Measure, Topology and Probabilistic Reasoning in Cosmology

Urbino International School in Philosophy of Physics XXVII
"The Philosophy of Dark Energy"
06 Jun 2024

Erik Curiel

Lichtenberg Group for History and Philosophy of Physics, Bonn Universität

Black Hole Initiative (BHI), Harvard University

Quantum Information Structure of Spacetime Consortium (QISS)

erik@strangebeautiful.com
http://strangebeautiful.com

(partially funded by an ERC Collaborator grant to Bonn
and a John Templeton Foundation grant to QISS)

Outline

recall from yesterday:

- testing theories of dark energy requires a great chain with many links joining high theory to material measures
- far more than a sudden juxtaposition of, on the one hand, the EFE, modifications thereof, models of novel scalar fields, . . . , and, on the other hand, the “simple readout” of observation
- the point: *it is not just theories of dark energy under test*
- tests *test more* than bare fundamental laws—they engage the long chain of theory, solutions, approximation, simulations, numerical calculations, . . .
- the very idea of a test in this sphere is more rightly attributed to *that whole chain* of links binding equations and other theoretical structures to the structured observational data themselves the product of much theory
- an expansion of the too-simple philosophical picture of a pure and simple theory/observation dichotomy. . .

- it is appreciated that one must analyze and control for errors and selection (and other) biases in empirical data, in ever more sophisticated ways as precision and resolution increases
- perhaps not so widely recognized that theoretical models, both in themselves and when used to structure and interpret data, *require the analogous*: delimiting the *regime of applicability* of those theoretical tools
- we must have the theories and their relations to possible observations under *epistemic control*

epistemic control includes at a minimum:

1. understanding the physical theory's regime of applicability and so, *a fortiori*, its breakdown scales
2. understanding the theory's relations to other valuable theories (theories precedent to the theory itself, *e.g.*, NGT *vis-à-vis* GR, and theories laterally and antecedently related, *e.g.*, fluid dynamics *vis-à-vis* solid state and *vis-à-vis* molecular kinetics, respectively)—not just by approximation and limiting relations and such, but also *relations among concepts*
3. understanding what will and will not count as evidence in favor and against
4. understanding the asymptotics of physically important quantities and dynamics
5. understanding what approximations and idealizations are justified
6. understanding the conditions under which we do and do not need explicit schematization of the observer, and understanding how to do it
7. understanding as well what kinds of physicalities the different parts of the physical theory's formalism and structures all respectively can have and when ("exist or not" is too crude)

- it is characteristic of appropriately unified kind of physical system, treated by a single theory, that there exist a set of scales at each of which all theoretical quantities simultaneously lose definition
- every theory, in so far as it treats an appropriately unified kind of physical system, not only has a regime of applicability, but it has a *single, unified* one, bounded on all sides by scales characterized by the values of different combinations of its quantities
- for Navier-Stokes fluids, *e.g.*, the definitions of pressure, fluid flow, viscosity, . . . , break down at spatial and temporal scales a few orders of magnitude greater than the mean free-path of the constituent molecules
- all quantities also lose definition when the fluid enters a strong enough state of turbulence, which can be characterized by (*inter alia*) a ratio of the fluid's kinetic energy to a measure of its viscous damping—a scale independent of that characterized by the mean free-path

This seems, indeed, to be one of the markers of a physical theory:

- the existence of a set of characteristic scales for its physical quantities
- at each of which all the theory's physical quantities simultaneously lose definition—
- "places" where all the kinematically and dynamically relevant structures of the theory break down all at once
- in the sense that the theory becomes inadequate for an appropriate treatment of any system beyond the determined boundaries

- although we perhaps naively tend to think of scales determined by spatial, temporal and energetic quantities when considering how and where theories break down in their capacity to provide sound representations of phenomena, any quantity in any theory can provide such a measure
- velocity provides a breakdown scale for Newtonian mechanics
- acceleration and scalar curvature provide different breakdown scales for various theories of gravity, such as GR
- no breakdown scale, moreover, can be a single number holding for all systems the theory treats
- Navier-Stokes theory, for instance, becomes inadequate for different fluids at different energies and spatial and temporal scales

- often it is not a bound on a single quantity, such as a value of energy, a value of spatial length, *etc.*: classical Maxwell theory, *e.g.*, breaks down when the ratio of the field's amplitude to its frequency approaches $\hbar$
- nor is it ever the case that there is a single characteristic scale for each theory
- Navier-Stokes theory breaks down:
  1. when various measures of flow complexity indicate the fluid is approaching turbulence
  2. when the fluid is too viscous
  3. over time scales comparable to equilibration time after a sharp disturbance
  4. when temperatures become large enough that heat loss due to emission of blackbody radiation becomes non-neglible
  5. when ambient electromagnetic field becomes strong enough to ionize the fluid's constituent molecules
  6. and on and on

- all of which shows, moreover, that sometimes a breakdown scale is determined by physical quantities not even representable in the theory (such as the electromagnetic field for Navier-Stokes)
- sometimes, moreover, approximations used to construct models of particular behavior, such as surface waves in fluid dynamics, have characteristic breakdown scales different from those of the material in which the phenomena manifest (Lamb 1932, ch. IX)
- in such cases, the theory can provide appropriate and adequate models of the systems in the relevant states, only not in the way the approximations definitive of the behavior at issue require

A breakdown scale, then, is something like the following:

> a measure of or function of or relation among quantities, such that, when the joint state of the system and its environment imply that the values of some of the system's quantities do not satisfy the measure, function or relation, then the theory can no longer provide good models of the system

- breakdown scales can *never* be determined by analysis of *the formalism and theoretical machinery of the theory alone*, without input from knowledge acquired by experimentation in particular and empirical investigation in general
- they are rather fixed by knowledge that one can gather only from investigations grounded in that part of the epistemic content of the theory not captured by the formalism by itself, largely in experimental and observational practice
- as such, they change with the increasing scope and depth of our experimental reach

what does it mean to say that the theory cannot provide good models of systems outside its breakdown scales?

- one of the most important markers of this is that the system's quantities lose unambiguous definition with respect to the theory's resources for modeling them
- for a Navier-Stokes fluid, for example, different sorts of thermometers that allow spatial discrimination on scales only a couple of orders of magnitude greater than the mean free path of the fluid's molecules will record markedly different "temperatures" depending on characteristics of the joint system that one can ignore at larger scales—the fine details of the fluid's convective flow in relation to the geometry of the thermometric system, for instance, and even the transparency of each thermometer to the fluid's particles[1]
- as the fluid approaches turbulence, to take another example, the values of all its quantities begin to vary rapidly in time and eventually cannot be measured by any conventional means—the quantities are no longer well defined

---

1. See, *e.g.*, Benedict (1969) for detailed exposition of the complex interplay among theory, model and experiment one must take account of in attempting to define a physical quantity such as temperature based on the behavior of real measuring devices.

without knowledge of a theory's regime of applicability, and in particular when a given system of interest lies in its scope, we have *no grounds whatsoever* for trusting what the theory purports to say about the system

– but it is exactly such knowledge we lack for all theories about dark energy

– we have no idea what their respective regimes of applicability are, for we have no direct experimental or observational knowledge of their postulated constituents

The vague accuracy of events
dancing two by two with language,
which they forever surpass

– William Carlos Williams
Paterson

'represent'

standard philosophical senses of designative, depictive or verisimilous representation of world by mathematics of physical theories

e.g.:

- Tarskian relation of designation between elements of mathematical space ("the space of states") and states of physical system: semantic view (designative—Suppes 1960; van Fraassen 1980)
- or possible-worlds semantics (depictive—Lewis 1970; Butterfield 1984)
- or existence of "homomorphism" kind between mathematical stuff and stuff in the world *à la* the structuralists (verisimilous—da Costa and French 2003; Giere 2010)

The Motto

meaning is fixed by ontology

- even instrumentalists and empiricists (*e.g.*, Carnap and van Fraassen) subscribe to (*e.g.*) Tarskian-like semantics such as to give empirical content to the mathematical formalism of theories
- $\Rightarrow$ meaning of mathematical formalism determined by standard representational relations with respect to a fixed ontology
- even though not realists about ontology

natural accompaniment:

empirical content accrues to the mathematical formalism of theory largely if not wholly by virtue of this kind of representation—any physical significance the mathematics has derives from it

van Fraassen (1980, p. 8, his emphasis):

Science aims to give us, in its theories, a literally true story of what the world is like; and acceptance of a scientific theory involves the belief that it is true. This is the correct statement of scientific realism.

the breakdown of theories

we use theories to model systems, and to model them well, *i.e.*, fruitfully, in a way that teaches us much and also displays our conceptual mastery of them and their behavior. . .

even when the systems are in states such that the models are in no way predictively accurate

when, that is, there can be no question of the math representing the system in any standard sense.

- predictive accuracy of Navier-Stokes theory irremediably breaks down, *e.g.*, as a fluid approaches turbulence—it leaves its *regime of adequacy*
- nevertheless, even while the dynamical equations of the theory no longer yield accurate predictions by any reasonable measure...
- other parts of the theory, the *kinematical relations and constraints* among its quantities (*e.g.*, that the shear-stress tensor is symmetric, and that heat flux is always independent of the pressure gradient), still are meaningfully applied to model some aspects and features of the system in those states
- it is still in its *regime of propriety* (Curiel 2017, 2025)

- we use those kinematical, *not the dynamical*, relations, among other purposes, to guide the design of instruments to probe the systems (Curiel 2025)
- indeed, satisfaction of the kinematical constraints *is necessary for the dynamics to be well posed* (Curiel 2022)
- $\Rightarrow$ *cannot even investigate the possible truth of dynamical claims unless kinematical constraints are satisfied*
- shows that the theory can capture something of deep physical significance about the systems even when it is not wholly descriptively, much less predictively, accurate of them

but, again, there can be no question of standard representation without predictive accuracy *standard views cannot capture at all a large part—I think, indeed, the large majority—of the empirical content of physical theories*

– *indeed, standard views of representation cannot even get off the ground without the prior fixing of a regime of applicability*

– *an almost wholly pragmatic affair that requires much of the meaning of the theory's terms to have been already fixed, and which in turn provides the basis for much of that meaning* without knowledge of the theory's regimes of propriety, of adequacy, of applicability—

we have no epistemic control over the theory at all

- one of the problems with traditional confirmation theory (a prejudice inherited from Popper):

  any discrepancy between theory and observation theory is viewed as a disconfirmation
- but that is deeply wrong, from an epistemological standpoint
- often, a violation rather serves to show the boundary of the regime of applicability
- other times, it shows merely that we are awaiting better calculational techniques, or the capacity to incorporate more details in our models, which we suspect to be relevant but do not yet know how to handle
- the interesting question, then, is when a mis-match between prediction and experimental result constitutes a crisis (to use the Kuhnian locution) for a theory, a true anomaly

consider the case of the calculation of Lithium-7 abundance due to early universe nucleosynthesis, a prediction of ΛCDM

- the calculated prediction differs from observed values by a factor of at least 2 or 3
- but no one seriously worries that this constitutes a crisis for QFT, GR or ΛCDM:
    1. the two main sources of observational data from which measured abundance values are calculated—baryon-to-photon ratios derived from the CMB, and the abundance directly observed in Population II stars—themselves disagree, suggesting that better measurements are needed to truly test the predictions
    2. while the theoretical discrepancies with both measured values are "relatively small", they are still unacceptable but nonetheless not so gross that it freaks people out
    3. $\Rightarrow$ because we can plausibly believe that more accurate calculations, taking into account more and finer details of interactions and dynamics, higher-order effects, and so on, will significantly lessen the discrepancies

- no one has yet argued convincingly that this cannot be done, as Le Verrier (1859) did for Mercury's anomalous precession, generating a true crisis in astronomy and the theory of Newtonian gravitational theory in the second half of the 19th Century
- in light of the fact that successive theoretically improved models, taking ever more systematic dependencies of the systems into account, and observationally improved data have whittled the discrepancy for Lithium down, we feel soothed and comforted (the method of successive approximations—Smith 2014; Harper 2011; Smith and Seth 2020)
- it's nothing like the case of Mercury, where everything was tried, both theoretically and experimentally, and nothing worked to lessen the discrepancy even the slightest bit

A man said to the universe:
"Sir, I exist!"
"However," replied the universe,
"The fact has not created in me
A sense of obligation."
— Stephen Crane

common forms of questions

- probability of particular kinds of observations, given our cosmological situation
- probability that value of universal constant lies in fixed range
- probability that initial conditions of particular kind occurred
- probability that particular large-scale structures would form or local features obtain
- probability that a particular global (causal, topological, projective, conformal, affine, metrical) property obtains

common examples

- probability that observers such as ourselves would come to exist in the sort of spatiotemporal region we occupy in a spacetime of this sort
- probability that we are "typical" observers in the universe
- probability that conscious observers exist at all
- probability that the cosmological constant has any non-zero value, and has, moreover, the one actually observed
- probability that a generic spacetime is singular to the future
- various "fine-tuning problems": approximate flatness of observed universe; approximate homogeneity of observed universe; seemingly required special entropic state of very early universe; seeming exact canceling of Λ_v by Λ_E

common ways to try to make questions precise

- fix non-probability measure on family of spacetimes, look for properties forming a set with large or with zero or near-zero measure
- fix topology on family of spacetimes, look for properties forming an open, dense set or a nowhere-dense set
- statistical mechanics and thermodynamics: standard considerations give rise to appropriate probability distributions on a family of spacetimes
- anthropic principles: existence of conscious observers, or of large-scale or local structure of a particular form, or of . . . , places sufficient constraints to determine an *a priori* probability distribution on family of spacetimes

Most Common Way

1. no measure is defined, but assumed that there really is a natural, appropriate one
2. a topology is crudely postulated
3. hands are waved at vague notions of properties forming open or nowhere-dense sets
4. underlying intuition is implicitly, silently invoked that "real underlying measure" is consonant with topology
5. conclusion:
    - "open" $\Rightarrow$ "generic", "highly likely"
    - "nowhere-dense" $\Rightarrow$ "rare", "highly unlikely"

one is generally also concerned about the stability of these conclusions “under small perturbations”

(consonance of measure and topology)

Problems

1. meaning of probability, when there is only one physical system of the type at issue to observe
2. justification, kinds of evidence available, when one cannot measure frequencies
3. isolation, clarification and justification of various assumptions one must make in order to apply probabilistic reasoning
4. physical significance of measures and topologies, relevance to property or feature at issue
5. relationship between topological and measure-theoretic concepts and methods, especially in infinite-dimensional cases

“generic” $\approx$ “most systems are similar in this respect” “most” is a measure-theoretic notion

“similar in this respect” is a topological notion we need a Borel measure

(the measure is defined on all open sets, the Borel sets)

"large is open and dense, small is nowhere-dense"

(If the topology arises from or is compatible with a complete metric, one can use the more general criterion that a "large" set be a G_δ-set, *i.e.*, that it be a countable intersection of open dense subsets, in so far as the complement of a G_δ-set in this case is nowhere dense. In the same case, one can use the more general criterion that a "small" set be *meagre*, *i.e.*, that it be a countable union of nowhere dense subsets, in so far as the complement of a meagre set in this case is dense. This is known as the Baire Category Theorem.)

"stable" $\approx$ "small changes in values shouldn't significantly change likelihoods of being similar" “small changes in values” is an algebraic notion (addition, multiplication, translation)

we need an appropriately translation-invariant
Borel measure

(not always in physics: *e.g.*, the exponential distribution for radioactive decay; but here we need it)

vague conceptual speculation

in physics, topology is generally prior to, more fundamental than measure in probabilistic reasoning:

- we need to characterize similarity before we can formulate comparison class (σ-algebra) to achieve quantitative rigor of assigning exact probabilities
- we need to show stability to trust our testing of the predicted probabilities

four standard topologies on families of spacetimes

family of cross-sections $\mathfrak{G}$ on the fiber bundle of Lorentz metrics over a candidate spacetime manifold M (connected, paracompact, Hausdorff, four-dimensional):

1. compact-open
2. Whitney (open)
3. Sobolev
4. parameter topology

Sobolev is strictly finer than Whitney, which is strictly finer then compact-open; in general, no strict relation to parameter topology (we'll discuss this later)

idea: fix standard of "distance" between Lorentz metrics by fixing arbitrary positive-definite metric on M, use it to assign magnitudes to the algebraic differences of Lorentz metrics

compact-open topology

A neighborhood of $g_{ab} \in \mathfrak{G}$, $\mathcal{N}(h_{ab}, K, \epsilon; g_{ab})$, determined by a positive-definite metric h_{ab} on M, $K \subset M$ compact, and real number $\epsilon > 0$. $g'_{ab} \in \mathfrak{G}$ is in the neighborhood if and only if

$$h^{mn} h^{rs} (g_{mr} - g'_{mr})(g_{ns} - g'_{ns}) < \epsilon$$

everywhere in K.

$\Longrightarrow$ cares only whether metrics are similar on bounded regions in interior of spacetime; doesn't care about relative asymptotic behavior; the coarsest physically reasonable topology to use on $\mathfrak{G}$ (jointly continuous)

example (Geroch 1971)

- $\mathbb{R}^4$, Minkowskian coordinate system (t, x, y, z)
- sequence of metrics: diag$(t_m, -1, -1, -1)$, for $m \in \mathbb{I}^+$, where
$$t_m := 1 + \frac{m}{1 + (x - m)^{1/2}}$$
- roughly, each metric essentially flat almost everywhere except for sharp curvature peak around t-y-z-hypersurface $x = m$
- as m increases, curvature peak becomes higher and sharper, as it moves further out the x-axis

- not “physically reasonable” that the sequence converges to Minkowski spacetime
- because curvature grows without bound
- yet it does so converge under the compact-open topology
- $\Rightarrow$ problem: compact-open topology too coarse
- “not enough open sets” to stop pathological sequences from converging

Whitney (Open) Topology

A neighborhood of $g_{ab} \in \mathfrak{G}$, $\mathcal{N}(h_{ab}, \epsilon; g_{ab})$, determined by positive-definite metric h_{ab} on M, and real number $\epsilon > 0$. $g'_{ab} \in \mathfrak{G}$ is in the neighborhood if and only if

$$h^{mn} h^{rs} (g_{mr} - g'_{mr})(g_{ns} - g'_{ns}) < \epsilon$$

everywhere in M.

$\Longrightarrow$ cares about behavior on entire manifold, accounts for relative asymptotic behavior

example (Geroch 1970, 1971)

- $\mathbb{R}^4$, Minkowskian coordinate system $(t,\, x,\, y,\, z)$
- sequence of metrics: $\text{diag}(t_m,\, -1,\, -1,\, -1)$, for $m \in \mathbb{I}^+$, where
$$t_m := 1 + \frac{1}{m^2 + x^2 + y^2 + z^2}$$
- roughly, each metric essentially flat almost everywhere except for spherically symmetric bump of curvature centered on origin;
- bump decreases smoothly to zero as m increases

- "physically reasonable" that the sequence should converge to Minkowski spacetime (it does in compact-open)
- yet it *doesn't* under Whitney topology
- problem: Whitney topology is too fine—
- too many open sets, reasonable sequences can't converge

example (Geroch 1971)

one-parameter family of metrics $\{\lambda g_{ab}\}$, for $\lambda \in \mathbb{R}^+$, where g_{ab} is *any* Lorentz metric on *any* non-compact M

- fails to form continuous curve under the Whitney topology
- but each metric in the family represents the *same physical spacetime*!
- multiplying metric by a constant only "changes units" (*e.g.*, `km` to `cm`)
- Riemann tensor, Ricci tensor, Einstein tensor, stress-energy tensor, derivative operator (affine structure), projective structure, . . . , all remain the same

I conclude. . .

compact-open

example doesn't show it's bad for all purposes, but surely not good for global cosmological arguments, where asymptotic behavior matters

Whitney

physically meaningless; especially no good for global arguments

Sobolev

even worse, since strictly finer than Whitney

situation is nice in $\mathbb{R}^n$

- natural topology $\Rightarrow$ Borel sets, σ-algebra
- well defined notion of translation (addition)
- Lebesgue measure is unique complete, translation-invariant Borel measure
- Lebesgue measure is:
  - locally finite (every p has neighborhood with finite measure)
  - strictly positive (non-empty open sets have positive measure)

recall:

Definition

*A **Fréchet space** is a metrizable, locally convex topological vector space, complete with respect to a translation-invariant metric.*

Definition

*A **Fréchet manifold** is a differential manifold modeled on a Fréchet space (rather than on $\mathbb{R}^n$).*

if you don't feel comfortable with this, just think "really big, non-linear space with derivatives"

Theorem

The only locally finite, translation-invariant Borel measure on an infinite-dimensional, separable Fréchet space is the trivial measure (viz., the one that assigns measure zero to every measurable set).

$$\Downarrow$$

Any translation-invariant measure of physical interest on any reasonably well behaved infinite-dimensional linear space assigns infinite measure to all open sets, unless the measure is the trivial measure.

("trivial": measure assigns all open sets zero)

- families of spacetimes relevant to cosmology generally form infinite-dimensional Fréchet manifolds
- sum of 2 Lorentz metrics generally not a Lorentz metric…
- why family of spacetimes is a Fréchet manifold, not a Fréchet space

(more precisely: appropriate subspaces of the space of cross-sections of the bundle of Lorentz metrics over a spacetime manifold have the structure of an infinite-dimensional Fréchet manifold modeled on the infinite-dimensional Fréchet space of symmetric two-index tensor fields; in fact, the Fréchet manifold of metrics is an open, convex submanifold of the Fréchet space of symmetric, two-index tensor fields)

Is there a physically reasonable translation-invariant Borel measure on families of metrics (infinite-dimensional Fréchet manifolds)?

First problem: what topology to use?

- **compact-open**: coarsest "reasonable" one; doesn't care about asymptotic behavior, so no good for cosmology
- **Whitney**: no physical significance at all (λg_{ab} is not even a continuous curve for $\lambda \in \mathbb{R}^+$)
- **Sobolev**: finer than Whitney, so even worse; still, mathematically useful for proving stability results about initial-value problems, where ultra-fineness is a virtue

- what about a parameter topology, based on values of finite number of parameters characterizing a family of metrics (open set is product of intervals)?
- seems good for super-simple perturbation problems, but not for much else
- but that's what's often relevant in cosmology
- *but serious problem for us*:
  1. in the contexts of interest, like "calculating probabilities for values of Λ"—
  2. *e.g.*, inhomogeneous perturbations of FLRW characterized by a finite number of "coarse-grained" parameters—
  3. strictly speaking the family of spacetimes is still infinite dimensional
  4. since each tuple of determinate parameter values does not single out a unique metric
  5. only an infinite family of metrics all giving rise to the same "coarse-grained" parameter values

(similar problem arises for inflaton potentials in multiverse in eternal inflation) let's bracket the whole problem, and just pretend we have a good, physically significant topology

Second problem: what does “translation-invariant” mean here, *i.e.*, what is physically significant notion of “translation” for a metric in a family of metrics?

(recall: no linear structure on families of metrics)

Solution: look at how cosmologists handle perturbations (translation of one metric to nearby one) to get a grip on this.

not inaccurate caricature of "small" perturbation

- one-parameter family of spacetimes $\mathfrak{M}_\epsilon := \{(\mathcal{M}, (1 + \phi_\lambda)g_{ab}) : \lambda \in [0, \epsilon)\}$, for some small ϵ
- each ϕ_λ is a non-negative smooth function on $\mathcal{M}$ such that $\sup_{\mathcal{M}} \phi_{\lambda'} < \lambda' < \sup_{\mathcal{M}} \phi_\lambda < \lambda$, for all $\lambda', \lambda \in [0, \epsilon)$
- the family of functions $\{\phi_\lambda\}$ varies smoothly with respect to λ in the supremum norm, and the supremum approaches zero "slowly"
- by local Fréchet property, all the $(1 + \phi_\lambda)g_{ab}$ are Lorentzian for small enough ϵ

Then a small perturbation (1st-order, linear):

$$\left(1 + \left.\frac{\mathrm{d}\phi_\lambda}{\mathrm{d}\lambda}\right|_{\lambda=0}\right) g_{ab} \qquad (1)$$

crude sketch of appropriate generalization:

1. fix metric $g_{ab} \in \mathfrak{G}$
2. define "nearby metrics" by exponentiating g_{ab} using natural fibre-wise action of Lie algebra of GL(4) (quotiented by Lie algebra of Poincaré group)
3. the **$(\boldsymbol{\lambda}, \boldsymbol{\epsilon})$-affine translate** of g_{ab}, where λ is element of the Lie algebra and $\epsilon > 0$, is the exponentiation of g_{ab} in the λ direction for an "affine length" of ϵ

Definition

*A measure is **locally affine-translation invariant** if, for all g_{ab} and all λ and all "small enough neighborhoods of g_{ab}" N_g, there exists ϵ such that the measure of the (λ, ϵ)-affine translate of N_g equals the measure of N_g.*

Theorem (Curiel 2019)

There is no non-trivial, locally affine-translation invariant Borel measure on an infinite-dimensional Fréchet manifold $\mathcal{F}$.

("non-trivial": measures assigns at least some open sets finite measure)

There are three kinds of lies: lies, damned lies, and statistics [in cosmology].

– Mark Twain

genericity of future collapse singularities in spatially open spacetimes

- arguments due to Geroch (1966) that singularities are "generic" in spatially open spacetimes
- BUT arguments depend on assuming relation between topology and measure that just don't hold for any reasonable topology and measure on this family of spacetimes:
  1. assumed that "small perturbations" (in topological sense) that destroy property means family of spacetimes with that property has zero measure
  2. assumed that "small perturbations" (in topological sense) that preserve property means family of spacetimes with that property has non-trivial measure

Weinberg's 1987 anthropic argument for value of Λ

1. work with family of near-FLRW spacetimes (say, Szekeres spacetimes, to make problem precise)
2. existence of large, gravitationally bound systems places upper and lower bounds on possible values of Λ (too positive, potentially bound systems pulled apart, too negative, universe recollapses before they can form)
3. argue for topological stability of formation of such bound systems under small changes in value of Λ
4. use anthropic argument (presence of conscious observers as selection effect, assume we are "typical" observers, *i.e.*, value of Λ in our spacetime is "typical") to fix shape and peak of measure

BUT: implicitly assumed that topological stability implies largeness of size in fixed measure, which doesn't obtain in relevant spaces

Gibbons-Hawking-Stewart (GHS) Measure

- "minisuperspace" (Γ): roughly speaking, family of initial data for FLRW spacetimes with compact Cauchy surfaces—
- compact, constant curvature 3-geometries sourced by homogeneous, minimally coupled scalar fields, construct constraint-reduced phase space for appropriately gauge-fixed Hamiltonian formulation
- Γ is four-dimensional! (parametrized by field-intensity ϕ, field "time-derivative" off Cauchy surface $\dot{\phi}$, scalar curvature ${}^3R$, cosmological constant Λ, all constant on Cauchy surface by homogeneity)
- μ_{GHS} is standard Liouville measure on Γ (modulo a few technical difficulties)

problem of justification: equilibration

use of Liouville measure in statistical mechanics most easily justified by arguments based on properties of dynamical evolution and equilibration of system (*e.g.*, amount of time system spends in a portion of phase space proportional to its Liouville measure); these arguments not available when:

1. the system is not ergodic
2. OR one has not waited a time much greater than the equilibration time after the system was prepared
3. OR the system has a time dependent Hamiltonian that is varying on a timescale that is small or comparable to the equilibration time

but all those hold for "dynamics" of general relativity represented by minisuperspace and its Hamiltonian

put problem of justification aside for sake of argument: looks good?

- rigorously defined Borel measure on a finite-dimensional space!
- BUT: can't be turned into a probability measure, $\mu_{\text{GHS}}(\Gamma) = \infty$ (Γ not compact)

What to do?

still, can we meaningfully attribute a probability to occurrence of physical property X? Let $P_X \subset \Gamma$ be subfamily of Γ evincing X. Then 4 possibilities:

1. P_X is not measurable
2. $\mu_{\text{GHS}}(P_X) < \infty$:
3. $\mu_{\text{GHS}}(\Gamma \setminus P_X) < \infty$
4. $\mu_{\text{GHS}}(P_X) = \infty$ and $\mu_{\text{GHS}}(\Gamma \setminus P_X) = \infty$

Responses

1. can say nothing, but one assumes or stipulates or hopes or demands or pleads that physically significant properties not manifest such topological pathology in their distribution across spacetimes
2. unambiguously attribute a probability of zero to X
3. unambiguously attribute a probability of one to X
4. one can say nothing simple or straightforward, without ambiguity, but now one does not even have the solace of yelling at the property and demanding that it not be pathological, as in the first case, for there is nothing pathological about such topological behavior at all—but this is case of most interest

Regularization for Case 4

1. assume Γ is σ-finite (is a countable union of subsets of finite measure, and minisuperspace is)
2. find "physically appropriate" nested sequence of subsets of Γ, $\{S_i\}$, such that $\Gamma = \bigcup_i S_i$ and $\mu_{\text{GHS}}(S_i) < \infty$
3. define $\Pr(P_X) = \lim_{i\to\infty} \dfrac{\mu_{\text{GHS}}(P_X \cap S_i)}{\mu_{\text{GHS}}(S_i)}$

Obvious Problem

one can get any answer one wants by judicious choice of $\{S_i\}$

Example: Probability of Inflation in Γ

using "natural" regularizations derived from arguments based on (topological) stability of initial conditions yielding slow-roll inflation:

- Gibbons and Turok (2008) deduced extremely low probability for $N \gg 1$ e-foldings of inflation
- Carroll and Tam (2010) deduced extremely high probability for $N \gg 1$ e-foldings of inflation

Resolution: they used different characterizations of topological stability—regularizations—for initial conditions yielding inflation

I must emphasize:

> *I do **not** claim that the conclusions of such arguments are wrong, only that the arguments currently given have serious mathematical, physical and conceptual problems that must be addressed before any real confidence can be had in those conclusions.*

Guth (2000, p. 572), in discussing proposed regularization procedure for computing probabilities in eternal inflation of Vilenkin (1998) (emphases mine):

> *[A]lthough the results of [Vilenkin's] method seem reasonable, I do not at this point find them compelling. That is, it is not clear what principles of physics or probability theory ensure that this particular method of regularizing the spacetime is the one that leads to correct predictions. Perhaps there is no way to answer this question, so we may be forced to accept this proposal, or something similar to it, as a postulate.*

- why demand that we be able to calculate probabilities in all contexts, under any circumstances. . .
- even to the point that one is willing simply to postulate a method for doing so based on no principled reasons
- but rather constructed *ad hoc* to give one the results one *a priori* wants?

That is not science.

- I find it more attractive to give up requirement that one always be able to calculate probabilities
- that would require reconceiving ideas of predictions and their confirmation in such contexts. . .
- but I find that more palatable—
- more reasonable, more in line with the requirement that science be grounded in empirical data

– We want to be able to reason about the cosmos in particular ways, to answer questions of a particular sort.

– As poignantly intimated by the poem serving as this talk's epigraph, that bare want does not suffice to guarantee that the universe be such as to support the required reasoning.

Benedict, Robert P. 1969. *Fundamentals of Temperature, Pressure and Flow Measurements.* New York: John Wiley & Sons, Inc.

Butterfield, Jeremy. 1984. "Relationism and Possible Worlds". *The British Journal for the Philosophy of Science* 35 (2, Jun): 101–113. doi:10.1093/bjps/35.2.101.

da Costa, Newton C. A., and Steven French. 2003. *Science and Partial Truth: A Unitary Approach to Models and Scientific Reasoning.* Oxford: Oxford University Press. doi:10.1093/019515651X.001.0001.

Curiel, Erik. 2017. "On the Propriety of Physical Theories as a Basis for Their Semantics". Unpublished manuscript, draft available at <http://strangebeautiful.com/phil-phys.html>.

———. 2022. "Kinematics, Dynamics, and the Structure of Physical Theory". Unpublished manuscript, based on talks given at Univ. of Bonn, Inst. für Philosophie Colloquium (Oct 2019), and the conference "Measurement at the Crossroads 2020: Measuring and Modeling" (Università Cattolica del Sacro Cuore, Milan, Jun 2022). arXiv:1603.02999 [physics.hist-ph]. Most recent draft available at <http://strangebeautiful.com/phil-phys.html>.

———. 2025. "Schematizing the Observer and the Epistemic Content of Theories". Forthcoming in *Studies in History and Philosophy of Modern Physics*. arXiv:1903.02182 [physics.hist-ph]. Most recent version available at <http://strangebeautiful.com/phil-phys.html>.

van Fraassen, Bas. 1980. *The Scientific Image.* Oxford: Oxford University Press. doi:10.1093/0198244274.001.0001.

Geroch, Robert. 1966. "Singularities in Closed Universes". *Physical Review Letters* 17 (8, 22 Aug): 445–447. doi:10.1103/PhysRevLett.17.445.

———. 1970. "Domain of Dependence". *Journal of Mathematical Physics* 11 (2): 437–449. doi:10.1063/1.1665157.

———. 1971. "General Relativity in the Large". *General Relativity and Gravitation* 2 (1, Mar): 61–74. doi:10.1007/BF02450519.

Giere, Ronald N. 2010. "An Agent-Based Conception of Models and Scientific Representation". In "The Ontology of Scientific Models", edited by G. Contessa. *Synthese* 172 (2, Apr): 269–281. doi:10.1007/s11229-009-9506-z.

Guth, Alan H. 2000. "Inflation and Eternal Inflation". *Physics Reports* 333–334 (Aug): 555–574. arXiv:astro-ph/0002156, doi:10.1016/S0370-1573(00)00037-5.

Harper, William L. 2011. *Isaac Newton's Scientific Method: Turning Data into Evidence about Gravity and Cosmology.* Oxford: Oxford University Press. doi:10.1093/acprof:oso/9780199570409.001.0001.

Lamb, Horace. 1932. *Hydrodynamics.* Sixth edition. Cambridge: Cambridge University Press.

Le Verrier, U. 1859. "Lettre de M. Le Verrier à M. Faye sur la théorie de Mercure et sur le mouvement du périhélie de cette planète". *Comptes Rendus Hebdomadaires des Séances de l'Académie des Sciences (Paris)* 49:379–383.

Lewis, David. 1970. "How to Define Theoretical Terms". *The Journal of Philosophy* 67 (13, July): 427–446. http://www.jstor.org/stable/2023861.

Smith, George E. 2014. "Closing the Loop: Testing Newtonian Gravity Then and Now". Chapter 10 in *Newton and Empiricism,* edited by Zvi Biener and Eric Schliesser, 262–351. Oxford: Oxford University Press. doi:10.1093/acprof:oso/9780199337095.003.0011.

Smith, George E., and Raghav Seth. 2020. *Brownian Motion and Molecular Reality: A Study in Theory-Mediated Measurement.* Oxford Studies in Philosophy of Science. Oxford: Oxford University Press. doi:10.1093/oso/9780190098025.001.0001.

Suppes, Patrick. 1960. "A Comparison of the Meaning and Uses of Models in Mathematics and the Empirical Sciences". *Synthese* 12 (2–3, September): 287–301. doi:10.1007/BF00485107.

Vilenkin, Alexander. 1998. "Unambiguous Probabilities in an Eternally Inflating Universe". *Physical Review Letters* 81 (25, 21 Dec): 5501–5504. doi:PhysRevLett.81.5501.

Weinberg, Steven. 1987. "Anthropic Bound on the Cosmological Constant". *Physical Review Letters* 59 (22, 30 Nov): 2607–2610. doi:10.1103/PhysRevLett.59.2607.