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

I. I. Pasha[1]

Sternberg State Astronomical Institute, Moscow
Vavilov Institute for History of Science and Technology, Moscow

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[1] *email*: misterwye@mtu-net.ru

> You men are strange people – said Amaranta, unable to think up anything else. – All your life you fight against priests, but give prayer-books.
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> *G.G. Marquez. A hundred years of solitude*

> . . . a common foible of those who in the feeling of devotion are disposed to exaggerate the significance of their heroes.
>
> *Einstein 1953*

Introduction[2]

The modern density-wave theory of spiral structure in galaxies, sprung in the 1960s, had long been preceded by the theories of Bertil Lindblad. Those started back in the days when Hubble demonstrated that whirlpool nebulae reside far outside the Galaxy, and when Jeans conveyed an engrossing feeling of steady spirals ordered by yet unknown forces.[3] Astronomer by education, Lindblad did not yield to temptation by this imposing obscurity of force, and he headed for a dynamical spiral theory in terms of ordinary gravitation.[4] Right then, this task must have appeared extremely difficult, to be at best a matter of a lifetime of work, since the analytical methods of the patronizing disciplines (hydrodynamics, statistical mechanics) were rudimentary and gave almost nothing for the stellar-dynamical research. Still more striking was Lindblad's break-through in the field of stellar kinematics. By 1927 already he developed the theory of epicycles, having shown that a star moving on a nearly circular galactic orbit just oscillates about its mean radius (Lindblad 1926b). The frequency κ of such oscillations was given by the relations

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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.

[3] "Each failure to explain the spiral arms makes it more and more difficult to resist a suspicion that the spiral nebulae are the seat of types of forces entirely unknown to us, forces which may possibly express novel and unsuspected metric properties of space. The type of conjecture which presents itself, somewhat insistently, is that the centres of the nebulae are of the nature of 'singular points', at which matter is poured into our universe from some other, and entirely extraneous, spatial dimension, so that, to a denizen of our universe, they appear as points at which matter is being continually created" (Jeans 1929, p.360).

[4] Polemizing with Jeans on the spiral problem, Brown, a celestial mechanician from Yale University, defended already its gravitational status. In his mind, star orbits might at certain conditions correlate in shape and orientation so as to reveal a two-armed spiral-like envelope, thus delineating a "visible structure [. . .] due to the greater space density of visible matter in the neighborhood of the arms than elsewhere", i.e. a *stationary wave of condensation* (Brown 1925, p.109-10). Noticed though (Jeans 1929; Lindblad 1927c), Brown's work had no perceptible impact.

$$\kappa/2\Omega = (1 - A/\Omega)^{1/2} = c_\theta/c_r \qquad (1)$$

including the angular speed Ω, the Oort constant of differential rotation $A \equiv -{}^1\!/\!_2 r d\Omega/dr$, and the azimuthal-to-radial velocity dispersion ratio (Lindblad 1927b); the values of c_θ/c_r got remarkably close as calculated and empirically determined for the solar neighborhood (Lindblad 1929). These results reinforced the stellar-dynamical foundations and also they gave Lindblad confidence in his search of the origins and mechanisms of the galactic spiral phenomenon, but, quickly recognized and instigated by success, he was taken hostage, then and on, to the epicyclic-orbit scheme.

I. LINDBLAD'S ERA

> The only result that seems to emerge with some clearness is that the spiral arms are permanent features of the nebulae [...] perpetuated in static form.
>
> *Jeans 1929, p.360*

1.1 From unstable orbits to global wave modes

> It is natural that in this field, on which at that time nothing was ripe for harvesting, he did not immediately find the right path.
>
> *Oort 1967, p.333*

Though the fact of our larger-scale universe had begun to emerge through Hubble's work, it was not yet as clear on the quantitative side: well advanced in rank, the 'nebulae' still came short of size and mass against our Galaxy. This was made by the underrated galaxy-distance scale,[5] and the giant ellipticals, missing in the Local Group and nearby, got it the most. On the whole, the ellipticals were found to be one to two orders under the spirals, and the rather enigmatic barred galaxies were ranged somewhere intermediate (Hubble 1936).

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[5] It was not until the early 1950s that the distance scale was reconsidered (see Baade 1963, Efremov 1989) and the size of the Local Group doubled. Given the shifted zero-point in the Cepheid-luminosity calibration, Hubble's constant was reduced, and by the 1960s it fell from its original 550 *km/s/Mpc* down to 180 (de Vaucouleurs) or to 80 (Sandage). This gave a 3-to-7-fold increase in distance.

Original absorption-spectrum methods of detecting the galaxy rotation were sensitive only for bright central regions of comparatively close systems, the line inclination being established integrally, as a quantitative measure of overall uniform rotation. The emission-spectrum methods, in practice since the late 1930s, could as well catch the kinematics of the rather distant regions in our next-door spirals M31 and M33 (Babcock 1939, Mayall & Aller 1942). Limited and inaccurate though these data were (Fig.1), they took astronomers by storm and for almost two decades then they formed and served the idea of a standard rotation curve. The latter was understandably professed to obey $V(r) = ar/(1+br^2)$ and be scaled so as to co-measure its rising part to a live galaxy within its 'visible boundary'.[6,7] And on the barred spirals it was disarmingly clear "with no measurement" at all that in face of rapid bar destruction their rotation was nothing, if not uniform (Ogorodnikov 1958, p.517).

Genuinely matched with the empirical climate were the theoretical tastes of the epoch that followed closely Jeans' directive on unified cosmogony of galaxies and stars.[8] One relied on the study of gaseous figures; they were diagnosed to be open to evolutive secular instability created by dissipation factors acting in the steady-motion systems. The latter just "never attain to a configuration in which ordinary [dynamical] instability comes into operation" (Jeans 1929, p.199), so that "it is secular stability alone which is of interest in cosmogony" (Jeans 1929, p.214)[9] . Quite understandably, Lindblad's early work lay nearby in the feeling for global evolutionary processes.[10] Yet he was

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[6] This form of $V(r)$ emerged from the solution of Jeans' problem for an axisymmetric stationary stellar system with ellipsoidal velocity distribution. It greatly encouraged work on modeling the three-dimensional gravitational potential and mass distribution in the Galaxy (Parenago 1950, 1952; Kuzmin 1952; Safronov 1952; Idlis 1957).

[7] "Both in M31 and M33 the easily visible spiral arms lie in regions where the rotation does not deviate strongly from uniformity. It is remarkable in M31 that outside the nucleus [...] there is another region of nearly uniform rotation" (Weizsacker 1951, p.179). Vorontsov-Velyaminov (1972) was still confident that near uniform rotation was the type adopted by most of spiral galaxies.

[8] The idea of an overall one-time star formation early in the life of our Galaxy had long been predominant. In the late 1930s only the hydrogen-to-helium-synthesis energy source was proposed. That allowed evaluation of the fuel exhaustion time at a given star luminosity, and its shortness for the blue supergiants – 10^7 yrs – exhibited star formation as an ongoing process. This idea gained empirical support during the 1940s.

[9] In Jeans' view (Jeans 1929, p.213), as a nebula in uniform rotation shrinks, it alters (augments) density, not angular momentum, running through a one-parameter sequence of equilibrium figures. Remarkably, this same sequence is followed by a non-compressible liquid body as it enhances its momentum. According to Poincare, this body is secularly stable till it is a low-flattening Maclaurin spheroid. But when some critical eccentricity (momentum) is reached, it looses stability, takes another sequence of stable equilibrium figures – Jacobi ellipsoids – and then follows it at speedier rotations.

[10] "Now it is obvious from the scheme as Hubble described it that he had an impression or a belief, although he never quite admitted it, that it represented a continuous sequence. But I believe, on the contrary, that Lindblad put his finger on the essence of Hubble's

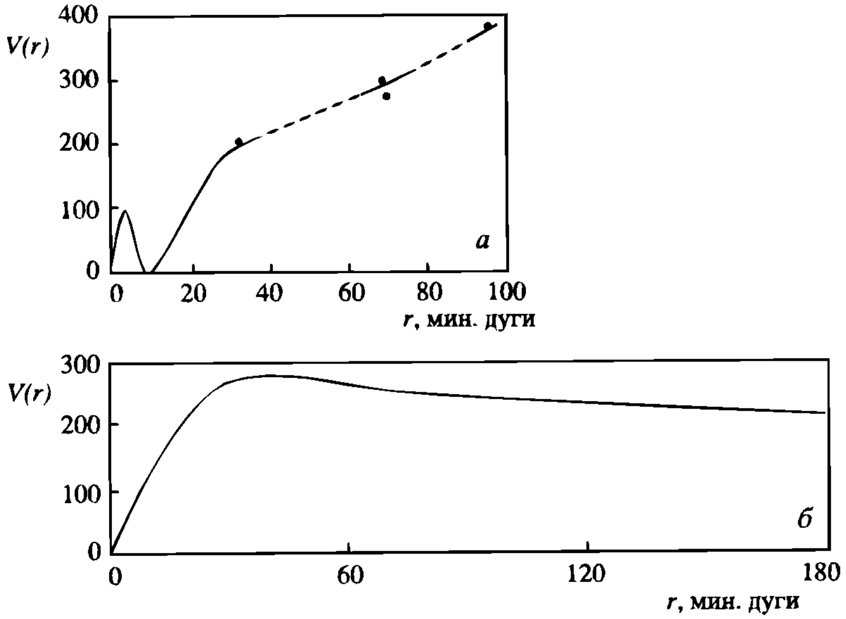

Figure 1: *The rotation curve of the galaxy M31*: *a* – as provided by the late-1930s optical data (Babcock 1939), *b* – as inferred from the mid-1950s radio data (Hulst et al 1957).

the first, and for more than thirty years almost the only one, who singled out the spiral problem and treated it as a separate, *stellar-dynamical* element in the general philosophy of galaxies.[11]

Lindblad started from a highly flattened lens of stars in uniform rotation ($\Omega = const, A = 0$ in Eqn (1)) created in the course of primary evolution (Lindblad 1926a, 1927a). Gravitational potential at its edge changes so abruptly with radius that circular orbits there get unstable ($\kappa^2 < 0$): those inside of, but close to, the edge need only a slight individual change in energy in order to be transformed into quasi-asymptotic orbits extending very far from the 'mother system' (the solar neighborhood belongs exactly to some such exterior that shows differential rotation obeying relations (1)). Still stars leave and return to their mother system spontaneously and equiproba-

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classification when he suggested that it is a series of increasing flattening, or increasing angular momentum" (Baade 1963, p.16-17).

"According to Lindblad's theory, the fully resolved spiral pattern is regarded as an advanced state which all nebulae will eventually reach in the course of their evolution" (Chandrasekhar 1942, p.180).

[11] The trend of this philosophy is sensed through the following reflection by Weizsacker (1951, p.165): "The evolution of a single object can be understood only if its temporal and spatial boundary conditions and the external forces acting on it are known. These are defined by the evolution of the larger system of which the object forms a part. So every single problem is likely to lead us back into the problem of the history of the universe".

bly in any point on its edge, which is not conducive to neat global patterns. But the hitch is removed upon the admission of either an outside disturber or an overall oval distortion caused by fast rotation.[12] In both cases, two opposite ejection points arise on the edge of the lens after a transitory process and, fixed in space, they pour material out in spiral-looking leading gushes. Turning to *intrinsic* mechanisms of galaxy structures, Lindblad laid greatest stress upon global modes of disturbances, called the deformation waves ('uncompressible' modes) and the *density waves* ('compressible' modes), and sought their unstable solutions (Fig.2).[13] Analyzing the effects such waves had on stars on asymptotic orbits (Fig.3), he proposed and refined scenarios of spiral-arm formation in an outer, shearing galaxy envisaged to keep up somehow the patterns as arranged by a mass of the affected orbits, rather than to destroy them (Lindblad 1927a, 1948, 1953).[14,15]

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[12] Circular orbits at the spheroidal edge are unstable for eccentricities $e_1 > 0.834$, and as the level $e_2 = 0.953$ is achieved (3.1:1 axis ratio), dynamical instability against the two-crest harmonic sectorial waves is thrown in, so that the figure gets oval.

[13] "The most important modes of density variation" appear to be of the type of $\sim (r/R)^m \cos(\omega t - m\theta)$ (ω and m being wave frequency and azimuthal wavenumber, R – the lens radius). "The conditions for instability have been investigated for the waves m =1, 2, 3. The greatest interest attaches to the wave m =2 because it tends to explain the formation of barred spirals. The density variation is accompanied by the development of four whorl motions. [...] The disturbances due to the four whorls on the motions in a surrounding ring structure [the latter thought of as having been formed previously] explain in a qualitative way the development of spiral structure" (Lindblad 1962, p.147).

[14] These articles provide a reasonable summary of Lindblad's theories prior to 1955. The asymptotic-spiral theory was thoroughly reviewed by Chandrasekhar (1942), and the wave-mode theory by Zonn & Rudnicki (1957). See also (Lindblad 1962; Contopoulos 1972; Toomre 1977, 1996; Pasha 2000).

[15] In Lindblad's bar-mode theory as it had progressed by the early 1950s (Lindblad & Langebartel 1953), three factors serve for the spiral formation. The first is the tendency for the formation of the rings, one at the galaxy center and one (or several) more in the distance, the bar occupying the inter-ring region. The second factor is the development of two diametrically opposed zones of enhanced density (see Fig.2). The third one is the increased centrifugal (radial) motion in these zones. If the bar-forming processes affect the galaxy kinematics but weakly, then the motions of distant material lag behind that of the main galactic body, and as the existing radial motions make the outer ring deform and break up, it forms the main spiral arms (I and II in Fig.3). Also, the effects of the bar wave show that material at the bar 'tips' has some extra rotation, so that, helped by the radial motions, it forms the inner spiral arms (VI in Fig.3). If the galactic angular momentum is above some certain level, the density wave can give no bar, and the deviations from axial symmetry it causes produce the appearance of ordinary spiral structure.

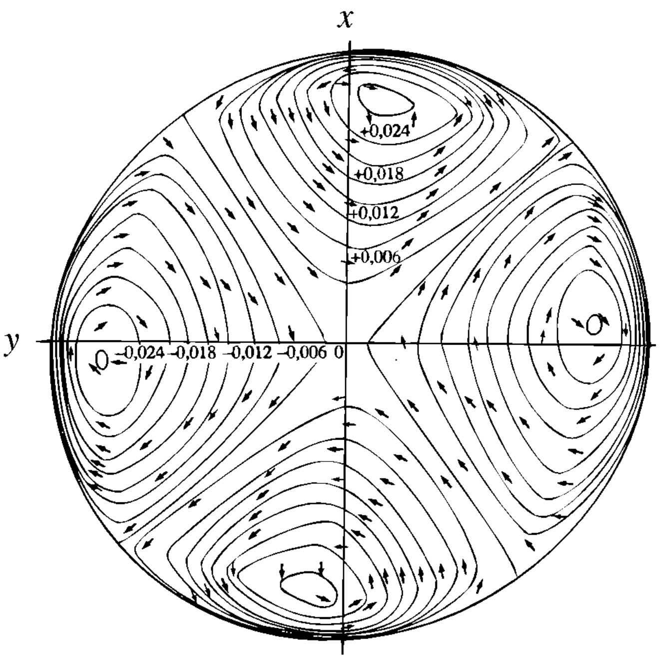

Figure 2: *The $m = 2$ wave mode in Lindblad's bar-spiral density wave theory.* Two wave maxima and minima are placed along the x and y axes, respectfully. These bisymmetrically located maxima and some extra concentration at a galaxy center are to explain the bar phenomenon. The arrows show systematic noncircular motions. (The figure is reproduced from Lindblad & Langebartel 1953)

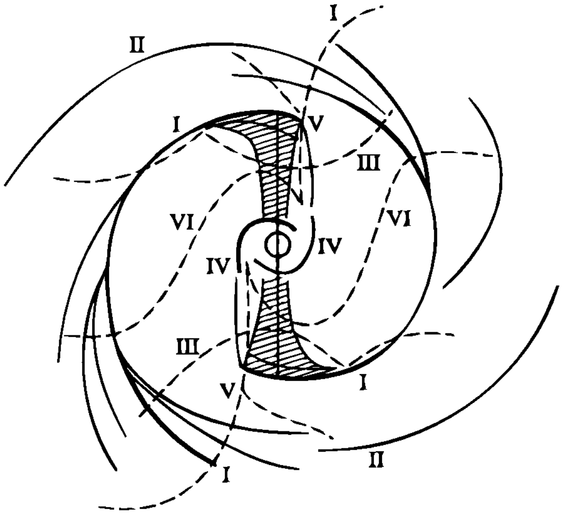

Figure 3: *The formation of spiral structure as envisaged in Lindblad's bar-spiral density wave theory.* (The figure is reproduced from Lindblad & Langebartel 1953)

1.2 Gas and dust

> The difficulty of cosmogonical theories lies in the interconnection of the facts.
>
> *Weizsacker 1951, p.165*

> Where a few years ago we seemed to be up against a blank wall of discouragement, we are now in an era of rapidly developing research.
>
> *Bok & Bok 1957, p.244*

Stellar dynamics of the 1940s - early 1950s was essentially the theory of a stationary galaxy arranged by the regular forces (see Ogorodnikov 1958) and the theory of quasi-stationary systems open to slow relaxation processes (Ambartsumian 1938; Chandrasekhar 1942, 1943). Together, they provided a basis serving well for getting certain practical dividends but still of little use for conceiving the underlying dynamical problems.

> "While these methods have contributed substantially toward the clarification of the peculiarly characteristic aspects of stellar dynamics, an impartial survey of the ground already traversed suggests that we are perhaps still very far from having constructed an adequate theoretical framework in which the physical problems can be discussed satisfactorily. In any case we can expect that the near future will see the initiation of further methods of attack on the problems of stellar dynamics" (Chandrasekhar 1942, p. vii-viii).[16]

The envisaged future did not happen to lie as immediately near, however. The theoretical thought kept on whirling around the idea of galaxies evolutionarily tracking over the Hubble diagram, one way or the other, and that opened in quite a few attempts at a synthesis of the available strict knowledge about gravitating figures in a softer (then bulkier) spirit of cosmogonical inclusion.[17] Accordingly, non-stationary – dynamical – problems of deformation of the systems and of density disturbances in them seemed difficult

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[16] "I remember very vividly the atmosphere in the 50's in stellar dynamics. On the one hand, we had the most general solutions of Liouville's equation by Chandrasekhar. But it was realized that the self-consistent problem required also the solution of Poisson's equation, which was very difficult in general. Thus people were discouraged." (*Contopoulos*)

[17] See, e.g., the "Critical review of cosmogonical theories prevailing in West Europe and America" by Schatzman (1954). It would be some fuller with an addendum on a theory developed in 1955-56, now in the Soviet Union, by Ogorodnikov. Finding that the works by Lindblad and Chandrasekhar on collisionless dynamics "really bar the way to studying the laws of evolution of stellar systems", he suggested a "more promising" – "synthetic" – hydrodynamical method with elements of statistical mechanics (Ogorodnikov 1958, p.20, 22), and with this he proved theorems on uniform rotation and nearly constant density for "dynamically determinable" systems, at their "most probable phase distribution". This enabled Ogorodnikov to start his supposed evolutionary sequence with the 'needle-shaped' galaxies, or strongly elongated ellipsoids in rotation about their shortest axis. Such needles are secularly unstable, above all at their long-axis extremities from where "the stars are detached in two winding arms" giving the picture of a typical barred spiral galaxy.

and therefore premature, while stationary problems were held as "natural and necessary" at that preliminary point, for "it is hard to imagine that at all stages the evolution of stellar systems has the violently catastrophic character" (Ogorodnikov 1958, p.13).[18] In this illumination, Lindblad's theory of unstable bar-modes was typically deemed extravagant and unacceptable (Lebedinski 1954, p. 31).

> "Such theories cannot yet help the progress of cosmogony, since uncertainty in them still prevails validity" (Schatzman 1954, p.279).

The delicacy of this sort of expert judgment – let alone its other virtues – reflected clearly that it was the issue of gas and dust that became a common focus of galaxy astronomy despite its stellar past.[19] By the 1950s, Baade discovered in M31 many hundreds of emission nebulosities (HII regions), having

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Material released during this gradual bar destruction feeds a spherical halo, while inside the bar a violent process of low-velocity-dispersion star formation starts, and these emerging Population I stars uniformly fill the new equilibrium figure – a thin disk-like Maclaurin spheroid. The remaining diffuse material of the bar (needle) winds up and, being still 'frozen' in the disk, forms spiral arms. Due to irregular forces, Population I and II stars get mixed, because of which the spiral galaxy cannot be in equilibrium: its disk dies out through dissipation, and a nuclear remainder drives up an eventual elliptical galaxy (Ogorodnikov 1958, p.29).

As well illustrative appears Weizsacker's theory of galaxies and stars built on a concept of supersonic turbulent motion in the original gaseous mass, the one picturing a general "evolutionary trend as far as it does not depend on the special conditions by which galaxies, intragalactic clouds, stars, planets, etc., are distinguished". The theorist understands the rapid flattening of that gaseous mass (in about one period of rotation) as due to the decay of its original turbulence, and he reduces its further evolution to some secular changes followed by a slow loss of the axial rotation of the galactic systems. In this way, galaxies of the type of the Magellanic Clouds or the M31 companions are to be obviously younger than the universe, and "elliptic galaxies are in a final stage which no longer shows the sort of evolution we consider". "Thus the large galaxies like our own can be as old as the universe, without having yet reached their final stage", the spiral structure being their "most conspicuous semiregular pattern". Weizsacker's judgment on it is twofold. He finds himself in a position to "try to understand spiral structure as a hydrodynamical effect [...] produced by nonuniform rotation", noticing that any local formation – "cloud formed by the turbulence" – will then be distorted into a segment of a spiral. On the other hand, he admits that "the abundance of systems with just two spiral arms is probably caused not by turbulence but by gravitation", which is in fair correlation with the presence of a bar. The bar is understood as an elongated equilibrium figure of rotation similar to Jacobi's liquid ellipsoids; it "can be kinematically stable only if the system rotates uniformly", i.e. in inner galactic regions. But just a little way out, the shearing effect of differential rotation comes into play, in order "not to destroy the 'bar' entirely but to distort it strongly", giving it some spiral contours (Weizsacker 1951, p.176-179).

[18] Zwicky reflected on the 'cooperative' effects in gravitating systems (both in stars and galaxy clusters) since the mid-1930s, and he believed that whereas the nuclei of spiral galaxies had already reached their equilibrium the spiral arms and interarm regions were still "transitory configurations" (Zwicky 1957, p.214). He thus did not treat the spiral structure from the natural, for collective phenomena, viewpoint of oscillations and waves in equilibrium media.

[19] "Why do the spirals always show the combination of a disk and a central spheroidal system? It must reflect the original density distribution in gas. [...] Can we imagine

concluded that “they are strung out like pearls along the arms” (Baade 1963, p.63). Gas and dust, he stated, are also distributed in this galaxy highly unevenly, grouping in its spiral arms.[20] Besides, no one already doubted the youth of high-luminosity stars since they were ascertained to still form in abundance, e.g. in the Orion nebula. The sheer weight of these individually weak facts convinced many workers that

> “the primary phenomenon in the spiral structure is the dust and gas, and that we could forget about the vain attempts at explaining spiral structure by particle dynamics. It must be understood in terms of gas dynamics and magnetic fields” (Baade 1963, p.67).[21]

The lion’s share of these discoveries was made possible due to the 200-inch Palomar reflector put into operation in 1949, although from 1951 onwards the interstellar gas was unprecedentedly attacked also by the 21-cm-line methods. Dutch radio astronomers presented “one of the truly historic diagrams of Milky Way research” (Bok & Bok 1957, p.244) – a detailed map of atomic hydrogen distribution (Hulst et al 1954).[22] It displayed extended fragments of tightly-wrapped spiral arms which in the solar vicinity matched ‘local arms’ in Sagittarius, Orion and Perseus.[23] Gas kinematics routinely analyzed, a synthesized rotation curve of the Galaxy was pictured (Kwee et al 1953), and the “primary task for the next few years” was claimed to get improved radio equipment “capable of tracing with precision the spiral structure of our Galaxy”.

> “While there is always room for theorizing, the emphasis must first of all be on careful observation and unbiased analysis of observations""(Bok & Bok 1957, p.248).

The new empirical facts – the tightly wrapped, nearly ring-like arms of the Milky-Way spiral, the concentration in them of Population I objects,

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that at some era in the past, the central spheroidal system of low rotation and the disk with very fast rotation actually resembled the equilibrium figure of the gas? One should really look into these things” (Baade 1963, p.17).

“The origin of the spiral systems is an unsolved problem as yet. Doubtless the interstellar material plays a major part in it. Therefore the methods [of stellar dynamics . . .] seem to be insufficient for a solution” (Kurth 1957, p.146).

[20] This was inferred from the lack of reddening of globular clusters in M31, one half of which lie behind the galaxy disk because of their spherical distribution. As Baade wrote (1963, p.70), initially one did not believe in this finding, since the gas layer in our own Galaxy was still held to be uniform.

[21] Baade has usually been quoted from his posthumous monograph (Baade 1963). It reproduces his 1958 lectures that vividly transmit the mid-century atmosphere in extragalactic astronomy. Many investigators of the time claimed to have agreed with Baade on the basic role of gas in the spiral arrangement (e.g., Weizsacker 1951, p.178).

[22] In 1958 this map was completed with the spiral fragments observed from Australia (Oort et al 1958).

[23] They were inferred in 1951 from data on the distribution of O-B associations and HII regions (Morgan et al 1952; see Gingerich 1985).

the *general* shearing character of rotation – were a surprise to Lindblad. He could not neglect them. But they demanded another, more fitting dynamical theory, and Lindblad put aside (but did not deny[24]) his business with unstable circular orbits and wave bar-modes. This step was largely favored by first numerical experiments in galaxy dynamics performed in 1955-60 by his son P.O. Lindblad with the big electronic computing machine installed in Stockholm (Lindblad & Lindblad 1958; P.O. Lindblad 1962). Those experiments showed the *trailing* – not the leading – spiral arms, the ones supported by fresh data on both the form of the Milky-Way spiral and the space orientation of many galaxies (Vaucouleurs 1958), and, after all, the ones put into orbit way back by Hubble (1943) in the framework of his working hypothesis that galactic spirals *always* trail.[25]

1.3 Winds of change

> The spiral structure is nothing more than a tracer element contained in a fairly uniform disk of material [...] This is probably related to the magnetic field in the disk.
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> *G. R. Burbidge 1962, p.295*

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[24] Via such shifts of opinion, Lindblad found himself on the way towards "a more definite theory" (Lindblad 1962b, p.148). There he might well be judged (Toomre 1977, p.439) as if even having finally conceded that his old leading-arm models were "not reconcilable with modern evidence" (Lindblad 1962b, p.146). Yet he blamed that on some other "early gravitational theories which interpret spiral structure as due to orbital motions of stars starting from a small nucleus" (Lindblad 1962b, p.146).

[25] Having completed by the 1930s his theory of asymptotic leading spirals, Lindblad (1934) turned to the empirical component of the problem of the 'sense of rotation' of spiral arms. The difficulty was with determining the near and the far sides of a galaxy, as this might be made no other than by way of speculation on the asymmetry of dust absorption along the minor axis of the visible image. There were at the time no reliable data on interstellar dust properties. To Lindblad's way of thinking, a stronger absorption was felt by a farther side (thought also to show sprinkles of dust veins in the bulge region), which maintained leading arms. After a categorical objection by Hubble (1943), he scrutinized the subject anew in his fundamental work with Brahde (Lindblad & Brahde 1946) followed by a succession of smaller articles during a decade or so. To criticize Lindblad for his leading-arm orientation was a commonplace. One agreed with him (and, evidently, with Hubble) in that the sense of spiral winding must be the same for all galaxies, which demanded only one good example of a nearly edge-on galaxy that might be clearly judged on both its spiral form and nearer side. Vaucouleurs (1958) gave such an example as got a high-quality long-exposure photograph of NGC 7331 taken with the 200-inch reflector. It favored Hubble's camp. Lindblad must have reserved objections on how the spiral form was to be inferred from that crucial case (he and his collaborators Elvius and Jensen had been studying this galaxy photometrically in several papers from 1941 to 1959, and he gave a rather incomplete summary on the topic in Lindblad 1962a), but for the absolute majority of astronomers the empirical component of the sense-of-winding problem was no longer acute.

> As far as I am aware, no single problem, not even a stability problem, has been solved in a differentially rotating self-gravitating medium. Even without magnetic fields, and even linearizing the equations, it is very hard to make progress.
>
> *Prendergast 1962, p.318*

> With our observations we have reached a point where we are simply unable to draw any definite conclusion, unless the theory helps us. I hope some day there will be action, because otherwise we are lost.
>
> *Baade 1963, p.266*

The post-war success in galaxy research gave priority to the empirical approach. By the late 1950s, it formed two flanks of evolutionary studies, morphological and quantitative. The first one, due mostly to the Palomar sky survey, called for elaborate classifications, catalogs and atlases of galaxies (Zwicky 1957; Morgan & Mayall 1957, de Vaucouleurs 1959; Vorontsov-Velyaminov 1959; Sandage 1961); the second exploited matters concerning stellar evolution and empirical data on individual galactic objects. As regards the theoretical approach, it too branched under the new conditions and its subject was now treated in distinct frames of physical, chemical and dynamical evolution.

On this dynamical side, the one to our present interest, true lodestars started shining by the 1960s. One of them was lit by the linear stability theory as applied to long-range force systems; denied so far, mostly by human inertia, its methods eventually penetrated into the galaxy dynamics.[26] Chandrasekhar (1953, p. 667) formulated the problem as follows:

> "When we know that an object has existed in nearly the same state for a long time we generally infer that it is stable; and by this we mean that there is something in its construction and in its constitution which enables it to withstand small perturbations to which any system in Nature must be subject. [...] Thus when we are confronted with a novel object – and most astronomical objects are novel – a study of its stability may provide a basis for a first comprehension".

To him, however, it was a matter of pure intellectual interest, above all. "For an applied mathematician, Chandrasekhar explained, problems of stability present a particular attraction: by their very nature, these problems lead to linear equations and linear equations are always more pleasant to deal

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[26] "I cannot agree that plasma physics methods penetrated in astronomy in the 50's. Of course these developments helped each other, mainly in the 60's, but this is natural. I think that in the 50's progress was sporadic, due to the insight of only a few people, but later many people followed the first pioneers". (*Contopoulos*)

with than nonlinear ones" (Chandrasekhar 1953, p.667).[27] In so thinking, he turned to most general, technically transparent models. One of such was Jeans' infinite homogeneous medium asked about whether the classical stability criterion $k^2c^2 - 4\pi G\rho > 0$ and the critical fragmentation scale $\lambda_J = (\pi c^2/G\rho)^{1/2}$ remain unchanged if the medium is involved in uniform rotation (and ρ are sound speed and material volume density; k, ω and $\lambda = 2\pi/k$ – wave number, frequency and length; G–gravity constant).[28] The answer came positive, with the one exception for perturbations propagating in the direction just at right angles to the rotation axis, when Coriolis force co-governs wave dynamics and modifies the dispersion relation into

$$\omega^2 = 4\Omega^2 - 4\pi G\rho + k^2c^2 \tag{2}$$

showing that any rotation with $\Omega > (\pi G\rho)^{1/2}$ entirely prevents the system from decay.

Safronov (1960a,b), interested in protoplanetary cloud dynamics as a part of his solar-system cosmogony, examined a more realistic model – a differentially rotating gas layer stratified along the rotation axis.[29] A short-

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[27] Particularly, this was the line in which the unified theory of ellipsoidal equilibrium figures was being developed later (Chandrasekhar 1969). "There was criticism by astronomers of Chandrasekhar's work on the classical ellipsoids because of its remoteness from the current needs of astronomy. Chandra's interest (and my own as well) was indeed motivated by non-astronomical considerations. What we found was a development by some of the great mathematicians of the 19th and early 20th century that had largely been forgotten, and in some mathematical respects was left incomplete. Chandra felt strongly that his work should, on general intellectual grounds, be completed. If that completion should have application in astronomy, so much the better, but that was not the motivation. His critics in astronomy were offended because he was not doing astronomy. Chandra, however, was more devoted to science (or his view of it) than to astronomy, and did not feel obligated to work on problems which were chosen for him by astronomers". (*Lebovitz*)

[28] "I do remember that at the time I wrote the paper, the spiral structure of the galaxies was not even remotely in my mind. Besides my paper was concerned with the Jeans instability of a gaseous medium and not to a system of stars... However, I am quite willing to believe that the basic ideas were included in earlier papers by Lindblad". (*Chandrasekhar*)

[29] Ledoux (1951), interested in the formation of planets from a primordial cloud, seems to have been the first to consider the stability of flat gravitating systems. He, as well as Kuiper who had turned him to this problem, suspected a change in the critical Jeans scale, realizing that an assumed cloud mass of about 10% that of the Sun would be enough for the cloud to act significantly on itself in the plane of symmetry. Ledoux found that for small adiabatic disturbances to the equilibrium state of an isothermal non-rotating layer Jeans' criterion remains unaltered if ρ is taken to be half the density value at $z = 0$. This did give only a correction to the clumping scale, which was of order 2π times the thickness. Fricke (1954) combined the efforts by Ledoux (1951) and Chandrasekhar (1953), yet he too could not escape certain arbitrary assumptions. And Bel & Schatzman (1958), having returned to Chandrasekhar's model, let it rotate differentially – in violation of the equilibrium conditions, though.

wave analysis led him to a relation

$$\omega^2 = \kappa^2 - 4\pi G\rho \cdot f(k,h) + k^2c^2 \tag{3}$$

that basically differed from Eqn (2) in its modified gravity term depending on both wavenumber and the layer's thickness h. The correction factor$f(k,h)$ evaluated, Safronov found – quite in Jeans' spirit – that rotating flat systems lose stability and must break up into rings as soon as their equilibrium volume density gets above some critical value.

In that same 1960, first results were supplied by collisionless collective dynamics, concerning the simplest, spherical systems.[30] Antonov (1960) found for them the now classical "stability criterion, rather complicated though", and Lynden-Bell (1960a) discovered a peculiar feature of their equilibrium states – the ability of collisionless spheres to rotate.[31]

Another lodestar for dynamical studies was the evidence provided by a bulk of higher-precision rotation curves obtained for spiral galaxies in the late 1950s by Burbidges and Prendergast. At long last, their general rotation was ascertained to be strongly differential. This fact, stripped now of all surmise, seriously warned astronomers that they were in the presence of a real problem of the *persistence* of spiral structure.

> "There appears to have been some feeling in recent years that individual spiral arms are long-lived features in a galaxy. [. . .] However [. . .] we shall show that the form of the rotation-curves for spirals will insure that the spiral form will be completely distorted in a time short compared with the age of a galaxy" (Prendergast & Burbidge 1960, p.244).

The quantitative estimates did show that the data on M31, M81, NGC 5055 "and probably all similar spiral galaxies" were in conflict with "certain apparently reasonable assumptions" – namely, at least with one out of the

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[30] Vlasov, a renowned plasma physicist, contributed to galaxy dynamics as well, via his article (Vlasov 1959) that had a special section "Spiral structure as a problem of the mathematical theory of branching of solutions of nonlinear problems". Through the collisionless Boltzmann and Poisson equations, he examined the equilibrium of an immovable plane-parallel slab, re-derived its density profile $\rho(z) \sim \mathrm{sech}^2(z/h)$, and 'disturbed' eigenvalues of the equilibrium solution, wishing to establish the character of "infinitely close figures of equilibrium". His new solutions turned out "ribbed", or spatially periodic, with the "exfoliation period" being close to 3 kpc and corresponding to the scale of "stellar condensations observed by Oort". Despite some technical flaws (e.g., his basically smooth function $\rho(z)$ played as stepped one in integrations), Vlasov's conclusion about possible "ribbed" static equilibria in the tested slab was formally correct. Still, surprisingly (at least in retrospect), he gave no stability discussion, already practicable in contemporary plasma physics and very fitting as it would be for his galactic model.

[31] "This is in contradiction to Jeans' result, but is obtained by using his method correctly and following the consequences" (Lynden-Bell 1960a, p.204).

following three: (a) only circular velocities are present in galaxy disks, (b) these velocities are independent in time, (c) material which is originally in a spiral arm remains in that arm (Prendergast & Burbidge 1960, p.244, 246).

The 'urgent problem' of the persistence of spiral forms was taken up by Oort. Speaking at a 1961 conference at Princeton of "every structural irregularity" in a galaxy as being "likely to be drawn out into a part of a spiral", he called for another phenomenon to turn to and conceive:

> "We must consider a spiral structure extending over a whole galaxy, from the nucleus to its outermost part, and consisting of two arms starting from diametrically opposite points. Although this structure is often hopelessly irregular and broken up, the general form of the large-scale phenomenon can be recognized in many nebulae" (Oort 1962, p.234).

Oort suggested "three ways out of this difficulty", one of which was that "the arms could retain their present spiral shapes if matter were constantly being added to their inner edges, while the outer edges would constantly lose matter" (Oort 1962, p.237-8). This possibility was given an eager discussion at the conference (Oort 1962, p.243).

Yet one more lodestar for galaxy dynamics was lit in the 1950s by numerical computer methods. They first served the calculating of three-dimensional star orbits; Contopoulos (1958, 1962) then stated their non-ergodicity and posed anew the problem of a third integral of motion. P.O. Lindblad, as we saw, turned the same Stockholm computer to studying the galaxy dynamics in terms of an $N-$body problem (Lindblad & Lindblad 1958; P.O. Lindblad 1962).

1.4 Dispersion orbits

> Most remarkably after that fine beginning [in 1925-27], it took Lindblad not three further months or years, but three whole decades, to connect this implied epicyclic frequency κ and the ordinary angular speed of rotation Ω into the kinematic wave speeds like $\Omega \pm \kappa/m$, which we very much associate with him nowadays, especially when muttering phrases like 'Lindblad resonances'.
>
> *Toomre 1996, p.2-3*

These fresh winds did not catch Lindblad unawares. The importance of differential rotation was already conceived by him from radio observations (Kwee et al 1954; Schmidt 1956), and he even noticed – for the Galaxy and, later, for M31 (van de Hulst et al 1957) and M81 (Munch 1959) – the curious empirical near-constancy of a combination

$$\Omega_2 = \Omega(r) - \kappa(r)/2 \cong const. \tag{4}$$

And the dynamical stability problems were *always* comprised by his spiral theories. Already from 1938 on, dispersion relations of type (3) surfaced in his evolving papers, growing more and more complicated by way of various gradient-term inclusions for a tentatively better description of the crucial – unstable – bar-mode (see Genkin & Pasha 1982).[32]

However, the idea of applying the collective-dynamical methods to shearing stellar galaxies hardly ever impressed Lindblad. He must have felt (Lindblad 1959) the limits of his hydrodynamical approach (long-wave solutions at differential rotation were unattainable analytically, while, on the short-wave side, the whole approach failed for want of an equation of state), not having yet a means of solving kinetic equations. Also, Lindblad perhaps doubted the very possibility of steady modes in shearing galaxies. Either way, the empirical relation (4) that he himself had stated inspired him the most. With it as a centerpiece he started a new, "more definite theory of the development of spiral structure" (Lindblad 1962b, p.148), one he called the *dispersion orbit* theory (Lindblad 1956, 1961). It was imbued, intuitively, with a hope that gas and Population I stars "are somehow aggregated on their own into a few such orbits in each galaxy – almost like some vastly expanded meteor streams" (Toomre 1996, p.3).

Lindblad described epicyclic stellar oscillations in a reference system rotating with angular velocity $\Omega_n = \Omega - \kappa/n$, $n = d\kappa/d\Omega$, and he imagined a star's radial displacement ξ to depend on its azimuth θ as $\cos n(\theta - \theta_0)$, θ_0 being apocentric longitude. The simplest forms of orbits occurred for integer n's, the case of $n = 2$ satisfying the empirical condition (4). For this case, "the most general form of an ellipsoidal distribution with vertex deviation" was obtained (Lindblad 1962b, p.152), with which Lindblad sought to calculate the total gravitational potential and, by extracting its averaged (over time and angle) part, to treat the remainder as a contribution to the perturbing force. He Fourier-decomposed this force and retained the $m = 1$, 2 harmonics to analyze disturbances to a ring of radius r composed

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[32] Lindblad's dispersion relation in its simplest form (Lindblad 1938) was rather similar to Safronov's relation (3), both showed the same terms, but, as Lindblad was focused on global modes and Safronov dealt with short-wave radial oscillations only, their treatment of the correcting factor in gravity term was technically different. Still, "Lindblad, despite all his words, never quite seemed to relate those formulas to any *spiral* structures, and [...] only applied them literally to non-spiral or bar-like disturbances". (*Toomre*)

of small equal-mass particles. Like Maxwell (1859) in his similar Saturn ring problem,[33] Lindblad obtained four basic modes for each m. Two of them described nearly frozen, practically co-rotating with material, disturbances to the ring density. Two others – "deformation waves" – ran with speeds $\Omega \pm \kappa/m$, the minus sign being for the slower mode. It was, at $m = 2$, "essentially this slowly advancing kinematic wave [...] composed of many separate but judiciously-phased orbiting test particles" (Toomre 1977, p.441) that Lindblad meant by his dispersion orbit $\xi(\theta)$. The fact that its angular velocity was independent of radius, $\Omega_p(r) = \Omega_2 = const$ (with an observational accuracy of the condition (4)), implied a stationary state for all test rings, i.e. over the entire radial span where this condition was well obeyed.

> "This fact greatly intrigued Lindblad – who did not need to be told that strict constancy [of $\Omega_p(r)$] would banish wrapping-up worries or that the nicest spirals tend to have two arms. Yet astonishingly, that is about as far as he ever got. [...] It never occurred very explicitly to [him ...] to combine already those 'orbits' into any long-lived spiral *patterns*" (Toomre 1977, p.442).

1.5 Circulation theory of quasi-stationary spirals

> The suggestion that the patterns are density waves is old and was first explored by Bertil Lindblad. His emphasis was mainly on kinematics and less on collective effects on a large scale, though many of the kinematical effects he discovered can still be seen in the collective modes.
>
> *Kalnajs 1971, p.275*
>
> His details were unconvincing, but no one can accuse him of missing the big picture.
>
> *Toomre 1996, p.3*

P.O. Lindblad's experiments with flat galaxies were planned to clarify the dispersion-orbit theory. They started with a plane system of several annular formations arranged by $N \cong 200$ mutually attracting points, and the development of "small deviations in shape and density of a bisymmetrical nature" (Lindblad 1963, p.3), applied to one of the rings, was studied. Two waves propagating along it were shown to rise first, one running slightly faster and the other slower than unperturbed particles, thus invoking a pair of corotation resonances, one on each side from the ring. These induced a

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[33] Maxwell's problem was on disturbances of N equal-mass particles placed at the vertices of an N-sided regular polygon and rotating in equilibrium around a fixed central body.

leading spiral; soon it rearranged into a trailing one and smeared out almost completely, but some trailing arms then re-appeared, owing evidently to a small oval structure retained at the center. This led P.O. Lindblad to propose that galactic spirals may involve a *quasi-periodic* phenomenon of trailing-arm formation, breakup and re-formation.[34]

B. Lindblad, however, got captivated by another view of these results. He even lost of his earlier dispersion-orbit enthusiasm and turned in 1961-62 to a concept "*On the possibility of a quasi-stationary spiral structure in galaxies*" (Lindblad 1963) in the presence of differential rotation.[35]

> "The morphological age of spiral galaxies as estimated [...] from considerations of the evolutionary process connected with star formation from gaseous matter ranges between 10^9 and 10^{10} years. In consequence it is natural to assume that the typical spiral structure is not an ephemeral phenomenon in the systems but has a certain steadiness in time [...and] to investigate how far gravitational forces alone can explain a spiral structure of a fair degree of permanence" (Lindblad 1964, p.103).

To begin with, Lindblad introduced an axisymmetric flat stellar system in differential rotation and, echoing the $N-$body pictures, imposed on it an initial *trailing* spiral pattern formed by some extra amount of stars. His calculations of the effect upon a nearby test star from such a spiral arm showed that, as it sheared, the star approached it and fell in, having no other chance to leave it than making slight epicyclic oscillations. Such an assimilation of material in just one galactic turn or so worked well against shearing deformation of spiral arms, through their exchange in angular momentum with stars attracted. As the result, the pattern's angular speed became the same all over, meaning its quasi-stationarity. Now two dynamically different regions arose in the system, an inner region with stars moving faster than the spiral, and an outer one, tuned oppositely; they were divided by a corotation region, where the material orbits at nearly the same rate as the pattern.

For a true stationary pattern not only its permanence in shape was needed, but also a balance of the stars' travel in and out of the arms. The latter was secured in Lindblad's eyes by his *circulation* theory (Lindblad 1963,

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[34] "I was delighted to see them [P.O. Lindblad's results] as evidence as to how much one could do already then (!) by way of interesting numerical studies with some hundreds of particles – in that sense his work was very inspiring. Yet [...] it also struck me that his study really dealt with not much more than the transient breakup of inherently unstable configurations of some 4 or 5 artificially introduced rings of material" that imitated "a revolving disk – one which [...] should be fiercely unstable if begun just as cold. [...] But, again, as a sample of what could already be done, P.O. Lindblad's work was indeed like a breath of fresh air". (*Toomre*)

[35] Lebedinski was another one who in his cosmogony of galaxies and stars admitted – still earlier – "the dynamical possibility of the formation of quasi-stable spiral arms rotating with a constant angular velocity for all the spiral" (Lebedinski 1954, p.30). Yet since Jeans' 1920s that idea, as such, did not sound as a novel dynamical motive. It got a really new sounding only when the fact of global galactic shearing was finally conceived.

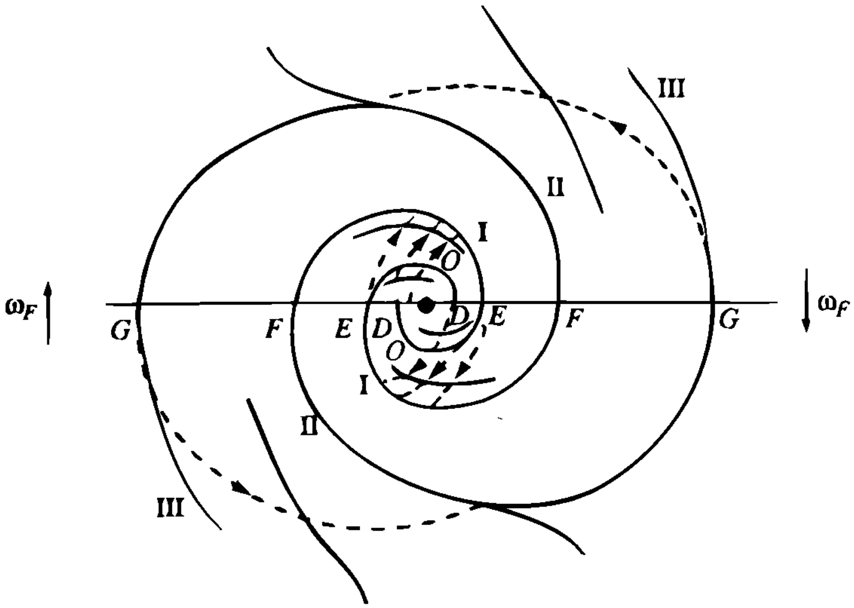

Figure 4: *Circulation of material in a galaxy having a quasi-stationary spiral structure.* The general rotation is clockwise, points F mark the corotation radius. See the text for more details. (The figure is reproduced from Lindblad 1964)

1964) developed in the framework of a trailing two-armed spiral model, each arm making one full convolution (or a bit more), comparably inside and outside corotation (Fig.4). Actually, each arm ended where, according to analytical estimates, its stars were effectively attracted by the next-to-last arm (outside corotation) and fell in it "in a shower of orbits". The assimilated stars kept moving slower than the spiral, thus having an along-arm ascent until a repeated flow down. Inside corotation (the region of much less interest to Lindblad), the circulation was set up as well, but in the opposite direction: stars captured by spiral arms got drawn down along them until sucked upward by the next-to-innermost spiral convolution.

This circulation theory was nothing but a sketch by 1964. Well treating quasi-steady spirals as a density *wave*, it gave no desired quantitative results regarding pattern speeds, arm pitch angles, interarm spacings, or the like. It also failed to explain *dynamically* the preference for trailing arms – although the dispersion-orbit theory had honestly done no better. It is regrettable that Lindblad, who died in 1965, did not have the time to complete this last work he had started, and only "left behind a long handwritten unfinished manuscript that in great mathematical detail studies the gravitational effects of spiral arms in his circulation pattern" (*P.O. Lindblad*).

* * *

The original spiral theories by Bertil Lindblad passed into oblivion. Among the causes for the passage were the feeble empirical base of the 1920s-40s, the frightening bulk of mathematics and scant help from the first computers even during the 1950s, a constant flux of changes in Lindblad's latest inferences and the rather opaque prose of his abundant articles,[36] and above all a lack of quantatively checkable predictions. Yes, one can readily agree that

> "all problems that in later developments turned out to be important in the theory of spiral structure had, in one way or another, already been touched upon or even studied by Lindblad" (Dekker 1975, p.18)

as well as that

> "such complex collective dynamics was perhaps too hard for anyone, no matter how talented, in those mid-20^{th}-century decades before computers, plasma physics, or any inkling of massive halos" (Toomre 1996, p.3),

but also true is that all of the spiral undertakings by Lindblad, however ingenious and farsighted they may appear to have been in retrospect, got sunk ingloriously in the silence of time.

An interesting question is: *why*? Why did it come to be that the true master of theory and observation had long been surprisingly close to but never quite at the point of recognition – opened in the 1960s to a pleiad of fresh theorists – that spiral structure is mainly a collective wave phenomenon in shearing galaxies? One can only suppose that Lindblad did not reach, let alone exploit, such wave-mechanical ideas partly because they were not in the air yet, but perhaps mainly because he was impeded by his life-long emphases on the orbits of individual particles. *All* his efforts on galaxy dynamics were fed by the stellar-epicycle concept, the pearl of his scientific youth. This set the trend for Lindblad's theories, and whenever some such orbital attack fell short of its destination, he did not get on with searching for totally different ways of continuing, but instead renewed his attack time and again under his old epicyclic-orbit colors.

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[36] "It has not been possible to do justice to all phases of Lindblad's researches", Chandrasekhar 'complained' already in 1942, but nonetheless he gave a "more or less complete bibliography" including 25 Lindblad's writings on the spiral problem (Chandrasekhar 1942). "The flow of his publications can be understood if one realizes that he thought in the form of a paper. When attacking a problem he started writing the paper at once". (*P.O. Lindblad*)

II. ON A NEW WAVE CREST

> During a time when it was fashionable to 'explain' the maintenance of spiral structure by magnetic fields, Lindblad persisted in the belief that gravitation was the dominant factor, and now we have come full circle back to this view.
>
> *E.M. Burbidge 1971, p.266*

2.1 Regenerative spirals by Lynden-Bell

> We deduce that our galaxy is likely to have had spiral arms for most of its lifetime and that as old arms coil up so new uncoiled arms must start to form from their corpses. The problem of describing such a mechanisms we call the regeneration problem.
>
> *Lynden-Bell 1960b*

In 1960 Lynden-Bell presented at the University of Cambridge his PhD thesis "Stellar and Galactic Dynamics" (Lynden-Bell 1960b)[37] considering some general aspects of stellar-dynamical and ergodic theories. Its separate part "Cosmogonical gas dynamics" was on the spiral problem. It stated, echoing the stress of the day, that "the arms are primarily the seat of gas and dust" (so that the lenticular galaxies, deprived of them, "can no longer give birth to a spiral structure"). It found the cosmogonical approach the most convenient – in case of full denial from Jeans' classic scheme as inoperable in the presence of differential rotation.

> "It seems impossible that the protogalactic gas was uniformly rotating when the stars formed. It seems more likely that as the primordial gas broke up into condensations [protogalaxies] each fluid element tended to preserve its angular momentum about the centre of the local condensation. The equilibrium reached is then one in which centrifugal force nearly balances gravity and the pressure is mainly important in preventing the system from becoming very flat."

Lynden-Bell analyzed realistic equilibrium configurations of a frictionless gas system and derived "an energy principle which should provide a powerful means of determining the equilibria on a computer". Any such configuration, when achieved by the system, is exposed to a slow secular evolution that "will not be determined by shrinkage due to the radiation of energy as in Jeans case, but by the transfer of angular momentum due to friction" neglected in the equilibrium derivations. The system "must: i) concentrate its angular

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[37] Leon Mestel was his advisor.

momentum into a very small fraction of its total mass, and ii) leave the remainder a more concentrated uniformly rotating or pressure supported body. This is borne out by observation on both the scale of the solar system and that of the galaxy. [...] We should thus expect a uniformly rotating central condensation surrounded by a differentially rotating disc" (Lynden-Bell 1960b).

It is with such an evolved disk of gas that Lynden-Bell linked his spiral considerations. In shearing deformation – a point-blank menace to 'any structural irregularity' – he, unlike many workers of the day, saw not an antagonist to the persistence of spiral arms, but a factor of their cyclic regeneration created through gravitational instability of the gaseous subsystem in a combined star-gas galactic disk (the stellar component being liable for gas equilibrium rather than for any collective dynamics). In such a setting, the problem needed a global stability analysis of a system in differential rotation, which technically was not feasible. That is why for want of the better Lynden-Bell employed the methods that had served Fricke (1954) with his $\Omega = const$ model; this led to a necessary and sufficient condition of Jeans' stability, $\Omega^2/\pi G\rho_0 > 2/3$ (cf. Sect. 1.3), and instructed the growth rate for unstable stages to be $\gamma \leq 2\Omega$. An $m = 2$ mode at $k \cong 1/3$ kpc^{-1} was found the most important, it fell down towards the disk edge and center, being long-wave and therefore fast-growing. This was in substance Lindblad's bar mode, one specified by a pair of condensations placed oppositely at $r \cong 9$ kpc from the center. Before density had grown by a factor e, rotation turned the system through 180^0 (at $\gamma = 2\Omega$). But as this passed, effects of shear (excluded from the strict stability analysis) just wound the "azimuthally independent structure" round the galaxy, at least once. This meant a grave radial-wavelength reduction, which was expected to be a cause for slowing down the growth rate as effectively as to turn off instability altogether. In this event, the spiral arms would expand back "to form the sheet from which we started", and the whole process might then recur. However, a more careful analysis confirmed the dependence of γ on k only "for systems very close to stability". This would be "far too sensitive to give the great variety of spirals" and could not apply "for any part of the observed spiral arms". The regeneration theory proposed, Lynden-Bell (1960b) concluded, was "therefore untenable".

But as it turned out later, this pessimism was rather excessive, since it became clear eventually that there was a good deal of wisdom even in such regenerative thoughts. This, however, is not how things developed immediately, because, as we will see in the forthcoming section, the old idea of steady spiral modes was about to gain a new and important burst of enthusiasm.

2.2 MIT enthusiasm

Chia Ch'iao Lin was not an astronomer. Since the pre-war time, he had been studying fluid flows. By the 1960s, he had had over 60 publications, a monograph on hydrodynamic stability (Lin 1955), a world recognition of an applied science expert, and a solid reputation at the department of mathematics in the Massachusetts Institute of Technology (MIT) where he worked since 1947. But he did feel a continual interest in astronomy, being admired with strict analytical papers by Chandrasekhar, with M. Schwarzschild' work on stellar structure, with Zwicky's morphological method. In 1961 this side interest became Lin's life-long vitality. That spring, on visit in Princeton,[38] he attended the aforementioned conference on interstellar matter and, having become familiar with the developments in galaxy research, he got captured by the problem of the persistent spiral structure.[39]

Back in MIT, Lin conveyed his galactic enthusiasm to his young colleagues Hunter and Toomre.[40] For quick acquaintance with current periodicals, a 'reading group' was formed;[41] a "friendly back-and-forth atmosphere" (*Toomre*) warmed open discussions and working visits of Woltjer and Lust, organized by Lin;[42] Lebovitz was hired in the department.[43] In 1962, Shu arrived there for doing his undergraduate course work under Lin's guidance,[44]

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[38] Stromgren invited him for discussions on stellar structure (*Lin*), largely in relation to his fresh interest in hydrodynamics of liquid helium (Lin 1959).

[39] In his early spiral papers, Lin often quoted Oort's statement reproduced in Sect. 1.3.

[40] At that time, the department of mathematics in MIT was vigorously enlarging its applied side. Hunter and Toomre were hired there in 1960, just after they had got their PhD degrees in fluid dynamics in England. Initially, they hoped to collaborate with Backus (*Hunter*; *Toomre*), a recognized leader in geomagnetic problems, but as he left MIT that year already, they two "soon caught some of Lin's fever for problems in the dynamics of galaxies". "Almost at the moment I first met him in fall 1960 I was struck with his breadth of scientific interests, his really excellent spoken English, [...] and his genuinely gracious manner of dealing with other people". (*Toomre*)

[41] "[We] were all becoming interested in astrophysical problems together. We read Martin Schwarzschild's book on stellar structure together". (*Hunter*)

[42] "It was a real pleasure to have such a thoughtful and articulate theoretical astrophysicist as Woltjer so close to chat with about this thing or that. [...] It was from his informal lectures that summer that I learned for the first time not only how Dutch and Australian radio astronomers working in parallel had more or less mapped the spiral arms of this Galaxy from the velocity maps, but also how astonishingly thin – and yet curiously bent – is our layer of 21-cm gas". (*Toomre*)

[43] "I had just received my PhD [working with Chandrasekhar], I wished to pursue applied mathematics, and I had received an offer of an instructorship from one of the best applied-mathematics departments in the country. Lin's motive I can only speculate on. He was interested in moving in the direction of astronomy and of the spiral-structure problem and perhaps figured I would be a useful participant. If this is the case, I suppose my stay at MIT may have been somewhat disappointing to him because I spent all of it in close collaboration with Chandrasekhar on a quite different set of problems". (*Lebovitz*)

[44] "I began work with C.C. Lin in summer 1962 as an undergraduate research assistant

and Hunter with Toomre, their instructorship finished, left MIT, one back for Cambridge, UK, the other for Princeton; their first papers appeared in 1963.

Hunter and Toomre made their debut in galaxy dynamics on a vital problem already posed but yet unanswered very basically (Kuzmin 1956; Burbidge et al 1959): How to connect the empirical rotation curves of galaxies with their equilibrium mass distribution? Toomre (1963) set forth a general mathematical method, and for a razor-thin disk model he derived a series of solutions well known nowadays as Toomre's models of n^{th} order (Binney & Tremaine 1987, p.44).[45] Hunter (1963) used a distinct thin-disk approximation and found another series of exact solutions. The simplest there was the case of uniform rotation and surface density $\mu_0(r) \propto (1 - r^2/R^2)^{1/2}$. For it only was the analytical study of equilibrium stability possible, and Hunter did it "using only pencil, paper, and Legendre polynomials" (Toomre 1977, p.464). This cold disk proved *unstable* for a wide span of axisymmetric and non-axisymmetric oscillation modes.[46] These papers by Toomre and Hunter had paved the way for further works on kinematical models and global dynamics of flat stellar systems.

2.3 Gravitational stability of flat systems

> Lin asked [Woltjer in 1961]: What are the circumstances that would be needed for either one or both of the stellar and interstellar parts of a supposedly smooth galactic disk to remain gravitationally stable against all large scale disturbances?
>
> *Toomre 1964, p.1217*

> The importance of collective effects in our Galaxy was first clearly pointed out by Toomre (1964). He showed that in the disk the stellar motions are sufficiently coherent to make it almost vulnerable to collapse. He also pointed out that the scale on which this would occur is quite large.
>
> *Kalnajs 1971, p.275*

As we have seen, Safronov already raised the question of gravitational instability in flat rotating systems, aiming at the breakup of a protoplanetary

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and continued through the fall and spring 1963, on the topic of spiral structure in galaxies as my undergraduate thesis project in physics at MIT [...] I knew Lin from even earlier because he is a close friend of my father". (*Shu*)

[45] Toomre's model 1 reproduced the result by Kuzmin (1956) then unknown to Toomre (Binney & Tremaine 1987, p.43).

[46] The stability of differentially rotating cold disks Hunter studied in his subsequent paper (Hunter 1965).

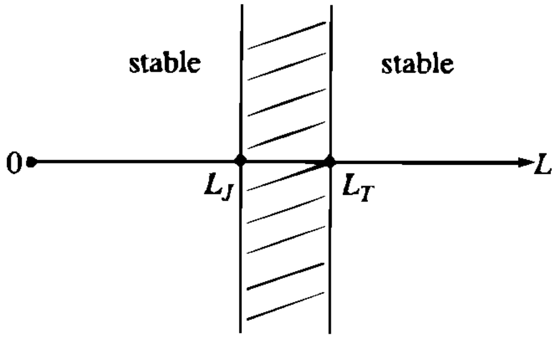

Figure 5: *Characteristic scales in a gravitating disk.* A cold rotating disk is stable for radial disturbances on the scales $L > L_T$, a non-rotating hot disk is stable of scales $L < L_J$, a hot rotating disk is stable on both scales. As the velocity dispersion becomes of the order of the circular velocity, one obtains full axisymmetric stability.

cloud into detached rings. Toomre, interested in basically smoother objects like galaxies, turned in 1961 to a rather close, although opposite in accent, topic, and by the summer of 1963 he prepared an article "On the gravitational stability of a disk of stars" (Toomre 1964, hereinafter T64).

The paper started with the general presentation of the problem as it was then seen.

> "The well-known instabilities of those Maclaurin spheroids whose rotational flattening exceeds a certain fairly moderate value suggest that the other sufficiently flattened, rotating, and self-gravitating systems might in some sense likewise be unstable. At any rate, these instabilities have been often cited as a likely reason why one does not observe elliptical galaxies exceeding a certain degree of oblateness. It is only when we turn to consider what are now thought to be the distributions of all but the youngest stars in the disks of the ordinary (as opposed to the barred) spiral galaxies that this classical result suggests a serious dilemma: How is it conceivable, in spite of these or analogous instabilities, that so much of the fainter stellar matter within such galaxies – and certainly the S0 galaxies – should today appear distributed relatively evenly over disks with something like a ten-to-one flattening?" (T64, p.1217)

The detailed study of the problem was preceded by a primary, qualitative stability estimate.

A rotating thin cold disk, in an approximate equilibrium between gravity and centrifugal forces acting on each mass element, is prevented from general contraction, still not from fragmentation. Small-size clumpings arise everywhere in such a disk, and then collapse, their gravity taking excess over rotation. But if larger-sized, they do not go as these two factors counteract each other. The demarcation length scale L_T proves plain co-measurable with the disk radius R. Thus the cold model, for all specifications it may have, is clearly unstable.[47] The part played by random motions is best visualized with an immovable sheet model. There instability is avoided if stars (other mass elements), having an rms velocity , cross a clumping zone in a time not exceeding that needed for an $e-$fold amplitude growth as registered in the cold case. Hence the largest yet ungrowing disturbance is found on an $L_J \approx c^2/G\mu_0$ scale, which is essentially the Jeans stability criterion. Now, letting the sheet rotate, one sees the two characteristic scales, L_T and L_J, be present (Fig.5). L_J gets closer to L_T for higher velocity dispersions, until they coincide at c's as high – in the order of magnitude – as the rotational velocity, thus meaning full stabilization against this sort of disturbances.

The strict analysis of *axisymmetric* disturbances to a razor-thin disk, performed in T64, supported these rough estimates. In the cold case, it led to a local dispersion relation

$$\omega^2 \ = \ \kappa^2 - 2\pi G\mu_0|k| \tag{5a}$$

or

$$\nu^2 = 1 - |k|/k_T \tag{5b}$$

linking the wave frequency in units of κ, $\nu = \omega/\kappa$, with a critical wavenumber

$$k_T = \kappa^2/2\pi G\mu_0, \tag{6}$$

the one to determine the shortest wavelength $\lambda_T \equiv 2\pi/k_T$ of ungrowing ($\nu^2 \geq 0$) disturbances (Fig.6).[48] The hot-disk analysis detected the minimum

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[47] Toomre got this estimate by the fall of 1961 and was struck with the fact that nothing had ever been said on the thing just shocking with its as simply derivable inference that cold disks be prone to violent instabilities. (*Toomre*)

[48] Analyzing axisymmetric disturbances to a flattened rotating cloud, Safronov (1960a,b) did not solve the Poisson equation. He was guided by the notice that short radial waves find adequate the cylindric approximation for a torus (ring). But the cylinder is the sum of 'rods', or elementary cylinders whose individual gravity is given by a simple formula, so that the business is just to integrate in infinite limits the elementary contributions over longitudal and transversal variables $x$and z. There Safronov was not perfect, however. His gently stratified cloud turned a stiff $2h-$thick plate as he took his introduced density function $\rho_0(z)$ out of integration over z. His subsequent integration over $x$was in an interval of $\pm\lambda/4$; that, he argued, ensured a predominant contribution to the perturbed force (which is qualitatively true). Had he integrated in infinite limits, and first – most trivially – over x, the gravity term in his Eqn (3) would have become $-2\pi Gk\int\rho_0(z/h)e^{-k|z|}dz$, and with the exponential factor serving as a thickness correction he would have accurately managed with any density profile – and, most obviously, would have found that in the zero-thickness limit that factor simplifies to unity, the integral just gives the surface density μ_0, so that the gravity term converts into $-2\pi G\mu_0 k$, the form in which it was presented soon by Toomre (1964) in frames of 'regular' methods of the potential theory.

radial velocity dispersion at which the system is still resistant against *all* axisymmetric disturbances (Fig.7):[49,50]

$$c_{r,\min} = \frac{3.36 G\mu_0}{\kappa}. \tag{7}$$

The real-to-minimum velocity-dispersion ratio

$$Q = c_r / c_{r,\min} \geq 1 \tag{8}$$

thus got a local disk-stability parameter.[51,52] In a *marginally* stable state $Q = 1$, disturbances of $\lambda_0 \cong 0.55\lambda_T$ proved most unpliant and barely suppressible. Our solar neighborhood would have such a $\lambda_0 \cong 5 - 8$ kpc, but if some $Q \cong 1 - 1.5$ were not preferred empirically, implying a certain stability reserve. Of course, "it was as yet impossible to rule out instabilities altogether", but should any actually be present, they would not do with scales responding to the challenging 2-kpc spacings, as these "must almost certainly be judged as stable". This "is important as an argument against any suggestion that the existing spiral structure in this Galaxy might be the result of collective stellar *instabilities*" of the sort considered (T64, p.1236).

Still, the linear theory developed could not lay claim to very much. So it did not elucidate the cause of stellar disk heating, it even could not show any definitely what was to become with primary condensations appearing in a tentatively cold disk in one or two revolutions already. "It must not be presumed that such initial clumpings would necessarily have led to the formation of any *permanent* irregularities", Toomre noticed. "On the contrary, it seems much more likely that the bulk of the stars involved in any given (generally non-axisymmetric) instability [...] would eventually have dispersed themselves upon emerging from the opposite sides of the aggregation and upon experiencing the shearing effect of differential rotation".

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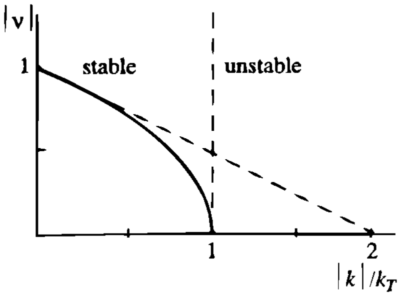
[49] To solve the Vlasov kinetic equation, Toomre used the characteristics method that for some three-dimensional purposes had already served Lynden-Bell (1962), who in his turn cited the original source (Bernstein 1958) where that method had genuinely helped with the general disperion relation for the mathematically similar problem with a Maxwellian plasma in a magnetic field.

[50] Because of a technical error in Toomre's analysis, this minimum value was initially overestimated by 20%. Not so little if one considers that the difference in $c_{r,\min}$ for star and gas disk models (the latter case admits a *much* simpler analysis) reaches 7% only. It is this "substantial error" which was detected in 1963 by Kalnajs (cf. Sect. 2.4), as reported frankly in T64 (p.1233).

[51] Formally, the '$Q-$parameter' (8) was introduced by Julian and Toomre (1966).

[52] This quantitative analysis refines the above view of disk stabilization as it shows via Eqs (6) and (7) that locally the result is attained already once $L_J/L_T = (3.36/2\pi)^2 \cong 0.286$ (0.25 in a gas disk).



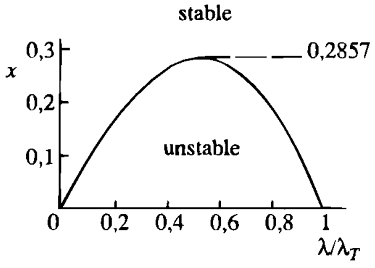


Figure 6: (left) *The dispersion relation curve for radial oscillations and tightly wrapped spiral waves in a cold disk.*

Figure 7: (right) *The hot disk neutral stability curve.* The disk is stable for all those the radial disturbances for which the parameter $x = k^2 c_r^2 / \kappa^2$ exceeds $x_{cr} = 0.2857$. This critical value determines the minimum velocity dispersion (7) sufficient to secure the axisymmetric disk stability. (The figure is reproduced from Toomre 1964)

> "It follows that an initially unstable disk of stars should probably have undergone not just one but several successive generations of instabilities, after each of which the system would have been left somewhat less unstable than it was previously. In particular, it seems likely that before very many rotation periods had elapsed, the disk would have approached a new equilibrium state that was again fairly regular and quite possibly axisymmetric, but in which the random velocities at the various radii had become – and would henceforth remain – about equal to the minimum values needed for complete stability" (T64, p.1237).[53]

Besides, since the total gravitational energy of the disk would have had to be the same during its evolution (the virial theorem), "the said redistribution of stars could not simply have consisted of an overall contraction, but would have had to entail a contraction perhaps in the inner parts of the disk jointly with a net expansion of the outer portions" (T64, p.1237) – as it was already seen by Lynden-Bell (1960b) from the gas-dynamical viewpoint.

As regards *non-axisymmetric* disturbances, it was pointed out in T64 that because of the specific action of the Coriolis force those are restrained

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[53] Asked to reminisce on how he had originally understood those dispersion velocities "about equal" to the needed minimum in the new equilibrium state – on whether or not this was a factual suggestion of marginal stability of our stellar disk, or some extra amount was yet permitted for its stability – Toomre has responded: "It is hard for me to reconstruct from this vantage point what exactly I meant or hoped by that statement. Probably I was mostly just trying to rationalize the surprising fact which I had then unearthed that the minimum theoretically needed $c_{r,\min}$ and the observed amounts seemed to agree so well within their considerable uncertainties, meaning within a factor of 1.5 or thereabouts, rather than some 2 or 3 or 4 [...] From about 1966 onwards, I was surely of the opinion that any Q less than about 1.5 here was highly suspect, if not downright ludicrous, because of fierce heating of cooler disks by their embedded gas complexes. But that came a little later. In 1964 my views were no doubt more permissive toward $Q = 1.0$". (*Toomre*)

even more effectively than radial disturbances, thus requiring no addition for $c_{r,\min}$. However, Toomre remarked, a question that his discussion left "completely unanswered" was "to what extent a similar amount of random motion [$Q = 1$] might affect the character of the most extensive non-axisymmetric disturbances, in particular those which ought to determine whether or not a given disk might prefer to develop into a barlike structure" (T64, p.1235).[54]

2.4 Kalnajs' search for spiral modes

> One can draw a parallel between the attempts to talk about galactic evolution at the present time and the attempts to understand stellar evolution before the sources of energy in the stars were understood.
>
> *G. R. Burbidge 1962, p.291*

> The study of stellar systems, such as our own galaxy, is not limited by a lack of understanding of the underlying principles, but rather by the difficulty of solving the differential equations which govern the time evolution of the system.
>
> *Kalnajs 1962, p.i*

Agris Kalnajs began his undergraduate studies in Electrical Engineering at MIT in 1955. As a good student, he participated in a special course which emphasized physics and mathematics, and provided summer employment in the Microwave Research Lab at Raytheon, making measurements for computer modeling of magnetrons. There he learned about such things as electron motions in crossed electric and magnetic field, waves carrying positive and negative energies, modes, coupled modes, parametric amplification. All this proved to be really useful in a quite different field when he arrived in 1959 in the astronomy department at Harvard University and got involved in galaxy dynamics.[55]

In the fall of 1961 Kalnajs made a research examination on "Stellar kinematics" (Kalnajs 1962).[56] The task was to calculate self-consistent radial oscillations in a rotating stellar disk as a tentative explanation for the 'local' arms in our Galaxy. Their short spacing $L \leq 3$ kpc justified the small-scale analysis in the plane of a homogeneous thin sheet. Kalnajs solved the

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[54] Real progress in the study of this problem first came half a decade later.

[55] "It was probably David Layzer's course in classical dynamics which steered me towards stellar dynamics. I rather liked David's approach: he strived for elegance. He put a lot of thought in his lectures". (*Kalnajs*)

[56] As this was only an unpublished internal document, its outline below is mainly to illustrate how Kalnajs was then progressing.

Vlasov and Poisson equations as an initial-value problem and obtained an equation for the radial oscillations and a dispersion relation which was formally correct.[57] As he was interested in short waves, he made an asymptotic evaluation of the integral expression, and in the process left out "a factor 2π or something of that order" (*Kalnajs*). This and the reduced disk response at the short waves ($\lambda \sim 1$kpc) made him conclude that $\omega \cong \kappa$, because the self-gravity effects became "too small to be interesting" (*Kalnajs*): all the solutions oscillated and were traveling waves that, in passing, "tend to gather up the low dispersion objects such as gas" (Kalnajs 1962, p. ii). As a plausible "arm-like density wave" generator, an oval-shaped body at the Galaxy center was mentioned.

The error in this asymptotic evaluation was uncovered in the summer of 1963 when Kalnajs and Toomre finally got together, compared and cross-checked their notes, and detected each other's technical errors. Kalnajs looked anew at his radial-oscillation theory and re-evaluated the dispersion relation, this time into the form in which it entered his thesis (Kalnajs 1965).[58] In modern notation – whose convenience and clarity we owe undoubtedly to Lin – and without the uninteresting stellar disk thickness correction going through that original 1961-63 analysis,[59] it is

$$\nu^2 = 1 - |k|/k_T \cdot F_\nu(x), \tag{9}$$

where

$$F_\nu(x) = 2(1-\nu^2)\frac{e^{-x}}{x}\sum_{n=1}^{\infty}\frac{I_n(x)}{1-\nu^2/n^2}, \quad x \equiv k^2c_r^2/\kappa^2, \tag{10}$$

is Kalnajs' version of a factor to account for the role played by random motions of stars. There is no such play in the limit $x = 0$, relation (9) then reduces to Toomre's cold-disk result (5) that shows the gravity term proportional to the wavenumber and growing without bound. Now random

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[57] Following Landau's method correctly describing small oscillations in homogeneous electrostatic plasma, an arbitrary disturbance is initially imposed on the stellar sheet and its evolution is traced out. With time, the dependence on the initial conditions dies away, and the result is provided by the integrand poles whose expression – the dispersion relation – connects the established wave parameters.

[58] "Strictly speaking, I was the first to write down the dispersion relation. But that is not the important thing. What is more important is who made the best use of that equation. And here it was Toomre, who used it to discuss the stability of the Galactic disk – a distinctly more fundamental topic than the subject of my Research Examination. [...] By the time we got together in 1963, that is probably the way we understood our respective contributions". (*Kalnajs*)

[59] The thickness corrections were worth considering for wavelengths as short as 1.5 kpc as they reduced the radial force by a factor of 2 or 3, but for $\lambda \cong 6$ kpc the reduction was some 20%-30% at most.

motions arrest this growth: the total contribution of gravity only reaches a maximum at $x_0 \cong 1$, still giving rise to instability ($\nu^2 < 0$) if large enough, and for $x >> 1$ it becomes small. In the solar neighborhood that value of x_0 points to a radial wavelength $\lambda_0 \cong 6$ kpc, the one concluded by Toomre from his neutral stability analysis. Its commensurability with the radial size of the Galactic disk makes the local theory somewhat suspect.

> "When I wrote my Research Examination I was under the impression that the spacing between the spiral arms was about 1.5 kpc. After Toomre and I got together, it became clear to me that the 1.5 kpc waves/fluctuations were not the important modes of the Galaxy. [. . .] Also by the fall of 1963 I had obtained my own copy of Danver's thesis (thanks to my uncle who was at Lund University). Danver had measured the spiral patterns and came up with a typical pitch angle of 16°.6. This implies scales even larger than 6 kpc. [. . .] By this time Alar had published his disk models, and I could use them to estimate the scales at which these disks were most responsive, and they convinced me that a WKBJ approach [see Sect. 3.1] was too crude [. . .] and that – unlike plasma – galaxies were too inhomogeneous. [...] So the future was 'global modes and integral equations'." (*Kalnajs*)

Once he realized this fact, Kalnajs lost interest in the local theories, which were good for the stable small-scale solutions, and turned to *global* modes as the correct approach to the oscillation problem. In the fall of 1963 he presented to his thesis committee at Harvard "An outline of a thesis on the topic 'Spiral structure in galaxies' " (Kalnajs 1963), summarizing his ideas for a new theory of steady spiral waves. Because this document has been almost unknown, a long quotation from it appears to be quite appropriate.[60]

> "A feature peculiar to highly flattened stellar systems is the appearance of spiral markings, called arms. These features are most prominently displayed by the gaseous component of the galaxy and the young hot stars which excite the gas. However, the density fluctuations can still be seen in the stellar component, appearing much fainter, but also more regular.
>
> The division of the galaxy into two components, gaseous and stellar, appears natural when one considers the dynamical behavior of these two subsystems. The gaseous component is partly ionized and is therefore subject to magnetic as well as gravitational forces, and has a very uneven distribution in the galactic plane. The stellar system is quite regular, its dynamics being governed by the long-range gravitational forces arising from the galaxy as a whole; the density of stars is sufficiently low that binary encounters between stars may be ignored. The stellar component, which is the more massive, cannot support density fluctuations on a scale much smaller that the mean

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[60] "I do not recall exactly when I first learned that Lin was also interested in spiral density waves (it was probably a talk he gave at MIT), but at that stage our relations were most cordial and I also felt that my understanding of this topic was more thorough than his. So having produced a written document, I am pretty sure that I would have found it difficult *not* to boast about my achievements" (*Kalnajs*). "A written document" there refers to the "Outline" which at least Toomre received from Kalnajs in November 1963.

> deviation of the stars from a circular orbit (or the scale of the peculiar motions). The gas, on the other hand, would support smaller-scale fluctuations – at least in the absence of magnetic effects. The fact that observed spiral arms are not much narrower than the smallest scale that the stars will tolerate suggests that stars must participate actively in the spiral patterns.
>
> There is a fundamental difficulty, however, in the assumption that spiral arms are entirely stellar: if an arm can exist and does not grow in time, then its mirror image is also a possible configuration. This follows from the time-reversibility of the equations of motion combined with their invariance under spatial inversion. Thus the leading or trailing character cannot be decided on the basis of a linearized theory if we insist on permanency of the spiral markings. The observations indicate, however, that nature in fact prefers trailing spiral arms. Thus a plausible theory of spiral structure must include both the stars and the gas.
>
> I regard the galaxy as consisting of two components, gas and stars, coupled by gravitational forces. The stars provide the large scale organization and the gas discriminates between leading and trailing arms. ([*Footnote in the original text*]: The stellar system can be thought of as a resonator, and the gas would then be the driver which excites certain of the normal modes.) If the coupling is not too strong, one may at first consider the two subsystems separately, and afterwards allow for their interaction. Unfortunately, one cannot evaluate the magnitude of the coupling without calculating the normal modes of the two subsystems. For the gaseous component, only the crudest type of analysis is possible at present, since one should include non-linear terms in the equations governing the gas motion in order to be realistic. The stellar component, on the other hand, is sufficiently smooth that a linearized theory should apply, and the problem of determining the normal modes can be formulated, and, with a little effort, solved.
>
> I have chosen as my thesis topic the investigation of the stellar normal modes in the plane of a model galaxy. [...] Some qualitative features of the equations indicate that the type of spiral disturbance with two arms is preferred. This result does not seem to depend critically on the model, which is encouraging. The final proof has to be left to numerical calculations, which are not yet complete." (Kalnajs 1963, p.1-3)

It is seen therefore that Kalnajs was envisaging the disk of stars as a resonator in which global spiral-wave modes are developed. If *stationary*, the leading and the trailing components are just mirror-imaged, so that, superimposed, they give no spiral pattern. However, due to slow non-reversible processes occurring in real galaxies, the symmetry is violated.

In support of his normal-mode concept, Kalnajs considered large-scale non-axisymmetric disturbances to a hot inhomogeneous flat stellar disk, and derived for them a general integral equation whose complicated frequency dependence implied a discrete wave spectrum. He also pointed out the role of Lindblad's condition (4). When satisfied, large parts of the galactic disk could support coherent oscillations for the $m = 2$ mode, whereas for larger

m's there would be Lindblad resonances within the disk. Stars in these regions feel the perturbing wave potential at their own natural frequency,

$$|\nu| = 1, \quad \nu \equiv (\omega - m\Omega)/\kappa, \tag{11}$$

thus undergoing strong orbital displacement and making the $m > 2$ modes lose integrity[61] . Hence Kalnajs concluded that his "formulation of the problem" shows a dynamical preference for two-armed spirals and "gives little insight of what to expect in both the shape of the disturbances and their time dependence when $m > 2$" (Kalnajs 1963, p.13).

A summarizing exposition of the subject Kalnajs gave in his PhD thesis "The Stability of Highly Flattened Galaxies" presented at Harvard in May 1965 (Kalnajs 1965);[62] it contained an extended discussion lavish in ideas and technicalities. At the same time, the thesis became in fact Kalnajs' official public debut, so that to it as a reference point should we attach chronology when confronting certain factual points in the spiral history of the 1960s.

III. THE LIN-SHU THEORY

> I would like to acknowledge that Professors Lin and Toomre of MIT are also interested in the problem of spiral structure, and that I have benefited from discussions with them as well as their students.
>
> *Kalnajs 1963, p.13*

3.1 Working hypothesis and semi-empirical theory

> In hindsight, considering the crucial influence that the Lin & Shu (1964) paper had on the thinking of astronomers, it is only regretful that Lin did not decide (with or without me) to publish even earlier, because he certainly had all the physical ideas contained in our paper well before 1964.
>
> *Shu 2001*

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[61] A combination $\omega - m\Omega$ is called the Doppler-shifted wave frequency, one reckoned in a reference system corotating with disk material. The shift is due to the fact that waves are naturally carried along by flows.

[62] Kalnajs' thesis committee members were Layzer, Lin and Toomre, as officially confirmed from Harvard.

While Toomre, Hunter and Kalnajs had already presented their first results in the dynamics of flat galaxies, Lin still kept on thinking over the spiral problem.[63] Astronomers in Princeton had convinced him that, despite Chandrasekhar's criticism of Lindblad's theories,[64] the idea itself of a long-lived, shape-preserving spiral pattern is consistent with Hubble's classification system that relates spiral features with a galaxy's morphological type, its steady characteristic, thus suggesting that the spirals are steady as well. This view reminded Lin of wave modes in fluid flows that he had been studying for years back.[65] On purely heuristic grounds, discrete spiral modes seemed to him very reasonable as the natural result of wave evolution, and, if so, the patterns released might be associated with *slowly growing* or *neutral* modes. Lin raised this premise to the rank of working hypothesis, and around it as the nucleus he set to develop a *semi-empirical* theory.[66] It was seen to follow best the "urgent assignment from the astronomers [...] to make some specific calculations" and "to demonstrate the possibility of the existence of quasi-stationary spiral modes from the theoretical point of view [...] with understanding of the dynamical mechanisms relegated to a secondary and even tertiary position" (*Lin*).[67,68]

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[63] Lin's basic themes still were in hydrodynamics (e.g., Benney & Lin 1962; Reid & Lin 1963).

[64] That criticism (Chandrasekhar 1942) concerned only the asymptotic-spiral theory, and it was itself not flawless as attached to confusing empirical data of the 1920's – 30's.

[65] "I have been thinking of modes ever since I learned about the fine points of the Hubble classification". (*Lin*)

[66] "I adopted the empirical approach because of my close contacts with the observers (and with Lo Woltjer). Now that I have thought over the situation some more, I think I should admit that it is probably true that my past long-standing experience in the studies of hydrodynamic instability did (as you hinted) play a role in my thinking (although I was not conscious of it). But more important, I also feel (upon reflection) that the reason I adopted the empirical approach is really the natural consequence of my past education. My undergraduate education was in physics (at Tsinghua University of China, where all the major professors in Physics had doctorate degrees from English speaking universities such as Harvard, Caltech, Chicago and Cambridge), with all the pleasant memories of doing the experiments with precision and the satisfaction of having the data checked against theory. My graduate education was primarily at Caltech where I studied under Theodore von Karman. It is also there that I took a course from Fritz Zwicky who first identified the regular spiral structure in the Population II objects of the Whirlpool M51". (*Lin*)

[67] "Despite of my decades of experience with instability of shear flows, I did not bring these matters into the presentation of the 1964 paper, but commented only vaguely about instability. [...] There was no shortage of theoretical astronomers who understood the mechanisms perhaps better than I did; e.g. Lo Woltjer and Donald Lynden-Bell and perhaps even Peter Goldreich (even at that point). Goldreich turned out be the most successful leader in the understanding of the density waves in the context of planetary rings". (*Lin*)

[68] "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 LS64". (*Shu*)

> "The conclusion in the working hypothesis is *not proved or deuce*, but supported by an accumulation of theoretical analysis and empirical data. The adoption of this working hypothesis is a very important step in the development of a theory of spiral structure. It means that the authors are committed to back it up with the comparison of subsequent predictions with observational data." (*Lin*)

The coauthor to share Lin's fame and commitment was his student Frank Shu (Shu 1964)[69] who "found it remarkable that a scientist trained as a professional mathematician would place higher priority on empirical facts than deductive reasoning" and believed that "it was this broad-mindedness and clear vision that gave Lin a considerable advantage over his many competitors of the period" (*Shu*).[70] The Lin and Shu paper "On the spiral structure of disk galaxies" (Lin & Shu 1964, hereinafter LS64), in which "they first demonstrated the plausibility of a purely gravitational theory for density waves by a continuum treatment" (Lin & Shu 1966, p.459), appeared in August 1964.[71]

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[69] "All the original ideas were C.C. Lin's, and my original contributions were mainly to check the equations that he wrote down and posed as problems. (I did find a way to derive the asymptotic relation between density and potential by attacking the Poisson integral directly, but even there I initially blundered in not realizing the necessity of an absolute value on the radial wavenumber. The final derivation presented in the appendix of LS64 is due to Lin). I did considerable reading, however, on the astronomical side and may have contributed some ideas concerning how OB stars form and die in spiral arms. (This was the beginning of my lifelong interest in star formation.) Lin was indeed quite generous to include me as a coauthor on LS64, and I will always be grateful for his guidance and support of a young (I was 19 at the time) undergraduate student". (*Shu*)

[70] "Lin undoubtedly encouraged many of his younger colleagues – like Alar Toomre – to think about the problem of spiral structure. I can only imagine that Lin's treatment of people then much more junior than himself was equally as generous as his treatment of myself. Certainly, he must have discussed with Alar Toomre (and later Chris Hunter) his ideas about this problem. Toomre's early papers on the subject acknowledge this debt of introduction and inspiration. Why then did those early papers not carry Lin's name as a coauthor? I do not know, nor would I dare to probe (by asking either Lin or Toomre) for fear of opening old wounds that are best left closed". (*Shu*)

One way or another, no alliance was formed between Lin and Toomre. They "diverged in emphasis from the very beginning", so that "there were discussions, but no real collaboration" (*Lin*). As in agreement with this Toomre recalls that back again at MIT in spring 1963 he did decline Lin's "astonishing suggestion to write some such paper jointly, since he himself had contributed almost nothing very concretely to my gravitational (in)stability insights, and yet also since I likewise felt I had added next to nothing to his own spiral-wave hopes" (*Toomre*).

[71] That the historical Lin & Shu article was referred to as 'Lin's (1963) preprint' by Layzer (1964) and as 'Lin (1964)' by Toomre (1964) and Kalnajs (1965) as it was about to appear in the fall of 1964 speaks of its urgently extended coauthorship as Lin's last moment decision (so striking for a well-motivated and ambitious scientist).

Anyway, the Lindblad (1964) paper, also considering quasi-stationary circulation and the resulting spirals in differentially rotating galaxies, appeared half a year *prior* to Lin's patent. The authors had neither contacts nor fresh news on each other's most parallel work, and hardly could have it. "There was no justification to trouble B. Lindblad with a

The paper considered small non-axisymmetric disturbances to a razor-thin cold disk and found for them, through the governing hydrodynamic and Poisson equations, wave-like solutions of the type

$$\psi(r,\theta,t) = Re\{\varphi(r)\exp[i(\omega t - m\theta]\}, \quad \varphi(r) \equiv A(r)\exp[iS(r)], \qquad (12)$$

each specified by its eigenfunction $\varphi(r)$ and a pair of eigenvalues ω and m. For further advancement, the WKBJ-method was applied. It is valid for the case of phase $S(r)$ varying with radius much faster than amplitude $A(r)$, which features the *tightly wrapped* spirals, ones of small pitch angle between the circumferential tangent and the tangent to the constant-phase line

$$\omega t - m\theta = const. \qquad (13)$$

Depending on the sign of a radial-wavenumber function $k(r) = -\partial S/\partial r$, the spirals are trailing ($k > 0$) or leading ($k < 0$) (Fig.8). With $A(r)$ expanded in a series over a small parameter tan$i = m/kr$ (i being the pitch angle), the problem is solved to the lowest, i-independent order neglecting the azimuthal force component of spiral gravity. In this case, both leading and trailing arms act as just rings, so that the ensuing dispersion relation

$$\nu^2 = 1 - |k|/k_T, \quad \nu \equiv (\omega - m\Omega)/\kappa \qquad (14)$$

substantially repeats Toomre's equation (5) for radial oscillations. Importantly, relation (14) is valid for Re$\{\nu^2\}\leq 1$. This restricts the radial span of the WKBJ solutions, and in the neutral case Im$\{\nu\}= 0$ they gain the territory between the Lindblad resonances determined by Eqn (11) and equating the angular speed of an $m-$armed spiral pattern to a combination

$$Re\{\omega/m\} \equiv \Omega_p = \Omega(r) \mp \frac{\kappa(r)}{m} \qquad (15)$$

with the minus/plus sign discriminating, respectively, between the ILR and OLR. The two-armed spirals thus seem preferred as best covering an entire disk (Fig.9).

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novice being converted, Lin explains. I was waiting for a definitive new prediction before writing to him. Even then I would have done it through P.O. Lindblad for several obvious reasons. Unfortunately, by the time our result came out (IAU Symposium No 31) [see Sect.3.2] he already passed away" (*Lin*). Even less probable was any contact-making step from the other side. "About that time [fall of 1964] my father was on a trip around the world caused by the inauguration of the Parkes telescope in Australia, P.O. Lindblad recalls. On his way home he passed through the US [. . .] but he brought no news about density wave theories. [. . .] I think my father was aware of the existence of the LS64 paper but had not had the time to penetrate it. I know that he was happy to learn from Whitney Shane, who visited us around the beginning of June 1965, that his work on spiral structure had been more and more appreciated recently". (*P.O. Lindblad*)

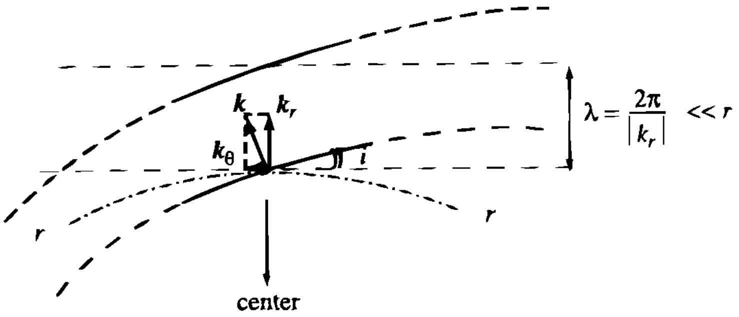

Figure 8: *The WKBJ approximation and the tightly wrapped spiral waves.* $k_r \equiv k$ and $k_\theta \ll k$ are the components of the local wavenumber $\boldsymbol{k}$. $\lambda = 2\pi/k$ determines the radial interarm spacing; it is small compared to the galactocentric distance r since $kr \gg 1$ (which is equivalent to small pitch angles $i \ll 1$).

Such was the mathematical basis of the original Lin-Shu density-wave theory, called elementary by its authors any later (e.g. Bertin & Lin 1996, p.229). It treated wave quantities Ω_p, γ, and m as free parameters burdened with no dynamical imposition, which made the theory so comfortable in imitating spiral grand designs by means of the curves $r(\theta)$ given by

$$m(\theta - \theta_0) = -\int_{r_0}^{r} k_T Re\{1 - \nu^2\} dr \tag{16}$$

and obtained through the integration of expressions (13) and (14). Sure, the results of this procedure were controvertible, already because the *fast*-growing waves – exactly those examined in LS64 – ruled out the proclaimed quasi-stationarity.[72] But the authors hoped that random motions, excluded from their analysis, would in fact stave off disk instability as definitively as to impose a state of near-stability open for *slowly* growing modes until a small but finite amplitude.

Toomre (1964) had reflected already on such a state of $Q \cong 1$ as settling *once* all over the disk-like stellar Galaxy, but yet he found it stable *still*, at least in our solar region. As a counterpoise, Lin with Shu diagnosed instability for another region, at about $r_0 = 4 - 5$ kpc from the center. With that, they pictured "a galactic disk, which is in part stable and in part unstable" and suggested "the possibility of a balance resulting in a neutral density wave extending over the *whole* disk and having a scale of the order of (but smaller than) the distance between the stable and unstable regions"

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[72] To soundly fit the empirical 2-3 kpc local-arm spacing in the Milky Way, LS64 chose a combination of angular speed Ω_p = 10km/s/kpc and growth rate γ = 50km/s/kpc (!) for their tentative two-armed spiral.

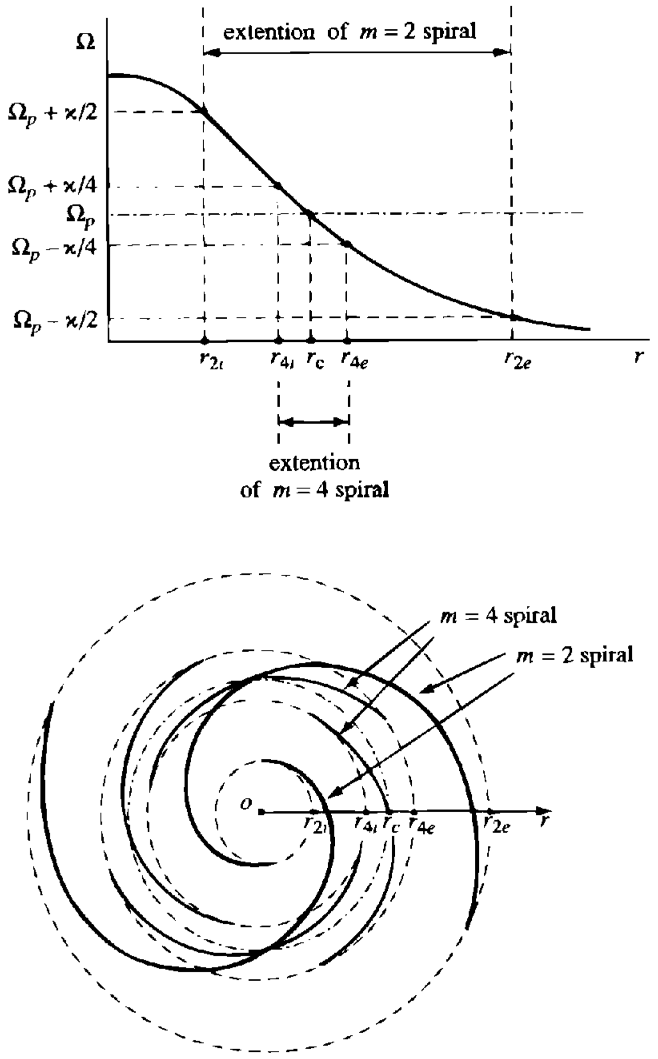

Figure 9: *The Lindblad resonances as confining the region accessible for the tightly wrapped spiral waves.* (*a*) – a rotation curve for a galaxy disk and its corresponding corotation and $m = 2$, 4 Lindblad resonances; (*b*) – the co-scaled view of the two and four armed tightly wrapped spirals.

(LS64, p. 651). It was this "suggestion of the possibility" that summarized Lin's early reflections and made his basic working hypothesis originally sound as a statement that

> "the total stellar population, which has various degrees of velocity dispersion, forms a *quasi-stationary spiral structure* in space of the general nature discussed above" (LS64, p.651).

As we can see, this statement hinges almost entirely on the opinion that, for our galactic disk to be equally stable at that r_0, the velocity dispersion must there exceed $c_{r,\min} \cong 80 \pm 10$ km/s, which cannot be the case, else "a considerable number of stars with high radial velocities would reach our neighborhood from the interior part of the Galaxy, contrary to observational evidence" (LS64, p.651). But was this opinion (the authors never repeated it) strong enough? First, it meant an inconceivable situation when some *massive* portion of a stellar galaxy remains *unstable* during all the period of formation in it of a global quasi-*steady* pattern. Secondly, and most important for astronomers, it had – already in 1964 – grave objections to the fact that the largest epicyclic deflection of the Lin-Shu "high radial velocity stars" from their 'home' radius $r_0 = 4 - 5$ kpc, equaled to $\Delta r \cong r_0 c_r / V_0 \sqrt{2}$, was in frames of Schmidt's model (cited in LS64) $1 - 1.5$ kpc only – too little to let those stars even come close from r_0, if not reach us. We find that the original QSSS hypothesis of Lin and Shu, called nowadays "a preliminary formulation" *only* (Bertin & Lin 1996, p.80), rested on a rather weak basis, both dynamical and empirical.

Very interesting in LS64 is the authors' notice on what had made their work get to print so urgently. A passage following their opening discussion of "at least two possible types of spiral theories", one of which "is to associate every spiral arm with a *given body of matter*" and the other "is to regard the spiral structure as a [quasi-steady] *wave pattern*", reads:

> "Toomre tends to favor the first of the possibilities described above. In his point of view, the material clumping is periodically destroyed by differential rotation and regenerated by gravitational instability.[73] [...] The present authors favor the second point of view [...] Since A. Toomre's (1964) point of view has been published, it seems desirable to publish our point of view even though the work is not yet as complete as the present writers would wish to have it." (LS64, p.646)

This puzzles. Although it is true that from about 1962 onward Toomre suspected – much as Lynden-Bell had already done in his thesis two years earlier, as it turned out – that at least the more ragged-looking spiral structures result primarily from recurrent gravitational instabilities in the plainly dissipative gas layer of a galaxy (*Toomre*), there was no explicit discussion of any such suspicions in T64 as actually published. One cannot help but think that this accentuated mention of 'Toomre (1964)' was more than just a mistaken reference, that actually it betrayed the influence that at least the cited paper had on Lin.

> *Shu*: "Here, I can only speculate, because certainly my foresight then was not as sharply developed as Lin's. Nor was I privy to the developing estrangement between him and Alar Toomre. [...] Lin had been thinking about the

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[73] "The prevalent thinking among the other prominent theorists of the time – and this included Alar Toomre – was that spiral structure was a chaotic and regenerative phenomenon – 'shearing bits and pieces', as Alar later put it in one of his papers". (*Shu*)

> problem of spiral structure nonstop since the Princeton conference in 1961. But he had a world-renowned reputation to protect and therefore was loathe to publish anything hasty before he had worked out his ideas mathematically to his satisfaction. [...] Lin (and later, I) felt strongly that spiral structure was, in essence, a normal mode. But by all the standards of what was then known, a normal mode could not be spiral (unless it grew ridiculously fast). Nevertheless, Lin felt sure that one should not do the naive thing of superimposing equal trailing and leading parts when the wave frequency is (nearly) real. And he probably wanted to discover the reason why before publishing anything. Alar's 1964 paper triggered him into premature action". (*Shu*)
>
> *Lin*: "The urgency in my submittal of our paper was to present a *different* perspective, not to fight for priority". "After reviewing the paper again, I think I could not have done much better or even any better". (*Lin*)

One way or another, we see that by 1964 Lin indeed had had several thoughts and feelings about spiral modes, and he was eager about gaining power to his perspective. At that, he knew of a growing optimism with shearing or evolving density waves[74] and, as well, of the parallel wave-mode interest at Harvard. The T64 paper[75] , apart from its engagements on disk stability, did mention Kalnajs' advancing efforts and, still more glaringly, it also mentioned and already *discussed* Lin's yet unpublished solutions.[76] This must have put Lin in a position to urgently patent his views, albeit makeshift in argument for want of better mathematics, and in so doing he rather awkwardly exhibited the opponents' preoccupations as an alternative already placed on record.

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[74] Goldreich and Lynden-Bell in England and Julian and Toomre at MIT set to work on this by 1964.

[75] The revised version of T64 was submitted in January 1964.

[76] Toomre concluded that "whatever differences there may exist between the shorter axisymmetric and non-axisymmetric disturbances, these must in essence be due only to the circumstance of *differential* rotation" (T64, p.1223). In Lin's hands, in contrast, this 'circumstance' still allowed the dispersion relation (14) for non-axisymmetric waves to be rather close to its axisymmetric analog (5), although the waves stood as steady-mode solutions of the WKBJ type. Yet, as well, the governing equations admitted an "altogether different family of approximate non-axisymmetric solutions" (T64, p.1223), with the radial wavenumber proportional to the disk shear rate A(Oort's constant), and growing with time, $k_r \propto At$. This meant that a spiral disturbance of the leading form ($t < 0$) unwrapped, started trailing, and then wrapped tighter and tighter ($t > 0$). Thus the point was that, on the one hand, differential rotation continuously deforms even the tightly-wound spiral waves of this sort, whereas, on the other hand, these "should probably be regarded as particular superpositions of Lin's solutions" (T64, p.1223). This discordance was thought to be removed by a fuller analysis beyond the WKBJ-limit.

3.2 A definitive (?) new prediction

> A desirable feature of the WKBJ waves is their mathematical simplicity; their physical relevance to the 'grand design' of a spiral galaxy is less transparent.
>
> *Kalnajs 1971, p.275*

"Just how much did Kalnajs' study of axisymmetric oscillations influence our work? The simple answer is: very little, if at all" (*Lin*). Such is Lin's judgment regarding the results he had set out in the summer of 1965.[77] Those got out of the printer in no less than one year (Lin 1966, 1967a), but an abridged and slightly updated version appeared as soon as February 1966, having become an "Outline of a theory of density waves" by Lin and Shu (1966), labeled 'Paper II'.

The three issues reported a WKBJ-styled dispersion relation for the razor-thin hot disk,

$$\nu^2 = 1 - |k|/k_T \cdot F_\nu(x),$$

$$F_\nu(x) = \frac{1-\nu^2}{x}\left[1 - \frac{\pi\nu}{\sin\pi\nu}\frac{1}{2\pi}\int_{-\pi}^{\pi} e^{-x(1+\cos s)}\cos\nu s ds\right]. \qquad (17)$$

From its Kalnajs' axisymmetric analog (9)-(10) it differed in the Doppler shift included in ν and in the form of the reduction factor $F_\nu(x)$.[78] It was an idea of some such dispersion relation, Lin and Shu (1966) remarked, that

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[77] Lin presented his first hot-disk results in June 1965 at a summer school at the Cornell University and at a mathematical symposium at the Courant Institute. These materials were published in two extensive articles (Lin 1966, 1967a) submitted in July. "I recall becoming aware of the relationship with the work of Kalnajs only when he brought up the issue in connection with Frank Shu's thesis presentation. I immediately recognized that there would probably be a way to make the connection through the application of the Mittag-Leffler theorem. Note that it is easy to derive the Kalnajs form from our integral form, but difficult to reverse the process. And our numerical calculations depended on the simple integral, since it was a time when large scale use of the computer was not yet available in a mathematics department. (I still remember the painful experience when my request – as chairman of the committee on applied mathematics – for a computer was turned down, even though the department had the funds. [...] Kalnajs might have been able to check the calculations with his infinite series through the use of the computer.)" (*Lin*)

[78] "I have little knowledge but I make this conjecture: Kalnajs was studying axisymmetric oscillations, not standing waves of the spiral form, and obtained his results through the use of results for analogous oscillations in plasma waves. (I learned a lot about plasma physics only after Y.Y. Lau joined our research group.)" (*Lin*)

had fed originally (LS64) their insight in the disk-stabilizing role of random motions.[79]

But an important dynamical, not chronological, point was that the hot rotating disk was seen to conduct radial and spiral waves rather distinctly. Given a state of marginal stability, the oscillatory radial neutral mode $\nu = \omega/\kappa = 0$ is well maintained by it along its medium radii (dying out at large r's),[80] the local wavelength function $\lambda_0(r)$ depending on mass and angular momentum distributions. In contrast, the spiral wave cannot be neutral as extendibly: its Doppler-shifted frequency $\omega - m\Omega(r)$ gets $r-$dependent. This ties the neutrality condition $\nu = (\omega - m\Omega)/\kappa = 0$ to a narrow corotation zone of $r \cong r_c$, and there only can the interarm spacing $\lambda(r)$ equal $\lambda_0(r)$, the rest of disk getting more and more stable against the wave as one travels away from r_c in or out. If so, why not to try to juxtapose the basic Lin-Shu concept of a balance and the solar-region stability inference by Toomre? For this, it seems sufficient to send corotation way beyond – to an outer disk region supposedly as permissive to marginal stability as to admit it – and to cancel all instability inside that r_c in favor of $Q \geq 1$. Lin and Shu did seem to have followed this way. Moreover, they adopted a $Q \equiv 1$ model (discussed already in T64), being captured by a picture of overstability, i.e. gradient instability held to mildly develop over the system and to provide some selective amplification of trailing, not leading, waves.

Besides, relation (17) tells $\nu(k)$ to decrease with wavenumber till k remains under some k_0, and then to rise up at $k \to \infty$ back to unity. Any intermediate value of ν is met thus twice, meaning two branches of WKBJ solutions, the shorter- and the longer-wave ones, their forms $r(\theta)$ being provided by equation (16) with $F_\nu(x)$ added in the integrand denominator. If $Q \equiv 1$, the branches join at corotation, showing there equal interarm spacings $\lambda_{sw}(r_c) = \lambda_{lw}(r_c) = \lambda_0(r_c)$. This value is the largest (smallest) for the

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[79] Lin agreed that the dispersion relation was already derived by Kalnajs "in the special case of axially symmetrical disturbances", but "by a quite different method" and "independently of the work of the author" (Lin 1966, p.902). He certainly appeared rather sensitive on the point of independence, beginning his spiral studies. His *first* appraisal of Lindblad's long-term emphasis on steady spirals was: "Indeed, independently of each other, B. Lindblad (1963) and the present writer came to the same suggestion of a *quasi-stationary spiral structure of the stars* in a disk galaxy" (Lin 1966, p.898). Again, referring time and again to different methods adopted by him and his various competitors, Lin found it difficult to closely compare those related issues. But, for example, Lynden-Bell (1962) and Toomre (1964) had used the same characteristics method as that taken in 1965 by Lin, with which he basically re-derived, again independently, this time from Toomre, that crucial differential equation of 'asymptotic' disk-stability and density-wave theories (cf. Eqn (53) from T64 with Eqn (7.15) in Lin 1966 and Eqn (A20) in Lin et al 1969), not having mentioned its factual use by his next-door institute colleague.

[80] Such behavior is well seen on Fig.3 from T64 showing results of numerical calculations of global radial modes for some illustrative cold-disk model.

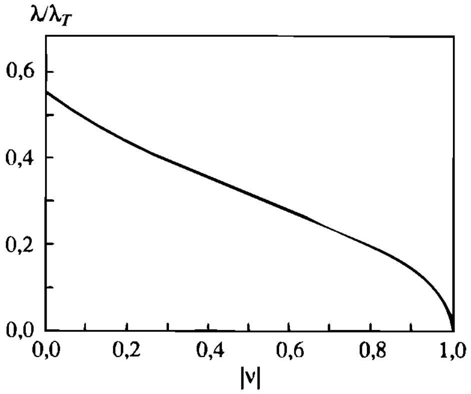

Figure 10: *The short-wave branch of the dispersion relation (17) for a $Q = 1$ disk model.* (The figure is reproduced from Lin & Shu 1967)

shortwave (longwave) branch: $\lambda_{sw}(r)$ falls down until zero ($\lambda_{lw}(r) \to \infty$) as one goes from corotation to ILR. Aimed from the outset at explaining the observed 2-3 kpc local spacings, Lin got tempted to acknowledge the shortwave branch, the more so as, not to forget, in 1964 he had had no choice when having to comment on this same gas-given spacing on the basis of relation (14) that seized but *one* – long-wave (!) – branch.[81] But things did not get all as clear by 1966, and this is why neither Lin (1966, 1967a) nor Lin and Shu (1966) were eager to go into the wave-branch question, keeping silent about any graphic view of their newer formula. Only at the Noordwijk IAU Symposium (August, 1966) they gave a graph, it displayed the *short*-wave-branch extension of the $\lambda(\nu)$ curve (Fig.10) on which they built a model for the full spiral of our Galaxy (Fig.11), tentatively two-armed and answered by a remote corotation (Lin & Shu (1967).[82] Spirals of this class show as slow a rotation as to almost guarantee the ILRs be present and lie in a relative proximity from the center. Namely, Lin and Shu connected our 'home' $m = 2$ ILR with the '3-kpc arm' which fixed the spiral pattern speed $\Omega_p = 11$ km/s/kpc.

> "My earliest recollection of realizing that there were separate long and short branches came when I was doing the numerical calculations for the spiral pattern that Lin wished to show at the Noordwijk symposium. As I recall, he was in the Netherlands and I remained behind at Harvard, and we corresponded by mail. I was considerably confused by which of the two branches

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[81] LS64 had assumed that because not all the stars but only those with smallest random velocities perceptibly contribute to the response of a disk, its effective surface density must be several times less than its full value.

[82] "This was my first meeting with the distinguished astronomers who made all the important observations related to spiral structure, many of whom worked under Oort's direction. Here we presented our first prediction of the spiral structure of the Milky Way, which remained to be an approximate representation, as indicated by Yuan's continual refinement over the years". (*Lin*)

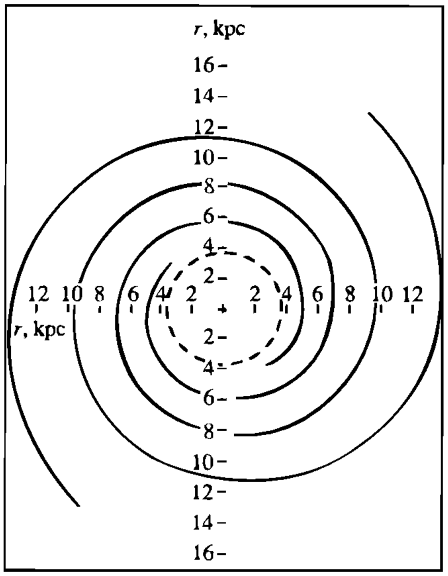

Figure 11: *The Lin-Shu model for the Galactic spiral density wave.* The model is calculated with the help of the dispersion curve in Fig.10. The dashed line shows the ILR region taken to be the residence of the '3-kpc arm'. This provides the pattern speed $\Omega_p = 11$ km/s/kpc. (The figure is reproduced from Lin & Shu 1967)

> should be used to generate spiral patterns (I had realized that a 'reduction factor' applied to our 1964 formula was an incomplete description, and that long and short waves were implicit to Toomre's evaluation of a critical Q for axisymmetric disturbances). Finally, Lin suggested that we should simply choose the short branch by fiat as the practical thing to do given the press of the Noordwijk presentation, and we were left to try to sort things out later. That's my memory of the events".[83] (*Shu*)

The Noordwijk diagram has been the first presentation of our Milky Way's density wave.

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[83] "Lin and Shu 1966 emphasis upon (and the dispersion relation for) the *short-wave* branch of nearly axisymmetric WKBJ-style density waves, which is something that Kalnajs (1965) also knew from his thesis but failed to emphasize nearly as adequately, escaped me altogether even though the same for the *long-wave* branch as well as the stability criterion were plain as day from T64 – and to a more limited extent even from Safronov (1960a,b), as I often agreed in retrospect. I think my trouble was that my own ongoing work then with Julian (Julian & Toomre 1966) [...] had also sensitized me to the severity of *phase mixing*. [...] Looking back, this made me suspect until well into 1965 that all *short* stellar-dynamical waves, unlike their over-idealized gas equivalents, would in fact be strongly damped and were probably not of much value. And right there I have cheerfully agreed for about 34 years now that Lin and Shu (and as an independent authority also Kalnajs, not at all to be omitted) together proved me to have been spectacularly *wrong*". (*Toomre*)

Afterword

As we have seen here, understanding the spiral structure of galaxies took many twists and turns even in the hands of Bertil Lindblad who seems rightly regarded the main father of this whole subject. By the early 1960s, with the arrival of computers, plasma physics and several fresh investigators, it entered a new period 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 undoubtedly C.C. Lin, whose 1964 and 1966 papers with Shu had a big and immediate impact upon other astronomers, at least as a welcome sign that genuine understanding of the spiral phenomenon seemed in some sense to be just around the corner.

In retrospect, even Lin occasionally let himself get carried away with too much enthusiasm as for instance when he wrote in his 1967 review article that his relatively exploratory work with Shu had already led to a "theory free from the kinematical difficulty of differential rotation", or that it "enables us to provide a mechanism to explain the existence of a spiral pattern over the whole disk while allowing the individual spiral arms to be broken and fragmentary" (Lin 1967b, p.462). Already at the time such optimism was not entirely shared by other experts. And by the late 1960s – as we shall see in Paper II – 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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