# Autobiography
## Alan H. Guth
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I was born in New Brunswick, New Jersey, USA, on February 27, 1947. My parents lived in Perth Amboy, New Jersey, until I was 3 years old, when we moved to Highland Park, NJ, where I lived until I went away to college. During my childhood my father owned a small grocery store in New Brunswick, and when it was destroyed by fire he bought a small dry cleaners business, also in New Brunswick. I have two sisters, Arlene (3 years older than me), and Lucille (6 years younger). I was interested in science from the time I was in grade school, and I still have vague memories of watching Don Herbert's (Mr. Wizard) wonderful television programs about science. Later, in high school, I was fascinated by science books such as Lincoln Barnett's *The Universe and Dr. Einstein.* I went to public schools in Highland Park, but left after my junior year to start at the Massachusetts Institute of Technology.

I came to MIT as a freshman in 1964, and majored in physics. I was fascinated by the idea that the world could be described by precise mathematical laws, so I chose physics because it was the branch of science most closely connected with the quest to discover the fundamental laws. I received a combined bachelors/masters (S.B./S.M.) degree in 1969, and a Ph.D. in 1972, specializing in particle theory. My masters thesis was supervised by Aron Bernstein, whom I still see frequently, and my Ph.D. was supervised by Francis Low, who unfortunately passed away in 2007. My Ph.D. thesis concerned an early version of the theory of quark interactions, and became obsolete very shortly after I finished my degree.

I was a postdoc for nine years in four different places, longer than almost anybody else I know. I began with three years at Princeton, where I worked with Dave Soper and Marvin (Murph) Goldberger on problems that were not too distant from my Ph.D. thesis work. Then I moved to Columbia University for three years, where I worked with such people as Norman Christ and Erick Weinberg. At Columbia I learned about gauge theories and topological structures such as magnetic monopoles (theoretical particles with a net magnetic charge). I then spent two years as a postdoc at Cornell, and then one year as a visitor at the Stanford Linear Accelerator Center (SLAC). At Cornell I learned about lattice gauge theory from Kenneth Wilson, and I also began my work on cosmology with Henry Tye. At SLAC I continued the work with Tye, and also worked with Erick Weinberg, who visited for the second of the two terms that I was there.

The story of how I became involved cosmology begins with two coincidences, both of which took place in the fall of 1978, during my second year as a postdoc at Cornell. Both in fact happened in the same week. On Monday (November 13) I happened into a lecture given by Bob Dicke, a well-known cosmologist from Princeton, who described a feature of the conventional big bang theory called the flatness problem. The problem concerned the extreme amount of fine tuning that is needed to make the conventional big bang theory work. As Dicke described it, the expansion rate of the universe at one second after the instant of creation has to have been exactly what we think it was to an accuracy of 15 decimal places. If the expansion rate were larger by just one unit in the

15th decimal place, then the universe would have flown apart so quickly that galaxies could never have formed. If, on the other hand, the expansion rate had been slower by just one unit in the 15th decimal place, then the universe would have rapidly collapsed, before galaxies or life could have formed. Nonetheless, the big bang theory includes no mechanism to arrange for the expansion rate to have had just this value, so it seems like a fantastically improbably coincidence.* I did not understand the calculations behind what Dicke was saying, but I was very impressed by the conclusion, and tucked it away in the back of my brain.

Later that week, on Thursday, the second very important event occurred. I was approached by Henry Tye, a fellow postdoc at Cornell, who had gotten interested in what was then the new idea of grand unified theories, known by their acronym, GUTs. I had worked previously on the theory of magnetic monopoles, so Henry asked me whether GUTs would imply the existence of magnetic monopoles. He had to teach me what a GUT is, but then I knew the answer: yes, GUTs would lead to magnetic monopoles, but they would be outrageously massive. The would weigh about $10^{16}$ times as much as a proton, so I told Henry to forget about them. There would never be enough energy available in particle accelerators to see these particles. But Henry immediately replied, "Why don't we try to figure out how many magnetic monopoles would have been created in the big bang." At first this seemed like a crazy idea to me, since I didn't think that we knew much about either GUTs or the big bang, so putting together these two areas of ignorance did not seem promising. But after about 6 months, spurred by the fact that Steven Weinberg, a physicist whom I greatly respected, had begun to work on the application of GUTs to cosmology, I started working with Henry on this question.

Henry and I soon reached the conclusion that if GUTs and standard cosmology were both right, then the universe would be swimming in magnetic monopoles. But monopoles are certainly very rare, if they exist at all, so this implied that GUTs are inconsistent with standard cosmology. We were, however, scooped in publishing this conclusion by John Preskill, who was then a graduate student working with Steven Weinberg. To make use of the work we had done, Henry and I decided to move on to the next question: is there some plausible way to modify either GUTs or standard cosmology to overcome the incompatibility?

In the cosmology of grand unified theories, the magnetic monopole production takes place at the *GUT phase transition*, a phase transition that is closely tied to the basic idea of GUTs. Above the temperature of this transition, the weak, strong, and electromagnetic interactions are unified, acting as a single interaction. Henry and I realized that if the phase transition did not occur promptly, but instead underwent a large amount of supercooling before occurring, the magnetic monopole production would be strongly suppressed. In December of 1979, Henry and I were working hard to finish our paper on

* It is called the "flatness problem" because general relativity implies a connection between the expansion rate, the mass density, and the geometry of space, and the value of the expansion rate that creates a universe like ours is the same value that makes the universe geometrically flat (i.e., Euclidean).

this, before Henry left at the end of the month for a 6-week trip to China. I had moved to the Stanford Linear Accelerator Center (SLAC) for a one-year visiting position, but Henry and I communicated daily by telephone. I had a reputation for being very slow at finishing papers, so Henry emphasized to me that it was very important to finish this paper before his trip. In writing the paper, we assumed that, as the supercooling took place, the expansion of the universe would be unaffected. I'm pretty sure that it was Henry who suggested that we check this assumption, and one night I went home to my rented house in Menlo Park, CA, and wrote down the relevant equations. It was clear that the supercooling would lead to the exponential expansion of the universe, and I also realized that same night that the exponential expansion would drive the universe to just the value that Dicke had specified — the flatness problem would be solved. I don't know exactly when I started calling this process inflation, but I know that I was calling it that by the end of December. The next day I explained all this excitedly to Henry, but apparently he was sitting way in the back of Dicke's lecture and never really heard Dicke explain the flatness problem. Henry was still concentrating on getting our paper finished before his trip, so we finished the paper with no mention of exponential expansion. Henry and I agreed that I would continue working on the exponential expansion, writing my own paper about that.

Just a few weeks later, at lunch in the SLAC cafeteria, several of my colleagues were discussing something called "the horizon problem," which I had never heard of. The problem, as my friends explained to me, was that the early universe expanded so quickly that there was not enough time for the different parts to communicate with each other, even if signals could be sent at the speed of light. So that made it very hard to understand how the temperature of the early universe became synchronized. Yet, we know from the cosmic background radiation that it was synchronized to very high accuracy. I realized that inflation would solve that problem, too.

I gave my first talk about inflation at the end of January, 1980, and the idea immediately met with enthusiastic interest, at least in the particle theory community. Sidney Coleman, a very well-known theorist from Harvard, was visiting SLAC that year and did a lot to publicize the new theory. I made a circuit of the U.S. that spring, giving talks about inflation, and soon received a number of job offers. I was able to skip the step of an assistant professorship, and accepted an associate professorship offer at MIT, where I have been ever since.

I did not submit an article about inflation until August of 1980, partly because I was not sure if the ending of inflation would work work out well. In the version of inflation I was considering (now often called *old inflation*), the inflationary period ended with a first order phase transition, which occurred in much the same way that water boils. Bubbles of the new phase would nucleate and then grow. Most of the energy in each bubble would initially be in the bubble walls. For the model to be successfull, these bubbles would have to collide early and merge into a uniform region. I worked on this bubble-merging problem with Erick Weinberg, but the breakthrough came when I visited Cornell and spoke with the mathematician Harry Kesten. The answer was unhappy, as he was able to show me that the bubbles would never merge into an infinite cluster, but would always remain in the form of finite clusters. Erick and I calculated that each of these finite clusters would be dominated by a just a few large bubbles, and hence could never become uniform. When I submitted my paper in August 1980, I included a summary of this problem, which was later dubbed "the graceful exit problem," and expressed hope that some variation would be found that avoids it. This problem was later solved by the invention of *new inflation* by Andrei Linde, submitted in October 1981, and independently by Andreas Albrecht and Paul Steinhardt, who submitted in January 1982.

The next important phase of inflationary research was the calculation of density perturbations that would arise from quantum fluctuations during the inflationary process. The possible quantum origin of density perturbations was suggested as early as 1965 by A. D. Sakharov, but the first serious calculation was done by V. F. Mukhanov and G. V. Chibisov in 1981, in the context of Starobinsky's model. In the context of new inflation, the question of density perturbations was a central subject of study at the Nuffield Workshop on the Early Universe, held in the summer of 1982 at Cambridge University, organized by Stephen Hawking and Gary Gibbons. It was at this meeting that I first met both Andrei Linde and Alexei Starobinsky. There was much debate during the workshop, but by the end all of us working on density perturbations agreed on the answer, and four papers emerged: one by Hawking, one by Starobinsky, one by So-Young Pi and me, and one by Bardeen, Steinhardt, and Turner. The bottom line was that the density perturbations had the right spectrum, to the crude accuracy with which it was known in 1982, but that with the originally proposed parameters they were much too large. But the method of calculation was established, and modified models could be found for which the amplitude of the perturbations would come out right.

Since this time I have remained at MIT, and have worked on a number of topics, most of which were closely related to inflation. One topic which has fascinated me is the question of whether the inflationary mechanism could, in principle, allow a super-advanced civilization to produce a new universe in the laboratory. Working with various collaborators, I discovered that within the bounds of classical physics, this would not be possible. However, if one considers the possibility of quantum tunneling, then it might be possible. This is still an open question.

Other topics on which I have worked include the question of whether or not general relativity allows the possibility of time travel. With Edward Farhi and Sean Carroll, I studied in particular a time-machine construction discovered by Richard Gott, and we found that the circumstances in which it could arise are highly restricted. In particular, we found that in the simplest types of model universes, it could never arise.

Another very important feature of inflationary models is that almost all of them lead to *eternal inflation*: once inflation starts, it never completely stops. Instead inflation continues eternally, but locally the inflation stops in places, producing what can be called pocket universes. Within our models, an infinite number of these pocket universes would be produced, leading to a *multiverse*. Alex Vilenkin, Arvind Borde, and I showed that (under reasonable assumptions) this process cannot have extended eternally into the past, but instead the inflating region must have had a past boundary of some kind. Jaume Garriga, Vilenkin, and I showed that such an eternally inflating universe does not completely forget the initial conditions that led to it, but always maintains certain features that are determined by the initial conditions.

An important question that arises from eternal inflation is called the *measure problem*, the problem of defining probabilities in such a spacetime. The issue is that in such a model, any type of event that is physically possible will occur an infinite number of times. Thus, to say that any type of event is more common than another, one must compare infinities. There is still no unique solution to this problem, but many procedures for defining probabilities have been explored, and many have been excluded because they lead to obviously incorrect predictions. I have explored mainly one particular procedure, called scale-factor-cutoff measure, which still appears to be viable.

My work on inflation has led to a number of awards. I have been elected to the National Academy of Sciences and the American Academy of Arts and Sciences, and have been awarded the Franklin Medal for Physics of the Franklin Institute, the Dirac Prize of the International Center for Theoretical Physics in Trieste (shared with Andrei Linde and Paul Steinhardt), the Cosmology Prize of the Peter Gruber Foundation (shared with Andrei Linde), and the Fundamental Physics Prize (also awarded to Andrei Linde, among others). I am now the Victor F. Weisskopf Professor of Physics and a Margaret MacVicar Faculty Fellow at MIT. I have written a popular-level book called "The Inflationary Universe: The Quest for a New Theory of Cosmic Origins" (Addison-Wesley/Perseus Books, 1997).