Lunar Neutrino Physics

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Abstract

The possibilities of the use of the moon as a base for conducting neutrino physics are examined, emphasizing neutrino astronomy. The principle advantage of the moon for this research is freedom from the atmospheric layer of the earth: cosmic rays hitting the atmosphere generate a rather copious source of neutrinos, which are a terrestrially inescapable diffuse background to neutrino astronomy. The cosmic ray generated neutrinos on earth are also a limiting background for other sensitive particle physics experiments, typically those performed underground.

The most severe limitation of conducting this type of physics research on the moon seems to be the pragmatic one of mass transport to the moon: many of the immediately obvious research initiatives which could benefit from lower backgrounds than possible on earth (eg. proton decay searches, low energy neutrino experiments) involve massive (megaton size) detectors. These experiments will thus not be practical until substantial manufacturing capability exists on the moon.

We do however identify two very different prospects for neutrino astronomy, in very different energy regions, which are worthy of more immediate study: 1) a 1 km^2 detector using the moon as the cosmic ray shield and the target for TeV neutrinos, and which observes the product muons emerging from the surface; and 2) more speculatively, an EeV detector employing acoustic detection to probe an otherwise inaccessible energy domain using the entire moon's core as the target. It is suggested that the first detector might perform experiments detecting a neutrino beam generated by a terrestrial accelerator, such as the SSC, permitting an interesting exploration of muon neutrino oscillations.

Several followup initiatives are suggested on low mass planar detectors and on lunar acoustic properties. It seems possible that the future of very high energy neutrino astronomy is on the moon.

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Introduction

First, an apology: this paper is not meant to be definitive in any way, but is only a tentative exploration of possibilities, and even those experiments considered are inadequately treated herein. Moreover, there is an inherent problem in such speculation as this in that it is very difficult to guess what physics will be relevant and what technology will be practical some years from now. That said we march bravely ahead.

Surely we can count upon the convergence now manifest between physics, astrophysics, cosmology, and astronomy, to continue for some time. In particular it would seem reasonable that any endeavor in neutrino astronomy which leads to a more sensitive detector than possible on earth will be at least as interesting as now, both in terms of particle physics and neutrino astrophysics.

Double beta decay, proton decay, dark matter searches, and generally, all the physics experiments that are performed underground on earth may benefit from similar sites on the moon. For example a low energy (few MeV) neutrino experiment may benefit from a lunar location in order to escape the background neutrino flux from terrestrial reactors, as pointed out by A. Mann in his paper in these Proceedings(1). One needs to examine the experiments individually, however, because the low energy radiation may not be better in particular instances (eg. if the limitation is due to local radioactivity of the surrounding rock, as is the case for double beta decay). Another concern for some of these, such as dark matter searches, is whether they will still be of high interest when a moon base becomes available.

For neutrino astronomy, the attraction of an experiment on the moon is that the background for high energy neutrino research will be $10^{-(3-4)}$ of it's level on earth. The background neutrino flux on earth is generated by the cosmic rays impinging upon the earth's atmosphere. The secondary pions and kaons produced in the atmosphere frequently decay before coming to rest. The cosmic rays striking the moon, and resulting secondary mesons, however, are generally absorbed prior to producing neutrinos. Very short lived particles, for example mesons composed of heavy quarks, decay so speedily that they result in neutrinos. The latter causes the limiting neutrino background on the moon. Since this flux is a result of heavier quarks, study of this 'direct production' (a misnomer) is of physics importance, particularly at extremely high energies, because it probes for new structure, for example a new generation of quarks, or for a new layer of matter (preonic structure). Thus just the study of the background flux of neutrinos on the moon has significant particle physics interest.

As Stenger has pointed out in his contribution to this Workshop(2), it appears that escaping the terrestrial cosmic ray neutrino flux provides no strong motivation for going to the moon to do neutrino astronomy at energies of a few GeV, nor does it do so for TeV energy range detectors of the size under construction in the ocean (DUMAND II for example, with 20,000 m^2 muon detecting area[3]). However, when moving to next generation detectors, with muon collecting area in the range of a few km^2 or more, backgrounds within a resolution circle of typically 1^o (determined by the neutrino-lepton scattering angle at a TeV) will become important, and a detector on the moon may then offer significant scientific advantage over deep ocean experiments.

First we discuss a neutrino detector in the TeV range, then we discuss the potential for detection of an SSC (or LHC or UNK or Eloisatron) type of neutrino beam in such a detector, and finally we examine the potential for a detector of much higher energy employing the acoustic radiation from 10^{18} eV neutrinos in striking the moon's core.

TeV Neutrino Astronomy

This discussion begins where Stenger's[2] ends, so the reader may want to review that paper first. However, this author is more optimistic about the potential of the moon for future neutrino astronomy. For the following discussion we take as a goal a minimum detector size of 10^6 m^2 for muon detection on the moon. The reader is further referred to the DUMAND II proposal[3], and to the various proceedings of DUMAND Workshops[4] to look at the prospects for neutrino astronomy for detectors in this size class. It appears that, in fact, detectors in the multi-km^2 class are going to be needed before we can really begin what astronomers would consider regular astronomy: the ability to look at sources at will, examining details of spectra and temporal behavior. The detectors on earth, proposed or currently under construction (eg. DUMAND II), will probably find a few sources, and may make great discoveries, but surely will not achieve the status of something like contemporary X-ray or gamma ray astronomy until at least another generation of instruments, and probably further yet in the future.

To give an example of possible rates: everyone's favorite X-ray binary system, Cygnus X-3, has been the subject of much study as a potential generator of neutrino fluxes. It is generally believed now that it may produce about 1 muon/1000 m^2/year[5] from neutrino interactions in the earth. The same flux would apply to muons emerging from the moon, and in our hypothetical 1 km^2 lunar array we could expect then as many as 1000 neutrino events per year from such a source. Moreover, this object, and similar objects, are well known as episodic emmitters, with outputs blooming by factors of a hundred or more for short times (typically one to a few times per year for bursts of a few minutes to a few days[6]). Particularly through the study of the temporal structure of such emissions one can begin to learn details of the astrophysics of such objects. One example of this is the potential to "neutrino-ray" the density profile of the companion star.

Assuming that the reader is convinced of the scientific worth of such an endeavor, let us examine some of the practical considerations. Taking as a given the necessity to utilize a minimum of material for such a detector, we are restricted to considering the use of the moon as target and shielding medium. Should one find substantial quantities of subsurface water, or develop a way to make glass in quantity on the moon, we could consider many attractive possibilities. We will assume that not to be practical for the present, but that we must import the detectors, which must detect and track muons emerging from the moons surface.

As an example, the technology of transition radiation detectors (TRDs) appears to have promise for this application. TRDs have the property of requiring only a small amount of mass per unit area, they are inherently directional, and produce signals proportional to a high power of the relativistic gamma factor of the traversing charged particle. This would permit measuring the energy of the emerging muon. If we could construct counters with 1 kg of mass per 10 m^2 of detector, then the total detector mass to cover 1 km^2 of area would be 100 metric tons, a plausible mass to consider sending to the moon. This seems to be an extremely low figure for mass per unit area, however. Note also that such a detector would almost surely have to be covered with shielding material against the downgoing cosmic rays (the amount needs study, and depends upon detector technique, but would be in the range of a few meters of moon dust). Clearly the engineering will require great cleverness.

The location for such a detector might be in a flat bottomed crater, with high walls (for maximum shielding). For good sky coverage (looking through the moon) the crater should not be far from the equator, so that the detector field of view would rotate over the whole sky once per 28 days. (Indeed, for the further future, 3 such detectors would cover the entire sky all the time). For purposes of detecting a terrestrially generated neutrino beam (see below), the location should be on the outer lunar surface.

The author suggests that it may be worth convening a technology study group to examine the projected limits of technology for such an endeavor. This might be followed by the investment of some funds to pursue likely techniques.

10 TeV Accelerator produced Neutrinos to the Moon

The idea of detecting a neutrino beam at great distance from an accelerator has been around for quite a while, but has not really been taken seriously for two reasons. First, the flux limitation has made detection rates very low, and second, the cost of bending the proton beam downwards into a target and subsequent decay tunnel has deterred even simple experiments using existing underground detectors. Both these objections may not apply to the hypothetical lunar neutrino detector.

First let us consider the rate question. A quick way to scale a rate is to employ the flux calculated for a proposed beam at the planned 150 GeV Fermilab Main Injector. We found that we might expect as many as 100 events of 20 GeV or more muons per week in DUMAND at 6000 km distance from Fermilab[7]. At 400,000 km on the moon we loose rate by a factor of 2.25 x 10^{-4}, but gain by area ratio of 50 for the 1 km^2 detector, in net being down to 1.125% of the rate in DUMAND. This is overwhelmed by the increase in rate due to larger machine energy, from which we get two exponents due to kinematics (neutrinos into a smaller solid angle), and one each from neutrino crossection and muon range. This would get us a factor of 2 x 10^7 for a 10 TeV machine, for a net gain of about 20,000 over the proposed DUMAND experiment. This is an overestimate because the Fermilab beam and duty cycle are probably several orders of magnitude higher than practical for the 10 TeV machine, and because a decay tunnel scaled from Fermilab is not practical (26 km). Even giving away a factor of 1000 for these would leave us with a rate of order of 1000 events/week, an eminently practical rate (with completely negligible background too).

The other problem for previously proposed remote neutrino experiments is also fortuitously answered because the neutrino beam naturally emerges tangent to the earth's surface, and it only remains to point it East or West. However, the alignment would occur only once per day, when the moon is rising (or setting). The dwell time of the beam on the lunar detector would only be a few seconds without beam steering; this is the biggest problem foreseen for such an experiment. This can be overcome by sweeping the beam to track the moon. It is difficult to imagine doing this over an angle of more than several degrees, corresponding to a dwell time of a few minutes. Beam steering would be needed anyway, because of the tilt of the lunar orbit relative to the equatorial plane. I estimate a rate of a few events per week in such conditions.

Is the physics of any interest? The important parameter for neutrino oscillations is the ratio of distance to energy, about 1300 km/GeV for the presently considered experiment, which corresponds to a δm^2 in the 10^{-4} eV^2 range. The latter is unchallenged by any other suggested experiment of which the author is aware for ν_μ oscillations. It is about a factor of 10 lower than the proposal of detecting a Fermilab neutrino beam at DUMAND, and is about a factor of 10^4 below existing accelerator based experimental limits. Given this unique opportunity it would seem to merit more careful consideration, though the beam steering requirements may prove fatal.

Another possibility for lunar neutrino research is to use the acoustical technique to search for neutrino interactions deep inside the moon. The idea is to detect a neutrino interaction via the feeble acoustical pulse produced when the neutrino generates a cascade of particles. The particles heat a long (10 m) thin (few cm) region of matter, essentially instantaneously on an acoustic time scale, leading to a miniscule radial expansion, and consequent radiation of a bipolar pressure wave. The outgoing pulse has a pancake beam pattern, being strongest in the direction perpendicular to the initial particle direction. The initial characteristic frequency is determined by the velocity of sound and the transverse shower dimensions, leading to typically a 20–50 kHz peak. In typical liquids (the moons core?), where the attenuation varies with frequency squared, the acoustic pulse amplitude possesses the peculiar property of decreasing as the inverse square of the distance from the source, including attenuation from the medium (no exponential decrease)(8). (The signal–to–noise ratio, in a thermal noise dominated medium decreases as the distance to the –5/2 power, again with no exponential fall off). At large distances the directionality decreases too, which could make reconstructing the original neutrino direction impossible if no wavelengths less than about 20 m remain in the detected signal. The flip side of this potential problem is that as the signal becomes less directional it is detectable, if at all, in a larger solid angle (fewer detectors needed to intercept the signal).

We have explored this detection method in the past, both theoretically(8) and experimentally(9), with the idea to employ it in the ocean. Our conclusion was that the threshold for detection of neutrino interactions was so high, around 10^{16} eV in the best circumstances, over a volume of ocean water in the few km^3 range, that no flux was likely. Of course the existence of such a flux would be a spectacular discovery, but the experiment such a long shot as not to be justifiable. The use of the moon may open up a new avenue however, in the following way.

Suppose that we are able to place geophones into the moon at a number of locations, so as to be able to image acoustic pulses from deep within the moon. Such pulses coming outwards from the core will have little background, because most of the lunar seismic activity will excite high order modes of the moon, and will not consist of solitary outwards–going pulses with known high frequency content. The same thing goes for meteorite impacts, from which one also probably gets substantial shielding by the thick dust layer on the moon's surface. With a sufficient number of such detectors (and we are not able to say now what such a number is), we can triangulate on the outgoing pulse and determine the location of the neutrino interaction, and the direction and energy of the cascade.

The target volume potentially available is quite amazing: about 10^{18} Tons (assuming about 1% of the mass of the moon can be monitored), roughly equal to the mass of the oceans! The threshold for detection needs study, but may be (author's guess) about 10^{18}eV. The cross section for neutrino interactions at 10^{18}eV may be about 10^{-33}cm^2(10) (an extremely interesting quantity which itself could be measured, in principle, by such an experiment, via the interaction depth distribution of events). The total effective cross section of the detection region would be then 6 x 10^{14}cm^2.

As an example to give some scale, let us calculate an event rate using the reported flux from the Fly's Eye experiment for particles coming from Cygnus X-3 with energy greater than 0.5 x 10^{18} eV, of 2 x 10^{-17}/cm^2/sec(11). These particles may well be neutrons, and if so, neutrinos are present in similar numbers. This flux falls approximately on a linear extrapolation of observations at lower energies, so we can take the flux above 10^{18}eV as 10^{-17}/cm^2/sec, so that the interaction rate from Cyg X-3 would be 200,000 events per year! Taking another example, the flux of neutrinos from our galaxy's central region may be about 10^{-19}/cm^2/sr/sec above 10^{18}eV(12), which would yield an event rate of 2000 events per year. Many other interesting phenomena might be observed, such as neutrinos from cosmic strings(13). Our estimates are very crude, but still probably alright at the order of magnitude level. The physics and astrophysics potential is thus enormous. The big uncertainty, upon which all this speculation hangs, is the detection threshold energy which as of this writing is little more than a guess, due to lack of lunar acoustic data.

An amusing possibility comes from considering the potential to acoustically detect a terrestrial neutrino beam interacting inside the moon. While individual interactions would not be detectable, a great many occurring simultaneously might be heard. For example, assume that a hypothetical beam from a 100 TeV machine might yield 10^{10} neutrinos of energy in the 10 TeV range. About 4% of these would be absorbed in traversing the moon, depositing 600 J of energy in a cylindrical region about 6 km in radius. Given the exact knowledge of time of deposition, and of beam impact one should be able to detect such a beam readily if the acoustic detection threshold is as guessed above. One could obviously make use of such a signal to map the interior of the moon.

Unfortunately, we can not go any farther without knowing information not presently available (to the author anyway): What are the acoustic properties of the deep moon in terms of noise, and attenuation versus frequency (the two most important questions)? We also need some material properties (specific heat, volume expansivity, speed of sound, energy loss rate) to calculate the pulse amplitude. The previous work on acoustic pulse generation by elementary particle cascades has (to this author's knowledge) only been concerned with pressure waves, but some radiation will occur in the solid by shear wave. This should be investigated, particularly since it could lead to interesting information (such as reconstructing the range, and perhaps angle, of the cascade from one observation point). Also, one must consider the Landau–Pomeranchuk–Migdal effect, which operates at extremely high energies, lengthening cascades. How practical is it to install geophones around the moon, and can they be placed sufficiently deep to have good acoustic coupling to the core region? Is the core region sufficiently homogeneous to have predictable acoustic propagation? How many such geophones could we reasonably expect to install? Is there sufficient interest in other science communities to support this effort as a cross–disciplinary endeavor (after all one would learn an enormous amount of geophysical information from such studies too)?

Summary

Two potential neutrino experiments for the moon have been identified, and both deserve further consideration in the author's opinion. (A number of others were considered and discarded too).

In the first case, that of a 1 km^2 TeV neutrino detector, practicality will hinge upon the technology of low mass large area detectors to be placed upon the lunar surface for recording upmoving muons. The author recommends that a study of potential detector technology be carried out. The physics is unquestionably unique and worth pursuing, though one must assess the competition from future deep ocean detectors. This experiment might also have the interesting ability to detect neutrinos generated in multi–TeV terrestrial particle accelerators.

In the second case, an acoustical experiment may be possible to probe ultra–high energies, using the moon's core as neutrino target. In this instance followup will require the interdisciplinary considerations of geophysicists and particle physicists to determine the likely lunar acoustic characteristics (most importantly noise and attenuation), and thus to determine if the detection threshold is really interesting (anywhere up to about 10^{20} eV would probably be worth pursuing). A study of the implantation of geophones is also needed. The experiment suggested would be truly spectacular, not approachable by any other means so far suggested.

It could be that the moon is indeed the location for the long range future of neutrino astronomy, and that the moon itself will be our neutrino telescope.

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