iant. At no point does the relation $A = 4\pi r_0^2$ (which happens to be true for nonrotating holes, but not for rotating ones) play any role. It has not been taken as a condition for deriving the Schwarzschild metric; Schwarzschild derived the metric in 1916, but the recognition of the significance of A dates from the late 1960's.

Brill and Salisbury's insinuation that in the Schwarzschild geometry wave and massless particle velocities are somehow different is the result of confusing the invariant (geometric) concept of velocity with that of coordinate speed which lacks physical significance. Since Yilmaz has made much of the supposed discrepancy in formulating his theory, I would like to go into this issue in some depth. It is easy to convince oneself that in *any* geometry waves and massless particles travel together. Because of the ever-present backscattering, "speed of the wave" can only refer to "speed of the wavefront." There are two simple ways to see that the front moves just as fast as massless particles. One is to realize that since the wave equation is a partial differential equation, the wavefronts are to be identified with the characteristics.[3] But for any kind of wave equation, these surfaces coincide with the lightcones of the spacetime. Of course, a massless particle travels right along the lightcones; so waves travel just as fast as massless particles.

The second way is to concentrate on high-frequency components of the wave: they suffer negligible backscattering, so their speed unambiguously defines the wave speed. Because of the high frequency, one is entitled to use the eikonal approximation in which the wave is represented by a phase factor and the gradient of the phase is identified with the wave vector.[2] The phase is found to obey the eikonal equation, which, as one well knows, is of exactly the same form as the Hamilton–Jacobi equation for a massless particle. Thus the dynamics of wave vector and massless particle momentum are identical: a wavefront travels so that a massless particle would just keep up with it.

Brill and Salisbury's argument that, since the horizon forbids exit of radiation, it must also forbid the "static" gravitational field from being felt outside the hole is a fallacy that every student of gravitation becomes familiar with. The difference is that radiation (transverse) components of fields are blocked by the horizon because they must travel along or inside the lightcone. By contrast the Newtonian or Coulomb (longitudinal) components may travel outside the lightcone (virtual quanta may have spacelike momenta) and so they are not obstructed by the horizon. Thus the hole can be detected from a distance by its Newtonian field, as it should.

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Neutrino oscillations credits

"Search and Discovery" in July (page 17) describes the present status of neutrino oscillations very well indeed, but the discussion of the origins of the subject is far from complete. It is only fair to the original authors that this omission be rectified.

To the best of my knowledge, the earliest mention of a "neutrino oscillation" appears in two papers.[1,2] written by Bruno Pontecorvo in 1957. It is clear from his writings that he was fascinated by the notion of particle–antiparticle transitions in the neutral kaon complex,[3] and in the first paper,[1] written in May 1957, he applies the same notion to other physical systems, especially muonium. At the very end of this paper he observes that, should the two-component neutrino theory prove to be incorrect, and should "neutrino charge" not be conserved, then neutrino–antineutrino oscillations could take place in vacuo. Five months later, encouraged by the possibility of a positive result in the original Davis experiment[4] (reactor antineutrinos on $Cl^{37} \to e^- + Ar^{37}$), he entered into a detailed discussion of ν–$\bar{\nu}$ oscillations, describing the principal experimental features of the phenomenon, and emphasizing the theoretical necessity for two Majorana mass-eigenstates that mix coherently in the physical ν and $\bar{\nu}$ states emitted in β-decay.

In 1959, Pontecorvo proposed an experiment[5] to determine whether the neutrino associated with the muon in weak interactions is distinct from the neutrino associated with the electron.[6] Even though he again drew an analogy with the neutral kaons, K^0 and $\bar{K}^0$, he did not pursue this analogy so far as to propose oscillations between ν_e and ν_μ. This latter proposal was left to Z. Maki, M. Nakagawa and S. Sakata[7] who in 1962 put forward a model of leptons that had $\varphi_e - \nu_\mu$ oscillations as one of its consequences. By the time Pontecorvo came to write the famous 1967 paper,[8] proposing neutrino oscillations as the solutions to the solar-neutrino problem, he too had realized the possibility of flavor oscillations in addition to the original particle–antiparticle ones.

It is therefore appropriate to conclude that Pontecorvo introduced the general notion of "neutrino oscillations" into particle physics, and that he invented a specific realization in the form of $\nu \longleftrightarrow \bar{\nu}$. Maki, Nakagawa, and Sakata, on the other hand, deserve the credit for inventing flavor oscillations. It is also fair to say that no single author has done more than Pontecorvo to stress the significance of these phenomena for neutrino physics.

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While I generally agree with what Peter Rosen says in his letter about neutrino oscillations I would like to add the following.

Rosen traced the line of thought of Pontecorvo and it is instructive to do the same for Sakata. In 1943 (published 1946) Sakata (with T. Inoue) proposed the π–μ chain to explain the cosmic-ray data. In this paper[1] he introduced the decay chain $\pi \to \mu \nu_\mu$, $\mu \to e \bar{\nu}_e \nu_\mu$ and observed that ν_e and ν_μ are in general different particles (ν_μ is called n and ν_e is called ν). In the 1962 paper,[2] in a sense he (with Maki and Nakagawa) returns to this equation by observing that if neutrinos indeed have non-zero masses, in general mass and weak eigenstates would not be identical. There they also discuss $\nu_\mu \longleftrightarrow \nu_e$

oscillations and $\mu \to e\gamma$ induced by the mixing. In the subsequent 1963 paper[3] they point out that the Brookhaven result[4] only shows that $\nu_\mu \to \nu_e$ is suppressed at a particular L/E and oscillations may still be taking place at other values of L/E. They also propose ways of measuring ν-masses of order 1 MeV by finding kinks in Kurie plots and discuss possible decays of heavier ν into lighter ν plus photon and give a reasonable estimate.

So in a sense Sakata and Inoue were the first ones to propose that ν_μ and ν_e were not identical. Subsequently, since the masses of both ν_μ and ν_e seemed to essentially zero, it seemed convenient and economical to assume that they were identical. In the period 1957–59 it was proposed by many people[5] that $\nu_{e\lambda}$ and $\bar{\nu}_{\mu R}$ make up a Dirac particle and then $\nu_e \neq \nu_\mu$, even if they have zero masses.

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September corrections

My friends in the English department might nod condescendingly to find a scientific journal's editors allowing such bloopers as "someone who's appetite is whetted" and "I grasped its grammer" (pages 68 and 98, September).

But what bothers me more is that your "We Hear That" item goofed *twice* in spelling the name of my friend and former colleague Lee Anne Willson, winner of the 1980 Annie J. Cannon Award in Astronomy. Sharpen up your blue pencils, please!

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