# Is High Energy Theory on a Dead-End Road?

*Scientific Stagnation, Institutional Gatekeeping,*

*and the Suppression of Intellectual Pluralism*

**Ervin Goldfain**

Global Institute for Research, Education and Scholarship (GIRES)

*Academic essay · October 2026*

## Abstract

High energy theory has not produced a single experimentally confirmed prediction beyond the Standard Model in roughly fifty years. This essay argues that this stagnation is no longer only a scientific problem but an institutional one. Five failures sustain it: a record of non-delivery that the field is reluctant to name as such; gatekeeping of publication and preprint channels that marginalizes dissent; a consensus that has hardened into dogma; indifference toward proposals originating outside the dominant programs; and a reward system that favors volume, hype and loosely argued speculation. Drawing on critiques by Smolin, Woit, Hossenfelder, Baggott, Ellis and Silk, López-Corredoira, Josephson and others, together with quantitative studies of scientific progress, the essay distinguishes documented structural failures from allegations that require case-specific evidence and answers the strongest objections. It concludes that the field is travelling a dead-end road unless it changes its incentives and broadens its methods. As one alternative, it presents the complex dynamics program of *Complex Dynamics and Foundational Physics: An Introduction* [17], a program that, like any other, must be judged by explicit definitions, mathematical consistency, recovery of established results and discriminating predictions.

The essay argues for disciplined intellectual pluralism rather than either uncritical orthodoxy or automatic acceptance of heterodox claims.

**Keywords:** high-energy theory; string theory; sociology of science; peer review; arXiv moderation; intellectual pluralism; Complex Dynamics; Foundational Physics.

## 1. Introduction: a crisis of expectations

The question in the title is not rhetorical. High-energy theoretical physics is not dead, and its achievements should not be minimized: Quantum Field Theory, the Standard Model (SM), General Relativity and the machinery of precision calculation remain among the most successful intellectual constructions in science. But a research program that consumes the careers of thousands of talented people and absorbs public money on the scale of national laboratories and multi-billion-euro colliders, owes its sponsors an honest accounting of what it has delivered.

For high energy theory (hep-th) that accounting is uncomfortable. The SM was essentially complete by the mid-1970s. Every experimental triumph since then, from the W and Z bosons to the top quark and the 2012 discovery of the Higgs boson [5, 13], has confirmed that framework rather than any of the theories built to supersede it. During the same half century, the theoretical community has invested overwhelmingly in a narrow set of programs: grand unification, supersymmetry, extra dimensions, string theory and, more recently, holography and the "landscape" of vacua. None has produced a confirmed novel prediction. Several were promoted to

funding agencies and the public with specific expectations, most prominently that supersymmetric partners would appear at the Large Hadron Collider (LHC). They did not.

The issue is not whether every theory must immediately yield a new particle or a near-term experiment. Foundational research often advances indirectly, and experiments can lag behind conceptual development. Failure of individual hypotheses is normal and healthy. What is not normal is a community that fails to update. As Kuhn [24] and Lakatos [25] taught, a research programme that keeps generating auxiliary hypotheses to protect its core, without ever predicting anything new, is *degenerating*. The thesis of this essay is that hep-th displays the full set of symptoms of such degeneration, and that the institutions of the field, rather than correcting the drift, now tend to reinforce it. The title question is treated here as a diagnosis to be established by evidence, not as a verdict assumed in advance.

## 2. Five decades without confirmed physics beyond the Standard Model

The empirical record is the starting point, and it is stark. The SM still requires about twenty-six free parameters fixed by hand. The fermion mass spectrum spans some thirteen orders of magnitude with no accepted dynamical explanation. The hierarchy problem, the metastability of the electroweak vacuum, the triviality of the Higgs sector in exactly four dimensions, the strong CP problem, the matter–antimatter asymmetry and

the origin of neutrino masses all remain open [17]. In cosmology, the ΛCDM model rests on a dark matter component never detected in any laboratory and a dark energy component whose very constancy is now questioned by DESI baryon acoustic oscillation data, while the Hubble and $S_8$ tensions have deepened rather than dissolved [17].

Null results are evidence, and they can sharpen theory by excluding parameter regions. This is not proof that the underlying ideas are false. But repeated null results should change research priorities. A program that continually moves its characteristic scales, broadens its parameter space, or reinterprets every outcome as compatible with its core commitments loses predictive bite. The relevant question is not whether a framework can be made consistent with observations after the fact, but whether it exposes itself to observations that could genuinely count against it. A framework can be elegant, technically rich and internally coherent while remaining underdetermined by data. In physics, mathematical depth is a resource, not a substitute for risky predictions, discriminating tests or explanatory compression.

The slowdown is not unique to particle physics, which makes it harder to dismiss as bad luck. Large-scale bibliometric studies find that papers have become steadily less disruptive across all fields [27], that research productivity per researcher has fallen sharply [9], and that in large fields new ideas struggle to displace the established canon [12]. Particle theory is

a large, mature field with heavy citation concentration. It fits that pattern almost perfectly.

## 3. String theory, naturalness and the authority of beauty

The flagship theoretical programs share a common profile:

- **Supersymmetry** was promoted for three decades as the natural solution to the hierarchy problem, with superpartners "expected" at the TeV scale. LHC null results have pushed the allowed mass scales ever higher, and naturalness, the original motivation, has quietly been redefined or abandoned [20, 21].
- **String theory** has produced deep mathematics but, after more than forty years, no unique low-energy prediction. The estimated number of vacua, often quoted as $10^{500}$ or more, turned the hope of a unique theory of everything into a landscape that can accommodate almost any observation [7, 31, 33].
- **Grand unification** predicted proton decay at rates that successive experiments have excluded for its simplest versions; the program survives by moving to ever more elaborate models [23].
- **Extra dimensions** and **Technicolor** generated large literatures and specific collider signatures, none of which has been observed [17].

Woit's *Not Even Wrong* [33] and Smolin's *The Trouble with Physics* [31] brought these concerns to a broad audience, emphasizing the risks of a

research ecosystem organized around a dominant program with limited empirical feedback. The books provoked heated responses [28, 29], but their central questions about testability, diversity and the sociology of research have never been satisfactorily answered.

Hossenfelder's *Lost in Math* [20] documents how aesthetic criteria such as naturalness, simplicity, symmetry and elegance have been substituted for evidence, and how those criteria have failed in every recent test; the argument has been widely discussed [2, 10, 19]. Beauty can be a powerful heuristic, and many successful theories possess striking formal structure. Yet aesthetic criteria are not infallible, and they become self-reinforcing when a community shares the same expectations. Nature is not obliged to conform to physicists' preferred forms of elegance. Kragh's historical study [23] shows that physics has passed through speculative phases before, but rarely for so long, and never with so little experimental guidance.

The appropriate conclusion is not that string theory, supersymmetry or naturalness-based reasoning must be banned. It is that no research program should receive indefinite epistemic privilege because it is mathematically compelling or institutionally established.

## 4. Gatekeeping: publication control, arXiv moderation and censorship

In high energy theory, visibility is controlled less by journals than by a single channel: the arXiv preprint server. A paper that does not appear in

hep-th or hep-ph is, for practical purposes, invisible to the community. That concentration of power would matter less if it were exercised transparently. It largely is not.

arXiv publicly describes its moderation and endorsement procedures [3, 4], and moderation is explicitly distinguished from peer review. In practice, however, moderation is carried out by anonymous volunteers under criteria that are only partly published. Authors can be required to find an "endorser" from within the field before posting at all. Papers judged unsuitable can be reclassified into the general physics category (physics.gen-ph), which many working physicists treat as a holding pen for cranks and never read. Roth's recent historical study [30] shows that this is not an accident of software but a continuation of a long tradition: from CERN's preprint lists of the late 1950s onward, classification schemes reflected the interests of the dominant institutions, and arXiv's general physics class functions today as a marker separating insiders from outsiders.

The most widely documented complaints come from Nobel laureate Brian Josephson [22]. In 2004 he described how his own posting privileges were barred by a flag in his record, how a paper submitted to hep-th was moved and confined to general physics, and how work by other qualified researchers was removed or refused, in some cases even after acceptance by refereed journals. Authors flagged through "reader complaints" were

not told the content of the complaint or given a meaningful means of appeal. López-Corredoira [26] reports comparable patterns in cosmology, where non-standard models are starved of attention and the resulting underdevelopment is then cited as proof that they deserve none.

Strong claims of censorship require specific evidence: documented editorial decisions, correspondence, inconsistent application of stated rules, identifiable conflicts of interest, or comparative data showing systematic differential treatment. Rejection, criticism or a lack of citations alone does not establish censorship, and the cases above are offered as documented examples rather than as proof of a coordinated system. Conversely, the absence of a centralized conspiracy does not make structural barriers harmless. When a small number of anonymous gatekeepers drawn from the dominant programs decide what counts as "real" physics, the definition of physics converges on those programs. Peer review compounds the problem: referees are chosen from the same pool, and a manuscript that questions shared assumptions faces reviewers whose careers depend on those assumptions. Screening is legitimate and necessary, but quality control without published criteria, stated reasons or a right of appeal is, from the author's side, indistinguishable from censorship.

## 5. Entrenched dogma, groupthink and the sociology of the field

Smolin's account [31] remains the most detailed insider description of how the field's sociology works. He identifies a cluster of traits in the string theory community: tremendous self-confidence, an unusually monolithic culture, strong identification with the group, a sharp boundary between insiders and everyone else, disregard for ideas from outside the group, and a tendency to interpret evidence optimistically while discounting the possibility that the program is wrong. Woit [33] reached similar conclusions from outside the string program, documenting how hiring in leading departments concentrated on a single approach for two decades.

These are textbook conditions for groupthink. Faculty positions, grants and prestigious fellowships are awarded by committees drawn from the dominant programs, so each generation reproduces the priorities of the previous one. Graduate students learn quickly that a career depends on working within an established research line, citing its leaders and avoiding questions those leaders regard as settled. Azoulay, Fons-Rosen and Graff Zivin [6] showed empirically, in the life sciences, that the premature death of a star scientist is followed by a marked increase in contributions from outsiders who had previously been deterred from entering the field. Planck's grim aphorism that science advances "one funeral at a time" turns out to be measurable. There is no reason to think theoretical physics is exempt.

The dogma is not only social but methodological. Four idealizations are treated as beyond question: that spacetime has a fixed integer number of dimensions at every energy scale; that physical systems evolve near thermodynamic equilibrium; that evolution is exactly unitary and local at arbitrarily high energy; and that the underlying classical dynamics is integrable, allowing a stationary-action description [17]. Each is well supported in the regime where it was tested. None has been established at the scales where hep-th applies it most confidently. Yet questioning them marks a researcher as an outsider.

Most troubling is the attempt to relax the rules of science itself to protect the dominant programs. Ellis and Silk [15] warned in *Nature* that some theorists now argue that a sufficiently elegant and explanatory theory need not be tested experimentally and insisted that "the imprimatur of science should be awarded only to a theory that is testable." Proposals for "non-empirical theory confirmation" [14] may be philosophically interesting, but in practice they function as a license to continue indefinitely without evidence.

## 6. Arrogance and indifference to new proposals

A healthy field responds to a serious alternative by criticizing it. An unhealthy one responds with silence. Researchers who propose frameworks outside the canon, especially those without appointments at elite institutions, report the same experience again and again: manuscripts

returned without review as "not of sufficient interest," conference programs closed to them, and no engagement with the substance of their arguments. Silence is more damaging than refutation, because it leaves no record that could be answered.

Unzicker and Jones [32] and Baggott [7] describe a culture in which the label "crackpot" is applied indiscriminately, so that any proposal not framed in the vocabulary of the dominant programs is presumed worthless before it is read. Feyerabend [16] argued long ago that methodological pluralism is a condition of progress, and that a science that rules out unfamiliar approaches by fiat cuts itself off from the very ideas it will eventually need. López-Corredoira's "snowball effect" [26] describes the resulting feedback loop: neglected alternatives remain underdeveloped, and their underdevelopment is then taken as proof that they deserved neglect.

Intellectual pluralism is not a demand that journals publish every speculative claim. It is a demand that a proposal be assessed by relevant technical and evidential standards rather than by the identity, reputation or theoretical allegiance of its author. The irony is that many of the tools hep-th now needs have been developed in neighbouring fields. Nonlinear dynamics, bifurcation theory, deterministic chaos, multifractal geometry, fractional calculus, self-organized criticality [8] and generalized (Rényi and Tsallis) entropies are mature, empirically validated disciplines. They

describe turbulence, phase transitions and far-from-equilibrium systems with great success. Yet their application to the ultraviolet sector of particle physics or to the primordial universe is routinely treated as eccentric, because it does not come from within the community's established research lines.

## 7. Noise over substance: sloppy speculation and hype

The same gatekeeping that excludes careful outsiders is strikingly permissive toward insiders. The clearest example is the December 2015 diphoton excess at 750 GeV: within months, hundreds of theory papers had been posted explaining a signal that then vanished with more data. The episode revealed a community rewarded for speed and citation capture rather than care. Hossenfelder [20, 21] has described the resulting culture of "ambulance chasing" and model-building for its own sake, while Baggott [7] calls the speculative end of the field "fairytale physics." Press releases about wormholes, multiverses and laboratory "simulations" of quantum gravity routinely generate headlines out of all proportion to their content. Grand claims are built on undefined terms, inconsistent dimensions, unsupported transitions, or equations that merely restate assumptions, and technical ornament obscures the absence of a clear mechanism. These defects are not confined to mainstream research; heterodox proposals are not automatically more insightful because they challenge consensus.

The deeper point is structural. A system that counts papers and citations, rather than predictions confirmed or problems solved, will always reward noise. Meanwhile, blunt instruments of control, such as opaque automated classification [30], are likely to fall hardest on precisely the independent researchers who were already struggling to be heard. A better filter is a substantive one: define the objects, state the assumptions, show the derivation, check dimensions and limiting cases, identify what would falsify the claim, and provide references that can be independently verified. Where these requirements are not met, neither fashionable terminology nor elaborate mathematical presentation should rescue the work, regardless of who wrote it.

## 8. Anticipated objections

A critique this sharp should face the strongest replies to its targets would make.

**"The problem is experimental, not theoretical."** Defenders argue that theory cannot be blamed for the absence of data at the Planck scale. There is some force in this. But theory chose to spend decades on programs that make no testable predictions at accessible energies, and in the case of supersymmetry it promised results that experiment duly tested and refuted. A theory community is responsible for proposing what can be checked.

**"String theory has paid off in mathematics and in other fields."** Polchinski [29] and others point to the AdS/CFT correspondence, insights into black hole entropy, and techniques adopted in condensed matter and nuclear physics. These achievements are real. They do not, however, answer the question this essay asks: whether hep-th has advanced our knowledge of the physical world at the energies it claims to describe.

**"Moderation is needed to stop genuine cranks."** It is. arXiv receives a genuine stream of worthless submissions. The objection here is not to moderation but to moderation without published criteria, stated reasons or a right of appeal. Those safeguards would cost little and would distinguish quality control from exclusion.

**"The critics are polemical and overgeneralize."** Some are, in places, and not every member of a research community behaves alike. But the institutional pattern described here does not depend on individual bad faith. It follows from incentives that reward conformity, prestige and formal elaboration, and it can be documented through predictions made and missed, hiring and funding concentration, and specific moderation cases.

**"Critics have no better alternative."** This objection is circular. Alternatives remain undeveloped precisely because they receive little funding, few positions and scant attention [26]. The appropriate response to a field in which nothing has been confirmed is not to keep funding only the approaches that have already failed to deliver, but to diversify.

None of these replies removes the central charge: fifty years without a confirmed prediction beyond the SM, combined with institutions that discourage the search for alternatives.

## 9. Complex dynamics as a research alternative

Criticism is only useful if it points somewhere. One response to stagnation is not merely to criticize dominant programs but to formulate alternatives with enough precision to be assessed. *Complex Dynamics and Foundational Physics: An Introduction* [17] offers one example of the kind of program the field has neglected. Its starting diagnosis is that many open puzzles of the SM and ΛCDM are not isolated failures, but symptoms of the four unexamined idealizations listed in Section 5. Where those idealizations break down, far above the electroweak scale and in the primordial universe, the book proposes that spacetime dimensionality becomes a dynamical, scale-dependent quantity. Its evolution ("dimensional flow") is governed by the mathematics of complex dynamics: bifurcations, chaos, multifractal geometry, self-organized criticality, fractional dynamics and generalized entropy.

The book develops this idea across eight parts. It reinterprets dimensional regularization as a hint of a physical process rather than a formal device, connecting it to integration over fractal measures. It links field theory to evolving dimensionality through three routes: nontrivial attractors of the renormalization group, the onset of Hamiltonian chaos,

and the triviality of the Higgs sector in exactly four dimensions. It then applies the framework to mass generation and flavor structure in the SM, to the recovery of classical gravity, and to cosmology, where it advances a multifractal "Cantor Dust" interpretation of dark matter and relates the $\sigma_8$ tension and the DESI results to primordial fractality.

Related literature shows that running or non-integer dimensionality is not, in itself, outside serious theoretical discussion. Causal dynamical triangulations find a spectral dimension that flows from four at large scales to about two at short distances [1], and multi-fractional spacetimes have been proposed as a route to quantum gravity [11]. Such work does not establish the book's specific proposals; each proposed connection must be derived and tested on its own merits.

The book is explicit that it reports no new particle or experimental confirmation, and that it is offered as "work in progress" to be "tested, criticized, extended, or discarded" [17]. It should be read as a research-oriented introduction and programmatic contribution, not as a claim that the foundational problems have been solved. Its scientific value will depend on whether its concepts can be operationally defined, whether its equations are dimensionally and mathematically consistent, whether known limits are recovered, and whether it yields predictions that can be distinguished from competing models. That is precisely the standard that should apply to string theory, effective field theories and every other ambitious proposal. A healthy field would read such a proposal, test its

claims and publish the results of that scrutiny, rather than ignore it because it originates outside the established research lines. Openness, however, must be reciprocal: heterodox work must accept detailed criticism, independent reproduction and the possibility of rejection.

## 10. What would count as progress, and what must change

A productive research agenda should make progress measurable. At minimum, a proposed theory should specify its basic entities and observables; recover established results in their tested domains; expose its assumptions and approximations; distinguish its predictions from those of rival frameworks; and identify the observations or calculations that could count against it. For foundational proposals, progress may initially be conceptual or mathematical, but the path toward physical interpretation should be stated rather than assumed. Theoretical communities should also keep a live record of failed expectations: a null experiment, an inconsistency or a failed derivation should update confidence, not disappear beneath a succession of flexible reinterpretations. Equally, a theory should not be rejected solely because its first proposed test is beyond present technology.

Several institutional reforms follow directly from the diagnosis above:

1. **Transparent moderation.** Preprint servers should publish moderation criteria, give authors written reasons for reclassification or rejection, and offer a genuine appeal to a named panel.
2. **Honest refereeing.** Editors and reviewers should distinguish a proposal that is unconventional from one that is technically deficient. Reports should identify concrete errors, missing derivations or evidential gaps, and a decision resting on scope or priority rather than correctness should say so.
3. **Funding diversity.** Agencies should reserve a fixed share of theory funding for approaches outside the dominant programs, judged by panels that include researchers from neighbouring disciplines such as nonlinear dynamics and statistical physics.
4. **Rewarding substance over volume.** Hiring and grants should weigh testable predictions, solved problems, careful negative results, replication and serious criticism, not paper counts, journal prestige and citation totals, which reward hype and padding.
5. **Accountability for promises.** Programs that make specific predictions to justify funding should report, publicly and in plain terms, when those predictions fail. A conjecture should not be marketed as a result, a phenomenological fit should not be presented as a unique derivation, and an analogy should not be mistaken for a mechanism.

6. **Open engagement.** Journals and conferences should create formats, such as dedicated sessions and published comment-and-reply exchanges, in which unconventional proposals are criticized on their merits rather than dismissed unread.

The goal is neither unrestricted speculation nor premature conservatism. It is *disciplined pluralism*: multiple approaches competing under shared standards of clarity, consistency and empirical accountability.

## 11. Conclusion: a dead end, or a warning sign?

Is high energy theory on a dead-end road? On the evidence of the past fifty years, the answer is yes for its dominant programs, unless the field changes direction. High energy theory has not lacked talent, money or mathematical sophistication. Its connections to quantum field theory and geometry, and its role in interpreting experimental results, remain substantial. What it has lacked is the willingness to admit failure, the openness to consider alternatives, and the institutional mechanisms that would force either.

The warning signs are now difficult to miss: flagship programs that predict nothing testable, gatekeeping that operates without accountability, a consensus enforced by hiring and funding, dismissiveness toward outsiders, and a publication system that rewards volume over substance. Each has been documented by physicists, philosophers, historians and

economists of science writing from inside and outside the field [6, 7, 12, 15, 18–20, 22, 23, 26, 27, 31–33]. The most defensible charge is not that every member of the hep-th community is dogmatic or censorious; it is that the field's institutions reward conformity, prestige and formal elaboration in ways that make it very hard to recognize when a program has reached diminishing returns. Acknowledging that problem is not an attack on science. It is part of science's self-correcting tradition.

The remedy is a renewed commitment to methodological humility. No school of thought should be immune from criticism, and no alternative should be granted credibility merely for being alternative. Theoretical physics advances when ideas are made precise enough to fail, when criticism is specific enough to help, and when the community remains willing to revise its most cherished assumptions. The Standard Model and ΛCDM are extraordinary achievements, and their puzzles are real. Solving them will almost certainly require ideas that do not yet belong to the canon. The task is not to replace one orthodoxy with another, nor to defend the road the field has been on, but to open new ones and to let evidence, rather than prestige or habit, decide which of them leads somewhere.

***Editorial note.***

*This essay is a critical argument about institutions and incentives, not a claim that every member of a research community behaves alike. Allegations about particular editorial or moderation decisions should be supported by primary documentation, and the cases cited here are referenced to their published sources.*

## References

[1] J. Ambjørn, J. Jurkiewicz and R. Loll, "Spectral dimension of the universe is scale dependent," *Physical Review Letters* **95**, 171301 (2005). doi:10.1103/PhysRevLett.95.171301.

[2] A. Ananthaswamy, "How the belief in beauty has triggered a crisis in physics," review of *Lost in Math*, *Nature* (12 June 2018). doi:10.1038/d41586-018-05374-9.

[3] arXiv, "Endorsement," official help documentation. https://info.arxiv.org/help/endorsement.html.

[4] arXiv, "Moderation," official help documentation. https://info.arxiv.org/help/moderation/index.html.

[5] ATLAS Collaboration, "Observation of a new particle in the search for the Standard Model Higgs boson with the ATLAS detector at the LHC," *Physics Letters B* **716**, 1–29 (2012).

[6] P. Azoulay, C. Fons-Rosen and J. S. Graff Zivin, "Does science advance one funeral at a time?," *American Economic Review* **109**(8), 2889–2920 (2019). doi:10.1257/aer.20161574.

[7] J. Baggott, *Farewell to Reality: How Fairytale Physics Betrays the Search for Scientific Truth* (Constable, London, 2013).

[8] P. Bak, *How Nature Works: The Science of Self-Organized Criticality* (Copernicus, New York, 1996).

[9] N. Bloom, C. I. Jones, J. Van Reenen and M. Webb, "Are ideas getting harder to find?," *American Economic Review* **110**(4), 1104–1144 (2020).

[10] J. Butterfield, "Lost in Math? A review of *Lost in Math: How Beauty Leads Physics Astray*, by Sabine Hossenfelder," PhilSci-Archive 15724; arXiv:1902.03480 (2019).

[11] G. Calcagni, "Fractal universe and quantum gravity," *Physical Review Letters* **104**, 251301 (2010). doi:10.1103/PhysRevLett.104.251301.

[12] J. S. G. Chu and J. A. Evans, "Slowed canonical progress in large fields of science," *Proceedings of the National Academy of Sciences* **118**(41), e2021636118 (2021).

[13] CMS Collaboration, "Observation of a new boson at a mass of 125 GeV with the CMS experiment at the LHC," *Physics Letters B* **716**, 30–61 (2012).

[14] R. Dawid, *String Theory and the Scientific Method* (Cambridge University Press, Cambridge, 2013).

[15] G. Ellis and J. Silk, "Scientific method: Defend the integrity of physics," *Nature* **516**, 321–323 (2014).

[16] P. Feyerabend, *Against Method: Outline of an Anarchistic Theory of Knowledge* (New Left Books, London, 1975).

[17] E. Goldfain, *Complex Dynamics and Foundational Physics: An Introduction. A Research Monograph on the Role of Complex Dynamics in High-Energy Physics and Cosmology*, first edition (Global Institute for

Research, Education and Scholarship (GIRES), October 2026). eBook available on Academia.edu and ResearchGate.

[18] J. Horgan, *The End of Science: Facing the Limits of Knowledge in the Twilight of the Scientific Age* (Addison-Wesley, Reading, MA, 1996).

[19] J. Horgan, "How physics lost its way," *Scientific American*, Cross-Check blog (25 June 2018). https://www.scientificamerican.com/blog/cross-check/how-physics-lost-its-way/.

[20] S. Hossenfelder, *Lost in Math: How Beauty Leads Physics Astray* (Basic Books, New York, 2018).

[21] S. Hossenfelder, "The uncertain future of particle physics," *The New York Times* (23 January 2019).

[22] B. D. Josephson, "Covert censorship by the physics preprint archive," University of Cambridge, Theory of Condensed Matter Group (14 November 2004). https://tcm.phy.cam.ac.uk/~bdj10/archivefreedom/main.html. See also "Correspondence with Cornell re the unsatisfactory management of the physics preprint archive," https://www.tcm.phy.cam.ac.uk/~bdj10/articles/arxiv_correspondence.html.

[23] H. Kragh, *Higher Speculations: Grand Theories and Failed Revolutions in Physics and Cosmology* (Oxford University Press, Oxford, 2011).

[24] T. S. Kuhn, *The Structure of Scientific Revolutions* (University of Chicago Press, Chicago, 1962).

[25] I. Lakatos, *The Methodology of Scientific Research Programmes*, Philosophical Papers Vol. 1 (Cambridge University Press, Cambridge, 1978).

[26] M. López-Corredoira, "Non-standard models and the sociology of cosmology," *Studies in History and Philosophy of Modern Physics* **46**, 86–96 (2014). Extended version: arXiv:1311.6324.

[27] M. Park, E. Leahey and R. J. Funk, "Papers and patents are becoming less disruptive over time," *Nature* **613**, 138–144 (2023).

[28] *Physics Today*, joint review of *Not Even Wrong* and *The Trouble with Physics* (2006). https://physicstoday.aip.org/reviews/not-even-wrong-the-failure-of-string-theory-and-the-search-for-unity-in-physical-law-the-trouble-with-physics-the-rise-of-string-theory-the-fall-of-a-science-and-what-comes-next.

[29] J. Polchinski, "All strung out?," review of *The Trouble with Physics* and *Not Even Wrong*, *American Scientist* **95**(1), 72 (2007).

[30] P. H. Roth, "How libraries classified physics preprints before arXiv and set the stage for distinguishing insiders from outsiders," arXiv:2603.26590 (2026).

[31] L. Smolin, *The Trouble with Physics: The Rise of String Theory, the Fall of a Science, and What Comes Next* (Houghton Mifflin, Boston, 2006).

[32] A. Unzicker and S. Jones, *Bankrupting Physics: How Today's Top Scientists Are Gambling Away Their Credibility* (Palgrave Macmillan, New York, 2013).

[33] P. Woit, *Not Even Wrong: The Failure of String Theory and the Search for Unity in Physical Law* (Basic Books, New York, 2006).