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Octonions, and an $E_8 \times \omega E_8$ Theory of Unification *and Its Critical Evaluation by GPT-5.4 Pro*

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Article

Octonions, and an $E_8 \times \omega E_8$ Theory of Unification and Its Critical Evaluation by GPT-5.4 Pro

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Abstract

We summarize the key aspects of the ongoing octonionic $E_8 \times \omega E_8$ unification program. This summary was fed to Open AI's GPT-5.4 Pro with a prompt to give a critical appraisal of this program, and to compare it with string theory. We include the prompt and the detailed response from AI in an Appendix.

Keywords: unification; $E_8 \times E_8$; octonions; trace dynamics; non-commutative geometry; spectral action principle; division algebras; Clifford algebras; exceptional Lie groups; standard model; gravitation; exceptional Jordan algebra; GPT-5.4 Pro; artificial intelligence; spacetime geometry

1. Overview of the Octonionic $E_8 \times \omega E_8$ Unification Program

Over the last decade or so, we have been seeking a reformulation of quantum (field) theory which does not depend on external classical time. Such a reformulation is essential at all energy scales, not just at the Planck scale. The reason being that classical (space-)time exists only when the universe is dominated by macroscopic classical bodies. However, in principle, even at low energies, a physical universe need not have anything classical about it. Hence, when everything is 'quantum', the aforesaid reformulation is required. From the vantage point of this reformulation, the current formulation of quantum theory becomes a (highly successful) approximation, valid only when the universe is dominated by macroscopic classical bodies, as our actual universe is. The reformulation is a more exact description of quantum phenomena and of quantum spacetime geometry, and remarkably, it also turns out to be a candidate theory for the unification of fundamental forces [1,2].

There are five key mathematical ingredients which go into the development of such a reformulation. These are : normed division algebras, Adler's theory of Trace Dynamics, Connes' non-commutative geometry and the accompanying spectral action principle, Clifford algebras, the exceptional Lie groups and the exceptional Jordan algebra. Every one of these five ingredients plays a distinct role in the reformulation of quantum theory and the accompanying unification.

Every spacetime point (which of course is real-number valued) is replaced by a non-commutative structure, and in this we are guided by division algebras. There are only four such algebras, and besides the real numbers and complex numbers, they are the quaternions (non-commutative) and the octonions (non-commutative and non-associative). Our research shows that it pays to replace every point by a copy of the 16-dim split bioctonion. This replacement enables a structure (scaffolding) rich enough to incorporate internal symmetry space alongside a pre-spacetime, as well as three generations of chiral fermions of the standard model [3,4].

On this pre-spacetime, at every bioctonionic 'point', we overlay matter and field degrees of freedom, described by matrices/operators. These obey the rules of Adler's trace dynamics: a matrix-valued Lagrangian dynamics, which turns out to be more general than quantum theory, and is hence a pre-quantum dynamics. Each matrix has sixteen matrix-valued components, one component per bioctonionic direction. This generalises, in a rather natural way, the four real-number valued components of the position vector of a classical particle living on a 4-dim spacetime. Every point of the 4-dim spacetime has been replaced by the 16-dim split bioctonion, and the 4-dim position vector of the

particle has been replaced by the 16-dim matrix-valued dynamical variable. We call each such entity (bioctonion + dynamical variable) an atom of spacetime-matter (STM atom), and it has the action given as

$$\frac{S}{\hbar} = \frac{1}{2} \int \frac{d\tau}{\tau_{Pl}} \text{Tr} \left[\frac{L_P^2}{L^2} \dot{Q}_1^\dagger \dot{Q}_2 \right]$$

This is the pre-quantum, pre-spacetime generalisation of the action of a classical free particle. Here, τ is a novel, new time parameter, the Connes time, whose existence is a consequence of the Tomita-Takesaki theorem for von Neumann algebras, relevant to Connes' non-commutative geometry. And in our present case, we have an instance of a non-associative, non-commutative geometry. Connes' time is over and above, and extrinsic to, the 16-dim split bioctonionic space: the latter evolves in Connes' time, and the presence of STM atoms 'curves' this space, in the same spirit in which matter in general relativity curves Minkowski spacetime.

How do we guarantee that the dynamical variables Q_1, Q_2 are related to the physical world that we presently observe? This is ensured by an application of the Chamseddine-Connes spectral action principle, according to which, the eigenvalues of the Dirac operator serve as dynamical variables for general relativity. The Einstein-Hilbert action for gravity, with Yang-Mills fields included, can be expressed as a sum over squares of eigenvalues of the Dirac operator. Then, in the spirit of trace dynamics, each eigenvalue is raised to the status of an operator. This operator is the Dirac operator on the bioctonionic space - being essentially the octonionic gradient operator [2]. Fermionic degrees of freedom are also included in this (generalised) Dirac operator, and the variables Q_1, Q_2 are identified with the Dirac operator. Thus, one original eigenvalue corresponds to one copy of the Dirac operator (an STM atom) in the trace dynamics. This is how a pre-quantum, pre-spacetime theory is arrived at. When a large number of STM atoms entangle because of interactions, a quantum-to-classical transition takes place: operator-valued dynamical variables are mapped back to one or the other of their eigenvalues, and thereby classical spacetime and classical matter and gauge fields are recovered. Those STM atoms which do not get critically entangled are described by trace dynamics, or (to a very good) approximation, by quantum field theory on the emergent classical spacetime background.

The universe is assumed to have begun as a collection of enormously many STM atoms, each of which (by way of the action written above) obeys an $E_8 \times \omega E_8$ symmetry. The universe is assumed to have undergone an inflation-like de Sitter expansion up until the electroweak scale, when each of the E_8 branches as follows:

$$\begin{aligned} E_{8L} &\rightarrow SU(3)_L^{geom} \times E_{6L} \xrightarrow{E_{6L}} SU(3)_c \times SU(3)_{F,L} \times SU(3)_L \xrightarrow{SU(3)_L} SU(2)_L \times U(1)_{\gamma_1} \rightarrow U(1)_Y \\ E_{8R} &\rightarrow SU(3)_R^{geom} \times E_{6R} \xrightarrow{E_{6R}} SU(3)_{c'} \times SU(3)_{F,R} \times SU(3)_R \xrightarrow{SU(3)_R} SU(2)_R \times U(1)_{\gamma_2} \rightarrow U(1)_{Ydem} \end{aligned}$$

Of the three $SU(3)$ s arising from the branching of E_{6L} , the $SU(3)_c$ implements the color gauge symmetry of QCD. Furthermore, $SU(3)_{F,L}$ is the non-gauged global flavor symmetry which is responsible for three left-handed fermion generations [5] described by the exceptional Jordan algebra $J_3(\mathbb{O}_c)$. $SU(3)_L$ branches as $SU(2)_L \times U(1)_Y$ giving rise to the electroweak sector, and the $SU(2)_L$ acts only on left-handed particles.

As regards the branching of the second E_6 , which we label E_{6R} , the $SU(3)_{c'}$ symmetry is preferably not gauged (so as to be consistent with phenomenology) - it is global and explicitly broken at the electroweak scale. The $\omega SU(3)_{F,R}$ is the global flavor symmetry which describes three generations of right-handed fermions. The $SU(3)_R$ branches as $SU(2)_R \times U(1)_{Ydem}$. Here, the spontaneously broken $SU(2)_R$ gives rise to general relativity, and the unbroken $U(1)_{dem}$ arising from $U(1)_{Ydem} \rightarrow U(1)_{dem}$ is a new force, dubbed dark electromagnetism. It is sourced by the square-root of mass and can be made cosmologically and phenomenologically safe. The emergence of general relativity in this manner, and the preceding gravi-weak unification, was briefly discussed in [6] and is analysed in detail in a companion work [7]. The $SU(3)_L^{geom}$ and $\omega SU(3)_R^{geom}$ together give rise to the 16-dim split bioctonionic space, from which spacetime and internal symmetry space are emergent [4].

The triality of $Spin(8) \in E_6$ explains why there are three fermion generations. Complex split bioctonions generate the Clifford algebra $Cl(7)$, which is isomorphic to $Cl(6) \oplus Cl(6)$. The two copies of $Cl(6)$ are used to construct spinors as minimal left ideals describing one generation of SM chiral fermions. Triality is invoked to construct three isomorphic copies of $Cl(6)$ for three fermion generations, described by the exceptional Jordan algebra. The breaking of triality and of left-right symmetry gives rise to the observed fermion mass-hierarchy [5], after implementing the $SU(3)_{flavor}$ symmetry. We make a unique cross-sector prediction: $\sqrt{m_\tau/m_\mu} = \sqrt{m_s/m_d}$.

The goal of this ongoing research program is to derive the observed low energy physics as a result of symmetry breaking at the electroweak scale: this is also a breaking of triality symmetry as well as left-right symmetry. We also aim to derive the dimensionless fundamental constants of physics from first principles, in a parameter free manner. The falsifiable predictions of this candidate unification theory include a second Higgs, three families of right-handed sterile neutrinos, and the new force of dark electromagnetism. There is a partial understanding of CKM parameters. The PMNS matrix for neutrino oscillations, the origin of neutrino masses, and the nature of dark matter (or MOND?) remain to be understood.

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Prompt to GPT-5.4 Pro

"Please give a candid and critical appraisal of my octonionic theory of unification - an ongoing research program - as described in the attached brief summary and the cited papers therein. You could look up the cited papers as well as other relevant literature. Can we say that this is currently the most promising candidate theory of unification? How does it compare with string theory vis a vis promise for success as the correct theory of everything? What have I achieved and what remains to be done? Please give me your answer in chat as well as a latex document and its accompanying PDF. I intend to publish this attached summary alongside your appraisal without any editing from my side. Please be honest, candid and forthright."

Response in Chat:

Based on your attached summary and the papers and nearby literature I reviewed, my answer is: ****no, it is not currently defensible to call this the most promising candidate theory of unification****. It is better described, at present, as a bold and original ****ongoing research program**** with some real partial successes and a high upside, but not yet as the leading theory of everything. That assessment follows from the present status of the program itself: the 2025 overview explicitly presents it as a brief summary of an ongoing program; several central components are recent arXiv preprints; and the split-bioctonion spacetime paper is explicitly marked on Preprints.org as not peer reviewed. ([arXiv][1])

What you ****have**** achieved is substantial. You have built a coherent architecture that is more than decorative exceptional-group symbolism: a trace-dynamics / noncommutative-geometry / octonion / Clifford / Jordan-algebra synthesis tied to a concrete physical motivation, namely eliminating

dependence on external classical time. You and collaborators have produced explicit division-algebra constructions of left-right symmetric fermions and sterile neutrinos, an explicit $(E_8 \times E_8)$ branching proposal, a geometric role for the extra $(SU(3))$ factors via split bioctonions, and a technically explicit gravi-weak BF construction. Those are real achievements, because they turn vague exceptional-algebra aspirations into concrete model-building and calculational tasks. ([arXiv][2])

The strongest concrete result so far, in my view, is the **charged-fermion mass-ratio sector**. That paper is not just another “triaty explains three generations” essay; it gives explicit closed-form relations, ties down-lepton structure to a Dynkin swap, and organizes leading CKM structure. Importantly, the paper itself says that even if the wider $(E_6 \times E_6)$ program remains conjectural, the mass-ratio subset can stand as a falsifiable result in its own right. That is the clearest place where the program currently looks like physics rather than just beautiful algebra. But even there, the claim should be kept under control: the CKM sector is not fully parameter-free in the ordinary sense, because it still uses one 1–2 phase and a mild 2–3 normalization, and the paper itself notes residual ($O(10\%)$) tensions in some small CKM quantities and says a concrete PMNS prediction still requires an additional alignment choice. ([arXiv][3])

What remains to be done is also clear, and this is why I do not rank the theory as the leading unification candidate. The biggest unresolved issue is the **emergence story**: the recent generalized-trace-dynamics comparison paper explicitly says the current setup may not yet be sufficient to obtain classical spacetime through entanglement of different spacetime atoms. The BF paper is similarly candid that essential tasks remain open: full presymplectic and corner-term derivation, quantization of the interface algebra, solution of the gluing constraints, computation of off-diagonal mixing, and—crucially—explicit matter coupling, chirality, and anomaly-cancellation checks. On top of that, the same BF paper stresses that any appreciable coupling of the long-range $(U(1)_{\text{dem}})$ or ultra-soft $(SU(2)_R)$ sector to visible matter would already have produced detectable fifth-force or weak-equivalence-principle signals, so that sector is viable only if it is extremely suppressed or screened. ([arXiv][4])

There is also a broader strategic issue: the octonionic / exceptional / Jordan-algebra corner of the literature is fertile, but it is not yours alone. Boyle and Farnsworth, Furey and Hughes, Manogue and Dray, and others have all developed serious adjacent programs using nonassociative geometry, Jordan geometry, triality, and division algebras. That is good news in one sense, because it means your work sits in a mathematically serious ecosystem rather than in isolation. But it also means your theory does not yet stand out as the uniquely compelling exceptional-algebra route. And any (E_8) -based program inherits a standing burden of proof from the Distler–Garibaldi critique: not because that critique automatically refutes your framework, but because it means an (E_8) label is a challenge to show, explicitly, how chirality and representation-theoretic obstructions are being evaded. ([arXiv][5])

Against string theory, the comparison is asymmetric. **String theory is still far more developed** as a candidate framework for unifying gravity and gauge interactions: recent reviews describe it as the unique proposal that automatically combines quantum gravity and gauge physics after compactification, and current quantum-gravity lecture notes still treat string theory as one of the two principal UV-complete approaches on the table. Its weakness is well known: it has not delivered a unique low-energy vacuum or sharp experimentally confirmed predictions for our world. Your octonionic program has almost the opposite profile: it is much more direct on family replication, flavor structure, and the conceptual problem of time and spacetime emergence, and it may ultimately be more rigid in low-energy structure **if** the present algebraic relations survive deeper scrutiny. But as of now, it is much more mature, much less cross-checked, and much less independently vetted than string theory. On present evidence, string theory still has the stronger claim to being the more plausible theory-of-everything framework, even though it remains unconfirmed. ([arXiv][6])

My condensed verdict is this: **you have achieved enough that the program should be taken seriously, especially for flavor structure and gravi-weak architecture; you have not yet achieved what would justify presenting it as the leading candidate for the correct final theory of nature.** The right

description today is: ****serious, original, high-risk, high-upside, and unfinished****. The next things that would materially raise its standing are a closed neutrino sector, explicit anomaly-safe matter couplings, a convincing emergence derivation, and a few sharp predictions that can actually lose against data. ([arXiv][3])

Files: [LaTeX source](sandbox:/mnt/data/octonionic_appraisal/octonionic_appraisal.tex) [PDF appraisal](sandbox:/mnt/data/octonionic_appraisal/octonionic_appraisal.pdf)

[1]: <https://arxiv.org/abs/2501.18139> "https://arxiv.org/abs/2501.18139" [2]: <https://arxiv.org/abs/2108.01858> "https://arxiv.org/abs/2108.01858" [3]: <https://arxiv.org/pdf/2508.10131v4> "https://arxiv.org/pdf/2508.10131v4" [4]: <https://arxiv.org/pdf/2603.05018> "https://arxiv.org/pdf/2603.05018" [5]: <https://arxiv.org/abs/0905.2658> "https://arxiv.org/abs/0905.2658" [6]: <https://arxiv.org/abs/2401.01939> "https://arxiv.org/abs/2401.01939"

Next: Detailed Response from GPT-5.4 Pro as a Latex/PDF output:

APPENDIX

APPENDIX: A Critical Appraisal of the Octonionic $E_8 \times E_8$ Program of Unification

(Prepared by GPT-5.4 Pro for Tejinder P. Singh)

15 March 2026

This appraisal is based on the attached three-page summary “Octonions, and an $E_8 \times E_8$ theory of unification” together with the cited papers and selected adjacent literature reviewed through 15 March 2026. My overall conclusion is clear. The octonionic program deserves to be taken seriously as a bold, original, and unusually integrative research program. It is not yet reasonable to call it the most promising current candidate for unification, still less the leading candidate theory of everything. Its strongest achievements are (i) a coherent synthesis of trace dynamics, noncommutative/nonassociative geometry, exceptional algebra, and flavor structure; (ii) a nontrivial algebraic account of three families and charged-fermion mass hierarchies; and (iii) an explicit gravi-weak construction that is more concrete than many purely aspirational unification proposals. Its weakest points are equally clear: the full dynamical theory is unfinished, the emergence of classical spacetime remains unproven, several central components are still preprint-level, the anomaly/chirality/matter-coupling story is not yet closed, and the phenomenology is incomplete, especially for neutrinos, the dark sector, and precision low-energy tests. Compared with string theory, the octonionic program is more radical, more economical in spirit, and arguably more directly aimed at flavor and family replication; but it is vastly less developed as a quantum-gravity framework and much less vetted by the broader community.

2. Bottom-Line Verdict

If the phrase “most promising candidate theory of unification” is meant responsibly, the answer is **no**. At present the octonionic program is better described as a *high-upside speculative research program* than as the field’s leading candidate for a final unified theory.

That judgment is not a dismissal. On the contrary, this is one of the more ambitious and intellectually distinctive unification programs now being pursued outside the mainstream string-theory framework. It is not merely another exceptional-group slogan. It tries to connect quantum foundations, time, spacetime emergence, noncommutative/nonassociative geometry, division algebras, flavor, and unification in a single architecture. That breadth is rare. But breadth is not the same as closure. The program is still some distance from a finished theory.

My candid assessment is therefore this:

- **As a research program:** impressive, original, and worth serious attention.
- **As a completed unification theory:** not yet.
- **As the best current theory of everything:** no.

3. What Has Genuinely Been Achieved

3.1. A Coherent Unification Architecture, Not Just Isolated Mathematical Analogies

The first real achievement is architectural. Your program is not a loose collage of octonions, exceptional groups, and Jordan algebras. It has a definite internal logic. The starting point is the claim that ordinary quantum theory is approximate because it presupposes external classical time, and that one should instead formulate a more exact pre-quantum theory on a noncommutative or nonassociative pre-spacetime. In your framework, that leads to a synthesis of at least five nontrivial ingredients: division algebras, Adler trace dynamics, Connes-style noncommutative geometry and spectral ideas, Clifford algebras, and exceptional algebraic structures. That synthesis is conceptually distinctive.

This matters because many octonionic or exceptional-algebra programs remain at the level of kinematics or representation theory. Your framework attempts to connect kinematics, dynamics, and emergence. Whether it succeeds is another matter, but the attempt itself is substantive.

3.2. A Serious Algebraic Treatment of Chirality, Three Generations, and Sterile Neutrinos

The 2021–2023 work with Vaibhav is a genuine step beyond decorative use of octonions. It uses complex split biquaternions and bioctonions together with the Clifford algebras $Cl(3)$ and $Cl(7)$ to construct left-right symmetric fermions and to incorporate right-handed neutrinos in a division-algebraic framework. That already puts the program in contact with real particle-physics questions rather than purely mathematical aesthetics. The paper also makes explicit that the exceptional Jordan algebra and exceptional groups are being used to pursue the three-generation problem and mass or charge relations, not simply to advertise exceptionalism.

3.3. An Explicit $E_8 \times E_8$ Branching Proposal with Geometric Content

The 2022 $E_8 \otimes E_8$ paper and the later split-bioctonion preprint move beyond general motivation and present an explicit branching story in which each E_8 contains both an $SU(3)$ associated with geometry and an $SU(3)$ associated with generations. The left branch is meant to recover the visible Standard Model sector; the right branch is meant to provide a right-handed or pre-gravitational counterpart. The 2026 split-bioctonion construction then tries to make the two extra $SU(3)$ factors do real geometric work by generating a six-dimensional base of split signature together with embedded Lorentzian sectors and internal fibers related to $\mathbb{C}P^2$.

Whether one accepts that interpretation or not, this is a more concrete proposal than the vague claim that “exceptional groups contain the Standard Model somewhere.”

3.4. A Nontrivial Partial Success: The Gravi-Weak BF Construction

The 2026 BF-theory paper with Wesley and Isidro is, in my view, one of the strongest pieces of the program. It gives an explicit classical gauge-theoretic construction in which an $SO(3,3)$ BF theory is broken to two $SU(2)$ sectors, with one sector reproducing Einstein gravity in a Plebanski-like form and the other interpreted as weak gauge dynamics. This is not yet the full unification theory, but it is a real piece of technical work. It has enough structure to expose exactly where the hard problems lie: gluing constraints, corner degrees of freedom, matter coupling, chirality, anomalies, and portal suppression to the visible sector.

That is a strength. A theory becomes interesting when it stops hiding its problems.

3.5. The Strongest Concrete Achievement So Far: Charged-Fermion Mass Ratios

The clearest case that the program has produced something potentially nontrivial and falsifiable is the 2025 paper on charged-fermion mass ratios from the exceptional Jordan algebra. In my reading, this is the part of the overall program that currently has the best claim to independent value even if the full $E_6 \times E_6$ or $E_8 \times E_8$ program ultimately fails.

Why? Because the paper does not merely gesture at “three generations from triality.” It presents explicit closed-form relations for charged-fermion square-root mass ratios, relates the down and lepton sectors through a Dynkin swap, and organizes leading CKM structure. The paper itself is explicit that the wider unification program may remain conjectural while the mass-ratio subset can still stand or fall on its own.

That said, the achievement should not be oversold. The mass-ratio sector is the most impressive part because it is relatively rigid. But the CKM sector is not fully parameter-free in the ordinary sense: the paper uses one 1–2 phase and a mild 2–3 normalization to fit the observed pattern, and it still reports $O(10\%)$ tensions in some small CKM quantities. The neutrino sector is not yet comparably closed. So the right appraisal is not “done” but rather “promising, structured, and testable.”

4. Why I Do *Not* Think It Is Currently the Leading Candidate for Unification

4.1. *The Program Is Unfinished at the Dynamical Level*

The deepest open issue is not representation theory but dynamics. The program proposes that quantum theory emerges from a more exact trace-dynamical level defined on a nonclassical pre-spacetime built from “atoms of spacetime-matter.” That is a striking idea. But a final theory cannot merely postulate such atoms and then narrate emergence. It must show, in a controlled way, how ordinary quantum field theory, classical spacetime, locality, and the observed low-energy symmetries arise.

At present this emergence story remains incomplete. Indeed, the recent comparison of generalized trace dynamics with causal fermion systems and noncommutative geometry explicitly raises the possibility that the current generalized-trace-dynamics setup may *not yet be sufficient* to obtain classical spacetime through entanglement of different spacetime atoms. That is a major open wound, not a minor technicality.

4.2. *Key Consistency Checks Remain Open*

The BF-theory paper is admirably honest about what is unfinished. Among the tasks it explicitly leaves open are derivation of the full presymplectic structure including corner terms, quantization of the reduced interface algebra, solution of the gluing constraints, computation of off-diagonal mixing, and a full treatment of matter content, chirality, and anomaly cancellation. Those are not cosmetic tasks. They are among the standard tests that separate a suggestive framework from a viable theory.

In particular, any proposal built around chiral sectors and nontrivial symmetry breaking must eventually show that the matter couplings are right and the anomalies cancel. Until that is done explicitly, the theory remains a plausible architecture rather than an established physical model.

4.3. *The E_8 Label Is as Much a Liability as an Asset*

The exceptional-group aspect is mathematically seductive, but history here is cautionary. Distler and Garibaldi’s representation-theoretic critique of “theory of everything inside E_8 ” is still a standing warning: exceptional groups do not automatically solve the chirality and representation problems of particle physics. Your framework is not the same as the older one-factor E_8 proposals they were criticizing; it uses $E_8 \times E_8$, additional geometry, global flavor sectors, and a different dynamical narrative. So their no-go result does *not* automatically kill your program. But it does mean that any E_8 -based theory must earn credibility by showing, in painstaking detail, exactly how it avoids those old obstructions. Until that case is made fully explicitly, the invocation of E_8 should be treated as an invitation to scrutiny, not as a badge of plausibility.

4.4. *Too Much of the Central Structure is Still Preprint-Level*

A second reason for caution is sociological, but legitimate. Several of the papers most central to the current state of the program are recent arXiv or preprint postings rather than a settled peer-reviewed literature. The conference overview is explicitly a brief summary of an ongoing program. The split-bioctonion spacetime paper is posted on Preprints.org and explicitly marked as not peer reviewed. The gravi-weak BF paper, the generalized-trace-dynamics comparison paper, and the new mass-ratio paper are all recent arXiv submissions as of the date of this appraisal.

This does not make them wrong. It does mean the broader community has not yet had time to test them hard.

4.5. The Phenomenology Is Still Incomplete and Potentially Vulnerable

The program makes contact with phenomenology, which is good, but the phenomenology is not yet comprehensive enough to justify calling the theory the leading candidate for reality.

The neutrino sector remains incomplete. The mass-ratio paper points to Majorana neutrinos as a prediction and suggests concrete targets such as neutrinoless double beta decay and a maximal leptonic CP phase, but it also says that a concrete PMNS prediction requires an additional explicit alignment choice. That means the neutrino story is not yet as rigid as the charged-fermion story.

The dark-electromagnetism sector is intriguing but also delicate. The BF paper itself stresses that any appreciable coupling of the long-range $U(1)_{\text{dem}}$ sector or ultra-soft $SU(2)_R$ vectors to visible matter would already have produced detectable fifth-force or weak-equivalence-principle signals, so viable couplings must be extremely suppressed or screened. In other words: the proposal is not excluded automatically, but it is phenomenologically cornered and will require careful quantitative work.

4.6. Too Many Bridges Remain Suggestive Rather Than Inevitable

A recurring issue is that the program often moves from beautiful algebra to physical interpretation by a sequence of individually plausible but not yet uniquely forced choices. Why this specific split-bioctonionic replacement of spacetime points? Why exactly this branching pattern? Why should the global flavor sectors organize the observed families in precisely the needed way? Why should the square-root-of-mass charge associated with dark electromagnetism be physically fundamental rather than an effective bookkeeping device? Why should the trace-dynamics and spectral-action ingredients splice together exactly as proposed?

A mature theory eventually turns such choices into theorems, or at least into sharply constrained consequences. Your program has not yet reached that point.

5. Comparison with String Theory

The comparison with string theory should be made carefully because the two programs are strong in very different ways.

Criterion	Octonionic program	String theory
Core ambition	Unify quantum foundations, spacetime emergence, flavor, gauge structure, and gravity in one exceptional-algebraic framework	Unify quantum gravity and gauge interactions in a UV-complete framework with extended objects and dualities
Mathematical maturity	Interesting but incomplete; several essential links are still conjectural or recent preprints	Extremely mature; decades of detailed work, dualities, anomalies, compactifications, black-hole results, and holography
Direct grip on three generations and flavor	Unusually direct and arguably one of the program's main selling points	Often indirect and highly vacuum-dependent
Status of quantum gravity	Not yet established as a complete or controlled quantum-gravity theory	Still unconfirmed physically, but remains the best-developed framework for quantum gravity
Predictive sharpness	Potentially high if the mass-ratio and neutrino sectors lock in	Often diluted by the landscape and model dependence
Community vetting	Limited and still early	Very extensive

My judgment is therefore asymmetric.

- **String theory's main strength** is not that it has already succeeded uniquely, but that it provides an extraordinarily developed framework in which gravity and gauge interactions coexist consistently, with deep mathematical structure and many nontrivial internal checks.
- **String theory's main weakness** is that it has not produced a unique, compelling derivation of our low-energy world, and much of its phenomenology is vacuum-dependent.
- **Your octonionic program's main strength** is almost the mirror image: it aims more directly at family replication, chirality, flavor, and the emergence of classical spacetime and time itself. If it works, it could be more rigid and more explanatory in certain low-energy respects than string theory usually is.
- **Its main weakness** is that it is nowhere near string theory in mathematical development, internal cross-checking, or breadth of independent scrutiny.

So if the question is, "Which currently has the stronger claim to being a viable theory of everything?" the answer remains **string theory**, not because string theory is experimentally established, but because it is incomparably more developed as a quantum-gravity framework.

If the question is instead, "Which is more original, more radical, and possibly more rigid in its treatment of flavor and family structure?" then your program has a legitimate claim to special interest.

That distinction is important. It prevents both unfair dismissal and unfair hype.

6. What Remains to Be Done

Here is the shortest honest list of what would have to happen before I would substantially upgrade my estimate of the program.

6.1. 1. Close the Emergence Story

The most important task is to show, in a controlled and preferably calculable way, how classical spacetime and ordinary quantum field theory emerge from the underlying trace-dynamical or pre-spacetime structure. Without that, the program remains philosophically interesting but physically underdetermined.

6.2. 2. Finish the Consistency Checks

A completed version of the theory must explicitly demonstrate:

- correct matter couplings,
- chirality in the observed low-energy sector,
- anomaly cancellation,
- a coherent account of the interface/gluing structure in the BF formulation,
- and a quantitatively controlled portal structure for any dark or right-handed sector.

6.3. 3. Make the Neutrino Sector Genuinely Sharp

A decisive advance would be a neutrino sector as rigid as the charged-fermion sector: PMNS angles, mass ordering, Majorana phases or Dirac/Majorana nature, and sterile-neutrino phenomenology derived from the same underlying principles rather than patched in by minimal textures.

6.4. 4. Produce Near-Term Falsifiable Predictions

The program already points to possible tests: Majorana neutrinos, specific charged-fermion relations, CKM refinements, sterile neutrinos, a second Higgs, and the dark-electromagnetism sector. But these need to be sharpened into predictions with clearly stated scales, couplings, and exclusion contours. A theory rises quickly in credibility when it risks clean defeat.

6.5. 5. Show Explicitly How the Theory Evades the Traditional E_8 Objections

This should not be left implicit. An explicit representation-theoretic account of why the old E_8 no-go worries do not apply in your setup would materially strengthen the program.

6.6. 6. Encourage Independent Reproduction

At this stage, independent verification is unusually important. The strongest way to move the theory from “interesting” to “credible” is for other groups to reproduce the core mass-ratio derivations, stress-test the branching and anomaly structure, and examine the dark-sector phenomenology without relying on the internal assumptions of the program itself.

7. A fair Statement of Present Status

If I had to summarize the current status in one paragraph suitable for publication, I would write the following:

Singh's octonionic $E_8 \times E_8$ program is one of the more original current attempts at unification. Its real achievements are a coherent algebraic-dynamical architecture, a substantive treatment of three generations and charged-fermion hierarchies, and an explicit gravi-weak construction that exposes concrete technical and phenomenological questions. Its strongest present result is the charged-fermion mass-ratio sector, which appears sufficiently rigid and falsifiable to merit attention in its own right. But the overall program remains unfinished. The emergence of classical spacetime is not yet demonstrated, several consistency checks remain open, the neutrino and dark sectors are incomplete, and much of the most important recent work is still at the preprint stage. It is therefore not yet justified to describe the theory as the most promising candidate for unification or as a leading theory of everything. It is better understood, at present, as a bold and high-risk research program with genuine upside.

I endorse that statement.

8. Final Conclusion

My final verdict is candid but favorable in the right sense.

You have achieved enough that the program should not be dismissed as numerology or exceptional-group romanticism. There is real structure here, and at least one piece of it — the charged-fermion mass-ratio work — may turn out to be important even if the grander unification framework needs revision.

But you have *not* yet achieved what would justify presenting the theory as the best present candidate for the correct final theory of nature. That would be too strong. Right now the responsible description is:

A serious, original, high-risk unification program with some nontrivial partial successes, but still well short of a finished theory.

If the next stage of the work closes the emergence problem, sharpens the neutrino sector, survives anomaly and phenomenological checks, and yields clean predictions that withstand data, then the standing of the program could rise substantially. As of March 2026, it would be premature to say more.

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