Donald Lynden-Bell (*left*) with colleagues Allan Sandage (*center*) and Olin Eggen (*right*) at the Institute of Astronomy in Cambridge in summer 1985. Picture taken by Gerry Gilmore.

D. Lynden-Bell.

Searching for Insight

Donald Lynden-Bell

Institute of Astronomy, University of Cambridge, Cambridge, CB3 0HA, United Kingdom; email: dlb@ast.cam.ac.uk

Annu. Rev. Astron. Astrophys. 2010. 48:1–19

The *Annual Review of Astronomy and Astrophysics* is online at astro.annualreviews.org

This article's doi: 10.1146/annurev-astro-081309-130859

0066-4146/10/0922-0001$20.00

Key Words

biography, dynamics, galaxies, magnetohydrodynmic (MHD) jets, quasars, relativity

Abstract

Is space-time only brought into being by its energy content? The jury is still out, but other questions that have been with me for much of my life—giant black holes in galactic nuclei, the formation of the Galaxy, the connection between first-order phase transitions and negative specific heats, the cause of the large-scale flow of galaxies relative to the cosmic microwave background—have all received reasonable answers. I have found great fun in understanding the dynamical mechanisms underlying such phenomena as magnetohydrodynamic jets, relativistic disks, and the bars, spirals, and chemical evolution of galaxies. The great challenges for future astronomers will be the exploration of the 96% of the Universe now believed to be neither atomic nor baryonic but perhaps partially leptonic. However, most advances do not come via frontal attack but from "bread-and-butter" investigations in related areas where observation is possible today!

1. SCIENTIFIC PHILOSOPHY

God is truth, so the truth is a way to God. So argued the late medieval monks and it opened the way to the support of study not only in theology but also in natural philosophy (the name science was coined later). William of Occam was a monk living in Surrey in the years 1285–1349. He devised one of the most important principles for the advancement of natural knowledge. Even today, Occam's razor is often too sharp for the most imaginative but it remains a vital tool in the development of science. It does not show us a direct way to truth, but its economy of hypotheses curtails theorists' wild castles in the air. Newton (Newton 1730) thought that his studies of natural philosophy revealed something about the nature of God. By contrast, Darwin was fearful of the religious reaction to his work, which did indeed lead to a mighty revolution in theological thought that has still not percolated to all of Christendom!

As scientists, we must be led by curiosity over the mechanism of the natural world, but doing bread-and-butter science, straightforward extensions of what is known in order to elucidate new phenomena, is the main job. We should not spend all our time groping at great problems that may be beyond our capacity. Too often those of great intellect spend all their time so doing, achieve little, and become disillusioned when they could have achieved much. Although few of us now see the search for a deeper understanding of natural philosophy as a search for God, to which A.S. Eddington saw parallels in his work (Eddington 1929), many of us find those parts of science that have a bearing on philosophy to be among the most interesting (e.g., Penrose 2005).

Ideas are not readily described through the positions and motions of particles and fields but, as in philosophy, ideas and questioning are the gist of all science. I am not ashamed to ask questions that others claim are nonsensical and have gathered some at the end of this review. Knowing how often I have not seen the obvious implications of my own work, I regard all mankind, myself included, as somewhat shortsighted; thus, I regard the unexplained natural numbers such as $e^2/\hbar c$ as having their values for good physical reasons that have not yet been discovered by mankind. Anthropic reasoning I consider to be the refuge for those who "think more highly of themselves than they ought to think." That sort of escapism is not Science; think harder instead!

2. EDUCATION AND BACKGROUND

At school (Marlborough College), A.R.D. Ramsay was an outstanding teacher of mathematics. He showed us we had a gift. It was our calling to study the great mathematicians and learn to emulate them. Most importantly he showed us that we—even we—could invent the next steps for ourselves. We could develop mathematics! "Tubby" Middleton insisted that the way to turn physical problems into well-posed mathematical ones was to draw figures: a figure for forces, a figure for accelerations, and yet another for velocities. After his teaching of mechanics, I have seldom had difficulties in applying mathematics to physical problems. E.G.H. Kempson not only taught us good mathematics and good style but also took some of us rock climbing between terms. A lean wiry man, he had climbed on the 1935 and 1936 Everest expeditions and had very wide interests including music, art, and archaeology, as well as science. In my last term at school, he lent me his copy of Jeans's *Cosmogony and Stellar Dynamics* (Jeans 1919). From this I learned Poincaré's method of linear series, now the basis of Catastrophe theory. In 1968, I adapted this to thermodynamics (Lynden-Bell & Wood 1968) to discuss Antonov's gravothermal catastrophe, of which more later.

The Rev. John Rait, my great grandfather, became Rector of Alnwick in Northumberland but he was a childhood friend of the future Lady (John) Herschel. The two families remained in contact and it was doubtless his friendship with Sir John Herschel that led to the purchase of a fine Troughton & Sims $3\frac{1}{2}$-inch brass telescope. My father being the grandchild most interested in science, the telescope passed to him; so from a very young age, I marvelled at the mountains of the Moon, the Galilean satellites of Jupiter, and the crescent of Venus. Most mysterious were the galaxies, only faint clouds of light in the telescope, but we knew what wonderful objects they are from the time exposures taken with the great reflectors of Mt. Wilson and Palomar Observatories. Astronomy was one of many hobbies, but it was the one most tinged with wonder. On going up to Clare College, Cambridge, in 1953, I knew I would join both CUMC (the Cambridge University Mountaineering Club) and CUAS (the Cambridge University Astronomical Society).

I am much indebted to three of my teachers from my undergraduate years. Richard Eden realized I wanted to learn and allowed me to take the fast course in the Maths Tripos. In my second year (Maths Tripos, part II), Abdus Salam supervised me and realized that I had a deep interest in physics. He told me that I could not learn physics from mathematical theorists but that I must change courses after part II (Mathematics) to read part II of the Natural Sciences Tripos and learn physics from real physicists who did experiments. This I did and so got a far wider education than could be attained by following either of the standard courses. Jo Vinen taught me that year. He was then doing his experiment that demonstrated quantized vortices in liquid helium II (Vinen 1961), which was to earn him the Rumford medal of the Royal Society. It was he who took seriously my undergraduate attempt at a theory of Mach's Principle.

In my fourth year, I took the Mathematical Tripos part III, got a Distinction, and so earned the right to a research studentship. During those first four years I heard fine lectures from many famous men of science. Those courses that come to mind are general relativity from H. Bondi in 1954 and later from F. Hoyle, atomic physics from Otto Frisch, two fine quantum mechanics courses from P. Dirac, radio astronomy from Martin Ryle, and varied quantum mechanics courses from J. Ziman, N. Mott, and Denys Wilkinson. Donald Blackwell gave a very well-crafted unscheduled course on solar physics that introduced me to the magnetic wonders of the solar surface. I learned much statistical mechanics from Frank Powell's good course, and cosmical electrodynamics was expounded enthusiastically by L. Mestel, who became my research supervisor.

3. MACH'S PRINCIPLE, INERTIA, AND SPACE-TIME

As an undergraduate, I read Bondi's book *Cosmology* (Bondi 1952) and in it his chapter on the origin of inertia. On learning special relativity, I realized that the existence of magnetism could be deduced from Coulomb's inverse square law between a fixed and a moving charge. On Lorentz transformation, both charges move and the force four-vector transforms to give a magnetic as well as an electrical interaction in the new axes. Applying the same argument to Newton's inverse square law of gravitation, I deduced that there must be a gravomagnetism between currents of matter. I looked in vain for its manifestation in what was then (1956) known about the Solar System but found that such v^2/c^2 effects were too small to have been measured. This disappointment led me to the radical step of asking what the gravomagnetic effects would be if I took rotating axes with the Earth fixed. In those axes, the rotating universe gives a vast matter current; its gravomagnetic field is parallel to the spin axis and proportional to the Earth's spin rate $\mathbf{\Omega}$. Rough calculations showed that the force on a mass m moving with velocity $\mathbf{v}$ would be

$$F \sim 2m\mathbf{v} \times \mathbf{\Omega}\left(\frac{2GM}{Rc^2}\right),$$

where GM/R is the gravitational potential of the Universe at the position of the Earth. This force has the right sign and the right behavior with m, $\mathbf{\Omega}$, and $\mathbf{v}$ to be what we observe as the "fictitious" Coriolis force! Furthermore, even its magnitude would be correct if the Universe were closed so that $2GM/(Rc^2) = 1$. Can we deduce the gravitational potential of the Universe from the magnitude of the Coriolis force? Could it be that the fictitious forces were real and we merely find it convenient to use the special axes that make such long distance influences of the distant universe cancel out? Is it merely convention that leads us to dignify such frames as inertial frames? As yet I had no similar explanation of centrifugal force. I submitted an essay on this to Jo Vinen, who was then my supervisor, and he referred me to a recent publication by Dennis Sciama (Sciama 1953).

This early experience convinced me that there was some truth behind Mach's Principle that it is the distribution of mass-energy and its motion that determines the inertial frame at each point. However, the crudeness of my calculation of the gravomagnetic effect of a rotating universe led me to many years of study of how such calculations should be done in the context of general relativity.

The principle of equivalence can be regarded as saying that gravitational and inertial influences are equivalent, yet most of us think of gravitational force as involving two bodies but inertia as involving only the body itself. It was Mach who first questioned whether inertia too was not induced by the presence of the other masses in the Universe. He also questioned the reality of absolute space on the grounds that, according to Newton, it played an essential part in dynamics, but unlike other physical effects the dynamics of the bodies had no back reaction on the behavior of absolute space.

The absolute space that Mach was trying to eliminate goes back to Newton. When Newton first promulgated this concept, it met with strong opposition both from Bishop Berkeley and from G. Leibniz. Leibniz felt that Newton's invention was a useful one for keeping track of all the interactions between bodies, but like Cartesian coordinates, absolute space should not play any part in the dynamics of the relative motions of bodies. Whereas Newton elevated absolute space into the hand of God, Leibniz thought it did not exist and that only the motions of bodies relative to one another were real. Only in recent years has it been appreciated that Leibniz could have won this battle as far as classical dynamics is concerned. In a nonrotating universe like ours (Hawking 1969; Barrow, Juskiewicz & Sonada 1985), Newton's laws, applied in the frame relative to which the universe does not rotate, are exactly equivalent to a theory in which only relative distances and relative motions determine the dynamics (Barbour & Bertotti 1977, Lynden-Bell 1992b, Barbour & Pfister 1995, Lynden-Bell & Katz 1995). This relative dynamics is both truly Leibnizian and truly Machian in that there is no such thing as absolute rotation, or absolute acceleration, or any absolute motion at all! But science has moved on; we now need a theory of Mach's Principle that incorporates both special and general relativistic effects. Is general relativity such a theory? No! There are many exact solutions of Einstein's equations that do not obey Mach's Principle, the simplest being Minkowski space in which nongravitational dynamics is replete with inertia despite there being no gravitational-inertial interaction to cause it.

Einstein probably thought more deeply about Mach's Principle than anyone else. His statements show a gradual evolution but with no final conclusion. Initially he considered a theory in which space was flat but the velocity of light varied from point to point. From this early version of general relativity, he deduced that Newton's absolute space was wrong. He showed that inside an accelerated heavy spherical shell the inertial axes accelerate a little and that within a rotating shell the inertial axes rotate a little. Having seen that mass-energy and angular momentum modify inertial axes, he rejected the idea that inertial axes at infinity are absolute and wrote:

> The relativity of inertia is not fulfilled if the motions of bodies merely affect inertia. They must entirely cause it.

Einstein also wrote:

> There is no inertia of mass against space but only inertia of mass against mass.

It is no easy matter to eliminate inertia from space-time itself, and Einstein grappled long with the problem of the correct boundary conditions to be applied to the metric at infinity. His solution was miraculous, but amazingly it was not unprecedented because Schwarzschild (Schwarzschild 1900) had already conceived of the possibility that the Universe is a closed space. Einstein's solution was to eliminate infinity by closing the Universe. There were no boundary conditions because there was no spatial boundary. Here, another quotation from Einstein in 1953 near the end of his life is interesting:

> It took the genius of Newton to invent Absolute Space, so essential for the development of dynamics. It has taken even greater efforts to remove its influence from dynamics, a process that is even now still incomplete.

According to full general relativity, the inertial frame inside a slowly rotating but rapidly collapsing spherical shell rotates uniformly in space at a rate, Ω, that increases as time progresses until it coincides with the shell's rotation as the shell crosses its Schwarzschild radius (Lindblom & Brill 1974). There is no time delay in the rotation inward to the center. If we adopt axes that point in fixed directions at infinity, then inside the sphere, Coriolis and Euler forces are found in the right ratio. Centrifugal forces appear also but the solution is only true to first order in the rotation rate so centrifugal forces, which are of second order in Ω, are outside the solution's realm of validity. However, cylindrical systems can be solved exactly, and inside a uniformly rotating cylindrical shell (Embacher 1983), centrifugal forces appear in nonrotating axes and have the correct ratio to the gravomagnetic (Coriolis) effect even at high rotation rates. Thus, in general relativity, the inertial frames behave in the way that Mach's Principle predicts.

Einstein initially expected that the inertial frames were determined by the distribution of the stress-energy tensor $T_{\mu\nu}$. My first attempt at a relativistic theory of Mach's Principle (Lynden-Bell 1967a) assumed this, as did related works by Altschuler (1967); Sciama, Weylen & Gilman (1969); and Raine (1975). However, it was common knowledge among relativists that gravitational waves (which have $T_{\mu\nu} = 0$) carry both energy and angular momentum, and their energy too must have inertia. In fact, Gowdy (1975) has produced a closed universe solution of Einstein's equations, which is closed by the gravity of the gravitational waves in it whose stress tensor is zero everywhere. Jiri Bicak has great expertise in gravitational waves. This enabled us (Lynden-Bell, Bicak & Katz 2008) to demonstrate unequivocally that, just as in Einstein's thought experiment where a heavy rotating shell rotates the flat Minkowski space inside, so too a pulse of rotating gravitational waves outside a cylindrical (or spherical) region rotates the flat space inside. The internal space is and remains Minkowski space for all time but orientations in that space rotate relative to those defined in the asymptotic Minkowski space at infinity beyond the wave pulse. Thus, the effect of the pulse of rotating waves is to change the connection between the internal and external Minkowski spaces. No gravitational or inertial influences are felt either in the internal or in the asymptotic external spaces—of course, near but outside the wave pulse there is a gravitational field due to the mass-energy in the pulse—only in the region currently occupied by the wave pulse itself would a gravitational wave detector feel the wave pulse. Thus gravitational waves, which have no $T_{\mu\nu}$, certainly affect inertial frames and they along with rotating matter must be accounted for in the determination of inertial axes. What then is the current position? I think Einstein's beloved closed-space solutions of his equations with spherical topology are the only Machian ones and that others should be rejected as unphysical. Evidence that the Universe is nearly flat is expected if a closed universe has been greatly inflated because its curvature would be much reduced. As a thought experiment, consider first closed Friedmann-Robertson-Walker universes without a cosmological repulsion (Lynden-Bell, Katz & Bicak 1995). These expand to a maximum radius,

$$R = \frac{2GM}{c^2},$$

where M is the mass-energy of half the Universe, before they recollapse to a big crunch. M is not the sum of the rest masses in half the Universe but is reduced by the mutual gravitational binding of those masses. For the whole closed universe, this reduction reduces the total mass energy to zero. The mean density at maximal extension is proportional to M/R^3, which, with the dependency of R on M, is proportional to M^{-2}. Thus, the densities are least when M is greatest. If we ask which of these spaces is most like Minkowski's flat space, the answer is not one with very small M but one with very large M near maximum extension. If we compare such closed spaces, we find that those with smaller M have larger densities and smaller size. Also, the time for which the Universe exists between the Big Bang and Big Crunch is smaller. In the limit when M tends to zero, the Big Bang and Big Crunch are coincident; the world has infinite density and lives for no time at all. Thus, space and time disappear when the mass-energy in it disappears. This is all beautifully in tune with Mach's ideas, which required all inertia to vanish as $M \to 0$. We are thus led to the concept that space-time itself is generated by its energy content. Remove the energy and you are not left with Minkowski space but with no space-time.

There is a potential difficulty if we apply the above argument in the presence of a fixed cosmical repulsion term λ, because the removal of all energy would then leave a de Sitter space. However, this difficulty is circumvented if, in accord with some simple quasi-Newtonian concepts λ is not an absolute constant of nature but is related to the mass-energy M of the closed universe. Then if $\lambda \propto M$ (or indeed to any positive power of M), which is constant in any one universe, we recover the above behavior as $M \to 0$. Alternatively, if the λ term is thought of as part of the matter tensor and the mass-energy, then it will vanish as $M \to 0$. Although nothing is clear, I still have hopes that thoughts based on Mach's Principle may lead us to a definite prediction for the size of the cosmical repulsion. If we accept that perhaps λ might be proportional to M then it is not too unnatural to wonder whether Newton's constant of gravitation, G, might also be related to the mass of the Universe rather than be of the same nature as Planck's constant or the charge on the electron.

4. BREAD-AND-BUTTER SCIENCE

Most of my work is of this type, and I have found great joy in it especially because of the variety of problems, insights, and people encountered in different fields.

Grinding, polishing, and Foucault-testing my $6\frac{1}{2}$-inch telescope mirror while an undergraduate taught me that persistence pays, but my lack of patience makes me poorly suited to practical work. Leon Mestel taught me magnetohydrodynamics (MHD) very well but the problems were too intractable for a novice, so, stimulated by R. Woolley, I set about redeveloping stellar dynamics (Lynden-Bell 1962a,b) without the ellipsoidal hypothesis, which had dominated the work of Schwarzschild (1907), Eddington (1915), and Chandrasekhar (1942). At Les Houches, M. Kruskal showed us the power of MHD energy principles, which I adapted to (axially symmetric) gravitational fluid dynamics in my thesis, and Eliot Montroll's wonderful lectures on statistical mechanics confirmed my love of this subject. Among other students at Les Houches were Bob and Vera Rubin and Keith Moffatt, in whose car I traveled. They remained good friends of mine thereafter.

4.1. Orbits and Constants of the Motion

In my thesis (Lynden-Bell 1960), I invented new methods for finding those gravitational potentials in which orbits have extra algebraic constants of the motion, but these resulted in only minor extension of the categories known to Eddington and later Carter (1968a). Michel Henon and George Contopoulos were also working in this area of Stellar Dynamics and became good friends.

Recently, I returned to the even simpler problem of orbits in central potentials. With Shoko Jin (Lynden-Bell & Jin 2008), I found analytic orbits in all power-law potentials, and I have now extended this to all central potentials. My thesis also contained the theory of time-dependent accretion disks and first attempts at a theory of the spiral structure of galaxies.

4.2. Galactic Archaeology and Family Life

After being examined for the PhD. by Hoyle and Woolley, I took my Harkness Fellowship to Caltech and Mt. Wilson & Palomar Observatories. My college, Clare, had elected me to a research fellowship but the Harkness gave me the funding to visit the great centers of astronomy in the United States. In Pasadena, Wal Sargent, John Hazlehurst, and Roger Griffin were all postdocs too. Nancy and Bev Oke and Eleanor and Leonard Searle were very hospitable. I learned much from conversations with Maarten Schmidt and Hal Zirin and also knew and admired Bob Leighton, Fritz Zwicky, Horace Babcock, John Bolton and Ira Bowen. Jesse Greenstein was a good head of department.

Thanks to Woolley's insistence that I should offer my services to Allan Sandage, I was at the right place at the right time to play a part in ELS (Eggen, Lynden-Bell & Sandage 1962), which is one of the first observationally based papers on galaxy formation. Eggen and Sandage had devised the observational program. Eggen made many of the observations but he was so often observing that Sandage and I produced most of the arguments leading to our interpretation that the Galaxy formed in collapse. In retrospect, I think the main contribution of that paper was the invention of the tools that made galactic archaeology possible. We realized that the correlations of the kinematics of the stars with their chemical abundances opened this door, and our use of adiabatic invariants for the kinematics allowed us to describe an evolving system. We took the simplest initial model of a collapsing homogeneous spheroid, not because we thought that was the closest to reality but because it was one we could calculate (Lynden-Bell 1964). Much more has been read into this than it deserves. Later I explored the fragmentation and collapse of inhomogeneous spheres but the inclusion of rotation is then more difficult. Following our work on galaxy formation, I found that radiative cooling of an ionized cloud would indeed lead to its collapse (Lynden-Bell 1967c).

There are too few G dwarf stars of low metal abundance to agree with the simple model. Schmidt (Schmidt 1959, 1963b) interested me in solving this G dwarf problem, and later I wrote papers (Lynden-Bell 1975, 1992a) showing that a more gradual accretion of the Galaxy's gas mass removed the problem and gave separable generalizations of the simple model (Searle & Sargent 1972). Larson's steady model (Larson 1972) was a useful guide. I also sparked Pagel's interest in this subject, which became one of his major research topics as described in his good book (Pagel 1997).

Ruth and I got married in 1961, and she came to Caltech with me after the IAU meeting in Berkeley. She completed her PhD. in Chemistry under the stimulating supervision of Harden McConnell, who later moved to Stanford University. We left before ELS was finished and I learned, to my chagrin, that my eminent collaborators could not proofread mathematics. After enjoying the deserts of the South West, camping much of the time, we arrived to a wonderfully green Wisconsin in the spring, where I spent a happy six weeks visiting S. Chandrasekhar at the Yerkes Observatory in Williams Bay, Wisconsin. It was there I learned his virial tensor technique, which I used to demonstrate the large-scale instability of shape that leads to triaxiality in collapsing rotating bodies (Lynden-Bell 1964). Lin, Mestel & Shu (1965) later treated the nonlinear growth of these instabilities in the much simpler case of a nonrotating body.

On returning to Cambridge I became an assistant Lecturer in the Department of Applied Mathematics and Theoretical Physics (DAMTP) and Director of Studies in Mathematics at Clare.

It was not very long before I discovered that three half-jobs—lecturing, researching and teaching for one's college—add up to $1\frac{1}{2}$ rather than 1. I had two students of my own, Brent Wilson from New Zealand and Russell Cannon, and I took over the supervision of Ken Freeman when Leon Mestel was on sabbatical. First Peter Goldreich and then Jerry Ostriker came to Cambridge as NSF fellows, and I had productive collaborations with each of them. During Jerry's time, I took a sabbatical term in Leiden to see something of Oort, but I returned before Ruth gave birth to our daughter Marion. To get more time for astronomical research, I had decided to leave Cambridge and work under Woolley at the Royal Greenwich Observatory (RGO). Ruth was appointed to a half-time lecturership in Chemistry at the new University of Sussex. Ken Freeman was elected to a research fellowship at Trinity, but Russell Cannon, Brent Wilson, and Michael Penston, from whom I took over as advisor from Roger Tayler, came with me to the RGO.

4.3. Spiral Structure in Galaxies

In the years 1961–1972, my main drive was on this topic. In collaboration with Peter Goldreich (Goldreich & Lynden-Bell 1965), we discovered the amplifier, later christened the swing-amplifier by Alar Toomre who became a good friend. Then Agris Kalnajs and I showed how trailing spirals transfer angular momentum outward (Lynden-Bell & Kalnajs 1972). Bars occur when elongated 2-lobe, 1-turn orbits trap each other at one rotating azimuth. This can only happen when such orbital lobes move toward one another when so attracted rather than behave like donkeys, which do the opposite of what the rider intends (Lynden-Bell 1979).

4.4. Violent Relaxation

My open-ended paper on how a stellar system achieves a steady state (Lynden-Bell 1967b), which has rightly provoked much discussion, dates from my period at the RGO (1965–72) when much of my best work was done at Herstmonceux Castle. Recent work in plasma physics (Antoniazzi et al. 2007) lends support to my definition of collisionless entropy.

5. MARVELS OF GRAVITATION

Occasionally, bread-and-butter science leads to something with deeper implications. There are hints of this in Section 7.3 also. Probably most progress on deeper questions arises this way rather than via direct frontal attack.

5.1. Negative Heat Capacity

Given a piece of a previously unknown substance, one way to determine its constitution is to start by heating it. Thus, Sir Humphry Davy discovered strontium from the mineral strontianite that had been found in a lead mine near Strontian in Scotland. However, even Sir Humphrey would have been amazed had his strontianite become colder when he heated it. Can that ever happen? Even our language cries out against it!

In the mid-1960s, V.A. Antonov sent me his now classic Russian paper (Antonov 1962), which I found almost unbelievable. He took N atoms of mass m in a spherical container and maximized their entropy $S = -k\int f \ln f d\tau$—subject to the constraints of a fixed number of atoms $N = \int f d\tau$—and fixed energy $E = \int f m v^2/2 d\tau - Gm^2/2 \int f f'/|\mathbf{r} - \mathbf{r}'| \mathbf{d}\tau \mathbf{d}\tau'$, where f is the distribution function of the particles, $d\tau$ is the element of phase-space volume and the last term is the expression for the gravitational potential energy, V. As many of us already knew, he found that the equilibrium f was a Maxwellian distribution within the self-consistent gravitational potential $\psi(\mathbf{r}) = Gm \int f'/|\mathbf{r} - \mathbf{r}'| d\tau'$. To get this far, it is only necessary to demand that S must be stationary. Antonov next insisted that it must be a maximum. This led him to the extraordinary result that the entropy was indeed a (local) maximum provided that the ratio of the density at the center to that at the container was less than 708. If the density ratio exceeded that value, the stationary point was a minimax and there was no maximum to the entropy. To elucidate the origin of Antonov's extraordinary result. I studied the thermodynamics of a self-gravitating gas in a spherical container with the help of Roger Wood (Lynden-Bell & Wood 1968). At a density contrast of 389, well below Antonov's critical value, the heat capacity at constant volume becomes infinite and, for slightly greater contrasts, it reappears at large negative values. Thereafter, it increases as the contrast increases, finally crossing through zero again at Antonov's critical value. Thus, there is a range of density contrasts (389–708) at which maximum entropy systems exhibit negative heat capacity; they do indeed get colder when you heat them! A much older result for isolated systems follows from the Virial Theorem for equilibrium under gravity $2\mathrm{T} + V = 0$, where T is the kinetic energy, which for a monatomic perfect gas is related to temperature T by $\mathrm{T} = 3/2(NkT)$. Because the total energy $E = \mathrm{T} + V$, we find $E = -\mathrm{T} = -3/2(NkT)$, so the heat capacity $C = dE/dT = -3/2(Nk)$. However, isolated systems without containers cannot be in true thermal equilibrium under gravity because particles keep escaping, so Antonov's gas spheres in containers were the first true equilibrium systems displaying negative heat capacity. W. Thirring (1970) later explained that the standard thermodynamic proof that heat capacities are positive only holds for systems that can be in equilibrium in canonical ensembles. Microcanonical systems can have negative heat capacities. Despite Thirring's work, negative heat capacities remained a very controversial subject for many years. Remarkably, first-order phase transitions such as boiling, when viewed in the right way, can be seen as being due to microscopic elements (Lynden-Bell & Lynden-Bell 1977) with negative heat capacity. This is even the case for the phase transition of the Van der Waals gas (Lynden-Bell 1999)!

5.2. Quasars, Giant Black Holes, and Accretion Disks in Galactic Nuclei

Giant black holes were first conceived of in the fertile mind of the Rev. John Michell F.R.S. (Michell 1784), Rector of Thornhill in Yorkshire. In earlier years, he had been a fellow of Queens' College, Cambridge, and the third Woodwardian Professor of Geology. It was he who invented the apparatus that, after his death, was finally used successfully by Cavendish (Cavendish 1798) to measure G and, hence, the mass of the Earth. Michell, like his younger friend Henry Cavendish, was a staunch supporter of Newton's ideas, so for him light consisted of a train of corpuscles shot out of its source like bullets. Michell realized that those corpuscles that came from very heavy stars would be retarded by their gravity. Thus, the light from such stars would move less rapidly and be refracted differently by a prism. This would also be true if the star was moving away from the observer. Because aberration is proportional to the ratio of the Earth's velocity around the Sun to the velocity of the light, this too should be larger for such stars. Michell impressed on N. Maskelyne, the Astronomer Royal, and on William Herschel the importance of measuring such effects, but no effects were found at the low accuracy then achieved. Michell, however, persisted with the idea, suggesting that it might be used to measure stellar motions and the speed at which the Sun moved through the nearby stars. Finally he realized that a star not less dense than the Sun but over 500 times its diameter (i.e., one of $1.25 \times 10^8\ M_\odot$) would have so great a gravity that light itself could not escape, thus rendering the star invisible. Nevertheless such stars would still be detectable from measurements of satellites in orbit about them. Michell's ideas were transmitted to Paris not only via his paper in *Philosophical Transactions of the Royal Society* in 1784 (Michell 1784), but also as highlighted in correspondence between Sir Joseph Banks (President of the Royal Society) and the U.S. ambassador in Paris, Benjamin Franklin FRS. It was no doubt through this that Laplace heard of the idea, but he did not remember to whom it should be attributed when (after Michell's death) he wrote the first edition of his *Système du Monde*. By the time the second edition was published (Laplace 1809), Young had shown that light was a wave; Michell's argument was no longer watertight, so Laplace deleted all allusion to these massive dark stars. It took 211 years, from Michell's prediction, before the first giant black hole was unequivocally observed by Miyoshi et al. (Miyoshi et al. 1995) in the nucleus of NGC 4258. The giant black hole was found from measurements on gas in orbit about it, which gave a mass of $4.2 \times 10^7\ M_\odot$ in fair agreement with Michell's prediction. With his friend's prediction not verified in his day, Cavendish made the first calculation of the bending of light by a star in case that could be observed more readily. None of this bore fruit in the nineteenth century, although William Herschel did note the very bright, almost stellar nuclei of NGC 1068 and NGC 4151. Commenting on these works of Michell in her article on him in the *Dictionary of National Biography for 1885*, Agnes Clarke wrote, "He spent his later years fruitlessly speculating on the influence of gravity upon light."

The next advance came through the remarkable observations of Vesto Slipher at the Lowell Observatory in Flagstaff, Arizona. Not only did he measure some 25 radial velocities of galaxies with time exposures lasting three nights or more on each object, he also discovered that the spirals rotate. When he took the spectrum of NGC 1068, he found the emission lines to be so broad that he seriously doubted any Doppler explanation and wondered whether some pressure broadening could be involved. Slipher (Slipher 1917) should be recognized as the discoverer of active galactic nuclei.

After the World War II, some of those who had developed RADAR used their expertise to found radio astronomy. Important centers grew in England and Australia. Martin Ryle's group in Cambridge made successive catalogs of radio sources and demonstrated via source counts that the Universe must be evolving, so the steady-state theory was false (Ryle 1968). Soon after their discovery, many of the radio sources were found to be double and G.R. Burbidge (1959) produced his minimum energy theorem, which gave the least energy needed to make the observed synchrotron emission. These energies were very large, commonly greater than the binding energy of the whole Galaxy. As a student in Cambridge, I knew the importance and excitement of these new data but, working in a department of applied mathematics, I had never learnt that galaxies had nuclei. Once I got to Caltech, all that changed and I took short exposures of ellipticals to inspect their centers. Guido Münch even got me into the prime focus cage of the 200-in Palomar telescope to see the nucleus of M81. At Caltech, I saw a good deal of the radio astronomers who were getting Owen's Valley Radio Observatory going. In spring 1961, I remember meeting Rad (V. Radhakrishnan) in the library of Robinson building and resuming a discussion of the origin of the huge energies of the radio galaxies. Finally, I remarked that Eddington had pointed out that if Betelgeuse were of the same radius but made of water, then it would be so massive that space would close around it; however the conversation broke off as I searched for the reference (Eddington 1926). It was totally unclear how the distant pairs of radio lobes could be related to this but we were groping for something that might supply the energy. In August 1961, I attended the IAU meeting in Berkeley where Ambartsumian (1962) emphasized the importance of activity in the nuclei of galaxies. Work inspired by him made the Byurakhan Observatory of the Armenian Soviet Socialist Republic one of the great centers of research into active galactic nuclei, producing, e.g., the Markarian and Arakelian catalogs.

Back in Pasadena, I offered to work for Sandage for a couple of months. He asked me to use the microphotometer to assess the variability of candidates picked out by R.H. Norton in the globular cluster NGC 6712. This is less metal poor than other clusters that contain RR Lyrae stars. Indirectly this work led to my involvement in ELS. The Burbidges visited and I saw the plate of the radio source 3C48. The identified star had a very faint wisp attached that could only be seen if the plate was held at a glancing angle. Sandage had taken the spectrum of the star but despite its emission lines, he could not interpret it; nor could Bowen, who suggested Greenstein should be brought in to help. However, even he, one of the finest spectroscopists, was stumped. In early 1962, I remember Sandage returning to the office after an observing run on Mt. Wilson and excitedly explaining that 3C48 had varied by 0.2^m over a period of a few months. It could not be a galaxy of many thousands of light years in size. Thus, despite the difficulties with the spectrum, it must be small. This strongly favored a stellar rather than an extragalactic explanation.

We left Pasadena on April 1, 1962 to drive through the wonders of the South Western deserts and on to visit Chandrasekhar for six weeks at Yerkes Observatory in Williams Bay.

It was a whole year later that the riddle of the quasars was solved by Maarten Schmidt's discovery (Schmidt 1963a) of the redshift of 3C273. Soon after that, Greenstein & Schmidt (1964) showed that the redshifts could not be gravitational.

I was not at the first Texas conference on relativistic astrophysics, which occurred shortly after the shooting of President Kennedy. There, all the early ideas on what powered the quasars were discussed. Such great power coming from such small objects made most theorists favor some highly relativistic object or objects but the total energies meant that they must contain at least a million times more mass than the Chandrasekhar limit of 1.4 $M_\odot$. First searches for objects around the quasars indicated that they were not in clusters of galaxies, and this led some to believe that quasars were unassociated with galaxies and might perhaps be places where the Big Bang had somehow been delayed and was only now shining forth as white holes. Salpeter (1974) ignored the unsubstantiated data and, in a short note to the *Astrophysics Journal*, suggested that a heavy collapsed object moving through a galaxy would accrete interstellar gas and form an accretion disc. He derived the energy output per gram of material passing through the disc and down into the collapsed object as $0.058c^2$. This same efficiency was derived by Zel'Dovich & Novikov (1964) from a model in which a satellite orbiting such an object radiated gravitational waves as it spiralled down.

In 1965, Sandage (1965) discovered that radio-quiet quasi-stellar objects were far more common than the radio-loud quasars, while Schmidt (1968) demonstrated by his V/V_m statistic that quasars were nearly a thousand times rarer today than they were (in comoving coordinates) at a redshift of two. About this time, I went to a small conference on relativity at which Roger Penrose was to speak on "How to get energy out of a black hole." Wondering what he would talk about, I envisioned a swirling vortex heated by the friction of differential rotation. I buttonholed Roger and asked whether that was what he would talk about. He said no and encouraged me to publish the idea, but I thought it too elementary and ill-formulated. Roger talked on his beautiful Penrose process for extracting energy from rotating black holes.

It was now four years since I had left Cambridge for the RGO and from our home in Barcombe I drove to Herstmonceux with Bernard Pagel. Part of our route lay along the A273 road, and this daily reminded me that there was still no accepted theory of the quasar 3C273. At Herstmonceux, Cannon and Penston were monitoring the brightest quasars using the 26-inch refractor. The rapid variations of some of them showed their sizes must be very small. By 1969, I knew that the great era of the quasi-stellar objects (QSOs) was over. There was also evidence that QSOs have active periods and are only rarely very bright. Putting all this together, I made rough estimates of how common dead QSOs would be and concluded that the nearest one to a typical point in the Universe would be only 3 Mpc away. The overdensity of the Local Group then suggested that there might be dead QSOs in many massive galaxies, ours included. These dead ones would still be massive, 10^7–10^9 $M_\odot$, and so being over the Chandrasekhar limit, they should be black holes.

Such massive centers of gravitation would have many stars gathered around them so I predicted (Lynden-Bell 1969) that they were the nuclei of galaxies and that these would have high mass-to-light ratios, particularly near their centers. To give a picture of their formation process, I drew on my as yet unpublished work on accretion disks from my thesis. As molecular viscosity was insufficient, I invented a magnetic angular momentum transport. In the excitement of discovery, I failed to look back on my older work, so the disks in my paper lack the redistribution of energy via the torques and this has annoyed me ever since. That autumn (1969), we again visited Pasadena, where Sandage was so enthusiastic about my theory that he got me an invitation to the Vatican conference on the nuclei of galaxies. In the aftermath of that conference, I had breakfast with Ambartsumian who told me about the Armenian work on young clusters with flare stars. These figured strongly when, with Jim Pringle who had independently worked on accretion disks, I finally wrote up in 1974 the work on time-dependent disks (Lynden-Bell & Pringle 1974) that I had done in 1960.

Whereas relativists welcomed the greater relevance of their work that such theories brought, most astronomers were too conservative to believe that such exotic objects inhabited nuclei of apparently normal galaxies. Whereas Ekers and I (Ekers & Lynden-Bell 1971), Sargent (Sargent et al. 1978), and Penston (Penston et al. 1981) actively sought massive black holes in them, higher spatial resolution was needed to make a definitive case. This only came with the beautiful VLBI work of Miyoshi et al. (Miyoshi et al. 1995), although earlier work by Kormendy (1988) using the good seeing of Hawaii was later vindicated. On the theoretical side, a beautiful paper by Bardeen (1970) on how accretion turns Schwarzschild black holes into extreme Kerr ones is too seldom quoted. With Martin Rees, I reviewed what was then known about our Galactic Center (Lynden-Bell & Rees 1971).

At Maarten Schmidt's behest, I turned my attention to improving the statistical methods employed and in so doing solved a new important problem in statistics (Lynden-Bell 1971) that was then taken up by others (Woodroofe 1985). Those who read my paper, "Galactic Nuclei as Collapsed Old Quasars" (Lynden-Bell 1969), will realize that I had a young family at the time. I used to bathe my daughter, Marion, and when the water ran out it produced a great gurgle as the vortex opened and closed. Marion took so great an interest in this that her first word was "tex" (short for vortex). I have been greatly blessed in life to have Ruth, who understands and appreciates mathematics, as my wife. We both enjoy the crossword-puzzle aspects of solving differential equations, so much so that Edward, our son, had to lay down a new rule of good manners—no mathematics over dinner. After forty years, when I felt my paper had been forgotten in favor of later ones, it was a great joy to have many of my family present when Maarten Schmidt and I shared the first Kavli prize in Astrophysics for our work on quasars.

6. RETURN TO CAMBRIDGE AS PROFESSOR OF ASTROPHYSICS

When we left Cambridge in 1965 there had been much talk of a new Institute of Theoretical Astronomy that Hoyle had pushed for. Indeed, I understand that Cambridge University was offered such an institute but turned it down on the grounds that it would split off the astronomers from the Mathematical Tripos where their teaching was needed. In the Science Research Council it had already been agreed that, if Cambridge refused, the institute would go to the new University of Sussex, which was some 25 miles from the RGO at Herstmonceux Castle. When Cambridge University refused, Hoyle tendered his resignation as Plumian Professor. However, Sir John Cockroft, Master of the newly-created Churchill College, believed Hoyle to be very important for the future of Astronomy in the United Kingdom. Sir John persuaded Lords Wolfson and Nuffield to provide most of the funds for a building, a top-class computer, and 12 posts for five years. Thus, Fred Hoyle got his Institute of Theoretical Astronomy (IoTA) mainly from private donations; but though the University then accepted it, Fred was left feeling he was tolerated, not welcomed. Fred attracted many of the finest astronomers of the next generation. Rees, Stephen Hawking, Brandon Carter, Douglas Gough, Peter Strittmatter, Cyril Hazard, Jayant Narlikar, Peter Eggleton, John Faulkner, Sverre Aarseth, Paul Davies, and Chandra Wickramasinghe were all staff members for at least some of IoTA's first five years (1968–72). However, the funding situation worsened just as the initial private money was about to run out. The University had foreseen this possibility and had produced a report on the future of Cambridge astronomy. Pending this report, all university posts falling vacant at the Cambridge Observatories were frozen, so Professor R. Redman's last years as Professor of Astrophysics were especially difficult ones. As Hoyle wished, the report advocated that the IoTA be merged with the Observatories so that Cambridge could play its part in using the new Anglo-Australian Telescope (AAT) that was being built. However, although two previously vacated posts were unfrozen, there was no expansion in the number of posts available. Faced with this uncertain future, members of Fred's institute sought jobs elsewhere. We elected Rees to succeed Professor W. McCrea in Sussex, Faulkner went to Lick at Santa Cruz, Strittmatter to La Jolla and then on to Arizona, Narlikar to Bombay, Paul Davies to University College London, etc.

Sir Richard Woolley retired as Director of the RGO and as Astronomer Royal in 1971. By this time I had decided that, after seven years there, I should leave the RGO where I was the only theorist and go somewhere that I could learn more physics, in particular more general relativity. Two astronomical professorships were vacant in Cambridge, one in the Cavendish (which went to Alan Cook) and Professor Redman's Chair of Astrophysics. I consulted Ryle on which position I should apply for and, on his advice, chose the Astrophysics chair, whose former occupants were H. Newall, F. Stratton, and Redman. Unbeknownst to me, Hoyle hoped to persuade my friend Wal Sargent to take this chair, which would certainly have strengthened work with the AAT. However Wal, who had earlier been offered posts that he had not applied for, saw no need to apply, and when Hoyle could not get agreement among other electors, I understand he put forward his friend and collaborator Geoff Burbidge. The other electors did not agree to this either and, after considerable discussion, I was elected against Hoyle's wishes. This reinforced his earlier belief that the rest of the University was ganging up against him. I was perfectly willing to work under Hoyle's directorship and had no wish to have any extra administration. Hoyle had many commitments worldwide and was much involved in deciding how the AAT should be run.

The chemist Dr. Alan Sharpe was the very efficient chairman of Cambridge University's Faculty Board of Physics and Chemistry. It was the board's job to name the director of the new combined institution, so it fell to Dr. Sharpe to consult the existing staff of both the observatories and IoTA as to whom they would like as director. Some felt Hoyle was too often away from Cambridge, so Sharpe wrote to J. Hosie, the chief astronomical official at the Science Research Council, to ask whether their future funding of the combined institute was dependent on Hoyle being its director. Hosie correctly replied that this was a university matter and that further funding would depend on the strength of the case made for it. However, accompanying Hoyle on a flight for an eventual AAT board meeting in Australia, Hosie mentioned Dr. Sharpe's consultation. Hoyle, who already thought the University was ganging up against him, saw in this a sinister threat to deny him directorship of his own institute. Soon afterward, he again tendered his resignation. For reasons that remain obscure, he did not receive a reply from the Vice Chancellor. It could be that Fred was traveling so much that the letter never caught up with him, but the death of the Vice Chancellor's wife at this time might also have been a factor. Two months later, when news finally filtered back to Cambridge that Hoyle had received no reply, another was sent, but by this time Hoyle had decided that Cambridge did not want him and had set about making a new life for himself outside Cambridge. So the letter did not change his mind; however, he deferred his resignation for a year to have time to rearrange his life. Thus, it came about that I was immediately plunged into directing a newly merged department that had twelve members whose jobs would expire at the end of the year. It was the youngest and brightest who had no permanent jobs, and even the name of the newly merged body was controversial. The old Observatory members liked Observatory or Department of Astronomy (as a compromise), whereas the IoTA staff preferred to continue as an institute. Added to such matters, the Faculty of Mathematics had lost the Plumian chair when Hoyle founded the IoTA, and they now pushed to have it returned to them. My reaction was to secure as many permanent jobs as I could. DAMTP had a significant turnover of staff and its head, Professor G.K. Batchelor, agreed to give Carter a lectureship and to take over responsibility for finding funds for Hawking. Although I was reluctant to lose mathematical relativity—a subject in which personally I had great interest—this did ensure its continuation in Cambridge. The problem of the Plumian chair was amicably settled. The University agreed that Professor R. Lyttleton's personal chair, which would have reverted to a lectureship in the institute upon his retirement, would instead be transferred back to DAMTP and made a permanent one, and in return two lecturerships would be shared between that department and the Institute of Astronomy (IoA), as it came to be called. Gough filled the first joint lecturership, while Eggleton was appointed to a permanent post to continue his studies in stellar evolution at the IoA. I was delighted to again elect Professor Rees to a chair, this time to bring him back from Sussex to succeed Hoyle as the new very young Plumian Professor.

Although I never enjoyed administration, I did see it as my duty to listen patiently to those who wished to change things; sometimes it was easy to help, but more often the money was not available, for example, for more computing power. I felt it important that a head of department should be on top of his subject, so to this end I resolved never to be head for more than five years at a stretch. Martin Rees succeeded me and we took five year turns for the 23 years from 1972. He is a great entrepreneur and the IoA's rapid growth was largely his doing. I preferred the smaller more selective model that Hoyle had built.

7. THE STIMULATING DIVERSITY OF ASTRONOMY

7.1. Energy Principles for Inviscid Fluid Mechanics

It took much thought to devise a way of saying that two inviscid fluid flows had the same circulation structure, so that one had the possibility of evolving into the other inviscidly. Joseph Katz and I used our solution to get an energy principle for all steady flows (Lynden-Bell & Katz 1981) and to prove a basic theorem: "Among all flows that are iso-circulational to a given flow those of stationary energy are steady flows and vice versa." Joseph then derived the basic invariants from the symmetries of the Lagrangian (Katz & Lynden-Bell 1985), and I later reviewed the field in a posthumous tribute to Chandrasekhar (Lynden-Bell 1996).

7.2. Streams in the Galaxy's Halo

The long drawn out discovery of the Magellanic Stream led me to speculate in 1976 that other dwarf satellites of the Galaxy might have streams. I wrongly betted on my particular associations and lost to Sidney van den Bergh even though the basic idea was right. It is great fun trying to find possible associations, and in our ghostly streams paper (Lynden-Bell & Lynden-Bell 1995), Ruth and I correctly inferred that the globular cluster Pal 2 was a member of the Sagittarius stream despite being on the other side of the sky. Unsurprisingly, many of our other speculative suggestions proved wrong. Earlier Doug Lin had worked with me on computer simulations of the Magellanic Stream, and in 1982 we predicted the Large Magellanic Cloud's proper motion to be 2 milliarcseconds per year due East (Lin & Lynden-Bell 1982). The latest Hubble Space Telescope proper motion agrees excellently but those who determined it never read our paper! I remain impressed that both the Ursa Minor and Draco satellites of the Galaxy lie in the Magellanic stream's plane and have their major axes elongated along the line of the stream in the sky. My recent student Shoko Jin found interesting associations of streams with elongated high-velocity cloud complexes. Perhaps we should think of such things as the last surviving remnants of galaxy interactions, which were so beautifully explained by Toomre & Toomre (1972).

7.3. Relativistically Rotating Disks and Conductors

An infinite Mestel disk rotating under its own gravity is fully specified by its constant circular velocity, V. Nothing with the dimensions of a length can be made from V and the two constants G and c, which are intrinsic to general relativity. Thus, no characteristic length can occur in the metric, $g_{\mu\nu}$, of an infinite Mestel disk. From this nice idea, it follows that all the $g_{\mu\nu}$ must behave as powers of r. This allows one to solve the radial equation; and to find the stationary solution, one has to solve only an ordinary differential equation in θ. No analytical solutions being known, Serge Pineault and I produced numerical ones (Lynden-Bell & Pineault 1978). For fast rotations, an ergoregion develops around the disk in the form of a double-sided ergocone. More disks followed (Pichon & Lynden-Bell 1996). In 1968, Brandon Carter (Carter 1968b) showed that the gyromagnetic ratios of all charged Kerr metrics and of Dirac's electron were the same. Gravity is too weak to affect an electron so I put Newton's constant G equal to zero in the metric and found the remaining electromagnetic field to be $\mathbf{E} + i\mathbf{B} = -\nabla\Psi$, where $\Psi = -e/\sqrt{(\mathbf{r} - i\mathbf{a})^2}$ and $\mathbf{a} = (0, 0, a)$. That is the field of a charge at an imaginary point. I remain fascinated by this beautifully simple field (Lynden-Bell 2004). It can also be derived as the field of a relativistically rotating charged conducting disk as the tip speed tends to c. The same field crops up as a component in the field of a charged conducting sphere rotating at relativistic speeds and as an electromagnetic field in which a charged particle's orbit has extra constants of motion. I consider the electron to have this intrinsic field; if it is a point charge at all, it is at an imaginary point along its spin axis, $a = \hbar/2mc$ in this case.

7.4. Large-Scale Streaming of Galaxies and the Cosmic Microwave Background Dipole

For about seven years, I much enjoyed the Samurai collaboration (Dressler et al. 1987, Faber et al. 1989), which led to our finding a large-scale streaming of galaxies over and above their Hubble flow. This streaming persisted to galaxies whose redshifts were 4000 km s^{-1}. Alan Dressler coined the name the great attractor for the aggregation causing it, and Ofer Lahav and I took an active interest in its association with the cause of our motion (Lynden-Bell et al. 1988) through the cosmic microwave background that gives rise to its apparent dipole (Lynden-Bell & Lahav 1988). In a good collaboration, one learns to admire the great abilities of others in areas of one's own incompetence! Sandy Faber chaired us with admirable diplomacy.

7.5. Io, Magnetic Towers, Jets, and Bursts

Since 1994, I have made some analytical progress in MHD, the subject that nearly sent me to the "slough of despond" as a novice. Once before, my background was invaluable when working with Goldreich on how Io's orbital position controls the decametric radiation from Jupiter. Our theory

(Goldreich & Lynden-Bell 1969) led to one of the few clear predictions that proved correct. When the hot spot where Io's flux tube meets Jupiter was finally found, it pointed 15° ahead of the orbital position of Io just as we had predicted. The self-similar force-free field published with C. Boily in 1994 (Lynden-Bell & Boily 1994) for Mestel's festschift led me to an understanding of magnetic towers that grow in height with each turn of their magnetic footpoints. This led on to a general theory of force-free MHD jets (Lynden-Bell 2006). Recently, Konstaninos Gourgouliatos and I have found an analytic solution for a relativistic force-free explosion of magnetic energy confined by a cone. Currently, I see all jets from stars, quasars, or γ-ray bursts as products of swirling disks winding magnetic fields (Gourgouliatos & Lynden-Bell 2008).

7.6. Exact Optics

To ameliorate expenses on the dome and the tube, large-aperture telescopes must be short, so the optics must be fast, maybe *f*/1. The angles that the light makes with the optical axis will not be small. In exact optics (Lynden-Bell 2002), we redevelop analytical optics keeping the exact trigonometric functions of such angles rather than small angle expansions. I have much enjoyed collaborating with my old friend Roderick Willstrop, who taught me what was done previously and used his expertise in ray tracing to explore the accuracy of the off-axis images (Willstrop & Lynden-Bell 2003) that analytical methods achieve. So far, we have the complete analytical theory giving the shapes of the mirrors, etc., that have images with neither spherical aberration nor coma for all two-mirror systems (two free parameters) including a spectrograph for which all Seidel sums are zero (Willstrop 2004). More recently we have extended this to all three-mirror systems, but while those with parabolic primaries are analytically solved the others leave us with a first-order nonlinear Riccati differential equation to solve. That was also true for Schmidts.

8. QUESTIONS

In one of the early conferences on cosmic gas dynamics, Professor W.H. McCrea asked, "How does a clump of gas know it is too heavy to form a star if it has not yet tried?" I take this as an example of a good question framed in an amusingly anthropomorphic way and copy the style in some questions below.

1. The spectra of homonuclear diatomic molecules such as the Swan bands of C_2 never cease to be a cause of wonder to me. Their rotational spectra have half the lines "missing" in C_{12}–C_{12}, have all lines fully represented in C_{12}–C_{13}, and have alternating intensities in C_{13}–C_{13}. How does the molecule know that its nuclei are identical so that parity can forbid C_{12}–C_{12} from having odd rotational angular momentum states? What matters is not whether we can distinguish the nuclei but whether they are indistinguishable "in principle."
2. In the infinite open or flat universes, the region over which the density exceeds 10^{40} g cm^{-3}, say, during the Big Bang, has infinite volume, and we can find elements of it as far apart as we like. Is this a non-Machian reason for rejecting them? Only in the closed universes are such densities confined to a finite volume that shrinks to zero as $\rho \to \infty$.
3. Thanks to A. Abragam, we know that spin systems can exist in a thermodynamic equilibrium at negative temperatures that are hotter than $T \to +\infty$. We also know of systems that get cooler when you heat them; that is, they get a larger value of $1/T$ on being heated. Can one devise a system that does that and simultaneously has a negative temperature?
4. Waves and countable entities (solitons) occur in classical nonlinear wave equations. Had these equations and their properties been known in 1925 would not atomic physicists have

associated the quanta with the solitons? How would quantum theory have developed and would the interpretation of its mathematical symbols be the same as that we teach today?

5. Newton believed that it should be possible to verify theoretical concepts by finding their direct counterparts in nature. Thus he aspired to find THE theory of the solar system, not merely a theory that fitted the facts. How far do our theories fulfill Newton's ideal? Can we deduce the existence of Hilbert spaces and wave functions, etc., directly from observation?
6. My particle physics friends assure me that the electron is a point. I think a point electron should have infinite electromagnetic energy and should be very massive with no dipole moments. Surely what they really mean is that no length other than $\hbar/(m_e c)$ enters the quantum electrodynamics of, say, electron-positron scattering experiments. I am much intrigued by the field (cf. Section 7.3) $\mathbf{E} + i\mathbf{B} = -\nabla\Psi$, where $\Psi = -e/\sqrt{(\mathbf{r} - i\mathbf{a})^2}$, and $\mathbf{a} = \boldsymbol{\sigma}\hbar/(mc)$, where $\boldsymbol{\sigma}$ is the spin. This can be thought of as the field of a charged conducting disk with a tip speed of c.
7. On the matter side of Einstein's equations, we find not only gravity's constant G times the density but also G times the pressure (in the spatial components). In spherical symmetry, the gravity due to pressure acts very strangely. Inside a neutron star it bends the orbits of particles that have no interaction with the neutrons, but outside the gravity is solely due to the density. The gravity of the pressure vanishes anywhere that the pressure vanishes, even when the pressure inside the neutron star is a significant fraction of density times c^2. Because such pressure terms cause both inflation and the acceleration of the Universe, should not we astronomers understand how this works? T. Padmanabhan points out that away from spherical symmetry one can see gravitational effects due to such terms. The gravity of two opposed light beams is twice as strong as that due to the mass-energy of the beams.
8. Is the cosmical constant Λ a fundamental constant or one related to the total mass-energy that our Universe contains as discussed in Section 2? Could Newton's G have the same nature?

DISCLOSURE STATEMENT

The author is not aware of any affiliations, memberships, funding, or financial holdings that might be perceived as affecting the objectivity of this review.

LITERATURE CITED

Altschuler B. 1967. *Sov. Phys. JETP* 24:766
Ambartsumian VA. 1962. *Trans. IAU XI* 13:145–60
Antoniazzi A, Califano F, Fanelli D, Ruffo S. 2007. *Phys. Rev. Lett.* 98:0602
Antonov VA. 1962. *Vest. Leningrad Univ.* 7:135. Transl. IAU Symp. 113 p. 525
Barbour JB, Bertotti B. 1977. *Nuovo Cim. B* 38:1–27
Barbour JB, Pfister H. 1995. *Mach's Principle: From Newton's Bucket to Quantum Gravity*. Berne: Birkhauser
Bardeen JM. 1970. *Nature* 226:64
Barrow JD, Juskiewicz R, Sonada DH. 1985. *MNRAS* 213:917–43
Bondi H. 1952. *Cosmology*. Cambridge, UK: Cambridge Univ. Press
Burbidge GR. 1959. *Ap. J.* 129:849
Carter B. 1968a. *Phys. Lett. A* 26:399–400
Carter B. 1968b. *Phys. Rev.* 174:1559–71
Cavendish H. 1798. *Philos. Trans. R. Soc.* 88:469–526
Chandrasekhar S. 1942. *Principles of Stellar Dynamics*. New York: Dover
Dressler A, Lynden-Bell D, Burstein D, Davies RL, Faber SM, et al. 1987. *Ap. J.* 313:42–58

Eddington AS. 1915. *MNRAS* 76:37
Eddington AS. 1926. *The Internal Constitution of Stars*. Cambridge, UK: Cambridge Univ. Press
Eddington AS. 1929. *Science and the Unseen World*. London: George Allen & Unwin
Eggen OJ, Lynden-Bell D, Sandage A. 1962. *Ap. J.* 136:748
Ekers RD, Lynden-Bell D. 1971. *Astrophys. Lett.* 9:189
Embacher F. 1983. *J. Math. Phys.* 24:1182–86
Faber SM, Wegner G, Burstein D, Davies RL, Dressler A, et al. 1989. *Ap. J. S.* 69:763–808
Goldreich P, Lynden-Bell D. 1965. *MNRAS* 130:125
Goldreich P, Lynden-Bell D. 1969. *Ap. J.* 156:59–78
Gourgouliatos KN, Lynden-Bell D. 2008. *MNRAS* 391:268–82
Gowdy RH. 1975. *J. Math. Phys.* 16:224
Greenstein JL, Schmidt M. 1964. *Ap. J.* 140:1
Hawking SW. 1969. *MNRAS* 142:129
Jeans JH. 1919. *Problems of Cosmogony and Stellar Dynamics*. Cambridge, UK: Cambridge Univ. Press
Katz J, Lynden-Bell D. 1985. *Geophys. Astrophys. Fluid Dyn.* 33:1–33
Kormendy J. 1988. *Ap. J.* 335:40–56
Laplace PS. 1809. *System of the World*, Vol. 2, p. 367. Transl. J Pond. London: Phillips
Larson RB. 1972. *Nat. Phys. Sci.* 236:7
Lin CC, Mestel L, Shu FH. 1965. *Ap. J.* 142:1431
Lin DNC, Lynden-Bell D. 1982. *MNRAS* 198:707–21
Lindblom L, Brill DR. 1974. *Phys. Rev. D* 10:3151
Lynden-Bell D. 1960. *Stellar and galactic dynamics*. PhD. thesis. Univ. Cambridge, Cambridge
Lynden-Bell D. 1962a. *MNRAS* 123:447
Lynden-Bell D. 1962b. *MNRAS* 124:95
Lynden-Bell D. 1964. *Ap. J.* 139:1195
Lynden-Bell D. 1965. *Ap. J.* 142:1648
Lynden-Bell D. 1967a. *MNRAS* 135:413
Lynden-Bell D. 1967b. *MNRAS* 136:101
Lynden-Bell D. 1967c. *Radio Astronomy and the Galactic System*, *IAU Symp.* 31, ed. H van Woerden, pp. 257–64. London: Academic and New York: Part IIE
Lynden-Bell D. 1969. *Nature* 223:690
Lynden-Bell D. 1971. *MNRAS* 155:95
Lynden-Bell D. 1975. *Vistas Astron.* 19:299–316
Lynden-Bell D. 1979. *MNRAS* 187:101–107
Lynden-Bell D. 1992a. In *Elements and The Cosmos*, ed. MG Edmunds, RJ Terlevich, pp. 270–81. Cambridge, UK: Cambridge Univ. Press
Lynden-Bell D. 1992b. *ASP Conf. Ser.* 30:1
Lynden-Bell D. 1996. *Curr. Sci.* 70:789
Lynden-Bell D. 1999. *Phys. A* 263:293–304
Lynden-Bell D. 2002. *MNRAS* 334:787–96
Lynden-Bell D. 2004. *Phys. Rev. D* 70:105017
Lynden-Bell D. 2006. *MNRAS* 369:1167–88
Lynden-Bell D, Bicak J, Katz J. 2008. *Class. Quantum Gravity* 25:5018
Lynden-Bell D, Boily C. 1994. *MNRAS* 267:146
Lynden-Bell D, Faber SM, Burstein D, Davies RL, Dressler A, et al. 1988. *Ap. J.* 326:19–49
Lynden-Bell D, Jin S. 2008. *MNRAS* 386:245–60
Lynden-Bell D, Kalnajs AJ. 1972. *MNRAS* 157:1
Lynden-Bell D, Katz J. 1981. *Proc. R. Soc. Lond. Ser. A* 378:179–205
Lynden-Bell D, Katz J. 1995. *Phys. Rev. D* 52:7322–24
Lynden-Bell D, Katz J, Bicak J. 1995. *MNRAS* 272:150–60. Erratum. 1995. *MNRAS* 277:1600
Lynden-Bell D, Lahav O. 1988. In *Large-Scale Motions in the Universe*, ed. UC Rubin, GV Coyne, pp. 199–217. Princeton: Princeton Univ. Press
Lynden-Bell D, Lynden-Bell RM. 1977. *MNRAS* 181:405–19

Lynden-Bell D, Lynden-Bell RM. 1995. *MNRAS* 275:429–42
Lynden-Bell D, Pineault S. 1978. *MNRAS* 185:695–712
Lynden-Bell D, Pringle JE. 1974. *MNRAS* 168:603–37
Lynden-Bell D, Rees MJ. 1971. *MNRAS* 152:461
Lynden-Bell D, Wood R. 1968. *MNRAS* 138:495
Michell J. 1784. *Philos. Trans. R. Soc.* 74:35–37
Miyoshi M, Moran J, Herrnstein J, Greenhill L, Nakai N, et al. 1995. *Nature* 373:127
Newton I. 1730. *Opticks: or a Treatise of the Reflections, Refractions, Inflections and Colours*, pp. 394–403. New York: Dover. 4th ed.
Pagel BEJ. 1997. *Nucleosynthesis and the Chemical Evolution of Galaxies*. Cambridge, UK: Cambridge Univ. Press
Penrose R. 2005. *The Road to Reality: A Complete Guide to the Laws of the Universe*. London: Jonathan Cape
Penston MV, Boksenberg A, Bromage GE, Clavel J, Elvius A, et al. 1981. *MNRAS* 196:857–87
Pichon C, Lynden-Bell D. 1996. *MNRAS* 280:1007–26
Raine D. 1975. *MNRAS* 171:507–28
Ryle M. 1968. *High. Ast.* 1:33
Salpeter EE. 1974. *Ap. J.* 140:796–800
Sandage A. 1965. *Ap. J.* 141:1560
Sargent WLW, Young PJ, Lynds CR, Boksenberg A, Shortridge K, Hartwick FDA. 1978. *Ap. J.* 221:731–44
Schmidt M. 1959. *Ap. J.* 129:243
Schmidt M. 1963a. *Nature* 197:1040
Schmidt M. 1963b. *Ap. J.* 137:758
Schmidt M. 1968. *Ap. J.* 151:393
Schwarzschild K. 1900. *Vierteljahrsschr. Astron. Ges.* 35:337–47
Schwarzschild K. 1907. *Göttingen Nachr.*, pp. 614–32
Sciama DW. 1953. *MNRAS* 113:34
Sciama DW, Waylen PC, Gilman RC. 1969. *Phys. Rev.* 187:1762–66
Searle L, Sargent WLW. 1972. *Ap. J.* 173:25
Slipher VM. 1917. *Lowell Obs. Bull.* 1:59–62
Thirring W. 1970. *Z. Phys.* 235:339
Toomre A, Toomre J. 1972. *Ap. J.* 178:623–66
Vinen WF. 1961. *Proc. R. Soc. Lond. Ser. A* 260:218–36
Willstrop RV. 2004. *MNRAS* 348:1009–18
Willstrop RV, Lynden-Bell D. 2003. *MNRAS* 342:33–49
Woodroofe M. 1985. *Ann. Stat.* 13:163
Zel'Dovich YaB, Novikov ID. 1964. *Sov. Phys. Dokl.* 9:246