Response to Comments on
"NEUTRINO FLAVOR EVOLUTION
NEAR A SUPERNOVA’S CORE"

Jim Pantaleone

Physics Department, Indiana University
Bloomington, IN 47405

Abstract

In a recent paper I concluded that the neutrino background is important for calculating neutrino flavor evolution in supernovae, and including it weakens the connection between r-process nucleosynthesis and cosmologically relevant neutrino masses. Comments on this paper have been posted to this bulletin board; one by J. Cline and the other by Y.Z. Qian and G.M. Fuller. Here I briefly discuss these Comments and demonstrate that they are inaccurate and ill-considered. No corrections to my paper are necessary.

1Permanent address: Department of Chemistry, Physics and Astronomy, University of Alaska, Anchorage, AK 99508
In an interesting Letter [1], Qian et. al. used heavy element nucleosynthesis in supernovae to probe cosmologically significant neutrino masses. Therein they described the evolution of neutrino flavors in a supernovae using standard expressions developed to describe solar neutrinos (see e.g. [2]). However the neutrino background is much larger in supernovae than in the sun. In a recent paper [3] I included the neutrino background perturbatively, and calculated that it was not negligible. It tends to reduce adiabaticity, however reliable conclusions are difficult because for most of the parameter region the evolution is very non-linear. Hence the connection between neutrino masses and conventional nucleosynthesis is weakened. Here I respond to the Comments on my paper which have recently been posted to this bulletin board.

1 Response to Cline’s Comment.

In his Comment, Cline [4] claims that the off-diagonal elements in the potential from forward $\nu - \nu$ scattering should always be dropped in the flavor basis. He argues that flavor states evolve with definite phases, and so the off-diagonal terms (which are phase dependent) will average to zero from summing over many neutrinos.

This argument is fine for massless neutrinos. However for massive neutrinos, it is the mass eigenstates which evolve with definite phases. His argument breaks down then. To find the neutrino mass eigenstates in a neutrino background is a nonlinear problem. Then the flavor off-diagonal elements of the $\nu - \nu$ potential must be included and the summation over neutrinos calculated explicitly. This is what I proposed in Refs. [5], this is what was adopted by subsequent authors in Refs. [6], [7], and even in [8], and it is what is done in my present manuscript.

2 Response to Qian and Fuller’s Comment.

I will attempt to ignore the hostile tone of Qian and Fuller’s (QF) [8] Comment, and objectively discuss the three physical points they raise.

A) First, QF claim that in the $\nu - \nu$ potential the oscillatory cross terms between the zeroth order mass eigenstates will always ”average to zero”. Thus they maintain that I was
"wrong" and "made a conceptual error" when I included such a term in Eqs. (13) and (14), and then when I stated in the conclusions that this oscillatory term could be important. This criticism by QF is not really relevant to my paper since I avoided using the oscillatory term by choosing to do my calculations in the limits where the amplitude of the oscillations vanish, \( P_e = 0 \) or 1. However the issue of oscillations is important for future calculations, so let’s examine it.

My expressions for the density matrices given in Eqs. (13) and (14) are completely correct. The elimination of arbitrary phases from summation over different neutrinos at the same point in phase space has indeed already been carried out. The phase which remains is that part of the interference term between the mass eigenstates which is generated after a neutrino goes through its resonance \([9]\). This phase is the same for all neutrinos at a point in phase space, so it is not affected by summation at this stage of the calculation. QF apparently do not appreciate this point.

When calculating the \( \nu - \nu \) potential at first order in perturbation theory, one must integrate the zeroth order neutrino density matrix over phase space. Then the remaining oscillatory term is certainly greatly reduced in size. However, when partial adiabaticity occurs, the oscillatory term only become irrelevant only when \(< \cos(\alpha + \beta) >\) is less than order \( \sin \theta \)—which ranges as small as \( 10^{-3} \). Hence a very careful, quantitative calculation of the size of the oscillatory term is required to establish when it is indeed negligible. QF give no quantitative estimate to justify neglecting this oscillatory term.

Furthermore, while neglecting all oscillations does lead to gross simplification in the description of flavor evolution, it is not always reasonable. For example, numerical studies in the context of the early universe \([7]\) have found oscillatory solutions which are approximately energy independent. Thus what usually gives the largest reduction in oscillations, averaging over the energy distribution, does not work for very nonlinear situations. This demonstrates that phase effects can not always be neglected.

B) In their second point of criticism, QF are extremely vague.

They derive an expression, their Eq. (3), and give it as the "correct" description of the crossing probability. However this expression is identical to the expressions presented in my paper (in the text at the end of Sect. 2.1, and in the paragraph containing Eq. (23))! It is
thus difficult to determine exactly what they are objecting to.

They do stress a difference in how the expression should be interpreted. They state that "The essential nonlinearity of this problem demands that a self-consistent iteration be performed". They imply that my method, perturbation theory, is "wrong" because I have not "iterated" the solution.

However, it is obvious that my method, perturbation theory, is sufficient for my modest goals of determining when the problem is nonlinear, and how the nonlinearity tends to alter the evolution. My calculations clearly demonstrate that, contrary to the attitude in Ref. [1], the problem is generally very nonlinear. Thus QF’s present insistence on the necessity of iteration implicitly supports the results of my paper. For these reasons, I take QF’s second point not as a criticism but as a grudging endorsement of my paper.

Furthermore, QF’s suggested plan of attack, iteration, is not well-defined. They suggest iterating the density matrix, $\rho(r, k, \phi)$, over only a small part of the relevant phase space. This will lead to a fixed point, but it is generally not unique. Different iteration procedures will give different physical results. Thus, when the nonlinearity of the problem is important, it is difficult to perform reliable calculations.

C) As a third criticism, QF object to my statement that the adiabatic limit ($P_c \approx 0$) is the most appropriate zeroth order solution. This is because $P_c \approx 70\%$ for $E_\nu = 25\text{MeV}$ along the $Y_{e=0}$ line in Fig. 2 of Ref. [1].

However, $P_c$ is suppressed (approximately exponentially) and quickly approaches zero i) for values of $\Delta$ or $\sin \theta$ inside the $Y_{e=0}$ contour and ii) for neutrino energies less than 25 MeV. Suppression (i) applies for all the interesting values of $\Delta$ and $\sin^2 \theta$ except those very near the perimeter of the $Y_{e=0}$ contour. Suppression (ii) applies everywhere because the $\nu - \nu$ potential depends on lower energy neutrinos than nucleosynthesis does. Hence using the adiabatic limit in the potential is indeed valid.

In summary, QF’s Comment is superficial and is completely irrelevant to my paper. Their Comment is predominantly a set of ill-considered suggestions on how to generalize my work to describe the very nonlinear flavor evolution. A careful analysis shows that QF now implicitly endorse most of my results—although they don’t acknowledge them.
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