WHAT NEXT? PRIORITIES IN THEORY AND OBSERVATIONS

Summary of the panel discussion.

Panel Members: I. R. King, L. Spitzer (chair), A. Toomre, S. D. Tremaine, T. S. van Albada, S. D. M. White

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The papers presented at this symposium show that the theory of a spherical cluster composed of mass points has made great progress; the major processes occurring seem to be well understood in principle. In particular, evaporation and mass stratification account for the early contraction of a cluster core, while the gravothermal instability is responsible for the final collapse. Formation of binaries by three-body encounters can stop the collapse, though the detailed nature of the ensuing expansion is still not fully explored. While further interesting research remains to be done on the evolution of a spherical aggregation of mass points, the broad outlines of the subject seem clear.

There are two main areas where pioneering research seems needed in the future years. The first of these areas concerns the finite size of the stars. While tidal capture seems reasonably well understood, the somewhat closer and almost as numerous direct physical collisions pose many problems. How does a composite object formed from such a collision evolve with time? Its evolution as an isolated object and its interaction with single stars, binaries and other objects all need further study. Such objects may be stepping stones in the complex path to the formation of neutron stars and black holes, and may affect core dynamics in ways not yet analyzed.

A second area that calls for new research includes the orgin and history of actual clusters. Some properties of clusters, such as the observed chemical gradient of stars as a function of distance from the cluster center, may provide clues to the early history of these systems. The possible early disruption of clusters, resulting partly from extensive mass loss by the more massive stars, and partly from interactions with the Galaxy, needs further analysis. While many traces of initial condition and early history have been obliterated by later evolution, some may still remain. Their study may clarify our views of the early history of the Galaxy.

J. Goodman and P. Hut (eds.), Dynamics of Star Clusters, 499–509.

https://doi.org/10.1017/S0074180900147734 Published online by Cambridge University Press

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As an outsider, I have enjoyed this Symposium very much. It has certainly taught me again that the globular clusters -- though, with my profuse apologies to Bob Mathieu, probably not their poor open cousins found in our present-day disk -- constitute a fascinating subject, so teasingly simple at first sight, but so rich with dynamical phenomena and paradoxes. An this setting at the Institute for Advanced Study has also proved suitably glorious, especially now that the organizers have kindly brought back the Sun after all that rain.

Perhaps more interesting than such praise, though, may be two aspects of this Symposium that leave me at least mildly disappointed. One was simply the fact that all the hard and clever work by several of you from this past decade, carrying the dynamics soundly beyond the core collapse that loomed large as recently as the 1974 meeting in Besancon, has not been rewarded by anything remotely as memorable as, say, the formation of a supermassive central object or even any rapid dissolution of the cluster itself. In that sense, your recent solid consensus that binaries form and take over the heating is certainly a pity. It means that the answer to Haldan Cohn and Piet Hut's cheerful query "Is there life after core collapse?" is merely "Yes, but it is pretty dull."

The other thing that disappointed me, a bit more seriously, is that during the past few days I have heard scarcely a word of speculation or of reasoned puzzlement directed at the grand unsolved questions of how the globulars came into being in the first place, or how they ended up where they are now. When we next gather to discuss clusters, I hope we will all agree that at the 1984 meeting in Princeton we were silly to shy so completely from these vague, fuzzy, untidy, and yet ultimately very vital issues. Given all that we know already about the puzzling correlations of metallicities with the present kinematics and whereabouts of globular clusters -- and also about giant molecular clouds, young LMC globulars, some remarkably metal-rich globulars claimed in Cen A, giant starbursts detected in other peculiar galaxies especially by infrared techniques, and yes, even about the mergers of galaxies -- I cannot help feeling that the time is ripe for some bright astrophysicists to pull it all together into a cohesive dynamical picture that afterwards, like core collapse, will be declared only too obvious!

For what it is worth, my own guess remains that, like today's scrawny open clusters, all globulars were basically manufactured in the relative peace, quiet, and high densities found in the <u>disks</u> of galaxies, and that they were evicted from such nurseries only subsequently during the mechanical violence of early mergers that helped to create the spheroids or bulges. I would not grieve desperately if such conjectures were proved wrong. But I would be sorry indeed if they and other plausible stories like, say, the old Dicke-Peebles theme emphasizing Jeans masses did not receive a close and critical scrutiny at nice future gatherings like this one.

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I would like to raise a few questions related to the origin of globular clusters, which should enter sooner or later theories describing their evolution.

First there is the question whether or not the present structure of globular clusters still contains information regarding the formation phase. One might think that internal relaxation and gravitational shocks have obliterated all memory of the initial conditions. But there is at least one piece of information which suggests otherwise: the absence of clusters with high central and mean densities in the outer parts of the Galaxy; there is a corresponding lack of low density clusters in the inner parts of the Galaxy. On the whole, mean and central densities of clusters are correlated with the background density of the population II component, indicating that the present central densitites of globular clusters still bear some relation to their central densities after formation. The absence of low density clusters in the inner region of the Galaxy can probably also be attributed to disruption by gravitational shocks, but it is hard to understand the absence of high density clusters in the outer regions other than in terms of primeval conditions. (If it is true that entire clusters have been disrupted by shocks it would be important to include energy input by shocks in studies of core collapse.)

Next one may ask whether there are other differences between clusters that may affect their evolution. In other words, is it proper to work with some standard cluster representing the 'mean,' or is there such a wide range in individual properties that there is also a large variety of evolutionary 'path?' Take, for example, the number of primeval binaries. The blue stragglers in M3 may be an indication that this cluster is exceptionally rich in binaries. If so, this has probably a pronounced effect on its dynamical evolution. Can this effect be predicted, and observed? Further, there are probably differences in the mass spectra, which will also effect the evolution.

These considerations show that, from the theoretical point of view, there is probably not a unique path of dynamical evolution. This implies that globular cluster evolution is not such a 'clean' problem. A full description will involve several physical processes, leading to a large number of parameters. Yet, the overall similarity of globular clusters shows that different conditions may still lead to similar global properties.

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For the first time we seem to have a coherent picture of core collapse. Three features of this picture have particularly impressed me:
1. As Jeremy Goodman phrased it, core collapse seems to be "robust." Every model presented here, no matter how crude, has exhibited core collapse. N-body computations, whether with few or many bodies, chosen with a single mass or from a spectrum of masses; Monte Carlo calculations by Hénon, by Spitzer, or by Stodółkiewicz; self-gravitating gas spheres in insulating boxes; numerical solutions of the Boltzmann equation with or without strong encounters and with or without anisotropic velocity distributions -- all of them collapse in a qualitatively similar fashion. The pre-collapse evolution of a cluster appears to be a calculation which is almost impossible to do wrong. Even McMillan and Lightman's complicated and ingenious hybrid code undergoes a standard core collapse, which implies that either their code or core collapse is surprisingly reliable, though I am not sure which. The re-expansion of the core after collapse appears to be a more subtle process, which is rather more difficult to get right, but I am confident that this process as well will be understood in the next few years.
2. Core collapse is surprisingly unspectacular. Here we have a cluster of 10^5 stars, whose only goal in life is to increase its entropy, and all it does is to spit out a few garden-variety binary stars of which hundreds are seen already in any small volume of the galactic disk. A few years ago many astronomers believed that core collapse represented the death of globular clusters; now it seems to be only the sort of mild upheaval which one might associate with puberty.
3. The study of globular cluster evolution is now similar to the study of stellar evolution in that almost all of the important physical processes are known and relatively well understood. Dr. Stodółkiewicz's code, in particular, is strongly reminiscent of a stellar evolution code, and just as in the study of stellar evolution we can look forward to ever more accurate evolution tracks as the cross sections and transport coefficients are improved. The great advantage over stellar evolution is that we don't have to treat convection. The disadvantage is that we don't have a Hertzsprung-Russell diagram (yet); there is still no accepted observational criterion even for distinguishing pre-collapse from post-collapse clusters.

Progress on other theoretical topics has been much less rapid. We need more work on the evolution of the outer parts of clusters, by evaporation and by external gravitational shocks from giant molecular clouds and the galactic disk. Our understanding of even the classical problem of stripping by the Galactic tidal field is still rudimentary, although some progress was reported here by Seitzer, and a series of beautiful papers on tidal stripping were written by Hénon around 1970. Since we now know that clusters survive core collapse, they must ultimately die from processes like evaporation, tidal stripping and external shocks. We need to know the lifetimes of clusters and what they leave behind when they die. It is quite possible that hundreds or thousands of globular clusters have already died, and a proper understanding of their fate may resolve many puzzles in galactic astronomy (such as the galactic bulge X-ray sources, to name just one).

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I have been impressed by the extent to which the discussion at this meeting has centered on the evolution of the inner core of globular clusters. The investigation of core collapse and its posterity raises many fascinating and fundamental questions in theoretical N-body dynamics, and, as we have seen, the last few years have brought remarkable progress in understanding the relevant processes. Nevertheless, I think that we should be wary of concentrating too much on a region which is extremely difficult to observe from the ground, and may well not give up its secrets even to Space Telescope. Excellent kinematic and metallicity data are now being acquired from stars from the core right out to the tidal radius in several clusters. It seems to me that we may learn more about the present dynamical state of clusters. as well, perhaps, as about their origin and evolution, by studying the bulk of the cluster which we can see, rather than the innermost core which we cannot. The very pretty analogies between stellar evolution and cluster evolution which Piet Hut likes to draw should not blind us to the fact that the stellar distribution of many clusters may be affected more by external than by internal influences. The data shown by Freeman, Lupton, Mayor, Seitzer and others provide much material for theoreticians to digest, and complex modelling of the type discussed by Stodółkiewicz, Seitzer, Terlevich and Wielen is needed to decide which processes determine the present structure of clusters.

A question which has been barely touched on here is that of the origin of star clusters. Stellar dynamics alone will clearly be insufficient for an understanding of the processes involved, but it would be valuable to know the extent to which cluster evolution has erased any memory of initial conditions from the present structure. The problem of cluster formation is as ill understood and as messy as the related problems of star and galaxy formation but it is clearly of major importance. I believe the investigation of young clusters and of cluster-forming regions both in our own Galaxy and in the Magellanic Clouds offers us some hope of understanding how, where and why clusters form, as well as of showing us the structure of clusters as they are born. The relationship between these objects and Galactic globular clusters is unclear, but it would become much clearer if we understood how the properties of a cluster depend on the medium from which it condenses. We should perhaps also remember that most stars may form in loose associations and in open clusters, rather than in rich globular systems. For these reasons I was happy to hear Bob Mathieu discuss the properties of young open clusters, and I would have welcomed some discussion of the dynamics of cluster-forming regions as inferred from recent infrared and millimetre wave observations. While it is difficult to observe very young clusters and to provide a coherent theoretical framework in which to interpret such observations, I believe the possible payoff in increased understanding of the cosmogony of stars and clusters more than justifies the risk of prospecting in such a mire of complexities: in the words of an old saying from the north of England "where there's muck there's brass."

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This meeting has brought theoreticians and observers together, but it has seemed to me that the communication of each group has been among its own members, without a lot of dialogue between the two. As I said in my own talk, I am interested in answers and believe that they come from interpretation, which is so often the result of juxtaposing observation with theory. I wish we could do more of this.

Clearly, the topic that has had the most attention is core collapse. There seems to be general agreement that it must occur, and also fairly good agreement about how it proceeds. (There is of course some disagreement about gravothermal oscillations, but they are each of such short duration that I don't think they are likely to be observed.)

What I have been most interested in is knowing how a cluster should behave after core collapse. Here I thought that the answer was becoming clear: The collapsed core (presumably stabilized by binaries) becomes isothermal with only a little re-expansion, and its center is for practical observational purposes still singular. Further expansion is very slow, so the core keeps this singular isothermal profile thereafter. Tidal-capture binaries are more numerous than 3-body-formed binaries, but they don't matter much, because they're too hard. But now I'm not so sure. I've heard it said that perhaps tidal binaries do have an important effect and collapsed cores re-expand completely. I wish we had a clear statement on this.

What I do know is that several clusters have what look like collapsed cores -- but not many. What's more, there don't seem to be intermediate cases; either a core is collapsed or it's normal. But there are things about this that bother me. Qualitatively it fits with the picture of cores remaining in a permanent post-collapse state, but quantitatively there aren't enough such clusters. There are other cores with short relaxation times, but they haven't collapsed. Or maybe they've collapsed and re-expanded -- but in that case why don't we see other intermediate types that are still in process of re-expanding?

I don't think that either the theories or the observations are good enough to answer these questions. I've said where I think the

https://doi.org/10.1017/S0074180900147734 Published online by Cambridge University Press

theories are lacking (or perhaps where my understanding of them is lacking), but there's one more area that ought to be mentioned: mass mixtures and segregation. At the same time let me say that I doubt that this is a serious problem, because otherwise real clusters wouldn't look so much like single-mass models. (I think that this is because a lot of the mass of a cluster is in white dwarfs, which have nearly the same individual mass as the red giants.)

On the observational side, I think that we clearly need better studies of clusters that have collapsed cores and of other high-concentration ones that don't. A lot of this can be done with carefully directed ground-based observations, and it very much needs to be done before Space Telescope flies. But there is no doubt that ST will have a major impact on this problem, because it's a problem that very much needs high-resolution star counts. I hope that a meeting 10 years from now will have all of this settled and can indeed treat cluster evolution not as a speculation but as a science.

DISCUSSION

SEVERNE: I would like to suggest that a fundamental problem remains in connection with the Fokker-Planck equation itself, which lies at the basis of so much of the work on globular clusters. The encounter term in the Fokker-Planck equation is derived by perturbing rectilinear trajectories. This approximation is essentially inconsistent for self-gravitating systems: it leads to mean free paths, orders of magnitude larger than in the systems studied and gives rise to the oldest unresolved divergence in statistical mechanics. It now appears possible to study encounters by perturbing more realistic (quasi-) periodic trajectories. My poster contained a suggestion in this direction; may I plead that more work be done on this basic problem.

HEGGIE: Concerning the disagreements on gaseous calculations between myself and Drs. Bettwieser and Sugimoto, Dr. Sugimoto has made the interesting suggestion that there may be an analogy with numerical difficulties encountered many years ago in the computation of helium shell burning in stars. On his advice I intend to check the time-step control in my code.

I wish also to express my agreement with those panel members who urged the development of satisfactory models for the outer parts of clusters. I suggest that those with dynamical codes should produce a sequence of analytic fits to their models at different stages of dynamical evolution, and present these in a form suitable for use by observers. But I would discourage attempts to build elaborate multi-component cluster models from lowered Maxwellians in equipartition, since there is little dynamical justification for such distributions even in a relaxed core, let alone in the rest of a cluster.

SHAPIRO: Would the panel be willing to anticipate the results of their posed question on the origin of clusters by answering the query "do you believe in miracles?"? For if you don't, how do you explain the curious fact, first pointed out by Spitzer 10 years ago, that roughly half of all clusters have sufficiently short relaxation timescales to have undergone collapse already while the rest have not. Why half and not something closer to unity or to zero? This fact is telling us something - perhaps that there were once many more clusters than we can now observe but have since disrupted.

TOOMRE: Shall we have a poll on whether we believe in miracles or not (laughter)? You may well be right, globular clusters lead a dangerous life and we are after all dealing only with the survivors.

SPITZER: The possibility of cluster disruption is a very fascinating one, and has to be taken very seriously. It is not easy to find the small residue that a cluster would leave, but it would be an interesting problem observationally.

VAN ALBADA: I think there is some evidence for moving groups amoung population II stars.

TOOMRE: Let me turn Shapiro's "miracle"-comment around. It has now become rather disreputable to claim that the solar system has been formed by miracle, even though we know of only one example yet. In contrast, we are dealing here with over a hundred miracles around our galaxy alone, so we have a certain safety of numbers, folks!

WHITE: I am not quite sure what the "miracle" is either. One thing which impressed me from all these calculations of core collapse, is how little it seems to do to the cluster. They do not look that much different after core collapse than they did before. It is not clear to me that by changing the parameters of the cluster you could not make many of the observed clusters have their core collapsed and yet have that fact not show up in the observations. Therefore I do not see any need for them to dissipate completely as a result of collapse-like processes. There may very well have been very many more clusters at the time of the formation of the galaxy, but various people have put forward theories that explain why the ones we see are the ones that are left now.

KING: I just do not see the mechanism for destroying a cluster. Core collapse binds it more strongly. In particular the calculations that Haldan Cohn showed us, indicate that a cluster does not come very quickly out of its collapsed state but rather reexpands very slowly. I don't see it at all as a way of getting rid of clusters. I think globular clusters are with us, to stay.

SPITZER: I think the disruption has to be done by some other, external, agent.

KING: Yes.

LARSON: I was glad to hear some of the panelists, especially Alar Toomre, emphasize the problem of the origin of globular clusters. In addition to their size, mass and chemical content, another important characteristic that globular clusters carry around from the time of formation (perhaps with some modification) is the stellar mass function. I've become increasingly impressed with the importance of the Initial Mass Function in controlling the structure and evolution of clusters, expecially if the IMF contained substantial numbers of massive stars that evolved to leave white dwarf, neutron star, or black hole remnants. The new data on velocity dispersions now becoming available suggest that at least some clusters contain substantial amounts of mass in dark form, presumably remnants, and such remnant populations would constitute another important tracer of formation conditions.

SPITZER: Thank you for pointing out this important aspect of the mass function.

DJORGOVSKI: I would like to iterate Ivan King's remark that there seems to be a discrepancy between the number of detected post-collapse clusters and the number of clusters which should have done it, but show no signs of it. Post-collapse cores are apparently easy to detect, and there are no "intermediate" cases presently known.

My second remark concerns another queer duck, M 79. That cluster looks like nothing that anybody ever predicted, with its strange bend in the surface brightness profile. It cannot be fitted with either a King model, or a power law. According to the error-bars, the bend is highly significant. Unless Nature is playing a particularly devious statistical fluctuation on us, I think that we may be dealing with something new. Please remember that there was only one M15, until somebody tried to look for more.

INAGAKI: I think there are two important problems. (1) Why are only a few binaries observed in globular clusters though theories predict that a lot of binaries are formed by tidal dissipational encounters? (2) It is necessary to construct realistic models of globular clusters including a mass spectrum, and to take account of deviations from equipartition.

GOODMAN: I'd like to make a few scattered points. First, a plea to the observers: I would like to know how far out the globular cluster system extends in our own galaxy and in nearby bright spirals such as M31. Second, I would like to point out to Drs. King and Djorgovski that it *is* easy to hide post-collapse phenomena in clusters: it is only necessary to have a component of heavy, non-luminous stars that collapse and puff up the luminous component. If the heavier stars do shine, one might see a shoulder in the luminosity profile such as Dr. Djorgovski noted for M79. Thirdly, I'd like to call everyone's attention to the calculations of Dr. Stodolkiewicz, in which 98% of the cluster mass evaporated. I do not understand which of the many physical effects he included is principally responsible for the evaporation, although tidal truncation clearly plays an important role. His results may have very important implications for the possibility of cluster disruption.

Finally, a plea to the purest theoreticians: I think there needs to be more work on the connections between short-mean-free-path gaseous systems and long-mean-free-path stellar systems. I do not understand why gaseous models work so well and agree in such detail with stellar-dynamical calculations.

GRINDLAY: Let me add several comments: (1) As I mentioned before, I urge this from dynamicists among us, to seriously attack the problem of cluster disruption by Giant Molecular Clouds. Since a simple application of Spitzer's (1958) impulse approximation theory suggests disruption *will* occur, specifically: what will such a dissolving cluster look like? What is the expected timescale for dissolution of the final $\sim$100 stars? I would hope that by the time of the next symposium on stellar dynamics of clusters these problems will have been treated. (2) We have discussed the question of dark matter in globulars extensively, but only in the context of dark remnants (neutron stars, white dwarfs, etc.) which we now know *must* be in cluster cores. What about the questions of dark matter in globular cluster halos - such as the axions suggested by Peebles? What about the question of "low mass" dark matter, such as proposed by Bahcall for the galactic plane, as a constituent throughout globular clusters? Again, it seems these questions must be addressed for a complete discussion (at the next such symposium) of the internal dynamics of globulars. (3) Finally, partially in response to many of the comments just made on such questions of cluster origin, evolution and relationships (dynamical , chemical, etc.) to their parent galaxies - let me mention that we have proposed an I.A.U. Symposium on the "Large Scale Properties and Structure of Globular Cluster Systems" to be held at Harvard in August, 1985. This would also serve to commemorate the centennial anniversary of Harlow Shapley's birth. I hope this conference will take place and that you all can participate.

COHN: The results that I presented here, as Dr. King has noted, indicated that three-body binary heating does not cause cores to expand back to their pre-collapse size. However, Dr. Stoldolkiewicz's additional effects do appear to indiate an expansion of the cluster cores to their pre-collapse size. Thus, I would like to speculate that a post-collapse cluster might look rather similar to a pre-collapse cluster, the differences in structure being subtle. This is an important issue for both theoretical and observational investigation.

FREEMAN: I was delighted at the emphasis that the panel put on cluster formation. Please remember the LMC: it has globular clusters of all ages, down to a few million years. We can in principle provide a lot of observational information about their mass functions, chemical properties and internal kinematics. So, if anyone working on the theory of cluster formation would like to know what a real forming cluster is actually doing, please ask (laughter).