Dear colleagues,

We are thankful to you and to the Referee for the careful examination of our work

“Study of neutrino spin oscillations in a gravitational field with a differential equations method”.

Guided by the comments of the Referee, we have prepared the revised version of the manuscript where the following modification have been made:

1. The text below Eq.(8) on page 4 has been modified.
2. The sentence “We fix the mass of SMBH at M = 10^8M⊙" is removed.
3. We have modified the value of angle form θi = 90◦ to θi = 89.427◦ on page 6.
4. In the caption of Figure 1., \theta has been changed to \theta_i on page 7.

To expedite further refereeing we have marked major changes with the bold font.

We hope that our work can be accepted for the publication in Physics of Elementary Particles and Atomic Nuclei in this revised form.

Below, we provide the reply to the comments of the Referee.

Yours sincerely,
Mridupawan Deka and Maxim Dvornikov

We are thankful to the Referee for her/his comments.

Referee: The authors presented a talk "Neutrino spin oscillations in the vicinity of a black hole" at the Baldin seminar. However, they submitted their paper "Study of neutrino spin oscillations in a gravitational field with a differential equations method" for their contribution for the Baldin seminar proceedings. This (article) title is not so attractive as the title of the talk since the physical effect (neutrino spin oscillations) should not depend on a selected method which was used for their analysis. The title change must be explained.
Author: We agree with the referee that we submitted an article different from our talk. However, these two are not completely different topic. The submitted article deals with a numerical method which is different in approach than the talk. Of course, the physical effect can not depend on a selected method. However given the complexity of the problem at hand, one should demonstrate it numerically. That is what has been shown in the article, for the case only when the gravitational interaction is present.

We believe that it is our prerogative what to submit for the proceeding.

Referee: In their letter attached to the revised paper submitted on January 5 the authors wrote that they revised a paper with the title "Impact of antiparticle degrees of freedom on neutrino flavor oscillations in frames of quantum field theory".
Author: It was purely an innocent mistake from our side. In our previous reply of Jan 28^{th,} 2026, we corrected this mistake.

Referee: In my previous reviews I recommended the authors to deal with equations without singularities for calculations of neutrino geodesics as I recommended and as many other authors did in the past.
Author: We submitted our reply on January 5 that we study the particle spin evolution in addition to the test particle motion. We look for both trajectory and spin evolution in one single technique based on differential equations. As a result, it is not possible to consider the method suggested by the referee in our work.

Referee: The authors wrote: "Reply of the authors: Our limits of integration is always between x_min = (x_{tp} + epsilon) and x_max = 100, where we set epsilon = 10^{-10}. We tried different values of epsilon, ranging from 10^{-7} to 10^{-15}, and we hardly see any difference in our results for epsilon < 10^{-10}. In our opinion, our approach to avoid the singularity is reasonable". Concerning the issue I would like to say that it is known that improper integrals with singularities in the denominator can be both convergent and divergent. A consideration of specific cases is not a proof that the approach is applicable in general. Indeed, in this case, the program continues computing, rather than stopping the program. In fact, for convergent integrals, the procedure described by the authors may be acceptable, while for divergent integrals it is obviously inapplicable. The critical neutrino trajectories corresponding to multiple roots of the polynomial correspond to cases of logarithmically divergent integrals in the equation.
Author: We agree with the referee that our approach is not valid in case the integral is divergent. In this regard, we would like to draw attention to the Ref. 24 in our article, Gralla et. Al, 2018. In the appendix C of this Ref., it is shown that the integral is convergent at the leading order in the expansion of epsilon (Eq. (C12b) in Appendix C).

Referee: The authors insist that they have created a software package to calculate the decrease in the neutrino flux in the vicinity of a supermassive black hole. In their calculations, they use one of the most powerful computer complexes in Russia to calculate the set of neutrino trajectories captured by a black hole, and then discard this data.

In their reply the authors wrote "Just after the above lines, we now add a few lines (in bold font) that after the formation of the beam of incoming neutrinos with these y and w values, we discard the particles falling to the shadow area"

Thus, the authors carry out calculations for trajectories, which are subsequently not used. In the future, they plan to extend this practice to larger samples and possibly more powerful computers. This practice of using resources to calculate unnecessary data is not only far from optimal, but also simply does not make sense. It is clear that initially, for those Carter constants of motion for which neutrino capture takes place, geodesic calculations do not need to be performed.
Author: The comment of the referee does not make any sense. Nowhere in the text we claim that we have performed computation for the particles which fall into the shadow area and then we discard them.

Our numerical computation involves only the scattered neutrinos. We define a general distribution of neutrino beam at infinity, and we then make sure that only the scattered neutrinos from this distribution go into the computation (meaning travel towards the BH). We have described in the text how we numerically impose conditions to distinguish between the scattered and captured neutrinos. The captured neutrinos are discarded before the computations of trajectory and spin precession start. Hence the question of discarding some computed data do not arise at all. Anyone with basic experience of computation would do the same.

Referee: The authors wrote "Suppose that SN, the Earth, and the SMBH in the galactic center, which neutrinos gravitationally interact with, are almost along the same line." In my previous review, I wrote that the authors' assumption that considering a supernova explosion behind a black hole in the Galactic Center has a very low probability, and the authors responded to this remark by unsubscribing, which is unlikely does not mean impossible, and included this assumption in the corrected text of the article. Really, the authors wrote in their reply: "In addition, we note that, despite 'we do not observe supernova explosions in our Galaxy for centuries' it does not mean that such an event will not happen in the nearest future." The statement of the authors of the article that if an event has not occurred, but may occur, without assessing the probability corresponding to this event is unconvincing since the authors suppose a SN explosion in a very tiny region of our Galaxy.
Since the authors strongly want to include this assumption (about SN explosion at the Galactic Center) in the text of the article, they should either note that this is a thought experiment or actually evaluate the likelihood of such an event occurring and how this neutrino deficit could be detected if there are many other sources of neutrinos in addition to the supernova and the supermassive black hole in the Galactic Center. According to my estimates, a supernova explosion in the vicinity of the Galactic Center occurs once every 10^26 years, while the age of the universe is 10^10 years.
Author: We do not think that there is either a sanction or an embargo which dictates that one should study an event only after it happens.

On the other hand. we believe that the our study perfectly fits the motivation as well as meets the standard of theoretical physics. This study can be extended to study other astrophysical objects, such as pulsars, neutron stars, etc, albeit with the relevant changes.

Referee: The authors wrote "Referee statement: "How could we identify neutrinos from Supernova explosions near the Galactic Center from neutrinos emitted from other sources." Reply of the authors: In the first revision of the manuscript, we have admitted that 'we are developing the numerical methods in the current stage of our research'. The question raised by the Referee cannot be discussed in details within the scope of one work. When the technical part of our research is completed, we plan to turn to the description of realistic astrophysical neutrinos."
It seems it is not suitable that the answer of the authors of the physical article is that "we are developing numerical methods", and only tomorrow we will think about how these methods relate to a realistic physical model. I do not find also original numerical methods in the paper which were not used earlier by other authors. If the authors do not know which (astro)physical problem the methods they are developing will be applied to, then are they really developing the methods necessary for realistic problems?
Author: The main goal of the current work is the numerically check consistency of the results obtained from the integral and ODE methods. It has been mentioned in our replies a few times already.

For physics goals, we would like to draw the attention to Refs. [10]-[18].

Referee: Using the authors' terminology, solving the same series of equations by calculating integrals or by solving differential equations can be perceived only as an element of program debugging, and does not represent any physical significant result.
Author: We have explained the motivation of the current work. We do not agree with the referee that it is an “element of program debugging”.

Referee: It is much more interesting to discuss cases when these two methods give different results, as happened in some cases when the green dot do not coincide with the red circle.
Author: Unfortunately, such differences didn’t occur, and we have to live with it. The difference showing the case where the green dot do not coincide with the red circle is negligible, and such cases are numerically possible.

Referee: It would be interesting to see the dependence of the function P_LL, for example, on the radial coordinate for neutrinos falling and leaving the black hole, for an orbit where the difference of this function P_LL from unity is significant. The physical meaning of this case must be discussed.
Author: We are interested only in the gravitationally scattered neutrinos.

Referee: The article does not provide a clear answer to the main question of whether there are neutrino spin oscillations in the gravitational field of rotating black hole or not.
In the seminar talk the authors claimed "No spin oscillation in the presence of only gravity and background matter."The question arises as to whether it is possible to draw such a general conclusion from the simulation results for the selected examples.
It seems that the authors of the presented article themselves do not have a definite opinion on such a physical phenomenon as neutrino spin oscillations in the gravitational field of a black hole.
Author: In the first paragraph of page 8 of our article, we wrote that “no spin oscillations occur in the neutrino scattering under the influence of only the gravitational field.”

Referee: As noted earlier, the authors presented a the Baldin seminar paper entitled «Neutrino spin oscillations in the vicinity of a black hole".It follows from the title of the report that there is such a physical phenomenon as neutrino spin oscillation in vicinity of black hole. However, in page 21 of the Deka's presentation
https://indico.jinr.ru/event/5429/contributions/33127/attachments/23248/41116/Deka.Mridupawan_Baldin_2025.pdf
it was declared that "⇒ No spin oscillation in the presence of only gravity and background matter". In figures presented in the paper for many cases P_LL is not 1 but significantly less than 1, but the authors declared that there is no spin oscillation. The authors must explain this issue.
Author: The figures on slides 22 and 23 of Deka's presentation take into account the effect of the toroidal magnetic field in the accretion disk in addition to those of gravity and background matter. The toridal magnetic field is mentioned in slide 18 of the presentation. We find that toroidal magnetic field has a significant effect of spin oscillations as can be seen in figures on slides 22 and 23. Hence P_LL is significantly less than 1 for many cases.

Referee: In the last version of the paper the authors wrote "We fix the mass of SMBH at M = 10^8M⊙"
The authors wrote in their reply: "We agree with the Referee that the SMBH mass used in the present work does not correspond to that in our Galaxy. However, since we consider the gravitational interaction only, our results do not depend on the absolute value of the BH mass". If the result does not depend on mass why did the authors re-write the phrase from one of their previous paper "We fix the mass of SMBH at M = 10^8M⊙"
In my previous report I mentioned "The authors selected θi = 90◦, but cos θi = 0.01" because it was not clear since for θi = 90◦ we have cos θi = 0, while arccos 0.01 = 89.427◦ but the authors simply ignored my remark as many others.
There is a feeling that the authors are not focused on improving the presentation of the material, taking into account the comments of the reviewer, but on leaving the content of the article unchanged and writing a formal response to the reviewer, more like a bureaucratic reply. I think that is not constructive approach.
Author: We have removed the sentence “We fix the mass of SMBH at M = 10^8M⊙" in the new version. We have modified the value of angle form θi = 90◦ to θi = 89.427◦ in the new version.