Inflationary Cosmology and the Horizon and Flatness Problems: The Mutual Constitution of Explanation and Questions

by

Roberta Brawer

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Abstract

This thesis, a study of conceptual change in contemporary cosmology, presents a sociological and conceptual history of the horizon and flatness problems and examines the relationship of these problems to the inflationary universe model, a new model of the dynamics of the very early universe. It is meant as a case study of how scientific questions emerge and become acknowledged by communities, and how, in actual practice, question and explanation interact together.

I argue that before the development of the inflationary model, the flatness problem was generally not considered a serious scientific problem by most researchers in the cosmology community. The horizon problem was more widely recognized, but was nonetheless treated as marginal by a large number of practitioners. After the development of inflation, both problems became prominent as serious shortcomings in the big bang model. I argue that this process is best understood as a mutual constitution of questions and explanation and, in particular, that a new explanation shifted the boundaries of what was communally marked as metaphysical conundrums and what were considered legitimate scientific questions.

The first chapter formulates the two questions quantitatively and develops the nature of the solutions provided by the inflation model. The second chapter outlines the history of the questions as reflected in the scientific literature. The third chapter describes the views of two dozen leading practitioners, probed in an interview project conducted by Alan Lightman and myself. The final chapter consists of discussion and argument for the view of mutual constitution.

Thesis Supervisor: Vera Kistiakowsky
Title: Professor of Physics

For my mother and stepfather,
poet and novelist,
who taught me to open to the solaces of words

my lifetime
listens to yours

Muriel Rukeyser

Acknowledgements

This project has taken a long time to complete. It has been a presence in my life -- sometimes a loose end, sometimes a dead weight -- for several years, and finishing it has involved, in part, a struggle to make peace between my former identity in physics and my current work in anthropology of science. Very luckily, I have received enormous support and friendship during this time. My advisor, Vera Kistiakowsky, has stood by me through thick and thin, offering support, sound advice, and an example of deep integrity and determination. She is now an emeritus professor at MIT, and it is a very great privilege to have been (perhaps) her last formal student. Ed Bertschinger, my reader, has generously given of his time, support, and vast knowledge of cosmology to me and to this project. He has managed that difficult balance of being open to my writing a non-traditional thesis with encouraging in me the kind of conscientiousness and care that is so evident in his own work and teaching.

Susan Carey inspired my choice of topic through her work in conceptual change in childhood and in science. More profoundly, watching the joy and humor and love she brings to her own and other's lives as scientists healed something deep in me and inspired me to try again at MIT. Domino, her family's much-loved (by us all) black lab, stayed with me the year I returned to the MIT physics department and reminded me every morning that, for some, each new day can be met with delight.

I am grateful to Alan Lightman for our lively and invigorating three-year collaboration on the interview project from which most of the quotations in Chapter 3 are drawn. I thank Alan Guth for his kindness and patience in discussing his views on the horizon and flatness problems with me at great length. George Koster has been a gentle presence at the physics graduate office since I first came to MIT. Peggy Berkowitz has brought a special sparkle to the life of that office as well as warmth and humor into the lives of many grad students here.

While physics is no longer my profession, my years studying it brought many dear friends into my life. By some enormously fortuitous twist of fate, several friends from undergraduate days at Columbia ended up here in Cambridge with me. Lisa Olson and I have shared hundreds of meals together since we would do E&M problems and make omelettes and chili in my tiny apartment on West 100th St. We have had innumerable conversations, about physics and most everything else, during walks around the Charles, trips to Walden Pond and hikes in the mountains. Since she has moved to Princeton, I miss hanging out with her and her (now three-- Welcome Brent!) sons; I miss her jokes, her wise counsel, and her daily presence in my life.

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horse, and started vet school this fall. Her happiness and her gumption are an inspiration to me.

Prisia Cehelsky and I were roommates during our first year at MIT and friends thereafter. She taught me her knack for making plants thrive and made me realize that sometimes I could cook, at least in her presence, downright fine dinners. A little of her gentle, caring ways rubbed off on me, and I am very grateful.

Ren and Mackie Davidson have shared with me their love and attention to the rhythms of outdoor life. They have taken me out on hikes to gentle waterfalls and riverpools in Vermont, down by the barn to split logs, off into the woods carrying planks to fix the horsebridge, and out by the sea to collect mussels for dinner. Their children, Gray and Kate, have opened their exuberant arms and hearts to let me in for hugs and whispers. They have all made sure that, at least on occasion, I don't fail to notice important matters like the porcupine quills on the barn floor or the funny texture of the pulp of wild grapes.

And finally, David Meltzer, one dear friend from New York who did not end up in Cambridge, has spent hundreds of hours on the phone with me, sharing my joys and all too often listening to my frustrations, gripes, and sorrows. David read drafts of the thesis, talked about it with me for more hours, I'm sure, than he cares to remember, and encouraged me to keep going through periods when I was convinced that I had had enough. Thank you, David, from the bottom of my heart.

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I have also been extraordinarily lucky to encounter some very skilled teachers outside of the academy. I thank Bob Nicoll, Melissa Wentworth, and Ellen Halpern each for teaching me a bit about their own particular understandings of energy -- understandings that have come to seem just as remarkable to me than those I encountered clustered around the same word in the world of physics. From Sam James and the folks in my Tuesday morning group, I have learned how to practice listening together with both heart and mind, both to myself and others. Marcia Fisch and Narayan Liebenson Grady, in their own very special ways, have helped me -- slowly, slowly, slowly, ever so cautiously, gingerly, carefully -- to trust, until I have come to believe, deeply although still with difficulty, what Muriel Rukeyser said so courageously many years ago: "However confused the scene of our life appears, however torn we may be who now do face that scene, it can be faced, and we can go on to be whole." I think they both already know how profoundly grateful I am.

Although we see each other infrequently, I carry my family in Michigan -- my father, Nae, Sarah, Phil, Judy and Chis -- in my thoughts and in my heart. And warmest congratulations to Steve and Madina and Yasmin. I look forward to March in Stockholm.

I don't know if I can find the words to thank my family in Maryland -- my mother and stepfather, my brothers John and Brian, my sister-in-law Elissa, and my niece Jenny -- for the love and support they have given. When I became ill five years ago, I needed them enormously, and they have all been there for me, present as air, nourishing, kind, thoughtful, funny, loving, and present, always present. We have all weathered, together, some very hard and scary times these past few years. I don't know how to express what it has all meant to me. Fortunately, I can lean on the words of our family friend, William Meredith: "That's what love is like. The whole river is melting. We skim along at great peril... Do the people there stand firmly on icebergs? Here all we have is love, a great undulating raft, melting slowly. We go out on it anyway. I love you, I love this fool's walk. The thing we have to learn is how to walk light." Thank you all for helping me try to learn to walk light. And I am buoyed by my warm, comforting memories of my aunt, Felicia Harrison, and the many people woven into our family tapestries, most especially Marj Rayburn, the Parrys, and Herman and Ruth Widder.[1]

I am very, very glad to have this thesis done, accepted, bound, archived and finished. Probably no one shares that pleasure with me more than my mother and stepfather, Betty and Hugh Parry, and for that sharing... and much more, I would like to dedicate it to both of them.

[1] I think Emma wants to make sure she is included. Yes, Emma, you are here, too.

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Introduction

In January of 1981, Alan Guth, a young particle theorist, published a paper in *Physical Review* entitled "Inflationary Universe: A possible solution to the horizon and flatness problems."[1] In this paper, Guth presented a new model of the dynamics of the very early universe, characterized by a staggeringly rapid expansion of space in the first fractions of a second after the big bang. This new model, dubbed the inflationary universe, was soon to become an extremely influential development in modern cosmology.

As the subtitle of his paper indicates, Guth's model was proposed as a solution to two outstanding cosmological puzzles, the horizon and flatness problems. Guth described the horizon problem as well-known; the flatness problem was characterized as less celebrated. Indeed, Guth felt it important to include an appendix to his paper arguing that the flatness problem was in fact a true scientific puzzle. Prior to Guth's paper, there are only a handful of references to the problem in the published literature.

Over the past fifteen years, the inflationary universe model has served as the basis for hundreds of papers in cosmology. As this new model has gained prominence, so have the questions that it resolved. Yet both the sociological and conceptual dynamics of the ways question and explanation have interacted in this process are complex and interesting.

Studies in the history of science have revealed that the relationship of question and explanation in science is not straightforward. In his classic study on scientific development, *The Structure of Scientific Revolutions*, Thomas Kuhn radically historicized our understanding of the relations between scientific question and theory. Kuhn argued that scientific knowl-

[1] Alan Guth, "Inflationary Universe: A Possible Solution to the Horizon and Flatness Problems," *Physical Review* D23(1981):347-356. Important modifications of this model were proposed independently by Andrei Linde and by Andreas Albrecht and Paul Steinhardt. See A. Linde, "A New Inflationary Scenario: A Possible Solution of the Horizon, Flatness, Homogeneity, Isotropy and Primordial Monopole Problems,"*Phys. Lett.* 108B(1982):389-393; and A. Albrecht and P. Steinhardt, "Cosmology for Grand Unified Theories with Radiatively Induced Symmetry Breaking,"*Phys. Rev. Lett.* 48(1982):1220-1223.

edge does not continuously develop as a cumulative web of understanding, asymptotically approaching the truth through expanding the range and breadth of questions that are woven into its fabric. Rather Kuhn argued for discontinuous breaks in which the cultural logic structuring the relationships between theories, questions, and ontologies breaks apart, to be re-formed and laid down again on nature whole, structured with a different set of relations.

In this process, Kuhn points out, the problem field addressed by scientific inquiry shifts as well. Some problems may be banished from science, as for example, was the outcome of the effort following the publication of Newton's Principia to find a mechanism for gravitational attraction that was consistent with the seventeenth century's emphasis on mechanico-corpuscular explanations. Gravity, as an innate attraction between bodies, was uncomfortably reminiscent of Aristotelian and scholastic explanations in terms of essences, explanations that had come to be seen as invoking occult qualities. Yet although much effort was expended by both Newton and his supporters, the problem remained intractable and was gradually abandoned, leading to the near- universal acceptance by mid-eighteenth century scientists of the interpretation of gravity as an innate attraction without further explanation. In other cases, issues that were previously non- problematic became, within the new framework, core problems demanding scientific explanation. With a change of paradigms, Kuhn argues,

> Some old problems may be relegated to another science or declared entirely "unscientific." Others that were previously non-existent or trivial may, with a new paradigm, become the very archetypes of significant scientific achievement. And as the problems change, so often, does the standard that distinguishes a real scientific solution from a mere metaphysical speculation, word game, or mathematical play.[2]

For Kuhn, this process is not a mere progression from error or ignorance to proper knowledge. Rather, opposing paradigms may often be incommensurable, such that advocates of

[2]Thomas S. Kuhn, *The Structure of Scientific Revolutions* (Chicago: University of Chicago Press, 1970), 103.

each may disagree about the list of problems that must be solved as well as the meaning of basic terms, which are often structured for each in a different web of relationships. For Kuhn, one set of problems and terms invoked within a paradigm cannot be translated into the other without residue. Thus, this shift in problems he notes is often symmetrical, not inclusive. Old problems are often lost as new problems are gained.

In a recent paper in Science entitled "When Do Anomalies Begin?," Alan Lightman and Owen Gingerich revisit this issue and describe five problems in the history of science which were "taken as givens or ignored in the old framework" and were recognized as anomalies "only after they are given compelling explanations within a new conceptual framework."[3] The flatness problem is one of the problems described. Lightman and Gingerich pose the history of the flatness problem is an example of "retrorecognition," a psychologically-based phenomenon in which certain problems or anomalies are overlooked by scientists and are only brought to consciousness after there is an explanation. As Lightman and Gingerich put it, "[i]f unexplained facts can be glossed over or reduced in importance or simply accepted as givens, the possible inadequacy of current theory does not have to be confronted. Then, when a new theory gives a compelling explanation of the previously unexplained facts, it is "safe" to recognize them for what they are."[4]

Thus, in distinction to Kuhn, Lightman and Gingerich imply a realist model of science in which questions previously unseen come to be recognized "for what they are." They invoke a mechanism of cognitive repression to account for "retrorecognition" phenomenon, that is uncomfortable for scientists to own up to the explanatory inadequacy of accepted theory. While this phenomenon is oft-noted and undoubtedly real and significant, I think it is too facile an explanation for the socio-cognitive dynamics of the flatness problem and its sibling, the horizon problem.

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[3]Alan Lightman and Owen Gingerich, "When Do Anomalies Begin?" *Science* 255(1991): 690- 695.
[4]Ibid., 694.

In this thesis, I will explore the history of these two problems. I will argue that the histories of the two problems differ and that these differences are not well accounted for by a simple cognitive repression model. I will argue that, in the case of these two problems, it is less a matter of retrorecognition by the community as a mutual constitution of questions and explanation. A new explanation shifted the boundaries of what was communally marked as metaphysical conundrums and what was marked as legitimate scientific questions.

Furthermore, I will consider how, in fact, this mutual constitution of question and explanation happens across a community of diverse actors, embedded in overlapping, but distinct, socio-professional worlds. I will argue that the flatness problem has had, and still does have, multiple meanings for scientists and is embedded in several different contexts of interpretation.

In short, in this thesis, the focus of investigation is on the two questions to which the inflation model was offered as a solution. What was the status of these two problems prior to the development of inflation? When were the questions first posed? When could they have been first posed, i.e. when were the theoretical and observational foundations underlying the questions first firmly established? How were the questions received when they were first articulated? What, if any, efforts had been made to solve them? In what ways has the status of the questions changed as a consequence of the inflationary universe model?

More broadly, the intent here is to explore the relationship between question and explanation in the scientific process. This investigation is meant as a case study on the issue of how scientific questions emerge and become acknowledged by the community and how, in actual practice, question and explanation interact together.

The first chapter describes the quantitative formulation of the two questions and the solutions offered by the inflationary universe model. The second chapter outlines the history of the questions as reflected in the scientific literature. The third chapter describes the views of two dozen practitioners whose thoughts about the problems were probed in an interview project conducted by Alan Lightman and myself. The final chapter consists of discussion and conclusions.

Chapter One: The Nature of the Problems

The Standard Model

The horizon and flatness problems both arise out of the standard model of modern cosmology – the hot big bang model. This model is remarkably simple. In its essential aspects, the evolution of the universe is described by two differential equations. Qualitatively, at early times, the universe is pictured as an extremely hot, dense system of thermalized matter and radiation undergoing rapid expansion. Due to gravitational attraction, the rate of expansion is continually slowing down. As the universe expands, the density of matter and energy decrease and the temperature falls. The average properties of the universe – energy density, expansion rate, temperature – are considered to be everywhere the same. On large scales, gravity is the dominant force.

This picture rests on three primary observational results: distant galaxies appear to be moving away from us with velocities proportional to their distance; a faint radio static has been detected, coming uniformly from all directions in space and exhibiting an intensity spectrum characteristic of blackbody radiation at a temperature of 2.7 °K; and astronomical measurements of the relative amounts of the chemical elements in the universe indicate that approximately 75% (by mass) is hydrogen and approximately 25% is helium, with only trace amounts of all other elements.

When coupled with general relativity and two basic symmetry assumptions, these three observations fit snugly into a theoretical framework describing the structure and evolution of the universe as a whole. The big bang model is not ad hoc; the equations describing it emerged out of the most widely accepted theory of gravity, general relativity. Furthermore, while the empirical observations are sparse, they nonetheless establish reasonably strict constraints. Only a few other models have been proposed, and none is considered to encompass the observations as well.

The horizon and flatness problems have become prominent in recent years as indicating shortcomings of the standard big bang model. Neither problem is based on an empirical or logical inconsistency of the model. Rather, it is argued that the two problems necessitate special assumptions about the initial conditions of the universe, assumptions so implausible that an alternative explanation is required.

The horizon problem arises from the observed uniformity of the microwave background radiation. The temperature of this radiation is isotropic to better than 1 part in 10^4, after accounting for a small dipole variation due to the peculiar motion of the earth and the galaxy. Yet at the time of emission, regions in opposite directions in the sky from which the radiation emanated were well outside of each other's horizons. Thus, no physical process propagating with velocity at or below the speed of light could have brought these regions into thermal equilibrium. The horizon problem is the inability of the standard model to account for this state of homogeneity.

The flatness problem arises from the observational constraints on omega (Ω), a dimensionless parameter that describes the ratio of the actual mass density of the universe to the critical mass density. A universe with critical density ($\Omega = 1$) is globally flat, at the borderline between a closed, finite universe and an open, infinite universe. Observational constraints put omega well between between 0.01 and 2 today, and most likely within an order of magnitude of 1. However, $\Omega = 1$ is an unstable equilibrium point. In order for omega to fall within the given constraints today, it had to be equal to 1 to within one part in 10^{15} at the time of nucleosynthesis, approximately one second after the big bang. The flatness problem is the puzzle of explaining why the early universe exhibits such fine tuning in the value of this parameter.

To describe the flatness and horizon problems quantitatively, we need the basic mathematical framework of the big bang model. Evidence that the universe is expanding is based on the the observation that the spectral lines of nearly all galaxies are shifted to the red. Interpreting this redshift as a Doppler shift leads to the conclusion that most galaxies are moving away from us. Indeed, correlation of the measured redshifts to the apparent luminosities of the galaxies yields the relationship

$$v = Hr$$

where v is the measured velocity of any galaxy, r its distance, and H is the constant of proportionality called the Hubble constant. [1]

When a medium is uniformly expanding, as with the Hubble law, the distance between any two selected points simply changes by a universal scale factor. It is convenient to introduce an expanding coordinate grid, generally called comoving coordinates. The coordinate distance between any two coordinate notches will be constant, but the physical distance will change with time. The physical distance can be determined by multiplying the coordinate distance by the scale factor. To describe how the system evolves over time, it is sufficient to describe how this scale factor changes over time. If at time t_o the distance between two notches is r_o, then the distance at at later time, $r(t)$, is

$$r(t) = R(t) r_o$$

where we have normalized $R(t_o) = 1$. To see that this statement implies the Hubble law, differentiate both sides with respect to time,

[1] Any uniformly expanding medium would exhibit this relationship between velocity and distance. For example, consider stretching a rubber band with equally spaced notches marked on it. The notches will move farther apart from each other, yet, at any given time, the distance between any two neighboring notches will the same everywhere on the rubber band. Imagine that the rubber band is stretched until the distance between notches is twice the original distance. A notch originally one centimeter away from a chosen reference notch would now be two centimeters away; a notch originally two centimeters away is now four centimeters away. Thus, the notch that was originally twice as far away has traveled twice the distance in the same period of time.

$$\dot{r}(t) = \dot{R}(t) r_o = \frac{\dot{R}(t)}{R(t)} R(t) r_o$$

Setting $H = \dot{R}/R$, and using the equation above gives the Hubble law relating velocity to distance,

$$\dot{r}(t) = H r(t)$$

In the generalization to a 3-dimensional space, the notches can replace by a vast, expanding 3-dimensional grid. Such an expanding coordinate system is called a set of comoving coordinates. Galaxies following the Hubble flow maintain the same comoving coordinates, although the physical distances between galaxies grow.

This picture is based on the assumption of homogeneity and isotropy of the universe, as seen by comoving observers. The term homogeneity refers to the assumption that the average properties of the universe are the same at every spatial location. Isotropy refers to the assumption that, at any given spatial location, all directions are equivalent. For example, the expansion rate or the temperature are the same in all directions. The term spatial location refers to the spatial locations of observers moving along with the universal expansion. Together the assumption of isotropy and homogeneity form what is known as the Cosmological Principle, a key assumption of the big bang model.

The cosmological principle constrains the possible geometry of the universe. It also permits a universal definition of time called cosmic time, which is the proper time measured by any comoving observer. Hypersurfaces of constant cosmic time may be curved, but the curvature must be uniform. The spacetime metric with this symmetry is called the Robertson-Walker metric,

$$ds^2 = c^2dt^2 - R^2(t)\left(\frac{dr^2}{1-kr^2} + r^2(d\theta^2 + sin^2\theta d\phi^2)\right) \tag{1}$$

The factor $R(t)$ is the universal scale factor previously mentioned. With suitable scaling of $R(t)$, the curvature constant k can be chosen equal to 0, +1, or -1. The dynamics of the model universe can be fully expressed by equations governing the dynamics of the scale factor. Physical distances can then be recovered by use of the above metric. With this metric, general relativity yields the equations, [2]

$$H^2 = \left(\frac{\dot{R}}{R}\right)^2 = \frac{8\pi G\rho}{3} - \frac{kc^2}{R^2} \tag{2}$$

$$\ddot{R} = -\frac{4\pi}{3}G(\rho c^2 + 3p)R \tag{3}$$

The expression for ρ in the above equation is the total mass density, or equivalently, energy density divided by c^2; p is the pressure. The above formulas are valid for contributions to the energy density both from non-relativistic matter and from radiation or relativistic particles. Today, the energy density of non-relativistic matter – stars, galaxies, and intergalactic dust – dominates over the energy density contributed by the 2.7 °K background radiation by about three orders of magnitude. However, since each has a different dependence on the scale factor, $R(t)$: $\rho_{mat} \propto 1/R^3$ and $\rho_{rad} \propto 1/R^4$, at earlier times, the energy density contributed by radiation would have dominated.

The Flatness Problem[3]

To describe the flatness problem, it is convenient to introduce the dimensionless density parameter $\Omega = \rho/\rho_c$, where ρ_c is the critical energy density, defined by setting the space

[2] I will take the cosmological constant to be zero for this discussion.

[3] The following treatment is based on unpublished lecture notes written by Alan Guth.

curvature term equal to zero in equation (2).

$$\rho_c = \frac{3H^2}{8\pi G}$$

Both ρ_c and H change with time.. At any given time, the critical density depends on the expansion rate at that time. Nonetheless, if the mass density is greater than ρ_c at *any* time, then gravity will eventually halt the expansion, and the universe will recollapse. Such a model is called a closed universe model. If the mass density is less than ρ_c, then the gravitational force will become continually weaker and weaker, and the expansion rate will asymptotically approach a constant, no longer slowing due to gravity. This is an open universe model. If the mass density is exactly equal to the critical density, the universe is called flat. In this case, the universe continue to expand forever, with the expansion rate asymptotically approaching zero.

The value of ρ_c today can be calculated from the present value of the Hubble constant, H_o, which is believed to lie within the range of 50 to 100 km s^{-1} Mpc^{-1}. Expressing this uncertainty in terms of a parameter h gives $H_o = 100h$ kms^{-1}Mpc^{-1} or $H_o = 3.24 \times 10^{-18} h$ s^{-1} with $.5 \leq h \leq 1$. Then $\rho_c = 1.88 \times 10^{-29} h^2$ gm/cm^3. Estimates of the actual mass density in the universe today conservatively fall within two orders of magnitudes of ρ_c. Thus, today, the density parameter Ω is close to 1.

To see how omega varies over time in the standard big bang model, equation (2) can be written in terms of Ω directly.

$$\frac{\Omega - 1}{\Omega} = \frac{3kc^2}{8\pi G \rho R^2}$$

If the energy density is predominantly due to the rest energy of non-relativistic matter, $\rho \propto 1/R^3$ and

$$\left|\frac{\Omega - 1}{\Omega}\right| \propto R$$

If the energy density were predominantly due to radiation or relativistic particles, $\rho \propto 1/R^4$ and

$$\left|\frac{\Omega - 1}{\Omega}\right| \propto R^2$$

Thus, as we extrapolate backwards, the scale factor R will approach zero, and Ω will approach 1. Moving forward in time, as R increases, Ω moves away from 1, provided it is not exactly equal to 1. To calculate the deviation from 1 quantitatively, we will need to know how R depends on time. It is sufficient for this calculation to consider the case of $k = 0$,

$$\left(\frac{\dot{R}}{R}\right)^2 = \frac{8\pi G\rho}{3}$$

For matter-dominated era, when the primary contribution to the mass density is non-relativistic matter, $\rho \propto 1/R^3$, thus

$$\dot{R} \propto \frac{1}{\sqrt{R}}$$

$$R \propto t^{2/3}$$

For radiation-dominated era, $\rho \propto 1/R^4$, and

$$\dot{R} \propto \frac{1}{R}$$

$$R \propto t^{1/2}$$

At the present time, the universe is matter-dominated, as can be seen by calculating the ratio of the energy density of radiation to the energy density of matter today. The energy density of blackbody radiation is a function only of its temperature

$$\rho_{rad}c^2 = aT^4$$

Using $a = 7.56 \times 10^{-15}$ erg/cm^3 °K^{-4} and $T_{rad} = 2.75$ °K,

$$\rho_{rad}c^2 = 4.33 \times 10^{-13} \text{ ergs/cm}^3$$

$$\rho_{rad} = 4.8 \times 10^{-34} \text{ gm/cm}^3$$

Estimates of present-day mass density based on luminous matter, are of the order,

$$\rho_m = 3 \times 10^{-31} \text{ gm/cm}^3$$

However, there is strong observational evidence for the existence of dark matter within galaxies, matter that is not light-emitting at any wavelength. This raises estimates of the mass density by about a factor of 10. Thus, at the present time, $\rho_{rad}/\rho_{mat} \approx 10^{-4}$.

In order to calculate the value of omega at earlier times, we need to know when the transition from a radiation-dominated universe to a matter-dominated universe occurred. We will approximate this time of transition t_T as the time at which the ratio $\rho_{rad}/\rho_{mat} = 1$. At that time, the energy density of matter and radiation would have contributed equally.

Since $\rho_{rad} \propto 1/R^4$ and $\rho_m \propto 1/R^3$, we can write an expression for this ratio at any time.

$$\frac{\rho_{rad}(t)}{\rho_m(t)} = \frac{\rho_{rad}(t_o)}{\rho_m(t_o)}\frac{R(t_o)}{R(t)}$$

During the matter-dominated era, we can use the relation $R(t) \propto t^{2/3}$.

$$\frac{\rho_{rad}(t)}{\rho_m(t)} = 10^{-4} \left(\frac{t_o}{t}\right)^{2/3}$$

If we take the present age of the universe, t_o, to be approximately 13 billion years, then the above equation gives a transition time, t_T, of about 13,000 years.

Estimates of Ω_o today fall within the range, $0.1 < \Omega_o < 2$ and thus,

$$\left|\frac{\Omega_o - 1}{\Omega_o}\right| < 10$$

During the matter-dominated era, this ratio scales with the two-thirds power of time.

$$\left|\frac{\Omega - 1}{\Omega}\right| = \left(\frac{t}{t_o}\right)^{2/3} \left(\frac{\Omega_o - 1}{\Omega_o}\right)$$

Thus, at the time of transition between radiation and matter-dominated eras, this ratio is four orders of magnitude smaller.

$$\left|\frac{\Omega_T - 1}{\Omega_T}\right| = 10^{-4} \left(\frac{\Omega_o - 1}{\Omega_o}\right)$$

In the radiation-dominated era, this ratio scales directly with t. At t=1 second, the time of nucleosynthesis,

$$\left|\frac{\Omega_N - 1}{\Omega_N}\right| = 2 \times 10^{-16} \left(\frac{\Omega_o - 1}{\Omega_o}\right)$$

$$|\Omega - 1|_{t=1sec} < 10^{-15}$$

The flatness problem is the puzzle of explaining this very fine tuning of omega – a relationship which has no explanation in the standard model.

The Horizon Problem

To describe the horizon problem, it is necessary to discuss the equations that represent the trajectory of light within the expanding universe model. Light propagates along world lines for which the spacetime interval is zero. Thus,

$$0 = ds^2 = c^2dt^2 - R^2(t)\left(\frac{dr^2}{1-kr^2} + r^2(d\theta^2 + sin^2\theta d\phi^2)\right) \tag{4}$$

We consider light emitted by one comoving observer at the coordinate r_e and cosmic time t_e and received by another comoving observer at coordinate r_o and time t_o. For convenience, choose r_o, the coordinate of the receiving observer as the origin. By the cosmological principle, any point can be chosen as the origin without loss of generality. With this choice of coordinates, light will propagate along lines of constant ϕ, θ. Then, from the above metric, the time of propagation is related to the coordinate distance transversed by the following expression,

$$\int_{t_e}^{t_o} \frac{cdt}{R(t)} = \int_0^{r_e} \frac{dr}{\sqrt{1-kr^2}} \tag{5}$$

This expression can be used to derive a relationship between the observed redshift of light emitted from distant galaxies and the scale factor. If light with frequency ν_e, and period $\Delta t_e = 1/\nu_e$, is being emitted, then the first wavefront will follow a trajectory described by the above equation. The crest of the second wavefront will be emitted from the same comoving coordinate at a time Δt_e later, and will follow a trajectory described by the following equation.

$$\int_{t_e+\Delta t_e}^{t_o+\Delta t_o} \frac{cdt}{R(t)} = \int_0^{r_e} \frac{dr}{\sqrt{1-kr^2}} \tag{6}$$

The right hand side of both equations are the same. Subtracting the first equation from the second and noting that Δt_e and Δt_o are small enough that for both $R(t + \Delta t) \approx R(t)$, then

$$\frac{\Delta t_o}{\Delta t_e} = \frac{R(t_e)}{R(t_o)}$$

or equivalently,

$$\frac{\lambda_o}{\lambda_e} = \frac{R(t_e)}{R(t_o)}$$

We define the redshift parameter z as the fractional change in wavelength of the emitted light.

$$z \equiv \frac{\lambda_o - \lambda_e}{\lambda_e} = \frac{\lambda_o}{\lambda_e} - 1$$

$$1 + z = \frac{\lambda_o}{\lambda_e} = \frac{R(t_o)}{R(t_e)} \tag{7}$$

Given an explicit expression for $R(t)$, measurements of the redshift would then allow us to calculate the time at which the light was emitted. More generally, the redshift parameter z can be used as an implicit expression for time. Note also that z expresses the amount of expansion of the universe since the light was emitted. If $z = 1$, then the scale factor, and thus the size of the universe, has doubled between emission and reception of the light.

The proper distance of the emitting source at the time the light is received is the coordinate distance of the emitting region multiplied by the scale factor at the time of reception.

$$d_{prop} = R(t_o) \int_0^{r_e} \frac{dr}{\sqrt{1 - kr^2}} = R(t_o) \int_{t_e}^{t_o} \frac{cdt}{R(t)}$$

The finite speed for the propagation of light sets a limit on the distance from which signals can be received by an observer located at $r = 0$. For any given time of emission t_e, there is a region of comoving space defined by $r \leq r_e$ such that signals outside of this region could not have reached $r = 0$ by time t_o. The big bang model postulates a beginning of time defined as the time when the scale factor $R(t) = 0$. Thus, the integral

$$R(t) \int_0^t \frac{cdt'}{R(t')}$$

represents the farthest distance that a light signal could have traveled since $t = 0$. If this integral converges, then there exists what is known as a particle horizon, which represents the farthest distance over which regions could have communicated since the big bang. For Friedmann models, the integral does converge. We will call d_H the proper, or physical, distance of the horizon and r_H its radial comoving coordinate.

$$d_H(t) = R(t) \int_0^t \frac{cdt'}{R(t')} = R(t) \int_0^{r_H} \frac{dr}{\sqrt{1 - kr^2}} \tag{8}$$

For the case of $k = 0$, the horizon distance can be easily calculated. In the radiation dominated era, $R(t) \propto t^{1/2}$, and

$$d_H(t) = t^{1/2} \int_0^t \frac{cdt'}{t'^{1/2}} = 2ct$$

In the matter dominated era, $R(t) \propto t^{2/3}$ and

$$d_H(t) = t^{2/3} \int_0^t \frac{cdt'}{t'^{2/3}} = 3ct$$

Today we detect microwave radiation coming uniformly from every direction of the sky. The physical distance of the source of that radiation at the time of emission was

$$d_{CBR}(t_e) = R(t_e) \int_{t_e}^{t_o} \frac{cdt}{R(t)}$$

Thus at the time of emission, the sources of radiation coming from opposite directions of the sky would have been separated by a physical distance of approximately

$$d_{sep} = 2d_{CBR}(t_e)$$

The size of the horizon at the time of emission was,

$$d_H(t_e) = R(t_e)\int_0^{t_e} \frac{cdt}{R(t)} \tag{9}$$

Now we can ask whether regions emitting the microwave radiation would have been in causal contact with each other. To do this, we calculate the ratio of the separation distance to the horizon size. If this ratio is greater than 1, the two regions are outside each other's horizon; if the ratio is greater than 2, no signal emitted from an intermediate point could have reached both regions.

$$\frac{d_{sep}(t_e)}{d_H(t_e)} = \frac{2\int_{t_e}^{t_o} \frac{cdt}{R(t)}}{\int_0^{t_e} \frac{cdt}{R(t)}}$$

To calculate this ratio, I will consider the simple case of a matter-dominated $k = 0$ universe.

$$d_{sep}(t_e) = 2R(t_e)\int_{t_e}^{t_o} \frac{cdt}{R(t)} = \frac{6ct_oR(t_e)}{R(t_o)}\left(1 - \left(\frac{t_e}{t_o}\right)^{1/3}\right)$$

$$d_H(t_e) = R(t_e)\int_0^{t_e} \frac{cdt}{R(t)} = \frac{3ct_oR(t_e)}{R(t_o)}\left(\frac{t_e}{t_o}\right)^{1/3}$$

$$\frac{d_{sep}(t_e)}{d_H(t_e)} = 2\left(\left(\frac{t_o}{t_e}\right)^{1/3} - 1\right)$$

This expression can be written directly in terms of the redshift parameter z, eliminating the dependence on time.

$$1 + z = \frac{R(t_o)}{R(t_e)} = \left(\frac{t_o}{t_e}\right)^{2/3}$$

$$\frac{d_{sep}(t_e)}{d_H(t_e)} = 2\left((1+z)^{1/2} - 1\right)$$

The microwave background was emitted at about $z = 1500$, which corresponds to a temperature of $T \approx 4000$ °K. Thus, the separation distance was approximately 80 times the horizon distance at the time of emission.

The Inflation Model as Solution to the Flatness and Horizon Problems

In the inflationary model, there is a brief period during which the expansion rate of universe is exponential rather than a power law. This inflationary spurt, which is taken to occur from approximately 10^{-35} seconds to 10^{-32} seconds after the hypothesized big bang, is the key aspect of how the model provides solutions to the flatness and horizon problems. After this brief but dramatic inflationary era, the expansion rate returns to the more leisurely rate of the standard model.

A key element of the inflationary epoch is that the energy density during this era is taken to be constant and, thus, is not reduced by the expansion of space. The inflationary scenario requires that regions of space be extremely hot, with temperatures of $T > 10^{14}$ GeV, and that these regions are trapped in state known as the false vacuum in grand unified theories. In such regions, the energy density cannot fall below the energy density of the false vacuum.

Under conditions of constant energy density, and assuming that the space curvature term is negligible compared to the mass density term, the solution to equation (2) reprinted below:

$$H^2 = \left(\frac{\dot{R}}{R}\right)^2 = \frac{8\pi G\rho}{3} - \frac{kc^2}{R^2} \tag{10}$$

becomes

$$R = Ce^{Ht}. \tag{11}$$

with

$$H = \sqrt{8\pi G\rho/3}$$

In this picture, a supercooled region of space trapped in the false vacuum will expand exponentially, and as the energy density is at a fixed minimum, energy is created throughout this rapidly expanding region. The duration of the expansion depends of details on the GUTs model and of the phase transition scenario. One type of grand unified theory Guth considered lead to a value of 1/H of approximately 10^{-34} seconds. It is generally considered that the inflationary era lasts for 10^{-32} seconds or longer, in which case the expansion factor would be at least e^{100}. For concreteness, the expansion factor is often taken to be of order 10^{50}. At the end of the inflationary era, a phase transition to the true vacuum occurs, with the release of tremendous amounts of latent heat energy.

The inflationary model solves the horizon problem in a clear, direct way. To see this, consider again the expression for the horizon distance, equation (9), reprinted below.

$$d_H(t_e) = R(t_e)\int_0^{t_e} \frac{cdt}{R(t)}$$

This expression can be used to calculate the change in the horizon size during the inflationary era, during which $R(t)$ has the exponential form as expressed above in equation (11):

$$d_H(t_{end}) = R(t_{end}) \int_{t_{start}}^{t_{end}} \frac{cdt}{R(t)}$$

In the above, t_{start} and t_{end} are respectively the times that the inflationary epoch begins and ends. Thus, the increase in the horizon size during inflation is approximately

$$d_H(t_{end}) = \frac{ce^{Ht_{end}}}{H}$$

For the numbers discussed above, this yields

$$d_H(t_{end}) = \frac{1}{H} ce^{100} \cong 10^{19} cm.$$

At present, the radius of the observable universe is of order $3ct \cong 10^{28}$ cm. At the end of the inflationary era, $R(t)$ was smaller by about a factor of 10^{-27}. Thus, at that time, the presently observable universe had a physical size of about 10 cm. This was well within the enormously larger horizon size calculated above. Indeed, it can be seen from the arguments above that the expansion factor during inflation had to be at least 10^{25} in order to solve the horizon problem

In brief, during the inflationary epoch, tiny regions of space that were in causal contact and had had time to reach thermal equilibrium were blown up to occupy huge volumes of space. Thus, the horizon problem is obviated. Regions that were considered to be outside each other's horizons in the standard model were, in fact, once in causal contact in the inflationary picture.

Finally, to see how the inflationary model solves the flatness problem, consider again the expression of eqn (2) when it is written in terms of omega:

$$\frac{\Omega - 1}{\Omega} = \frac{3kc^2}{8\pi G\rho R^2}$$

During the inflationary era, all terms in the right hand side of this equation will be constant except for the scale factor R. As discussed above, the scale factor is considered to increase by a factor of 10^{50} during the inflationary epoch. Thus, the denominator on the right hand side of the equation is reduced by a factor of 10^{100}, which means that after inflation, omega will equal to one to very great precision.

Thus, in the inflationary era, omega is driven to be extremely close to one. The model provides, therefore, a dynamical explanation for the extremely close balance between the expansion rate and mass density in the very early universe.

A key consequence of the inflationary model is that at present omega will still be equal to one to very high precision.[4]This value is not supported by observational measurements. Nearly all evidence points to a value of omega today on the order of 0.2 or less. However, it is hypothesized that that there may be non-baryonic dark matter that has gone undetected so far and which will bring omega up to one. This discrepancy between the inflationary model and current observational evidence plays an important in discussions about the legitimacy of the flatness problem, which are taken up in Chapter 3 of this thesis.

[4]In the past year or so, theorists have developed models called "open inflation" which result in omega less than one. To my understanding, the status of such models are generally considered tenuous, and they require fine-tuning to achieve this result. Thus, one might argue that the orginal fine-tuning of omega has been replaced by an alternative fine-tuning. I am very grateful to David Kaiser for calling my attention to these models and generously sharing his personal collection of preprints with me.

Chapter Two: The History of the Problems

Today one can find hundreds of references to the flatness problem in both textbooks and research literature. Yet, prior to Guth's 1981 article, I have found fewer than half a dozen published discussions of the problem. The history of the horizon problem displays a similar, but less dramatic, pattern. While both problems hit the scientific limelight after 1981, the horizon problem had been recognized as a serious issue prior to Guth's inflation paper and had been discussed both in research literature and in graduate level textbooks.[1] This chapter traces the conceptual history of each problem, considering the more well-known horizon problem first.

To my knowledge, the horizon puzzle was first articulated by Charles Misner. In 1967, Misner began to ponder the isotropy of the the microwave background radiation and raised the general question: why is the universe isotropic? Noting that the microwave background radiation was found to be isotropic to within 0.2%, Misner remarked that this constituted the most accurate observational datum in cosmology. "As such," he states, "it surely deserves a better explanation than is provided by the postulate that the Universe, from the beginning, was remarkably symmetric."[2] Not satisfied with simply attributing this property of isotropy to the initial state, Misner hoped that observed isotropy of the universe could be shown to be a consequence of dynamical processes that occurred in the early universe. In particular, he wished to show that a broad class of homogeneous but anisotropic solutions of the equations of general relativity would, due to mechanisms such as neutrino viscosity, all evolve to a generally isotropic state.

This research effort was part of a more general reorientation that Misner wished to

[1]See, for example, Steven Weinberg,*Gravitation and Cosmology* (New York: John Wiley and Sons, 1972), 525-6; and Charles W. Misner, Kip S. Thorne, and John A. Wheeler, *Gravitation* (San Francisco: W.H. Freeman, 1973), 815-6.

[2]Charles W. Misner, "The Isotropy of the Universe," *The Astrophysical Journal*, 151(1968):431.

promote. Up until the mid-1960s, research in cosmology was often considered to be a search for two numbers, the Hubble constant and the deceleration parameter.[3] Assuming a zero cosmological constant in the Friedmann models, it was believed that these two numbers could determine the ultimate fate of the universe – whether it would expand forever or eventually recollapse.

In his 1968 paper on the isotropy of the universe, Misner suggested a different approach. "Rather than taking the unique problem of relativistic cosmology to be the collection and correlation of observational data sufficient to distinguish among a small number of simple cosmological solutions of Einstein's equations, I suggest that some theoretical effort be devoted to calculations which try to "predict" the presently observable Universe."[4] The difficulty with this approach, as Misner pointed out, rests with the matter of initial conditions.

Initial conditions are usually not a matter of fundamental concern in physics. In general, a theoretical physicist's conception of nature is embodied in dynamical laws often expressed as differential equations governing the time evolution of a physical system. A physical system can evolve very differently depending on its initial conditions. Yet this multiplicity of the ways particular systems develop can be unified by underlying fundamental laws. For example, imagine releasing a number of massive bodies in the vicinity of the sun, and then allowing the system to evolve. The later arrangement and motion of the masses will depend on their positions and velocities when released. One may end up with nine planets orbiting the sun, or the bodies may have all flown away far from the sun. To the physicist, the final arrangement of the masses is not fundamental; it is a consequence of particular conditions at the time of release. Rather, it is the dynamical laws of gravitation that express the fundamental essence and universality of nature.

[3]See, for example, Allan R. Sandage, "Cosmology: A Search for Two Numbers," *Physics Today*, February 1970.

[4]Misner, "Isotropy of the Universe," 432.

In the field of cosmology, though, this outlook runs into problems. We have only one system. There is only one universe, and the big bang model postulates a beginning to it. Traditionally, explanations of the nature of the universe rely on both dynamical laws and a specification of the initial state. However, the initial conditions at the time of the birth of the universe are unlike any other specification of initial conditions. They cannot be taken to be the consequence of a prior physical process. To the extent that initial conditions play a necessary role in understanding of the structure of the universe today, they lie outside of the domain of physical explanations.

Concern about the issue of a beginning has been a recurring theme throughout the history of 20th century cosmology. In his famous 1917 paper first applying general relativity to cosmology, Einstein introduced a cosmological constant term into the equations for the purpose of producing a model of the universe that was both unchanging and infinite in time, obviating the need to consider a beginning. In 1931, Eddington remarked that "philosophically the notion of a beginning of Nature is repugnant to me."[5] In the late 1940s, a key motivation of Fred Hoyle's introduction of the steady state model of the universe was to avoid the necessity of specifying an initial state.[6]

As Misner remarks, the "difficulty in using relativistic cosmology for predictive rather than merely descriptive purposes lies in the treatment of initial conditions."[7] To deal with this, Misner wished to survey a limited class of solutions to the Einstein equations "to see whether some presently observable properties of the Universe may be largely independent of the initial conditions admitted for study." [8]

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[5]Quoted in Helge Kragh, "The Beginning of the World: Georges Lemaitre and the Expanding Universe," *Centaurus* 32(1987):114-139, p.130.

[6]Fred Hoyle, "A New Model for the Expanding Universe," *Monthly Notices of the Royal Astronomical Society* 108(1948):372.

[7]Misner,"Isotropy of the Universe," 432.

[8]Ibid., 432.

Misner recalls that his approach at the time was motivated as an effort to "change the goals of scientific cosmology from describing the universe to explaining it." He wished to try to show that the universe as we find it is not simply consistent with the laws of physics, but in fact a necessary consequence of the physical laws – "that no very different cosmos was allowed (or was plausible)."[9]

Misner's paper on the mixmaster universe in 1969 contains the earliest published statement of the horizon problem that I have seen.[10] In this paper, a new cosmological model is proposed, based on a metric which would eliminate particle horizons in one direction. In Misner's model, this process goes on "infinitely many times with different directions having the open channels of communication each time."[11] Thus, the mixmaster model was explicitly based on a quantitative statement of the horizon problem and was put forward as an effort to develop models that "may lead to some insight into how the broad-scale homogeneity of the universe may have been produced at very early times."[12] While it was later shown that Misner's model was not a workable solution, the horizon problem was originally introduced in conjunction with a new cosmological model proposing a solution.

The first published statement of the flatness problem that I have been able to uncover is a brief parenthetical passage in Robert Dicke's Jayne Lectures in 1969. Dicke's own recollection is that he first formulated the problem when preparing these lectures.[13] In his final lecture, Dicke discusses cosmology, deriving the Friedmann equation within a Newtonian theory of gravity. He then mentions in passing:

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[9]Written notes received from Charles Misner, August, 1989.

[10]Charles W. Misner, "Mixmaster Universe," *Physical Review Letters* 22(1969):1071. The name, "Mixmaster," was taken from the brand name of a common kitchen appliance.

[11]Ibid.,1071.

[12]Ibid.,1071.

[13]Interview with Robert Dicke in Alan Lightman and Roberta Brawer, *Origins: The Lives and Worlds of Modern Cosmologists* (Cambridge: Harvard University Press, 1990), 207. I believe all other later statements of the problem can be traced back to Dicke.

> There are peculiar puzzles about this Universe of ours. As it gets ever older, more and more of the Universe comes into view, but when new matter appears it is isotropically distributed about us, and it has the appropriate density and velocity to be part of a uniform universe. How did this uniformity come about if the first communication of the various parts of the Universe with each other first occurred long after the expansion? C. Misner suggests that the extremely young Universe was highly disorganized and that, because of the disorder, the various parts were brought into contact with each other before the disorder was eradicated.
>
> Another matter is equally puzzling. The constant ${v_o}^2$ in [the equation]
>
> $$v^2 = \frac{8\pi G \rho r^2}{3} - {v_o}^2$$
>
> is very small, so small that we are uncertain with our poor knowledge of ρ as to whether or not it is zero. But the first term on the right [in the above equation] was very much larger much earlier, at least 10^3 times as great when the galaxies first started to form and at least 10^{13} times as great when nuclear reactions were taking place in the "fireball," assuming that the "fireball" story to which I will return is correct. The puzzle here is the following: how did the initial explosion become started with such precision, the outward radial motion became so finely adjusted as to enable the various parts of the Universe to fly apart while continuously slowing in the rate of expansion?[14]

In 1979, Dicke and his former student James Peebles write an article on cosmology for the Einstein Centenary celebration. They state the flatness problem more fully and try to argue for its persuasiveness. Again, the discussion begins with the horizon problem and then moves to the flatness puzzle.

> The distant galaxies observed in well-separated parts of the sky are so far apart from each other that there is not time enough since the big bang for a signal to have traveled from one to the other. Observers on Earth can see and compare them, being about half-way in between, and in line with homogeneity it is found that galaxies are quite similar. By comparing radiation background intensities across the sky it is also found that the temperature and expansion rate are precisely synchronized across the visible universe. Even though the separate parts of the visible universe are not visible to each other they are evolving in very precise unison.
>
> Are the structural relations between widely separated parts of the universe a problem? In the past these parts were much closer together. But close proximity in earlier times does not eliminate the problem. Assuming that causal relations require the transport of information from one place to another at a velocity not exceeding that

[14] Robert H. Dicke, *Gravitation and The Universe: The Jayne Lectures for 1969* (Philadelphia: American Philosophical Institute, 1970), 61-62.

of light, the zone of influence around any given object is not only smaller in the past (because the universe is younger) – it contains less matter.

The relationships of widely separated parts of the universe are not the only problem. There is a remarkable balance of mass density and expansion rate. In general relativity theory with $\Lambda = 0$ the two are related by the equation

$$H^2 = \left(\frac{1}{a}\frac{da}{dt}\right)^2 = \frac{8}{3}\pi G\rho(t) - \frac{c^2}{R^2a^2}$$

where a(t) is the expansion parameter, R is a constant, and $|R|a(t)$ is the magnitude of the space curvature (measured in a hypersurface of roughly constant proper number density, at fixed cosmic time t). The present relative value of the two terms on the right side of this equation is poorly known, because the mean mass density, ρ, is so uncertain, but it is unlikely that the first term is less than 3 per cent of the magnitude of the second. Since ρ varies as a^{-3} (or more rapidly if pressure is important) the mass term on the right-hand side dominates the curvature term when a is less than about 3 per cent of its present value. Tracing the expansion back in time, one finds that at $t \sim 1 sec$, when much of the helium is thought to have been produced, the mass term is some 14 orders of magnitude larger than the curvature term. This means that the expansion rate has been tuned to agree with the mass density to an accuracy better than 1 part in 10^{14}. In the limit $t \rightarrow 0$, this balance between effective kinetic energy of expansion, measured by H^2, and the gravitational potential energy, measured by $\frac{8}{3}\pi G\rho$, is arbitrarily accurate.

As was first pointed out by Lemaître, [the previous equation] gives a reasonable approximation to the evolution of separate parts of an inhomogeneous universe, so this precise initial balance of density and expansion rate (with a well-synchronized start) must apply to each separate part. Otherwise, the universe would run amuck. The inhomogeneities would produce large and irregular space curvature, leading to black holes of all sizes. . .

The global H and ρ say that the present value of $c/a|R|$ is less than $10^{-17}s^{-1}$. It seems curious that such a small quantity should have been built into the universe at the big bang, and so it has often been suggested that the only 'reasonable' value of $R^{-2} = 0$. This was more or less the position of Einstein and de Sitter (1932). . .

We consider this argument from simplicity attractive but perhaps somewhat weakened by the fact that when [the above eqn] is applied to sections of the universe R^{-2} does not vanish. Matter does cluster (at least, that part in galaxies) on scales less than approximately two orders of magnitude smaller than the horizon. . . So to account for the observed large-scale clumping of matter one must suppose that in the limit $t \rightarrow 0$ the local balance of expansion and gravity was extremely accurate but not exact![15]

What lesson do Dicke and Peebles mean to suggest in their articulation of these puzzles?

[15]R. H. Dicke and P.J.E. Peebles, "The Big Bang Cosmology – Enigmas and Nostrums," in *General Relativity: An Einstein Centenary Survey*, eds., S.W. Hawking and W. Israel (Cambridge: Cambridge University Press, 1979), 506-508.

At the beginning of their article, they state clearly that the two enigmas "lead us to think than an important piece of the picture may be missing. . . They certainly have something to teach us. But what is it?"[16] In their discussion, Dicke and Peebles do not claim any priority for formulating these puzzles. Indeed, they say that "most of the conundrums and nostrums discussed here trace back, in one form or another, to the lively discussions in the early 1930s" and that they "are reviewing and interpreting lore revealed to us when we came into the field."[17]

These latter remarks, I believe, are not quite accurate. The two puzzles discussed, now known as the flatness and horizon problems, certainly could have been formulated and discussed in the 1930s.[18] The theoretical foundations of the big bang model and the Friedmann equations were all in place. But as far as I can presently tell, the puzzles were not formulated until the late 1960s. As discussed earlier, to my knowledge, the flatness problem was first articulated by Dicke in 1969 in a series of popular lectures on cosmology, and the horizon puzzle was formulated by Charles Misner in connection with his work on mixmaster models.

Prior to 1981, the only other printed discussion of the flatness problem that I have been able to find is in a 1974 paper by Hawking entitled, "The Anisotropy of the Universe at Large Times."[19] In this paper, the fine-tuning between the expansion rate and mass density is pointed out and is used as a step in broader argument that attempts to answer the question of why the universe is isotropic. Briefly, the argument involves introducing the anthropic

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[16]Ibid., 504.

[17]Ibid., 504.

[18]The horizon problem can be formulated without explicitly invoking the microwave background. See, e.g. A. Zee, "Horizon Problem and the Broken-Symmetric Theory of Gravity," *Physical Review Letters* 44(1980):703-706.

[19]S.W. Hawking, "The Anisotropy of the Universe at Large Times." In *Confrontation of Cosmological Theories with Observational Data*, edited by M.S. Longair (Dordrecht-Holland: D. Reidel, 1974). This work builds on arguments in an earlier paper by C.B. Collins and S.W. Hawking "Why is the Universe Isotropic?" *The Astrophysical Journal* 180(1973):317-334. The flatness, or expansion rate, problem, however, is not discussed in this earlier paper.

principle coupled with the notion of an ensemble of universes. It is worth quoting in detail as it contains a very clear statement of how an anthropic argument can be used to solve the flatness problem.

> One now has to face the question of why the Universe should be expanding at so nearly the critical rate to avoid recollapse. It seems difficult to explain this in terms of processes in the early stages of the Universe because the differences would be so small at these epochs: a reduction of the rate of expansion by one part in 10^{12} at the time when the temperature of the Universe was 10^{10} K would have resulted in the Universe starting to recollapse when its radius was only 1/3000 of the present value and the temperature was still 10,000 deg. The only 'explanation' we can offer is one based on a suggestion of Dicke(1961) and Carter(1970). The idea is that there are certain conditions which are necessary for the development of intelligent life: out of all conceivable universes, only in those in which these conditions occur will there be beings to observe the Universe. Thus our existence requires the Universe to have certain properties. Among these properties would seem to be the existence of gravitationally bound systems such as stars and galaxies and a long enough time-scale for biological evolution to occur. If the Universe were expanding too slowly, it would not have this second property for it would recollapse too soon. If it were expanding too fast, regions which had slightly higher densities than the average or slightly slower rates of expansion would still continue expanding indefinitely and would not form bound systems. Thus it would seem that life is possible only because the Universe is expanding at just the rate required to avoid recollapse. The conclusion is, therefore, that the isotropy of the Universe and our existence are both results of the fact that the Universe is expanding at just about the critical rate. Since we could not observe the Universe to be different if we were not here, one can say, in a sense, that the isotropy of the Universe is a consequence of our existence.[20]

To my knowledge, this is only explanation offered for the flatness puzzle in the scientific literature prior to Guth's work. In 1981, Guth's paper on the inflationary universe model appeared. Guth not only stated the problem very clearly, he gave it the name "flatness problem" and developed a new model with a dynamical explanation. Acknowledging the controversial nature of the problem, he wrote an appendix to convince skeptics that it is a legitimate problem. By the early 1990s, the problem was discussed widely – in the scientific literature, in popular literature, and in both introductory and advanced textbooks.[21]

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[20] Hawking, "Anisotropy of the Universe," 285-286.

[21] One measure of how fully a topic has been integrated into a scientific culture is its appearance on standard

What accounts for this very rapid transition? Why is the problem taken extremely seriously today while it was almost ignored 15 years ago? The immediate answer is that there is a solution; with inflation, there is a way of addressing the problem. This is certainly an important component to understanding the phenomena, but this answer leads to other questions. Why should the legitimacy of the problem depend on the existence of a solution? In other words, one can argue that the determination of whether the flatness problem indicates a shortcoming of the big bang theory "should" be independent of whether there exists a revision to the big bang model (or another model) that resolves it. For example, when the Hubble constant was believed to be about ten times larger than it is taken to be today, there was a contradiction between the age of the universe and the age of the earth, the latter being about twice as old. While people may have had different feelings about how serious that problem was, the *existence* of a problem was not in dispute.

graduate exams. At MIT, a set of "137 Questions" used by students to study for the astrophysics oral exam includes: "What is the flatness problem and how is it resolved?"

Chapter Three: Views of Leading Practitioners

The previous chapter recounts the history of the flatness and horizon problems based on examining published literature. In several ways, the two problems appear to have a similar history. Both first appeared in the literature in the late 1960s. The horizon problem was presented in the technical literature with a model proposing a solution. The flatness problem was presented in a series of lectures for the general public and was posed simply as a puzzle. Both concern initial conditions at the "birth" of the universe, and both can be solved by positing special initial conditions. Prior to inflation, the horizon problem was referred to more frequently in the literature than the flatness problem, but both were mentioned relatively rarely. After inflation, the problems are commonly discussed.

To get a richer, more detailed picture of the emergence and history of the flatness and horizon problems, 27 leading researchers in cosmology were interviewed. This study was part of an oral history project on cosmology conducted in 1987-1990 by Alan Lightman and myself. Regarding the flatness and horizon problems, the researchers were asked about their reactions when they first heard of the problems, their current views, and whether their views changed with the development of the inflation.

An historian of science would be quick to issue a methodological caution about an analysis based on recollections. Retrospective reminiscences by scientists are considered notoriously unreliable. Not only are memories fallible, but scientists are apt, quite understandably, to filter their recollections through their current frameworks of understanding and belief. [1]

[1]For a thoughtful discussion of these issues, see J.L. Heilbron, "An historian's interest in particle physics," in *Pions to Quarks: Particle Physics in the 1950s*, ed. Laurie Brown, Max Dresden, and Lillian Hoddeson (Cambridge: Cambridge University Press, 1989). Indeed, some scientists interviewed were well aware of such problems. For example, when Roger Penrose was asked how he regarded the horizon problem when he first heard of it, he was reluctant to reply because of the concern that he might be projecting his present views back to then. "I know what I think now, but you are asking me an historical question and I'm not quite sure."

Such cautions are quite well-taken, especially, on the issue of the scientists' recollections about their reactions before the development of the inflationary universe model, and to a lesser extent, the issue of whether their views changed with inflation. Nonetheless, I have two responses. First, although it is nearly impossible to evaluate the accuracy of individual recollections, one can make general assessments. On the whole, my sense is that many recollections are reasonably reliable. For example, many scientists associated hearing about the horizon problem from Charles Misner and hearing about the flatness problem either from Robert Dicke or one of his colleagues from Princeton. These recollection are quite consistent with the picture that emerged from examining the published literature. Second, and more important, my concern is not with the accuracy of any individual recollection, but rather with developing a general cultural map of the range of views represented within the community. And finally, much of this analysis will be based on scientists' expressions of their current views at the time of the interview where the problem of faulty memory does not arise.

Although the two problems currently appear similar, the interviews reveal subtle and significant differences in the ways each has been viewed. The views expressed about the horizon problem are fairly straightforward, but the interviews reveal a complexity associated with the flatness problem. In particular, although this is not apparent in the current literature, the legitimacy of the flatness problem remains somewhat controversial even today, while there is presently little to no controversy concerning the status of the horizon problem.

Views on the Horizon Problem

The horizon problem seems to have struck a chord, albeit a light one, with many scientists when they first heard of it. A few people recall mentioning it on occasion in talks or feeling a kind of vague worry about it. Several scientists expressed recollections

A few other interviewees expressed similar cautions.

similar to those of Chicago astrophysicist David Schramm, who recalls being bothered by it and thinking that it required some rather special initial conditions. Clearly, the existence of the problem was known within the community prior to the development of the inflation model. The puzzle was described in two influential textbooks, Weinberg's *Gravitation and Cosmology* and Misner, Thorne, and Wheeler's *Gravitation.* Most scientists interviewed recall having recognized or acknowledged it as a problem. A few puzzled over it; most felt that there was no way to address it at the moment and by and large classified it as a question of initial conditions and tended to ignore it.

Some scientists, such as British astrophysicist Martin Rees, recall feeling that it was a puzzle for the future, likely to be addressed with as-yet-undeveloped physics, such as a theory of quantum gravity. Rees clearly associated the problem with Misner's research program remarking that the "development which had certainly made everyone in Cambridge well aware of this problem was the so-called Misner program. Misner spent a whole academic year – 1967-68 I think it was – in Cambridge and lectured about this."[2] In Rees's memory, the problem was acknowledged by the community but was not a cause of much consternation. "I think that people felt that there may be some answer, but it was premature to seek it at that time. It was obvious that back at the Planck time, quantum gravity would come in and that would transcend the concept of a classical horizon anyway. So perhaps we'd have to wait for quantum gravity before getting any more convincing answer to why the initial conditions for classical cosmology were that way."

Rees's colleague, British mathematical physicist Roger Penrose, recalls remembering other people worrying about the horizon problem but for him, it wasn't especially compelling. "I remember other people worrying about it. My reaction was not being quite

[2]Alan Lightman and Roberta Brawer, *Origins: The Lives and Worlds of Modern Cosmologists* (Cambridge: Harvard University Press, 1990), 159. The following quotations in this chapter are from the complete interviews that are excerpted in *Origins* unless alternative sources are indicated.

sure what they were worrying about." As mentioned earlier, Penrose is cautious about his memory because his current views are clear and strongly held. For Penrose, the horizon problem is eclipsed by deeper problems about how the big bang and the initial singularity relate to the second law of thermodynamics and why the universe began with a relatively low state of entropy. "The horizon problem is a minor part of that problem. It's not the big problem. . . It's one part of the initial singularity and why the initial singularity is so special."

To another scientist, Caltech astronomer Wal Sargent, the problem was not a worrisome one because he felt uncertain about the legitimacy of extrapolating so far back in time. As Sargent explained, "I had a view, until quite late, that we didn't know enough to extrapolate the expansion of the universe very far back." After he began research on the microwave background himself, though, he recalls beginning to take the problem more seriously.

In contrast, Caltech astronomer Maarten Schmidt recalls that the problem "seemed indeed like a very fundamental problem to me, as soon as I heard of it... Somebody had to come up with a solution that I had not the faintest feeling for." Harvard astrophysicist Margaret Geller also recognizes it as problem that needs to be resolved. "But it's not the kind of problem that I'm equipped to answer as a scientist. So I essentially ignore it."

Thus, there were a variety of views. A few scientists, such as Misner and Peebles, took the problem to be a deep puzzle; others saw it as a minor problem, either considering it a problem for a future theory or an issue eclipsed by other deeper problems. Some questioned whether it even made sense to extrapolate back to such early times. Few, however, had strong feelings or views about the problem. In short, most cosmologists seem to have shared the view of particle theorist Steven Weinberg. Asked about whether he was bothered by the problem, he replied, "I suppose I took a Mr. Micawberish attitude: something will turn up. You know, Mr. Micawber is always saying something will turn up when he's in trouble.

You can only worry about so many things, and I have never devoted myself professionally to worrying about that. I took due notice in my book that there was such a problem."

Yet, although Weinberg did not worry about the problem, he found the solution of an very early period of extremely rapid inflation quite appealing. "I thought that was really very charming when I heard that explanation. That was what really sold me that something like inflation was probably true." Others echoed this assessment. Theorist Jim Peebles recalls that the question of how the universe got to be so homogeneous was something that had worried him a lot through the years, and to his mind "the inflationary idea certainly is a brilliant way to solve the problem." Once inflation was developed, nuclear astrophysicist David Schramm described how he would "always mention [the horizon problem] in talks, as opposed to just occasionally mentioning it, because there was a solution. You don't always show your warts, but you can always show your medals."

Marc Davis eloquently captures the sense not only of excitement but of *empowerment* generated by inflationary model's solution of the horizon problem. Davis states that he first heard of the problem in the early 1970s. At that time, he says, he gave it little thought, considering it an initial value problem. "I didn't have any basis for understanding how I could argue one way or another about initial values. It seemed a bit bizarre, but so be it. That's about as deep as I went on it." Then after inflation was invented, Davis states that he was amazed because the inflationary model promoted the horizon problem to a tractable problem.

> That sold me immediately. Here we had a real explanation for these things that we didn't think could be addressed at all. In a sense, when people ask about the role of God in science, scientists frequently answer that God is just over the horizon, or just beyond. He was pushed out of the solar system when Newton made celestial mechanics tractable with his inverse square law. He was pushed back to initial conditions when the big bang model came around, because we didn't need God once the initial conditions were set, but we sort of needed Him to set them up. But now inflation has pushed him back to the Planck time . . . That's what so impressive – when you can actually push

> back your ignorance to the point that where you can address a question that you didn't think was in the bounds of science at all.

Remarking that inflation has strong appeal, Davis also indicates that it is certainly not proven to be correct. "I think what people like is the *results* of inflation. They like what it does, that we have a dynamical result of these amazing observations. I think a lot of us would be happy to use whatever mechanism is shown to work to give these results. It doesn't have to be inflation."

Thus, Davis was sold on inflation because it provided an elegant dynamical explanation for the homogeneity of the universe throughout regions larger than those that could have equilibrated under the standard model. But he is not wedded to inflation. It is the fact that there is *an* explanation to this puzzle that most impressed Davis. Having such an explanation is, in Davis's words, as if God had been pushed back a little bit further and the abandoned territory had been captured by science.

Views on the Flatness Problem

For many scientists, the inflationary model was valued because it provided a way to address the horizon problem, which was seen as a real, although not pressing, problem requiring a physical explanation. Views on the flatness problem, though, reveal a great deal more complexity. In subtle and complicated ways, the flatness problem and the inflationary universe model have functioned to legitimate each together. The problem legitimates inflation in obvious ways. It has been repeatedly invoked as an unresolved flaw in the big bang model whose solution is an heroic achievement of the inflation model. Yet the inflation model has simultaneously legitimated the flatness problem in casting it as a soluble problem and in so doing moving it across and blurring the boundaries demarcating the metaphysical from the scientific.

It would be too simplistic, and indeed incorrect, to argue that the flatness problem was not acknowledged as a scientific problem before the inflationary solution was invented. As we will see, for some scientists, the problem was taken quite seriously before the advent of the inflation model. For others, such as described by Charles Misner, the development of a solution served to legitimate what was previously a dismissible conundrum. And for some the problem still is considered outside the boundaries of science. A key to beginning to disentangle these complicated dynamics is to recognize that the boundaries separating legitimate from illegitimate, metaphysical from scientific, are not clean and pure nor universally agreed on. They are not clearly marked borders, but are rather fuzzy zones that are continually shifting through a set of complex psychological and sociological dynamics involving both individuals and communities. In order to begin to get a sense of this, we look in a careful and detailed way at views of several individuals.

Charles Misner recalls first hearing about the flatness problem from Robert Dicke in the fall of 1969. He recollects spending a few hours mulling over it and then dismissing it. At the time, he felt that the energy of the universe was not a free parameter, but rather might somehow be constrained by the Einstein equations, a view he related to the three discrete possible values of k. He is very clear, though, about how his thinking then changed.

> I didn't come on board thinking that the [flatness] paradox was really serious until the inflationary models came out. . . It was not crucial that the inflationary universe be right. What was crucial was that the inflationary universe provided an example that turned the Dicke paradox into a standard physics problem. Here, by proposing a certain dynamics, you could solve that problem. Dynamics is an explanation to me, whereas a fiat that the universe starts out homogeneous [or flat] is not an explanation. . . It could be solved correctly or incorrectly, [but] once you have seen one example of an *attempt* to solve it, you could feel that this is real physics, and if that attempt doesn't work, some other one could. . . Then [after inflation], I took it seriously as a problem that needed a solution.

For Misner, the development of the inflationary universe model not only provided a solution to the flatness problem, it also validated the legitimacy of the problem itself. Indeed, for Misner, the advent of inflation changed the boundaries of scientific discourse by shifting the demarcation of scientific versus metaphysical questions.

This same theme that the existence of a solution changed the status of the problem is echoed by another scientist, astronomer Wal Sargent. Sargent recalls hearing of the flatness problem sometime in 1970s, but not considering it a serious concern. "I cannot give you a rational reason why. I was much more taken with the apparent homogeneity of the universe." After inflation, though, his view about the problem shifted, and he came to regard it as important and serious. "I suppose the fact that there was a potential explanation gave it more credibility. That's the only reason I can think of."

Reflecting on these remarks and the previous account of the published literature, it becomes tempting to argue that the flatness problem developed legitimacy as a scientific problem predominantly through the advent of a model offering a solution. Without doubt, inflation was crucial in establishing the status of the problem within the mainstream of cosmological discourse. Rarely mentioned before 1981, the problem now appears prominently throughout the literature. In that sense, the existence of a solution clearly elevated the perceived significance of the problem that, prior to inflation, was not of deep concern to most cosmologists.

Yet before making this argument, it is important to disentangle the various meanings associated with the notion of a scientific question gaining legitimacy. A psychological, a sociological, and a cultural lens each offer distinct angles on the issue. For instance, one might make a psychological argument based on a claim that individual scientists went through a process, akin to the one Misner describes, of initially dismissing the problem and then, by seeing a means of solving it, becoming convinced that indeed it was a serious problem. It is difficult, though, to distinguish this process from the psychological impact of the sociological phenomenon that, once a solution was posed, the flatness problem was frequently discussed and written up as both a motivation and achievement of the inflationary universe model. And finally, there is what might be called a more cultural form of legitimation by which a whole set of questions previously relegated to unaddressable initial conditions become accepted and part of reputable discourse.

In addition, it is also important to raise the question of legitimacy for whom? With the advent of inflation, a whole new set of practitioners, many from particle physics, were brought into the field. Most of these people first heard of the flatness problem with the inflationary solution. Thus, the problem had the imprimatur of legitimacy at first contact.

If one considers the community of researchers working in cosmology before the development of inflation, the interviews reveal two key points. First, in contrast to Charles Misner, several scientists, notably Steven Weinberg and Stephen Hawking, knew of the flatness problem long before 1981 and took the problem quite seriously before the inflationary model was proposed. Second, the legitimacy of the flatness problem as a scientific problem is still controversial among several researchers in the cosmology community. Thus, it would be misleading to take Misner's or Sargent's conversion as typical of cosmologists working in the field.

About the flatness problem, British theorist Stephen Hawking recounts that he "was aware of it about 1967, although it was not called the flatness problem until 1981. But I wrote a paper in 1971, on the isotropy of the universe, which referred to it. I think it was generally known long before inflation. . . At that time, the only explanation seemed to be the anthropic principle."

Elementary particle theorist Steven Weinberg was also well aware of the problem, and for him, it represented a robust argument that, in fact, the space curvature was zero or omega is one. In Weinberg's view, the inflationary universe model is of great importance, but not for its ability to solve the flatness problem. Asked if he would be surprised if the universe were absolutely flat, Weinberg responded that that seems to him the most plausible thing. The fact that omega is so close to one, he expanded, "never seemed to me that puzzling because it's perfectly well understood just in terms of the assumption that the universe is flat, k=0."

> Long before I ever heard of inflation, it always seemed to me that since omega was so close to one now, and since the universe is pretty old in terms of any fundamental time scale like the Planck scale, that probably means that omega was exactly one." He continued, "it didn't seem to me anything unnatural in saying that for reasons that we cannot now understand, the universe happens to have k=0. So I don't regard that as a triumph of inflation. I never have. But the other two [the horizon and monopole problems] are good enough.

Thus, Weinberg was aware of the flatness problem long before inflation. The problem was sufficiently compelling to him that it led him to believe that the mass density was exactly the critical density and space curvature was zero. Thus, for Weinberg, the issue was quite compelling. Indeed, when asked if his view of the flatness problem changed after inflation, he replied, "Well, yes, in the sense that it suddenly became plausible, as it hadn't been before, that maybe in fact k is 1 or -1 . . . whereas it would have been very implausible without the [effects of inflation]. So, in a way, it opened up these other possibilities." Astronomer Maarten Schmidt expressed similar feelings that the flatness problem was both compelling and a powerful argument for the view that omega was thus exactly one.

In stark contrast to Weinberg and Schmidt, Princeton-trained astrophysicist Margaret Geller believes such arguments are not only unpersuasive, but unscientific and objectionable. Indeed, for Geller, the flatness problem itself is not a scientific question, but an issue of philosophy. "I think it's a non-problem. We've got one universe, it has a density, and we're going to measure it. It's only one system. I think the flatness problem is really a non- issue.

My first reaction to that – and I'm probably a maverick – was, 'What are people talking about?'" Pressed about whether she felt that the nearness of omega to one at very early times requires an explanation, she replied, "No, I don't think it needs an explanation at all. It doesn't bother me one bit. It's the way the model works. The universe was denser in the past and omega gets closer to one."

> I guess this is a view that is not widely held, but it's always seemed to me like an argument of religion rather than an argument of science. I have never understood this argument as an argument of science. I've heard it many, many times. I can repeat it. But I just can't see if as a scientific argument. Because the universe is *one* realization. It's one system. So how can you talk about a priori probabilities. We have one system. This is the system we're in and we're going to measure it. And so, I don't understand it. My set of principles on which I operate to do science do not include that. I can understand it as a philosophical issue, but I can't understand it as an issue of science. . . It just happens that our cosmological model is such that if you go far back enough, you get arbitrarily close to a flat universe, so big deal.

Geller's Harvard colleague, John Huchra, expressed similar views. Both Huchra and Geller have something of a naturalist approach to science. They feel close to observations and both vehemently reject invoking the flatness problem as an argument that omega must be exactly one.

Huchra recounts that he heard of the flatness problem and inflation together. He relates the problem to the argument – an argument he is extremely skeptical of – that since the probability of omega being so close to one yet not one is extremely small, therefore it must be one. For Huchra, discussions of the flatness problem are intertwined with the solution –either by inflation or by the improbability argument – that omega is equal to one, and he fervently objects to the commitment he sees within the community to the belief that omega is equal to one. He states, "Any individual realization of a universe or a physical system or whatever, if there are lots of possible states for that physical system, has a probability that's very small . . . The universe is what it is. Just because we can't think of a good way to form galaxies doesn't mean that galaxies shouldn't be there. In the course of looking at the universe, I've seen a lot of awfully improbable things. Some are terribly, terribly improbable."

Two theorists, Stanford's Robert Wagoner and Berkeley's Joseph Silk, share Geller's unease about crediting the flatness problem as a scientific problem. Wagoner had heard of the flatness problem long before inflation. He described how he had developed a counter-argument, designed to illustrate how the flatness problem was not a true puzzle.

> You just take the time reversed argument with a star. I drop a star from any possible initial condition, with different energies, bound or unbound. . . Start a star collapsing and it's going to look like omega equals one when it reaches high density." Regardless of the initial energy, as the density gets high, omega will rapidly approach one. Wagoner offers another example. "Think of an oscillating universe even though we can't calculate at the 'bounce.' At maximum, it can have arbitrary energy. So it looks flat at high density. So what. It's very flat at high density. That's not a mystery. . . It's a consequence of dynamics, from an arbitrary initial condition.

As Wagoner is pushed about his arguments, his underlying view that such arguments are philosophical and not scientific comes out. "I don't think any of these arguments are relevant because I think they are philosophical. Let observation decide what omega is. . . I'm interested in finding out what the universe really is, by observation. That's what I'm interested in. That's my own motivation."

Joseph Silk also contests the legitimacy of the problem. He first heard of the flatness problem when he read Alan Guth's 1981 paper. He doesn't consider it compelling because, he comments, "in my view, it's all initial conditions – given the fact that we have only one universe. I see nothing whatsoever wrong with starting things off within an amount epsilon of being flat at the beginning so as to arrive today flat or almost flat. You may well have to make assumptions like that to get inflation itself to work. That is fine tuning. The flatness problem is just another version of fine tuning."

As Silk continues, he raises the very significant question of whether indeed the universe is flat. "I don't lose sleep at night worrying about the flatness problem. In fact, I personally think it's unlikely the universe is flat. One solution to the flatness problem, of course, is that the universe was born flat and always is flat. It may or may not be inflation that does that for us. The argument means nothing to me. In fact, I suspect that it could well be that the universe is open today. All the observations point to that course."

A very different position was articulated by University of Texas astronomer Gerard de Vaucouleurs, who had little interest in either the flatness or horizon problems. When asked if he considered the horizon problem as a serious problem, he replied indicating that such a question was outside the boundaries of his interest: "No. This did not interest me. I'm an observer of galaxies. My own interest in cosmology has been the concept of heterogeneity – the hierarchical structure of the universe – and, of course, the work on the Hubble constant." When asked if he put any stock in the flatness problem, he replied more forcefully: "No. I believe this is not the type of topic I am interested in because I don't know what it means. I don't know how a whole universe can be created out of nothing. . . So I really believe it is premature to deal with such large, enormous extrapolations. I'm not saying I favor some other cosmology. I say I don't know."

Ed Turner comments eloquently on the sociological dynamics interweaving the inflationary model and the flatness problem. When he first heard about inflation, he considered it extremely clever and a big surprise. "I was not expecting that someone would come up with such a clever idea about the early universe that would help so much with the causality problem and all these other things that one was aware of – these little uglinesses of the early big bang." But he also considered it very speculative and had expected that it would have been put aside, catalogued as a clever idea about the early universe, a possibility that might well have happened. "I would *not* have expected what did happen, which is that it became the foundation of a whole renaissance in the study of the early universe. The whole thrust of cosmology in the last ten years has in many ways been exploring the ramifications of that idea. I think I would have thought it was too speculative and hard to check for that to have happened."

Asked why he thought the inflationary universe model did catch on so dramatically, Turner replies:

> First of all, I think it just does have the property that if offers explanations for things that are otherwise hard to explain, and it is very clever. But the other reason I think is that it is a paradigm, if I may use that word, or a model which allows one to do lots of cute calculations. It's a sort of theorists' gymnasium, so to speak, where one can go and there's lots of nice problems to do and to be worked out. . . I think that does tend to happen when some new idea comes up that has lots of problems or aspects of it that can be worked out. Naturally people do. There's nothing wrong with that. But I think it often gives the *impression* that the theory is likely to be right, or that the field is more important than maybe it is. In other words, the theory of star formation is obviously incredibly important to astrophysics, but you don't really have theorists flocking on to that, just because there's not really all that many well-defined calculations and problems you can set yourself and solve.
>
> If you made a great pile of all the papers written in inflationary cosmology, it would be very impressive, and there's some kind of subconscious impression that forms in the community that this must be right because there's so much work on it or it must be in the right direction. Really, I think a more external rationalistic approach would say – "Well, what are the hard pieces of evidence that this actually occurred?" I wouldn't say there's any evidence against it, but there's not a whole lot for it. I still regard it as something that could easily be right or could easily be wrong. You shouldn't be surprised either way.

On the flatness problem, Turner is also eloquent.

> I probably heard about it when I was still at Caltech, probably from Jim Gunn. I think I thought of it as one of those things that's a little hard to know what to do with. It seems curious, but maybe it's just the way things are – like the sun and the moon being the same angular size in the sky, which they are incredibly precisely. That's something that just doesn't have any explanation – that seems like it must be an accident in our understanding of things and you're a little uncomfortable to see such a strange fluke, but since it's only one thing and there are many things you can notice in the world, you're not uncomfortable enough to really worry about it. The flatness problem didn't

bother me a lot. It was something that, if someone could come up with an explanation for it, great, but otherwise I don't think I would have lost any sleep over it. I thought of it as either something that didn't require an explanation, or, *if* there was going to be an explanation, it seemed to me off on the horizon. It was a problem for some future generation or something, something far away.

Having an explanation for a while, it now seems more important to have it explained. As a merely practical matter, I don't think that inflation is likely to be replaced as our best consensus, or best guess, at the early history of the universe *unless* it is replaced by another theory that can also explain [the flatness problem] in some way. I think that we won't give up inflation for some other theory unless the other theory can also cover that base.

One could imagine, although only barely, I think, that some experimental test of inflation would prove that the universe had never inflated. I can't quite imagine how that is. If you had some empirical evidence of that I guess you might be forced to go back to throwing up your hands. But I have a feeling that in a search for some replacement theory, this [the flatness problem] would be taken seriously. I don't really quite think that it has to be, in the sense that if one wanted to, [one could] go back to a kind of pre-inflationary idea where you blamed a lot of things on just the initial conditions. Initial conditions are really, in this context, a very special kind of idea or thing, and it's not very aesthetic to imagine that the universe had some [special initial conditions]. It's certainly much more pleasing to have some theory to explain it, but it doesn't strike me as intolerably ugly to just have to say, "Well, the fact that anything exists at all is so strange in a way, that maybe the initial conditions are just very special in some way." I don't know. As far as the practical evolution of the field, I think it will continue to play a very big role.

Complexities of Views about the Flatness Problem

Four basic views on the flatness problem have emerged. In the first view, the flatness problem is seen as indicating that something important is missing in the *physics* of the standard cosmological model. In this category, there is a spectrum of positions ranging from those who take the problem very seriously to those who consider it of moderate to light importance. Two leading proponents of the strong view, Robert Dicke and Alan Guth, hold that the flatness problem indicates that there is a significant shortcoming in the standard model. Several other scientists, including Sandra Faber, Don Page, Jim Gunn, and David Schramm hold a weaker version of this view, acknowledging the problem, but at least initially, not seeing it as especially compelling.

For other scientists, most notably Steven Weinberg and Maarten Schmidt, the problem was taken seriously, but it was not viewed as demanding an explanation involving new physics. Rather, the existence of the problem is taken as an argument that the universe is flat, that is, $k = 0$. This view elevates the problem to a significant status by considering it important in determining the empirical question of space curvature of the universe. With this view, the problem is obviated since omega is taken to be exactly one, and thus remains exactly one throughout the evolution of the universe.

A third view holds that the flatness problem is not a *scientific* problem at all. A key argument of this view, articulated by Margaret Geller, is the universe is *one* system, and it makes no sense to talk about a priori probabilities. Omega is a physical quantity characterizing the average mass density in the universe, it must have some value, and there is no need to evoke a special explanation of that value. As Robert Wagoner also points out, the equations describing our cosmological model simply have the property that as one extrapolates back to $t = 0$, omega becomes arbitrarily close to one. In addition, there is a strong position of this view, held by Gerard de Vaucouleurs, that we simply don't know enough to extrapolate back to tiny fractions of a second after the big bang with any degree of confidence.

A fourth position interprets the flatness problem in light of the anthropic principle. This view has particular strength among British cosmologists. The argument is put forward that life could only have evolved in a universe that is very close to flat. Were omega not extremely close to one at early times, the universe would either have recollapsed well before life could evolve, or the universe would have become so rarified early on that galaxies would never have formed, nor solar systems or planets to support life. Thus, the answer to the question why is omega so close to one today, becomes simply because we are here to ask the question. Often implicit in this perspective is a willingness to entertain the view that there exists an ensemble of many universes, each with differing values of omega. Thus, the value of omega is explained by a selection effect. Life comes into being only in those universes in which omega is very finely tuned to one.

What is most striking is that views on the flatness problem seem to relate to some very deep principles that guide the scientists, that tap into their approach to their subject and the questions they ask of nature. Nearly all the scientists that reject the legitimacy of the flatness problem see themselves as primarily interested with discovering what the universe is like. These include de Vaucouleurs, Geller, Huchra, Rubin, and Wagoner. These scientists express a certain humility about our efforts to understand the universe, seeing the universe as more complex than existing theoretical models can capture.

Those scientists who buy into the problem most strongly are generally comfortable with the belief that mathematical models capture the essence of what it means to understand the universe. When pressed to respond to arguments such as Geller's contesting the legitimacy of the flatness problem, Alan Guth replied that he would take a step back and say, "You cosmologists might just accept all kinds of things as givens, but us particle physicists, we believe deep down that everything is calculable."[3] Along these lines, some theorists find themselves gripped by the question explaining the origin of the universe without having to postulate initial conditions that lie outside of the laws and hence the explanatory power of science. A passage written by Stephen Hawking in 1989, with the heading "The problem of initial conditions," illustrates a sense of such desire:

> The real problem with spacetime having an edge or boundary at a singularity is that the laws of science do not determine the initial state of the universe at the singularity but only how the universe evolves thereafter... In order to pick out one particular state of the universe from among the set of all possible states that are allowed by the laws, one has to supplement the laws by boundary conditions which say what the state of the universe was at an initial singularity or in the infinite past. Many scientists are

[3]Interview with Alan Guth on November 7, 1988. See Appendix, p. 80.

> embarassed at talking about the boundary conditions of the universe because they feel it verges on metaphysics or religion. After all, they might say, the universe could have started off in a completely arbitrary state. That may be so, but in that case it could also have evolved in a completely arbitrary manner. Yet all the evidence that we have suggests it evolves in a well-determined way according to certain laws. It is therefore not unreasonable to suppose that they may also be simple laws that govern the boundary conditions and determine the state of the universe.[4]

To a large extent, these groups break down between theorists and observers, but not entirely. The stronger correlation is between how the scientists relates to nature and how they relate to the subject of cosmology. Is the universe a system *sui generis*, one system of which we are a part, and of which we wish to discover what is it like? Or does the universe become a object of contemplation, an entity which can be manipulated in thought, and for which many universes or many systems are possible?

Pulling at the flatness problem begins to unravel the threads of an underlying diversity of views in the very notion of what it means to ask a scientific question and to answer it. The flatness problem doesn't separate off cleanly. It is bound in multiple ways to the questions of what it means to understand the universe, via its connections with the value of omega, the hypothesized dark matter, and the inflation model. It is a submerged site of contestation. As the problem moved into mainstream discourse, it offered an opening for changing the nature of the questions asked. Questions about the initial conditions of the universe had now gained the stamp of legitimacy in scientific discourse.

[4]Stephen Hawking, "The edge of spacetime." In *The New Physics*, edited by Paul Davies. (Cambridge: Cambridge University Press, 1989),68.

Chapter Four: Conclusions and Discussion

In Steven Weinberg's popular book, *The First Three Minutes*, he muses on the serendipitous nature of the discovery of the microwave background radiation and speculates about why the big bang theory had not led to a systematic search for this radiation well before 1965. Weinberg considered several reasons, but the one he took to be most important was, in his words, that "it was extraordinarily difficult for physicists to take seriously *any* theory of the early universe."[1] What the discovery of the microwave background accomplished, in Weinberg's eyes, "was to force us all to take seriously the idea that there *was* an early universe."[2]

As Weinberg points out, in the big bang model physicists are dealing with circumstances so remote in time and so extreme in terms of density and temperature that it is hard not to feel a sense of discomfort about whether "the numbers we play with at our desk have something to do with the real world." Even worse, in Weinberg's eyes, was the problem of legitimacy: "there often seems to be a general agreement that certain phenomena are just not fit subjects for respectable theoretical and experimental effort."[3]

The difficulties Weinberg raised about taking the big bang theory seriously would seem to loom even larger in the case of the inflationary universe model. The inflationary universe model has no empirical validation such as the microwave background to vaunt. Yet, while it is certainly not universally accepted, the model suffered surprisingly few problems in being taken seriously among the cosmology community. The reasons for this are not self-evident. The model has few testable consequences, and it is not easily falsifiable. It is dealing with the fantastically remote times of 10^{-35} to 10^{-33} seconds after the hypothesized big bang. It invokes grand unified theories, theories of elementary particles that themselves

[1]Steven Weinberg, *The First Three Minutes* (New York: Basic Books, 1977), 131.
[2]Ibid., 132.
[3]Ibid., 132.

lack empirical validation. And its chief quantitative prediction is a precise value for the mass density of the universe, a value in which the mass density and expansion rate of the universe are critically balanced. While existing constraints do not rule out the model's prediction that omega is equal to one, present measurements do not support it.

When asked why the inflationary model caught on so widely, many cosmologists answer along the lines of Sandra Faber: "Because it solves these three or four classic problems of conventional cosmology. The horizon problem is one, the flatness problem is another. Those really speak to me." The attractiveness of the inflationary universe model rests strongly its explanatory effect in resolving puzzles about initial conditions rather than on new predictions or tests of its empirical consequences. The model also gives structure to the very early universe, a previously structureless domain. The lack of overlap between the theories is confined to a duration of about 10^{-32}. As theorist David Schramm remarks, "You don't have to throw anything away. Inflation solves some of the problems, some of the things that used to be nagging embarassments that you put under the rug. Now you don't have to sweep them under the rug. So it enabled you as a big bang lover to be even more of a big bang lover." Of course, though, some cosmologists do feel more than a nagging embarassment about inflation's prediction that omega is almost exactly one, which is contrary to most observational evidence.

One of the most provocative and detailed models of the relationship of questions and explanation in the scientific process has been developed by Thomas Kuhn.[4] A central tenet of Kuhn's argument is his characterization of much scientific work as puzzle-solving rather than problem-solving. Puzzle-solving, which constitutes much of what Kuhn called normal science, premises current theory and attempts to resolve a question or empirical anomaly. Kuhn characterizes a puzzle-solving tradition as one in which the practitioners of the field

[4] *Kuhn, Structure of Scientific Revolutions.*

expect a solution to be found and share criteria such that they will concur in their judgment of the validity of a solution.[5] In the puzzle-solving tradition, when a solution eludes a researcher, "Only the practitioner is blamed, not his tools."[6]

With the inflationary universe model, the flatness problem was transformed from a problem which was simply not addressable into a puzzle. To the extent that the flatness problem had even been recognized, failure to solve it was most definitely considered a failure of tools.[7] Most commonly, the problem was simply not acknowledged. But the current constitution of the flatness problem as a puzzle was a result of the development of the inflationary model. It was not a motivating force for the invention of inflation.

In December, 1980 when Guth was developing his inflation model, he was not trying to solve either the flatness or horizon problems. Indeed at that time, he knew nothing of the horizon problem and had never quantitatively calculated the flatness problem. Identified firmly as a theoretical particle physicist and not a cosmologist, Guth had been persuaded by his colleague Henry Tye to examine the implications of the recently invented grand unified theories for the rate of production of magnetic monopoles in the early universe. Grand unified theories, extremely attractive to particle physicists, predicted the existence of far more magnetic monopoles than experimental observation would allow. Following up the fine points of a calculation, Guth realized that the application of such theories to the early universe resulted in a phase transition which affected the development of the scale factor at extremely early times. Rather than a simple power law expansion of the scale factor from the hypothesized time of the singularity, at the first tiny fractions of a second, the expansion of the size of the universe was exponential. Guth immediately realized that his model would

[5]Thomas S. Kuhn, "Logic of Discovery or Psychology of Research?" In *Criticism and the Growth of Knowledge*, edited by Imre Lakatos and Alan Musgrave (Cambridge: Cambridge University Press, 1970), 7.
[6]Ibid., 7.
[7]As Dicke and Peebles put it in their "Enigmas and Nostrums" paper, "this problem is trying to tell us something, but what is it?"

solve the flatness problem, which he had heard posed in a talk a year earlier. A few weeks later, he learned of the horizon problem, and quickly realized the model could also address that problem.

In Kuhn's nomenclature, Guth and Tye's efforts to solve the monopole problem was part of a puzzle-solving tradition. Grand unified theories were premised, the question was raised about the extent of monopole production in these theories, and the theoretical tools existed to investigate and answer the question. When it appeared the monopole production was massively greater than current empirical constraints would allow, Guth and Tye postulated mechanisms such as a super-cooling period to try to account for the discrepancy. As Guth emphasizes, when he developed the inflationary universe model, he was *not* trying to solve the flatness problem nor account for the origin of the universe. Those problems could not support a puzzle-solving tradition.

Guth's model was taken quite seriously and generated a great deal of excitement among particle physicists. More significant, though, Guth's model formed a bridge between high energy particle theory and more traditional astrophysically-based cosmology. Two key supports of that bridge were two problems that few particle physicists knew of before Guth began presenting his model: the flatness and horizon problems.

Indeed, as I have argued in this study, these two questions, and especially the flatness problem, had a marginal status even among traditional cosmologists. Although many cosmologists had heard the problems posed, few thought that they were matters for which tools existed by which to conduct inquiry. As a consequence of this lack of tools and the fact that the issues concerned matters as speculative as the initial state of the universe, the questions touched the borders between what was considered scientific and that which was ruled out of science, labelled often as philosophy.

Yet when linked together, the inflationary universe model and these questions had rhetorical force that each lacked independently. Without these two questions, there would be little to counter tendencies to dismiss inflation as mathematical speculation with little connection to the actual universe. The monopole problem could easily be taken as a problem with grand unified theories, rather than a problem about the physical universe. In fact, the Russian particle theorist Andrei Linde reports that three years before Guth's paper appeared, he was studying similar problems with a colleague at the Lebedev Institute.

> We understood that the universe could be exponentially expanding and bubbles would be colliding, and we saw that it would lead to great inhomogeneities in the universe. As a result, [we thought] this is bad. What is the reason to publish such garbage. But at that moment, we didn't realize the main advantages of this [exponential expansion], that it could possibly solve the flatness and horizon problems.

Unaware of the horizon and flatness problem, Linde reports that he dismissed the inflationary process as "garbage." Whether or not Linde's account is considered accurate, without these two questions, the inflationary universe model was untethered. And conversely, without the inflationary model, the flatness and horizon problems were easily kept at the margins of scientific discourse. Yet together, each – question and explanation – served to mutually legitimate each other. A new web of belief about the early universe was laid down whole, question and explanation both coming into prominence together.

How are we to understand this process? One view, expressed by Lightman and Gingerich, is that this process constitutes "retrorecognition." The flatness problem was overlooked and ignored until there was a solution that made it safe to recognize it for the significant shortcoming in the big bang model that it really was. On this view, though, it is puzzling why the horizon problem was more readily recognized as a puzzle before the invention of inflation and why the flatness problem remained controversial among many scientists well after the development of inflation.

I suggest that a better way to understand the cognitive and social dynamics of the inflationary model of the early universe and the horizon and flatness problems is a process of co-constitution. Theory and questions together were co-constituted as objects of scientific discourse. In this process, cognitive borders – especially the borders marking off the domains of the scientific versus the metaphysical – were restructured.

What is especially apparent in this story is that cosmologists are very reluctant to engage in debate about the location of these borders. Just as discussion of the flatness problem was rare prior to the invention of inflation, critique of its legitimacy as a scientific issue is rare today. To my knowledge, no scientist has written a critique of the flatness problem expressing the skepticism that is evident in discussions in the community.

I contend that there is no answer to the question of whether the flatness problem is a true scientific puzzle or a metaphysical conundrum. It is a matter decided by practices of a community, not by logical argument. With the development of the inflation model, the flatness problem became embedded within a set of theoretical craft practices that legitimated it. For some, once there is a solution, the problem was constituted as scientific. For others, once there was a solution, the question of whether it is a scientific puzzle or not became moot.

Thus, in considering what distinguishes a metaphysical question from a scientific one, one might hypothesize that a question becomes a scientific one when what would count as an answer begins to become apparent. The actual solution itself need not be known or determined, but for a question to be considered legitimate, it is required that *some* solution appear possible. If there is no realm of of investigation, which at least in principle has the potential of solving the problem, then the problem is excluded from scientific discourse.

When it was initially posed, the horizon problem fell into the category of Kuhn's notion of a puzzle. It was posed along with a theoretical model attempting to solve it. After it was shown, that the model was not tenable, the horizon problem became something of an orphan, grudgingly acknowledged but no longer connected with a legitimating explanation.

What is particularly interesting in the case of the flatness problem is the transition to legitimacy involved only the development of a new cosmological model that explained it. Nothing about the question *qua* question changed in 1981. No new empirical observations significantly changed the constraints on omega; no new theoretical developments put the *question* into a new light. Basically a question that was by and large dismissed by the community suddenly became embraced by the community. Indeed, not only did the solution validate the question, the very credibility of the inflationary model itself rested heavily on its ability to solve the flatness problem along with the horizon problem.

Inflation showed that the problems are tractable, that is, they are suspectible to a physical and dynamical explanation. In doing so, it elevated the status of the problems to legitimate and important scientific questions. As this happened initially, the problems were strongly coupled to the inflationary model. However, eventually a kind of decoupling seemed to occur, and the problems took on an independent status of their own. They became problems that need to be addressed and inflation is one possible solution.

What is especially striking is that many cosmologists are very conscious of this process and of the socio-psychological dynamics between questions and explanation. For instance, James Peebles recently wrote, "It is notable, though, and perhaps significant that in the decade since the [inflation] concept was discovered and the homogeneity problem made very visible, nobody has proposed a reasonably definite alternative resolution to the puzzle. Unless or until that happens, or the concept somehow can be shown to be untenable, we must expect that inflation will continue to occupy a central place in the exploration of concepts in theoretical cosmology."[8] In this assessment, Peebles points to two aspects of the process of co-constitution. He notes both the role of the inflationary model in making the horizon problem very visible, and simultaneously, that inflation is unlikely to be abandoned without an alternative explanation of the problem.

In sum, this study has touched on a number of ways that question and explanation interact in the scientific process. Clearly, issues of legitimacy are intimately tied to social processes, to the construction of cognitive boundaries, to the tools practitioners possess, and to the communities and lifeworlds of practice they work within.

[8]P.J.E. Peebles, *Principles of Physical Cosmology*, (Princeton: Princeton University Press, 1993), 393.

Appendix

Interview with Alan Guth by Roberta Brawer[1]

Brawer: I'd like to clarify some background on the flatness problem and the role that the flatness problem played in the development of the inflationary universe picture. I recall that you heard the talk by Robert Dicke in the fall of 1978.

Guth: Right. I've now got that date straight.

Brawer: Did you think about the flatness problem or did you discuss it with anyone between that time and the time that you were working on what developed into the inflationary universe model?

Guth: I can't say completely for sure, but my guess is that I spoke to a few people about it on the day of the talk, asking "Did you go to the talk yesterday? What did you think about it?" In fact, I remember talking to Ken Wilson about it and being puzzled by his response. He kind of shrugged his shoulders and said, "Well, of course, there's this big divergence in the time scales between one second after the big bang and today. So that explains it." I didn't really know what he meant by that, whether he meant that it explains it in the sense that it demystifies it or explains why the calculation comes out the way it does. But anyway, I don't think that after that, I even thought about it for quite a long time, including the time when I was working on cosmology, that was not a concern of mine. I really was getting into cosmology from the back door. I was worried about magnetic monopoles. During the time when I got back into cosmology, somewhere I realized that the flatness problem did amount to a *measure* of RT the scale factor times temperature, which is a quantity which is roughly speaking invariant as the universe evolves. R goes up, T goes down. Knowing that piece of information allowed me when I first saw the expontential expansion to realize very quickly that that would solve the flatness problem. In the end, the temperature after the phase transition would be roughly the same as the temperature before the phase transition began, but R would have increased by this huge factor and that's the way I was looking at it at that time. It was very easy once I knew that RT was the problem. But it certainly was not something I was concerned about. I was really not thinking about trying to invent a new cosmological scenario. I was really just worrying about the magnetic monopoles.

Brawer: Yes, you described in the interview with Alan that you had done work to try to suppress the production of monopoles and that you [and Henry Tye] had come up with the idea of a first-order phase transition and a supercooling period. You mention that [your collaborator] Henry Tye remarked that you were assuming in that work that the scale factor didn't change. When you developed inflation, were you just investigating whether that assumption was valid?

[1]Interview conducted in Cambridge, Massachusetts on November 7, 1988.

Guth: That's right. That's right. When I discovered the exponential expansion, I was just trying to check our previous assumption that the behavior of the scale factor was not affected much by the phase transition.

Brawer: This may be hard to remember because I'm asking very specific questions, but when you saw the exponential expansion of the scale factor, did it then click with you right away that this may have physical meaning because of the flatness problem, or was there some period of time in between?

Guth: It was all in one evening. It was very quick. Whether it was all in five minutes or an hour, I don't remember. It was all in one evening. I discovered that when you look at the equation for evolution, it was obvious it was an exponential and, as I said, somewhere along the line, I had realized that the flatness problem amounted to the value of RT and with that in mind, I immediately realized that this [inflation] would change that [value]. I think it was actually the next day that I went back and calculated the flatness problem in detail. I had never really done that up until that point.

Brawer: I was going to ask you about that. So you hadn't calculated it in detail but you had seen that the issue amounted to the factor of RT. You had formulated that way of looking at it?

Guth: Right.

Brawer: Do you remember when you formulated it that way?

Guth: No, I don't. I would guess that sometime or other in the fall of 1979 when this work was nearing completion, but I hadn't yet invented inflation. My guess is that at some point I was just trying to plug in numbers to the basic equations of cosmology to see how things work. And the numerical value of RT is a useful number – [it's useful] to express things in certain formulas in terms of that fixed quantity. My guess is that at that point I calculated it. What you realize when you calculate it empirically is that the empirical value is very, very large. It's a dimensionless number. It turns out to be 10^{87} if I remember it right. When you come up with a dimensionless number like that, it startles you and at some point you realize that its the same question that Dicke was talking about.

Brawer: That's very interesting. Do you know whether you were the person who coined the name for this problem as the flatness problem?

Guth: Gee. I don't know. I really don't know. I don't remember if Dicke used it. I never looked back to see if it's used by that name in the Einstein Centenary volume. Have you checked that?

Brawer: I don't think it is.

Guth: I guess I don't know who coined the name. I certainly don't claim any credit for it, but I also don't know where I got it from.

Brawer: Speaking about names, do you remember why you chose the name inflation?

Guth: To be honest, I don't remember exactly when I started calling it inflation. I think the reason was just the obvious one, that you had this dramatic period of expansion which [makes] the model different from other models. So inflation seemed like a natural name, but I don't remember exactly when I came up with it.

Brawer: I'm interested in the various reactions that people had to the flatness problem when you first started presenting the inflation model. In the interview with Alan, you mention that you gave a talk at SLAC...

Guth: Yes, the first talk I gave about it was at SLAC and I'm pretty sure that was at the end of December. Maybe it was in January, I'm not completely positive.

Brawer: So it was very soon.

Guth: It was very soon. It was either December or January.

Brawer: Do you remember how you motivated your presentation? Did you talk about the monopole, flatness and horizon problems?

Guth: Yes, but not with equal emphasis. My recollection is that when I started giving talks about this – for maybe the first year or two – I more or less presented it in the same sequence that we actually did it. That is, the monopole problem came first and three-quarters of the talk was about the monopole problem and at the end, I said, "By the way, this same scenario also solves these other problems in cosmology – the horizon and flatness problems." I think that's how I organized it.

Brawer: That's interesting because at the time that you wrote your paper, in the title you emphasize the horizon and flatness problems.

Guth: Yes. I think the reason for the switch is that the late spring of 1980 was about the time that I realized that the model didn't quite work – this problem of the inhomogeneities caused by the bubble nucleation – the problem which was later solved by new inflation. And the solution to the monopole problem hinges pretty crucially on the same issues – how the Higgs field finally develops more or less uniform expectation value, in other words, how the phase transition ends. So at the time I wrote that paper, I realized that the solution to the monopole problem was still somewhat questionable. It was the question that was most closely related to the aspect of the model that failed, while with the horizon and flatness problems, you still needed to somehow have some kind of a smooth phase transition to successful eliminate those problems, but still the problems were eliminated during the inflationary period. You just had to find some phase transition that would not recreate them. While the whole monopole issue is really the issue of how the phase transition takes place and I realized that that was on pretty shaky ground by the time I wrote the paper.

Brawer: I see. Do you remember what kind of reaction you got to the flatness problem as a scientific problem when you first starting presenting inflation? I imagine that may be different depending upon whether you were speaking to a particle physics or a more astrophysics cosmology audience.

Guth: That's right. It was very different. What I recollect was that to a particle physics audience, there was *no* problem convincing them that this was a serious problem with the standard model. It was a very elegant feature of inflation that it eliminated this problem. To astrophysicists, it was much thornier. They had lived with this situation for a long time and most of them were aware that the early universe was incredibly close to flat. And that, for the most part, was never thought of as a problem. It was a fact that people who worked on the early universe knew because they had plugged in the numbers and they learned it kind of the other way around. They learned that you could always describe the early universe with very simple equations because you could make the very accurate approximation that it was nearly flat. As a useful approximation, this was well known to everybody. So with that mind-set, it was hard to convince – and many of them are still not convinced – that the flatness problem is really a problem, that it really indicates that there is some shortcoming to the standard model.

Brawer: However, to the particle physics community...

Guth: To the particle physics community, they were ready to accept it as a problem. It was very similar in character to another problem in particle physics that was being talked about a lot at that time, which is called the hierarchy problem of grand unified theories. The problem I'm talking about in grand unified theories is just the question of understanding the large difference in the mass scales between the fundamental scale of the grand unified theory at 10^{14} GeV and the next most important scale below that which if the weak interaction scale of a few hundred GeV. There's a factor of 10^{12} there in energy scales and, as used, that was just a parameter that you put into the model. But particle physicists were all very much aware that that was an embarassment to have a dimensionless number of 10^{12} sitting around as a parameter in your theory. People have always felt – and still feel – that if that's true, there should be some way to explain it. So the flatness problem was very similar. There's this large dimensionless number that you needed to describe cosmology – basically the number of photons in the universe – and dimensionless numbers should be, to a particle theorist, of order one unless there's some good reason for them to be very different. So to the particle theory community, it was quite natural to believe that whenever you see a large dimensionless number, there's something that underlies that. But it's a prejudice that was very strong in the particle physics community and to other physicists, it didn't seem nearly so obvious.

Brawer: Do you think that many of the particle physicists had worked in cosmology and were aware, as you say the astrophysicists were, that the early universe could be considered flat? When you presented the problem to them, do you think that was the first time they were hearing of it?

Guth: The flatness problem?

Brawer: Yes.

Guth: I think that for many of them it was. Probably for most. I'm not sure, but I think so. Dicke and Peebles had been speaking about it. The flatness problem originated with them. I did not notice it or invent it. But I don't think too many of the particle theory community had been in touch with the talks or writing of Dicke and Peebles. As far as know, nobody else was making any noise about the flatness problem.

Brawer: Yes, I was going to ask you about this later, but in trying to trace the literature about it, there seems to be very little. Dicke did give a series of talks in 1969 in Philadelphia in which he has a paragraph mentioning it in passing. There doesn't seem to be any other published literature about it. And yet it seems to me that there is nothing about the problem which would have prevented it from being posed back in the 1930's.

Guth: Absolutely.

Brawer: So there is a period of almost 40 years before it is even posed by Dicke initially in his lecture in 1969 and then another 10 years until it was discussed more fully in the Einstein Centenary volume. Do have any thoughts about why the problem emerged so slowly?

Guth: First of all, there is a little bit more history to it than we've mentioned so far – or least, so I gather. I received a letter from John Barrow. I should try to find it and give it to you. I don't know if you know John Barrow. He's an astrophysicist and general relativist at University of Sussex. He claimed that there was a whole long history to this problem that I had always ignored and he gave me some references, some of which I was able to track down and some of which I was not able to track down. Some of them were published in very obscure journals. Among other items, I believe that he referred to some work by Dirac. Dirac had noticed this very large ratio, basically RT or something equivalent to it. And if I remember it right, Dirac used it not in the same way that Dicke was using it, but Dirac used it as an argument for a completely flat universe. The idea was if you have this number that's very large – I don't know if he was using the number or the inverse – but if you have this number that's very large, then it should be infinite, if it's that large. Or talking about the inverse, anything that's that small is probably zero. Now I think that's somewhat different from Dicke's point of view, which was that if this number is very large, you should find some mechanism to make it large. But nonetheless, it is at least making the same observation and trying to draw some kind of a consequence from it.

Brawer: I would be very interested in that.

Guth: I'll try to track down this letter. I have it in my files someplace. I'll try to find it and send you a copy.

Brawer: Thank you. I believe that Dirac used that kind of an argument in another context.

Guth: That's right. It's similar to famous Dirac argument that the gravitational constant should be changing.

Brawer: How do you respond to that more well-known argument using the large numbers...

Guth: Well, of course, I think everyone now believes that that argument was wrong. At least, it reached the wrong conclusion. The bounds on the rate of change of the gravitational constant I believe now exclude the Dirac hypothesis. I think it was a pretty weak argument because you have these very large numbers but you only knew their exponents, so the coincidence that he was talking about, was only a coincidence that could be verified to an accuracy of 5 or 10% – the accuracy to which you know that the exponent of the gravitational constant over the proton mass squared times some [factors of] h and c is the same as the age of the universe. That's not a highly accurate statement – I think something on the order of a 10% statement – it's 10% of the exponent. It's interesting to pursue ideas like that. Certainly I don't think that Dirac is any kind of a fool for trying it. But I think an argument like that doesn't sound very compelling. And it did turn out not to be correct. And as I mentioned on the question of flatness itself, my understanding is that Dirac drew the conclusion that that meant that the universe had to be flat. Actually, there's some validity to that kind of reasoning. It's similar to the kind of reasoning that a lot of us use today to convince ourselves that the cosmological constant is probably zero. But it's still short of trying to come up with a dynamical reason for it – which for the cosmological constant is also an important question and maybe Coleman has done it.

Brawer: I think that other people had a similar reaction as Dirac. I'm thinking back to the interview with Steve Weinberg. He said he had a similar reaction to the flatness problem. It meant for him that the universe was such that $k = 0$...

Guth: The universe was flat and that's that. That's right. In fact, I've spoken with Steve about it and Steve never seemed to be very impressed with the flatness problem as an indication that there was anything wrong with the standard cosmology. He was willing to just accept $k = 0$ and that's that.

Brawer: In your mind, do you think that saying $k = 0$ is an acceptable solution to the flatness problem?

Guth: No, I don't. For a reason that I've spoken to Steve about and he always seems semi-convinced temporarily but then he always goes back. But the reason is something which is pointed out rather clearly in the Dicke and Peebles article in the Einstein Centenary volume. That is, it's not just a question of fixing one number. The universe is not a Robertson-Walker universe exactly. There are fluctuations in the density and there are deviations from the Hubble flow. In order to solve the flatness problem, it's not enough just to set the mean value of the expansion rate equal to what it's supposed to be given the mean value of the mass density. Both of these are really local quantities. So you can imagine describing each by a Fourier series. The first term is the mean value but then there are all these fluctuations, which are *there*. The flatness problem says much more than that the mean value of the mass density has to be finely tuned. In fact, all of the Fourier components of the density fluctuations have to be extraordinarily small in the early universe in order for the universe to evolve the way it did. So you have a very large number of constants that have to be fixed at unusually small values and it's hard to see any way of understanding that except by having some kind of a mechanism that does it.

Brawer: Now let me see if I understand that. It seems to me that that formulation is bringing in the question of homogeneity.

Guth: Yes, that's right. I'm now mixing the two up in some way.

Brawer: When one speaks about the flatness problem in terms of the *mean* value, it seems to me to be independent of the causality, or horizon, problem. Would you agree with that?

Guth: That's right. What I'm saying is, I think, still partially independent, although I agree that once you start talking about spatial variations, you could no longer be completely independent. But the point is that it's one problem to try to understand how what's going on at this end of the universe can even know what's going on way at the other end of the universe, when they are separated by distances that are something like a hundred times the horizon distance, which is the situation for the release of the cosmic background radiation. So there's that qualitative question of how do the different regions know about each other. But in addition to that, then there's the quantitative questions of just what is the amplitude of what inhomogeneities there are. And there *are* inhomogeneities. There is no principle which says that the universe is completely homogenous. That's not the case. There wouldn't be galaxies if that were the case. So given that there are inhomogeneities, you have the question of trying to understand the magnitude of those inhomogeneities. And you can describe those inhomogeneities as a local deviation from flatness. In some places the mass density is a little bit more than it should be according to the expansion rate. In other places, the mass density is a little bit less. What I'm saying is that if you think about it in those terms – and this was the way Dicke and Peebles were thinking about it – then it's not just a question of on the *average* having a mass density of the universe equal to the right value. You really have to tune it locally, or if you think of doing it as a Fourier expansion, there's a huge number of Fourier components, all of which have to be tuned to be enormously small in the early universe.

Brawer: Yes, but isn't that equivalent to saying that the inhomogeneities on every scale have to be small?

Guth: Yes, I think it is. Of course, the way I'm saying it is that the deviation from flatness on every scale has to be small. Certainly, it does involve the mixing of the two problems. But the mixing is, at least somewhat I think, conceptually distinct. In the case of the horizon problem, the aspect of the universe that seems amazing is the scale over which things appear to be homogeneous. In the case of this elaboration of the flatness problem, it's not the length that's amazing, it's the amplitude that's amazing.

Brawer: I see. However, if the amplitudes were not small, then there wouldn't be any meaning . . .

Guth: To homogeneity. That's true. That's true. So the two problems are merged, I have to admit.

Brawer: Okay. I know I'm often asking you very similar questions in slighty different ways to get as much of your thinking on this as I can. It appears that the homogeneity problem, or the horizon problem, was thought to be quite compelling within the astrophysics community whereas the flatness problem was not. Do you have any thoughts about why the first problem was seen as compelling and the second not, other than what you've already mentioned?

Guth: I'm not sure that I do have any good explanations for it outside of what I already said. It seems to me that as far as my understanding of the history goes, it's almost accidental that the horizon problem was spoken about as a problem while the flatness problem was spoken about as a problem until very late, long after people were already aware of the phenomenon. And then it was, I think, just psychologically hard for people to come to believe that a fact that they were aware of for ten years is now suddenly a problem.

Brawer: I think that's a very good point. However, you could imagine something similar having happened with the horizon problem because the cosmological principle was invoked in the very beginning of modern cosmology. It was assumed that the universe was homogeneous as just a way of getting off the ground [in doing calulations in the theory]. And then somehow, instead of it being an assumption, the question then became, "Well, how did it become so homogeneous?"

Guth: Right.

Brawer: So you had a similar kind of, I would imagine, shift, even with the causality problem.

Guth: Yes, I think that's right. I'm not sure what to say. I'm also not sure, by the way, how accurate it is to say that the horizon problem was considered to be an important problem by everybody. It is certainly mentioned in several places at least, but I practically only know about several places that mention it. One is Steve Weinberg's book which doesn't really count as a viewpoint of an astrophyscist since he's a particle theorist. It is, I believe, mentioned in Misner, Thorne and Wheeler which is certainly a legitimate [example of] an astrophysicist point of view. But outside of those places, I don't know where it was mentioned.

Brawer: Well, I haven't looked at this thoroughly yet, but I think shortly after the discovery of the microwave background, it was formulated, probably by Misner, who was working on his mixmaster models.

Guth: That's true. The mixmaster model was certainly motivated by the horizon problem.

Brawer: And people in the community recall there being discussions about it. As far as we can tell from the interviews, there seems to be a great deal recollection about the horizon problem. So based on anecdotes, at least, the community seems to have recognized that as a real problem. Though, there was always the question of how do you address it. To a certain extent, if people don't have a way of addressing a problem, they're less willing to say it's a problem.

Guth: That's correct.

Brawer: But, perhaps since there was this work with the mixmaster model, maybe that had a effect [in helping to validate the problem.] It also seems that the causality problem was formulated right after the microwave background was discovered. That seems to be the point in which there was a transition from just assuming the cosmological principle to then raising the quantitative question of how did the homogeneity come to be.

Guth: Yes, that makes sense. [Tape is turned over. Guth mentions Rindler's paper discussing horizons.] At that time, there were very flexible kinds of cosmologies that were being considered. So he talked about what properties led to horizons and what kinds don't lead to horizons, but there was no particular model that was examined. He never spoke of the horizons as really a big problem, just as something interesting to calculate.

Brawer: Yes, I looked at that paper and it seemed that he was trying to clarify matters like the difference between a particle horizon and an event horizon.

Guth: That's right.

Brawer: Let me go back and ask you about a position that some people take on the flatness problem. We've discovered in our interviews that there are some people who even now will say that there is no flatness problem.

Guth: Oh yes, there are people who still say that. Actually, I had something more I wanted to say about that too, but go ahead, ask me your question.

Brawer: Please go ahead.

Guth: OK. Concerning my answer to Steve Weinberg's point of view, that maybe it just sort of a law of nature that k should equal zero. I think another aspect of my objection to that point of view is related to my notion that the laws of physics should be well-defined and simple. So if you're saying that there is a law of physics that $k = 0$, the statement that the universe is $k = 0$ should be a well-defined statement. And it really isn't. Even if inflation is right or if it's not right, k is only a parameter of the Robertson-Walker model of the universe. There's not really any quantity that you can measure in the real universe that's precisely the Robertson-Walker k, because of the fact the our universe is not Robertson-Walker *exactly*. It really has perturbations in the Hubble flow and perturbations in the mass density. So if you tried to formulate a fundamental law that $k = 0$, it would have to be a very crazy sounding kind of law. It can't really be about anything that you can measure in a finite volume. If you measured the average value of the Hubble constant at the same time for a volume of space, then [it's not the same] as you let that volume go to infinity. It's not the kind of local law that we've used to – the fundamental laws of physics which have a clear-cut meaning. You measure a quantity and you measure something else and all the [quantities] are equal to something else. There's just no statement about $k = 0$ that's like that. $k = 0$ is really a phenomenological hypothesis.

Brawer: That's a very interesting point. That seems relevant to the history of how people have thought about this since one of the classical questions in cosmology was what is k, of the three possible values. Your point of view seems to come from a different direction than that traditional outlook.

Guth: That's right. Inflation really says that it's none of the three possible values, but differs from place to place, on a very large scale.

Brawer: That brings to mind a point, which Steve Weinberg may have mentioned to you also. In his interview, he says that he had previously concluded that k is probably zero and, for him, inflation reopened the possibilities for $k = +1$ or $k = -1$.

Guth: Well, that's true. The same thing [happened] with homogeneity. Before inflation, since we knew the universe was homogeneous over this very large distance scale, it seemed quite natural to adopt the cosmological hypothesis that just said the universe was absolutely homogeneous, for reasons unexplained. But once you have inflation, which gives you a very long correlation length without necessarily an infinite correlation length, then is does seem much more natural to assume that the correlation length is just long, but probably not infinite.

Brawer: Let me ask you about something which you discuss in the appendix of your paper, since I'd like to see if you have anything additional to say. Some people who object to the flatness problem will use the argument that our model is built such that omega goes to one when you extrapolate back to the early universe.

Guth: That's right. No matter what parameters you have today, you extrapolate back and omega goes to one.

Brawer: So from that point of view, they say there is no flatness problem. You're always going to get arbitrarily close to one if you go far enough back.

Guth: My response to that, which I think is what I wrote in the paper, is what's impressive is the time scales. If you believe that the primary time scale in the early universe was the Planck scale or even the scale of nucleosynthesis, 10^{-35} seconds or 3 minutes, the time scale set by the deviation of omega from one, we know is at least something on the order of 10^{10} years. So the amazing thing, if you phrase things that way, is how long that time scale is compared to all the time scales associated with the early universe. Again, in back of this, is the particle theorist's idea that whenever there is a large dimensionless parameter, it needs explanation.

Brawer: Well, let me pick up on that. I actually don't know if there is an explanation for this in particle theory...

Guth: Well, there are lots of things that we can't explain, but we at least admit that we should be able to explain them.

Brawer: I see, because I was going to ask about the ratio of the gravitational to the electromagnetic force betweem two protons, for example.

Guth: Oh, absolutely. That's part of the hierarchy problem. That is, if you look at the different energy scales that occur in our theories, there's the grand unified scale of 10^{14} or 10^{15} GeV and then there's the Planck scale of 10^{19} GeV and the ratio that you mention is just the Planck mass over the proton mass, squared I guess, since you talking about ratios of forces. So that's an often talked about, but not solved problem of how the fundamental theories at the Planck scale [relate to] particles that are a lot lighter than the Planck scale. So it's not something that we know how to answer, but it is considered a problem by everybody.

Brawer: Another point of view related to this previous one that as we extrapolate back, omega gets arbitrarily close one is the reply that the universe is just one system. Therefore, it has a density. It's just one system and there's no way you can talk about probabilites.

Guth: Yes, that's right, that's another thing that people say. I regard that as total nonsense myself. Basically, I regard as total nonsense the idea that if you have only one of something, that you can't talk about probabilities. The clearest evidence for that is, yes, we have only one universe and we talk about probabilities of things happening in that universe all the time. People say, well you can repeat experiments within the universe. I can flip a coin many times. To that, I always point out that if you flip a coin a million times in succession, the probability of getting so many heads and so many tails is exactly the same as if you flip a million coins at one time and consider it one experiment. The only thing that I think can be said about probabilities is that you can only make real, honest-to-God, hard predictions when you have a probability that's very, very small or very, very close to one. Whenever, the probability of something is in the vicinty of a half, then we don't know if it's going to happen or not and whichever occurs, you can't be surprised. The idea of repeating something, to me, only has significance in probability in that by repeating something many times, you can construct situations that have very, very small probabilities. The probability of a coin coming up heads once is one-half, but the probability of it coming up heads a thousand times in a row is one-half to the thousandth power. But I don't think that the probability of a coin coming up heads a thousand times in a row is any sense lower than anything else that has that same probability, whether it's one event of a thousand events. Anything with a probability of one-half to the thousandth power has a probability of one-half to the thousandth power, and you'd be very surprised if it happened.

Brawer: I'm not sure I followed that. As I understood it, the argument is that to even define probability, you have to be able to perform the experiment a number of times.

Guth: I don't believe that. This is controversial. I don't know if my point of view here is shared by anybody. Well, I know it's shared by some people. I have spoken about it with some people. But it's not widely shared. It's certainly a thorny question, what you mean by probability in the real world, and I think most thoughtful people are aware that it's not a terribly well-defined concept. To me, the only statement that you can make about probabilistic systems is that if you know that the probability of something occuring is incredibly small, and it then occurs, you have good reason to believe that the theory which you used to calculate the probability was wrong. When you say that probabilities are defined by repeating something many times, I think that would be a very nice way of looking at it if you could repeat it an infinite number of times. Then you just take the ratio of the number of times the coin comes up heads and that's the probability. But as long as you can only repeat it a finite number of times, then I don't think it really matters, in the philosophical sense, a lot that it's more than once. In any case, if you flip a coin some finite number of times, you have some finite set of possibile outcomes and you have certain probabilities that you assign to each possible outcome. And that's the same as if you flip a coin once. The only difference is that with a thousand coin flips, many of the outcomes will have extraordinarily low probabilities. Then if they occur, then I'll think the coin was biased.

Brawer: Let me try to understand. You saying that the argument against the fact that omega just goes to one as you let the scale factor decrease is the notion of the time scale, that the time scale is so long compared to a naturally defined time scale. Now as I've heard it discussed, the counter-argument is well, it had to be something. It had to be some time scale, and it's only one universe.

Guth: Yes, right. What's my response?

Brawer: Yes, in terms of a probability argument.

Guth: Okay, in terms of probability as I'm looking at it, I guess I would take a step back if someone wanted to discuss this and say, "You cosmologists might just accept all kinds of things as given, but us particle theorists, we believe deep down that everything is calculable. Perhaps, things are governed by quantum physics, so that there is a probability distribution, but deep down we believe that everything is calculable, at least, in the sense of calculating the probability distribution." And what I would then say about this ratio – you can describe it in different ways – let me describe it in terms of the value of RT, which I think is something like 10^{87}. Actually, it's $(RT)^3$ that's 10^{87}. It's basically the number of photons in the universe that's $(RT)^3$. T^3 is the density of photons and R^3 is the volume. So this number had to be something, that's certainly true. But if we were to try to write down the probability distribution of it – have it come out of some kind of a theory, we would have to strain a great deal to write down a probability distribution for which 10^{87} was a likely outcome.

Brawer: Yes.

Guth: Okay, I've taken a step back. I first wanted to convince the person that it's reasonable – maybe he won't be convinced – I'll tell him that I believe that it's reasonable that there is an underlying theory which does produce a probability function. So the listener may or may not accept that. But let's suppose he does accept that, but then says, "Okay, suppose you have a probability distribution. You have one event. How can you decide whether that's acceptable or not?" To that, my point of view is that you never have more than one event. There's only one universe. If you flip a coin a thousand times, you still, in the end, state the number heads and the number of tails. And that's one experiment with one outcome. You have the fraction of times it comes out heads. And you decide on the basis of that whether or not you believe the coin was biased or unbiased. And in a probabilistic world, nothing is ever definitively determined. It is possible for a coin to come up heads a million times in a row. You can't say that that's impossible. All you can say is that it's highly improbable and if it did occur, you *probably* guess that the coin was biased and not that you were extraordinarily unlucky. Same thing here. If you have an a priori probability distribution, and you try the experiment and the result is something way off in the corner that has a probability of only 10^{-87}, I think it's reasonable to assume that the theory you used to calculate that probability is probably wrong. Probably the coin was biased – the probability is not really 10^{-87}.

Brawer: Now, there is no theory that gives you a probability of 10^{-87}, is that right?

Guth: That's right. So that's why there were two parts to this, either one which of which could be questioned. Part one is that we believe deep down that there should be an underlying theory. And the thing about 10^{87} is the particle theorist's rule of thumb that dimensionless numbers turn out to be of order one unless there's a good reason otherwise. So, in other words, the kind of probability distribution for a dimensionless number that you'd write down a priori, would only give significant probability to numbers that are of order one unless there's a good reason otherwise. So if you do the experiment and the outcome turns out to be 10^{87}, then the conclusion is simply that there must have been some good reason why it wasn't of order one.

Brawer: Yes, I see your argument now. Let me just end on this question. Is it part of your thinking in making this argument that you're willing to entertain the notion of there being a ensemble of possible universes. Do you feel comfortable with that?

Guth: I am comfortable with that way of thinking. I'm not necessarily *determined* to think in that way. I'm also comfortable with the idea that maybe there's, in some sense, one universe. I think it's also a hard question to define because if depends a lot on how you define universe. I think I'm happy either way, but I'm certainly comfortable with the idea that there may be an ensemble of universes and that that may be the best way of thinking about things. I do, generally speaking, subscribe to the Everett-Wheeler point of view on quantum mechanics – the many worlds interpretation.

Brawer: Thank you very much.