Fine-tuning and Naturalness in the Foundations of Physics

Let us finish by critically analyzing the rationale of arguments from fine-tuning and naturalness in particle physics and cosmology.

Along the way some other numerological coincidences will also be discussed.

Introduction

Scientific method = cycle of hypotheses-generation followed by experimental comparison.

But where do hypotheses come from to begin with?

Scientists do not randomly guess hypotheses

that would waste too much time.

Instead, much of the scientific enterprise today

is dedicated not to testing hypotheses but to selecting hypotheses worthy of test.

All through their education, scientists learn to identify worthwhile research topics

and then judge their own and colleagues' work by the so-acquired experience.

In this discussion, analyze whether "unnaturally" small or large numbers require explanation

and thus whether hypotheses that explain such numbers are promising research topics.

An unnatural number requires explanation when it is in a quantifiable sense unlikely.

Unfortunately, as I will show, most unnatural numbers presently

studied in the foundations of physics are not quantifiably unlikely.

It follows that the corresponding problems of naturalness are ill-defined and might not be problems at all.

Types of Fine-tuning

A good hypothesis must, most importantly, be compatible with already existing knowledge, including both data and requirements of mathematical consistency.

To be interesting, the hypothesis must moreover make plausible new predictions though such predictions may be not be amenable to test in the near future.

In the foundations of physics, the requirement that a hypothesis be mathematically consistent, compatible with existing data and still make a new prediction is difficult to fulfill.

Since physics is a mature discipline, current theories work extremely well already and are therefore hard to improve further.

There isn't even much need for improvement, because the theories that currently constitute the foundations of physics the cosmological concordance model and the standard model of particle physics explain the presently available data just fine.

So what is a theorist in the foundations of physics to do?

The literature reveals that many of the research-efforts in theoretical high energy physics and cosmology focus on a few "big problems."

We will have a closer look at these problems and investigate how problematic they really are.

Naturalness (in general)

Physicists use the word “natural” in general

to mean that a theory’s dimensionless parameters are not much larger or much smaller than 1.

Since any small number can be converted into a large number by taking its inverse,

these two cases do not have to be distinguished from each other.

Physicists usually do not quantify exactly how much larger or smaller than 1 a number may be.

The tolerance for how far away from 1 is permissible depends on any individual’s belief

that such a number may derive from an as-yet undiscovered calculation.

This belief is strongly based on experience. By experience, for example,

it is not difficult to obtain factors of about 100 from powers of 2π that frequently appear.

It follows from the definition of naturalness that two numbers

which are much closer together than each number’s absolute value

are also suspicious and “unnatural.”

That is because in this case the difference between the two numbers would be a small number.

I mention this example because

naturalness problems in physics often originate in such small differences.

The belief in naturalness is usually rationalized by claiming

that numbers which are very large or very small are unlikely.

They appear cherry-picked for no other reason than correctly describing an observation and require

Technical naturalness (in particular)

The notion of “technical naturalness” applies only to quantum field theories in particular.

Technical naturalness, originally formulated by ‘t Hooft,

is a weaker criterion than naturalness in general because it still permits certain small numbers.

A small number is permitted if it has an explanation, typically because it is protected by a symmetry.

To understand technical naturalness,

first note that quantum field theories are energy-dependent.

At higher energies, new processes become possible,

and interactions can decrease or increase in relevance.

This means that the dimensionless numbers (“parameters”)

which appear in a quantum field theory depend on energy;

the “parameters run” as the terminology has it.

The energy in question is determined by the type of experiment

for which one wants to make predictions

and the running of parameters

can be calculated using the renormalization group equations.

Digression on Renormalization Group

In theoretical physics, the **renormalization group** (**RG**) refers to a mathematical apparatus that allows systematic investigation of the changes of a physical system as viewed at different scales.

In particle physics, it reflects the changes in the underlying force laws (codified in a quantum field theory) as the *energy scale* at which physical processes occur varies, energy/momentum and resolution distance scales being effectively conjugate under the uncertainty principle.

A change in scale is called a scale transformation.

The renormalization group is intimately related to *scale invariance*, a symmetry in which a system appears the same at all scales (so-called self-similarity).

As the scale varies, it is as if one is changing the magnifying power of a notional microscope viewing the system.

In so-called renormalizable theories, the system at one scale will generally be seen to consist of self-similar copies of itself when viewed at a smaller scale, with different parameters describing the components of the system.

The components, or fundamental variables, may relate to atoms, elementary particles, atomic spins, etc.

The parameters of the theory typically describe the interactions of the components.

These may be variable couplings which measure the strength of various forces, or mass parameters themselves.

The components themselves may appear to be composed of more of the self-same components as one goes to shorter distances.

For example, in quantum electrodynamics (QED), that we just studied, an electron appears to be composed of electrons, positrons (anti-electrons) and photons, as one views it at higher resolution, at very short distances.

The electron at such short distances has a slightly different electric charge than does the dressed electron seen at large distances, and this change, or *running*, in the value of the electric charge is determined by the renormalization group equation.

End of Digression

The best known case of running parameters are the standard model's coupling constants

which set the strengths of the three interactions.

The coupling constant of the strong nuclear force, for example,

becomes smaller at higher energies,

a property known as "asymptotic freedom."

The other couplings also run with energy.

It follows that if we change the energy at which a theory is applied,
the theory will trace out a curve in an abstract "theory-space."
In this theory-space each point represents a theory or,
since the set of parameters for all possible interaction terms in the Lagrangian
defines the theory, each point is a combination of parameters respectively.
The change of all possible theories with energy is known as the "flow" in theory-space.

To understand technical naturalness,
note now that each theory at high energies is connected
by the renormalization group flow to a theory at low energies and vice versa.
This makes it meaningful to ask what happens with the theory at low energies
if we change the corresponding parameters of the theory at high energies
because the two parameter-sets are related by the renormalization group equations.

To read the literature on the subject, it is helpful to know that particle physicists often refer
to high energies as "ultraviolet" (UV) and
to low energies as "infrared" (IR).
A theory is then said to be technically natural if the theory in the IR
does not sensitively depend on the choice of parameters in the UV.

Phrasing naturalness in terms of a sensitive dependence on the theory at high energies is useful because it allows physicists to quantify just how unnatural a theory is. Several different measures for this have been introduced in the literature, but the exact definitions are not so relevant here. For our purposes it is more relevant to understand the underlying reasoning for why these measures quantify something of interest.

High energies correspond to high resolution and hence short distances. It is therefore basic reductionist reasoning that the theory at high energies is more fundamental. The idea of quantifying naturalness through the sensitivity on the high-energy parameters is then that the compatibility with what we observe at low energies should not require improbable coincidences in the more fundamental theory.

Because we don't know any better, the parameters at high energies could really have had any values, and the precise choice should not affect what happens at low energies where we find the standard model. In this way, technical naturalness reflects the idea of general naturalness, that humans shouldn't cherry-pick parameters.

A theory, then, is technically natural if

getting a low-energy phenomenology

that is within measurement precision of the standard model

is likely if we were to randomly pick the parameters at high energies.

This assumption is ill-defined because

we don't have a probability distribution on parameter space,

neither at low energies nor at high energies.

I will explain which problems this brings in later.

But first some more words on technical naturalness.

I often hear technical naturalness being referred

to as "the UV physics decouples."

I have found this phrase to cause much confusion, so allow me some words of caution.

The change of parameters in the UV,

which is done to quantify technical naturalness,

is not a process that is physically possible.

The theory that describes our world is defined by one particular choice of parameters.

i.e., keep in mind the example of the standard model's coupling constants.

At a given energy, these parameters have some specific values.

We can't change these values because that would amount to changing the laws of nature.

The physics at high energies indeed "decouples"

but that alone is a not a sufficient criterion for a natural theory.

That the physically possible processes at high energies decouple at low energies

means for example that to calculate the orbit of the moon

you don't need to know what the electrons in the moon's atoms do.

Nobody knows why that is so, but this decoupling is evidently a property of nature.

Decoupling is necessary to use effective field theory and is

hence an assumption that underlies the whole framework of the renormalization group already.

This means the UV physics decouples

whenever effective field theory can be used,

regardless of whether or not the theory is natural.

Technical naturalness, therefore, is a criterion
separate from the decoupling of short- distance processes.
It quantifies a sensitivity to a virtual ("mathematical") change of parameters,
not a sensitivity to a change that can actually happen.
Another common confusion is that naturalness is necessary
for the validity of making an expansion of the theory
because the convergence of higher order contributions
assumes that the parameters in the expansion don't unduly increase.
True, but again, while this is certainly an assumption necessary
to make sense of the whole framework on which naturalness builds,
it is not a sufficient (or even necessary) requirement for naturalness.
This can be illustrated by the typical naturalness problems
which are not so much worries about the overall magnitude of parameters
(relevant for the expansion)
but about "suspicious" cancellations between them.
(Indeed such cancellations might help a series converge.)

So why believe in naturalness?

I am not sure just why naturalness has become such an exceedingly popular criterion

to decide whether or not a theory is promising.

A key reason is certainly that the masses of particles in the standard model

are all technically natural, except for the Higgs-boson.

There are further three commonly named historical examples

in which the presence of an unnatural number signaled

that a theory had reached its limits

and some new effect had to show up beyond a certain energy.

These three examples are:

1) the mass of the electron is small compared to the contribution that stems from the self-energy of its electric field (technically infinitely large)

2) the difference between the masses of the two charged pions is much smaller than either one's mass, and

3) the absence of flavor changing neutral currents in the standard model, which signals that a constant in front of a term enabling such processes must be small.

The first two examples were found to be naturalness problems

only after the new processes had been observed (the positron and ϱ-meson, respectively).

The third example lead to the prediction of the charm quark,

to my knowledge the only prediction ever made based on a naturalness-argument.

On the other hand, we now know of at least three failures of naturalness:

The cosmological constant,

the mass of the Higgs boson,

and the strong CP problem.

These will be discussed later.

Fine-tuning In Cosmology

The Cosmological Constant Problem

The universe expands.

And not only that, according to presently available data its expansion is speeding up by the day.

Trouble is, in the framework of general relativity an accelerated expansion

cannot be caused by matter or energy of any type that we know.

An accelerated expansion of the universe requires

a peculiar type of "dark" energy to get the expansion to accelerate.

This energy must grow in proportion to the volume of the universe,

or at least do something very similar to this.

To phrase it differently, the density of dark energy must remain constant as the universe expands.

The simplest type of dark energy is just a constant,

known as the cosmological constant and usually denoted Λ.

Long believed to zero, the best current data put Λ at a small but nonzero, positive, value.

The cosmological constant has units,

so what it means for it to be small requires explanation.

In quantum field theory, the vacuum carries a non-zero energy-density

which derives from virtual particle contribution.

For the standard model, the dominant contribution to the vacuum-energy

– let us call it λ –

is proportional to the fourth power of the mass of the heaviest particle.

The heaviest currently known particle is the top quark,

which has a mass of about 10^{11}eV.

The energy-scale associated with the cosmological constant is about 1/10 eV.

The ratio between the two energies is about 10^{-12}.

The unknown origin of this small number is the cosmological constant problem.

However, the contribution from quantum field theory is not in and by itself observable.

In observables, this contribution

always appears together with a free constant from general relativity.

Let us call this other constant

(sometimes referred to as the “bare” cosmological constant) λ'.

It can be chosen by the requirement that $\lambda + \lambda' = \Lambda$,

i.e., that the two contributions together reproduce the measured value, Λ.

This requirement follows the same logic

by which infinities in quantum field theories can be removed.

By noting that the infinities are not themselves observable

and therefore it is possible to subtract another, suitably chosen, infinity

so that a finite term remains whose value is then determined by measurement.

The same is possible for the cosmological constant,

except that here the subtracted term is (usually assumed to be) finite.

However, since Λ (the observed value)

is much smaller than λ

(the contribution from the standard model), the requirement to reproduce the observed value means that λ'

(the bare contribution from general relativity)

and λ must almost, but not exactly cancel.

These constants are dimensionful,

hence speaking about their absolute values is meaningless.

But the statement can be rewritten without units to say that $1 + \lambda/\lambda'$ must be a small number.

It is here where fine-tuning arguments become relevant.

The typical argument goes like this:

We don't know λ,

so we will assume that it could take on any value

between $-m^4_{\mathrm{p}}$ and m^4_{p} where m_{p} is the Planck mass

and approximately 10^{29}eV.

If the value of the constant is randomly chosen with uniform probability in that interval,

then the probability that it will just by chance almost cancel λ' and leave behind λ is miniscule.

Since the cosmological constant scales with the fourth power of mass,

it is highly sensitive to whatever is the corresponding parameter at high energies.

For this reason the above quoted value for the vacuum energy from the standard model which scales with the mass of the heaviest particle has a theoretical uncertainty that is itself estimated to scale with the Planck mass.

If there are any heavier particles that we have not yet seen, for example, these would dominate the contribution.

That's why the problem is sometimes stated in terms of comparing the Planck density to the (density associated with) the cosmological constant, which results in the (more frequently quoted) 120 orders of magnitude mismatch.

However, regardless of exactly how one formulates the problem, the cosmological constant is both generally and technically unnatural.

The Flatness Problem

The spacial curvature of the universe is presently very small, so small that it's compatible with a flat universe.

The contribution of curvature to the expansion of the universe (the Friedmann equations) however increases relative to the contribution from matter.

To see this, divide the first Friedmann equation through by the square of the Hubble-rate to get a dimensionless expression:

$$\frac{8\pi}{3m_{\rm p}^2}\left(\frac{\rho_0^{\rm r}}{(a\dot{a})^2}+\frac{\rho_0^{\rm m}}{a\dot{a}^2}\right)+\frac{\Lambda}{3}\frac{a^2}{\dot{a}^2}-\frac{k}{\dot{a}^2}=1 \qquad (1)$$

Here, a is the scale-factor,

$\varrho^{\rm r}_0$ and $\varrho^{\rm m}_0$ are some initial values for the density of radiation and matter, respectively,

and k is the (dimensionful) curvature parameter.

Since a increases, the cosmological constant term will eventually come to dominate.

But also the contribution from the curvature term grows faster than the contribution from both radiation and matter.

This means if the contribution from the curvature density is unobservably small today,

in the past it must have been tiny compared to the other densities.

Where does its small initial value come from?

That's the flatness problem.

Again the expectation is that "typical" numbers should be of order one,

while for the universe to be flat,

one needs a factor of 10^{-60} or so to get today's value

to be compatible with observation

(the exact value depends on the time when initial conditions were set

and is not so relevant in the following).

The flatness problem is one of the problems that the theory of inflation

the idea that the universe underwent a phase of exponential expansion

attempts to solve [12].

The supposed problem can be removed by choosing a value that's compatible with observation.

Statements about the value's likelihood cannot be made because the probability distribution is unknown

and cannot be empirically determined because we have only one universe in the sample.

The WIMP Miracle

Weakly interacting massive particles (WIMPs)

are one of the most popular candidates for dark matter.

Their popularity derives from a numerical coincidence,

which is that particles with mass nearby on the electroweak scale

and with a cross-section typical for the weak interaction

would be formed in the early universe

with about the right abundance for dark matter.

Unfortunately the particle have not been detected.

The expected cross-section has been **repeatedly revised** to stay below experimental bounds.

Fine-tuning in Particle Physics

The Higgs Mass

The Higgs is the only fundamental scalar field.

For this reason, the mass of the Higgs-boson receives large contributions from loop corrections,

a problem which does not occur for any other particle.

These contributions are estimated to be of the order of the energy where the theory

(in this case, the standard model) breaks down,

which is close by either the Planck mass

or the energy scale where a grand unified symmetry is (believed to be) restored.

This means the contribution from the loop corrections is at least

13 orders of magnitude larger than the actual mass of the Higgs-boson.

This problem can be remedied by

subtracting the (total) contribution from all quantum fluctuations

and henceforth ignore it because it is not in and by itself an observable.

That the quantum contributions for the Higgs-boson's mass don't make practical trouble

is evidenced by the large number of excellent predictions

which agree with measurements in spite of the supposed problem.

However, the introduction of a new term

that almost but not exactly cancels the contribution from the quantum fluctuations

is thought to be fine-tuned for the same reason

that the cosmological constant is fine-tuned.

If the two contributions had a typical,

almost uniform distribution over an interval

from minus the Planck mass to the Planck mass,

then the probability that they almost cancel is tiny.

The most popular solution to remedy

the unnaturalness of the Higgs-boson's mass was supersymmetry.

In supersymmetric extensions of the standard model,

additional particles appear beyond some energy scale.

Supersymmetry renders the Higgs-boson's mass natural because it enforces a cancellation between different contributions to the mass.

However, the leading contributions to the Higgs-boson's mass

then scales with the masses of supersymmetric partners.

This means if the supersymmetric particles

are heavier than the Higgs-boson itself,

then the naturalness problems return.

The same happens for any other type of new physics
that comes in at some energy scale
which must be beyond what we probed so far.
This is why the data delivered by the LHC
has now ruled out a technically natural explanation for the Higgs mass.
Though I want to add that of course
it is still possible a natural solution exists,
just that it is more complicated than previously thought.

The Strong CP problem

Quantum-electrodynamics is symmetric under a CP-transformation,
which is a combination of changing the electric charge of a particle to its opposite (C)
and changing the parity of the particle (P).
The weak nuclear force violates this symmetry,
as we have known since the 1960s.
The strong nuclear force could violate it but for unknown reasons doesn't
at least no such symmetry violation has been seen in any experiments.

This can be formalized by writing down a contribution to the Lagrangian

that violates the symmetry and saying that the factor in front of it

the theta-parameter, denoted θ

is either very small or zero.

Why this factor is so small is known as the "strong CP problem."

A solution to the strong CP problem

is to turn the theta-parameter into a dynamical field

that takes on a minimal value in a self-induced potential.

This solution works and is technically natural,

but it was noticed quickly after its proposal

that the field would be accompanied by a particle, the "axion". T

he axion was looked for and experimentally ruled out.

In response to this,

the axion models were amended so that the axion became harder to detect.

Dubbed the "invisible axion,"

it has become one of the most popular candidates for dark matter,

though there is still no evidence for its existence.

Gauge Coupling Unification

As we saw earlier,

the three coupling constants of the standard model are energy-dependent.

Their energy-dependence is so that they converge towards each other.

In the standard model, however, the curves do not ultimately meet in one point.

If one adds particles to the standard model which are so heavy

that we have not yet seen them,

this changes the slopes of the running of the coupling constants.

Models in which the three curves meet in one point

are said to allow for “gauge coupling unification.”

Gauge coupling unification is interesting

because if the three interactions of the standard model

arise from one unified theory

with a symmetry that was broken at high energies,

then the gauge couplings should meet at an energy close to the breaking scale.

But while this is an appealing idea, it is neither necessary for consistency nor supported by data.

It has been known since the 1990s that

if one adds supersymmetric partner particles to the standard model,

then the running gauge couplings happen to meet in one point

(up to measurement precision).

This numerical coincidence has been one of the biggest motivators for supersymmetry.

The argument is that such a meeting of curves

would be unlikely to happen by chance and

is hence requires an explanation, like for example supersymmetry.

However, how well the gauge couplings meet in supersymmetric models

depends on the masses of the superpartners.

Since the new data from the LHC has pushed up the lower bounds,

the superpartners – if they exist – must now be quite heavy,

and this makes gauge coupling unification worse.

For this reason, some theorists now argue that additional terms are relevant in the calculation

so that gauge coupling unification can still be maintained in supersymmetric models

(presumably implying that the couplings shouldn't have met without these terms to begin with).

Problems with Fine-tuning Arguments

Circularity

The major problem with fine-tuning arguments both in cosmology and in particle physics

is the reference to probabilities without defining a probability distribution,

or, if a distribution is defined,

the failure to explain where the distribution itself comes from.

It is commonly – most often without stating explicitly –

assumed that the probability distribution is almost uniform

over an interval that (for the dimensionless parameters) stretches from -1 to +1.

A prototypical example is a normalized Gaussian of width 1 around 0,

which you may keep in mind as example for the following.

This means one assumes a width of order 1,

to justify that a probable parameter is of order 1,

which is an obviously circular argument.

It is easy to see that the argument is circular because one could,

e.g., assume a probability distribution with a width of, say, 10^{-14},

which would lead to the conclusion that the "typical" difference between

two randomly picked numbers is of order 10^{-14}.

We get out what we put in. On that account, hence, any number is equally "natural."

A particle physicist would likely object at this point

that a probability distribution which explicitly refers to a small number like 10^{-14}

is itself already fine-tuned.

But this merely brings up the question

what's the probability of a probability distribution and so on,

resulting in an infinite regress

unless some number or distribution is just postulated to be better than all others.

If one wants to remove the problem of circularity

one necessarily has to postulate a probability distribution

which brings back exactly the arbitrary choice

that the criterion of naturalness was supposed to remove.

Naturalness is hence either ill-defined or meaningless.

This need to choose a probability distribution

for measures of technical naturalness used to be well-known.

In 1995, Anderson and Castano, in one of the first papers to quantify technical naturalness,

clearly stated that the choice of a probability distribution

"necessarily introduces an element of arbitrariness to the construction"

But the issue is no longer discussed in today's literature.

Occam's Razor

A probability distribution from which to calculate the most likely choice of parameter
adds unnecessary structure to the theory
and is thus in conflict with the dictum of simplicity.

We could have chosen a parameter and be done with it.

The probability distribution
and all the not-observed values of the parameters
are unnecessary for the derivation of any observable
and they should therefore be stripped by Occam's razor.

Indeed, as I noted earlier,
this is evident by just looking at how practitioners in the field do their calculations.

No one in their right mind would start
with defining a useless probability distribution
over a space from which eventually only one value is needed.

The anthropic prediction for the cosmological constant
is often named as an example for the usefulness of probabilistic arguments.

This argument amounts to guessing a probability distribution,
then adding anthropic priors,
and then deriving a likelihood for our observation.

It is certainly interesting that the so-obtained most likely value
agrees well with actual measurements.
From an axiomatic standpoint, however,
this calculation merely replaces guessing
what we observe by guessing a probability distribution for what we observe.
It's still a guess, albeit one that can't be ruled out because it's probabilistic anyway.

In recent years, arguments using Bayesian inference have become fashionable,
both in cosmology and in particle physics.
Some people seem to believe that this changes anything about the problems with naturalness,
but Bayesian inference just moves the problem
from the choice of a probability distribution to the choice of priors.
The priors are then assumed to be "natural" to justify what is natural.

But it doesn't matter which way one attempts to calculate probabilities,
this will not put naturalness arguments on a solid mathematical footing.
The reason is that, when it comes to the laws of nature,
we don't observe repeated events or sample over many outcomes.

Any talk about probability distributions or priors refers

to the distribution of theories in some mathematical space,

almost all of which we cannot observe.

We have only one set of laws of nature

The multiverse is the assumption

that all of these unobservable theories are as real as ours.

In the case of the multiverse at least the problem of calculating probabilities

is widely acknowledged and has entered the literature

under the name "measure problem."

The attempt here is to calculate a measure

according to which the parameters that we observe in our universe

are (ideally) the most likely ones (compatible with the existence of life).

One can only hope that such a measure would not require

more parameters than the parameters that one can supposedly calculate with it.

But even so, imagine that approach was one day successful

and someone would indeed manage to find a measure

according to which the values of parameters we observe

are the most likely ones, using fewer parameters as input.

It would mean that physicists had discovered

a way to calculate (some of) the parameters of current theories

using a simpler set of parameters by searching

for some optimum of some function.

This would be great,

but all the talk about probability distributions could be removed from this finding.

I want to emphasize that I do not say

one should not pursue such thoughts.

It seems possible to me that reformulating problems

in terms of probability distributions on a multiverse

will help with finding a solution.

Insights sometimes come in unexpected ways.

I just want to make clear that doing so cannot be justified on rational grounds.

No "Fine-tuning," No Theory

Fine-tuning problems arise from an attempt to quantify the probability

of some specific assumptions of physical theories,

that being the numeric values of dimensionless parameters.

But all our theories have many other assumptions

that are chosen for the only purpose of explaining observations.

General relativity, for example,

postulates that we live in a Riemannian manifold,

and quantum mechanics

postulates states are described by vectors in a Hilbert-space (or Fock space, respectively),

and the axioms of Hilbert spaces, and so on.

We also postulate, for no particular reason

other than that it describes what we see that observables are real-valued,

that vacua are stable, and that infinities aren't physical.

None of these assumptions are mathematically necessary.

They're there just because they work.

We could now start discussing what's the probability

to get any set of assumptions which we use

from out of the infinite number of mathematically consistent

sets of axioms that we could have picked.

But we don't discuss this.

That's because the purpose of science is to describe observations

and we simply pick those assumptions which are up to the task.

Why make an exception for numbers?

As an aside, this misunderstanding of the purpose

of a scientific explanation is the origin of most types of multiverses.

They arise because some physicists

refuse to select assumption “just because” they describe observations.

Ambiguous Parameters (Technical Naturalness in Particular)

Any quantification of technical naturalness

uses a specific set of parameters in theory-space,

which are chosen by a certain basis in the expansion of the Lagrangian.

A different choice of basis in theory-space

can be used to remove naturalness problems

by suitably expanding or shrinking certain sectors of the parameters.

Of course one could then complain that such a choice of basis

wouldn’t be natural, but that brings on the question which

– if any – basis is natural and what that even means.

Technical Naturalness hides Finetuning

Let us now consider that we had

a probability distribution at high energies,

so that we could quantify fine-tuning.

Even so, that the standard model without the Higgs is natural
doesn't mean that the theory at high energies is not fine-tuned.
It means that the standard model isn't sensitive
to whether or not the theory at high energies was fine-tuned.
This was not, of course, the reason why naturalness
was introduced to begin with.
But in hindsight, that the masses of the standard model particles
are natural besides that of the Higgs-boson
means the Higgs-boson is the only particle
that allows us to decide whether or not the underlying theory is finetuned.
Such a conclusion could not have been drawn
from the other masses to begin with,
hence projecting it on the Higgs boson wasn't a rational inference.

Let me emphasize that the argument here
is not that it is wrong to think the theory at low energies
is sensitive to the parameters at high energies.

This sensitivity is a property of the theory and I do not question it.

I am questioning the reason to think that this sensitivity matters

or, more to the point,

that the absence of such sensitivity means

a theory is a promising explanation for natural phenomena.

Frequently Asked Questions

Q: Can we explain the preference for numbers of order one by arguing that such numbers more commonly appear in mathematics?

A: No, in math you can find numbers of all sizes and shapes.

It is true that the math-numbers we are exposed to in school – e, γ, π, the Feigenbaum constants, and so on – are of order 1.

But if you dig around a little you find numbers both large and small in mathematics.

A good example is the number of elements of the 16 sporadic groups which takes on values from 8×10^3 to about 10^{54} .

Q: Are you saying we should stop looking for explanations?

A: Of course not.

Any better explanation is a step forward.

My point is that inventing a probability distribution to explain a parameter

just adds unnecessary clutter.

It doesn't explain anything and it's not good scientific practice.

Q: Should fine-tuning arguments be discarded?

A: Fine-tuning arguments work fine if one knowns the probability distribution.

For example, we can make reasonable statements about how probable our galaxy or our solar system is because we have collected statistics from other galaxies and solar systems.

The criticism of heliocentrism based on the argument that the absence of observable parallax implied the stars had to be “unnaturally” far away was wrong for exactly this reason: They had no probability distribution but the erroneously postulated one by assuming that the stars should be likely to have similar distances to the planets as the planets have among each other.

We now understand the distribution of stars and their typical distances comes about dynamically during structure formation and that there is nothing “unnatural” about the distance of our Sun to the other suns.

The relevance of the example from heliocentrism is that progress was not made by choosing the theory that “naturally” explained the absence of parallax by putting earth in the center of the universe.

Instead, the correct explanation was that the small number was probable according to a suitable distribution.

It would be possible to treat fine-tuning arguments as a hypotheses, but it doesn’t presently look as if they’re particularly well-working hypotheses.

Q: What should physicists do instead of obsessing about small numbers?

A: I'd suggest they focus on well-defined problems,

or at least make an effort to come up with well-defined problems.

Given that the standard model is technically natural except for the Higgs-mass,

it seems plausible that technical naturalness

does have a rigorous mathematical basis under certain circumstances.

The question is under which circumstances.

Q. Doesn't relying on Bayesian inference solve the problems with technical naturalness?

A: No, it doesn't.

The Bayesian approach to technical naturalness is merely a different way

to quantify the sensitivity of the low-energy parameters

on the high-energy parameters.

This is a good way to avoid having to pick one particular measure for naturalness.

But I don't question the sensitivity itself;

I question if it is rational to believe a theory less sensitive

to high energies is more likely to be correct.

The Bayesian approach doesn't say anything about this.

The reason that technically natural theories – like eg supersymmetry – come out ahead in Bayesian assessment is that these theories are more rigid in a well-defined sense:

The additional symmetry (which is what makes the theory natural) favors more restrictive models because these models essentially have parameter-correlations built in by way of the symmetry requirement.

These Bayesian assessments, however, do not quantify the presence of the additional axiom which is the symmetry itself (or whatever other assumption it is that makes a model natural).

It is common practice in the literature of Bayesian assessments to compare models with different assumptions (rather than just the same model with different parameters), but that doesn't mean it's good practice.

Of course if I add an assumption – like supersymmetry – which enforces a near-cancellation of parameters, and do not account for that assumption then this model will appear simpler and hence preferable.

But this just moves the question from

what was the probability distribution in theory-space

to what were the priors of these different models to begin with.

And there is no reason to assume that a theory

is more likely to be a better description of nature just because it is more rigid.

Why does it matter?

Physicists' belief that a correct theory should be natural

was the reason many of them thought the LHC

should see additional new particles besides the Higgs-boson.

This has not happened.

It is also the reason why dozens of experiments were commissioned to search for

WIMPs, axions, and signals of a grand unification (like eg proton decay).

We are here looking at billions of dollars of investment.

While theoretical expectations are certainly

not the only reason to commission experiments –

experiments are also driven by technological possibilities

and experimentalists' interest – it is without doubt one of the reasons.

The focus on ill-motivated theories, therefore, creates a vicious cycle in which we attempt to find evidence for unpromising theories by experiments which deliver little guidance on the development of better theories, resulting in more fruitless theories and further experimental null-results.

To showcase the concern, allow me to quote from a recent comment in Nature by Ji Wu and Roger Bonnet [22].

In their comment, titled "Maximize the impacts of space science," the two space scientists advocate that

> "Agency managers should first assess options with the research community to reach a consensus on which scientific frontiers are most likely to yield major breakthroughs."

Naturalness arguments have been extremely important in the foundations of physics to quantify which ranges of parameter-space are promising to look for new phenomena.

Their failure, therefore, requires attention and a revision of method.

Moreover, the continued failure of predictions
in the fields of cosmology and particle physics
erodes public trust in the foundations of physics.
This is unfortunate because it is the area of science in which
we are most likely to find entirely new laws of nature –
provided we look for evidence in the right places.

Conclusions

I have argued here that the popularity of arguments
from naturalness and fine-tuning
in the foundations of physics is problematic.
These arguments are not mathematically well-defined
because they refer to probabilities without a probability distribution.
If one attempts to remove the problem
by defining a probability distribution,
this introduces an arbitrariness which conflicts with naturalness itself,
because naturalness is a criterion invented to remove arbitrariness.

If one does not specify the probability distribution

(which is most often the case),

naturalness remains ill-defined.

Arguments from naturalness are then merely aesthetic criteria

with little historical evidence of being useful.

I conclude that attempts to solve ill-defined naturalness problems are a waste of time.

I will not deny that I too feel that fine-tuning

is ugly and natural theories are more beautiful.

I do not, however, see any reason for why this perception

should be relevant for the discovery of more fundamental laws of nature.

When Beauty Gets in the Way of Science

Insisting that new ideas must be beautiful blocks progress in particle physics.

The biggest news in particle physics is no news.

In March, one of the most important conferences in the field took place.

It is an annual meeting at which experimental collaborations present preliminary results.

But the recent data from the Large Hadron Collider (LHC), currently the world's largest particle collider, has not revealed anything new.

Forty years ago, particle physicists thought themselves close to a final theory for the structure of matter.

At that time, they formulated the Standard Model of particle physics

to describe the elementary constituents of matter and their interactions.

After that, they searched for the predicted, but still missing, particles of the Standard Model.

In 2012, they confirmed the last missing particle, the Higgs boson.

The Higgs boson is necessary to make sense of the rest of the Standard Model.

Without it, the other particles would not have masses, and probabilities would not properly add up to one.

Now, with the Higgs in the bag, the Standard Model is complete.

The Standard Model may be physicists' best shot at the structure of fundamental matter,

but it leaves them wanting.

Many particle physicists think it is simply too ugly to be nature's last word.

The 25 particles of the Standard Model can be classified by three types of symmetries that correspond to three fundamental forces:

The electromagnetic force, and the strong and weak nuclear forces.

Physicists, however, would rather there was only one unified force.

They would also like to see an entirely new type of symmetry, the so-called "supersymmetry,"

because that would be more appealing.

Oh, and additional dimensions of space would be pretty.

And maybe also parallel universes.

Their wish list is long.

It has become common practice among particle physicists

to use arguments from beauty to select the theories they deem worthy of further study.

These criteria of beauty are subjective and not evidence-based,

but they are widely believed to be good guides to theory development.

The most often used criteria of beauty in the foundations of physics

are presently simplicity and naturalness.

By “simplicity,” I don’t mean relative simplicity,
the idea that the simplest theory is the best (a.k.a. “Occam’s razor”).
Relying on relative simplicity is good scientific practice.
The desire that a theory be simple in absolute terms,
in contrast, is a criterion from beauty:
There is no deep reason that the laws of nature should be simple.
In the foundations of physics, this desire for absolute simplicity
presently shows in physicists’ hope for unification or,
if you push it one level further,
in the quest for a “Theory of Everything”
that would merge the three forces of the Standard Model with gravity.

As an ex-particle physicist, I understand the desire to have an encompassing theory for the structure of matter.

The other criterion of beauty, naturalness,
requires that pure numbers that appear in a theory
(i.e., those without units) should
neither be very large nor very small;
instead, these numbers should be close to one.

Exactly how close these numbers should be to one is debatable,

which is already an indicator of the non-scientific nature of this argument.

Indeed, the inability of particle physicists to quantify

just when a lack of naturalness becomes problematic

highlights that the fact that an

unnatural theory is utterly unproblematic.

It is just not beautiful.

Anyone who has a look at the literature of the foundations of physics

will see that relying on such arguments from beauty

has been a major current in the field for decades.

It has been propagated by big players in the field,

including Steven Weinberg, Frank Wilczek, Edward Witten, Murray Gell-Mann, and Sheldon Glashow.

Countless books popularized the idea that the laws of nature should be beautiful,

written, among others, by Brian Greene, Dan Hooper, Gordon Kane, and Anthony Zee.

Indeed, this talk about beauty has been going on for so long

that at this point it seems likely most people

presently in the field were attracted by it in the first place.

Little surprise, then, they can't seem to let go of it.

Trouble is, relying on beauty as a guide to new laws of nature is not working.

Since the 1980s, dozens of experiments looked for evidence
of unified forces and supersymmetric particles,
and other particles invented to beautify the Standard Model.
Physicists have conjectured hundreds of hypothetical particles,
from "gluinos" and "wimps" to "branons" and "cuscutons,"
each of which they invented to remedy a perceived lack
of beauty in the existing theories.
These particles are supposed to aid beauty, for example,
by increasing the amount of symmetries,
by unifying forces,
or by explaining why certain numbers are small.
Unfortunately, not a single one of those particles has ever been seen.
Measurements have merely confirmed the Standard Model over and over again.
And a theory of everything, if it exists, is as elusive today as it was in the 1970s.
The Large Hadron Collider is only the most recent
in a long series of searches that failed to confirm those beauty-based predictions.

These decades of failure show that postulating new laws of nature
just because they are beautiful according to human standards
is not a good way to put forward scientific hypotheses.
It’s not the first time this has happened.
Historical precedents are not difficult to find.
Relying on beauty did not work for Kepler’s Platonic solids,
it did not work for Einstein’s idea of an eternally unchanging universe,
and it did not work for the oh-so-pretty idea, popular at the end of the 19th century,
that atoms are knots in an invisible ether.
All of these theories were once considered beautiful,
but are today known to be wrong.
Physicists have repeatedly told me about beautiful ideas
that didn’t turn out to be beautiful at all.
Such hindsight is not evidence that arguments from beauty work,
but rather that our perception of beauty changes over time.

Physicists must, first and foremost, learn from their failed predictions. So far, they have not.

That beauty is subjective is hardly a breakthrough insight,

but physicists are slow to learn the lesson

—and that has consequences.

Experiments that test ill-motivated hypotheses are at high risk to only find null results;

i.e., to confirm the existing theories and not see evidence of new effects.

This is what has happened in the foundations of physics for 40 years now.

And with the new LHC results, it happened once again.

The data analyzed so far shows no evidence

for supersymmetric particles, extra dimensions,

or any other physics that would not be compatible with the Standard Model.

In the past two years, particle physicists were excited

about an anomaly in the interaction rates of different leptons.

The Standard Model predicts these rates should be identical,

but the data demonstrates a slight difference.

This “lepton anomaly” has persisted in the new data,

but—against particle physicists’ hopes—

it did not increase in significance, is hence not a sign for new particles.

The LHC collaborations succeeded in measuring the violation of symmetry
in the decay of composite particles called "D-mesons,"
but the measured effect is, once again,
consistent with the Standard Model.
The data stubbornly repeat: Nothing new to see here.

Of course it's possible there is something to find
in the data yet to be analyzed.
But at this point we already know that all
previously made predictions for new physics were wrong,
meaning that there is now no reason to expect anything new to appear.
Yes, null results—like the recent LHC measurements—are also results.
They rule out some hypotheses.
But null results are not very useful results if you want to develop a new theory.
A null-result says:
"Let's not go this way."
A result says:
"Let's go that way."
If there are many ways to go, discarding some of them does not help much.

To find the way forward in the foundations of physics,
we need results, not null-results.
When testing new hypotheses takes decades of construction time
and billions of dollars, we have to be careful what to invest in.
Experiments have become too costly to rely on serendipitous discoveries.
Beauty-based methods have historically not worked.
They still don’t work. It’s time that physicists take note.

And it’s not like the lack of beauty is the only problem
with the current theories in the foundations of physics.
There are good reasons to think physics is not done.
The Standard Model cannot be the last word,
notably because it does not contain gravity
and fails to account for the masses of neutrinos.
It also describes neither dark matter nor dark energy,
which may be necessary to explain galactic structures.

So, clearly, the foundations of physics have problems that require answers.
Physicists should focus on those.

And we currently have no reason to think that colliding particles
at the next higher energies will help solve any of the existing problems.
New effects may not appear until energies are
a billion times higher than what even the next larger collider could probe.
To make progress, then, physicists must,
first and foremost, learn from their failed predictions.

So far, they have not.
In 2016, the particle physicists Howard Baer, Vernon Barger, and Jenny List
wrote an essay for *Scientific American* arguing that
we need a larger particle collider to "save physics."
The reason?
A theory the authors had proposed themselves,
that is natural (beautiful!) in a specific way,
predicts such a larger collider should see new particles.
This March, Kane, a particle physicist,
used similar beauty-based arguments in an essay for *Physics Today*.

And a recent comment in *Nature Reviews Physics*
about a big, new particle collider planned in Japan
once again drew on the same motivations from naturalness
that have already not worked for the LHC.
Even the particle physicists who have admitted their
predictions failed do not want to give up beauty-based hypotheses.
Instead, they have argued we need more experiments to test just how wrong they are.

Will this latest round of null-results
finally convince particle physicists that they need new methods of theory-development?
I certainly hope so.

As an ex-particle physicist myself,
I understand very well the desire to have
an all-encompassing theory for the structure of matter.
I can also understand the appeal of theories such a supersymmetry or string theory.
And, yes, I find quite fascinating the idea that
we live in one of infinitely many universes that together make up the "multiverse."
But, as the latest LHC results drive home once again,
the laws of nature care heartily little about what humans find beautiful.