Author's responses are in red.

1 Summary and Overall Assessment

The article "Black Holes Do Not Exist" argues that massive compact objects (MCOs) should not be considered as standard black holes described by stationary solutions of Einstein's field equations. Instead, the author proposes that MCOs are "space blisters". These are horizonless objects undergoing continuous asymptotic collapse without any central singularity. The author interprets the Oppenheimer-Snyder (OS) collapse model as supporting their proposal and applies its late time asymptotic equations to Sagittarius (Sag) A*. The article also heavily criticizes the current scientific consensus on black holes, specifically stationary metrics, regular extensions across event horizons, and the regular continuation of infalling worldlines across the event horizon.

In my review, I found that the paper has several foundational and interpretive errors. The review is hence focused on highlighting and correcting all these errors with commentary on possible improvements.

2 Major Critical Comments

2.1 Definition of a black hole and interpretation of observational evidence

In the introduction, the author has correctly identified the 3 most important properties of a classical Schwarzschild black hole but only the event horizon is definitional:

1. For the first property, collapse below the Schwarzschild radius is specific to spherical bodies and rotating black holes are described by Kerr geometry and have different horizon radii. Hence this property is not definitional.
2. For the second property, the event horizon is actually the formal definitional property since a black hole is a region of spacetime containing events from which no causal signal can ever reach future null infinity.
3. The central singularity is a predicted property of black holes coming from classical general relativity and the properties depend on the model. Schwarzschild has a spacelike $r = 0$ singularity, while Kerr has a ring singularity in its ideal analytic solution. Singularity is therefore not a defining feature.

In a later section titled "Which model is better?" the author raises the valid point that current observations linked to black holes do not necessarily prove that black holes are the only explanation. In general, the existence of an alternative model does not falsify the theory of black holes. Falsification (that too at best for a specific astrophysical object) would require evidence incompatible with the predictions of the relevant black hole model. The author does not provide any such observation. Also, they do not establish any theoretical inconsistency in the models discussed in their article. Hence, the overall conclusion that black holes do not exist is an overstretch.

The article does not aim to empirically falsify the black hole model. It is impossible in the light of the currently available empirical data. Other methods are used to judge merits of each model, which necessarily are not as persuasive as empirical evidence. And yes, there is a plenty of theoretical problems in models of black holes. The previous referee (the established astrophysicist) thinks that the information paradox is the most famous example.

2.2 Stationary solutions objection

In the sections 'What mistakes have been made?' and 'Which model is better?', the author suggests that the stationary Schwarzschild and Kerr solutions are physically inappropriate because Einstein's field equations are dynamical. This reasoning is not correct. Dynamical field equations can consistently admit both time dependent and stationary solutions which the author also seems to later agree upon explicitly in response to other reviews. Stationarity is a property of a particular solution and does not contradict the dynamical character of the underlying equations.

I agree that this reasoning is not correct. But this is not the reasoning contained in the paper. That stationary models are inappropriate for MCOs is the conclusion, not an argument.

The author bases their rejection of stationary solution in the claim that the matter distribution inside a collapsing object evolves with time and thus cannot be represented by a stationary solution. This does not invalidate Schwarzschild or Kerr modeling. These metrics are vacuum solutions and do not assume a stationary matter distribution within a collapsing star. They are not intended to describe the entire formation process. In spherical collapse, Birkhoff's theorem (also correctly invoked by Gemini's in its report) implies that the vacuum exterior is Schwarzschild even while the interior matter distribution evolves. For a rotating object, the exterior geometry can approach Kerr after dynamical disturbances decay. Stationary exterior solutions and dynamical collapse solutions are for different purposes. They are not two different models of the same system but rather two different situations. We cannot choose one over another for mathematical convenience. Ironically, in the section "Which model is better?" the author attempts to emphasize that in their "space blister" model, the coordinates of distant observers are sufficient, because they are supposedly intuitive and mathematically more elegant to them. The author provides no theoretical or observational demonstration that the stationary character of Schwarzschild or Kerr makes these geometries physically inappropriate for massive compact objects.

No theoretical demonstration? Isn't the entire section "Which model is better?" about the theoretical demonstration? I enumerate there a few criteria by which to judge models and then use them. Also, I do not criticize Schwarzschild and Kerr metrics for their exterior properties but because of their interior properties.

The comparison with cosmological solutions is also misleading. Stationary solutions were rejected because of instability and observational evidence for cosmic expansion. The black hole "paradoxes" are about curvature singularities, global extensions, or compatibility with quantum theory. These paradoxes do not arise from the stationarity of Schwarzschild and Kerr solutions. The author reemphasizes this in the section "Which model is better?" that abandoning the theory of black holes in favor of space blisters can solve all the paradoxes easily, but again without any justification.

I wrote this example in the response to ChatGPT: space blisters -> no singularities -> no cosmic censorship. I think the "author did not show how this theory solves the paradoxes" argument comes from not understanding the space blister model. Instead, I suggest you try showing that, for example, information paradox can exist also in the space blister model. That would be a good counterargument. Good luck with that, though.

2.3 Misinterpretation of the OS model

The author misinterprets the OS model for a permanent asymptotic collapse model in which neither an event horizon nor a central singularity form. This interpretation contradicts the physical content of the original OS solution which actually describes the dynamical collapse of a homogeneous, spherically symmetric, pressure-less body. Although the surface approaches the Schwarzschild radius only when the exterior Schwarzschild coordinate time coordinate tends towards infinity, observers comoving with the collapsing matter cross the horizon and reach zero areal radius after a finite proper time (See the abstract of the paper and then at page 455). Furthermore, Equation (21) shows that the areal radius vanishes at the finite comoving proper time $\tau = -G/F$, while equation (22) shows that the matter density diverges there. This clearly shows a central singularity. At page 459, formation of event horizon is also described even though modern terminology "event horizon" is not used.

First, I am not sure what on page 455 you refer to besides the abstract, because this is where the abstract is. The abstract says: "The total time of collapse for an observer comoving with the stellar matter is finite, and for this idealized case and typical stellar masses, of the order of a day;" Then, on page 459, the authors state: "(…) τ tends to a finite limit (…) After this time τ_0 an observer comoving with the matter would not be able to send a light signal from the star; the cone within which a signal can escape has closed entirely. For a star which has an initial density of one gram per cubic centimeter and mass of 10^{33} grams this time τ_0 is about a day."

τ_0, which the authors write about both in the abstract and in the fragment on page 459, refers to the "remaining time," i.e., the amount of proper time within which the comoving observer reaches the event horizon, not the zero areal radius. Right? Why do you think the authors are writing about the time necessary for reaching the zero areal radius rather than the event horizon?

Secondly, could you point me to a source which analyzes the OS paper in this way? A textbook, or a lecture script, or a paper? Something that would interpret the equations in the OS paper with a collapse to a zero areal radius in mind. Did you take the above interpretation from such a source or did you come up with it by yourself?

Thirdly, Oppenheimer and Snyder set up their model in equations (2)-(5), which are valid only in the Schwarzschild coordinates, in accordance with equation (1), that is they are valid only during the phase of the collapse before the horizon has been formed. Then, they write on page 456: "We have been unable to solve these equations except when we place the pressure equal to zero." Then on page 457 (top of the right column): "For the solution of this problem, we have found it convenient to follow the earlier work of Tolman and use another system of coordinates, which are comoving with the matter." These quotes clearly indicate that the problem O & S are trying to solve is to find the solution to equations (2)-(5) which describe the situation before event horizon has been formed. The comoving coordinates are introduced explicitly for the purpose of solving equations (2)-(5), and not to investigate what happens after the event horizon has been formed. In other words, O & S made it explicit in their paper that they did not want to analyze anything related to the formation of the event horizon or thereafter, don't they?

Finally, for $\tau = -G/F$, equation (21) implies $e^{\omega} = 0$. This is true. But why do you think that Oppenheimer and Snyder were thinking about it/were interested in it/their model was about it? They never raised this argument themselves – it was raised by somebody else somewhere else, not in their paper. So, isn't it just a conclusion added later by other physicists who (unlike Oppenheimer and Synder) were analyzing what happens after the event horizon has formed?

> Yes. My original citation was imprecise because I did not distinguish properly two different finite proper times in the OS solution:
>
> 1. **Finite proper time to reach the causal boundary/event horizon**: On page 456, the paper mentions that although the asymptotic isolation takes infinite Schwarzschild coordinate time for a distant observer, "for an observer comoving with the stellar matter this time is finite and may be quite short." Later on page 459, in the paragraph after Eq. (37), it is shown that for fixed value of R, as $t \to \infty$, the comoving proper time τ tends to a finite limit denoted as τ_0 and after this τ_0 time, the comoving observer won't be able to send a light signal from the star. This corresponds to crossing the event horizon in modern phrasing.
>
> **Sure, it does. But this phrasing is also compatible with space blisters. Note that in the space-blister model the comoving observer isn't able to send a signal after the finite limit denoted as τ_0, because this limit is never reached. So, there is nothing in their paper that can be linked specifically to reaching or crossing the event horizon.**

2. **Finite proper time to reach the zero areal radius**: Here I should have cited equations (21) and (27) since they together show that at a finite value of $\tau = -G/F$ the areal radius reaches zero and by eq. (22) we see that the density also diverges there. The paper does not explicitly identify this limit as a central curvature singularity. This is the standard modern interpretation of the limiting behavior of the OS solution. I tried to find some paper that exactly mentions the same OS paper equations and arguments, like for example, section 32.4 of the book "Gravitation" by Misner, Thorne, and Wheeler also treats the singularity as a feature of the OS collapse model.

So, we have a few equations which, if we extend the spacetime considered by OS, can be interpreted as indicating a collapse to a central singularity. This is not the interpretation of the paper's authors but this is an interpretation added sometime later by other people. Thank you. That is my point.

The section 32.4 is based not on the OS model but on the model of Beckedorff and Misner. It is strange how hard it is to find an authoritative source with the derivation of this "standard modern interpretation" of the OS model.

Coming to the third and fourth questions, it is crucial to note that changing coordinates does not mean that Oppenheimer and Snyder changed to a different physical problem. They solved the same collapse problem by introducing comoving coordinates and later related those coordinates back to the Schwarzschild coordinates. They showed that while the Schwarzschild coordinate time t tends to infinity, but the comoving proper time remains finite. While they do not explicitly mention a central curvature singularity, the absence of such terminology does not remove these properties from the solution. The causal boundary follows from their finite τ_0 result after Eq. (37), while Eqs. (21), (22), and (27) imply collapse to zero areal radius where density diverges at finite comoving proper time. These are mathematical consequences of the solution even if the OS paper did not explicitly state them.

Yes, changing the coordinates does not change the problem. So, what is the problem they are trying to solve? I asked you whether by defining their problem by equations (2)-(5), O & S make it explicit that they do not want to analyze anything beyond what can be described using external observer's coordinates. You dodge this question because you would like to answer "no" but the correct answer is "yes." They do not mention central curvature singularity because their model is not about it. The causal boundary follows from their equations with additional assumptions in place (which they do not make). So, these are not the mathematical consequences of their solution, these are the mathematical consequences of their solution plus some assumptions added later by other people.

2.4 Pulsar clock and gravitational redshift

In the section ‘What exists instead?’, the author tries to build a thought experiment involving an accreting pulsar whose locally measured rotational period is assumed to remain constant. This assumption is not generally justified. Accretion changes mass, radius, internal structure, angular momentum, and moment of inertia which ultimately cause the spin period to evolve. Even if this assumption is admitted, the ever-slowing angular velocity does not logically connect to the absence of an event horizon. The pulsar may collapse to a black hole anyway in finite time forming an event horizon and the distant observer would still see an ever-slowing angular velocity due to red-shifted signals. The received pulse frequency approaches zero in Schwarzschild coordinate time but it does not imply that the pulsar’s local clock is physically slowing in the same way.

No, I am not building a thought experiment. In the same way as a description of what a person falling through an event horizon may feel is not an argument for a black hole, this description is not an argument in favor of my theories (so it does not have to “logically connect”). Unfortunately, the referee keeps taking introductory overviews and illustrative descriptions as my arguments. Okay, so, what then are the arguments I am making? First, the article defines space blisters as objects modeled by a certain class of models. An example of a model from this class is the OS model. The most important arguments should then concern the comparison between the two classes of models: the space blister models and the black hole models. These arguments can be found in the “Which model is better?” section. Then, there are other arguments peppered throughout the article, aimed at people who have some incorrect preconceived notions, such us that an incomplete spacetime necessarily violates equivalence principle or that the OS model is about the collapse into a black hole.

2.5 Termination of proper time

In the section “What mistakes have been made?”, the author briefly states that the flow of time must end as an in-falling observer reaches towards massive compact object. They better described this in the section “What exists instead?”. The author here defines “remaining time” as the proper time required to reach the to be horizon and claims that since the collapse is asymptotic therefore the remaining time slows down and eventually stops for the in-falling observer. This is incorrect because proper time does not slow down but rather ticks normally and the horizon is reached in proper time as noted by the original OS model paper as well.

Although the author is correct to say that timelike geodesics inside a classical black hole can terminate at the central curvature singularity after finite proper time as classical general relativity cannot extend those geodesics further. Such a curvature singularity does not exist at the event horizon hence the claim made by author that the flow of time will end cannot be supported by the fact that timelike curves end at the curvature singularity. In the space blister model, there is no geometric feature that stops the timelike geodesics at the horizon.

Why do you think that "remaining time slows down"? Could you give a quote from the article that made you think that? Wasn't the remaining time precisely defined as a measurable amount of time ("…the proper time required to reach the event horizon."), like, for example, 3 hours?

> The article defines "remaining time" as the finite amount of proper time required for the infalling observer to reach the would-be horizon. Then they claim that this finite proper time is never completed because just before the horizon is reached "time itself ends". However, proper time along the infalling observer's worldline does not slow down or freeze at the horizon. I would emphasize that proper time does not slow down relative to the observer's own clock. Any apparent slowing is relative to another observer or coordinate time. The author's argument appears to use the asymptotic behavior of Schwarzschild coordinate time as motivation for terminating the observer's worldline after a finite amount of proper time.

> **Yes, the observer's proper time does not slow relative to his own clock. Obviously. I even explain what observer experiences until the very last moment and there is nothing about slowing time there. So why do you think that my arguments are confused? I asked for a quote, where is it?**

2.6 Incomplete flat spacetime example

To support the claim that time can end without a singularity, the author introduces the flat spacetime M:

$$d\tau^2 = e^{-2t^2}dt^2 - dx^2 - dy^2 - dz^2, (t, x, y, z) \in \mathbb{R}^4.$$

Define a new time coordinate by

$$T(t) = \int_0^t e^{-u^2}du.$$

Since $dT = e^{-t^2}dt$, the metric becomes

$$d\tau^2 = dT^2 - dx^2 - dy^2 - dz^2.$$

This is the ordinary Minkowskian metric. However, because the integral converges as $t \to \pm\infty$, the coordinate T covers only the finite interval

$$-\frac{\sqrt{\pi}}{2} < T < \frac{\sqrt{\pi}}{2}.$$

The author's spacetime is therefore only a finite temporal portion of Minkowskian spacetime. The finite proper time of the timelike curves do not refer to any physical mechanism to stop time. It results from removing the regular Minkowskian regions before and after this finite interval. The spacetime is geodesically incomplete but smoothly extensible across both boundaries. There is no curvature singularity or other geometric

obstruction preventing the continuation. This artificially restricted spacetime M hence cannot justify terminating worldlines at the proposed boundary of a "space blister".

It is true that M is a subset of the regular Minkowskian spacetime. But why is this a problem? From mathematical point of view, everything is correct: M satisfies the definition of spacetime – a time oriented Lorentzian manifold. The definition does not say that it must be fully extended. From physical point of view, Einstein equations are also satisfied at every point of this manifold. So, it does not violate any rules of general relativity. Or does it? It seems that you don't like it because this spacetime can be extended. Is there a rule that spacetime must be always maximally extended? This is a theme criticized by the article: people keep extending the spacetime without providing a justification for it. You seem to be doing here exactly what the article accuses physicist of incorrectly doing: assuming that "the time must continue" without a reason. Keep in mind that the claim made in the article is: "This illustrates how, without violating the laws of general relativity, the flow of time can stop even in a flat spacetime." So, does this example illustrate it? And if not, how does it violate the laws of general relativity?

> The spacetime M does not violate general relativity but its timelike curves terminate only because the remaining regular region has been excluded artificially. There is no physical or geometric structure, such as a curvature singularity, metric not being differentiable, etc. that prevents the smooth extension of M to include the artificially omitted region.
>
> **Thank you. So, the example achieves the goal I set out for it. You don't like it because you are making the "the flow of time must continue" assumption.**

2.7 Rejection of horizon penetrating coordinates

The author regards Eddington-Finkelstein (EF) and Kruskal-Szekeres (KS) coordinates as unnecessary constructions introduced to preserve the "the flow of time must continue" assumption. This is invalid reasoning. In Schwarzschild coordinates, the metric components become singular at the Schwarzschild radius, but curvature quantities remain finite there. The breakdown therefore belongs to this specific coordinate chart not to the spacetime geometry.

EF and KS coordinates are used because they remain regular at this boundary and permit the geodesic equations to be continued without singular behaviour at the horizon. The same conclusion follows directly from the comoving coordinates used in the OS solution (see section II of OS's paper).

The author's preference for coordinates associated with a distant stationary observer on grounds of simplicity or intuition has no physical merit. Failure of a coordinate chart cannot imply that the spacetime itself inextensible.

Again, the important question is what the manifold is. What coordinates we use is secondary. I do not like EF and KS coordinates because they are used to describe events which, in my opinion, do not belong to the actual spacetime manifold, i.e., they describe regions of spacetime that are unphysical. I already explained that in the previous rebuttal.

2.8 Claimed resolution of black hole problems

In the section 'Which model is better?', the author claims that several problems associated with black holes can be easily resolved by replacing them with space blisters. It is conditionally true that a model containing no event horizon or singularity would logically not contain problems that depend upon them. But this does not provide evidence that such a model exists or that black holes do not.

Yes, but the existence of such a model provides evidence that it exists. You can find an example of such a model in the article.

White holes and additional exterior regions belong to particular maximal extensions (region III and IV of the KS diagram) and are not required in an ordinary stellar collapse spacetime. Cosmic censorship is a conjecture regarding the visibility of singularities rather than a paradox caused by stationary black hole solutions. Questions arising outside the classical model cannot be resolved just by proposing an alternative classical geometry. Each claimed resolution thus requires details of which assumptions generate which problems and how a mathematically consistent space blister model avoids them. Hence the author's claims on space blister being easy solution of several "black hole paradoxes" is both irrelevant and exaggerated.

I disagree. Once you understand the space blister model it is fairly obvious, at least for some of these problems, that they would not exist if the model reflected reality. For example, information loss paradox: at every hypersurface of simultaneity the information about the state of the matter falling into a space blister is preserved for the same reason it is preserved in any other system – all particles have an identifiable state and are subject to the known laws of physics.

2.9 Calculations regarding Sag A*

In the section "What is Sagittarius A*?", I appreciate the author for considering the corrected versions of Oppenheimer and Snyder's equations (38) and (39) as provided by Mitra and Singh. But the subsequent calculations made by the author still do not show that the OS model prevents the formation of an event horizon or the subsequent singularity in the case of Sag A*.

1. The coordinate time t is incorrectly taken as the age of the Milky Way. In the OS model, the correct quantity would be the Schwarzschild coordinate time elapsed since a specific initial collapse configuration consistent with the assumptions of the OS model. The author assumes Sag A* to enter this configuration as soon as the Milky Way formed, but provides no

justification for that assumption. Sag A* may have gained mass through accretion and mergers during its history, whereas the OS model assumes a spherical body with constant mass. The age of the Milky way hence can only be a rough upper bound on the age of Sag A*, not the value of t. The correct value of t is undetermined without a formation model and appropriate initial conditions.

This is somewhat nitpicky. You can make the same criticism about the original OS paper – it is hyper-unrealistic but it communicates the idea effectively.

2. In the second point, the Schwarzschild radius calculation is correct. While Sag A* may be rotating, for a simple crude model as stated by the author, the Schwarzschild model is a good approximation.

3. In the third point, the fraction R/R_s is a dimensionless comoving shell label and does not exactly signify the distance from the origin as R is not the instantaneous physical or proper distance from the center. The author further makes an unjustified assumption that the boundary can be assumed as the Schwarzschild radius, although the asymptotic approach occurs only in Schwarzschild coordinate time.

Why is this assumption unjustified? One referee made a calculation that after 3 hours the star collapses to within a Planck length from the Schwarzschild radius. So, its boundary is pretty close to the Schwarzschild radius, isn't it? Do you think that the outer shell of the collapsing star could be significantly away from the Schwarzschild radius after billions of years (as measured by an external observer)?

> I am not claiming that the outer surface cannot become extremely close to the Schwarzschild radius. It can. My objection is that the article interprets the asymptotic coordinate behaviour as a physical statement that the stellar boundary permanently remains just outside the event horizon. For a distant Schwarzschild observer, the areal radius of the outer surface may become numerically extremely close to the Schwarzschild radius. But my overall point is that this does not support the existence of a permanently horizonless boundary because in finite comoving proper time the outermost surface will also cross the horizon.
>
> **Schwarzschild coordinates are used for calculations, and you yourself say that "For a distant Schwarzschild observer, the areal radius of the outer surface may become numerically extremely close to the Schwarzschild radius," so why do you object to the boundary being almost at the Schwarzschild radius? This does not make sense.**

<u>2.10 Faulty interpretation of the calculations</u>

The physical interpretation given after the table is also incorrect. First, the value $e^{\lambda} = 1$ at the center does not establish that the surrounding spacetime is flat. A single metric component evaluated at one location contains insufficient information about curvature. Flatness should be derived from the metric derivatives or curvature tensors. A regular center

can satisfy $e^{\lambda} = 1$ while the spacetime in its neighborhood remains curved. This point was also raised by the anonymous reviewer (see page 3 of that review).

In response to the anonymous referee, the author responds that local flatness "has nothing to do with specific values of the components of the metric tensor." This point is correct but directly undermines the interpretation presented in the article. The "locally flat" is a generic property of every smooth spacetime and provides no evidence that the curvature vanishes. To actually demonstrate that the spacetime is flat, author should have means showed that the Riemann curvature tensor vanishes in a neighborhood. The evaluated limit at $R = 0$ corresponds to the regular central origin of the event horizon, not to the later curvature singularity. Consequently, the author's calculation does not evaluate the geometry at the singularity or demonstrate its absence.

Yes, but where does it say in the article that the spacetime/space is flat? The article only says that it is locally flat, which, as you point out, is trivially true for every smooth spacetime. So, what is the mistake here?

> Here my objection was not that the author claimed the entire spacetime is flat. The author is right that the article describes the center as locally flat, and in my review, I did not oppose this generic property of a regular spacetime event. The issue is instead the inference drawn from the center calculation. The absence of a curvature singularity cannot be established from $e^{\lambda} = 1$ at one event. One would need to examine the relevant curvature quantities like Riemann curvature tensor in the limiting region and show that they remain finite.
>
> **This is incorrect. If the metric tensor is finite, differentiable, and generally "well-behaved", then the curvature is finite and the existence of a singularity can be ruled out at that point. Also, you yourself write that this point corresponds to the "central origin of the event horizon, not the later curvature singularity," so you seem to be in agreement that there is no singularity here. This sounds self-contradictory.**

At the boundary, the collapsing matter is moving in the radial coordinate. Its proper time must therefore be calculated from $d\tau^2 = e^{\nu}dt^2 - e^{\lambda}dr^2$, not from e^{ν} alone.

Would the calculation in the article be correct for a stationary observer close to the Schwarzschild radius rather than an observer comoving with the boundary?

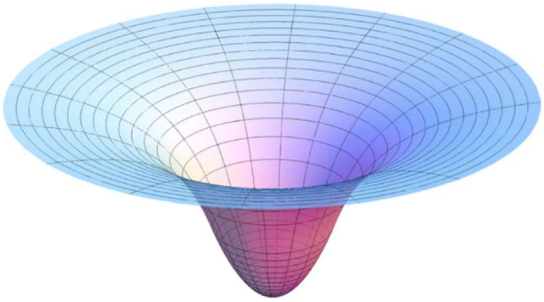
> Yes, for a stationary observer at fixed radius outside the Schwarzschild radius, the radial term vanishes. Hence the proper time satisfies $d\tau = \sqrt{e^{\nu}}dt$. Such an observer would not be comoving with the collapsing boundary considered in the OS model.
>
> **Ok, thank you. I made an appropriate change in the text to avoid this problem.**

At constant t, the radial distance and volume elements are:

$$dl_r = \sqrt{e^{\lambda}}dr, dV = \sqrt{e^{\lambda}}r^2 \sin\theta \, dr \, d\theta \, d\phi.$$

But its divergence near the horizon only reflects the Schwarzschild coordinate spacings and does not establish a blister shaped geometry. Schwarzschild coordinates fail at the horizon, whereas the OS spacetime continues across it in comoving coordinates. Together with the unjustified value of t and the incorrect table of late time coordinate limits do not represent the present interior of Sagittarius A* and provide no evidence for a horizonless space blister.

I am not sure but the first sentence seems to indicate that you disagree with the statement that "This is consistent with a gravity well diagram showing an almost vertical slope at the Schwarzschild radius, then tapering off, and rounded at the bottom. This picture, far from that of a bottomless hole, looks more like a swollen blister, filled to the brim with stalled, degenerate matter." I am not sure why you disagree with this statement. For a given time coordinate t the gravity well diagram looks something like this:



Except that the slope is almost vertical around the Schwarzschild radius (the slope is related to $e^{\lambda/2}$). Why do you think this picture is incorrect?

No, I am not saying that such a diagram is incorrect, it is a correct gravity well diagram for any specific Schwarzschild time slice while the star is collapsing. In this diagram the radial proper distance elements become larger near the Schwarzschild radius. The problem is about interpreting them as a region filled with stalled degenerate matter which is not implied by the article's calculations. The shape of the diagram depends on the chosen spatial slicing and coordinate representation. As I have also explained in my review, the OS model describes the matter as continuing to collapse in comoving coordinates.

Ok, but you wrote "divergence near the horizon (…) does not establish a blister shaped geometry" so you were talking about geometry, not the material content of the star. In the end I understand that you agree that the shape of a bister is ok. As far as the "stale degenerate matter goes" – I am picturing state of matter closely resembling this of the interior of a neutron star, but this is speculative and a topic for future research.

I don't understand the reminder of the paragraph either. Ok, so you may have reservations about the chosen value of t. But would the results be qualitatively different if we picked any other reasonable value? I think not. Or do you disagree? So, if we eliminate this reason

(invalid t), what are the remaining reasons why this exercise with Sgr A* is incorrect? I.e., why the content of the table cannot very crudely describe the interior of Sgr A*?

> The whole exercise is built on interpreting late time Schwarzschild coordinate behavior as the actual physical interior state of the collapsing star. Even with the correct numbers, that interpretation does not follow from the OS model. The calculations establish neither the absence of an event horizon nor the absence of a central singularity.
>
> **This is precisely what O&S do in their paper in its last two paragraphs. They interpret late time Schwarzschild coordinate behavior as the actual physical interior state of the collapsing star. Are they wrong? And, no: these calculations do establish their absence within the fragment of spacetime described by the model. Unless you think their model is incorrect.**

2.11 Coordinate transformations

As previously mentioned, the OS model describes matter crossing a causal boundary and reaching a central singularity in finite comoving proper time. The scientific literature correctly regards it as a model of black hole formation. The author is claiming otherwise which is just a repetition of author's previous claims which I have addressed already. The author then tries to posit that it is inappropriate to use coordinate transformations to extend the spacetime.

I am yet to see any calculation made by or any quote from the OS paper that has anything to do with reaching central singularity. Note that the "$\tau = -G/F$" argument was not made in the OS paper, so this is not their line of reasoning. They did not make this calculation.

> The OS paper does not explicitly state the finite proper time required to reach the central singularity in modern terms. However, this does not mean that the mathematical consequences of equations (21), (22), and (27), discussed in section 2.3 above, lie outside the physical content or scope of the OS solution. Those equations imply that the areal radius reaches zero and the density diverges at a finite comoving proper time, which we today identify as the central singularity of the collapse solution. Numerous later reviews and textbooks therefore correctly interpret the OS model as describing classical black hole formation.
>
> **The mathematical consequences of these equations lie outside of the scope of the OS solution, because they require an additional assumption, namely that the spacetime can be extended. The numerous later texts are incorrect about that.**

In standard convention, a coordinate chart maps a region of spacetime events into a subset of R^4 (coordinate values). The coordinate map C defined by the author instead takes coordinate values to map them to spacetime events and is therefore an inverse coordinate map in standard notation. This is not an error but different convention. The set A is then the

domain containing all coordinate values for which C represents events in M. The author then states 4 possibilities:

1. First is the case where the complete domain (all the coordinate values) maps onto the complete codomain (entire spacetime M).

2. Second is the case where only part of the coordinate domain is needed, but it still represents the entire spacetime. Although the author's example of polar coordinates contains a coordinate singularity at the center (angular coordinates are undefined there) but it is minor issue and does not affect the argument of author significantly.

3. Third is the case where the coordinates cover only part of the spacetime. The author correctly identified here that Schwarzschild coordinates are example of this case.

4. The fourth is the most interesting case where the author talks about extending the domain to allow for a building an extended coordinate chart that gives access to additional spacetime events. Region III and IV of the KS diagrams is a valid example but later in the text, the author tries to warn that because these regions are physically doubtful hence the region II should be doubted as well.

They justify this by claiming that the because the OS model paper used Schwarzschild based coordinates which cover a region of spacetime the author called as M_{OS} therefore the coordinates cover the entire OS spacetime and to the author the events covered by comoving coordinates are beyond the OS spacetime and therefore artificial and unphysical. This is an incorrect interpretation of the coordinate transformations. The OS model used the comoving coordinates to solve the collapse precisely because the first coordinates become inadequate. The events after horizon crossing follow from that solution and its initial data.

Yes, I advocate here for providing justifications from spacetime extensions as opposed to just automatically going through with them. And no, I do not justify it with anything in the OS model. I justify it using the many examples of unphysical extensions already deemed as such by the scientific community.

The OS model used comoving coordinates because they are easier to use not because they allow the authors to evaluate what happens after horizon crossing. The authors are pretty explicit about it. The incorrect application of coordinate transformation is to take a well-defined spacetime manifold – like the one in the OS paper in equations (2)-(5) – and to implicitly change this manifold through coordinates that are "well-behaved" outside of it.

3 Comments on Previous Reviews

The author repeatedly accuses the previous reviewers of misunderstanding, careless reading, or fabricating invalid arguments, while avoiding the substantive mathematical objection most of the time. However, the author correctly identified some genuine weaknesses in all these reports despite themselves making weak or incorrect defenses in response to them.

1. I found the Gemini report to be weak despite agreeing with its overall assessment. In section A, it points out that the author is rejecting all the stationary solutions of the field equations. The author responds by writing that the standard stationary black hole solutions are mathematically correct but are not physically appropriate (consistent with observations) for real MCOs because the interior matter distribution continually evolves. They reject Gemini's verification by pointing that they are concerned about the interiors not the exteriors. Their objection is reasonable but their defense is wrong since the stationary black hole solutions are vacuum solutions and do not assume any stationary interior matter distribution. Moreover, observations mainly constrain the exterior gravitational field. A dynamical interior does not prevent the exterior from being stationary or approximately stationary. Therefore the author has not shown that observational evidence conflicts with Schwarzschild or Kerr modeling.

This was not the Gemini's point. Gemini wrote that my criticism of stationary metrics is incorrect because stationary metric can be used even in a dynamic setting to model exterior of the star. However, it is irrelevant, because I criticize stationary metrics when they are used to describe the interior, not the exterior. I agree that exterior may be stationary also in a dynamic setting. And, well, you are wrong. Stationary black hole solutions have a very specific assumption about the interior matter distribution – they are called "vacuum" solutions because their interior is also empty (except for the singularity).

In GPT's version, section 1 and 2 tackle the same problems, although that review is much weaker but the responses made by author are similar to the ones made in Gemini's report.

2. In the section B, I mostly agree with Gemini's verification as it correctly points out the errors but the first line stating that the author conflates proper time and coordinate time is partially incorrect and the last line stating that ending time violates the equivalence principle is misleading because at the central singularity the equivalence principle does not even apply as it is not a regular spacetime event. The author's response to the verification starts reasonably that they are not conflating coordinate time with proper time but actually they are using the asymptotic behaviour of Schwarzschild time to justify terminating a worldline after finite proper time.

The author then rejects that the OS model has a central singularity. While the OS paper is not using the modern terminology, their calculations in fact show the formation of an event horizon and a central singularity. The author should see equation (21) which shows that at a finite proper time $-G/F$ the areal radius becomes zero and equation (22) which shows that the density diverges there. Later the text after equation (37) clearly defines what we today call an event horizon. The OS model also earlier mentions the problems with Schwarzschild like coordinates and then switches to comoving coordinates to get over the limitations of the coordinate system to reveal the actual geometric properties of the collapse (see how the authors of the OS model describe this in section I and then solve it in section II). Finally, while the Gemini's equivalence principle invocation is unnecessary, the author's defense on the

grounds of their example spacetime is incorrect as that example does not have anything physical that limits time, the author merely constructed it by preventing its regular continuation.

OS calculations do not show the formation of an event horizon and a central singularity – for that additional assumptions about extendibility of their spacetime are needed. OS do not define an event horizon but describe a limiting behavior of a phenomena for which the limit may be never reached. They switch to comoving coordinates to facilitate calculations, not to extend their spacetime with event horizons and singularities. And yes, the spacetime example I provided is extendible but so are many other spacetimes whose extensions are considered unphysical. There is nothing inherently wrong with this example.

The sections 3 and 5 of GPT's report raises similar questions and the author has responded essentially the same as they did for Gemini's report.

3. In section C, I agree with Gemini to point out the author's clear dismissal of coordinate transformations. Gemini argues that Eddington-Finkelstein and Kruskal-Szekeres coordinates demonstrate the regularity of the horizon and permit the spacetime geometry to be continued across it. In response, the author redirects the discussion toward regions III and IV (in Kruskal-Szekeres diagram) of the maximally extended eternal Schwarzschild solution. However, these regions are not expected to occur in an ordinary stellar collapse spacetime. Region II arises from the regular future evolution of the collapsing matter. No empirical evidence for regions III and IV is therefore required to establish the formation of region II. Moreover, a coordinate transformation neither creates new spacetime regions nor determines which regions are physically realized.

No, coordinate transformations clearly "create" new spacetimes by extending what previously was considered to be the spacetime manifold. This was the very purpose of introducing EF and KS coordinates – to show that the spacetime does not "end" at the event horizon, but that it is larger and includes more points that was previously thought.

Similar but much weaker arguments were made by GPT in section 4 of its report, and the author has reasonably pointed it out. Note that pointing out weakness of the review does not validate the original article's conclusion.

4. In section D, I do not like how Gemini has overstated the facts and made a too-strong and hence incorrect conclusion. While the observations of the black holes M87* and Sag A* are highly consistent with the Kerr solution, they do not provide any direct evidence of an event horizon. Same goes for the GW observations, they are overwhelmingly in support of black holes but does not rule out objects with physical surfaces. But the author should not treat this as a defense of their response, as even if any alternative model of MCOs exist, it does not necessarily rule out existing models and the author showed no quantitative comparison showing that its model satisfies the observations any better.

Similar can be said for the section 7 of ChatGPT's report although I find the comments made my GPT partly irrelevant, even weaker in term of argument and too strong in terms of conclusion. But even there, the author has followed similar rhetoric of assuming the lack of direct evidence of event horizon to be the evidence for their nonexistence.

This is a misrepresentation. I never treat lack of evidence for existence as evidence of non-existence. This is a logical fallacy and neither my article nor my responses to criticism contain such reasoning.

5. In the section 6, GPT reasonably asks the author to substantiate the claim that abandoning black holes resolves several major theoretical problems. While it is fair for the author to say that a single article need not develop the complete theory, nonetheless the overall paper anyway failed to construct a consistent horizonless and nonsingular spacetime following the field equations.

I use the OS spacetime as an example with an assumption that it cannot be extended. I am not sure, but I guess you treat this usage as invalid because it must be extended? If so, it would be easier if you explicitly argued somewhere why you think that spacetime cannot be extendible.

6. While I also agree with the core conclusion of ChatGPT's report, I find the section on minor comments being unnecessary because it does not actually develop any significant criticism of the article.

7. In contrast, the anonymous review is not a formal report but a long and unorganized casual commentary. It is still incomplete in my opinion, and openly limited by the reviewer's available time. Although it contains several important and strongly put comments that align with my own review. One major highlight that adds to their credibility is that they took back the comments on criticism concerning Mitra and Singh when the author correctly points out that the paper is employed only to minor corrections for the missing factors in equations (38) and (39).

4 Recommendation

In view of the fundamental conceptual, mathematical, and interpretive problems identified above, I recommend rejection of the article. The conclusions of the article are based on misinterpretation of the Oppenheimer-Snyder solution, an unjustified truncation of spacetime at a regular boundary, and incorrect calculations concerning Sagittarius A*. These problems cannot be resolved through minor revisions. A substantial rework is necessary to productively investigate horizonless alternatives, but it should provide a complete spacetime model, demonstrate consistency with Einstein's equations and compare its predictions quantitatively with current observations.