kalnajs' growing bar mode: excess densities (a) in the stellar disk, (b) in its gas layer.* Large and small circles mark the outer Lindblad and corotation resonances, point S – Sun's position. (The figure is reproduced from Kalnajs 1970)

Frank Hohl and Roger Hockney worked out a more accurate computational scheme in the late 1960s (Hockney & Hohl 1969). Unlike Prendergast and coworkers, Hohl's interest lay in 'pure' dynamics of collisionless models.[69] In Basel he experimentally confirmed the fact of fast – for a period of one revolution – small-scale fragmentation of cold star disks and its prevention by a massive (no less than four disk masses) spherical halo (Hohl 1970a). The hot disks were checked separately (Hohl 1971) to get stable in Toomre's axisymmetric sense at the initial 'temperature' $Q = 1$, but then they still remained unstable against relatively slowly growing large-scale disturbances that caused the system to assume a very pronounced bar-shaped structure after two rotations (Fig.15);[70] in major features it confirmed the growing bar-spiral mode picture that Kalnajs (1970) had obtained via his integral equation. The total lack of spiral shapes of respectable duration in this and every other purely stellar-dynamical experiment conducted with sizable fractions of 'mobile' mass was a result that almost spoke for itself.[71] Indeed, just like the modal work of Kalnajs and the N-body results of Prendergast et alia, it cautioned everyone at Basel that this strong tendency toward bar-making very much needed to be understood and tamed lest it overwhelm the QSSS hopes of Lin and all his admirers.

Afterword

By the beginning of the 1970s the spiral subject was in considerable disarray. The still popular QSSS hypothesis of Lin and Shu, along with their illustrative semi-empirical theory, was confronted with serious difficulties. Lin and his associates were put clearly on the defensive over their tightly wrapped (quasi)-steady modes on two principal fronts: from the radial propagation at the group velocity that would tend to wind them almost at the material rate, and from the tendencies of galaxy disks toward a strong global instability that appeared likely to overwhelm them. Of course, one might claim that all such threats were just imaginary and temporary, and only of academic interest, on the ground that nature itself had overcome them (as

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[69] To avoid computational artifacts, Hohl had carefully examined properties of his numerical schemes and showed that his $N-$body models were indeed collisionless (Hohl 1973) and their behavior was independent of the particle number, cell size and integration time step (Hohl 1970b).

[70] In two more rotations, a nearly axisymmetric distribution of stars around a massive central oval resulted, revolving about half as fast as the initial disk.

[71] "It is conceivable, of course, that some milder instabilities which might themselves have led to more enduring spirals, were thwarted in these experiments by a kind of overheating from the fierce initial behavior. This seems unlikely, however, because of Hohl's extra tests with that artificial cooling (Hohl 1971)." (Toomre 1977, p.468)

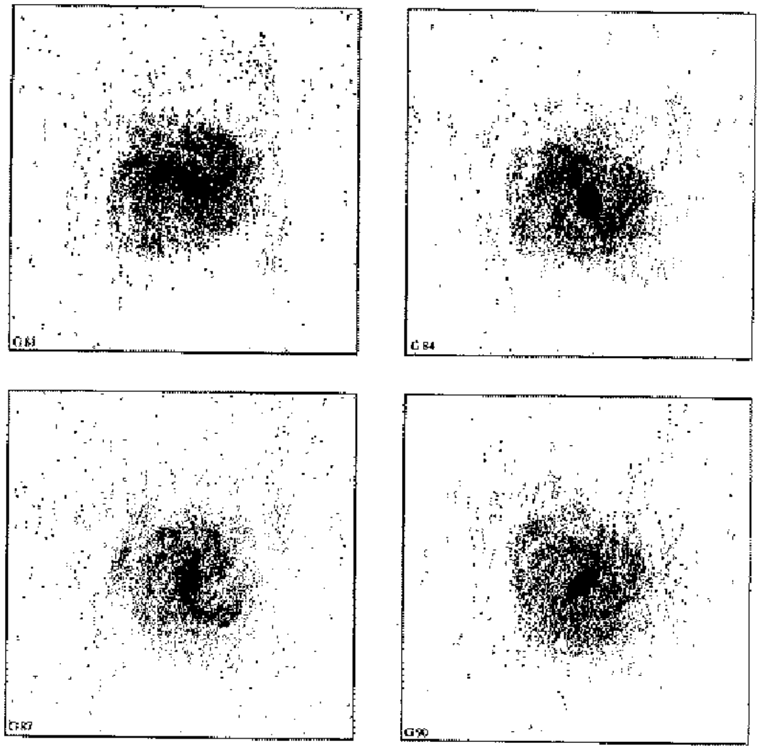
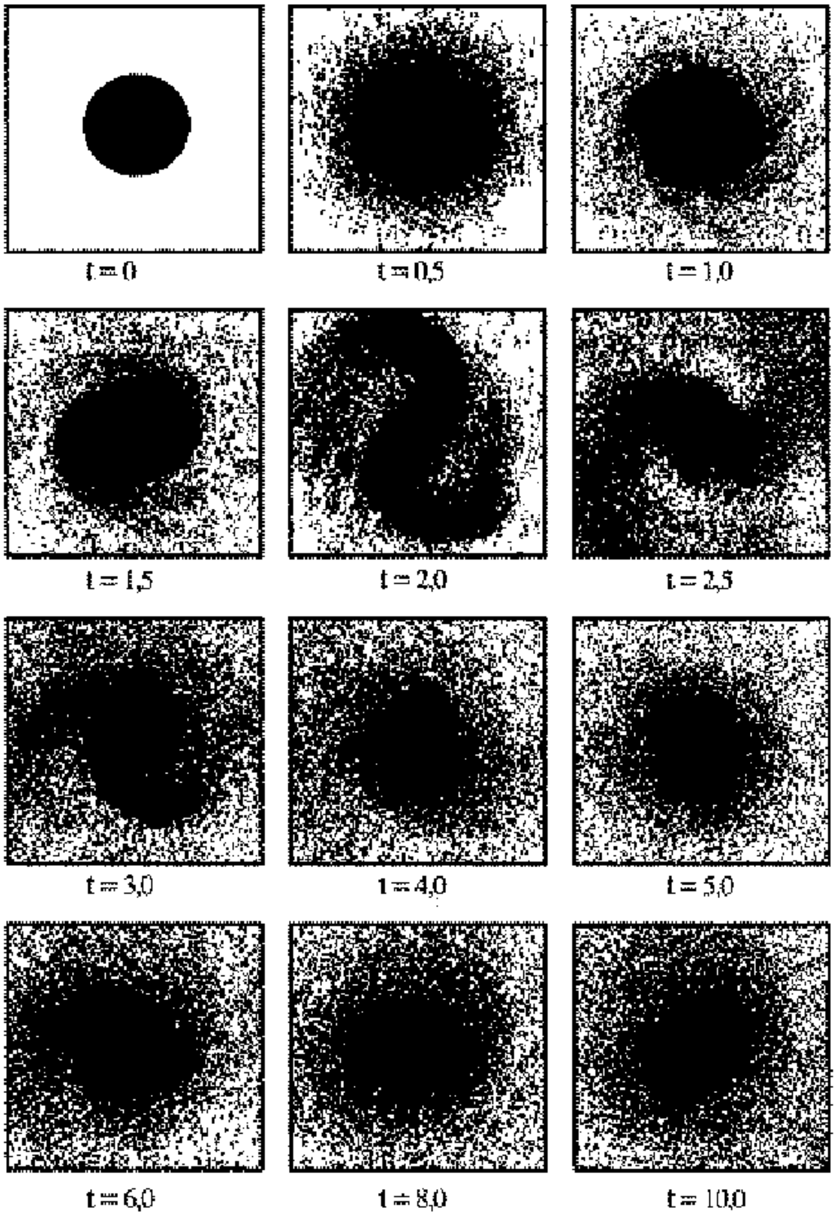


Figure 14: *The formation and evolution of the bar-spiral structure in a partially cooled gravitating disk.* (The frames are reproduced from Miller et al 1970)

say for the case of the bar-making instability of stellar disks, the rescue from which was actively sought in the 1970s in a massive inert halo that in fact was not needed). One might also be confident that the QSSS hypothesis must be correct, as illuminated by the everlasting truth of Hubble's classification of the galactic morphologies. One might even take pride in the historical fact that an interesting and very promising concept developed, although not connected to the wave steadiness, on spiral shocks in interstellar gas and their induced star formation. But such a heuristic approach did not stimulate very strong progress in understanding dynamical principles of the spiral phenomenon; moreover, it often misled, and a rich irony was already that the supposed QSSS favorites M51 and M81 (Lin held originally that a large majority of the galaxies – 70% – "are normal spirals like the whirlpool" (Lin 1966a, p.877)) turned out most probably not to be quasi-steady at all. A further irony was the continuing failure of Lin and Shu to account the trailing character of their 'modes', while that was already grasped by their direct 'deductive' opponents. But the greatest irony lay in the fact that the concept later known as swing amplification, worked out by the mid-1960s, was originally denigrated by Lin's camp as relating exclusively to 'material arms', whereas it turned out in the end to be of vital importance to this en-



t = 0 t = 0,5 t = 1,0

t = 1,5 t = 2,0 t = 2,5

t = 3,0 t = 4,0 t = 5,0

t = 6,0 t = 8,0 t = 10,0

Figure 15: *The evolution of a stellar disk from an initially balanced state of uniform rotation and marginal stability* $Q = 1$. Time is in initial rotation period units. (The figure is reproduced from Hohl 1971)

tire spiral enterprise including the variants of chaotic ragged patterns, tidal transient grand designs and growing or quasi-steady modes.

The 1970s that came promised many interesting events in the spiral arena, because – here we repeat what we said in the beginning of the paper and with it close our narrative – by that time it had become very clear to everyone that much hard work still remained to explain even the persistence, much less the dynamical origins, of the variety of spirals that we observe.

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