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MAKING A SCIENCE OF OBSERVATIONAL COSMOLOGY: THE CAUTIOUS OPTIMISM OF BEATRICE TINSLEY

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Cosmology may plausibly claim to be both the oldest and the youngest of the sciences. For example, Norriss Hetherington's 1993 *Encyclopedia of cosmology* contains entries on Cave Dweller Cosmology and Megalithic Cosmology beside articles on Grand Unified Theories and Quantum Cosmology. Most interestingly, it is not the archaeological articles but the contemporary ones that raise the question of whether the cosmology they address is "respectable" science.[1] For contemporary cosmology, the problem stems from the difficulty in obtaining data: cosmology is a science of the extremes of space and time, so hard data that would establish fundamental parameters have until very recently remained tantalizingly beyond reach. As Helge Kragh has argued in his history of the steady state *v.* big bang controversy, factors other than observed data (philosophical preconceptions, ideas of theoretical elegance, scientific style, religious belief, personality, nationality, friendship and allegiances) played the dominant role at least until the mid-1960s.[2]

This paper discusses the life and work of Beatrice Tinsley (1941–81). Tinsley began her short but spectacular career optimistic that the study of galaxies would bring cosmological measurements within astronomers' grasp, and in so doing, make cosmology an observational science. Galaxies, being bright, were already recognized as key benchmarks astronomers could use to measure distance and motion, but it was also recognized that extending such techniques to extreme distances would require an understanding of galaxy evolution.[3] The problem, of course, is that light travels at a finite speed, so that as one looks out to great distances in space, one looks back in time, seeing galaxies as they were in the past. If young galaxies differ systematically from older ones, then measurements that assume their similarity are bound to err. Tinsley created computer models, or simulations, of the change in colour and brightness (or photometric evolution) of galaxies as the stars within them age. Hers were the first evolutionary models sufficiently detailed and realistic that they were used in cosmological debate. They were also robust; today astronomers still use models that, though more sophisticated in input and analysis, are clearly descended from Tinsley's. Ironically, though Tinsley's galaxy studies were successful, they suggested that correcting galaxy measurements for evolution would make it harder, not easier, to discriminate between competing cosmological models. Thus as her career progressed, Tinsley increasingly pursued galaxy evolution for its own sake, and came to regard other kinds of observation more useful for establishing cosmological parameters.

Some of the most interesting aspects of this story concern Tinsley's motivation

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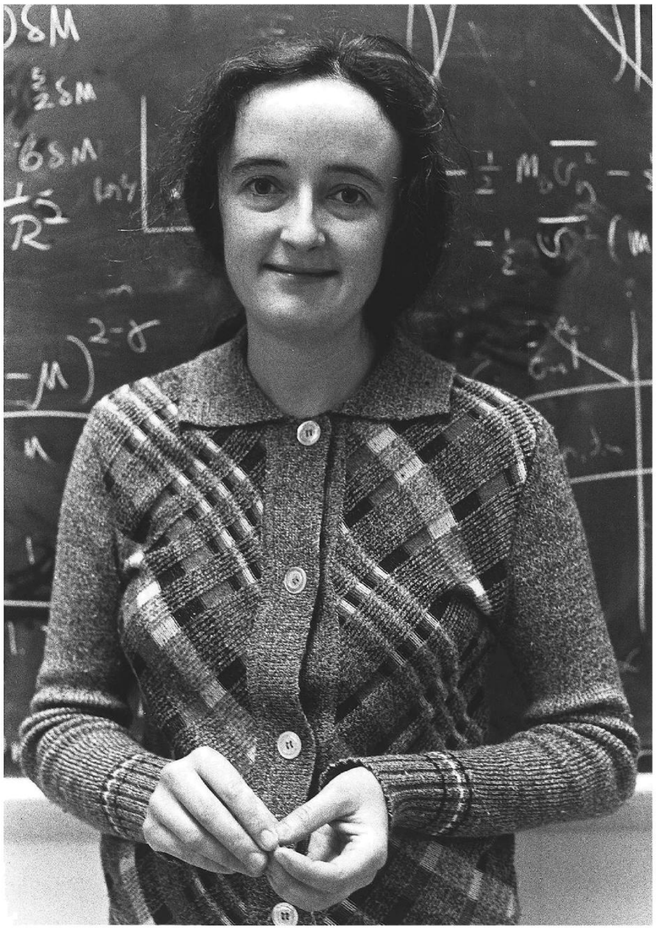

FIG. 1. Beatrice Tinsley, shortly after her move to Yale (courtesy Sterling Library, Yale University).

and timing. In the 1960s, galaxy studies were in their infancy. Considerable observational work had been done to categorize galaxies' shapes (spirals, barred spirals, ellipticals), and there were theories of dynamical evolution that sought to explain how those shapes were produced by the motion of stars within galaxies. However, the number of researchers at work on galaxies was small; their work was not well-coordinated, and there was little consensus on many of the facts most basic to their subject, for example, the stellar content of galaxies. We have, therefore, a very interesting situation. Cosmology was still a barely observational field, and galaxy studies was barely a field at all. Yet, within a few years, galaxies came to be seen as one of the key kinds of astronomical observation (and there were only a few) to be used in making cosmology into observational science. Two nascent activities seem to have contributed materially to each other's birth. Tinsley's own project was risky, even doubtful, because too little was understood about the necessary components of such a theory: the galaxies' initial stellar population, and the evolutionary history of the stars. Why was she so optimistic that cosmology was ripe for observational test? Certainly there were many still comfortable asserting that cosmology was fundamentally a philosophical exercise, not an observational one. Why did she focus on galaxy evolution? Why didn't she concentrate on relativity theory, or on one of the more widely-accepted empirical approaches to cosmology, for instance, radio source counts, or the nucleosynthesis of heavy elements? To explore the answer to these questions, we will focus on two key periods in Tinsley's career: its early stage, when she made her first choice of research topic, and its midpoint, when she re-entered research after a period of child rearing, began to work with new collaborators, and redirected her research.

Beatrice Tinsley was born in England in 1941 and raised in New Zealand, where her father, Edward Hill, was at first an Anglican vicar and later became mayor of the city of New Plymouth. She arrived in the United States in 1963, having just completed a master's thesis in solid state theory at Canterbury University, where she had established a reputation as an exceptionally able student.[4] She came to the U.S. because her husband, Brian Tinsley, who had just finished his own Ph.D. in atmospheric physics, had found a job at the Southwest Center for Advanced Studies (SCAS), a research institute that would soon become the University of Texas at Dallas. Their departure from New Zealand was a typical move: to avoid insularity, the small island nation encouraged promising young scientists to go abroad at least for a few years, and many found permanent jobs more readily overseas. Dallas was, perhaps, an atypical destination, but Brian was attracted by the idea that his research there (unlike so much of atmospheric physics) would not be military-sponsored.[5] Beatrice knew there was a relativity group at SCAS, and she planned to use grant money that she brought from New Zealand to study cosmology with them, hoping to combine graduate study with motherhood. At first, the Tinsleys' situation looked extremely promising: just after they arrived, Dallas hosted the first Texas Symposium in Relativistic Astrophysics, a star-studded international meeting devoted to the very puzzling question of quasars.

Quasi-Stellar Radio Sources, as they were then called, were one of astronomy's hottest topics in 1963. In the decades since the Second World War, when military surplus radio equipment had first been put to astronomical use, astronomers had been able to match up some of the radio sources they detected in the sky with objects they observed in visible light. Some were evidently external galaxies in which structure could be seen. Others, because they looked point-like and their radio intensity scintillated or twinkled, were thought of as radio stars.[6] One such source was called 3C 48, so named because it was the 48th source in the third catalogue of objects identified by radio astronomers from Cambridge. It had been shown by radio astronomers from Jodrell Bank to have a very small diameter, and when Alan Sandage observed it using the Palomar 200-inch telescope, he saw not a galaxy, but a small, blue point-like object. It looked stellar; it varied in brightness, and Sandage and Matthews announced that it was stellar, but peculiar. As Sandage later said, it had the "weirdest spectrum I'd ever seen". The emission lines did not match those of any known substance.[7] Another point-like radio source, 3C 273, was observed by Maarten Schmidt in late 1962, and it had an equally puzzling spectrum. Some months later, when Schmidt was writing up his observations, the answer to the puzzle suddenly struck him: noticing a familiar pattern of decreasing spacing between six of the lines in the spectrum, he realized that he was staring at the familiar Balmer series of the hydrogen spectrum — but they were redshifted by an incredible 0.16. "I was stunned by this development", Schmidt wrote. "Stars of magnitude 13 are not supposed to show such large redshifts!" Minutes later he had found Jesse Greenstein, his Caltech colleague who had been puzzling in just the same way over the spectrum of 3C 48. Schmidt told Greenstein of his finding, and looking at the spectrum with the idea of a large redshift in mind, they quickly identified magnesium, oxygen and neon in the spectrum, this time redshifted by a even more surprising 0.37.[8]

Identifying lines of such high a redshift only deepened the puzzle. If the redshift was cosmological, 3C 48 lay more than three billion light years away and was the second most distant object ever measured. To be visible so far away, these objects would have to be a hundred times brighter than a giant galaxy. Yet they could not be large. 3C 273 was bright enough that it appeared on thousands of photographic plates made in the preceding decades, and examining these, Harlan Smith and Dorrit Hoffleit of Yale found that several times it had flashed and dimmed so abruptly that it could not be much more than a few light months across, a million times smaller than our galaxy's diameter, and 10^{18} times smaller in volume. That so much light could be produced in so tiny a volume seemed incredible.

Only one energy source could be up to the task of supplying the vast energy required: gravitation. Even as Schmidt and Greenstein were poring over spectra, and before Smith and Hoffleit had shown how small the energy-producing region must be, Fred Hoyle and William Fowler had suggested that strong radio sources could powered by what would literally be superstars: 10^5 to 10^8 solar masses worth of gas, accumulated at the centres of galaxies. That such a compact and massive object — we would now call it a black hole — could actually exist was a conjecture. Hoyle and Fowler's proposal was (as Kip Thorne, another relativity theorist has put it)

> a radical departure from tradition. This was the first time in history that astronomers and astrophysicists had felt a need to appeal to effects of general relativity to explain an object that was being observed. Previously, relativists had lived in one world and astronomers and astrophysicists in another, hardly communicating. Their insularity was about to end.[9]

Hoyle and Fowler's theory appeared in the 16 March 1963 issue of *Nature*, as did Schmidt's and Greenstein's papers on the redshifts of 3C 48 and 3C 273. It was obvious these objects were so perplexing, and the kinds of expertise needed to evaluate them were so diverse, that it was time for the researchers to confer.

As it happened, the relativists in Texas were itching to hold a conference. Ivor Robinson of Dallas, and Alfred Schild and Engelbert Schuking of Austin, were recent arrivals in Texas. The relativity group at the Southwest Center for Advanced Studies, which Robinson had been imported to build, was still very small, so he was intellectually lonesome, and all three were eager for scientific excitement.[10] They had the encouragement of Larry Marshal, a physicist who headed the SCAS's Office of Scientific Personnel, and who recognized that a conference might attract the attention of the kind of people the Center would eventually like to recruit. The SCAS, UT Austin, and a number of local and national institutions provided funding,[11] and the little summer conference first envisaged soon mushroomed into an international extravaganza. "Having become Texanized", Schuking reports, "we all agreed: Why not do it in a big way?"

Making quasi-stellar sources the subject of the Texas symposium was Schucking's idea, and was an odd one in view of the fact that none of the organizers was an astronomer:

> This was going to be principally an astronomical conference, and as far as we could see there wasn't a single astronomer in Dallas. But we fixed that. The suspicion existed that quasars might have something to do with relativity and thus might fit into an imaginary discipline combining astronomy with relativity. One of us — Alfred, Ivor, or I? — invented a catch phrase for this new field of science: relativistic astrophysics. We could use our reputations as relativists to promote ourselves. The subject is now well established. There are textbooks about it.[12]

The Tinsleys found the meeting exhilarating. It drew an audience of more than three hundred scientists from nearly twenty countries, including, as Beatrice wrote to her parents, "Just about every famous living man in the field from all over the world". Both Beatrice and Brian were new to major international conferences and unused to seeing their scientific heroes close up. From her letters and his memories, one has the feeling they were as impressed by the audience as they were by the discussion.[13]

In several dozen papers read and discussed over three days, astronomers sorted through the observational evidence, generally agreeing that quasars were neither local stars nor galaxies. Theorists debated whether the gravitational collapse suggested by Hoyle and Fowler was even possible. Since a body must spin faster as its radius decreased, wouldn't it break up into numerous smaller parts? If it did collapse, would it actually generate the immense energy required? As Beatrice understood, the meeting was inconclusive. "The pooling of ideas must have been of tremendous value, to researchers, but I don't think they have any more certainty as to what the strange objects are than they did.... I wish I knew enough to appreciate it fully." Numerous physicists more senior than the Tinsleys must have felt the same.

A scientific meeting, however, need not solve its problem to be success, and this meeting was rewarding because it brought together relativity theorists and astrophysicists, two groups whose interests had previously seemed divergent. This aspect of the meeting was whimsically captured in the after-dinner speech of the British theorist Thomas Gold:

> Relativists with their sophisticated work were not merely cultural ornaments but might actually be useful to science! Everyone is pleased, the relativists who feel they are being appreciated, who are suddenly experts in a field they hardly knew existed; the astrophysicists for having enlarged their domain, their empire, by the annexation of another subject — general relativity. It is all very pleasing, so let us all hope that it is right. What a shame it would be if we had to go and dismiss all the relativists again.[14]

At the meeting, little was made of the single topic that would probably have interested Beatrice most: the possibility of using quasars as cosmological probes. None the less, the connection between astronomy and relativity theory that Gold spoke of was still close to what Beatrice was looking for: applications of general relativity to actual astrophysical problems. This would hold out hope for that most elusive goal, real science bridging cosmological speculation and astrophysical data.

After her exhilarating introduction to Dallas, however, disenchantment soon set in. Ivor Robinson remembers that Beatrice had come to see him, aflush with the possibility of doing general relativity with astrophysical and cosmological applications, and when he told her what they were really working on her face fell.[15] During her first year in Dallas, she did a great deal of independent reading in relativity and cosmology and participated in relativity group discussions and seminars, but still she grew increasingly dissatisfied. The problem was three-fold: intellectual, because the Dallas group treated relativity as a “mathematical game”; professional, because the Dallas community regarded her as Brian Tinsley’s clever wife, rather than as a scientist in her own right; and personal, because Beatrice and Brian did not conceive a child as rapidly as they had hoped. Within a year, she wrote to her parents that she had reached a “dead end” and was getting depressed at her “scientific stagnation”. In 1964, she decided to enroll in the Astronomy Department at the University of Texas at Austin.

From Tinsley’s point of view, studying at Austin was a very reasonable plan. To members of the Astronomy Department, including Harlan Smith, who had recently arrived and was department chairman, it seemed a “singularly unlikely venture”.[16] Good as her academic record looked, it came from a small foreign school with which Austin had no prior experience, making it difficult to judge her qualifications. Despite her determination that the way to make relativity more than a game was to link it to astronomy, she had no formal background in the later field. It was hard to believe that a serious professional astronomer was hidden within this housewife from the Dallas suburbs. Moreover, instead of moving to Austin to go to graduate school, she intended to commute the 400 miles back and forth from Dallas every week. Several Austin faculty who were members of the department at the time now say that had Tinsley's admission been voted on by the Faculty Committee, it is unlikely she would have been admitted to graduate school. In 1964, however, the department chairman could make unilateral decisions to admit graduate students. When he met Tinsley, Harlan Smith was sufficiently impressed — especially by the fact that she had never in her life gotten a grade lower than A — to give her a chance.[17] Sailing through preliminary exams with flying colours, she was soon researching a dissertation on the evolution of galaxies under the guidance of Gerard de Vaucouleurs.

We now approach the interesting questions: Why would an aspiring cosmologist switch from relativity theory to galaxies? Why pick out galaxy evolution as a way of bringing cosmological theory to an observational test? Before we answer this, however, it worth briefly examining her commitment to cosmology. First, Tinsley's interest in cosmology was longstanding, stemming (I think) from issues central to her upbringing. Tinsley came from a religious family active in an evangelical movement first called the Group Movement and eventually known as Moral Rearmament. This group emphasized dedication to the Great Things in life, and while Beatrice rejected her parents' religion, she retained their impatience with things that could be considered little matters. Beatrice was a precocious student, and there were very public expectations of her intellectual greatness. These were also very deeply internalized expectations, and they steered her towards cosmology from a early age, because cosmology addresses the biggest questions of all. Cosmology, more than most branches of the physical sciences, could seem a fitting substitute for religion, for someone seeking such a substitute. That she early toyed with becoming a cosmologist is clear: on first seeing an expensive anthology entitled "Theories of the Universe" in the university book store, she called it "gorgeous", bought it with her godmother's gift money, and described it to her parents as "a collection of writings from Plato to Einstein and Bondi.... One could more or less add to it oneself".[18] She wrote a master's thesis in a solid state topic only because research opportunities in the physics department at the University of Canterbury were very limited and cosmology was not an option. Her one-time advisor recognized that it was not her first choice, and one of her student contemporaries puts it more bluntly: she was bored to tears with it. Brian says that she could also be disdainful of his field, atmospheric physics, which was the other option.

What would an aspiring cosmologist in the 'fifties and early 'sixties have thought interesting? The big debate that surely would have grabbed her attention was the controversy over the steady state versus the big bang. There was enough popular writing about it that it would have been easily accessible to an interested secondary school student. It was on the air, as well as in the air: we know Tinsley was sufficiently devoted to the BBC to take the BBC's magazine, *The listener*, as a reminder of home when she moved to the U.S., and she avidly clipped and scrap-booked articles on science, religion and humanism. So it is likely that she listened, for example, to Bernard Lovell, director of the Jodrell Bank radio observatory, evaluating both sides in a BBC program of 1958, and she may also have heard a BBC symposium of 1959 pitting steady-state proponents, Bondi and Lyttleton, against critics, Whitrow and Bonnor.[19] Both sides of the debate were represented in the selections in the anthology by Munitz that she bought herself. (Munitz, himself a philosopher, joined the debate as a critic of the steady-state cosmology, especially the creation of matter). It is tempting to speculate that she leaned towards the steady state in reaction against the theological resonances of cosmology: in 1951, Pope Pius XII endorsed the Big Bang as consistent with the Biblical account of creation. But the situation is far too murky — for example, many adherents of the steady state argued that in postulating a universe that continues physically unchanged forever, rather than one destined for catastrophic destruction, their theory is more theologically acceptable — and the evidence is too scant to draw conclusions like that with any certainty.

More important than conjectures about which side of the debate Beatrice might have favoured even before beginning her graduate career, however, is that we notice the boundaries of the debate as she might have encountered and assimilated them. Regional styles may well play a role, and an attractive aspect of discussing Tinsley is that her move from New Zealand to America (specifically SCAS and the astronomy department in Austin) makes her a good test case for regional styles of cosmological argument. In his recent book, *Cosmology and controversy*, Helge Kragh distinguishes between British and European approaches and American approaches to cosmology.[20] The first tradition, having its roots in the work of Lemaître, Eddington, Einstein, Friedmann, De Sitter, and Milne, was the more philosophical and the more interested in space, time, motion, matter density, and the field equations that govern them. The American tradition, by contrast, was more empirical, taking advantage of work done at the great western observatories. First it centred on the distance and redshift (recession) of galaxies, and some especially important examples are the work of Slipher at Lowell, and Hubble and Humason at Mount Wilson. After the war, nuclear physicists brought a new focus to American cosmology: the synthesis of heavy elements. In Kragh's account, steady-state cosmology, with its concern for underlying philosophical principles and arguably ad-hoc creation of matter, emerges as a reasonable product of the British tradition, while big-bang cosmology, with — as Gamow put it — its 'factual' emphasis on element synthesis, epitomizes the American approach.[21] Tinsley's own developing cosmological interests can be mapped against Kragh's geography. Whatever she met of cosmology as an undergraduate, she met in New Zealand, where intellectual culture and higher education were still largely British-dominated. She then moved to Dallas, where the relativity group was composed mainly of European émigrés and was led by Ivor Robinson, described by Beatrice as "evidently English ... huge, gesticulating — with a plum in his mouth".[22] This group's dedication to theoretical aspects of relativity certainly fits Kragh's characterization of Anglo-European cosmology, though their choice of an astronomical topic for the First Texas

Symposium on Relativistic Astrophysics stretches the category. She first would have been immersed in American-style cosmology in Austin, where she worked with Gerard de Vaucouleurs and where she was, for the first time, associated with an institution that had its own significant observatory, the MacDonald Observatory.[23] Though she was always a theorist rather than an observer, she suddenly gained plenty of opportunities to interact with them. Beatrice's own interests shifted from the fundamental properties of space, time and matter, first to the practical problems of measuring cosmological parameters in general, then specifically to the evolution of galaxies. While some of this shift is attributable to the waning fortunes of steady-state cosmology, Tinsley's own conversion happened fast enough that the change in her immediate intellectual environment is surely responsible rather than the change in the wider astronomical community.

We know Tinsley's steps towards her dissertation work in considerable detail, because they were recorded in her research notebook, which she saved.[24] The notebook is striking because it shows how quickly Tinsley's thoughts developed; in the autumn of 1965 she was casting about widely among a wide range of topics; by the end of March 1966, she had decided on a topic and laid down the outline of her project in all its essential details. The plan was a mature one; in it Tinsley posed questions and laid plans rich enough that in addition to guiding her dissertation work, they remained fruitful parts of her research program fifteen years later.

As Tinsley's notebooks make clear, she was interested from the beginning in observational tests of cosmological models. From September 1965, through that winter, calculations designed to elicit observational parameters from various General Relativity cosmological models alternate in the notebook with discussion of actual observables (source counts, sizes, magnitudes, cluster diameters, and redshifts). As she toyed with one observable after another, she worried about evolutionary effects and selection effects, but she seems at first to have looked on them as cases to be ruled out, or corrections to be made if necessary, rather than phenomena to be studied in themselves. In March 1966, following a conversation with Harlan Smith, Tinsley focused her attention on the evolution of galaxies, and wrote down a virtually complete plan of her dissertation research. At that point her optimism was certainly high: on 11 March she described the evolution of elliptical and S0 galaxies as an "ideal method for [cosmological] model testing".

Tinsley's goal was to calculate the change in the colour and luminosity of galaxies as the population of stars within the galaxy ages. The most straightforward part of her work was writing a computer program that integrated the total light of a galaxy by adding up the light of each star within it, taking account both of the evolution of the initial stellar population, and of the birth of new stars from the enriched material recycled to the interstellar medium by previous generations of stars. This was a work of synthesis: Tinsley assembled her galaxy model using the work of numerous stellar theorists. The method sounds simple, and the idea was not original to Tinsley; Alan Sandage had outlined a similar but more rudimentary analysis in 1961.[25] Tinsley was, however, the first to make a detailed, numerical simulation. The simplicity of the method, however, is only apparent; several serious challenges faced anyone actually trying to implement it in (or even after) the early 1960s. The problem is that many of essential components of the model were just barely good enough for Tinsley's purposes. First, the initial mass function for the stellar population was poorly understood, so that it was difficult to determine what fraction of a galaxy's light to attribute to dim, long-lived, low-mass stars, and what fraction to the bright, short-lived, massive ones. Tinsley and Sandage would dispute this point for years. Compounding the mass problem was that stars' evolutionary histories were known in barely sufficient detail. Nor was it known how efficiently matter is recycled from dying stars to the interstellar medium, so it was difficult to guess how rapidly successive generations of stars become richer in metals. To forge ahead in the face of these obstacles required courage and optimism, and one of Tinsley's key contributions was surely her conviction that the time for making such model galaxies was indeed ripe.

Tinsley's optimism was warranted. She was able to assemble a series of models that evolved into galaxies with photometric properties matching those presently observed, and the change in their colour and magnitude over cosmological timescales was substantial. She then calculated the evolutionary correction appropriate to several competing cosmologies and found that it reduced the difference between the magnitude–redshift relations that they predicted, making it harder to choose between them. Though this was not the model-testing result Tinsley first sought, it opened an intriguing possibility. Popular cosmologies faced a considerable embarrassment in that the age they gave for the universe was smaller than the age of some of the stars it contained. Tinsley evidently hoped that by showing that galaxy observations did not rule out alternative cosmologies, she might help to rekindle interest in cosmologies permitting a sufficiently old universe.

Having successfully completed an ambitious dissertation, many a fledgling Ph.D. would have gone directly to a job or postdoctoral fellowship. Beatrice Tinsley had other plans. Just before she defended her work, she and Brian adopted Alan, an infant newly born to a member of Brian's family. Two years later, they adopted Teresa. For about five years, Beatrice was mostly occupied raising the children, though she found time to publish four articles, three based upon her dissertation.[26] When both children entered pre-school she tried to resume her career, but was unable to get a satisfactory job. At first, she looked only in Texas, because Brian was by then sufficiently established in Dallas that moving would have involved substantial sacrifice. Dallas offered part-time teaching and an institutional affiliation through which she could funnel research support (which she received from the National Science Foundation after 1970) but no full-time job. Though the institution was expanding rapidly, and administrators at first apparently welcomed Tinsley's proposal that an astronomy program be established and that she fill its first position, neither the program nor the job materialized. Austin, too, offered part-time appointments, and more important, contact with a community of astronomers. For a time, she resumed the weekly commute familiar from graduate school, and she toyed with the hope that she and Brian might someday both hold full-time Austin positions, but again she was disappointed. This professional rejection must have been particularly hard to take, especially as her reputation was growing rapidly outside Texas.

The years 1971 and 1972 saw Beatrice Tinsley's remarkable reemergence as an active researcher. In spite of her family responsibilities, teaching, and the Dallas–Austin commute, she found time to publish eleven papers, and her papers began to be cited by others.[27] Her early papers expand upon her dissertation's argument for evolutionary corrections; she explored their applications to magnitude–redshift and colour–redshift relations and to the cosmological deceleration parameter, q_0. She also worked to improve the data that could be used as input for her models. For example, she collaborated in 1971 with Hyron Spinrad of Berkeley on an evolutionary model that reproduced the light of the M31 disk population. The significance of a paper like this was that Spinrad and his colleagues had, between 1966 and 1971, developed a model for the current stellar population of the M31 disk that was based upon narrow-band photometry, so it was better understood than the population of giant ellipticals (which, because of their brightness, were cosmologically the most interesting galaxies). Since the wide-band photometry of the two was considered a reasonable match, it could be hoped that their populations really were identical. Much was at stake; as Tinsley reiterated in many of her papers, the proportion of the light contributed by massive versus low-mass stars (more precisely, "the slope of the luminosity function at the top of the present main sequence"[28]) was the main determinant of a stellar population's photometric evolution, and the fact that different authors adopted different giant-to-dwarf ratios led to sharp and continued disagreement on the need for an evolutionary correction.

From the point of view of Tinsley's future career, the most striking papers of these years may be a pair entitled "Galactic evolution: Program and initial results", and "Stellar evolution in elliptical galaxies".[29] In these papers, which together totalled nearly thirty pages, Tinsley proposed to make (and soon reported on) improved models that better integrated population synthesis with the chemical evolution of interstellar gas and more physical theories of stellar birthrate. She shortened the time increments in her simulations for periods when the gas content of a galaxy might change rapidly. In addition, she tried to match a wider range of stellar populations, called for the use of narrower-band photometry, and tried to model the evolution of individual spectroscopic lines. Obviously, this was not a short-term project; many of these proposals occupied her for the next decade. Viewed in the light of Tinsley's stop-and-start career, these papers constitute a public staking of Tinsley's claim to her past and future research program, perhaps necessitated in her eyes by the growing numbers of other researchers doing related work, and they may have served as a challenge to herself to do competitive work under adverse circumstances. If it was an unusual strategy, it was an effective one.

Tinsley's awareness that the evolution of galaxies remained a field rich in problems did not stop her from pressing her galaxy results as far as she could in the service of cosmology. In the papers we have discussed up to this point, she had developed evolutionary corrections to observable quantities, like colour and magnitude. In 1972, however, she calculated the difference her models of galaxy evolution should make to cosmology's central theoretical representation, the Hubble diagram. On the Hubble diagram, the recession of nearby galaxies is represented by a straight, sloping line, because the expansion of the universe makes recessional velocity be a linear function of distance. However, if gravitation slows the expansion of the universe, the straight line becomes a curve, and its curvature is measured by the deceleration parameter, q_0, detectable at very large distances. In "A first approximation to the effect of evolution on q_0",[30] Tinsley argued that ignoring evolution would make the universe seem to decelerate more than is actually the case. This has two consequences. If the deceleration is diminished, then the universe is more likely to expand forever, that is, to be open. Yet (as Tinsley had shown for magnitude–redshift relations in her dissertation) the effects of evolution made galaxies a less effective tool for discriminating between different cosmologies.

This train of thought led to Tinsley's most famous cosmological publication, and leads us back to the broader question of cosmological tests. Tinsley's publications, conference travel, and visiting appointments at Mount Wilson and Palomar Observatories in 1972 and at the University of Maryland in 1973 brought her into contact with new collaborators. In 1974 she, J. Richard Gott and James Gunn (both of Caltech) and David Schramm (recently arrived in Austin from Caltech) published "An unbound universe?"[31] The paper surveyed a wide range of arguments: estimates of distance and expansion from observations of galaxies and quasars, estimates of age from meteoritic abundances, estimates of present density from individual galaxies, galaxy groups and clusters, and intercluster gas, estimates of big-bang era density from deuterium. Though no individual argument could be described as certain, and even collectively they had loopholes, still they pointed to an open universe: the density is too low (by about a factor of 10) for it ever to collapse upon itself. The result was in keeping with Tinsley's own results for galaxies, but galaxies were far from the strongest of the supporting arguments. "It is remarkable", the authors wrote, "that the best constraints come from very local data (ages of elements in meteorites, interstellar deuterium, dynamics of nearby aggregates of galaxies), not from observations of galaxies at large".[32] As Schramm later laughingly reminisced, "We were sort of young Turks wanting to upset the establishment.... One of the motivations was to show that the best way to solve cosmology was not the Hubble diagram".[33] There is no evidence that Tinsley was any less pleased to be iconoclastic than Schramm. Yet she had started her astronomical career with the explicit hope of making galaxies a more accurate cosmological test. One wonders if, for her, the triumph was a little bitter-sweet.

The period of the paper's writing and publication was one of tremendous transition for Tinsley. The paper attracted great attention: its citations are better measured by counting columns than entries; later it would appear in popularized form in *Scientific American*. Tinsley herself received the Annie Jump Cannon Award, to support junior women in astronomy. Still, she received no offer of a proper job in Texas, and complained in letters to her father that she felt "rejected and undervalued intellectually ... a *gut* problem to me".[34] Both personal and professional factors fed a sense of dissatisfaction that led to the Tinsleys' divorce in 1974. Placing herself on the job market alone, now willing to consider positions across the country, unconstrained by the formidable problem of finding two academic jobs in the same place, Beatrice almost immediately found herself receiving offers from first-rank institutions. Of the two she considered most seriously, Chicago and Yale, she chose Yale, both for its New England atmosphere, and for the opportunity to work with the galaxy dynamicist Richard Larson, who would become one of her most important collaborators. Her position at Yale provided Tinsley, for the first time, with professional security and, she wrote, "a sense of hope and power over the future that has escaped me for years".[35]

Tinsley's years at Yale saw a shift, even eventually a reversal, in the nature of her work. It is not that she lost interest in galaxy evolution, in cosmology, or in the relation between them; indeed, she continued publishing on all of these topics for the remainder of her life. However, she came to treat the study of galaxies less as a tool for cosmological testing, and more as an end in itself. At the same time, as her confidence in other kinds of cosmological tests increased, she grew to hope that cosmological parameters could be established with enough confidence that they could enhance our understanding of the early period of galaxy formation.

This shift was, in part, the unfolding of the very program Tinsley had laid out for herself in 1972, when she announced her return to research. Important additional impetus came from wider developments of the field. For instance, after hearing Tinsley speak on the evolution of galaxy populations, the Princeton theorist Jeremiah Ostriker suggested that galaxy mergers would also strongly affect photometric evolution. As he recalls telling her,

> I bet I can think of several dynamical effects that can make galaxies become brighter by that much — which isn't to say that your correction isn't correct, but there are probably others as well, which will have opposite signs. If this one changes it one way, they will probably change it in the other.[36]

Ostriker and his student Scott Tremaine then calculated that when galaxies merge, the net brightening could have "drastic effects" on the Hubble diagram.[37] Tinsley and Gunn agreed, writing in their own analysis of the effect that

> the temptation is great, given the uncertainties we have investigated (and the ones we have not ...), to dismiss the Hubble diagram as a cosmological test. Yet this reaction would be premature, given the importance of the value of q_0 for cosmology. An enormous amount of work clearly needs to be done.[38]

The sense that work needed to be done motivated another of the truly formative conferences of Tinsley's career. Unlike the First Texas Symposium that she had attended as an newcomer and outsider, this one was Tinsley's own party. In 1977 she and her Yale colleagues convened a watershed symposium on stellar populations. Much as the Texas participants claim that their meeting marked the beginning of relativistic astrophysics as a field, participants at the Yale meeting report that it, for the first time, brought together the many strands of galaxy evolution. Its *Proceedings*, edited by Tinsley and Larson, became a classic reference. More than this, David Schramm mused in an interview before his death, the Yale conference marked Tinsley's emergence not just as a full professional, but as a true insider. Before it, he said, Tinsley seemed to him like a child playing at the margins of a picnic. After it, she was at the table.

Sadly, she was left little time to enjoy the feast. In 1978 Tinsley learned that a lesion on her leg had become a melanoma, a malignant skin cancer requiring immediate surgery. She survived for three years, continuing her research and teaching while undergoing extensive radiation and chemotherapy. Eventually she was hospitalized in the Yale infirmary, only blocks away from the astronomy department, and there she completed her last paper days before her death on 23 March 1981, at the age of forty.

It seems fitting to end this paper with one of Tinsley's last published statements on the relation of galaxy studies to cosmology. It comes from an account of her contribution to a Les Houches summer session in 1979. By then travel was taxing for her. Nonetheless, she went to France to participate. Writing afterwards she reflected, "The rates of evolution are by no means well enough known for the Hubble diagram to be 'corrected' with any confidence. Perhaps the best approach is to turn the test around: let local data, in particular the density and age of the Universe, give the value of q_0 ... and then let the Hubble diagram tell us how elliptical galaxies evolve".[39] This is just the opposite of the approach that had shaped cosmology since Hubble, and that Tinsley took in her early research: that galaxies are the prime source of cosmological data. The first justification for Tinsley's cosmological optimism was gone. But in its place she seems imbued with a new optimism — one she would have thought quixotic even fifteen years before — that cosmology itself would soon be founded on data good enough that it would be able to contribute to our knowledge of galaxies.

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16. Harlan Smith to Kenneth Klivington (Sloan Foundation), 10 July 1974. From the University of Texas Archive.
17. *Ibid.*
18. Her father's retrospective comment on this quotation: "Prophetic". From Hill, *op. cit.* (ref. 13), 34. The anthology is almost certainly Milton K. Munitz (ed.), *Theories of the universe from Babylon to modern science* (New York, 1957). This matches Beatrice's description in being 429 pages of text as compared to the "425 pages of small print" she describes.
19. Scrapbooks in the possession of Brian Tinsley. Public aspects of the controversy are discussed in Kragh, *op. cit.* (ref. 2). For discussion of the broadcast debates, see pp. 243, 246.
20. Kragh, *op. cit.* (ref. 2).
21. *Ibid.* Kragh's preferred term is 'pragmatic', rather than 'factual'. It is also worth noting, with Kragh, that the subsequent clash between the two theories was far more complex than a simple transatlantic rivalry.
22. Hill, *op. cit.* (ref. 13), 44.
23. DeVaucouleurs himself, who worked in France, England, Australia and the United States, and whose publications span a wide range of astronomical topics, is not a convincing exemplar of Kragh's national styles, but perhaps such a polymath can be expected to defy categorization.
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