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CELESTIAL MECHANICS: PERSPECTIVES FOR THE NEXT TEN YEARS

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Some guesses are offered about possible developments in the astronomical areas of celestial mechanics and stellar dynamics. These are followed with some warnings about placing too much reliance on large computational studies. It is too easy to design the computation to prevent any discoveries. It is too easy to design the computation so a discovery would not be noticed even if the computation represented it correctly. Some independent checks are crucial.

I. PERSPECTIVES

Predictions where developments are likely to occur in the next ten years are notoriously chancy in any scientific area. Anyone who questions that ought to look back at the older predictions about where some field of science would go in the succeeding ten years. It can be a chastening experience, especially if you made the predictions.

A word of warning. I am here under false pretenses. I am not a celestial mechanic (some honest celestial mechanics are here at this workshop). Rather, I work in stellar dynamics. The two fields differ in emphasis: few vs many bodies and high accuracy vs qualitative effects. The break of few/many bodies comes at about ten—about the number of major planets in the solar system.

Both are examples of the gravitational n-body problem. At $n = 2$, we get the familiar Kepler problem. At $n = 3$ we get the celebrated 3-body problem that dominated much of 19th-century mathematics. It is a problem of unbelievable richness. It led (in the hands of mathematicians like Poincaré) to many of the modern techniques we've heard about at this workshop: topological methods, surfaces of section, Hamilton-Jacobi (and, through that, symplectic maps) $\cdots$. At $n = 4$ and more, all bets are off. A finite-time singularity is possible at $n = 5$; an explicit example was described recently in which several of the particles fly off to infinity in finite time. I like n about 100,000. But already the Kepler problem contains hints of trouble. The "solution" in the mechanics books is not a solution in the ordinary sense. It is implicit. Just try to express x and y as functions of the time, and you will see what I mean.

To return to the perspectives, a good way to proceed is to look back over the past few years, to see where advances came and to try to identify the forces that drove those improvements. This listing of important advances is highly personal; you would get a different list if someone else were making it up.

Some important advances in celestial mechanics are (1) Inclusion of Lie algebras (Deprit, Hori). These have been particularly important for computer-generated series developments. (2) Greatly improved accuracy of ephemerides, mainly driven by much more accurate observations. Ten to twenty years ago the residuals in the lunar orbit were several kilometers; today they are 10–30 cm. Residuals in solar-system ephemerides are less than 1000 km in the orbit of Saturn. A global ephemeris covers about 15 000 orbits of the LAGEOS satellite with residuals on the order of 10 cm over the entire period. These remarkable improvements were largely driven by greatly improved observations: lunar laser ranging, spacecraft ranging, radar ranging. They have forced inclusion of non-gravitational terms into the equations: atmospheric drag, light pressure, etc. (3) Realization of some of the richness and complexity of possible solutions: chaos, ergodicity, etc. (Moser).

Three advances stand out in stellar dynamics (1) Larger computers allow better resolution in grid models. These are based on n-body calculations like those described for plasmas by Birdsall and Langdon. Current programs allow 10^5 to 10^6 particles in a 64^3 active cartesian grid. These programs have been used for lots of experimental studies in the dynamics of galaxies. Newer computers (e.g., Cray-2 with 256M words of memory) will allow finer grids, at least to $256.^3$ (2) Special geometries for special problems (e.g., polar coordinates for galaxies or star clusters strongly concentrated toward the center). These are specialized n-body problems. (3) Static self-consistent galaxy models by linear-programming methods. Schwarzschild constructed model galaxies with prescribed shapes and radial runs of density. The idea is to calculate lots of orbits in the (smooth) gravitational-potential field generated by the prescribed density distribution. Then one imagines particles scattered along these orbits and picks just the right number of occupants along each orbit to regenerate the prescribed density distribution. Linear programming comes in because negative occupancies are not allowed. This is an approximate method, one that achieves self-consistency within some tolerance. Stability of the resulting models, while crucial, can only be decided by recourse to n-body experiments like (1) or (2) at the present time.

The pattern that emerges in this survey is that progress is driven by (a) new kinds of observational data (experimental data in the language of physics), (b) new ideas, and (c) available computer hardware. Trends in the next few years from (c) are reasonably predictable—more experiments, more kinds of experiments, finer spatial resolution. A lot depends on access to the computers, and that comes down to dollars.

Progress from (a) is also somewhat predictable, at least in stellar dynamics. Greatly improved angular resolution and sensitivity (VLA and VLBA in the radio, ST in the visible, ultraviolet, and near infrared) and new wavelength ranges (IRAS and SIRTF in the infrared, x-ray satellites, continued IUE work in the ultraviolet) will produce finer observations that will strain our modeling capacity, driving heavier computational use. Models that permit much richer detail and that include more kinds of physics (gas dynamics, radiative transfer) will be required. These advanced observational facilities will produce new science only if we are able to interpret the observations through detailed models. Astronomers have a challenge to keep a budgetary balance between data acquisition and interpretation to maximize scientific yield.

Anyone who tries to predict the next good idea (b, above), much less the most influential idea in the next ten years, is a fool. Yet, ten years hence, one of those may well turn out to be the feature that drove the really great scientific advances. Science responds to the opportunities, but we can't predict where the opportunities will arise.

It may seem odd that perhaps the most charming and challenging dynamical problem of them all—Saturn's rings and other planetary ring systems (Uranus, Jupiter, and possibly Neptune)—is not on the list. So far, it has not produced any radically new insights or techniques. This problem is charming because of the sheer beauty of the objects. It is challenging for the same reason that long-term orbital stability in accelerators or storage rings is challenging. From a dynamical point of view, Saturn's rings are the oldest system we know. The orbital period is about a day, and the system has probably been around five billion years. That gives it an age of about 10^{12} orbital periods. All kinds of secular effects are important. The fine "phonograph-record" appearance has resonances written all over it. Great efforts on the dynamics of planetary rings during the next ten years can be predicted with certainty; some progress is likely. Work on this astronomical problem and on the problems of orbital longevity in accelerators and storage rings can each benefit from progress in the other. I urge you to keep in touch with each other.

Quite apart from the details, I confidently predict an exciting ten years.

II. COMMENTS

Enough of the predictions. We leave that topic and switch to a completely different subject: some warnings concerning possible pitfalls I think have not been adequately stressed in the presentations and discussions I have heard in this workshop. These comments are based on experience amassed in several years of running large gravitational n-body numerical experiments.

The strongest warning is: Look out for the problem you didn't foresee. The thing that will kill you (if anything does) is something you didn't expect. This means that you must allow unexpected things to happen and, further, that you must be able to recognize them when they do. Let me illustrate this by something we found that was totally unexpected (to us, at least). It came in one of our problems in galaxy dynamics.

We were studying a disk of stars embedded within a larger mass that we think of as an elliptical galaxy. The problem is the dynamical development and longevity of the disk. Some elliptical galaxies have dark lanes crossing them, and the disk represents the dark lane. These objects are called dark-lane ellipticals. The prototype is the radio source, Centaurus A (= the galaxy NGC 5128). Elliptical galaxies look like featureless "fuzz balls." They have bright centers, but the brightness falls off smoothly in all directions away from the center until the galaxy can no longer be seen against the night sky. There is no sharp outer boundary. Quite a few dark-lane ellipticals are known. Not all are strong radio sources like Centaurus A. The party line says these systems result from a recent merger of a (smaller) dislike galaxy with the elliptical galaxy, and that they are short-lived.

We wanted to see what we could learn about the dynamics of these systems by numerical experiments. We set up a disk of stars inside a self-consistent self-gravitating "galaxy." The galaxy is centrally condensed and its density decreases outward. Its density is more or less constant on nested oblate-spheroidal surfaces. The disk contains a negligible fraction of the total mass (1%). It is rotationally supported against the gravitational forces of the background galaxy. These models form a one-parameter family, characterized by the angle between the disk normal and the spheroid axis.

We ran this problem at NASA-Ames Research Center, with a group that included Althea Wilkinson of the Astronomy Department at the University of Manchester in England and Bruce F. Smith of the Theoretical Studies Branch at Ames. The series of experiments and the initial conditions (these are initial-value problems) were designed by Althea Wilkinson. All three of us participated in the scientific parts of the project.

A motion picture was shown at the workshop to illustrate the three-dimensional form adopted by the disk after some time. The disk precesses in the nonspherical potential. It precesses at different rates at different radii, which produces a beautiful warp. The film makes this warped shape apparent.

We routinely make motion pictures of all experiments. They are essential to help us understand what is going on dynamically. We can see when something unexpected is happening. Our most important discoveries have all come from features first noticed in the motion pictures. Later we analyze things in more detail. The motion pictures tell us what to analyze. Quality control is a big problem with motion pictures.

The unexpected part showed up as we were studying this system on a new high-speed graphics device at Ames. We had zoomed in for a tight view of the center, watching temporal developments. The center was whipping about. We were concerned lest this be a numerical instability. A motion picture showing a close-up view of the disk center was made as one of our checks. That motion picture was shown at the workshop as well. It shows a remarkable combination of orbiting, rotation, precession, and nutation all going on at once.

The pattern of precession plus nutation convinced us that the process was physical, not numerical. Another check is shown in Fig. 1, which is a plot of the trajectory of the normal to the disk center, as seen on the unit sphere. The view in Fig. 1 is down the spheroid axis. The circle represents the equator of the sphere. Orbital frequencies, precession rates, nutation rates, and several other frequencies characteristic of the problem have been determined.

The galaxy seems to have decided that its center was going to be somewhere other than where we thought it would be. The disk center was left orbiting around the new galaxy center. A self-consistent galaxy can shift its center, a discovery that was quite unexpected. The center moved by an amount that would correspond to 100 parsecs or less in our galaxy. Some of the features in the motion picture look a lot like recent VLA maps of the galactic center.

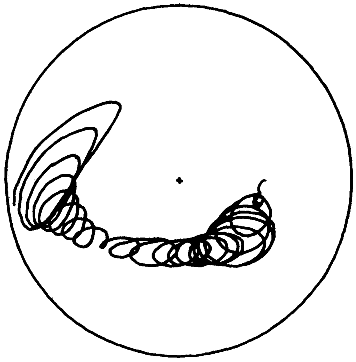

FIGURE 1 Trajectory of normal to center of disk as seen on the unit sphere. The trajectory starts at the right side (small amplitude) and winds around downward and to the left, ending on the left side with large amplitude. It does not loop around the pole of the sphere.

The moral for this workshop comes in three parts.

(1) Look out for the unexpected. I worry that some of the techniques suggested to extend orbital integrations to very long times for the Superconducting Super Collider (SSC) design may preclude responses that didn't already show up in shorter integrations. The very purpose of long-time integrations could be defeated if this happens.

Examples can be cited where some essential physics was missed because of approximations. Axisymmetric geometries provide one such example. Angular-momentum transfer through nonaxisymmetric gravitational potentials is precluded. High central densities in real galaxies show that angular-momentum transfer must have taken place at some stage of the galaxy's formation. This cannot happen in simulations constrained to remain axisymmetric. Such axisymmetric simulations of protogalaxy collapse led to the wrong density profile, and to great pronouncements about "cosmological infall" and other stuff like that. Recently, simulations starting from identical initial conditions, but allowing nonaxisymmetric shapes along the way, have produced density profiles that agree with observation.

Experiments with actual beams in a ring are probably the only safe check that you haven't designed SSC orbit calculations in such a way that real, physical, catastrophes are precluded by some feature of the computational design. The importance of experimental verification cannot be overemphasized.

(2) Integration accuracy. This question is similar in spirit to quality control in industrial processes, an area in which there has recently been a change of philosophy driven by VLSI production. The emphasis has shifted from trying to enforce high quality on individual items, which leads to unacceptable rejection rates, to designing the circuits so they will operate reliably with readily attainable production tolerances.

Our approach in integrations is something like this. Global (collective) effects dominate in galaxy dynamics, and we can study these without need for high integration accuracy on any one orbit. We use many particles instead. You go after things with a fine scalpel, seeking high accuracy, while we use a shotgun approach, looking at many orbits with poorer accuracy on any one orbit. It is not obvious how to redefine the SSC design problem to reduce demands on integration accuracy, but it is worth some effort to see whether it can be done.

(3) Think about the physics. Describe the physics in a form that fits the computer. This cuts out one level of approximation. Approximations are involved in expressing physics in the form of differential equations, and further approximations arise in solving those differential equations on the computer.

Our numerical method preserves phase measure and particle number exactly, while energy and angular momentum are conserved only approximately. We want to kill two-body relaxation and to study effects on a short (dynamical) time scale. This approach is well suited to that need. You probably can't be cavalier about anything when you want to go to long time scales. But it still helps to look carefully at the physics you are trying to represent and to remember that the physics is not embodied in a set of differential equations.

III. ACKNOWLEDGMENTS

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