THE DYNAMICS OF OPEN CLUSTERS

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This is a review of a subject that turns out to be rather dismal. Both in theory and in observation we know and understand open clusters poorly -- either in comparison with globular clusters or on an absolute basis.

In saying this I do not mean to imply that we know nothing about the dynamics of open clusters. We know a great deal, in fact, about the dynamical processes that go on in them. What we lack, however, is a basic understanding -- or rather an understanding of their basic dynamics. Piecewise calculations we can make readily, but for lack of a proper framework we can never be sure that our calculations are giving answers that are quantitatively correct.

Thus, for example, it has been clear ever since the work of van den Bergh and Sher (1960) that there is a real deficiency of faint stars in open clusters, and it is equally clear to anyone that this could have come from the dynamical escape of low-mass stars. We claim to know how to calculate the escape of stars from clusters -- and our theory is reasonably reliable. Yet when we try to apply this theory it turns out that the results depend completely on what model we assume for the cluster, but no basic theory has yet told us what this model should be. For this reason I will leave uncertain applications aside and concentrate on the basic problem of finding dynamical models for open clusters.

The dynamical difficulties for open clusters are best understood, I believe, by contrasting them with globular clusters, where we have a reasonably good understanding, and seeing what it is that makes the globular clusters easy and the open clusters difficult.

What makes globular clusters easy to deal with is the existence of two important inequalities, which can be expressed in terms of characteristic times. The first of these times is the crossing time, t_{cr}, which is the length of time that a typical star takes to move across the cluster. Next comes the relaxation time, t_{rlx}, in which

James E. Hesser (ed.), Star Clusters, 139–155.

individual stars exchange energies and their velocity distributions approach a Maxwellian equilibrium. Finally there is the evolution time, t_{evol}, in which the cluster evolves as a result of dynamical processes.

For globular clusters

$$t_{cr} << t_{rlx} << t_{evol}, \tag{1}$$

and this is what makes them easy. What happens is that stars move rapidly around the cluster and establish an equilibrium between their density distribution and their velocity distribution, and relaxation operates so slowly as not to disturb this equilibrium. In fact the situation is even better than that: relaxation actually determines what form the distribution functions take.

It works as follows. The equation that describes the dynamics of a stellar system in a general way is the Boltzmann equation,

$$\frac{\partial f}{\partial t} + \vec{v} \cdot \nabla f - \nabla V \cdot \nabla_v f = \left(\frac{\partial f}{\partial t}\right)_{enc}, \tag{2}$$

where the term on the right symbolizes the complicated expressions that describe the changes wrought in a velocity distribution by stellar encounters. When we consider globular clusters, however, we are protected from the full horror of this equation by the first part of inequality (1). The terms on the left-hand side of the Boltzmann equation relate to orbital mixing -- the exercise in which the cluster makes its density distribution and its velocity distribution mutually consistent -- and the time scale for this process is the crossing time. The encounter terms, on the other hand, have as their time scale the relaxation time; so they are much smaller.

Thus as a first approximation we can describe the equilibrium of a globular cluster by setting the encounter terms equal to zero. The time-independent equilibrium is then easy to handle; with a straightforward algorithm we can go from a velocity distribution to the corresponding density distribution. But now relaxation theory steps in and tells us just what the velocity distribution ought to be. We can write down the Fokker-Planck equation, which is just $\partial f/\partial t$ set equal to the encounter terms, and find its steady-state solution; this is our velocity distribution. When we put it into the algorithm that finds density distributions, out comes a family of models; and they actually look like globular clusters, so we can put some faith in them. (For details, see King, 1966 and 1967).

It's not really as completely solved as I've suggested here; there are still problems. Nothing in the above procedure allows us to decide how much anisotropy there should be in the stellar motions;

that has to be found from observation. But we know how to make models that have anisotropic velocity distributions, and when we get velocity observations such as those of Gunn and Griffin (1979) or Cudworth (1976 a,b) we can come even closer to a true picture of a globular cluster (as, indeed, Gunn and Griffin did in the paper referred to).

There are other problems of detail, and we have quite serious difficulties in predicting the evolution of a globular cluster, but I think that we have a pretty good dynamical picture of what a globular cluster is like at a given moment of time.

For an open cluster all those beautiful advantages disappear. The troubles in the theory of open clusters (and in the observations too, for that matter) all come from the small number of stars. There are superficial difficulties, which can be dealt with; but there are also fundmental difficulties for which I don't know any answer.

Let me first dispose of a problem that I thought was serious but has turned out not to be. In discussing the theory of globular clusters I referred to the Fokker-Planck equation, which was introduced into the theory of stellar encounters some 35 years ago in a very elegant way by Chandrasekhar (1943). It describes the evolution of a velocity distribution very nicely, but it makes one strong statistical assumption about the physics: that all individual encounters produce only small velocity changes. If you look at the criterion for that condition to be satisfied, it turns out that there must be a large number of stars in the system. When the number of stars is small, large velocity jumps become important and the Fokker-Planck equation isn't valid any more. This is of course the case for a open cluster.

But that turns out not to be serious, because there is another equation, the Kolmogorov-Feller equation, which can play a similar role when the velocity changes involve large jumps. It was Agekyan (1959) and Petrovskaya (1970 a,b) who first showed how to apply this equation to stellar velocity distributions, but only in the recent work of Retterer (1979) has the Kolmogorov-Feller equation come into the practical domain. The steady-state solutions are significantly different from those of the Fokker-Planck equation, but now we know how to calculate them.

The real trouble, however, is that we are unable to apply these elegant solutions to building models of open clusters. An open cluster cannot be quasistatic in the way that a globular cluster is, because the inequality $t_{cr} \ll t_{rlx}$ is not satisfied. For many open clusters the crossing time is of the order of, or even longer than, the relaxation time. The formula for their ratio is

$$\frac{t_{cr}}{t_{rlx}} = \frac{N}{31 \ln(N/2)} , \tag{3}$$

(King, 1967), where N is the number of stars in the cluster. This expression is equal to unity when N = 130, right within the range of richness -- or rather poorness -- that characterizes open clusters.

This is the basic reason that we don't have dynamical models of open clusters. Look again at Equation (2). For globular clusters we said that the last term was so small that we could make a static model by setting the middle terms equal to each other, and that we could then see what kind of velocity distribution to use by looking at the form of the relaxation terms. But here we can't do that. The relaxation time is a measure of the effect of the encounter term, and its near-equality to the crossing time says that the encounter term is comparable in size to the other terms; so we certainly can't neglect it. We have to take the whole equation at once, and this is what I have no idea how to do. In the globular-cluster treatment we had one equation -- the Liouville equation -- for which the well-known theory of differential equations led to the simple elegance of Jeans' theorem, and then another -- the Fokker-Planck equation -- where a simple separation of variables led to a steady-state solution. Here we have the whole equation. It isn't homogeneous, so separation of variables won't work; and it isn't a simple differential equation, because the encounter term has integrals in it.

If I had to face this problem -- and I do hope that some one else will first -- there are two approaches that I think I would try. One is an elegant long shot. The theory of non-equilibrium statistical mechanics has a principle called minimum generation of entropy, which claims to constrain the way in which a non-steady system settles down into a state of steady leakage. The first trouble, however, is that I don't know how to define entropy for an unconfined, self-gravitating system. But I suggest the idea on the chance that some one will know what to do with it.

The other approach is just the opposite: it's quite straightforward and couldn't be less elegant. It is simply to start by ignoring the differences and using the globular-cluster method. This won't give the right answer, but it may get us close enough to see what adjustments to make next. Sometimes by first solving the wrong problem we can shed some light on the right problem. To put it more charitably, iteration is a valid method, and a globular cluster might be a reasonable first approximation for an open cluster.

Sometimes a naive approach has a lucky success. That was true with the globular clusters, where the steady-state solution of the Fokker-Planck equation fits better than we had any right to expect it to. The model-building algorithm assumed that this distribution held at the center, and the whole model then followed from that assumption. But if you apply Jeans' theorem to find the form of the velocity distribution elsewhere, it turns out that at any other point it is actually close to the steady-state Fokker-Planck solution for the value of the potential that applies to that point (King, 1966, p. 65). Perhaps a naive approach to open clusters will have a similar piece of luck.

Open clusters have one more pitfall, however, that makes them different from globulars: the mass function in an open cluster has a much wider range, and I believe that this widely spread mass function is going to play an important role in the dynamics. This is not so in globular clusters, because stellar evolution has wiped out the upper end of the mass function.

Here was another great piece of luck in our dynamical attack on globular clusters, and one that is not generally realized. When dynamical models of the now familiar type were made by Michie (1963 a,b; Michie and Bodenheimer, 1963) and then by me (King, 1966), they were always based on the simplifying assumption that all stars have the same mass. The models were a good fit to observed density distributions, so we blessed them as truth and stopped there. It's a natural human tendency to stop when you have an answer that looks satisfactory, but we should always remember that that is the attitude that left us content with an 88-day rotation period for Mercury.

Let us look at the true dynamical situation in globular clusters. When the stars of different masses interact with each other, they relax toward a Maxwellian distribution in which the higher-mass stars have a lower velocity dispersion. Because they all move in the same gravitational field, the high-mass stars stay closer to the center, and the low-mass stars spread out more. There is no reason to expect such an inhomogeneous mixture to look like a single-mass model, and in general it won't.

Yet the single-mass models did fit actual globular clusters, and we asked no questions but just smiled smugly at our success, and even lectured on it. How come the models fit when they shouldn't? The answer is that globular clusters really are, for practical purposes, single-mass systems. This is what Illingworth and I found out when we tried fitting detailed observations with a realistic mixture of masses (Illingworth and King, 1975). The red giants (which are the stars whose distribution we observe) took on a proper distribution only if we put the dominant mass into *white* dwarfs rather than red dwarfs.

It is easy to see why this is so. The dominant type of stars are the only ones that move in their *own* gravitational field, and they will therefore be the ones that have a distribution like a single-mass model. But if the red dwarfs are dominant, then the red giants, with their much higher individual masses, will heap up very steeply to the center, quite differently from what is observed. So the dominant mass can't be in red dwarfs. If we put it in white dwarfs, however, because they have almost the same individual masses as the red giants the cluster almost *is* a single-mass system. This seems to be the case in reality. (For the only detailed publication of such models, see DaCosta, 1980.)

It is interesting to think that if the majority of the mass in a globular cluster were in red dwarf stars, the naive single-mass models wouldn't have worked, and we might have thrown them away and decided that that wasn't a good way to attack the dynamics of globular clusters. Sometimes we have been just plain lucky.

The older open clusters are somewhat like the globulars in this general respect: the more massive stars are gone, and the mass range is not large. But for the younger open clusters we need to face the problem of a widely spread mass function as an essential factor in the dynamics. Also, the older open clusters are richer -- they had to be in order to survive -- and they are therefore not badly approximated by globular-cluster models. It is for the young, poor open clusters that we are going to have to invent new dynamical approaches.

We at least have some good idea of the dynamical processes that affect open clusters. Wielen (1975) lists some of them in his earlier review of this subject: (1) stellar encounters, (2) galactic tidal forces, (3) tidal shocks by cloud complexes, and (4) mass loss from evolving stars.

Stellar encounters I have already discussed, as the phenomenon that in effect molds the shape of the velocity distribution. The galactic tidal force plays a complementary role in molding the edge of the cluster. By limiting the cluster in spatial extent, it correspondingly limits the energies of stars bound in the cluster and thus shapes the high-energy cutoff of the velocity distribution, which in turn makes the envelope of the cluster what it is.

Tidal shocks differ from a steady tidal force, in that their impulsive nature continually pumps energy into the cluster, and this influences the cluster's dynamical development. (The globular clusters also suffer tidal shocks, each time they dip through the galactic plane; the numbers are different for them, but the effect is similar.)

Quite different for open clusters, however, is the effect of mass loss from evolving stars. Since most mass loss comes from the massive stars that have short lifetimes, such effects are long since done with in globular clusters; but in young open clusters they can be important, or even dominant. Mass loss has two effects. One is that the total cluster mass decreases, while the energy increases -- becomes less bound, that is. This latter is because the mass loss is driven by astrophysical causes within the stars. In effect, the escaping mass is given the escape velocity without dynamical cost, and this is like adding energy to the dynamical total. The second effect of mass loss is that it directly changes the mass function, and this will necessitate a readjustment in the cluster's equilibrium.

Thus mass loss will be an important process during the early life of a cluster. There are other ways, too, in which the early stages of a cluster's life pose dynamical problems. We certainly see clusters that are less than a relaxation time old, and there are very probably some that have not existed for even one crossing time. For such clusters we could not postulate an equilibrium model, even if we knew what such a model was. This is a disadvantage, but it is also an advantage. The very youngest open clusters must still bear upon them some signature of the process that formed them, and perhaps we will one day be wise enough to decipher it.

To these four dynamical processes in open clusters it is now clear that we must add a fifth: the effect of binaries. It is somewhat surprising that a problem that has been with us so long has been so slow in finding what I suspect is its rightful place, at the center of the study of the dynamics of star clusters. We should remember that binaries were discovered computationally by von Hoerner (1960), the first time he ever put an N-body problem on a computer. The calculation was stopped, for economic reasons, by the fact that three of the stars had a chance encounter which left two of them as a binary. To follow the orbital motion of the binary he had to use such a short time step that he couldn't do anything useful any more. This happened again and again in N-body calculations; but then people like Aarseth (1972) and Bettis and Szebehely (1972) figured out ways of coping computationally with binaries, so that their effect could be followed.

It turned out to be quite characteristic of the evolution of N-body systems that a hard binary (as we now call it) would form, usually from two of the most massive stars, and that this binary would thereafter dominate the evolution of the rest of the cluster. In effect the binary acts as a sink of binding energy (Heggie, 1975). Through encounters with the other stars it becomes more tightly bound; in doing so its energy becomes more negative, and thus it gives positive energy to the other stars, inhibiting the contraction of the rest of the cluster.

This dynamical effect of binaries could turn out to be quite important, because it offers one of the possible solutions to the dilemma of core collapse. Theory predicts, and numerical simulations seem to confirm, that stellar encounters will eject stars from a cluster, that the ejection will then cause the core of the cluster to contract, that the contraction will make the ejection go faster -- and so on, with the core collapsing to infinite density in a finite time. The trouble is that we don't see this happening in any real cluster that we know of, nor do we even see the aftermath of core collapse anywhere -- unless, of course, we succumb to the temptation of attributing globular-cluster X-ray sources to core collapse just because we don't know how to explain them in any other way. Personally I would rather not take the easy route of disposing of two perplexing problems by letting them annihilate each other (let the theoreticians engage in that process); for the time being I would rather pursue the hope that it is the formation of binaries that stops the core collapse. In voicing such a prayer I have to admit that it is less likely for globular clusters than for open clusters, since the relative importance of binaries goes down

https://doi.org/10.1017/S0074180900091798 Published online by Cambridge University Press

as N goes up; but as evolution proceeds there are fewer and fewer stars in the core, and that is the number that counts.

The study of the dynamics of star clusters, and particularly of the process of core collapse, has been helped immeasurably by numerical simulations. These occupy an intermediate ground between theory and observation. They are, in fact, experiment. Simulations are one of the very few cases in astronomy where we are not confined to seeking out and observing the experiments that nature is already performing for us; gravitation is such a simple process that we can choose our conditions and let the experiment run on our computer. We quite literally calculate all the forces and simply integrate the equations of motion of the N bodies. Not that it is all that easy in practice; N bodies have N(N-1)/2 interactions, and the limitation is very much an economic one. The effective cost goes up as a quite unpleasant power of N, and in practice not much can be done with more than 1000 bodies.

This would mean that simulations could do nothing for globular clusters, but fortunately we have another simulation method to use for them. In the so-called Monte Carlo method (Hénon, 1972; Spitzer, 1975) the individual stars are idealized by smoothing them out into spherical shells, which in the aggregate represent the velocity and density distributions rather than the individual stars. The Monte Carlo part then consists of making occasional random changes in the velocities, to simulate the effect of stellar encounters.

But this statistical game-playing removes the Monte Carlo method one step farther away from reality, and I think that at this point one has to face the one real shortcoming of experiment. You can do an experiment carefully and you can do it right, but you always have to wonder whether you are doing the right experiment. Have you chosen the relevant initial conditions and put in all the right dynamical processes? For example, what if no one had ever recognized that tidal shocks are an important dynamical process for open clusters? We can calculate with exquisite elegance and accuracy, but without the right physics we can hardly expect to get the right answer.

So ultimately we must turn to observations of real clusters, to guide the theory onto the paths that it has not yet found. Or we would turn to observations, rather, if good ones existed. Most of the data that we have on open clusters are, as the astronomical euphemism goes, useful for statistical purposes. On this level good dynamical studies have already been made -- I would cite in particular the discussion of dynamical ages by Wielen (1971) -- but crude data will never lead us to a fine theory.

In deploring the lack of good data I do not mean to indict the astronomers who have worked on open clusters. The data will always be hard to get, because of difficulties that are inherent in the problem. Basically it all comes down to the small N. Even if we were studying a simple dynamical system, the number of stars would be uncomfortably small. But remember the importance of the mass function, and consider with horror what happens when we take a value of N that was already marginally small, and then divide it into several separate mass groups.

To make it worse, nature, in a rare act of complete malice, degrades our statistics even further. Because the open clusters live in the Milky Way, they have a rich foreground and background of field stars, which ruin the statistics. Small numbers are uncertain by the square root of the number, and in this case we suffer from the square root of the large number of field stars rather than from that of the small number of cluster stars. Near the center of a cluster this isn't so bad; star counts can determine the core radii of many open clusters reasonably well. But in the envelope the field stars swamp the cluster stars statistically, and at that point you either move to another field of astronomy or else find a way of getting better data.

Fortunately, better data can be had -- but at the cost of considerable effort. I believe that the future of open-cluster dynamics is going to rest on good membership studies in some well-chosen clusters, largely from painstaking work on proper motions. The need for selecting the individual stars that are members is already recognized by those who do color-magnitude arrays (see the example given by Gretchen Harris in her review in this symposium), but we need it just as much for dynamics. Here is where it begins to hurt, though; for the dynamics we also need the fainter stars. They are harder to measure, and often the plates of earliest epoch don't go that faint. It is indeed fortunate that astrometric techniques have recently been developed (see, for example, Chiu, 1977) that will probably produce the needed accuracy for time baselines of only a few decades.

In only a few open clusters do we have refined proper-motion studies from which purified member lists can be extracted for dynamical studies. I would not count M67 and NGC 188 in this class, in spite of the accuracy with which the proper motions of their stars are known. First, there are no proper motions, and therefore no membership probabilities, for the faint stars whose distribution is so vital to a dynamical study. Also, as I remarked previously, these old open clusters do not differ sufficiently from globular clusters to allow us to beat a path into a really new area of dynamics.

Our near neighbors, the Pleiades and the Hyades, offer a much better opportunity. For the Hyades there is already a study by Pels, Oort, and Pels-Kluyver (1975). They go down to $M_V = +10$ over a large region and to $M_V = +12$ in a smaller region. Some of their conclusions are interesting. They see a central concentration that depends strongly on M_V and is especially marked for the A stars and the yellow giants. A model based on truncated Maxwellian distributions fits the various groups reasonably well. (So perhaps a naive application of the globular-cluster approach <u>is</u> a good first approximation.) Interestingly, they follow envelope densities beyond what ought to be the tidal limit of the cluster; they suggest that what they are seeing is the

https://doi.org/10.1017/S0074180900091798 Published online by Cambridge University Press

flux of stars that are actually in the act of escape.

But much still remains to be done with the Hyades. First, the proper motions used by Pels et al. are not all of the highest accuracy, and their membership list is not perfectly clean (Upgren and Weis, 1977; Weis et al., 1979). Second, many faint stars discovered by van Altena (1969) ought to be included. Finally -- and perhaps most important -- the Hyades are so close to us that the perspective effect in their motion allows distances to be determined for individual stars, so that alone among clusters this one can be studied in three dimensions rather than two.

The Pleiades also offer a good target. Most photographed of clusters, they have many early-epoch plates; and because the distance modulus is small, these plates reach to low-luminosity stars. Jones (1970) has already shown that proper motions can even tell us about internal velocities in the Pleiades, and we can hope for much from the extensive study that van Leeuwen has begun at Leiden. Praesepe is also a hopeful case (Jones, 1971).

For these low-distance-modulus clusters I should also mention the use of accurate radial velocities as a membership criterion. The acvent of cross-correlation velocity-meters is making this a real possibility. Griffin and Gunn have, for example, used this technique to study internal motions in M67 (in a work that remains lamentably unpublished). Now that a Coravel machine is in operation at Haute Provence, and another is being built for La Silla, I can commend cluster stars to those observers as an excellent target.

Perhaps the best cluster of all on which to concentrate dynamical studies at the present time is M11. McNamara, Pratt, and Sanders (1976) have determined proper motions good enough to identify 800 members, covering a wide range of mass, too. The study of the dynamics of open clusters is a difficult task; but if I had to start somewhere, I think that I would start with the distribution of stars in M11.

I have outlined this whole subject in a somewhat pessimistic way; but in a sense, that pessimism is a kind of optimism. When you look at the problems you see what they are, and it is not far from there to seeing what needs to be done. Personally I have taken the route of the globular clusters, because the open clusters are so much harder. But I have indicated some of the things that need to be done with them, and I hope that courageous people will go out and do them, so that even more courageous and foolhardy theoreticians will use the observational data to make the dynamical theories of open clusters that we so much need.

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DISCUSSION

FREEMAN: That was a really wonderful talk; thank you, Ivan. Comments or questions?

KEENAN: What you mentioned about Pels-Kluyver, I've always found that as some work that worries me a lot, because the numerical simulations that I'm going to talk about on Thursday and other things indicate that once a star gets beyond the King tidal limit it escapes very rapidly. And, secondly, when it is in the escape mode, the binding energy is positive, it goes away very rapidly. So it worries me very much that the Hyades seems to be too big.

KING: Okay, let me first make a personal remark. Don't call it the King tidal limit. It was first pointed out by van Hörner - before anybody even looked he was wise enough to see that such a thing should exist. Now they address this point in their paper and they don't claim that the stars will be held there. They say this is the flux of escaping stars; and that the number of stars seen there and the rate at which they will leave the cluster, which is relatively gradual, is consistent with the number that are seen at the present time. I can say also that that paper is something of a *tour de force* because Pels died in the middle of the work so Mrs. Pels-Kluyer and Oort put the paper together and somehow got it finished up and I think they did an awfully good job of piecing together something that probably was in pieces at the time that it fell into their hands.

LODEN: Just two clarifying points speaking about stellar encounters in the clusters. Do we include only encounters between members or between members and intruders? That was the first clarifying point. Then you spoke about the galactic tidal field; do you mean a more or less isotropic field, or the strongly anisotropic field that is created by the neighbouring clusters?

KING: Two questions. All right the first one: I think that it is a safe enough approximation to be quite valid to consider only encounters of stars with each other in the cluster. A very long time ago (I still feel young enough to consider twenty years a very long time ago), I did some calculations of this and I found that there were indeed circumstances under which the energy contributed to a cluster by stars passing through it would make a difference, but there were always circumstances under which the cluster was already tidally unstable. No cluster that is tidally stable is influenced seriously by intruder stars that are passing through. The thing that I did not mention that is important, and this is very hard to take into account, is that one must consider not only the encounter of stars with each other but the encounter of stars with those awful binaries. They make a great difference. As a matter of fact, we've got to the point now where, and you'll hear from John Retterer about this, we can handle the encounter of stars with binaries, but as the binaries become more important it then becomes necessary to ask what happens when a binary encounters a binary. Now Spitzer in his most recent n-body calculations (this will be a paper by Spitzer and Matthew which I hope Lyman is writing this summer - I think he may actually be doing it), puts binaries into a Monte Carlo calculation. The thing that held them up the most was that they didn't know how to handle the encounter of binaries with each other. Now this summer Matthew is trying to do four-body orbits and try to see what binaries do when they encounter each other. But that's still a missing thing, so for the question you asked, the answer is encouraging; but the questions you didn't ask and that I didn't even mention are serious.

LODEN: Thank you.

BOK: May I point out that when I worked on this fifty years ago, and it's probably all wrong, I had the feeling there were three types of clusters you dealt with: Pleiades and there the thing is done internally; Hyades, borderline case; and the Ursa Major moving group, a typical example where the encounters between the stars mean nothing and where the field does the whole trick. So you have to see with whom you live, with what particular group, but the Ursa Major is one where the effects of the field seemed important - maybe its different now, I don't know.

KING: I think you're simply dealing with the thin edge of the end of a cluster. Like . . .

BOK: Yes, that would be it. Then the field stars take over and "boom" tear 'em to bits. Then the differential rotation. That was a very simple picture.

KING: I've been trying to talk about the more stable ones, the ones that you would call Pleiades. Your second question about tidal forces. No, I am referring to both kinds of tidal force and I perhaps did not sufficiently emphasize that they are essentially different in their dynamical effects. The galactic tidal force enters into the potential field and sets a limit on the size of the potential well in which the cluster lives. That's a permanent fact which doesn't change. The passing clusters, clouds, complexes, spiral arms, whatever, they have the effect of turning on a strong tidal force and then turning it off again. That shakes up the cluster and puts energy into it. That's a different dynamical phenomenon.

CAYREL: Could the stars belonging to Eggen's Hyades moving group add something to our knowledge of the Hyades cluster? In particular, could we use them to count all the Hyades, the original number of the Hyades cluster?

KING: I think they can add to our knowledge in a population sense, but not in a dynamical sense. I believe that the Hyades moving group cannot be stars that have escaped from the Hyades, because the Hyades are the second largest cluster in that group.

Praesepe is a member of the group also: it has the same HR diagram, the same metal abundance, and the same space velocity as the Hyades. Clearly what we have is a very large association which in forming included two clusters. After all we have h and χ Persei, which are still very close together, and we have the Hyades and Praesepe, which are far apart. As a matter of fact, Pels, *et al.* make the comment that those Hyades group stars are a complication. There is a danger that because of the similarity in motions you will identify group stars as being cluster members when they are merely superimposed on the cluster on the plane of the sky.

BOK: Then when you have that, then you have a very great, long term, slow, nice energy transfer, isn't it?

RAJAMOHAN: Open clusters or in nebulosity more massive stars form and remove the nebulosities by blowing them away. But until that time it's all embedded in nebulosity; what is its affect in terms of the dynamical theory?

KING: That's a very interesting problem. One really has to ask the question, "why are there star clusters at all?" Something of the order of 1% of the material in new-star formation ends up bound as clusters. The remainder is unbound as associations. Something happened to cause those stars in the clusters to remain together. I've thought about that dynamical problem. I'll give you one idea for whatever it's worth. Take some stars that were bound and are no longer bound because the gas went away. They have a velocity distribution; in that velocity distribution there must be some values close to zero. Is there a center of the velocity distribution - stars whose relative velocities are so low that they will remain bound to each other and they will constitute a cluster? Is there a threshold below which this happens and above which there isn't any cluster? I don't know the answer, but I think it's a problem that is very worth investigating.

FREEMAN: I have a question along the same line. When I was a boy there was a famous paper by van den Bergh and Sher which showed some quite remarkable cutoffs in the luminosity functions. Are they believed to be the result of a funny initial mass function or are they more likely to be an evolutionary effect?

KING: Well that's the problem I referred to saying that we know perfectly well what the dynamical phenomena are; the only trouble is that each calculation gives a different numerical answer. I think Sidney did an early calculation. (To van den Bergh). Before that paper wasn't it? Before the star counts you did something on M67 calculating escape rates of stars of different mass? It was a bold attempt, but I don't think Sidney believes the answer any more than I do. (Laughter). That, I think, is exactly what we need honest models of star clusters for, because it's only in those models that we'll really be able to calculate the escape rates. That paper showed that clusters are deficient in low luminosity stars. I thought that there were some clusters included in the paper that were so young that you couldn't have got rid of those low mass stars dynamically, but it's hard to say quantitatively.

BLAAUW: First of all, I think you said that Praesepe and the Hyades are farther apart than h and χ Persei. As far as I know it's the opposite. The distance of Praesepe from Hyades is something like 150 parsecs, or so; as far as I know, the photometry of h and χ Persei indicates that they may be several hundred parsecs apart. But apart from that, the comparison is very interesting, isn't it, because there also you have the two clusters and you have this cloud of stuff around them which undoubtedly has something to do with those clusters. But what is this relation? Is this an escape phenomenon? Is it something that indicates the original belonging together, whatever it may be? And if that is understood then perhaps also that thin tail of membership in the Hyades that you referred to might also be better understood. It may be something more general than just the Hyades.

KING: That's a very useful comment; I hope that the people with the tape will use Dr. Blaauw's comments to put down what I should have said. (Laughter).

BLAAUW: Another small remark regarding what you said about the development of the paper by the Pels and Oort. I think what really happened is that when Pels died, the measurements had just about been completed. The discussion of the measurements had not been completed. There was no beginning at all of analysis in the sense of dynamics, etc., so if there is something lacking in the paper due to Pels dying, it is that maybe the observational data are not as complete as one thinks they are.

HARDY: Going back to the problem of escape versus mass ratio. At least there is an instance where in a galaxy you find the same example, IC 1613. I think Sandage used old material from Baade to look at the luminosity function and he found a very sharp cutoff at a very bright luminosity. Now, of course, a galaxy is not a cluster, but if the stars escape from a cluster they should go into the rest of the galaxy. Where would they go from a galaxy? In other words it seems that there is a real cutoff in young objects up to fairly bright luminosity.

KING: Yes, and, furthermore, if one calculates dynamical time scales for IC 1613, they're ridiculous; the relaxation time is so long that nothing whatever has happened to it. So it can't be dynamical.

SCHOMMER: Could one illuminate some of the theoretical problems by studying the luminosity distribution or the velocity dispersion of some young populous clusters in the LMC, where the number of stars is larger but where the mass range is also larger?

https://doi.org/10.1017/S0074180900091798 Published online by Cambridge University Press

KING: I've always stayed away from Magellanic cloud clusters, except for their own sake, because they are so miserably crowded. Yes, they're rich enough that you can study the distribution from the light distribution in the bright stars, but you don't get anything about the faint stars. And I think you will probably end up, however many decades from now, understanding those clusters by comparing them with Milky Way clusters that are nearby and already understood in some detail at that time.

BOK: Not 'til we have the space telescope. That will make a big difference. Therefore you would be silly to work on them now. But get the programmes written for the near future!

KING: It is well known that the space telescope will solve all the problems of astronomy. (Laughter).

BOK: Now, now, now be careful. This is a special one for which it is ready made.

KING: People are going to have to compete for that observing time and there are only so many hours a year and the trouble is that the space telescope in principle could solve all problems, and in practice you won't get the observing time to solve all problems. (Laughter).

BOK: But then some of us who are older than you are say we all die anyhow, and the next generation will do nothing but space telescope problems and they will have all the time they want.

FREEMAN: For an alternative view on this question of what the Magellanic Cloud clusters are worth, please come to my talk on Thursday afternoon. (Laughter).

KING: I think saying that the space telescope will solve everything is exactly like those people you run into at cocktail parties who ask, "do you still look through telescopes now that you have radio?" (Laughter).

CUDWORTH: First a comment and then a question. The comment: McNamara is working on another proper-motion study, this time M35, and he should produce very nice internal motions there in another year or two. The question, for Ivan or any of the spectroscopists, is whether you do, indeed, see massive binaries in the centers of young clusters?

KING: Somebody yesterday was talking about binaries, was that in an open cluster?

TRIMBLE: There are a couple of examples. To my knowledge probably the central binary in the Ursa Major . . .

KING: Yeah, there are such binaries, but I don't think anybody has answered the question that I think you're really asking. Is there a correspondence between massive binaries that are observed and the massive binaries that show up in the simulations and the theories? That I don't know the answer to, and I think the answer really is "go and look".

KRAFT: Eggen called attention some years ago to possibly the analogous situation between the Hyades and Praesepe that also exists between younger clusters such as α Persei and the Pleiades. That is, α Persei and the Pleiades share essentially the same motion; have identical color-magnitude diagrams; and, in fact, we showed some years ago that even rotational velocity distributions are identical for the single stars in the clusters. So here again, perhaps, is an example of a double-cluster situation that could be embedded in a moving group. And my question is, is there any evidence for the existence of a Pleiades moving group and, if so, how firm is it?

KING: I thought Eggen identified a Pleiades group in the velocity distributions of early type stars.

BLAAUW: Now that is right, but he did not identify another cluster that belongs to that group, and I think that was the question. May I make another remark on the binaries in clusters? What you call hard binaries in that sense is still not what we call close binaries. I think there should be no misunderstanding there. Close binaries are something with separations of the order of, let's say, a few astronomical units to 10 AU. What you call hard binaries in this context, as far as we know, are still of the order of a 100 AU and more, isn't that so?

KING: That is exactly so. I haven't done calculations in open clusters. In a globular cluster the critical distance for a hard binary is about 20 AU. In an open cluster, with lower velocities, it must be considerably larger than that, probably hundreds of AU's.

MERMILLIOD: The clusters that Eggen included in the Pleiades group have very different ages in comparison to the Hyades-Praesepe group. The earliest spectral type in IC4665 is B3, and in 2287 it is B8 or B9, so there is a large range of age in these five or six clusters that are included in the Pleiades group. It is a different situation than in the Hyades and Praesepe moving group.

KRAFT: Do you have any examples of faint stars that belong to those groups? Ones that are, say, fainter than F stars? Where are the faint stars that belong in these groups?

MERMILLIOD: For bright stars membership in this group can be determined by proper motions, but for faint stars we must rely only on open clusters.

KRAFT: I see.

FREEMAN: Well, if there are no more questions, thank you all for a very nice session, and thanks again to Ivan.