This rebuttal was provided in the form of private correspondence with an established astrophysicist, whose works have been cited around 30,000 times (by the time of this communication, according to Google Scholar). Some fragments are redacted with (…) to keep the identity of the referee secret.

Author's responses are in red.

(…) I didn't write a review in the style of a scientific journal because this isn't a paper for a scientific journal. Instead, I'll just offer some casual comments (…)

Oppenheimer-Snyder describes the creation of an event horizon and singularity in finite time for an observer co-moving with collapsing matter. The author seems to disagree with this statement, and I couldn't quickly figure out why, but it has something to do with the rejection of coordinate systems penetrating the event horizon. Of course, one can and should ponder the physicality of various theoretical objects, but at least mathematically we have a well-defined problem. We have Einstein's field equations (which the author does not deny), and we are examining how, from defined initial conditions, the differentiable manifold described by the equations, coupled to the energy-momentum tensor field, evolves. This isn't high school mathematics, but then, of the many difficult things in the world, this isn't the most difficult (especially in the most simplified OS model). To solve this problem, we'll use a coordinate system that is non-singular on this manifold (for example, longitude and latitude are good coordinate systems on a differentiable manifold modeling the Earth's surface, but at the North Pole it causes problems because we're in every longitude at once. Very imprecisely, but perhaps figuratively, at the event horizon we have every time coordinate collapsed to a single point in a coordinate system convenient far from the black hole. But nothing happens at the North Pole or at the horizon to suggest that instead of getting offended by geometry, we couldn't simply use a better-chosen coordinate system to describe this place. There's nothing "dodgy" about this at all. The author writes that you can't just swap coordinate systems (at least mathematically speaking, yes, you can) and suggests, " Perhaps, one day the consensus will form that not only regions III and IV of the maximally extended Schwarzschild spacetime are unphysical, but also region II." However, in the collapse, regions III and IV don't arise at all (due to completely regular initial conditions—a dust cloud collapsing under the influence of its own gravitational mass), while region II (the interior of a black hole) certainly does. These regions III and IV of the maximally analytical Schwarzschild extension are mathematically completely correct and are revealed by a good choice of coordinate system. They are physically rejected not arbitrarily, but precisely because they don't arise from physically motivated regular initial conditions (and the black hole, region II, does).

Mathematically, the author relies on Mitra & Singh's 2013 paper, which was published in a mid-tier journal, doesn't have a free preprint available on arxiv, and has only five citations, so it's unlikely to demonstrate a real error in the fundamental problem of general relativity: the OS collapse. Many people have recalculated the OS model between 1939 and 2026, including myself and probably every student in a general relativity course. This model has various physical problems (it's as simple as possible, spherically symmetric, and what kind of matter is it that, no matter how hard you compress, it doesn't resist pressure? It turns out that isotropic pressure isn't an issue, and symmetry is ok, but that's a completely different topic), but it's mathematically correct. I understand that such an argument, rejecting a paper because of a weak journal and low citation count, might not be convincing to everyone, but a paper pointing out a fundamental flaw in OS would be a huge sensation and would be very well cited (unless we truly believe in the existence of a black hole lobby that censors scientists because " too much emotion, money, and effort have been invested in this research area to let it go easily," but such talk is pure scum).

Regarding the ontological status of black holes, they are indeed currently the default interpretation for certain classes of astrophysical objects. But it's not as if the black hole lobby imposed a narrative after OS's 1939 paper. It took several decades of theoretical and observational research (and a bit of slashing with Occam's razor) to solidify this paradigm. It's not inviolable at all; serious papers are published daily addressing the challenges of this model (the information paradox is probably the most famous example). Many people, myself included, are working on defining increasingly better observational tests of the nature of objects we interpret as black holes, with a glimmer of hope of finding something that would violate the paradigm, bringing the authors eternal fame and glory, and perhaps even some money. The observational evidence for the existence of black holes is currently quite strong (although the "strength" of the evidence can be subjective), in my opinion much stronger than it was ten years ago (LIGO, EHT, GRAVITY...), but it's not conclusive because it's very difficult to define a conclusive test of the nature of an object that a) we don't have in the laboratory but observe its interactions with its environment from very far away, b) by definition, the event horizon doesn't emit detectable material or electromagnetic signals on its own. I suspect that indirect evidence will become increasingly stronger in the near future, but at this point I can't imagine any evidence within the reach of science that would be absolutely convincing. Perhaps one day we'll build a black hole in a laboratory, or observe one up close somewhere in space, but that's definitely science fiction, not science right now...

In short, maybe black holes exist, maybe they don't, but this paper adds nothing to the discussion.

In the first paragraph, the referee repeats issues raised by the AI. I have already addressed them both in the article and in my responses to AI. The referee writes in a way as if (s)he did not read my arguments.

The point about the Mitra & Singh paper is completely wrong. I just used it for a slightly different version of the equation because the original supposedly has a typo. If I used the original equation from the OS paper, the qualitative results would be the same.

I fully agree with the content of the third paragraph. But these are just some general remarks that do not refute anything I wrote in the article.

The referee disregards the elephant in the room: my interpretation of Sgr A* as an example of a space blister. This is the key question. Why can't there be an object characterized in a way I describe it? And, in general, what is wrong with the idea of space blisters? Even if the points made by the referee were correct (which they aren't), they wouldn't do anything to refute my main idea.

There's no point in delving too deeply into this; I now see that this is precisely the old "frozen star" conceptual problem. I've explained why the reasoning is flawed; the author arbitrarily rejects conclusions based on a coordinate system that isn't pathological (commoving/penetrating/free-falling coordinates) because it doesn't agree with the physical interpretation of these results. The analogy between regions III and IV and region II is flawed, which I also tried to explain, and the author's answers unfortunately don't make much sense. What I didn't write earlier (because I didn't think about it) is that the given metric is misinterpreted by the author, it is an approximate metric which for a fixed R (the "number" of the collapsing sphere) tells about the geometry for a late time t, but this time t is, analogously to Schwarzschild, a pathological coordinate on the horizon, so the limit $t \to \infty$ corresponds to the moment in which the collapsing sphere R passes the growing event horizon (this is better seen in the co-moving coordinates, the ones the author arbitrarily rejects). So the behavior for $R = R_b$ (the last layer, the surface of the collapsing star) should asymptotically correspond to the horizon of a static black hole in the Schwarzschild coordinates with t of a distant observer (that's right, in the table the author shows $|g_{tt}| = e^{\nu} \to 0$ and $|g_{rr}| = e^{\lambda} \to \infty$), while what happens at $R = 0$ is less obvious, because in this case the time $t \to \infty$ corresponds to the first appearance of the horizon in the center of the object (the horizon then grows to the size r_0 = Schwarzschild radius). But $|g_{tt}| = e^{\nu} \to 0$ makes sense (just like the horizon in the case of $R = R_b$), and above that $|g_{rr}| = 1$ might be a good idea, but it certainly doesn't imply flatness of spacetime at $R = r = 0$. Curvature is a scalar (invariant to transformations of the coordinate system), so showing the constancy of a

component of the metric tensor in a given coordinate system says nothing about the curvature. Crucially, for $R = 0$, the time coordinate t doesn't allow us to investigate a true singularity, because it has a coordinate singularity when the horizon is formed, which happens before the singularity arises.

What I describe is indeed similar to the concept of a "frozen star." However, in the early 1970s, this term ended up being synonymous with black holes (see, e.g., "Introducing the black hole" by Ruffini and Wheeler, 1971, or Stephen Hawking's "Black Holes: The Reith Lectures"). Unlike in those late "frozen stars," in the model I propose time ends, and there are no horizon and no singularity. Finally, the term "frozen star" was recently recycled to denote various new hypothetical objects: either superdense objects made of string fluid or low-mass stars with very low temperatures made of iron. For these reasons, I decided not to use this term.

About the coordinate systems – more important thing, one that I elaborate on in the chapter "A note on coordinate transformations," is the actual physical spacetime manifold we are considering. The issue of coordinate systems is secondary. In the model of a space blister, the manifold is different from what current theories imply. The rejection of certain patches of the extended manifold as unphysical is not "arbitrary" but supported by paradoxes related to black holes and other arguments provided in the article.

I do not reject comoving coordinates. I fully agree with how Oppenheimer and Snyder used them in their paper. However, I do not agree with using them to justify extending the spacetime manifold to unphysical regions.

Why exactly does the referee think that I misinterpret the OS metric? It doesn't follow from what s/he writes. Why does the referee think that what happens at $R = 0$ is "less clear"? To the contrary, it is perfectly clear. The equations clearly show what happens. Oppenheimer and Snyder also comment on what happens for $R < R_b$ in their paper, so it seems that it was clear to them too. Here, instead of looking at what the equations actually imply, the referee starts to talk about horizons and singularities while nothing like that emerges from the OS' equations.

And obviously, space is <u>locally flat</u> there – as opposed to singular – because even curved spacetime is almost everywhere locally flat. It has nothing to do with specific values of the components of the metric tensor. The referee probably did not read it carefully and glossed over the word "locally."

- *In the first paragraph, the referee repeats issues raised by the AI. I have already addressed them both in the article and in my responses to AI. The referee writes in a way as if (s)he did not read my arguments.*

Indeed I did not. I did it now but I find nothing there to address my criticism, and some other answers are trivially wrong, like Gemini 2B points 2 and 3.

To address point 2, OS does show formation of singularity in a finite comoving time. I think the author is confused by the OS language which is quite obsolete today. Remember it was 1939, long before Penrose and more conceptual developments in the theory. OS singularity is essentially a corollary of the fact that all mass is being squeezed into zero volume after finite comoving time. Some modern GR textbooks like MTW that I'm linking to later could contain much more pedagogically mature discussion of the OS collapse than the paper itself.

"OS does show formation of singularity in a finite comoving time" – please provide a quote, an equation, or anything else from their paper that can reasonably indicate the existence of a singularity. Please show me where in their paper "all mass is being squeezed into zero volume after finite comoving time." There is nothing like that in their paper.

I made similar argument in my responses to ChatGPT. Referee claims to have read them but his/her arguments indicate otherwise. To indicate the existence of a singularity or an event horizon, one has to extend the OS model. I make this point in the "A not on coordinate transformations" section, but the referee seems to have skipped it.

To address point 3, the equivalence principle does not allow an inertial observer to experience anything "special" in a regular point of spacetime. Mind you that singularity is not a regular point of spacetime, it's the edge of it so "special" things can happen there. Time ending for an inertial observer in a flat spacetime is violating the equivalence principle. I think that the author does refute the equivalence principle in "What mistakes have been made?" section, or perhaps uses some different definition of that principle.

Although the referee claims (s)he read my answers, it doesn't seem that (s)he did. As I said in my response to AI: "I provided an example of a flat spacetime in which time ends and the equivalence principle is not violated". If you had a good argument, you would explain why the example I provided was incorrect.

I don't have time to go through other items point by point but there are more misunderstandings there, like confusion about what the coordinate invariance means.

Let me reiterate my main criticism though. The author compares fully extended Schwarzschild regions III and IV (that are mathematically perfectly fine but do not appear in the collapse problem for the initial conditions reason) and region II (which forms in the collapse problem). The comparison is not justified and non-existence of region II doesn't follow in any way from the fact that III and IV are not physically realized in a collapse scenario. The whole misunderstanding comes from arbitrary rejection of a comoving / horizon-penetrating / free-falling coordinate system as a tool to study spacetime geometry. There is nothing wrong a priori with such a system of coordinates.

I agree that "non-existence of region II doesn't follow in any way from the fact that III and IV are not physically realized in a collapse scenario." I never made the alleged argument. I also agree that "There is nothing wrong a priori with such a system of coordinates." It seems that the main criticism comes from the misunderstanding of the article. That comparison was made to illustrate that extending spacetime with coordinate transformations does not always lead to good physical results. And the main point of the article would still stand even if I removed the entire chapter on coordinate transformations. So, focusing main criticism here makes little sense.

By the way, I added the chapter about coordinate transformations because, after some initial feedback, I saw people confused by my claims that there is no horizon or singularity in the OS paper. Initially I thought it was obvious – just read the paper to see. But it turns out a sort-of mythology was built around that paper which ascribes to its authors things they did not do. Surprisingly many people believe it uncritically. But no wonder – both Penrose and Hawking repeat these myths throughout their books. This is what a "scientific discourse" boils down to here: a struggle against myths.

- *The point about the Mitra & Singh paper is completely wrong. I just used it for a slightly different version of the equation because the original supposedly has a typo. If I used the original equation from the OS paper, the qualitative results would be the same.*

I admit my point about the M&S paper was wrong. I assumed that there must be some new formal calculation-based argument in the attached text that I just didn't get to (I don't even seem to have access to this M&S paper, and I assumed that perhaps the argument is hidden there). Now I see that there is actually no new argument and the author is confused about the good old "frozen star" interpretation. I think the best resource to look into in order to gain some understanding is the intro to chapter 33 of the MTW textbook (but also the whole stellar collapse section), for example here: [LINK to the book]

And this should give the referee a pause: maybe if (s)he was wrong here, his/her method of selective reading and jumping too quickly to the conclusions is suboptimal, and (s)he is also wrong in other places? Unfortunately, nothing like that happened.

Ad rem. I like this book and would also recommend it ("Gravitation" by Misner, Thorne, and Wheeler – the book is in the references of my article, and – surprise – I read the books and papers I use as my references). Their conclusions about the existence of black holes are different from mine. Still, the methods I use to arrive at my hypothesis seem to agree with the principles of general relativity described in that book. The intro to chapter 33 is correct until the point where they start talking about "passing through the horizon" on page 874. I think this cannot happen because the horizon does not belong to the actual

physical spacetime manifold. It belongs to its "made-up," unphysical extension obtained by unscrupulous coordinate transformations.

I don't know about the typo in OS, but regardless, this metric describes what happens to the collapsing layer R at large time of the distant observer t/r_0, which means when the layer R approaches the growing event horizon and t coordinate explodes. For $R = R_b$ you recover the behavior of the Schwarzschild horizon ($|g_{tt}| = e^{\nu} \to 0$ and $|g_{rr}| = e^{\lambda} \to \infty$). For $R = r = 0$ it's tricky because its $t \to \infty$ limit describes the first appearance of the event horizon, $|g_{tt}| = e^{\nu} \to 0$ makes sense then $|g_{rr}| = e^{\lambda} \to 1$ is less clear. But this is again a limitation of the coordinate system that doesn't let you study the event horizon (or singularity). The $R = 0$ analysis can't find the curvature singularity because time $t \to \infty$ when the horizon first forms in the center of the collapsing star, before the formation of the singularity. It's not a coordinate system that would allow us to study $r = 0$ following the formation of an event horizon. No conclusions about the interior structure can be made that way.

Again, for $R = r = 0$ nothing is tricky here. Just plug the values into the equation. It seems tricky because you are trying to find a horizon or a singularity there, while none exists.

That $e^{\lambda} = 1$ at the center of the star throughout the entire process of collapse is obviously true. If it's false, then a tiny sphere of radius $\varepsilon > 0$ at the center of the star would not have a surface of $4\pi\varepsilon^2$, which would violate the definition of Schwarzschild coordinates. So even if black holes exist, up until the very creation of the horizon, in Schwarzschild coordinates the metric at the center has $e^{\lambda} = 1$. Not $e^{\lambda} \to 1$, but $e^{\lambda} = 1$, always, by definition. Nothing is unclear here.

"No conclusions about the interior structure can be made that way." – why? These equations are a perfectly valid solution to Einstein's equation, aren't they? I just plug the numbers. Where is the mistake? The referee assumes that there must be a horizon or a singularity there, and because the equations don't show them, (s)he concludes that the analysis must be wrong. This is not a scientific reasoning. I argue in the article that horizons and singularities do not exist. Then you say here that because they exist, there must be something wrong with my analysis without showing what actually is wrong.

In any case, it's also important to notice that none of this coordinate-based reasoning strictly implies flatness of spacetime or "ending of time" in a geodesic incompleteness sense (such statements must be formulated in a covariant way, independently of the coordinate system). g_{tt} going to zero is a coordinate statement.

As a reminder – spacetime is <u>locally flat</u> everywhere except at singularities, as per the equivalence principle. I don't understand what point the referee is trying to make about the flatness of spacetime. I also agree that these calculations do not prove that "time ends." I never claimed they did. Whether time ends or not depends on the status of the "the flow of time must continue" assumption and the extent of the spacetime manifold. I

cannot prove this assumption wrong; I can only argue that things make more sense if we abandon it.

- *I fully agree with the content of the third paragraph. But these are just some general remarks that do not refute anything I wrote in the article.*

OK

- *The referee disregards the elephant in the room: my interpretation of Sgr A* as an example of a space blister. This is the key question. Why can't there be an object characterized in a way I describe it? And, in general, what is wrong with the idea of space blisters? Even if the points made by the referee were correct (which they aren't), they wouldn't do anything to refute my main idea.*

In summary "space blister" seems to be exactly the "frozen star". In practice, this is not such a bad disagreement between us, because (unless I misunderstand) you agree that the external vacuum metric is Schwarzschild, all the falling stuff is super-close to the event horizon (or whatever is there at $r = r_0 = 2M$) after reasonable t/r_0 passes (surface of the collapsing star can be evaluated in time t and it approaches $r_0 = 2M$ with an exponential decay in t/r_0 from the above), so whether we call it a black hole, a "frozen star", or a "space blister", it looks the same for the observer that remains in a distance and is not inclined to jump into this compact object. Furthermore, everywhere above $r_{star} = 2M\left(1 + e^{-\frac{t}{2M}}\right)$ black hole, space blister, and frozen star are all described by a static Schwarzschild metric. So if my quick calculation is correct, if Sgr A* would be forming in an OS collapse, within 3h from the beginning it would be described by static Schwarzschild spacetime everywhere outside of r = 2M + Planck length. Good enough for all our observations, no empirical distinction between BH, FS, SB.

No, all the falling stuff is not close to the $r = r_0 = 2M$ sphere. Instead, the matter is distributed within this sphere. In realistic scenarios, the proper density is likely to be larger at the center, where $r = 0$.

But thank you for explaining why there is no empirical distinction between BH and SB. The AIs in their rebuttals were very insistent that there is a distinction which favors BH over SB.

The disagreement is about the content of the interior, which can be explored by a free-falling observer (empirically) but not by a distant observer (time coordinate divergence). The mathematics of GR shows what a free-falling observer would experience (by using comoving

coordinates and charting the encountered region II geometry). You reject these results. But why exactly? What happens to the free-falling observer following layer R of the collapsing star in your interpretation? Does such an observer forever fall onto $r_0 = 2M$ in their comoving frame? How about the one that was initially at $R = r = 0$? The math of GR shows these observers cross the growing event horizon and later reach the singularity where their trajectories terminate. Perhaps you agree with math but you say this is not really realized in a physical reality below $r_0 = 2M$? If so, this seems to be more of a philosophical-ontological point and all I can do then is to agree to disagree (my point is not empirical either, naturally, it is an expression of trust in predictions about physical reality derived from mathematical formulations of physical theories when smoothly extended from the regime where they have been empirically tested).

I do not reject the mathematics of general relativity. My proposal is perfectly compatible with the basics of general relativity: general covariance, Einstein's equations, etc. The difference is in the extent of the spacetime manifold. And the decision to extend the manifold is not automatically the result of "the mathematics of GR" – instead, it is an arbitrary decision of physicists. Finally, when comparing the two theories – space blisters and black holes – the former seems much less convoluted, as I argue throughout the paper. This is why I reject the results that insist on extending the spacetime manifold to include horizons and singularities.

What happens to the infalling observers is described in the "What exists instead?" chapter. The observer does not fall forever in his comoving frame – to fall, it takes exactly the amount of time I call "the remaining time." These questions are testament to how carelessly the referee read the article. ☹

"The math of GR shows these observers cross the growing event horizon" – no, it does not. The math of GR shows what happens on the manifold and if you come up with an unphysical manifold that has no correspondence to reality, the math of GR will helpfully oblige and show you what could happen if such a manifold existed.

Regardless, it's important to point out that the presented interpretation of the late time asymptotic metric parametrized with a distant observer's time t, $g_{mn}(t, R)$ is incorrect, as commented on above. In particular, it doesn't imply spacetime flatness or "time-stopping" in the center (because $g_{tt}(t \to \infty, R = 0)$ goes to zero and corresponds to first horizon formation in an early stage of collapse, and doesn't allow to describe the singularity formation). These coordinates are somewhat confusing as R is not r, and t asymptotes are different for different R. But this is again why the comoving system of coordinates is far more insightful and thus preferred.

Yes, it does not imply flatness, but this region is locally flat anyway, as per the equivalence principle. And as I already wrote, I agree that this particular calculation does not imply "time-stopping." So, again, why is the interpretation incorrect?

I highly recommend the GR textbook on this topic. :) These are definitely good and not trivial questions, but I still feel like the author didn't spend enough time reflecting on, for example, MTW and the foundations of relativity before formulating his thoughts in the attached article.

I did. It is the referee who did not spend enough time getting familiar with the presented ideas before attempting to criticize them.

I also dislike statements like those in this article, where a simple mathematical operation of changing coordinates is suddenly "suspicious," and scientists are too emotionally and financially invested in their paradigms to see the author's brilliant arguments. In general, such "everyone is dumb but me" rhetoric (though unfortunately also popular in academia) usually, in my experience, doesn't betray a genuine interest in understanding the problem, and the responses to AI reviews also don't reveal any deeper reflection. Therefore, I don't have much hope that the author and I will reach an understanding, and I probably won't continue the discussion in subsequent messages due to limited time resources.

I see the referee didn’t like the part about begin emotionally and financially invested and thus having hard time to abandon one’s views. But isn’t it an obvious and benign observation of anyone who lived among humans for at least a while? Look what Hawking writes in “Black holes and baby universes” (in “My Position”): “In practice, people are very reluctant to give up a theory in which they have invested a lot of time and effort.”