FINAL DISCUSSION

King: I should like to emphasize the problem of the cores of globular clusters. A cluster core – within one core radius of the center – has about 10^4 stars, and the N-body calculations have not yet reached this domain. Yet the Monte Carlo results disturb me very much because they are adjusted to represent the right N and they predict a central behavior that we do not observe.

Contopoulos: Is there any evidence whether we have collisions, actual collisions, in the centre of clusters?

King: I don't know how you would observe a collision. There has been a nova observed at the center of a globular cluster; but as far as I know, it was a perfectly authentic nova.

Spitzer: Dr King remarked that there was a discrepancy between the Monte Carlo computations, which predicted a collapsing core, and the observations of globular clusters, which do not show such a collapse. It is not obvious to me that such a discrepancy is necessarily present. We do not yet understand the collapse in detail, but it is conceivable that such a collapse may be intermittent. For example, a hard binary may eject stars from the core and finally get ejected itself by recoil. The remaining stars would then resume their contraction, with another collapse occuring in due course. We cannot predict that this occurs, but we cannot exclude this possibility.

Lynden-Bell: I agree that a cycle of increasing relaxation time can occur with hard binaries forming and being ejected in turn but I would speculate that we might still see the core of high density even when the central binary was removed and that this core might quite rapidly make another binary. Of course this core might be much smaller than the observed core but I am still worried that it is not seen.

Contopoulos: Do we have any evidence from numerical experiments whether a very hard binary can explode or be ejected?

Aarseth: Close binaries can certainly be ejected from small clusters. The recoil effect is particularly strong if the particle masses are similar.

Hénon to Lynden-Bell: You pointed out that in my models, the halo expands faster than the core, so that the contrast increases and there is an apparent contradiction with observations. These models, however, are isolated; while real clusters are subjected to the tidal field of the Galaxy, which will stop the expansion of the halo at some point.

Lecar: Isn't the formation of energetic binaries considerably enhanced in a system with a mass spectrum?

Aarseth: Equal-mass systems do form energetic binaries and at least in small systems the time-scale is not much longer than for unequal masses.

Freeman: The most massive stars in globular clusters are probably about 0.8 $M_{\odot}$:

Hayli (ed.), Dynamics of Stellar Systems, 447–451. All Rights Reserved.

Illingworth's M/L ratios mean that the lightest stars are around 0.1 $M_{\odot}$ and maybe less, so the mass range is not so small.

Severne to Lynden-Bell: In view of the remarkable agreement now obtained between the N-body calculations and the Monte Carlo method based on the standard Fokker-Planck equation, do you think that it is still worth investing much effort into improving the description of encounters, in particular, retaining the curvature of the trajectories?

King: To expand somewhat on Severne's remark, there is a serious gap that I would like to bring up. In the domain of the N-body problem, we have simulations and we have observation, but we have no theory. For stellar encounters, we have the Fokker-Planck equation, but its validity breaks down just in the range where the N-body calculations are done. In this domain the simulations clearly show the importance of large energy changes, and these are just what invalidates the Fokker-Planck equation. Hénon has shown how to calculate the statistics of these large changes, but we lack a way of following their effect in the form of a differential equation. I know of no way of following a diffusion process in which the individual steps are large, except for some recent papers in the Soviet literature that used the Kolmogorov-Feller equation. Can anyone present say how relevant that work is to this problem?

Heggie: In these papers the Kolmogorov-Feller equation is solved numerically and a variety of problems are treated: 'equipartition' between different masses, escape, and so on.

Lynden-Bell: The numerical method is not equivalent to the Monte Carlo approach?

Heggie: No, in this work the distribution function is treated directly, not individual particles.

Hénon: One possible improvement would be to use the full Boltzmann equation instead of the Fokker-Planck equation, which is a limiting case valid only for small velocity changes.

King: I have tried to use the Boltzmann equation directly, by making a Taylor expansion and integrating term by term. It behaves as you would expect: beyond the second order, none of the terms contains the logarithmic factor. The trouble is that the series seems to converge quite poorly; so I gave up.

Hénon: Is it necessary to expand the Boltzmann equation? One should rather use it in its closed form, as an integral.

King: I don't know how. I hope that someone who is here today will go home and do it.

Hénon: Numerically at least, one can imagine how it could be done.

Spitzer to Lynden-Bell: One step would be possible in this direction. In the Princeton computations it would be possible to perturb the stellar velocities in accordance with the exact probability distribution function for two-body encounters, taking into account both the close and distant encounters. Not all features of the problem would be taken into account, but some would be.

Lynden-Bell: I have two general worries about the Fokker-Planck equation and its coefficients.

(1) They are calculated in the absence of the curvature in the cluster orbits although the effect extends to longer distances.

(2) The far field effects neighbouring regions of phase space similarly and should not lead to any change in phase density whereas the Fokker-Planck equation seems to me to assume that they contribute to the changes in phase density so important in cluster evolution.

Severne: A related problem which remains open and which is possibly of some importance is the description of the dynamics of the violent relaxation phase. Taken globally, violent relaxation gives a very satisfying picture of the overall evolution of clusters and galaxies. While the end state is well specified, we have no operational characterization of the evolution during violent relaxation.

Lynden-Bell: Several people have done simulations but I an very doubtful that one can do much analytically; even the theory of the equilibrium does not agree at all perfectly.

Lecar: I understand that contrary to early expectations, the structure of a stellar system with a cut-off Maxwellian distribution of velocities is quite sensitive to the cut-off. Equilibrium statistical mechanics provides no prescription for the cut-off.

Lynden-Bell: The cut off in the Maxwellian does not come out of equilibrium theory without special assumption. However the assumption is the same as one uses in cluster cores that the relaxation is mainly confined to the well bound stars in a central core.

Lecar to Wielen: A comparison of systems with a mass spectrum would provide a sensitive check on the validity of extrapolating N-body simulations to large N. Do you have such a comparison?

Wielen: I made a comparison of the mass segregation found in Monte Carlo models and in N-body simulations. Unfortunately, a detailed comparison is hampered at present, because our computer outputs do not provide the same quantities up to now. A rather global comparison, using the mean stellar mass as a function of radius, indicates no severe discrepancies between Monte Carlo and N-body results.

Spitzer: We have similar results from Monte Carlo computations by Shull and myself for a system with three components. These will be available shortly for comparison with the results by Dr Wielen.

Lynden-Bell: I would like to make the speculative remark that the binding energies of giant elliptical galaxies are around 10^{61} erg or more and in the gravothermal catastrophe the evolution causes the concentration of the energy into few degrees of freedom in the cluster core. Typically this may lead to 10% or so of the energy in the central core or in small systems the central binary. It is remarkable that this would give us 10^{60} erg in giant E nuclei, very much the order of magnitude needed for their radio source explosions. Have you any remarks on this, Bill?

Saslaw: The coincidence between the energy required for radio source and the energy of a massive binary in a galactic nucleus could occur if there is approximate equipartition between the binding energy of the binary and the rotational and magnetic energies of its individual components.

Contopoulos to Lynden-Bell: I was puzzled by your remark that the energy goes into a few degrees of freedom. If one would apply ergodic arguments, one would expect the energy to go to all degrees of freedom, not be concentrated in a few of them.

Lynden-Bell: The gravothermal catastrophe concentrates the kinetic energy into very few degrees of freedom because the entropy increases as one goes further from equipartition in this problem due to the open phase volume of the system.

Miller: The matter of concentration of energy in a few degrees of freedom is easily understood as a state of maximum phase volume in the microcanonical ensemble. The maximum phase volume is attained in a state in which all the (negative) energy is concentrated in a single binary with all the remaining stars at rest at very large distance. Those remaining stars should be at rest because non-zero velocities for those stars requires more negative energy for the binary, which reduces the phase volume more than enough to compensate the increase of phase volume because of the velocities of the single stars.

I prefer this formulation to those based on f_1's because it avoids the logical inconsistencies inherent in the f_1 description. These mainly center about the requirement that correlations be generated at higher order, and the experimental result that the correlation energy is a substantial fraction of the total cluster energy.

Lynden-Bell: I think to make your phase volume statement you need a confining sphere around the system.

Miller: A confining volume is not required.

Bardeen: I would like to bring up the question of halos vs warm disks, and whether a large velocity dispersion in the center can stabilize an otherwise cool disk. The large N numerical simulations carried out so far do not help, but my gas disk calculations seem to indicate that a large Q near the center is not sufficient to stabilize the disk by itself, particularly if a substantial fraction of the mass has a low Q. Much more investigation of this point is required. However, I have been impressed during this meeting by how little observational evidence there is for low Q, since Q is only known for the solar neighborhood in our Galaxy. In the outer parts of a galactic disk large Q is consistent with a velocity dispersion very small compared with the circular velocity, so direct measurements of the velocity dispersion may be only possible near the center, if there.

Innanen: The work of Van Flandern at Washington seems to indicate that G may be at least very slowly time-variable. Are there any comments on this?

Gott: I believe it was Dr I. I. Shapiro's opinion that that result was incorrect.

Freeman: The words 'hot' and 'disk' mean a highly anisotropic velocity dispersion: σ_z must be small for the disk to be fairly thin, yet σ_R, σ_φ must be large. My question is to large N-body computors: would the process that heat the disk keep the heating to the plane, while keeping the σ_z small?

King: One way of converting motions parallel to the plane into z-motions might be the Spitzer-Schwarzschild mechanism, provided we still think that that mechanism is relevant to the circumstances in the Galaxy. The original analysis considered only motions parallel to the plane, and I'm not sure that the full 3-dimensional case has ever been treated in this sense.

Freeman: There is the difficulty, when measuring velocity dispersions in the disks of edge-on galaxies, that differential rotation along the line of sight induces some extra dispersion which would be as large maybe as the dispersion one is trying to measure.

King: There is one galaxy where observations of a hot disk might be available. Many years ago, when Oort discussed NGC 3115, he remarked that either Humason or Minkowski had observed a considerable velocity dispersion. I believe that Schmidt (who is no longer here this afternoon) has some newer and better spectra, and perhaps from them he can help to answer this question.

Schmidt: Image-tube spectra of NGC 3115 were obtained in 1969. No analysis of the velocity dispersion has been undertaken. T. Williams has determined the rotation velocity from these spectra. He finds no evidence for the broad secondary minimum in the rotation curve previously found by Minkowski*.

Gott: The importance of star formation in the formation of galaxies has been mentioned. I think this is a point that can not be overemphasized. It is my own feeling that this is the key factor in determining whether a spiral or an elliptical galaxy is formed, of what disk and halo components are produced. Another important question is the dynamics of the early gaseous component. It is important to know what the early gas clouds are like, (i.e. what are their mean free paths between collision) so that one may know how dissipative and how viscous the early gas may be.

Freeman: I want to mention again the young globular clusters in the Magellanic Clouds – these are only a few collapse times old, have stars in the mass range at least 10 to 0.5 $M_{\odot}$, and appear from their brightness distribution to be dynamically like the old Milky Way clusters. There is cluster formation going on here before our eyes and we could maybe ask why. We can easily now do obvious things like comparison of distributions for different mass classes in these clusters. If there is anything N-body computors would like us to look for, please let us know.

Larson: It is certainly clear that further progress will require a better understanding of the gas dynamics and star formation processes in forming galaxies. As a theoretician, it'is my impression that further improvements in our understanding of star formation in galaxiws will have to come from observations, so I would like to encourage observers to try to identify and study carefully any galaxies which may still be in the process of formation. A possible example is NGC 5253, a small galaxy of elliptical outline whose interior structure is very irregular and shows evidence of concentrations of gas and young stars. This galaxy has recently achieved notoriety as a prolific producer of supernovae, which indicates very active recent or ongoing star formation. Perhaps this is the place to look to understand more about star formation in forming galaxies.

* This answer was sent later on by M. Schmidt to the editor.