The Multiverse: a Very Short Introduction

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Chapter 5: All the worlds from the primordial bubbles

This Chapter discusses our third multiverse. It was proposed by some cosmologists in the mid-1980s on the basis of their theories about the Big Bang origin of the cosmos. Like the Everettian multiverse in the last Chapter, it is agreed even by its proponents to be very speculative, and hard to confirm. This common feature is unsurprising. As we discussed in Chapter 1, there might well be, within a branch of physics, theoretical (or more generally: conceptual) reasons for a proposal that is hard to confirm---and whose assessment thus calls on conceptual, even philosophical, arguments.

But there is also a dissimilarity from the Everettian multiverse. For that multiverse arose from the very general structure of quantum theory. Recall that although we concentrated for simplicity on elementary quantum theory, e.g. the quantum replacement of classical point-particles, all our more advanced quantum theories, including our quantum field theories of electrons, quarks etc. equally face the measurement problem (Chapter 4.3, 4.4). And the reasons in favour of the Everettian solution to that problem apply equally to them; (as do the reasons against the Everettian solution).

On the other hand, the cosmological multiverse arises from a very specific mechanism called 'inflation', that is speculated to have operated for a tiny fraction of a second in the very early history of the universe, i.e. very soon after the Big Bang. Thus cosmologists' rationale for proposing inflation was not that it solved a problem about the general structure of a physical theory, quantum or otherwise. Rather, it answered pressing 'Why?' questions. It promised to explain some facts that were otherwise puzzling, even mysterious. These questions, called the 'flatness' and 'horizon' problems, were about the value of a physical quantity "having to be just-so", i.e. having to take a value that was constrained to many decimal places, if our cosmological theory was to adequately describe empirical observations. Thus just-so-ness is usually called 'fine-tuning'.

This dissimilarity has two broad consequences for the plan of this Chapter: one about philosophy, and one about physics. First: it prompts the philosophical question 'What counts as an explanation?'; which will lead to another related question 'How could science confirm a multiverse proposal?' Accordingly, these questions will be this Chapter's main philosophical questions. Second: the details about what was considered puzzling, how the proposed mechanism of inflation explained it, and how inflation gives rise to a multiverse, all involve advanced physics; and this Chapter will have to omit almost all of those details. Fortunately, these omissions will not prevent us pursuing the philosophical questions that arise.

I should also declare at the outset another omission of some advanced physics, additional to that involved in the cosmological multiverse. Namely: I will not give details about a fourth multiverse proposal, that again comes from some very advanced physics---string theory. String theory's multiverse is called 'the landscape'; (for reasons I will touch on). But it turns out to raise much the same philosophical questions as the cosmological multiverse does; and so it will be enough to say a little about it when broaching those questions.

In a bit more detail: both the cosmological multiverse and the landscape proposals suggest that what we usually called 'constants of nature', like the amount of electric charge on an electron or the speed of light, could vary across the multiverse. That is: they could take different values in different universes within the multiverse. So both proposals provide a framework encompassing different such values; and so they both prompt an ambitious project: to somehow explain the value that we in this universe measure, by invoking appropriate features of how the values vary across the multiverse. So here is another reason for this Chapter's taking explanation and confirmation as its main philosophical topics.

In this endeavour to explain the values we measure, the obvious overall strategy would be to argue that the value we measure is in some precise sense generic, or typical, of the values across the multiverse; and is thereby to be expected. But as I will discuss, there is a second, less obvious, strategy. It invokes what are called 'selection effects', or (a better-known jargon) 'the anthropic principle', to argue that the value we measure is likely to be observed. I will label these 'strategy (1)' and 'strategy (2)'.

With this background, I can now explain the plan of the Chapter. There will be four stages. The first stage (just Section 1) is ground-clearing. I need to clarify the relation between the Everettian and cosmological multiverses. Doing this will also explain why the rest of the Chapter can proceed with hardly a mention of quantum theory and its problems of interpretation. Besides, this Chapter is largely independent of the Chapter 3's discussions of logic and logically possible worlds. (We will however see one main theme in common to all three multiverses: probability.)

Then in the second stage, I introduce modern cosmology and its multiverse. Section 2 will summarize what cosmology had established by about 1980. Section 3 will do two jobs. First, I report the puzzling facts about fine-tuning that remained unexplained (i.e. the flatness and horizon problems), and how inflation could explain them. Then I will sketch how inflation led to a multiverse. In Section 4, I will briefly discuss string theory.

In the third stage (Sections 5 to 7), Section 5 reviews some of the philosophical literature about explanation, and ends by formulating my strategies (1) and (2). Then I turn to strategy (1): explaining a fact by showing it is generic. Section 6 describes the strategy's successes and its positive features; Section 7 describes its difficulties, including in cosmology.

The fourth stage (Sections 8 and 9) is about strategy (2): explaining a fact by showing it probable (or probable enough) to be observed. Section 8 explains the strategy with examples from outside cosmology, indeed outside physics; it also introduces the jargon of 'selection effects'. Section 9 discusses the strategy within cosmology. It introduces the jargon 'the anthropic principle'; and as an example, summarises the anthropic explanation of the value of the cosmological constant.

Finally, Section 10 concludes. Here, I recommend a framework for confirming a theory of the multiverse that incorporates ideas from both the strategies: the idea of being generic, and the idea of probable (or probable enough) to be observed.

Thus Sections as follows:

Chapter 5.1: Comparing the Everettian and cosmological multiverses

The mere phrase 'the cosmological multiverse' suggests there should be connections with the Everettian multiverse proposal. For as we saw, the Everettian proposes a quantum state of the cosmos, written with the capital Greek latter Psi, i.e. Ψ. (Recall Chapter 4.6's decision to use this phrase, not the more usual 'quantum state of the universe'.) So the job of this Section is to describe these connections. But as one would expect from the Preamble's setting aside advanced physics, I shall have to set aside many details: an omission that, again, I consider justified by its not affecting our philosophical questions about explanation and confirmation.

When one meets the phrase 'the cosmological multiverse', and bears in mind that modern cosmologists of course accept quantum theory, one naturally expects that: (i) some cosmologists will endorse the Everett interpretation; and even (ii) the cosmological multiverse will turn out to be an elaboration of the Everettian one.

The first of these expectations, (i), is indeed true. Among cosmologists interested in interpretative questions about quantum theory---and we will see that the cosmological multiverse raises such questions---the Everettian interpretation is popular.

But on the other hand, the second expectation (ii) is true only in a liberal, i.e. logically weak, sense of 'elaboration'. Yes, a cosmologist may well accept that there is a quantum state of the cosmos, and may also argue that it describes the cosmological multiverse they advocate. But the universes (or worlds) in this multiverse are very different from the Everettian universes (or worlds) defined by the continual, ubiquitous, rapid and general process of decoherence for macrosystems that we discussed in Chapter 4.7. For (as I said in the Preamble) the cosmologists' many universes are produced by a specific mechanism, 'inflation', that is speculated to have operated for a tiny fraction of a second very soon after the Big Bang. In many, perhaps even most such universes, there will indeed be (later, long after the universe starts) macrosystems such as dust-particles, or even pointers and cats: macroscopic objects which can decohere. And for these, the Everettian can then claim that the last Chapter's account, with its Ψ, applies.

So the upshot is that if we accept the cosmological multiverse based on the idea of inflation, then the quantum state of the cosmos in the sense we envisaged in the last Chapter (i.e. Ψ with components, i.e. summands, describing various decoherent worlds: cf. Chapter 4.8) is really a description of a single universe within the cosmological multiverse. This implies that the cosmologist we envisaged at the start of the last paragraph---who accepts that there is a quantum state of the cosmos, and says it describes the cosmological multiverse they advocate---will mean by 'quantum state of the cosmos' something yet more dizzying than Ψ. That is: more dizzying than the meaning that in the last Chapter we envisaged (and the Everettian advocated) for this capital Greek letter. For it will mean a quantum state, presumably a superposition, with a component i.e. a summand for each universe within the cosmological multiverse. So in short: this cosmologist's 'quantum state of the cosmos' is a sum of 'quantum states of the cosmos', in the sense of the last Chapter's Everettian: a sum of different Everettian states Ψ_i, where the suffix 'i' is a label on the summands, i.e. the different universes.

Here, I should give a clarification. For the sake of a clearer exposition, I have in the last two paragraphs taken decoherence as a process that occurs to macrosystems due to their interaction with an environment, e.g. dust-particles immersed in air: just as I did in the last Chapter. As just explained, this gives a crisp contrast with the specific mechanism of inflation. But cosmologists also apply the idea of decoherence, in various detailed ways, to the cosmos as a whole, including at very early times.

One main way this is done takes all the material in the cosmos to be the system (the analogue of the dust-particle), and spacetime to be its environment (the analogue of the air). Here, 'material' will include not just matter e.g. atoms, electrons, quarks, but also radiation e.g. electromagnetic radiation; and in order to give content to the idea of this material interacting with spacetime, one needs states of spacetime that respond to the states of matter, so that one describes spacetime with general relativity, according to which spacetime is indeed responsive in this way. In this way, even without macrosystems such as dust-particles, decoherence can occur. So cosmologists will talk of all the cosmos' matter degrees of freedom being in an improper mixture (cf. the end of Chapter 4.7) of states that are definite for some appropriate quantities.

In short: the cosmologist/Everettian contrast is not as crisp as I first suggested. But in the rest of this Chapter, we can safely take this contrast to be valid. That is: we will think of the cosmologist's 'quantum state of the cosmos' as a sum of different Everettian states Ψ_i: states which, as announced in the Preamble, can differ in the values of constants of nature such as the charge of the electron. This picture of the relation between the two multiverse proposals will set us in good stead for our philosophical questions about explanation and confirmation.

Of course, these are not just speculative, but also imprecise and dizzying, ideas. Just as I remarked (in Chapter 4.6) that no Everettian has the faintest idea how to write down in detail the Everettian quantum state of the cosmos Ψ, no quantum cosmologist can now write down in detail <u>their</u> quantum state of the cosmos. For as this Chapter will report: we do not know---and we may never know---the underlying physics of inflation. So a sketch-definition of 'universe' for the cosmological multiverse, on analogy with the Everettian's sketch definition of 'world' (Chapter 4.8) is beyond current knowledge.

But agreed: you can't keep a good idea down. I should also report that some quantum cosmologists, sympathetic to the Everett interpretation, have proposed mathematical formulas for the quantum state of the cosmos at very early times. Of course, the details vary from one author or research programme to another. Thus some of these formulas are independent of whether there was a very brief period of inflation; while some incorporate such a period. Some are independent of decoherence at very early times, e.g. of the matter degrees of freedom; while some incorporate it. One such formula, proposed by Hartle and Hawking in 1983, and independent of both inflation and decoherence, is called 'the no-boundary proposal'. It has been much studied and developed since then: and indeed, related to both inflation and decoherence.

This reflects the more general fact that most quantum cosmologists recognize the relevance of interpretative questions, and related methods and ideas like decoherence, to their scientific work. This relevance is shown by the point above: that cosmologists' models often incorporate decoherence at very early times, long before there were macrosystems like dust-particles.

But for this book's theme of the multiverse, the most vivid example of this relevance will be the fact in Section 3 below: that the 'primordial bubbles' of this Chapter's title, which give rise to the cosmological multiverse's universes, will involve an implicitly Everettian view of the quantum state. Thus recall the basic mystery of the quantum state that I stressed in Chapter 4.2 and 4.3: that the quantum state <u>is</u> an assignment of complex amplitudes ("square roots of probabilities") to classical alternatives. That is: to say that 'there is a primordial bubble (with such and such characteristics)' will imply---commit one to believing---that there <u>is</u> a corresponding classical alternative. In this sense, to propose that all the universes ('bubbles') in the cosmological multiverse are equally real is to assume a resolution (on a cosmic scale) of the quantum measurement problem: a resolution with a distinctly Everettian flavour.

Nor is this vivid example the only place where modern theoretical cosmology meets the measurement problem on a cosmic scale. Even without going back in time so far as the putative period of inflation, and without postulating a multiverse, modern cosmology describes early states of the universe, e.g. a minute, or a year, or ten thousand years after the Big Bang, in terms of quantum theory---and so the measurement problem arises.

Indeed, it arises in connection with something so basic and vivid to us as the existence of stars, planets and galaxies. For as I will explain in the next two Sections, there is a weak electromagnetic signal throughout space, called the 'cosmic microwave background' (CMB) radiation, that dates from about 380,000 years after the Big Bang---and which we can directly observe. We observe this CMB radiation to be very smooth and uniform: it looks the same in all directions. But it has tiny 'wrinkles': which are really variations in the quantum amplitudes for various densities of mass in regions of space. This means that a peak among these wrinkles is a "seed" of a clump of matter becoming localized, under the gravitational attraction of its component parts, in one region of space rather than another. Such a clump, once localized, can grow, pulling yet more matter in by its gravity, and eventually produce, for example, a star.

But note that here, the word 'seed' is a metaphor, that hides the problematic issue of the collapse of the wave-function. For whereas a real seed is an actually existing object that grows into an actually existing plant, this peak of quantum amplitude is only a higher (square root of a) probability for the event of clumping to happen here, rather than there. (Unfortunately, the metaphor is entrenched in textbooks as well as popular expositions. Only the better textbooks admit that this transition, from peaks and troughs of quantum amplitude to a classical, slightly uneven, distribution of mass-density across space, is problematic---since it is a cosmic version of the "collapse of the wave-function", which all agree is problematic.)

So much by way of sketching connections between the Everettian and cosmological multiverses; and more generally: connections between the interpretation of quantum theory, especially its measurement problem, and quantum-to-classical transitions in the very early cosmos ('primordial bubbles') and in the not-so-early cosmos ('wrinkles' in the CMB).

Obviously, these connections are important, indeed fundamental. But this Chapter will not go in to further details about them, for two reasons: one negative and one positive. The negative reason is that most cosmologists, even quantum cosmologists, agree that these cosmological aspects of the measurement problem (or more neutrally: of the interpretation of quantum states), are not yet precisely enough formulated to be addressed as a problem within physics. In short: we do not know enough, and the time is not yet ripe. The positive reason is that (as we shall see) even if we restrict ourselves to a "classical outlook", there is so much to explore, in both the physics and the philosophy of modern cosmology. At least: there is certainly enough for this Chapter.

Chapter 5.2: A golden age of cosmology

We live in a golden age of cosmology. It began in the twentieth century, especially in its second half. But already in the first half of the century, there were four momentous developments: two observational and two theoretical.

First, we discovered that the nebulae, that appeared in telescopes like cloudy smudges rather than point-like stars, were really other galaxies of vast numbers of stars, like our own Milky Way. So the cosmos turned out to be vastly larger than had been envisaged. Second, we discovered that the cosmos is expanding. More precisely: any two galaxies are receding from each other, i.e. the distance between them is increasing. (The speed of recession is approximately proportional to the distance between them.) But this is not an expansion of matter into a pre-existing empty space, like an explosion of a firework or a bomb. Rather, the space itself is expanding. Agreed, that is impossible to visualize. We are bound to think of an ambient or embedding space relative to which the expansion occurs; and it was a struggle for physicists to accept this idea.

In this acceptance, the third development was crucial: namely, Einstein's discovery of general relativity, and its application to the whole cosmos. As mentioned in Chapter 2, general relativity is a relativistic theory of gravitation. According to it, gravitational influence propagates across space at the speed of light; not instantaneously, as in Newton's theory. And like Einstein's earlier theory of special relativity, it unifies space and time into a single entity, spacetime: which, being four-dimensional, is again unvisualizable. (Physicists' acceptance of these unvisualizable ideas, of expanding space or of spacetime, was helped by the rise of pure mathematics, reviewed at the end of Chapter 2: the increasing formalization of mathematics included liberating geometry from visual intuition.)

Einstein himself, immediately after formulating general relativity, applied the theory to the cosmos as a whole. In terms of our jargon of systems and their state-spaces (cf. Chapter 3.3): he took the cosmos as his system, and he described it as a spacetime, extending not just arbitrarily far in all directions in space, but also throughout the past and future. (This is reminiscent of our description of the cosmos, "our world", in Chapter 1.1.) So he aimed to find a solution to the equations of general relativity that described the whole of spacetime: not of course in its myriad details, but in the broadest possible terms. In particular, matter was treated as smoothed out uniformly across space, although of course it is in fact concentrated in great clumps. (For the galaxies are clumps; and within them, the stars are clumps.) By the mid-1920s, solutions of general relativity describing an expanding cosmos had been found, and in the following years they were investigated and elaborated. In some of these solutions, the expansion began from a very hot, very dense state: which came to be called 'the primeval fireball'.

The fourth development was the rise of astrophysics: i.e. the physics of stars. Quantum theory, discovered in the 1920s, was applied to describe in detail, not just how stars shine by burning helium, but also much else: the different types of stars, how other chemical elements are formed in stars (called '<u>nucleosynthesis</u>', since the stars synthesize i.e. make the nuclei of, elements), how and why some stars explode and others implode.

By the late 1940s, the third and fourth developments had been combined. For the ideas and results of astrophysics were applied to describe nucleosynthesis in the conjectured primeval fireball. This led to detailed predictions about the cosmic abundances of light elements like hydrogen, helium and lithium; and to the prediction of a pervasive but very faint electromagnetic radiation with a characteristic wavelength, that was a remnant of the fireball.

Thus matters stood in about 1960. It was the following years that really ushered in the golden age. Again, they were several momentous developments, both observational and theoretical. I shall pick out three, that were all well underway by 1980. Developments after 1980, including the proposed multiverse, will be treated in the next Section.

Foremost among observations was the discovery (by accident, in 1964) of the predicted remnant radiation. It is called the 'cosmic microwave background' ('CMB') radiation. It was immediately recognized as confirming general relativity's expanding cosmological solutions. In just a few years, almost all cosmologists accepted that the cosmos originated in a primeval fireball about fourteen billion years ago. (The fireball was soon renamed '<u>the Big Bang</u>': a name that had originally been suggested by sceptics, as a derisory label.) Besides, in the following decades, the CMB has proven to be an extraordinarily rich source of information about the early cosmos.

The second main observational development between 1960 and 1980 was the invention and deployment of several new sorts of telescope that enabled astronomers and cosmologists to study types of electromagnetic radiation other than visible light. For radio waves (with wavelengths much longer than visible light), there was ground-based radio astronomy, which had been pioneered in the 1940s. For microwave and infra-red radiation (i.e. wavelengths a bit longer than visible light) and X-rays (shorter than visible light), one needed to get above the Earth's atmosphere. For these wavelengths, dozens of satellite missions have yielded a profusion of data, both for astrophysics and cosmology: for example, data about the CMB and the cosmic abundances of elements.

The third development was theoretical. After 1960, there was a renaissance in the study of general relativity, both as regards its mathematics and its applications. Here, one highpoint was a cluster of theorems saying that among the solutions of general relativity (i.e. the spacetimes that are possible according to the theory), <u>singularities</u> are generic, i.e. typical. The idea of a singularity is a breakdown in the smooth structure of spacetime; and the theorems showed that they must occur under certain conditions. These conditions included when a star whose mass is above a certain limit, having burnt all its helium fuel, implodes under its own weight. This is called the 'gravitational collapse' of the star; and it leads to a <u>black hole</u>, in which the singularity lies. But more relevant to us: among these conditions were conditions that were understood to prevail in the early cosmos. This gave a new perspective on the simple expanding cosmological solutions of general relativity that had been recognized, already in the 1920s, as having an initial singularity: the original "point where the fireball began" (though not a point in spacetime itself). Namely: this initial singularity came to be regarded, not as an artefact due to the solution's admittedly very idealized treatment (e.g. smoothing out the matter), but as a robust feature of the solution that might well be physically real.

The result of these three developments was that by the mid-1970s, cosmologists had agreed on a model of the history of the cosmos, with an initial singularity about fourteen billion years ago, followed by a hot primeval fireball that cooled and expanded. It was called '<u>the standard model</u>'. (This is not to be confused with its namesake, the standard model in high-energy physics. That describes the physics of electrons, photons, neutrinos. quarks, i.e. the constitutents of protons and neutrons, etc. It also was formulated in the mid-1970s.)

In the last fifty years, this standard model has stood up amazingly well; (as has its namesake in high-energy physics). Agreed: in addition to elaborating ideas and methods that were already formulated in the 1970s, major new ideas have had to be added so as to accommodate observations. One such idea is that much, indeed the majority, of the mass of the cosmos is of an as-yet unknown form; (called '<u>dark matter</u>'). Another is that although one would expect the expansion of the cosmos to slow down, to decelerate (since the stars and galaxies, having mass, pull on each other gravitationally), it is in fact accelerating. (This is called '<u>dark energy</u>'.)

But these ideas need not concern us, for two reasons. First: their bearing on our topic, the multiverse, is tenuous. Most proposals about the nature of dark matter and dark energy do not give reasons for, or against, a multiverse. In effect, they are compatible with inflation's producing a multiverse, but do not especially support it.

Second: there is good theoretical reason to think that whatever the detailed physics of dark matter and dark energy turn out to be, it will not overturn the main outlines of what the standard model claims to have established. This is well illustrated by two of the standard model's "grand narratives" of the history of the cosmos: the thermal history of the cosmos (i.e. its density, temperature, pressure etc. at successive stages); and the history of the synthesis of elements, both in the primeval fireball and later in the stars. Indeed: it is a striking testimony to how well confirmed this standard model now is, that for both these narratives, the detailed story given in a technical exposition written today matches closely the detailed story in expositions written some fifty years ago.

So I end this review of our fortunate golden age in cosmology by taking as an example, the thermal history of the cosmos. This will set the scene for the next Section's description of the puzzling features that prompted cosmologists to postulate an even earlier, very brief period, of accelerating expansion: inflation.

I will give just three "snapshots" of what the temperature, density and relative size of the universe was, at the following times: (1) a millionth of a second after the Big Bang, (2) a hundredth of a second after it, and (3) ten million million seconds, i.e. about 380,000 years, after it. Note that we are now about a hundred thousand million million seconds, i.e. about fourteen billion years, after the Big Bang.

Before I give the numbers, let me adopt the <u>exponent</u> (or <u>index</u>) <u>notation</u>, using a superscript to indicate the number of noughts. So one hundred is 10^2; and a million is 10^6. Similarly for reciprocals, i.e. 1 divided by a number: one hundredth is 10^{-2}; and a millionth is 10^{-6}.

This notation prompts another important point. It will be helpful (though I admit, it is difficult) to think <u>logarithmically, not arithmetically</u>: to think, for example, that since the present time is about 10^{17} seconds after the Big Bang, the time $t = 10^{-17}$ seconds before the Big Bang is <u>as much before $t = 1$ second, as we are after it</u>. Though this sounds blatantly wrong, the rationale for it is that physics is a matter of scales. That is: if you change the situation you wish to describe by a factor of about 10, you may well need a very different description: and this is even more likely if you change by a factor of about 100. This trend holds whether the quantity whose value you change is time, or distance, or energy or temperature. So when cosmologists puzzle over what was the state of the universe, at say $t = 10^{-6}$ seconds, or how physical processes changed as a result of the cooling between, say, $t = 10^{-6}$ and $t = 10^{-2}$ seconds, we should not accuse them of straining at gnats, i.e. of foolishly concentrating on events so transient that they cannot be very important. For, agreed: the universe was changing unbelievably rapidly (arithmetically speaking!);

but the relevant processes change---and so our description must change---in crucial ways, depending logarithmically on the earlier time.

So here are the three snapshots.

(1): $t = 10^{-6}$ seconds after the Big Bang:--- The temperature was about 10^{13} degrees Centigrade. This is when protons and neutrons, i.e. the constituents of atomic nuclei, form: for at yet higher temperatures, they "melt" into their own yet-smaller constituents, quarks. The size of the universe relative to its size today was 10^{-12}, and the mass density was about 10^{17} grams per cubic centimetre.

(2): $t = 10^{-2}$ seconds after the Big Bang:--- The temperature 10^{11} degrees Centigrade. Atomic nuclei form: i.e. at higher temperatures, they "melt" into their constituent protons and neutrons. The size of the universe relative to its size today is 10^{-11}, and the mass density was about 10^{9} grams per cubic centimetre.

(3): $t = 10^{13}$ seconds (i.e. about 380,000 years) after the Big Bang:--- This is when atoms formed, by free electrons combining with nuclei; and the universe thereby became for the first time transparent to electromagnetic radiation. So for cosmology, this is a crucially important time. For our direct observations of such radiation cannot go back any earlier than this time. (But amazingly, we do observe this time: the CMB, the remnant radiation from the Big Bang, dates from exactly this time.) It is known as the 'recombination time': though all agree that 'combination' would be a much better name, since the electrons and nuclei were not stably combined at any earlier time. The temperature is about 3000 degrees Centigrade. (By way of comparison, the temperature at the surface of the Sun is about 6000 degrees.) The size of the universe relative to its size today is 10^{-3}, and the mass density is 10^{-21} grams per cubic centimetre.

I said that these claims about the universe's thermal history were now established. But I agree that when presented with these stupendous figures---so high for temperature and density, so tiny for time and size---one of course asks: 'Is all this really established as fact?'

I think the answer is Yes. Of course, the evidence is technical and varied---and I cannot go in to details. But I note that physicists' description of even my earliest snapshot, i.e. the description of protons and neutrons "melting" into quarks by the standard model of high energy physics, has been confirmed by terrestrial experiments. Indeed, I could have chosen an earlier snapshot. For it is common nowadays to take the boundary between known and speculative physics to be at about 10^{-11} seconds after the Big Bang. But agreed: there is a spectrum of caution and confidence (as we discussed in Chapter 1.4), and one could reasonably be more cautious, even taking e.g. one second as the start of what one calls 'established'.

Chapter 5.3: Inflation … eternally

So much by way of celebrating our golden age of cosmology, and its standard model as formulated in the mid-1970s and developed since then by e.g. the admission of dark matter. This Section turns to describing two puzzling features of it, which were recognized by 1980 and which prompted the idea of inflation---and so the multiverse. (I set aside a third puzzling feature, called 'the monopole problem': not just for brevity, but also because inflation's treatment of it is similar.)

They are called the 'flatness problem', and the 'horizon problem'. For both of them, the problem is not one of empirical adequacy, i.e. that the standard model from the mid-1970s gets some observational prediction wrong. The problem is that according to this standard model, an empirically measured number amounts to a coincidence so enormous that, as the saying goes: 'it cries out for explanation'.

So first, the flatness problem. The expanding solutions of general relativity fall into three classes:

> (i): those in which the matter is on average dense enough that gravitation eventually overcomes the expansion so that there is a contraction and ultimately a "Big Crunch" (called a 'closed universe');

(ii): those in which the average density is low enough that expansion goes on forever at some non-zero rate (with of course lower densities making for a higher final rate: called an 'open universe'); and

(iii): those "between" (i) and (ii) in that the average density is low enough that gravitation cannot overcome the expansion, but is high enough that the final rate of expansion is zero. (This is called a 'flat universe', since the instantaneous geometry of space, across the whole universe, gets ever closer to being Euclidean---so 'eventually-flat universe' would be a more accurate name.)

The density in (iii) is special. Not only is it the boundary between the regimes (i) and (ii): also, once a solution has that density it will have it forever. It is called the 'critical density'.

These ideas are often put in terms of the ratio between the universe's density (of course, as usual: taking the matter as smoothed out over all space) and the critical density. So this number is a pure number; it arises from dividing one density by another and so it has no units. It is written as the Greek letter **Omega**, i.e. Ω.

So here is the coincidence. In fact, we have measured Ω to be now close to 1; and indeed to have been close to 1 at all times later than about one second after the Big Bang. (This means the universe has ever since then been almost flat: its spatial geometry is almost Euclidean.) But in these solutions of general relativity, any difference of Ω from 1 in the early universe is very rapidly amplified. For example: if at one second after the Big Bang, $\Omega = 1{:}08$, then already at ten seconds $\Omega = 2$; and thereafter Ω keeps increasing exponentially. And on the other hand: if at one second after the Big Bang, $\Omega = 0{:}92$, then already at ten seconds $\Omega = 0{:}5$; and thereafter Ω keeps decreasing exponentially. In short: in these solutions, $\Omega = 1$ represents an equilibrium---but a very unstable equilibrium. In particular, for Ω to be about 1 today requires that it be stunningly close to this privileged value soon after the Big Bang. For example, at one second after the Big Bang it has to differ from 1 by at most 10^{-16}. This is about the ratio between the width of a human hair (viz. a tenth of a millimeter) and the average distance between Earth and Mars (viz. 225 million kilometres).

Indeed an enormous coincidence, 'crying out for explanation'.

Second, the horizon problem. It has a similar structure. Namely: although the standard model of the late 1970s is empirically adequate, it requires a feature of the CMB to be "just so" to an extreme degree: so extreme that it is implausible to treat it as a brute fact, without explanation.

The problem arises from the fact that the CMB, dating from 380,000 years after the Big Bang, is very uniform across the sky. Its wavelength, amplitude etc. is almost the same in whatever direction you point your telescope: its wrinkles are minuscule. More precisely: their proportional size is 10^{-5}: which is like having, on the surface of a pool of water one meter deep, a wave which is only a hundredth of a millimetre high.

One naturally asks why it should be so uniform. And this question becomes all the more urgent in the context of the standard model of the late 1970s. For it says that for two directions in the sky with a sufficient angle between them---namely, two degrees or more (the visual width of the moon, or more)---the two emission-events of the CMB that lie along those directions (about 13 billion years ago) have no common causal past. This means: no yet-earlier event could affect either of them, by an influence travelling at most as fast as light. Relativity theory suggests some helpful jargon for this. Given any event, the set of events to its past that could affect it by an influence travelling at most as fast as light, is called the event's past light-cone. So the point is: the standard model says the two past light-cones of the two CMB emission-events do not overlap.

This makes our question urgent. For this means there could not have been any kind of interaction between events in the past of the first emission-event and events in the past of the second emission-event. But since the emission events are so strongly correlated---their quantitative properties differ by at most the tiny factor 10^{-5}---one would naturally expect some such interaction. For think of how we explain various systems with uniform properties throughout their extent; (such states are called 'homogeneous'). For example: a cup of tea with milk throughout it, or an iron bar with its temperature equal along its length. We explain these by a past process of interaction. Namely, the system started in a non-uniform (heterogeneous) state: then the milk spread through the tea, the heat spread along the bar. (A process that ends in such an equilibrium state is called 'equilibration'.) But here, the standard model forbids such a process of achieving uniformity by an earlier interaction. For it says that no causal process of any kind could affect the two signals of CMB coming to us from these two directions in the sky.

In short: the standard model tells us to accept these signals' strong correlation as a brute fact, which is encoded in the state of the universe's matter and radiation at times earlier than the recombination time. That is hard to accept. And all the harder when one calculates that the angle between directions sufficient to imply no common causal past is only about two degrees—the visual width of the moon.

So the flatness and horizon problems have a common structure. They each take a certain feature (**Ω**, and the smoothness of the CMB, respectively) to be just so. That is: the feature has a value specified to many decimal places (also called: 'many significant figures'), without the standard model giving any account of why the feature is so exactly specified. This "just-so-ness" is called 'fine-tuning'. (Later in this Chapter, this phrase will get a more specific meaning in the context of selection effects.)

Enter the idea of inflation. It turns out that if we change the standard model by "inserting" into it a very brief and very early epoch of rapid, indeed accelerating, expansion, then we can solve both problems.

The basic idea of both solutions is quite simple. It turns out that whatever the value of **Ω** at the onset of the inflationary epoch, **Ω** will be driven close to 1 by the end of the epoch, and will remain close to 1 for a very long time thereafter---including until now. And recalling that **Ω** being one is a matter of a flat Euclidean spatial geometry, we can see the simple idea behind this calculation: an expansion of a highly curved surface makes a local patch flatter. Think of blowing up a balloon; or how the fact that the earth is large makes our local patch of it seem flat.

The situation is similar for the horizon problem. A suitable inflationary epoch changes the spacetime geometry in just the right way: it implies that the past light-cones of all emission events of the CMB---even for points on opposite sides of the sky---do in fact overlap. So with inflation, the cosmos' very early spacetime geometry allows for a suitable process of equilibration that made the CMB's properties so uniform.

The details of these solutions also work out well, in the sense that when one calculates how much inflation, and when, is needed so as to solve these two problems, one gets approximately the same results, despite the two problems being so different. Namely, the solutions are quantitatively correct, if we postulate:

(a) the inflationary epoch ends at about 10^{-34} seconds (which corresponds to a temperature of 10^{28} degrees Centigrade);

(b) the inflationary expansion is exponential, and started at, for example, 5×10^{-35} seconds with a characteristic expansion time (i.e. the time in which the radius of the universe is multiplied by about 3) of 10^{-36} seconds.

Taking (a) and (b) implies that in the course of the inflationary epoch, the size of universe expanded by a factor of about 10^{22}.

Agreed, these are dizzying figures; and the epoch is proposed to occur at times and energies very far beyond those we have confirmed in experiments or observed. So we are undoubtedly in the realm of extreme speculation; and accordingly, caution is in order. It would certainly be reasonable to give no credence to the idea of inflation; nor, therefore, to the details in the rest of this Section and the next. (But if so, the philosophical discussion of explanation from Section 5.5 onwards would still stand.)

Besides: having solved the flatness and horizon problems, i.e. avoided two fine-tunings, by conjecturing a process of expansion, one naturally asks: 'What caused this expansion: what is its mechanism?' For one might well suspect that unless there is such a cause, it is just a coincidence that the same quantitative details about the expansion solve both problems. In answer to this question, the advocate of inflation has, as the saying goes: good news and bad news.

The good news is that a mechanism has been formulated. Indeed: there are many proposed mechanisms which, needless to say, remain conjectural. Most of them involve postulating a new physical field (called 'the inflaton field', and written φ) which evolves i.e. changes over time according to a postulated potential energy function, written V(φ). And fortunately, from such a field and potential one can deduce some characteristic features of the CMB: namely, characteristic probabilities for the amplitudes and frequencies of the slight wrinkles (unevennesses) in the CMB's temperature distribution. And since these features have been observed by a sequence of increasingly refined instruments (mostly on satellites), these confirmed predictions are nowadays regarded as more important evidence that there was a brief epoch of expansion, than the epoch's solving the flatness and horizon problems.

But there is also bad news. The data we now have, and maybe all the data we will ever have, leave wide open which of the many possible mechanisms---which sort of field φ, and which potential V(φ)---actually occurred. (There are two aspects to this: the data leaves open the formal mathematics, e.g. what is the function V(φ); and it leaves open what the physical nature of φ is---it is almost certainly not one of the known fields.)

So far in this Section, we have reviewed two problems that were solved by the idea of an inflationary epoch, and broached the question of what mechanism led to that epoch. Now we are ready for the punch-line: that is, the punch-line for someone interested in the multiverse.

For it turns out that many models of the inflaton field and its potential involve a branching structure in which, during the epoch, countless spacetime regions branch off and themselves expand to yield other universes. (Note that I said 'many models'; so again, caution is in order.) As a result, the whole structure is a multiverse, whose component universes cannot now directly observe (nor otherwise interact with) each other, since they are causally connected only through their common origin during the inflationary epoch.

Besides, in a universe that branches off---often called a 'bubble' or 'domain' or 'pocket universe'---yet another universe can branch off; and also from that one, there can be a branching … and so on. In short: bubbles (domains) spawn more bubbles, endlessly. This idea of open-ended, maybe infinite, branching towards the future is called 'eternal inflation'.

As I stressed at the end of Section 5.1, advocating these bubble universes involves an implicitly Everettian view of the quantum state, i.e. of its assignment of complex amplitudes to classical alternatives. For to say that there is a primordial bubble (with such and such characteristics) implies that there is a corresponding classical alternative. So to propose that all the universes ('bubbles') in the cosmological multiverse are equally real is to assume an Everettian resolution of the quantum measurement problem.

There are two main types of model that involve eternal inflation, labelled 'false-vacuum' and 'slow-roll'. In both types, the inflationary expansion coming to an end is a matter of the inflaton field evolving, i.e. changing over time, to its state of lowest energy. Such states are called 'vacuum states'; or for short, 'vacua'.

So note that here 'vacuum' does not mean 'nothing', or 'no physical system'. Instead, it means 'state of lowest energy'; these are also called 'ground states'. By and large, physical systems tend to lose energy and to evolve to their vacuum states. So also here. Thus eternal inflation is a matter of the inflation field's tendency to get into its vacuum state being prevented: which is usually called 'frustrated'. This frustration usually occurs locally, i.e. in a certain region of spacetime, where the new bubble therefore branches off.

But as I said in this Chapter's Preamble, the details of inflation, especially eternal inflation, involve a lot of advanced physics: which I will omit so as to concentrate on philosophical questions about explanation and confirmation.

But before doing so, I should note that the cosmological multiverse is closely related to the multiverse proposed by string theory: namely through (i) the idea of many vacuum states, and (ii) the project, suggested by such states, of explaining the values of the constants of nature. That is the job of the next Section.

Chapter 5.4: Glimpsing the landscape of string theory

String theory involves, of course, yet more advanced physics. But as I announced in this Chapter's Preamble, I will set aside both its details and how they lead to a fourth multiverse proposal: not just for brevity, but also because the ensuing philosophical questions about explanation and confirmation are much the same as for the cosmological multiverse. So I will confine myself to just two topics. First, I will state the initial idea of string theory. Then I will note that it predicts many vacuum states: and discuss how this makes for strong similarities to the cosmological multiverse, especially as regards these philosophical questions.

String theory is a speculative attempt, that began in the mid-1980s, to unify general relativity's successful account of gravitation with quantum field theory's successful account of nature's other fundamental forces. (These are: the electromagnetic forces between charged particles, and two other forces between sub-atomic particles such as electrons, neutrinos and quarks, which are called the 'weak' and 'strong' forces.) The sense in which string theory aims to unify these forces is like the unification of electric and magnetic forces that Maxwell achieved (Chapter 4.1). Roughly speaking, the four apparently diverse forces are to be revealed as aspects of a single force.

We can glimpse why this is hard to achieve by looking at string theory's initial key idea. Namely, it "does for a string, what elementary quantum theory did for a point-particle"; (hence its name). Thus recall from Chapter 4.2 that elementary quantum theory replaced the state of a classical point-particle—in effect, its single actual position---by an entire function on all possible such positions, mapping each position to a "square root" of a probability (an amplitude) to be found there, if measured. Now, a point-particle is extensionless, and can be thought of as zero-dimensional; while an infinitely thin line, a mathematical curve, is one-dimensional. So just as we can think of classical point-particles as idealizations of tiny spheres, we can think of an infinitely thin line as an idealization of a thin filament---a string, though without the spiral threads. Such a string has, of course, no single position. Each of its constituents points has a position, and the configuration of the string as a whole is the infinite set of those configurations: which we might call a 'placement' of the string. So the quantum replacement of the state of this classical string is a function on all possible placements, mapping each placement to an amplitude … No wonder that advanced physics is needed.

But for this book's purposes, all we need is the upshot: that string theory predicts the system, i.e. the set of all the quantum strings, has very many different possible vacuum states. (This was realized in about 2003: until then, string theorists had hoped there was only one vacuum state.) And it is here that we see the relation to the cosmological multiverse. For some of the models involving eternal inflation say similarly that there are many vacuum states.

Note that here, the jargon can be confusing. For a state that is not the overall lowest-energy state (the state with energy lower than all others), but is only a local minimum with an energy lower than all its near neighbours in the state-space, is often called a 'false vacuum'. (We mentioned this jargon at the end of the last Section.) But it is 'false' only in the sense that the minimum is local. So this jargon is rather like calling a valley in a mountain range a 'false valley', just because it is higher above sea-level than the lowest valley in the entire mountain range. But this analogy with valleys and peaks has also prompted a more helpful jargon. In string theory, the whole state-space, i.e. the set of states of the quantum strings (varying in energy with some lower and some higher), is called '<u>the landscape</u>'.

(Incidentally: biology makes an analogous use of 'landscape'. In the theory of natural selection, the attributes of an organism, or of a population of organisms, make it more or less fit: where fitness is, roughly, a matter of having more offspring who live long enough to reproduce. Over time, natural selection, "the survival of the fittest", increases the proportion of fitter organisms. So over time, the population (descendants of the original organisms) gets a higher fitness "score". So the population "climbs to a peak in the fitness landscape". So high fitness is analogous to high energy: the biology-physics difference being that in biology fitness increases over the generations, while in physics an individual system tends to a lower energy---to a valley, not a peak. From the philosophical perspective of Chapter 3, the interesting point here is of course that this is another vivid illustration of our science being up to its neck in modality: almost all positions in the fitness landscape are not inhabited by a real organism or population---it is a realm of possibilities.)

Returning to physics: the relation between the string and cosmological multiverses is not just a matter of their both involving many vacuum states. It runs much deeper. Without going in to technicalities, we can state two main similarities, which make the two multiverses raise much the same philosophical questions about explanation and confirmation (as I said in this Chapter's Preamble). The first similarity will suggest an ambitious project of explaining the values of some constants of nature. Then the second similarity will suggest this project will stumble, unless one appeals to some philosophically contentious ideas.

First: both multiverses predict that the values of physical parameters that we can measure in experiments, i.e. at the comparatively low energies our experiments can achieve, depend on which vacuum state the system is in. Besides, among these parameters are what we usually call 'constants of nature', because we take them to be indeed constant in value across the cosmos. Examples are: the amount of electric charge on an electron, or the ratio of strengths between the electromagnetic and gravitational forces, or the speed of light.

Note that this uniformity, this "<u>geographical</u> unity of nature", is itself very remarkable. That is: we have no evidence that in some regions of the cosmos, the charge on the electrons there, or the relative strengths of the forces in play there, or the speed of light-signals propagating there, is different from what we measure hereabouts.

Here, I should give two clarifications of my phrase 'which vacuum state the system is in'. First: 'the system' is of course the cosmos. In cosmology, it is usually modelled as a collection of interacting fields, including the inflaton field; in string theory, it is modelled as a collection of quantum strings. Second: my saying 'which vacuum state' was a simplification. For of course the system is in general <u>not</u> in a vacuum state. It is in a state of higher energy; and in string theory or in quantum field theory, these higher energy states manifest as particles, such as electrons. Recall from Chapter 4.3 that in quantum field theory, particles are really energetic excitations of fields; and the "<u>tower</u>" of states, including the zero-particle i.e. lowest-energy state, the various one-particle states, the various two-particle states etc. is called <u>Fock space</u>. And similar comments apply in string theory. But although I simplified, it remains true that all the various higher states got by exciting a <u>given</u> vacuum state will share with it the values of physical parameters mentioned above, like the electric charge on an electron. So it would be more accurate to say: these values depend on <u>which tower</u> (which Fock space) of higher energy states, "standing above" which vacuum state, the system's state is in.

(By the way: we call the charge on an electron a '<u>physical parameter</u>', not a 'physical quantity', to reflect the idea that while a quantity's value varies across the states within a single tower of states, the value of a parameter is the same for all states in a single tower and only varies across towers. Another reason to use the word 'parameter' is of course to avoid using 'constant of nature': for the latter phrase would be misleading, since we now envisage that the value of such a "constant" varies across towers.)

This dependence prompts an ambitious but alluring project. For we now have a framework so broad that it encompasses scenarios (formally: towers of states above certain vacuum states) that differ from each other in the values of some parameters: parameters that we usually call 'constants of nature'---though this framework means we are now envisaging that they vary across a wider landscape. So this suggests we should invoke this framework to answer the obvious big 'Why?' question about such a parameter, namely: 'Why does it have the value that it does?'

But now we are "hit" by the second similarity, between the cosmological and string multiverses, that I announced above.

Namely: in both multiverses, the '<u>many</u> vacuum states' is estimated to be a dauntingly large number. In string theory, a recent estimate is 10^{500}. This is enormously larger than all the numbers in more established branches of physics. For example, the number of elementary particles in our universe, i.e. setting aside the cosmological multiverse, is estimated to be about 10^{100}. So the number of string theory vacua is larger by a factor---not of 400, but of---10^{400}. Similarly, in the cosmological multiverse: estimates of the number of bubble or pocket universes give vast numbers.

Obviously, to explore this set of states, this landscape---i.e. to understand it in quantitative detail, classifying valleys and peaks---is forever beyond human, or even superhuman, ability.

In the face of this impossibility, the project envisaged above, of explaining the values of the parameters, seems to stumble. For as I said in this Chapter's Preamble, the obvious overall strategy for getting such an explanation would be to argue that the value we measure is in some precise sense <u>generic</u>, or <u>typical</u>, of the values across the multiverse; and is thereby to be expected. One aims to explain the actual value we see by showing that is typical, and to be expected. But how can we do that, without understanding the set of states (the landscape, the towers above the vacua) in quantitative detail?

The rest of this Chapter will discuss suggestions for how to do this---albeit contentious ones. Section 5 sets the scene by discussing explanation in general, and formulating: first, the obvious strategy above, which I will label 'strategy (1)'; and another, 'strategy (2)' which invokes selection effects. Subsequent Sections will treat these strategies, in order.

<u>Chapter 5.5: *Angst* about explanation:</u>

Let us for a moment take a step back from the details of physics, and ask: how does one explain any fact? How does one answer any 'Why?' question?

There is a large philosophical literature about explanation, with rival accounts of what an explanation is, and what role explanations fulfil in the enterprise of science. But for our purposes, these accounts' agreements matter more than their disagreements; so this Section will for the most part summarize the agreements. But unfortunately, these agreements will not settle the questions raised at the end of the last Section. So those questions will have to wait for the next two Sections.

The accounts agree that in everyday life what counts as a correct or appropriate answer to a 'Why?' question obviously depends strongly on what the enquirer (and no doubt, also the respondent) knows, what their interests are, etc. These accounts also agree that such contextual and pragmatic features also apply to scientific explanation.

They also agree that in both everyday life and science, there is a spectrum of requirements one can impose on the answer, along the lines of: whether it must be believed by the respondent, or must be true, or even must be known to be true. Again, it is a contextual and pragmatic matter what requirement lying on this spectrum to impose on a would-be explanation for it to count as a genuine explanation.

They also agree on some helpful jargon, from Latin. The fact to be explained (or the proposition expressing the fact) is called the '<u>explanandum</u>', and what does the explaining (or the proposition expressing what does it) is called the '<u>explanans</u>'.

More substantively, and relevant to us: they also agree that what facts count as <u>needing</u> explanation is a contingent, and often historically determined, matter: no less in science than in everyday life. This is not just the obvious points that explanation must come to an end somewhere, and that where the respondent's chain of explanations terminates depends on their state of knowledge, which is a contingent and historically determined matter. (And as every five-year-old who persistently asks 'Why?' learns: where the chain of explanations terminates can depend on the parent's inventiveness, or patience.) There is also the more interesting point that acceptance of a scientific theory (or more loosely: of a research tradition or framework) can influence, even determine, which sorts of fact are taken to need explanation, and which do not.

A standard example of this from the history of physics is Kepler's endeavour (in his *Mysterium Cosmographicum* of 1596) to explain the relative sizes of the planets' orbits, and the number of known planets (viz. 6), by interpolating the five Platonic solids between the orbits. (The known planets were: Mercury, Venus, Earth, Mars, Jupiter and Saturn.) Thus Kepler believed that such a major structural feature of our solar system should have a systematic explanation. But nowadays, we accept, not just that there are more planets---Uranus was discovered in 1781, and Neptune in 1846---but that the sizes of the orbits are "merely" accidents of the history of the solar system. They no doubt have a very complicated causal explanation---if only we could know it. The explanation would involve the various radii (i.e. distances from the sun) at which the planets were first formed, how they interacted gravitationally etc. But these are (at least for the most part) matters of sheer happenstance, about how the solar system happens to have evolved. We do not expect the number of planets, or their orbits' sizes, to have any general or systematic explanation. (Needless to say, this is not to disparage Kepler's endeavour. Given his overall world-view, that there are six planets orbiting the Sun is a main, even pre-eminent, fact about the solay system about which it is very natural to ask 'why 6?'.)

Nor is it only particular facts that can come to be seen as not needing a general or systematic explanation. Very general patterns of behaviour can also fall out of the purview of explanation. (I say 'patterns of behaviour' to set aside controversy about laws of nature (cf. Chapter 3.6); but as we will see, the pattern might well be called a 'law' of a given theory.) A standard example of this is the idea of "<u>natural motion</u>".

In the long history, since the ancient Greeks' geometry and astronomy, of the precise quantitative description of motion, 'natural motion' is an inevitably vague term. But the rough idea is: motion that needs no explanation, since the body is "moving without interference". Thus in Aristotelian cosmology, the natural motion for the element earth (one of four: the others being air, fire and water) was downward---towards the Earth. But by the mid-seventeenth century, the mechanical philosophers (cf. Chapter 2.2) maintained that natural motion was motion in a straight line at constant speed---it was other motion that was "forced". For example, a body's accelerating towards the Earth was due to the Earth's gravitational force; and a block's slowing as it slid down an inclined plane was due to friction with the plane. This came to be called '<u>the principle of inertia</u>'. It was given its first clear formulation by Descartes; and later, it was Newton's First Law of Motion---that a body subject to no force at all moves in a straight line at constant (maybe zero) speed.

Thus for both the mechanical philosophers and Newton, the motion of a projectile in a straight line and at constant speed (neglecting gravity and air resistance) needs, in a sense, no explanation. Agreed: one can ask what causes set this projectile moving, i.e. what launched it. And agreed: the motion, once underway, is an instance of the principle of inertia; and so it can be deduced from the principle. But the motion, once underway, needs no explanation in the sense that <u>causes</u> need to be cited. It is enough that the motion instantiates, and can be deduced from, the principle of inertia. (Here, my 'it is enough' deliberately echoes Hume's and Newton's lowering our sights about the rationalist understanding of nature, discussed in Chapter 2.5 and 2.6).

Finally, these philosophical accounts of explanation also agree on the core idea of explanation: namely, that a <u>successful explanation shows that the explanandum was to be expected</u>.

Here, I choose the words 'successful' 'shows' and 'to be expected' deliberately. Thus the first two words signal how my formulation of the agreement between these accounts deliberately steers clear of some controversies that---irrelevantly for this book's purposes--- dominate the philosophical literature; as follows.

I say 'successful', in order to signal flexibility about pragmatic factors such as the enquirer's interests, and about whether the <u>explanans</u> must be true, or even known to be true.

I say 'shows', in order to signal flexibility about whether: (i) there must be an outright <u>deduction</u> of the (proposition expressing the) <u>explanandum</u> from the propositions comprising the <u>explanans</u> (which would be a strong sense of 'show'), or (ii) it is sufficient to render the <u>explanandum</u> probable (usually in the sense of having a high enough probability, conditional on the <u>explanans</u>).

(Of course, there are other controversies I have not touched on. For example: must an explanation of a individual event or fact (as against a general proposition) cite the causes of the event or fact? And: is explanation fundamentally contrastive, i.e. about answering 'Why A rather than B?' not just 'Why A?')

On the other hand, my third deliberate phrase, 'to be expected', signals a return to the questions at the end of the last Section. Here my deliberate choice of this phrase is to signal that I agree that the phrase is ambiguous: and that it is ambiguous in a way crucial to our concern with multiverse proposals' endeavour to explain the values of parameters such as constants of nature. For the phrase can be understood as referring to either of two different strategies for explaining some fact; in particular, explaining some apparent fine-tuning of a parameter's value. These strategies are:

> (1): showing that the fact: either is deducible (from the <u>explanans</u>); or is generic or typical, i.e. roughly speaking, one of the alternatives that have high enough probability; or
>
> (2): showing that although the fact is not deducible, and is <u>not</u> even generic or typical: it is likely (or has high enough probability) <u>to be observed</u>. (As I mentioned in this Chapter's Preamble: this strategy invokes '<u>selection effects</u>', or (a better-known jargon) '<u>the anthropic principle</u>'.)

The next two Sections explore strategy (1). The subsequent Sections explore strategy (2).

<u>Chapter 5.6: Expected because generic</u>

In this Section, I discuss strategy (1) in general terms, without considering the multiverse---though with examples from physics. I will first cast inflation's answer to the flatness and horizon problems---problems of fine-tuning, in a single universe---as an example of strategy (1). Then I mention some other examples. This will prompt a more general statement of what fine-tuning amounts to. Then I will give a bit more detail about three ways in which one can make precise the idea that the value of a parameter is to be expected, because it is generic. I put them under the labels: 'topology', 'effective field theory', and 'probability'. This Section will emphasize the first two of these. But probability will be a large topic for us, also in connection with strategy (2): so although I will introduce it here, the details will be postponed to subsequent Sections.

So the overall shape of this Section will be to start with fine-tuning as a problem, and to end with two approaches to answering the problem by saying that the parameter's value is in fact generic. Thus the tone of this Section's assessment of strategy (1) will be positive. With the examples and approaches considered here, the strategy has successes. But in the next Section, the difficulties that the strategy faces will move to centre-stage.

So first, let us recall the flatness and horizon problems from Section 3. In both, a certain feature (Ω, and the smoothness of the CMB, respectively) had to be <u>just so</u>. That is: the feature has a value specified to many decimal places, without our cosmological model (i.e. the model that was standard in the 1970s) giving any account of why the value is so tightly constrained---in short, fine-tuning. As we saw, inflation solved these problems by changing the theoretical context so substantially that the required values could arise through an (admittedly, conjectural) dynamical process, from generic initial states. For a suitable inflationary epoch drives Ω to become close to 1 by the end of the epoch, and to then remain close to 1 for a very long time, including until now; and it makes the past light-cones of all emission events of the CMB---even for points on opposite sides of the sky---overlap.

We can now construe this discussion in terms of the last Section's ideas about a successful explanation showing that the <u>explanandum</u> is to be expected, either by deduction, or by getting a high enough probability, from the <u>explanans</u>. And since it is <u>generic</u> initial states (i.e. states before the inflationary epoch) that lead to Ω being close to 1, and to the past light-cones overlapping, the explanation is <u>insensitive</u> to what exactly is the initial state. Such an explanation (or deduction, or calculation of high probability) is often called '<u>robust</u>' or '<u>stable</u>' or '<u>resilient</u>'. In short: here, the phrase 'to be expected', in 'showing the <u>explanandum</u> is to be expected', has the straightforward sense (1) at the end of the last Section.

These examples of inflation solving the flatness and horizon problems also illustrate other ideas from the last Section, such as the ideas that:

(i) explanations inevitably come to an end somewhere; and
(ii) the theoretical context moulds one's judgments about what is generic or probable (or probable enough to count as being explained), and what is not.

For after all, one can ask: (i) what explains (and-or what caused) the pre-inflation state, no matter how generic or probable one accepts it to be; and (ii) what justifies one's judgment about what is a generic or probable enough pre-inflation state. (I will shortly return to this topic of making precise the idea of a state of the whole cosmos being generic or probable.)

But although one can raise these questions, inflation's account of why Ω is close to 1, namely as a robust feature of a dynamical mechanism, is generally agreed to be a successful explanation---even though the dynamics is very conjectural. In short, it is a successful example of strategy (1).

Besides, there are several other examples in physics where a fine-tuned value gets explained as generic by a suitable change in the theoretical context. One such case was the explanation in the early 1970s of some fine-tuning in sub-atomic physics by postulating a new kind of particle (viz. a charmed quark): which was later empirically confirmed.

Indeed, such examples fall in to a wider category: of explaining a value (not necessarily fine-tuned) of a parameter, by a suitable change in the theoretical context---but not necessarily by showing the value to be generic or typical, e.g. by having high or at least moderate probability.

A famous case of this wider category---with the merit that it is simple enough to describe---is Maxwell's explanation of the speed of light. When Maxwell formulated his unified theory of electricity and magnetism (mentioned in Chapter 4.1) he found that some solutions to his equations described waves of the electric and magnetic fields that his theory postulated. That is: the theory described oscillating patterns of (values of) these fields (vectors located at points in our familiar 3-dimensional physical space), which propagated across space at a speed that is a simple function of two fundamental constants (called 'permittivity' and 'permeability') of the theories of electricity and magnetism. When he calculated this simple function from the known constants, the answer was the speed that had already been measured as the speed of light

(300,000 kilometres per second). Maxwell then inferred that light is waves of the electric and magnetic fields.

This is often, and rightly, celebrated as a reduction of one field of physics, the theory of light i.e optics, to another, the theory of electromagnetism (as we now call it). (Here, 'reduction' is meant in the sense of Chapter 3.1: viz. deriving the doctrine of one theory from that of another by augmenting the latter with suitable definitions.) But once light is indeed identified as being such waves, Maxwell's calculation can also be taken as an explanation of the speed of light. In effect the explanation is: 'these waves of the electric and magnetic fields must travel at this simple function of the permittivity and permeability constants; and given the actual values of those constants, the speed must therefore be 300,000 kilometres per second---as is observed.'

So much by way of examples of successfully explaining the value of a parameter. I turn now to formulating a bit more generally what fine-tuning really amounts to: what is the problematic 'just-so-ness' of a parameter's value. The idea will be that the parameter should not be a function of other parameters, that depends very sensitively on those other parameters' values.

To take perhaps the simplest example: the value of a parameter should not be an arithmetical difference of two other physically significant numbers that are nearly equal, but are both vastly larger in magnitude than the parameter itself.

Thus imagine a theoretical framework in which the chosen parameter, which I call p, is a millionth: $p = 10^{-6}$. And imagine this is "because" (i.e. because, according to the given framework) p is the difference of two other numbers, q and r, that themselves have some physically significant interpretations, and that are nearly equal but are both vastly larger than p. For examples, they might have values $10^6 +10^{-6}$ and 10^6. That is: $q = 10^6 +10^{-6}$ and $r = 10^6$ and $p = q - r = 10^{-6}$.

So the imagined framework makes the value of the parameter p fine-tuned. It is extremely sensitive to the exact values of these other numbers q and r: in my example, sensitive to their thirteenth digit. Had q and r been slightly different (in terms of proportions of their actual values, e.g. in their last digit), then p's value would have been vastly different (proportionately) from its actual value. In short: the framework, with its equation $p = q - r$, gives us an unsatisfactorily fragile derivation of p's value, not a robust explanation of it.

(Of course, since the value of a parameter usually depends on a human choice of units, these numbers 10^{-6} etc. should be dimensionless. That is, they should be pure numbers without a physical unit involved. For example, they could be a ratio of two masses, or of two densities, or of two electric charges, or of strengths of two forces.)

This example illustrated the idea of sensitivity with an arithmetical difference being tiny, and so liable to be vastly (proportionately) changed by a change in the numbers whose difference is being taken. But as I said, fine-tuning need not be a matter of an arithmetical difference. The function involved, whose values depend very sensitively on its arguments, could be a very different function. All such cases of fine-tuning prompt strategy (1): the value of a parameter should be shown to be generic or typical, in some precise sense that is defined by an appropriate theoretical framework. (Of course, this framework is often not the one in which the parameter's value is first observed or known: recall for example how Ω was measured to be close to 1, before the framework of inflation was suggested.)

But can we make precise this idea of being generic, in a more general way? That is: can we do so without invoking case-studies, with their case-specific functions like arithmetical difference, and case-specific theoretical frameworks showing the value to be generic? ('Being generic' is sometimes called 'genericity': a word so ugly that I avoid it.)

In my opinion, mathematics and physics provides three overall approaches to doing so. I suggest the labels: topology, effective field theory and probability. I will discuss the first two, which are comparatively specific to mathematics and physics. Then I will briefly discuss probability: which of course extends far beyond mathematics and physics, and which will also occupy us in subsequent Sections.

Topology is a major branch of pure mathematics that focusses on the idea of a continuous transformation: which means, roughly speaking, a transformation that preserves the nearness relations holding between the objects being transformed. Here, being near need not be a matter of a numerical distance. It can be a qualitative relation, and it can come in degrees. Thus topology has jargon like 'closeness', 'neighbourhood' etc.; and a set of objects endowed with such nearness relations is called a 'topological space'. Thus for a transformation T that shifts objects a and b respectively to T(a) and T(b) (in the same or a different topological space), we say that T is continuous if: whenever a and b are near, so are T(a) and T(b). On the other hand, T is discontinuous if there are objects a and b that "get pulled apart" by T, i.e. are such that T(a) and T(b) are not near.

In this way, a discontinuous transformation can express the idea of 'sensitive dependence on the inputs' (here: a and b), even without invoking number-valued functions. Or more precisely: it expresses one version of this idea, without invoking such functions. Applying ideas like these, mathematicians have made precise the idea that an object a in a certain set of objects {a,b,…} is generic, in the sense that it is like (in appropriate respects) the other objects in the space that are near it.

Mathematicians have even defined topological spaces whose objects, i.e. elements a, b, … of the set, are possible physical systems, each taken as subject to certain forces. (Since specifying a system and the forces on it prompts the traditional format of a physics problem, viz. 'For a given initial condition, how will this system change over time?', such spaces are often called spaces of problems.) In such cases, the objects i.e. elements are usually described by a mathematical function such as a potential energy function. So nearness of the objects is a matter of their having functions that are "nearly the same", on some criterion of approximate equality of functions.

I will not go in to these ideas in more detail. But I cannot resist noting that: (i) the jargon of the subject includes the alluring phrases, 'catastrophe theory' and 'structural stability'; (ii) since the elements of such a space are possible physical systems, very few of which are actual, we again see that physics is up to its neck in modality (cf. Chapter 3.3).

I call the second approach to making precise the idea of being generic, or typical, the effective field theory approach. It is like the topological approach, in two ways. It eschews probabilities; (which will be the third approach). And it describes a set of complicated entities with such mathematical precision, both about each entity and about their mutual relations, that the set deserves the name 'mathematical space'. (As discussed in Chapters 3.3 and 4.2, mathematicians call a set that is endowed with various structures, especially structures inspired by geometric or visual intuition, a 'space', even though its elements have nothing to do with points or regions of physical space.)

But there is also a contrast with the topological approach. There, the entities were physical systems, each taken as subject to certain forces; (as noted: often called 'problems'). But in the effective field theory approach, the entities, the elements in the mathematical space, are physical theories, where each theory is identified by, roughly speaking, the set of parameters that occur in its specification of the forces on the systems the theory describes. A bit more precisely: the forces are encoded in a special function: the Lagrangian. Or in some formulations, they are encoded in a "cousin-function", the Hamiltonian. Both the Lagrangian and the Hamiltonian functions have as arguments states in the system's state-space; so they are functions on state-space; and their value is a certain difference, or a sum, of energies of the state. Of course, only a few Lagrangians (Hamiltonians) will be instantiated by an actual physical system. So most of the elements in this space of theories are not actualized. So as in my comment (ii) at the end of the topological approach: we are up to our necks in modality.

The Lagrangian (or Hamiltonian) contains parameters, especially those whose value specifies how strong a force is (called 'coupling constants'). The list of these parameters' values specifies the theory, i.e. the element in the postulated space of theories.

The point of postulating this space of theories---and the link to expressing our topic of being generic---lies in the key idea that the parameters (including, despite their name: the coupling constants) are not really constant. For at high energies, they take different values than at low energies.

So as we mentally traverse a curve in the space of theories, from higher energies to successively lower energies, we can consider the functional dependence of a parameter's values, on various other parameters. In the mathematics, traversing such a curve is a matter, in effect, of summarizing the influence of the physical phenomena at higher energies (the physics described by points on the curve already traversed) on the lower-energy physical phenomena described by the point you are currently at---i.e. the point you are currently considering. A bit more precisely: 'influence' here means 'mathematical implications' not 'causal effects'; and 'summarizing the influence' is a matter of averaging the numerical values of the effects of the higher-energy phenomena. Or in yet more technical jargon: it is a matter of integrating out higher-energy modes of the system.

Traversing such a curve is called 'following the renormalization group flow'. For the variation of the parameters with energy is described by a mathematical structure called 'the renormalization group'. And the approach is called 'effective field theory' because in physics, 'effective' means, not 'efficacious' i.e. 'having a big or strong effect', but: 'approximate in a useful way'. So in physics, an 'effective theory' is a theory which is believed, and often known, to not be completely correct; but which is correct to a sufficient approximation to be useful.

So the overall idea here is that although we do not know, and may never know, the correct theory of physics at the higher energies that our experiments cannot probe, we can hope to formulate an effective theory of physics at the lower energies that our experiments can probe. (The reason for saying 'effective field theory' not just 'effective theory', is that these ideas were developed (in the period 1965-1975), mainly in the context of quantum field theory, and its topic of renormalization. In these developments, Ken Wilson (1936-2013) was a leading light.)

Besides, traversing a curve from high to low energies, in the above way, amounts to deducing what low-energy theory (low-energy point on the curve) is implied by the high-energy theory (point) at which the curve began. More precisely: implied by (i) the high-energy theory, taken together with (ii) the way of summarizing the influence of higher-energy phenomena---the way that the curve defines.

So the question arises: do the curves along which one proceeds, from two different points at some higher energy, towards lower energies, diverge or converge?

If they diverge, that means that small differences in one or other of the parameters of the high-energy theory one started from will imply large differences in one or more parameters of the low-energy theory one arrives at. That is: divergence means the low-energy physics, i.e. the physics we can now observe, is extremely sensitive to the values of at least one parameter describing high-energy physics, i.e. the physics we cannot now, and might never, probe with our experiments. So divergence is bad news. For it means fine-tuning of one or more parameters of the low-energy theory; and we might never be able to probe the high-energy physics on which the parameter depends.

On the other hand, convergence of the curves would mean that the values of parameters describing low-energy physics are robust to variations in high-energy physics. They stay approximately the same, when we envisage different high-energy theories, even substantially different ones. This is good news, in that we can hope to argue, even without knowing the correct high-energy theory, that the value of a parameter at low energies should be (approximately) thus-and-so. In short: we can hope to argue that what we see is generic---whatever the unknown high-energy physics, we would see it.

Finally, I turn to the third approach to making precise the idea of being generic, or typical. Of the three, it is by far the oldest: one might even call it 'venerable'. The idea is to appeal to <u>probability</u>. There should be a probability distribution over the possible values of the parameter, and the actual value should not have too low a probability.

This connects of course with statistical inference: in both everyday life and science, far beyond physics. There, it is standard practice to say that if a probability distribution for some variable is hypothesized, then observing the value of a variable to lie 'in the tail of the distribution'---to have 'a low likelihood' (i.e. low probability, conditional on the hypothesis that the distribution is correct)---disconfirms the hypothesis that the distribution is the correct one: i.e. the hypothesis that the distribution truly governs the variable.

This scheme for understanding typicality seems to me, and most interested parties---be they scientists or philosophers---sensible, perhaps even mandatory, as part of scientific method. Agreed: questions remain about:

(a) how far under the tail of the distribution---how much of an outlier---an observation can be without it disconfirming the hypothesis, i.e. without it being deemed to be atypical;

(b) how in general we should understand 'confirm' and 'disconfirm', e.g. whether in Bayesian terms or in traditional (Neyman-Pearson) terms; and relatedly:

(c) whether the probability distribution is subjective or objective; and more generally:

(d) what probability really means (Chapter 4.11, 4.12); and, after Hume's critique of his predecessors (Chapter 2.4, 2.5), what is the philosophical justification for induction.

But these questions are obviously not specific to physics, let alone our more specific topic of the cosmological multiverse. So I will not pursue them in general terms. But this is not to suggest that they are easy, or irrelevant to our topic. We will see them crop up several times in what follows.

<u>Chapter 5.7: Difficulties about being generic</u>

The previous Section described strategy (1), i.e. explaining a parameter by showing it to be generic; and some of its successes. In this Section, I report some of the difficulties it faces. I will begin with examples, and then move to general issues. These difficulties will prompt us, in the following Sections, to consider strategy (2).

So first, we should note that in recent decades in physics, strategy (1)'s "track-record" has been mixed. The previous Section noted some successes: especially inflation's explaining Ω and the homogeneity of the CMB, and the charmed quark. But by no means every apparently fine-tuned parameter has been explained along the lines of strategy (1). I will report two such recalcitrant examples. As in the previous Section, one example is from cosmology, the other is from high-energy physics. As we will see in later Sections, both examples have prompted some physicists, in particular cosmologists, to shift to strategy (2).

The first example is the <u>cosmological constant</u>. Introduced by Einstein as a possible emendation of this field equations for general relativity, and written as Λ, (i.e. the Greek capital lambda), this constant represents a repulsive force between material bodies. So in a cosmological context, Λ amounts to a cause or tendency for the universe to expand; so it opposes the gravitational force that tends to make matter clump together. And the eventual destiny of a universe that, like ours, is in fact expanding---whether to expand forever, or to come to a stop and re-contract---will be determined by the balance between Λ and gravitation. The current evidence is that the universe's expansion is accelerating: this means that we measure Λ to be positive.

However, we have no good explanation, following strategy (1), of the value of Λ: or even of its approximate value. Worse, theoretical estimates of the value of Λ (using the framework of quantum field theory) are wildly wrong. The estimates are wrong by very many factors of ten: in some estimates, the error is a factor of 10^{120}. (That is, 120 factors of ten: called '120 orders of magnitude'.) This discrepancy is called 'the cosmological constant problem'. It is of course agreed to be a major problem for physics. Regardless of one philosophical views about explanation, and in particular about strategy (1): it suggests a very basic conflict between quantum theory and general relativity. (But as mentioned, we will see that strategy (2) fares better in dealing with it.)

The second example is the mass of the Higgs boson. (This particle, first postulated in the mid 1960s, was discovered in 2012 at the particle accelerator at CERN, Geneva.) This is a dramatic example of the uncomfortable situation I described in the middle of the previous Section. That is: a parameter p is defined as a tiny arithmetical difference of two other numbers q and r, each of which is vastly larger than p and has an appropriate physical interpretation: so that the value of p is very sensitive to (changes enormously with) changes in q and r. My toy example was: $q = 10^6 + 10^{-6}$ and $r = 10^6$; so that $p = q - r = 10^{-6}$.

The mass of the Higgs boson is just such a parameter p: but with the exponent 6 replaced by 14. That is: the mass is a difference of two numbers that, written in decimal notation, match in their first fourteen digits, and then differ in the fifteenth digit. Indeed, uncomfortable.

And again, strategy (1) stumbles here. For the most popular version of strategy (1) for this case is to appeal to an idea that extends the standard model of high-energy physics that (as mentioned in Section 2) was consolidated in the mid 1970s. And this version of strategy (1) turns out to have a fine-tuning problem of its own.

The idea being appealed to here is called '<u>supersymmetry</u>'. We do not need details about it, but can just note the following. Supersymmetry comes in various versions. The "good news" is that some versions imply that the observed mass of the Higgs boson is generic in strategy (1)'s sense: the observed mass lies in a range where it is expected to lie. But the trouble is that in order to have this implication, these versions also imply that that there are other particles with a mass similar to the Higgs: particles that have not been observed. These particles, predicted by supersymmetry, are supersymmetric partners of known particles, and are called '<u>superpartners</u>'. But as I say, no such particles have been observed: not even with masses far from that of the Higgs.

We can put the problem a bit more precisely. The only way that supersymmetry can avoid the embarrassing implication that there are superpartners with a mass similar to the Higgs, is to postulate higher masses for the superpartners: so high that our particle accelerators---more generally, our experiments---cannot detect them. But to postulate this requires . . . fine-tuning the masses of the superpartners.

(A note about jargon: in physics, especially high-energy physics, '<u>naturalness</u>' is used to mean, in effect, the opposite of 'fine-tuning'. So in this jargon: the hope was that supersymmetry would show the Higgs mass to be natural. But the problem is that if all masses are natural, then the masses of the superpartners should be similar to that of the Higgs.)

So much by way of reporting examples where strategy (1) stumbles. I now move to a general statement of the difficulties strategy (1) faces. As I see matters, it is clearest to distinguish:

> (a): a group of difficulties each of which is not specific to cosmology, but arises from the variety, and often, the context-dependence or subjectivity, of the considerations that determine whether something counts as generic, and-or as not needing explanation, so that it can provide, or at least contribute to, an <u>explanans</u>: these are difficulties that we have already seen at various places in our discussion;
>
> (b): difficulties that are specific to cosmology, i.e. that arise from the fact that the system we are concerned with is the entire cosmos: these are difficulties we have touched on, but not focussed on.

I will treat (a) and (b) in turn. In (b), the main case will be probability; and here I will also mention some mathematical difficulties about defining probability distributions. The discussion of (b) will lead in to the next Section.

As to (a), here are three main ways in which we have seen variety, or context-dependence or subjectivity, about what counts as generic, and-or as not needing explanation. I present them in the order in which they came up in the previous two Sections.

First: In both Sections, we discussed how judgments about what is generic or not needing explanation are often moulded by the context of enquiry, and by pragmatic or even subjective factors. For example, recall Kepler's effort to explain why there were six (known) planets in terms of Platonic solids (cf. the start of Section 5.5); and the judgment that an initial state ('initial condition' in the jargon of physics) is generic at least in the sense that it provides an explanans for a later state (cf. (i) and (ii) at the start of Section 5.6).

Second: In what I called the topological and the effective field theory approaches to making 'generic' precise, there is again variety and context-dependence of judgments. This may seem surprising, since these approaches' definitions of notions like a topology or a renormalization group and its flow (on a space of physical systems, or problems, or theories) are, after all, mathematical.

But of course, being mathematical does not imply being unique. In general, a set can have many different topologies defined on it; and similarly, for defining renormalization flows on a set of Lagrangians (or Hamiltonians). Agreed: of the many mathematically consistent definitions, only some will be natural or significant, from the point of view of physics. But in general, the notion of 'physically natural or significant' is too vague and-or ambiguous to pick out a unique definition. So there will be a choice to be made, depending on context or aims.

Third: On the probabilistic approach to making 'generic' precise, there are similar difficulties. For a set can have many different probability distributions (in a more mathematical jargon: probability measures) defined on it. So the obvious question arises: what justifies the one being used? And even after accepting for whatever reason one distribution as correct, or at least as correct for one's purposes: there are the questions that I listed as (a) and (b) at the end of the previous Section. Namely: how far under the tail of the distribution---how much of an outlier---must an observation, for example of the value of a variable, be in order to count as not generic, as atypical? And even after accepting an answer to this, there is the more general question what framework of statistical inference (Bayesian? Neyman-Pearson?) we should adopt. That is: how should we infer from observations to hypotheses about what is the correct probability distribution?

So much for my group (a) of difficulties. I turn to (b): difficulties arising from the system we are concerned with being the entire cosmos. So we return to focus on the main question we first formulated at the end of Section 5.4. Namely: Given a cosmological multiverse of many bubble universes (domains), or the string-theory landscape, across which the value of a fundamental physical parameter, such as the cosmological constant or the electric charge on an electron, varies: How can we implement our explanatory strategy (1), to explain the value as "to be expected"?

As I see matters, there are two main points to make. The first is the obvious point that in this cosmological setting, the difficulties in group (a) are aggravated. For both the cosmological multiverse and the string-theory landscape, the theoretical context is so speculative that rigorous definitions are hardly to be had. So the difficulties are not just about having to appeal to context etc., to single out your preferred precise definition of being generic etc. Also, the daunting complexity of the relevant state-space (recall the 10^{500} vacua at the end of Section 5.4) makes it hard to give rigorous definitions of topologies, or renormalization flows, or probability measures.

We can express this point as a further comment on the problem we first admitted back at the beginning of Chapter 4's discussion of the Everettian multiverse (Chapter 4.6): that no

Everettian knows how to write down the state of the cosmos---the symbol Ψ, with its honorific use of a capital letter, is a promissory note. We also admitted in this Chapter's Section 1 that eternal inflation aggravated this problem, since the envisaged quantum state of the cosmological multiverse is presumably a sum or superposition, over the countless bubbles (domains), of each of their Everettian states Ψ in the sense of Chapter 4 (especially Chapter 4.8's sketch definition of 'world'). That is: the multiverse's state is a sum of Ψ_i, where the label 'i' on the summands labels the different universes. (So there might be dauntingly many states in the sum: in the context of string theory, 10^{500}.) So the present point is the further comment that not only are we unable to write down the state: also we cannot rigorously define such notions as the state being generic, or the appropriate probability distribution on states.

The second point is about <u>confirmation</u>; (a topic which will be developed in the following Sections). Suppose that despite the difficulties above, we could define various probability distributions on the states of the multiverse, and make sense of the idea that one of them is correct. Still, we would face the question: 'How can we gather evidence about which one is correct?'

The problem is obvious. We presumably cannot get empirical data about bubble universes other than the one we are in: for the spatiotemporal connection between "our bubble universe" and any others is through the inflationary epoch, which for us is long gone. (And even apart from inaccessibility: its extreme conditions, for example of temperature, put it so far beyond established physics that we could hardly expect to get interpretable data from it.) But without such data, it is very unclear how we could gather evidence about which probability distribution is correct. After all: our understanding of the phrase 'correct probability' derives from cases where there is a set of actual systems or events (tosses of a coin or coins, rolls of a die or dice, etc.) that are, or are believed to be, suitably similar. We then estimate probabilities by counting the proportions, the relative frequencies, with which certain features (heads or tails, scores on a die) occur. Agreed, there is debate about how best to make these estimates from observed frequencies: a debate addressed by theories of statistical inference---recall questions (a) to (d), at the end of Section 6. But all parties agree that there are deep connections between probabilities and frequencies: so if we do not know any frequencies, how can we guess probabilities?

<u>Chapter 5.8: Biased sampling: Eddington's net</u>

In the last two Sections, I discussed the successes, and the difficulties, of what (at the end of Section 5) I labelled 'strategy (1)' for explaining a fact: namely by deducing it, or showing it is generic, given some appropriate framework or <u>explanans</u>. Here, the sort of fact to be explained has been, since the end of Section 4, facts about the value of a cosmological parameter, or a constant of nature---though we now envisage that such "constants of nature" may vary from one bubble universe to another.

So now I turn to what I labelled 'strategy (2)': explaining a fact, that one admits may not be deducible or generic, by showing that it is likely (or at least has high enough probability) <u>to be observed</u>. In this Section, I discuss this strategy, and how it differs from strategy (1), in everyday terms, regardless of cosmology---to which the next Section will return.

The distinction between these two strategies lies in the fact that what is most probable to occur is not necessarily what is most probable to be observed. That is, we need to distinguish: (a) having high, or high enough, probability (or frequency) in a <u>total population</u> of cases; and (b) having high, or high enough, probability (or frequency) in the <u>sub-population</u> that we observe. The distinction between (a) and (b) arises from something familiar from very elementary applications of probability theory: <u>biased sampling</u>.

For example: when you take a sample from a set, say ten adults from a population of 10,000, in order to estimate the average height, your sample might be <u>biased</u>, in the sense that the frequency of the attributes of interest (here, being such-and-such metres tall) within the sample is different from its frequency in the total population of 10,000. Agreed: some difference in frequencies is to be expected: almost always, the sample frequency does not exactly equal the population frequency (or population probability)---this is called '<u>stochastic variation</u>'. And what counts as a large enough difference to earn the label 'bias' is a matter of how big a difference counts as significant for one's purposes---and so is partly a matter of judgment. For example, the sample might be biased, with large heights more frequent (as a proportion) than in the total population, simply because you chose the ten people from your local basketball club. And whether your consequent over-estimate of the population's average height is large enough to matter will depend on your purposes. For example, a five-centimetre over-estimate would matter if you planned to sell shirts to the population, but not if you planned to sell them lottery tickets.

So far, so obvious. But our interest lies in cases where the sample is biased, not as a matter of coincidence (as might well occur in the example of the basketball club), but as a systematic effect of the method of observation, or data-gathering. This is called a '<u>selection effect</u>'; (or: '<u>effect of observational selection</u>').

A famous example occurred in the 1936 US Presidential election. The incumbent Democratic President, Roosevelt, beat his Republican challenger, Landon, by a large margin. But one magazine had predicted Landon would win, on the grounds that it posted questionnaires to ten million subscribers---of whom about two million responded, mostly favouring Landon. But this was a selection effect. The subscribers were disproportionately Republican, compared with the nation at large; and the subscribers with the interest to send back a response were even more disproportionately Republican.

There is also a famous and vivid metaphor for selection effects, invented by the British physicist Arthur Eddington (1882-1944). In his book, *The Philosophy of Physical Science* (1938), he wrote:

'Let us suppose that an ichthyologist is exploring the life of the ocean. He casts a net into the water and brings up a fishy assortment. Surveying his catch, he proceeds in the usual manner of a scientist to systematise what it reveals. He arrives at two generalisations: (1) No sea creature is less than two inches long. (2) All sea creatures have gills. These are both true of his catch, and he assumes tentatively that they will remain true however often he repeats it.'

To sum up: Fishermen whose net has a mesh of say two inches, and who therefore observe that all the fish <u>in their catch</u> are longer than two inches, should not infer that all the fish <u>in the lake</u> are also longer than two inches. (Incidentally, Eddington intended his metaphor to teach a different and more contentious moral than just 'Beware selection effects'. Namely, a moral about the relation between physics and philosophy---which we will return to in Chapter 6.)

So far, we have thought of selection effects as a bug: as a hindrance to making good estimates of the probability or frequency of an attribute in the total population from which we sample. That is right: they are a hindrance, especially if we do not know the details of how our sample is biased. If we do know those details, we can try to "build in" the details to the procedure by which we make an estimate, so as to compensate for the bias. How to do this is a topic in the statistical theory of estimation: the rough idea is of course to conditionalize one's probabilities on a description of the sampling process. Similarly, if we know only some details, or have only probabilistic information about how the sampling is or might be biased: we "build in" what we know about the process. In short, this is familiar ground in the practice of statistical inference: specifically, in the theory of estimation. There may be practical difficulties about learning the details of the sampling process, and debate within statistical theory about how best to "build in" those details to the procedure for making an estimate. But there is no general or philosophical problem about the need to allow for these details.

But there is also another way to think of selection effects. Namely, as <u>explaining</u> the frequencies that we observe of an attribute (the height of a human, or their political views, or the length of a fish), despite the frequencies being different from those of the total population. It is of course this perspective that is encapsulated by strategy (2).

Again, I think there is no general or philosophical problem about this strategy, although there may well be difficulties about the details of the sampling process. As above, these could include, first, practical difficulties about learning the details. For example, does the fishermen's two-inch mesh really prevent any fish longer than two inches, if such there be, from getting caught? And there could be theoretical difficulties about how the calculation of an estimate should allow for such details. For example, how should my estimate of average height allow for my having sampled heights from a basketball club? Being told to conditionalize on the proposition 'All the people in my sample play basketball' is not much guidance, if I do not know any details about how much playing basketball favours the tall.

I do not mean to downplay these difficulties, whether in physics or in other sciences. In all sciences, the observational process is indeed liable to be biased, i.e. the value of the variable we wish to observe may be correlated with the process; and it can be a hard and complicated matter to recognize this, and to understand it in enough detail so as to compensate for the bias. Just think of the care that goes in to calibrating scientific instruments. But the point is that this is familiar ground in the practice of science and statistics: there is no a general or philosophical problem hereabouts.

Or rather: there is no such problem, outside cosmology. But when we consider cosmology, there may be such problems---as I discuss in the next Section.

<u>Chapter 5.9: Selection effects in cosmology: the cosmological constant</u>

So we return to the main question that we first formulated at the end of Section 4. Namely: Given a cosmological multiverse of many bubble universes (domains), or the string-theory landscape, across which the value of a fundamental physical parameter, such as the cosmological constant or the electric charge on an electron, varies: How can we explain the value that we measure?

And relatedly, since one confirms a scientific theory by its predicting---and one hopes: explaining---results of measurements and observations: how, if at all, can we confirm (or disconfirm) a theory postulating such a multiverse? The trouble is that it is not enough to say that a cosmological theory will assign differing probabilities to various values of such a parameter, of which each bubble universe exhibits one value; and that this enables us to assess the theory by ordinary statistical inference---along the lines that if the observed value is too much of an outlier, in the tail of the probability distribution, we will conclude that the theory is disconfirmed.

Indeed, it is not enough for two reasons. We spelt out the first reason at the end of Section 7. Namely: we measure and observe only our own bubble universe, our own "cosmic parish". So for a parameter that describes an entire such universe, like a "constant of nature", we only get one number---we cannot count frequencies. That is a miserably meagre basis on which to judge which probability distribution is correct. Indeed, it is too meagre even for estimating the average value of the attribute in question: imagine trying to estimate the average height of a population of 10,000 by measuring the height of just one person. And for the cosmological multiverse, we expect the number of bubble universes to be vastly larger than 10,000.

The second reason is, of course, selection effects: which prompt our explanatory strategy (2), that we explain a value of a parameter by its being probable (or at least has probable enough) <u>to be observed</u>. For in the context of measuring fundamental parameters in a multiverse, biased sampling threatens to be a significant problem. The problem is not just that, as in the first reason, each bubble universe exhibiting only one value means the sample size is so small as to be useless, thanks to stochastic variation. Also, established theories in both physics and chemistry show that many of the parameters at issue, such as the cosmological constant and the charge on the electron, are correlated with what the last Section called the 'process of observation'. That is, they are correlated with facts that underpin humans' being able to measure the parameter: such as the fact that life on earth depends on a suitable abundance of carbon and oxygen, or that stars exist with planets orbiting them for times long enough for the complex carbon chemistry of life to evolve. Thus we arrive at last at 'the anthropic principle': which for some fifty years has been the topic of heated debate in both cosmology and philosophy. (The phrase was suggested in 1973 by Brandon Carter (1942 -) a theoretical astrophysicist. People also talk of 'anthropic reasoning'.)

The general idea of these correlations is that modern astronomy and cosmology (as reported in Section 2) has shown our universe to be very unified: not just in what I called the 'geographical' sense that a parameter, such as the charge on the electron, takes the same value throughout the universe (Section 4); but also in the sense that what happens to its smaller parts, such as a star or galaxy, depends on truly global, i.e. universe-wide, features.

A good example is the parameter I mentioned first in this Chapter: the density parameter Ω, which is the ratio of the universe's density to the critical density that would make the final rate of expansion zero. (Cf. Sections 2 and 3. But here we are concerned, not with the speculative inflationary phase perhaps explaining Ω's fine-tuning, but with Ω's value much later on, and so within established cosmology: say after the recombination time 380,000 years after the Big Bang.) If the early universe were very dense, i.e. Ω was much greater than 1, the universe would have re-collapsed in far less than thirteen billion years, so that life could not have evolved; while if Ω was much less than 1, no stars would have formed.

Another example, which I will return to, is the cosmological constant Λ (introduced at the start of Section 7). Since it represents a universal expansion, it having a much larger value than it actually does is like Ω being much less than 1: it would have made the expansion too fast for stars to form.

A third example is the other parameter I mentioned, the charge on the electron: more precisely, the ratio of the strengths of electromagnetic to gravitational forces (which is greater, the greater the charge on the electron). If this had been much smaller than it is, gravitation would have been comparatively stronger, and the universe would have re-collapsed in far less than thirteen billion years, so that life could not have evolved.

These correlations are often "tight", in the sense that they are not probabilistic. They are not a matter of the probability of one proposition, the parameter's value, being altered by conditionalizing on another proposition, such as that there is abundant carbon and oxygen. They are a matter of one value, or range of values, being mathematically dependent on, i.e. a function of, another value or range of values. So these mathematical dependences can be, and often are, summed up in what Chapter 3.7 called a 'counterfactual conditional', along the lines: 'If the parameter had taken a different (or different enough) value than its actual one, then there would be no observations---at least, no observations by humans.' Witness the examples above.

Besides, the correlations are in several cases 'tight' in a numerical sense. Namely, only a narrow range of values of the parameter, such as a few percentage points around its actual value, is compatible with a fact like there being abundant carbon and oxygen. Hence this is also called 'fine-tuning'.

Agreed, this is not stupendous fine-tuning to many decimal places, such as we saw in the flatness and horizon problems (Section 3) and in the problem of the Higgs mass (Section 7). (The fine-tuning of Ω in the flatness problem was 10^{-16}, i.e. a hundred-million-millionth of one per cent.) But each of these stupendous fine-tunings occurred within a single theoretical framework; while here, many such frameworks are in play. For the physical and chemical facts and processes that link global features like cosmic expansion, or the comparative strengths of fundamental forces, to local features like the existence of heavier elements such as carbon, or of life on rocky planets orbiting stars, are very diverse. They range from nucleosynthesis in the early universe, and in stars, through planet-formation and the chemistry of water, to the evolution of life. So it is indeed striking that our established theories of these diverse facts and processes, when conjoined together, provide a "patchwork description" of them that implies these quantitative links, constrained to within a few percentage points.

But this is not to suggest that it is straightforward to spell out these implications: to quantify the correlation. As I said at the end of the last Section, in connection with calibrating scientific instruments: even within a single scientific theory, compensating for the fact that the observational process is biased can be a hard and complicated matter. All the more so, when there are several theories or frameworks in play, and when the parameter in question is correlated via various different mechanisms with various different aspects of our making observations.

For example, there are many different necessary conditions, each scientifically describable, of our observing the charge on the electron. The observer is alive; and life requires---one may well argue---complex carbon chemistry. Carbon requires stellar nucleosynthesis. And the complex chemistry of life requires---one may argue---that a planet orbit a star at a suitable distance (neither too hot nor too cold, like Goldilocks' porridge), and for a long enough time, so that life can evolve. All these correlations, and the mechanisms underpinning them, and these mechanisms' mutual relations, are very hard to disentangle. And this is so even if we somehow settle on some exact definition of 'observation' or 'life'; and it is so even for a single parameter such the charge on the electron, let alone all the physical parameters of interest.

But despite the complexities just mentioned, some examples are comparatively straightforward to calculate. So I end this Section with a bit more detail about one such: Weinberg's explanation of the value of the cosmological constant as an observation selection effect. Weinberg recognized that the requirement that life evolves in an expanding universe of the type considered in the standard model of cosmology is correlated with the value of the cosmological constant, by a single, and comparatively simple, mechanism. Thus he wrote:

> … in a continually expanding universe, the cosmological constant (unlike charges, masses, etc.) can affect the evolution of life in only one way. Without undue anthropocentrism, it seems safe to assume that in order for any sort of life to arise in an initially homogeneous and isotropic universe, it is necessary for sufficiently large gravitationally bound systems to form first . . . However, once a sufficiently large gravitationally bound system has formed, a cosmological constant would have no further effect on its dynamics, or on the eventual evolution of life (1987, 2607-2608; cf. also 1989, p. 7).

So the idea is that the evolution of life constrains the cosmological constant in a simple way, because we can think of (a positive value of) the constant as a long-range repulsive ('anti-gravity') force. Thus one assumes that (i) life can only exist on planets, and (ii) life takes a long time, say billions of years, to evolve. Since (i) requires that matter has the chance to clump together under gravity so as to form planets, the initial expansion cannot be too powerful. That is, there is an upper bound on the cosmological constant. On the other hand, (ii) means that the universe must last long enough for life to evolve. So gravity cannot be so powerful (the initial expansion cannot be so weak) that gravity overcomes the initial expansion in a Big Crunch, well before life has time enough to evolve.

Indeed, the calculation along these lines in 1997, by Weinberg and his co-authors, amounted to a <u>prediction</u> that the cosmological constant was positive. **(?ADD** Cf. Martel et al. (1997), which built on previous work, such as Weinberg (1987, 1989 Section V); Vilenkin (2007) is a fine review of the conceptual issues.) The positive value was only measured in the following year: (though there had been earlier hints).

Chapter 5.10: Confirming a theory of the multiverse

The last Section's report of the fine-tuned correlations, between cosmological parameters and facts about observers, focussed on the one universe we are in. But now let us consider these correlations in the context of the cosmological multiverse.

I shall first state---without repeating all the difficulties presented in Sections 7 to 9---the basic predicament that besets observers in a bubble universe, trying to confirm a cosmological theory. Then I shall sketch a scheme for overcoming this predicament: a scheme which I find clarifying. But I admit of course that it is fiendishly difficult to apply, i.e. calculate with, except in simple toy-models of such theories.

The basic predicament is that when we observe our universe, we are like Eddington's fishermen. Our observations of a physical parameter (e.g. the cosmological constant) are like measurements of the length of fish in the catch. And so we should not infer that in unobserved bubble universes---in domains other than ours---the parameter takes, or is likely to take, a value close to what we observe. For our established theories describe how the parameter's value is correlated with whether the domain has observers in it. So if in some other bubble, those theories are true, or approximately true, and there is no observer there, the value would be different. And if in this other bubble, those theories are badly wrong, i.e. not even approximately true, then anyway---all bets are off about the parameter's value.

The scheme I favour was first proposed by Srednicki and Hartle (about fifteen years ago). It has of course been developed since then: with proposals by such authors as Aguirre, Azhar, Hertog and Tegmark, and by Hartle and Srednicki themselves. But for simplicity and brevity, I will sketch a very simplified version: (the details of which are in Section 5 of Azhar and Butterfield (2016)).

The scheme aims to incorporate appropriately the ideas from both strategies, (1) and (2): the idea of being generic, and the idea of probable (or probable enough) to be observed. And it does this in a Bayesian way.

In a bit more detail, this means: the scheme prescribes probabilities for data D (say, the value of a cosmological parameter such as Ω or Λ) to be observed, conditional on the cosmological theory T, and other propositions encoding e.g. selection effects. (Probabilities like these, i..e probabilities of data or evidence conditional on a theory or hypothesis, are often called 'likelihoods'.) One then uses Bayes' theorem to calculate the probability of the conjunction of T with the other propositions, conditional on the data D. This is called the 'posterior probability' of the conjunction. Then the basic idea of Bayesian statistical inference is that after receiving the evidence or data D, one should set one's credence in the conjunction equal to the posterior probability.

To convey more about how ideas from both strategies, (1) and (2), get incorporated in the scheme, I will begin by summarizing the problems one faces in extracting predictions from cosmological theories of the kinds currently envisaged (including inflation). This will lead in to details about Srednicki and Hartle's proposal, which they call a '<u>framework</u>'.

I summarize the problems under three headings. These headings will echo the difficulties I presented in Sections 7 to 9, about such matters as: the definition of a probability function on a very large space of possibilities, how to specify the "fact about life or observation" that we need to conditionalize on in order to accommodate selection effects, etc. The headings are also a simplification, or amalgamation, of an analysis by Aguirre (2007). Aguirre lists seven problems, or headings, rather than my three: which I call 'Measure', 'Conditionalization', and 'Typicality'. They are as follows.

(1): <u>Measure</u>. As discussed in Sections 6 and 7: What are the elements of the set (called in probability theory: 'the sample space') on which the probability distribution (called: 'measure') is to be defined? Should they be domains, i.e. bubble universes, even though these vary greatly in volume? Or should the elements be spacetime regions of equal volume? Or some other option? And once a sample space is defined: which measure on it should we adopt?

(2) Conditionalization. As discussed in Sections 8 and 9: we need to allow for selection effects. But how exactly should we characterize our observational situation? How detailed should the proposition describing it, on which we will conditionalize, be?

(3): Typicality. As discussed in Sections 6 and 7, there are various problems about how to make precise the idea of a fact (in particular, our explanandum) being generic, or typical. In particular: How much "under the tails" of a probability measure can our observation turn out to be, without our then inferring that the theory is disconfirmed?

This trio of headings leads directly to Srednicki and Hartle's proposed Bayesian scheme for discussing the confirmation of cosmological theories. They define a 'framework' as a conjunction of:---

i): A cosmological theory T (though often cosmologists will say 'model'). This is taken as solving the Measure problem (1) above. So we write a probability P(/T); where the argument-place will be filled by a proposition about the value of a physical parameter, e.g. the cosmological constant;

ii): A 'selection proposition' that describes our observational situation: which is called a 'conditionalization scheme', and labelled as C. So we conditionalize on C as well as on T, and we consider: P(/T, C): where we expect the argument-place to be filled by a proposition D about "our seeing" some specific data;

iii): A probability distribution, denoted by the Greek letter ξ (pronounced ***xi***), and called the 'xerographic distribution' by Srednicki and Hartle. This is defined on those domains, i.e. bubble universes, that have a non-zero measure according to P(/T, C). ξ encodes typicality assumptions: as Srednicki and Hartle discuss, it need not be a flat distribution .

So the idea is that i) to iii) jointly implement our envisaged solutions to the problems (1) to (3) above; and so we consider: P(/T, C, ξ). Srednicki and Hartle call P(D / T, C, ξ) the 'first-person' likelihood of seeing data D. It is a probability "to be observed", rather than a probability "to be", thanks to its conditioning on C and on ξ: i.e. its encoding our observational situation and also the typicality assumption we are making.

Srednicki and Hartle then propose a Bayesian framework to compute degrees of confirmation of the framework, i.e. the conjunction of T, C, and ξ. That is, they use Bayes' theorem to calculate: P(T, C, ξ/ D). So they (and other authors, such as Azhar) show, for various 'toy' cosmological models/theories T (e.g. with finitely many bubble universes, so as to make the Measure problem easier), how various conditionalization schemes C, and typicality assumptions ξ, fare in the light of various data D.

To sum up: I suggest that this Srednicki-Hartle scheme of frameworks is a clear and convincing scheme for handling both selection effects and assumptions of about being generic (typicality). It gives both of them an appropriate role in the endeavour of confirming a theory postulating a cosmological multiverse.