GRAVITATIONAL INTERACTIONS BETWEEN GALAXIES

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Abstract. Recent theoretical studies of the consequences of fierce tidal interactions between galaxies are reviewed and compared.

1. Introduction

Assuming gravity has not deserted us over modest intergalactic distances, one might be tempted to claim all its effects on any interplay between galaxies to be no more than vastly scaled-up analogues of the familiar celestial mechanics that involves moons, planets and suns. Certainly there is a lot to be said for such intuition – and yet one must not trust it too far. The plain truth is that nothing in our usual experience (not even from theories of contact binary stars) has really prepared us adequately for those rare but by no means unimaginable situations where two or more galaxies either graze each other or actually interpenetrate in their respective orbits. Moreover, there is growing reason to believe that many such encounters take place at speeds only comparable to the mutual speed of escape; then already the simplest estimates suggest that the resulting mechanical damage must indeed be large. And finally, even if we were intimately familiar with the behaviour of lesser celestial bodies under similarly fierce but brief external forces, one could still protest that galaxies are only loose confederations of stars and other material endowed with a variety of separate orbits. Such assemblies may react very differently from the rocky or gaseous objects.

In short, of the many aspects of the dynamics of multiple galaxies, it seems most clearly the *violent tides* arising from very close passages which require further and probably quite extensive analyses. With one notable exception from a decade ago, calculations of such severe distortions have only lately begun to be available – and this review now summarizes what has emerged and what has not. First we focus on that seemingly age-old issue of whether at least some of the pronounced spiral galaxies owe their present appearances to the tidal forces of neighbours. Next we touch on some recent and perhaps more surprising demonstrations that certain narrow 'streamers', 'filaments' and 'tails' of peculiar galaxies may indeed have had such tidal origins. And thirdly we also ruminate whether some yet more bizarre-looking specimens in the sky might not represent actual mergers of galaxies which experienced especially severe tidal friction not very long ago.

2. Tidal Spirals

The idea that gravitational interactions might yield spirals goes back at least to Chamberlin (1901). Already he reasoned that any roughly parabolic close passage of

John R. Shakeshaft (ed.), The Formation and Dynamics of Galaxies. 347–365.

two comparable masses would subject either body to a fierce tidal force that would not only be two-sided as usual but also rather sudden or impulsive. Hence in the absence of intrinsic strength, any "concurrent rotation must obviously give rise to a spiral form", and "there should therefore be two chief arms to the resulting spiral", and furthermore "these must be curved in a common direction by the rotation of the mass".

With this emphasis on sudden tidal damage followed by differential rotation of whatever cause, Chamberlin might almost have been commenting on Zwicky's (1956) four sketches of a hypothetical passage of two galaxies, here reproduced as Figure 1.

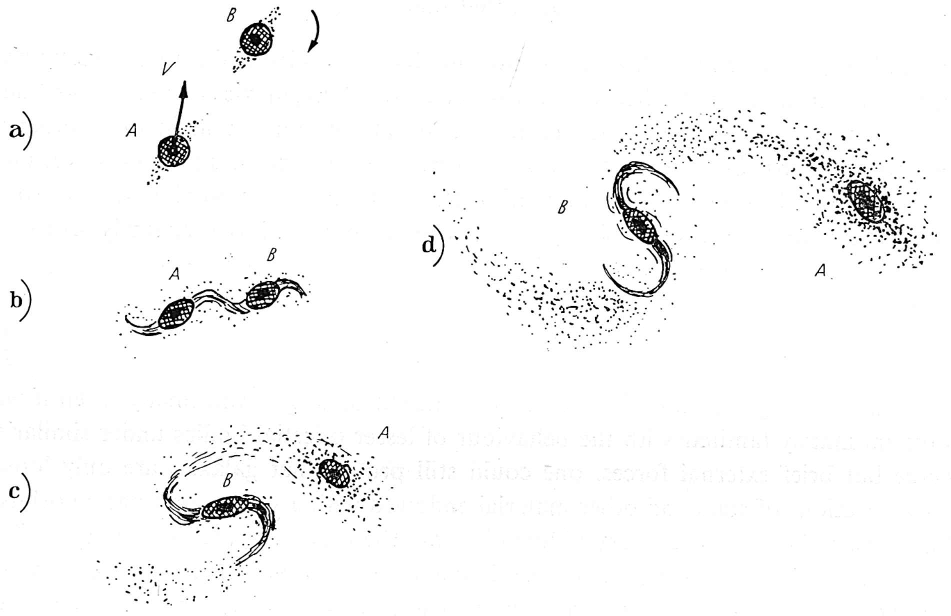

Fig. 1. "Schematic drawing of the possible formation of an intergalactic bridge between two galaxies passing each other", by Zwicky (1956).

It hardly matters that Chamberlin was in fact thinking only of an encounter between two stars: As presumed examples of such near-calamities, his article offered photographs of no fewer than six spiral nebulae including M51! Yet in retrospect even Zwicky's sketches reveal one startling need for calculations to buttress intuition: unlikely though it may seem today, both the tidal arm and counterarm of the clockwise rotating nebula *B* were evidently imagined by Zwicky to wrap themselves not in a trailing but a leading direction.

Of course, any serious numerical studies of the tidal spiral-making had to await the electronic computer – even though Holmberg (1941) briefly proved otherwise with his ingenious graphical integrations. Using the new computer, the most significant early work was undoubtedly that of Pfleiderer and Siedentopf (1961) and Pfleiderer (1963). Samples of their results appear in Figure 2.

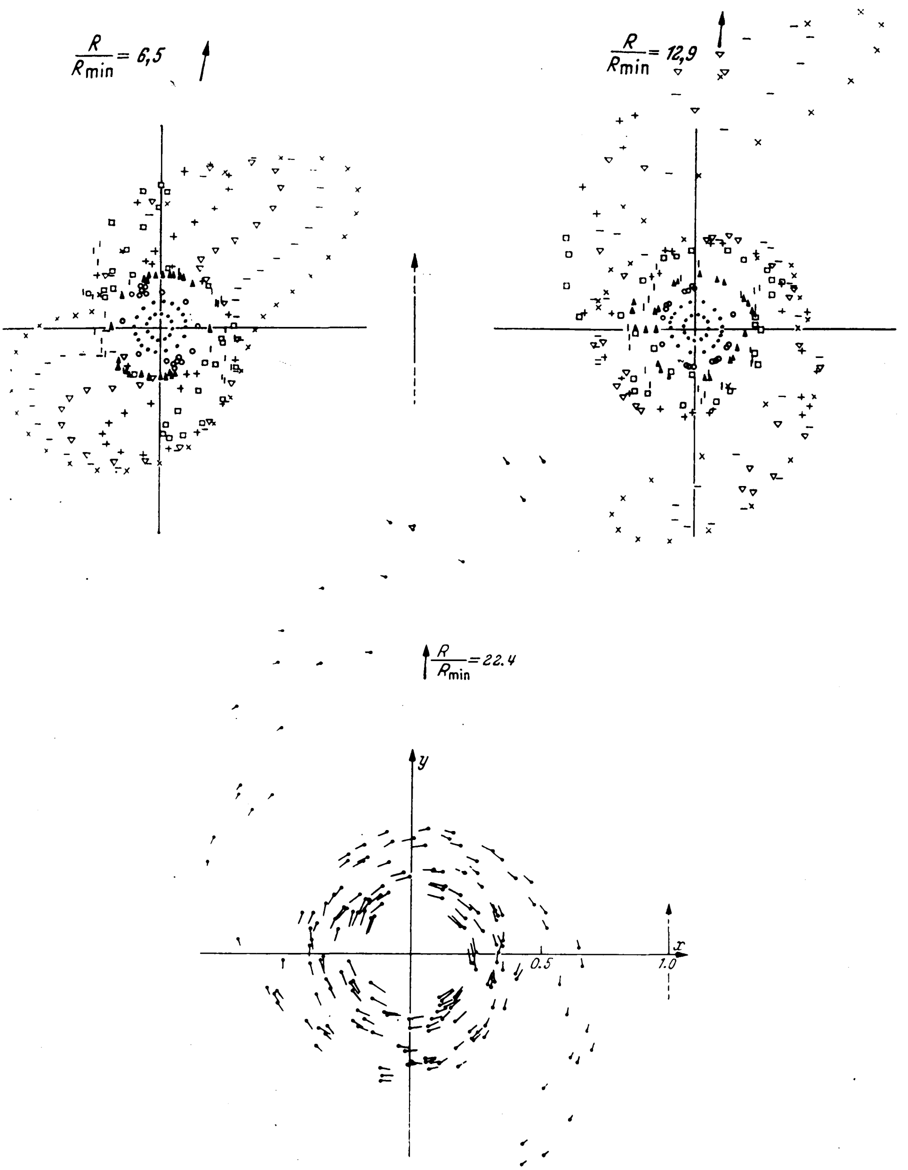

Fig. 2. Evolution of a counterclockwise rotating disk of test particles about a unit mass. The three views shown refer to elapsed times in the ratio 2:4:7 since the instant of closest approach of a three times heavier mass travelling upward on the right at a rate equal to four times its speed of escape. The slanting arrows point toward the present distant locations of that hyperbolic passer-by. The short line-segments in the bottom diagram depict the instantaneous velocity vectors of the various particles. The first two frames are from Pfleiderer and Siedentopf (1961), and the third from Pfleiderer (1963).

Pfleiderer and Siedentopf pretended boldly that all the mass of a galaxy could for these tidal purposes be thought of as concentrated at its very centre, or at least that its outer disk could be imagined to consist simply of a large number of non-interacting test particles orbiting initially in concentric circles. The main accomplishment of Pfleiderer and Siedentopf was to demonstrate outright that just this original orbital motion of the disk particles – rather than any fierce tidal torques envisaged by Chamberlin and implicitly also by Zwicky – could be relied upon to provide much of the differential rotation needed for the kinematic development (and later dispersal) of some rather impressive two-armed and *trailing* spiral structures after a tidal 'shock' of suitable duration and severity.

Strange to say, this inspired work then lapsed into almost total obscurity for the better part of a decade. Perhaps it was simply misinterpreted as an unpromising attempt to explain all kinds of spirals – even though Pfleiderer himself had expressly disowned that hope on the ground that sufficiently close passages of unbound galaxies seemed statistically much too rare. Or possibly the reason was that, despite Zwicky's and Vorontsov-Velyaminov's efforts especially in the 1950's, the roughly one-in-a-hundred abundance of strange tails or filaments in multiple galaxies, or of companions seeming to lie at the ends of spiral arms, was not yet appreciated nearly as widely as after the publication of Arp's (1966) *Atlas of Peculiar Galaxies*. And besides, Pfleiderer and Siedentopf themselves had offered no satisfactory bridges or tails.

Looking back, one key to producing good connected spirals was of course to slow down the passer-by. This point was first demonstrated in detail by Tashpulatov (1969;

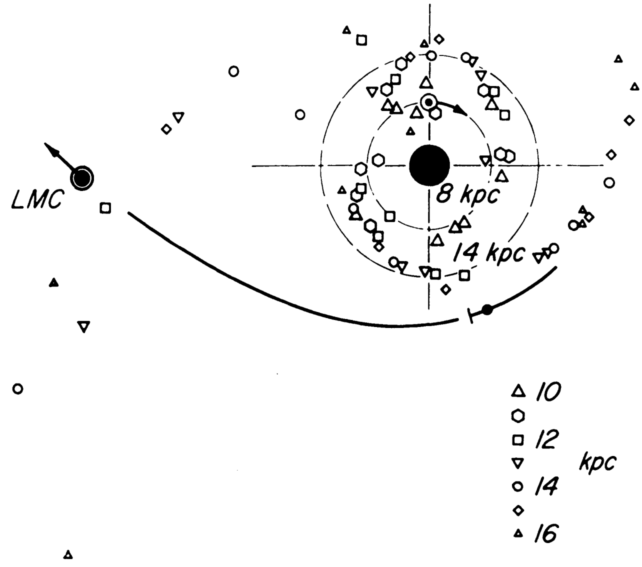

Fig. 3. Distortion of our Galaxy as the result of an imagined 36°-inclined direct and almost parabolic passage of a 3/16-mass Large Magellanic Cloud (Toomre, 1970). Only the hypothetical relative orbit and a central mass point have here been added for clarity.

see 1970 for some purported tails) and later by Yabushita (1971). But there was one slightly subtler key as well. As Figure 9 will soon attest incidentally, it is now known that passages in a direct sense of partners of roughly *equal* mass at speeds parabolic or slower tend either to accrete the near-side material too efficiently from whichever is to be regarded as the victim disk, or else they leave it much too splattered. Of the two logical alternatives which thus present themselves, the possible merits of using a distinctly more massive passer-by went largely unappreciated until the recent work of Clutton-Brock; more about that shortly. It was the other alternative of using a smaller mass which Toomre (1970) first stumbled upon, almost literally by accident.

To digress personally for a moment, Hunter and Toomre (1969), in their studies of the bending of this Galaxy by the vertical component of tidal force during a possible close passage of the Large Magellanic Cloud, were blissfully unaware not only of Pfleiderer and Siedentopf: they also did not realize the undue sensitivity – which those German authors had already implied – of any such disk to the horizontal components of the same tidal force during a *direct* encounter of low inclination. One can imagine my initial chagrin, therefore, when the kind of direct LMC orbit that had seemed preferable for bending proved, upon further checking with test particles mobile in all three dimensions, to yield the unacceptably spectacular commotion pictured in Figure 3!

Since then, that accidental recipe for making some fairly decent if only transient bridges and counterarms has been corroborated by Wright (1972), whose best example is probably the one in Figure 4. The same formula involving a small companion in a

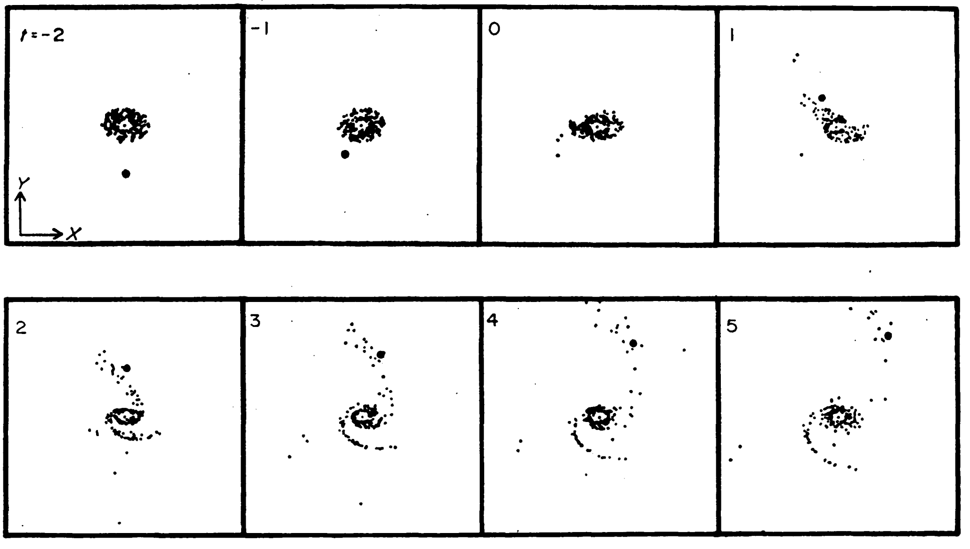

Fig. 4. Evolution of a disk of test particles during a 45°-inclined parabolic passage of a companion of one-quarter its own central mass. The view in this rearranged copy of Wright's (1972) Figure 7 is exactly normal to the orbit plane; that original diagram also shows simultaneous orthogonal views from two other directions.

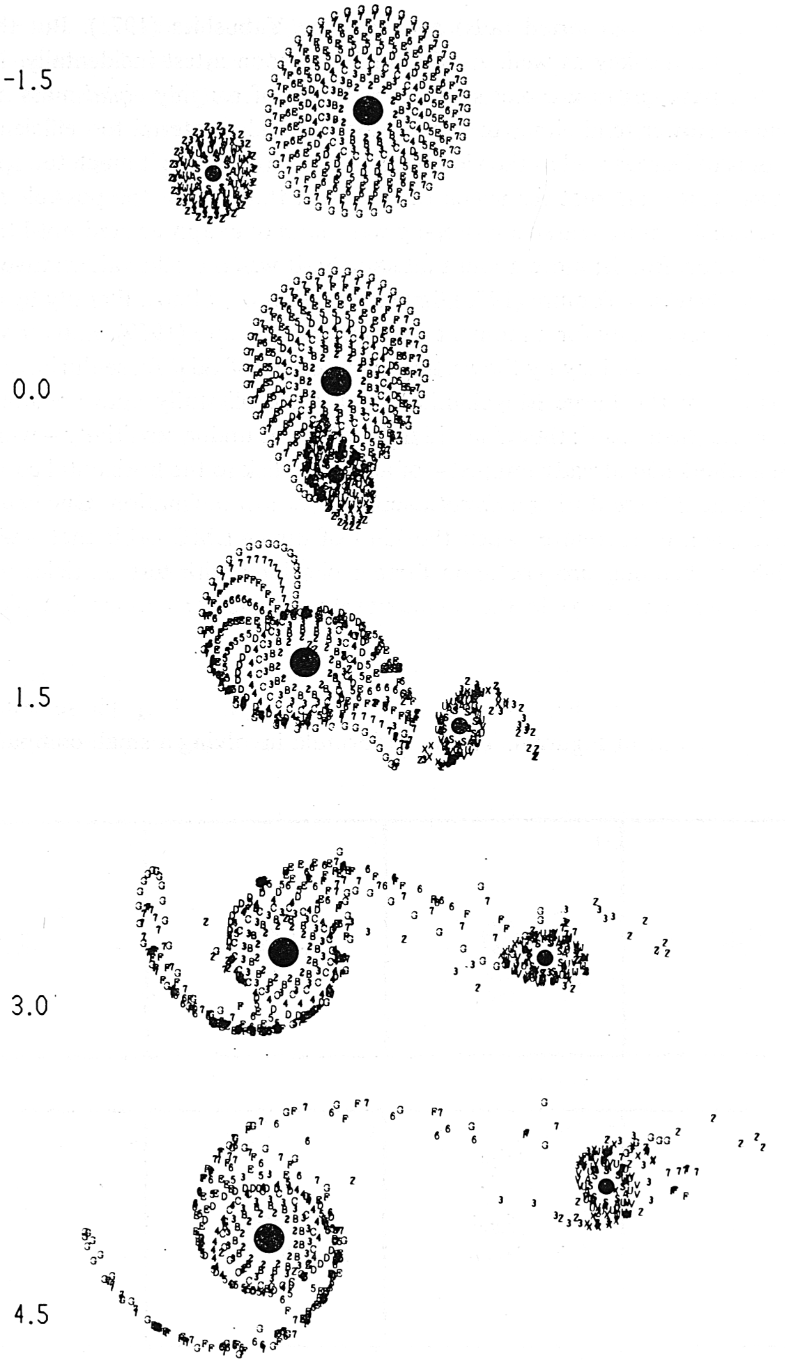

Fig. 5. Another 45°-inclined parabolic passage of disks of test particles with central masses in the ratio 4:1. Unlike in Figures 3 and 4, the closest approach of those masses here occurs at a position as far as possible above or below the spin plane of either disk; hence the two as-yet-mildly perturbed disks in frame 0.0 are still well separated in depth.

moderately-inclined slow orbit has also been explored extensively by Toomre and Toomre (1972; hereinafter 'TT'). However, rather than duplicate any of the latter material, Figure 5 presents an abbreviated time sequence from a related motion picture which my brother produced in early 1971; these movie frames have the advantage of also conveying the fate of the smaller disk.

Of course one should beware that diagrams like Figures 3–5 are biased toward the most picturesque. There is nothing very sinister about such bias, but it does mean that distinctly uglier and less striking tidal remnants should in reality be numerically predominant by at least an order of magnitude; fortunately, such a preponderance of the unsightly seems evident already in Sky Survey. More seriously, one should also note that those recent slight improvements on the tidally produced spirals of Pfleiderer and Siedentopf still suffer from a total neglect of both the self-gravity and any initial random motions of the particles. And most important, even if such crude computations do prove to have been reasonable, one should still not hasten to conclude that tidal spirals – though almost certainly seen in such examples as NGC 2535/6 and 3808 – constitute any more than some particularly striking exceptions among the answers to what is often abbreviated in the singular as 'the spiral problem'.

Speaking of those exceptions, it is sudden doubtful whether NGC 7752/3 belongs among the select few: Bertola and d'Odorico (1973) report that the line-of-sight velocities of the two members of that apparently connected pair differ by 340 km s^{-1}. However, as regards M51, the only tidal question which in my opinion remains grossly unanswered is whether any of the pronounced inner spiral structure of its larger member could be an *indirect* by-product of the tidal damage that seems undeniable near its periphery (cf. TT; see also Tully, 1972; Weliachew and Gottesman, 1973).

3. Tidal Filaments and Tails

Possibly the most compelling evidence that the two partners in M51 were indeed involved in some recent near-collision is not even the outer shape of NGC 5194. It is rather the general mess near – and in particular the two long and oppositely-directed streamers from – the satellite galaxy NGC 5195. Those two faint streamers may be viewed in the remarkable IIIaJ photograph by van den Bergh (1969) that has been reproduced in Figure 6. Although modelled only crudely in Figure 21 of TT, these filaments seem almost certain to represent the vaguely two-sided tidal damage inflicted upon the smaller galaxy by the larger, somewhat as in the present Figure 5.

Despite this one example that appears so incriminating, the systematic study of geometrical shapes obtainable when a small disk gets ripped fairly suddenly by the non-uniform gravity of a heavy mass during a roughly parabolic fly-by remains even more in its infancy than the reverse. The only important exploration of that nook of parameter space reported so far is one by Clutton-Brock (1972), who concentrated on the totally planar, parabolic encounters with heavier bodies either 8, 64 or 512 times the mass of the small model galaxy. Whatever Clutton-Brock's work may have lacked in quantity was more than offset by his interesting, if only approximate, inclusion of

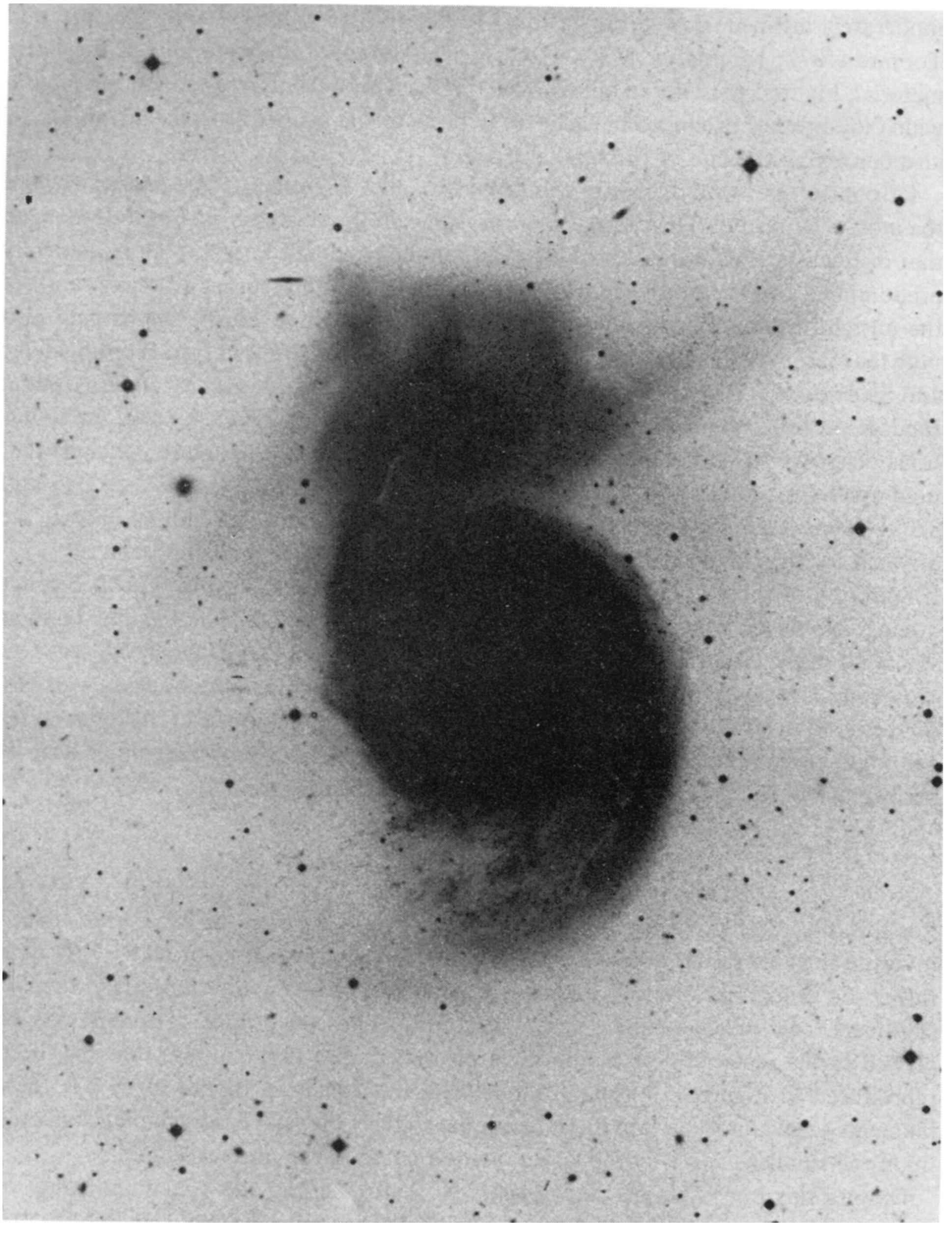

Fig. 6. **Photograph of NGC 5194/5, by van den Bergh (1969).**

the self-gravity of the disk stars, and by his assignment to those mass points of significant initial random velocities. He also retained a set of test particles, without such random velocities, to mimic at the same time the interstellar gas. Figure 7 is typical of Clutton-Brock's findings; it was traced directly from eight of his separate diagrams.

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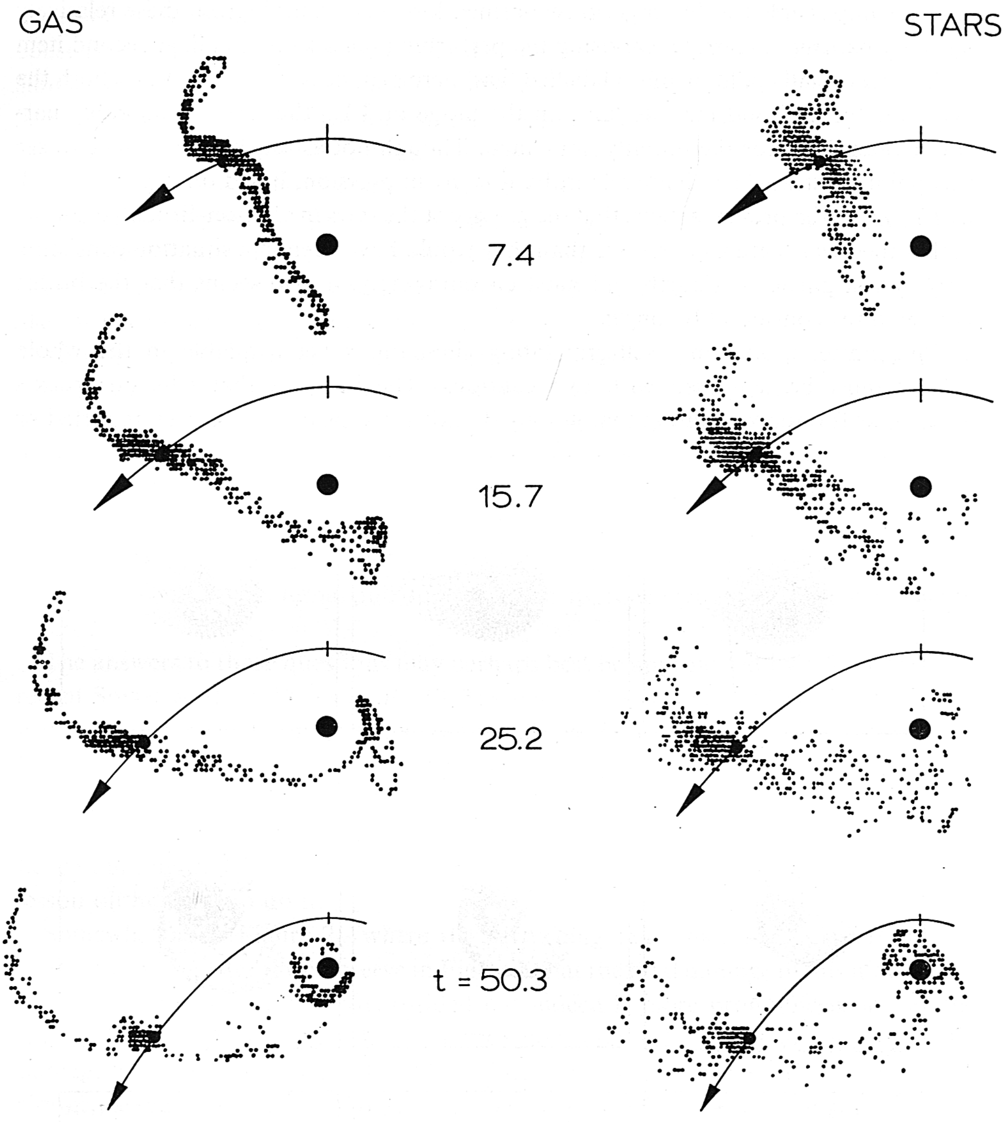

Fig. 7. Flat parabolic passage of a small, counterclockwise rotating disk past a body eight times as massive, after Clutton-Brock (1972). In each of these four pairs of views, the configurations of 'gas' and 'stars' have been drawn to the same scale; however, as in Clutton-Brock's original figures, that magnification here changes with time.

and only the orbits of the smaller body relative to the large one have been added to lend a sense of proportion.

Figure 7 reports several interesting things, one of which is that – except possibly during a brief early interval in mid-encounter – the small deformed disk simply does not look like a respectable spiral. This is consistent with my own limited experience;

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it seems impossible to obtain good two-armed kinematic spirals from these relatively leisurely passages except by choosing the perturbing mass to be small. A second item is that the initially cold medium labelled 'gas' here produces structures – i.e., both the near-side 'bridge' and far-side 'tail', in the usage of TT – that are considerably narrower than those for the initially hot 'stars'. Though not astonishing, it is nice to see this point illustrated so vividly. Thirdly, it is my impression, based on Figures 5 both of TT and of the present article, that the gravity of the stars in Clutton-Brock's example makes these gas features narrower than they would have been in a situation consisting only of test particles. Fourth and most encouragingly, it also seems that the bridge now endures considerably longer.

This praise of the only self-gravitating simulations yet available in the whole subject must be tempered with two cautions. The lesser is that Clutton-Brock's algorithms for approximating that gravity do not yet appear to have been reported or

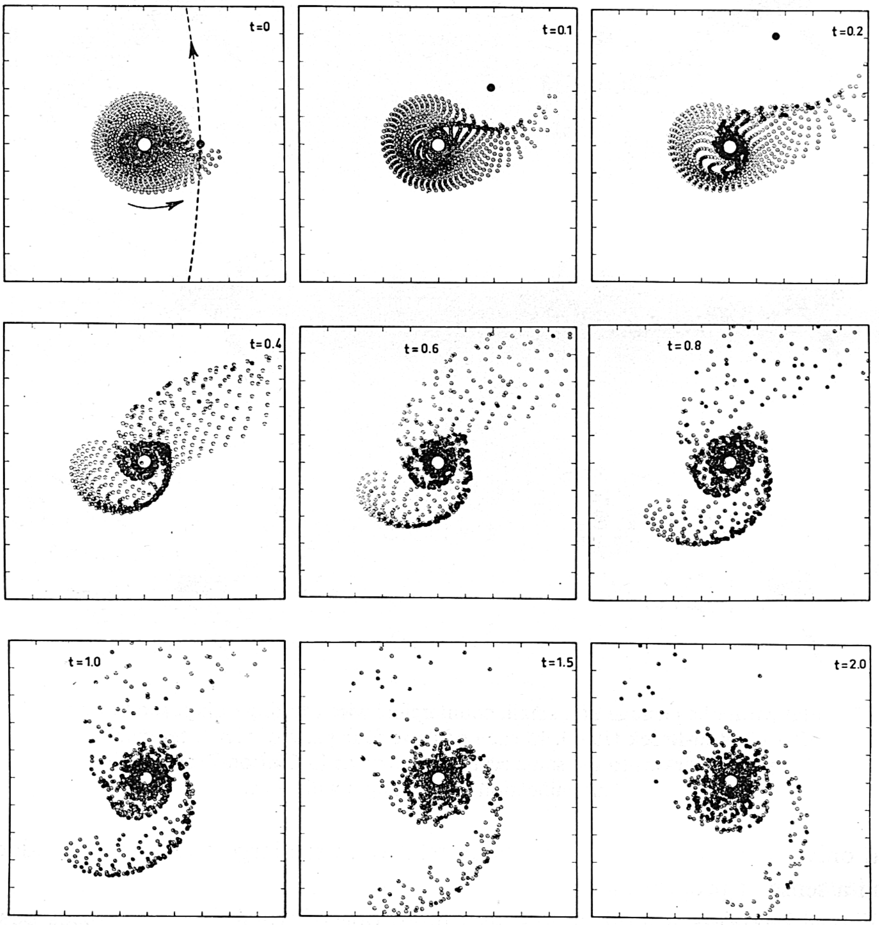

Fig. 8. Evolution of a disk of test particles following the passage "within the plane in the direction of galactic rotation" of an equal mass at twice the speed of escape, by Eneev *et al.* (1973).

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tested in much detail. The graver worry is that also Clutton-Brock "was unable to construct a completely stable disk. ... Sometimes the disk would explode during a tidal simulation". Unfortunately such symptoms are not unheard of; on the contrary, this lack of stable disk models without a great deal of random motion seems a source of fundamental difficulty even to understanding the structures of totally isolated disk galaxies (cf. Ostriker and Peebles, 1973; see also p. 134)

It is, however, not the passages of light or heavy culprits which up to now have yielded tidal results at first most contrary to intuition. That distinction belongs to the close encounters of approximate equals – provided 'close' means very near indeed and also provided the speeds are less than hyperbolic.

To reset the stage, we might recall one major reason why Vorontsov-Velyaminov (1958, 1961) among others grew distrustful of would-be tidal explanations of the various luminous extensions of peculiar galaxies: it was that *tails* of various kinds seem distinctly more common among double galaxies than any bridge-like filaments. How can this fact possibly be reconciled, Vorontsov-Velyaminov asked, with the notorious two-sidedness of tides as we know them? Or even granted an imperfect symmetry, why should the far-side tidal damage now appear so much more prominent than that from the side on which the external pull during a close approach would have been especially severe?

The answers to those questions may perhaps best be approached by reference to the recent Soviet work by Eneev *et al.* (1973; see also Kozlov *et al.* 1972). These authors produced several fascinating displays – and as I understand from Vorontsov-Velyaminov, also some movies – of the evolving tidal damage from the fly-by of an equally massive partner. However, Eneev *et al.* also continued very much in the spirit of Pfleiderer and Siedentopf by requiring those passages to be distinctly hyperbolic; hence our Figure 8 culled from their paper seems ironically most useful as a quick lesson on how *not* to do it.

Somewhat as in Figure 2 – where the perturbing mass had been greater but its passage was yet faster – we observe in Figure 8 that the tidal damage in this moderately rapid fly-by remains not only two-sided but indeed is more pronounced on just the side one would have expected. Yet notice how relatively splattered that near-side damage has already become: if these experiments had been repeated with progressively slower passages, this ugly and extensive spray would only have worsened until speeds about as slow as parabolic had been adopted (cf. Wright's Figure 1, or TT's Figure 2); with orbits of considerable inclination, the problem would indeed have persisted down to some distinctly elliptical encounters (cf. TT's Figure 16). The relief at last comes simply from the fact that a slow enough 'aggressor' actually captures most of the near-side debris, like a sort of gravitational vacuum cleaner. And that in turn explains why the later tidal remnants can then look bizarrely one-sided, with only the counter-arm growing with time into an ever-lengthening tail.

Since the only available theoretical study of tails of that sort seems still to be the one by TT, I may be the wrong man to review it further. However, it is perhaps of interest to view at leisure the development with time of that symmetric model of the 'Antennae',

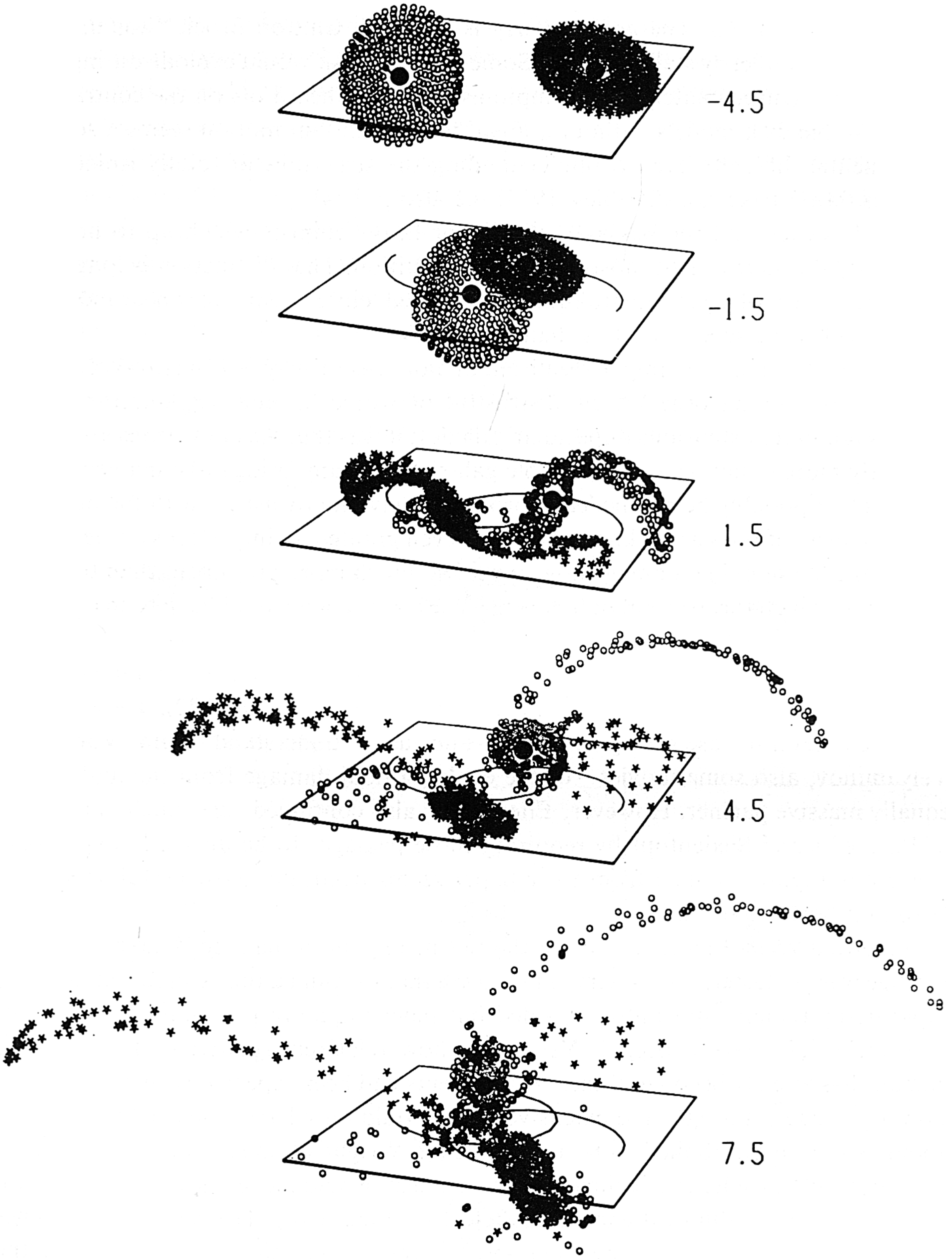

Fig. 9. Five views of the symmetric close encounter of two 60°-inclined disks of test particles presumed by TT (§VI.d) to caricature the recent history of NGC 4038/9. These views are equally spaced in time, with the instant 0 (not shown) meant to represent pericentre. The stereographic projection used here assumes a vantage point at a distance equal to 16 times that of the closest approach of the two central masses, or four times the edge length of the square that denotes their common orbit plane. The elevation of viewing is 20°.

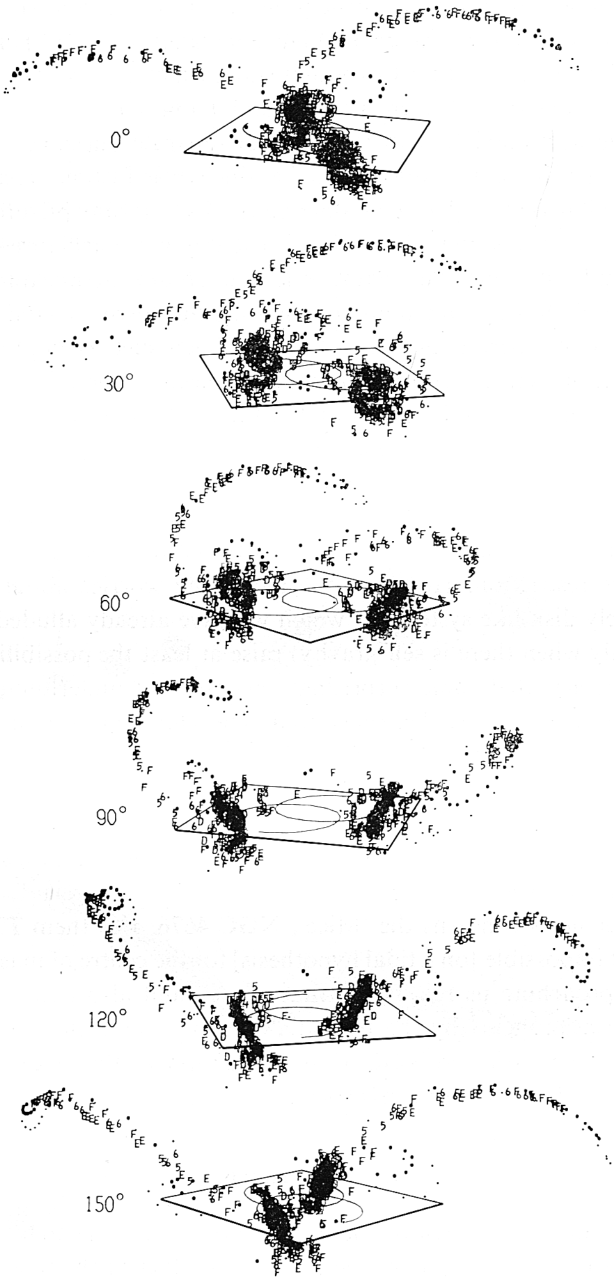

Fig. 10. End-product of the evolution in Figure 9, viewed at time 8.0 from six equally-spaced longitudes. The viewing elevation here is held constant at 15°.

NGC 4038/9, of which only two orthogonal snapshots could be offered in TT's Figure 23. This evolution is shown in Figure 9, taken directly from a new movie kindly prepared by my brother for this Symposium, which differs only in its perspective viewings from the one described by Toomre and Toomre (1971).

Also from the new movie comes the stack of separate but simultaneous views in Figure 10 depicting the same evolved model at one single instant. That set of pictures has here been included mainly to re-emphasize vividly – as only pictures can – that the real 'Antennae' and many similar peculiars probably have structures which are very three-dimensional. In a motion picture one can obtain an uncannily stereoscopic feeling for these models by pretending one is walking slowly around them!

We close with one sobering reservation about models such as employed in Figure 9, and with a brief, pleasanter report of a tentative success. The criticism once again is that our total neglect of the self-gravity and of the random motions in these tail calculations means at best that they can only mimic the tidal dynamics of *those components* of a real galaxy which (i) stem from regions far enough out to contain no more than, say, one-fifth of the total mass, and (ii) possess initial motions no more eccentric than perhaps the Sun in the case of our Galaxy. Until recently the latter might not have seemed a severe restriction. However, the increasing worries about the large-scale stability of purely disk-like systems to which we have already alluded (and which, of course, arise only when there is self-gravity) raise at least the possibility, discussed by Ostriker and Peebles, that some surprisingly massive but underluminous halos may coexist with most disks. Or else there may need to be a lot of mass in the higher-velocity stars of a disk itself. Neither class of rapidly moving old stars can be expected to form tidal ribbons remotely as narrow as those in NGC 4038/9 – and it would by inference be reassuring indeed to know that at least some such observed narrow arcs are themselves embedded in much thicker (if yet distressingly fainter) stellar crescents, rather as in Clutton-Brock's example.

The pleasanter news concerns the 'Mice', NGC 4676. For them TT had remarked that it is "all but impossible [on a tidal hypothesis] for the centre of mass of component *B* not to be approaching us relative to that of *A*", and also that some decade-old spectroscopic measurements by the Burbidges had indicated the opposite. Recently Stockton (1974) has reobserved NGC 4676. He reports a velocity difference of about 80 km s^{-1}, in the tidally desired sense.

4. Tidal Friction

This last section can be short because there is still not much that can be said definitively on the subject of possible orbital changes as a by-product of the tidal distortions. Of course the very notion that galactic encounters are not elastic has been with us for quite some time. For instance Holmberg (1941) undertook his calculations not so much to study spiral structures as in an attempt to see "whether the loss of energy resulting from the tidal disturbances at a close encounter between two nebulae is large enough to effect a capture". Since then, the capture idea has demonstrably intrigued also Zwicky (1959), Alladin (1965), Lauberts (1973), and several others. And surely today, having just viewed the possible tidal after-effects of some very close encounters, no one can in principle object to the assertion that such violence must cost mechanical energy of some sort.

Yet the real challenge here, it seems to me, concerns not principle as such but chiefly the expected rates. After all, the fractional changes *per encounter* which Alladin had in mind when he wrote that "given long enough, the components of a double galaxy will disrupt one another and give rise to a single loose system", or which Aarseth (1966) envisaged in surmising that "it might be possible that strong interactions between a few closely bound galaxies could in time lead to the formation of one larger common object, containing several galactic nuclei", were presumably rather small.

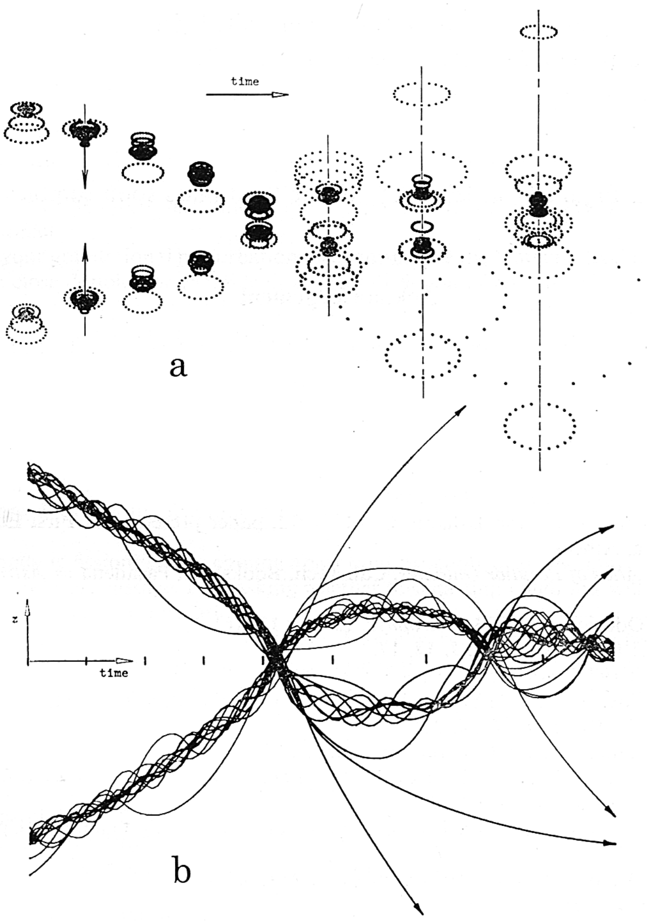

Fig. 11. Head-on encounter between two equal and approximately spherical stellar systems presumed to have started from rest at infinity. (a) Perspective views at eight (not entirely uniformly spaced) instants of the two clusters of twelve equal self-gravitating rings apiece used to simulate those systems. (b) Record of the axial coordinate z of each of the 24 rings, drawn to a $1.8 \times$ taller vertical scale as functions of the time t.

By contrast, as TT discuss, there are perhaps as many as a dozen NGC galaxies or pairs in the sky near which plausible tidal tails still appear prominent and yet where the main galactic bodies either seem suspiciously close or else may already have blended into one. For such imagined mergers (of what we supposed had been not truly unbound galaxies but merely long-period eccentric orbiters) the observations seem to demand a substantial loss of orbital energy in each single fierce encounter. An urgent question thus becomes: Can such rapid loss be justified theoretically for galaxies imagined composed, for simplicity, of only those collisionless mass points known as stars?

The vote is not in yet. However, as the (admittedly contrived) example in Figure 11 illustrates, it is far from obvious that the final answer will be negative. These tentative n-ring calculations by Larry P. Cox and myself simply underscore a point whose vehemence seems previously to have been appreciated (from impulsive-tide idealizations) only by Sastry (1972) and by Alladin *et al.* (1972). It is that just this one head-on penetration of the two systems – and all the ensuing 'splash' from the briefly doubled gravitational force, especially toward the axis – costs these model galaxies roughly one-half the maximum kinetic energy developed in their free fall from infinity. Consequently in Figure 11 at least 80% of the total mass soon tumbles into a single heap.

Acknowledgement

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DISCUSSION

E. M. Burbidge: In NGC 4038/9, how do your calculations fit with the observed velocities measured by G. Burbidge and myself (*Astrophys. J.* **145**, 661, 1966) and by Vera Rubin *et al.* (*Astrophys. J.* **160**, 801, 1970)? As I recollect, there is very little velocity difference between the two nuclei, and the largest velocities are negative ones on the far side of one of the components.

Toomre: That small velocity difference between the two nuclei of NGC 4038/39 causes no embarrassment at all to the very symmetric model that my brother and I concocted. More serious, possibly, is that our idealized disks seem to rotate in nearly opposite directions, whereas the real spins seem more closely parallel.

Arp: Would your gravitational perturbation mechanism give the 'antenna-like' tails if NGC 4038/9 started out as a close double, for example as a result of fission of an original simple body?

The question has also to be faced with regard to the Heidmann-Kalloghlian-Markarian pairs that seem to be flying apart. When these pairs were closer together, were they interacting gravitationally? Were they producing tails?

Toomre: I can't possibly swear that some sort of fissioning of galaxies might not give rise to tails like those of 4038/39, or even write ALAR TOOMRE in the sky, but I just think it very unlikely unless we are willing to postulate that the laws of gravity or inertia were different when any two such galaxies were still very close together. Otherwise, the tides then would have been so violent that the two galaxies should have become altogether disrupted and splattered.

Wright: Where does the angular momentum come from to form tails if we have a single body fissioning?

Arp: If the body is fissioning it is coming apart (and probably also rotating) – like an encounter that started at the closest passage. I am asking whether this starting condition will give the same end result as Toomre's usual tail calculations.

Miller: Some years ago when Prendergast and I were working on the large N-body calculations (*Astrophys. J.* **151**, 699, 1968), we did some experiments in which two star clusters fell together. After the collision, there were three blobs, one representing each of the initial clusters, and one at rest at the centre. The central blob was the largest. But this was a pure star case; models with some gas, and with star formation, can behave differently in detail although the gross features are much the same.

With regard to Arp's question, before we got the spiral model we had some models that started from a single gas blob and flew apart. The angular momentum and rate of star formation were both critical parameters governing the behaviour of these models. Most of those that flew apart went into two blobs, but at least one with high angular momentum and a low rate of star formation broke into four parts.

There is a question that I'd like to ask the observers in this audience: granting the observational difficulties involved, what is known (spectroscopically, for example) of the physical properties of these bridges and tails? In particular, if they show O and B stars, and gaseous emission, there may be serious trouble with time-scales. The stars needed to drive the gaseous emission most likely should have formed shortly after the time of closest approach and, according to the time-scales indicated by the Toomres (typically $3–5 \times 10^8$ yr), O and B stars could not live long enough to excite the bridges

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observed. It is difficult to see how to maintain sufficiently large gas densities in these bridges and tails to support continuing star formation.

Sargent: Dr Searle and I thought that a good test of the hypothesis for forming bridges described by Dr Toomre would be to examine the stellar populations of the bridges and to see if they are the same as those of the parent galaxies.

So far we have looked at the main body and the straight tail of the 'Mice', NGC 4676. We find that the stellar population of the tail is definitely of earlier type than that of the main body. This implies either that stars are drawn selectively from the parent galaxy or that star formation can go on in the drawn out material.

Miller: This has worried me about Toomre's calculations.

Toomre: Who says that the stars are mainly being formed now? I would guess that they formed mostly during the violent part of the tail-making.

Miller: If they were formed then, they would not still be blue and giving emission lines.

Toomre: Perhaps Wal Sargent can remind us whether the observation (Theys *et al.*: *Publ. Astron. Soc. Pacific* **84**, 851, 1972) of λ 3727 emission in one of the tails absolutely requires O and B stars?

Sargent: I think so.

Contopoulos: What is the time-scale involved from the point of closest approach?

Toomre: About 1.2×10^8 yr.

G. de Vaucouleurs: I don't quite see the difficulty, since the main body of the galaxy is bound to be redder than the tail which is essentially a piece of the disk. Perhaps you should put some gas in your models?

Miller: The problem is that the densities get too low for star formation to continue for a long time after the encounter.

Toomre: This problem about star formation is certainly a difficulty but it is hardly a disproof, since we simply don't understand well how stars are formed. Is there anyone here who would like to claim otherwise?

McAdam: Does the success in modelling tails with only stars suggest that magnetic fields with dust or gas cannot be common between galaxies or clusters? Is viscous damping or drag thought to be necessary?

Toomre: Our calculations say very little about magnetic fields or viscosity – except that they seem unlikely to be of any great importance in forming the bridges or tails.

Savedoff: For fast passages, one expects less interaction. Are there any features you would expect to be visible if the 5000 km s^{-1} relative motion in Stephan's Quintet were interpreted as an encounter?

Toomre: If the fly-by is, say, five or more times the escape velocity, the effect is very small. At 5000 km s^{-1} one would expect very little interaction.

Contopoulos: Already at 2000 km s^{-1} we found that nothing happens.

Tifft: On the basis of Zwicky's study of bridges, I have the impression that emission from bridges is rare, and that their most likely makeup is old stars.

Sargent: I only know about that one bridge – or really tail.

Wright: On the question of star formation in tails, I have the distinct impression that tails (at least initially) grow *thinner* with time – quite contrary to intuition. If there is gas in the tail, the consequent density increase is at least *one* of the conditions necessary for star formation. Additionally we have neglected self-gravitation and there is even the possibility of an intergalactic thermal instability leading to squeezing of the tail.

A second point is that we have just finished a survey at Parkes of 44 systems of interacting galaxies in a search for continuum radio emission. Our initial results show that interacting systems are *not* preferentially radio emitters nor are stronger than a comparison sample of non-interacting galaxies.

Allen: We have made similar radio continuum observations with the Westerbork telescope at 21 cm (Allen *et al.*: *Nature* **241**, 260, 1973) of 9 interacting systems, among which are NGC 4038/39 and NGC 4676 A/B. Our sensitivity and angular resolution of 25″ were sufficient to show that in no case could radio emission be detected from the bridges and tails of these systems. Emission was detected from the nuclei of some of the galaxies, and from dusty regions in the main bodies of some others. However, within the small sample which we have, the radio emission seems apparently neither more frequent nor more intense than that of galaxies which are not obviously interacting. (See also Burke, B. F. and Miley, G. K.: *Astron. Astrophys.* **28**, 379, 1973.)

Toomre: I accept that the average interaction does not produce strong radio emission, but there are also objects such as the companion of M51 to be considered.

Lewis: Would you comment on the likely effect of collisions among cluster galaxies in Coma or Virgo for providing substantial quantities of stellar material in the intra-cluster space.

Toomre: As I indicated, tidal damage of all sorts tends to be spectacular only when the encounters are roughly parabolic or even slower. Under such circumstances, I get the impression that the cast-away material amounts to no more than 10–20 %. In any cluster, where most close passages would of necessity be pairwise hyperbolic, all such loss of material would be yet further reduced. It's going to be very difficult to load a cluster with 10 times the present galactic mass by means of debris pulled out from former galaxies.

G. de Vaucouleurs: What we want is 1/2 to 1/3 of the mass of the galaxies.

Toomre: If there are junk-piles of debris to be found, I'm personally inclined to look at cD galaxies or, if mergers occur, even at ellipticals.

Contopoulos: In the case of two merging galaxies, does most of the energy go into random motions of stars or into escaping stars?

Toomre: I am not sure yet. Probably most goes simply into the internal motions or heat. In principle, at least, it is energetically possible for two finite stellar systems to merge and throw away nothing.

E. M. Burbidge: How does the expected number of encounters agree with the space density of galaxies and their velocities? I believe Ambartsumian concluded that the number of doubles found required that they were actually formed as doubles.

Toomre: Indeed so. Any stray (i.e. hyperbolic) encounters seem too unlikely by several orders of magnitude. I therefore strongly concur that most of the present or recent interacting pairs (about 1 in 100 of the NGC objects) must somehow already have been created as bound doubles. As Nelson Limber (*Astrophys. J.* **142**, 1346, 1965) wrote: 'A possibility that may warrant further consideration is that these systems represent old double galaxies, whose semi-major axes and orbital eccentricities are such as to have brought about only now, for the first time, the very close encounters that we observe.' We may indeed be dealing with pairs of galaxies which until recently were still falling together, or which once fell together, missed, and are now coming close for the second or third time.

G. Burbidge: Are you saying that these have been bound systems for 10^{10} years and the close encounter has just happened?

Toomre: Yes. It's of course very improbable but I remind you that, if we see a certain fraction of such encounters now, there must have been many more in the past. Logically, a substantial fraction of all galaxies in the sky would thus have to be remnants of encounters. Would you like to contemplate which single class of galaxies is numerous enough to accommodate such remnants?

Davies: It should be emphasized that the outer parts of spiral galaxies contain a high proportion of neutral hydrogen. These same outer regions are the ones involved in tidal interactions. One might therefore expect that 10 to 50 or even 100 % of the material in the tidal arms might be gaseous hydrogen. This should be investigated optically and at 21 cm.

Abell: You have described several systems, which appear to be so well understood in terms of tidal interaction as to provide strong evidence that tidal interaction does, in fact, occur, and hence that the two galaxies involved must be at about the same distance from us. Am I correct in assuming that among such good examples there is *no* case in which the two members of the pair have very different redshifts (which would suggest non-cosmological components to redshifts)?

Toomre: You are correct. The nearest thing to an exception might be the often-quoted Zwicky triplet that appears as No. 175 in Arp's *Atlas*. One of those galaxies has a redshift perhaps 7000 km s^{-1} less than the other two. But as we discuss on p. 648 of our *Astrophys. J.* **178** article, that situation is probably a superposition, since a so-called bridge there certainly looks to us like a tail.

Yet I don't want Chip Arp to think I always argue against him. If anyone wants to claim, as I suppose I do, that NGC 7603 is merely superposed on its neighbour with that 8000 km s^{-1} excess redshift, let him also remember that there would then still be those wisps (tails?) of that Seyfert-like main galaxy to be accounted for.

Abell: Do you think that the merging of galaxies by two-body collisions in clusters could build cD galaxies; especially those with multiple nuclei, as in NGC 6166?

Toomre: Yes, I *suspect* so. I believe at least Aarseth and Ostriker have guessed likewise – but the problem remains: how can we tell for sure?