THIS PAPER IS INTENDED ONLY FOR MEMBERS OF THE DUMAND STEERING COMMITTEE. IT CONTAINS MATERIAL NOT INTENDED FOR GENERAL CIRCULATION. ITS PURPOSE IS TO GENERATE COMMENTS, SUGGESTIONS, AND EVENTUALLY ACTION.

ON STRATEGIES FOR DUMAND

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INTRODUCTION: SUMMARY OF REQUIREMENTS

A large and novel project like DUMAND can succeed in being funded only under certain circumstances, which are seldom explicitly stated. It seems worth while to review and make explicit what those circumstances are, and to see how close we come to achieving them. It will also be worth while to indicate both our strong points and our weaknesses. Additions, comments, and corrections are solicited.

1. The primary requirement is the existence of a clear, valuable, and preferably unique scientific goal. Secondary goals are valuable, as is technical progress consequent on the carrying out of the project; but the project must stand or fall on the basis of its primary goal.

2. If the project involves new technology, it must be demonstrated that the new technology is achievable. Progress in this direction before final approval will almost certainly be required.

3. The project must have the approval of the scientific community it relates to; and the more enthusiastic the better. It must have a sufficient constituency to warrant preferment above other projects in the same scientific area, in the same cost range.

4. The project must have a core of dedicated personnel and adequate full-time leadership. Nobody wants another Mohole.

5. The project must have a clearly defined program for achieving its goal.

6. To be acceptable to a Federal funding agency, it must conform to that agency's mission, and be clearly identified with it, so that its success will reflect credit upon that agency. Projects requiring multi-agency support have difficulty in satisfying this requirement.

7. The cost range in which a project falls determines its competition. It is therefore relevant to requirement No. 3, which relates to its support in the scientific community.

This paper will assume the 1980 DUMAND G2 Standard Array, as described in DUMAND Note 80-11, will supplant the current 1978 Standard Array.

PRESENT STATUS

DUMAND is now halfway through the first year of a two-year feasibility study. It has been in formal operation for five years now, and a great deal of work has been accomplished. It has had far more difficulty than the average new scientific project in defining itself, its goals, its technology, and its constituency. This was inevitable in view of the character of the project, which is unique in the degree to which it crosses conventional dividing lines between scientific disciplines, and the manner in which it flexes the muscles of several different technologies simultaneously.

The 1980 DUMAND Summer Workshop and Symposium is an opportunity for stock-taking, and probably the last at which so large a fraction of the DUMAND community will be assembled before it becomes necessary to begin the last step in initiating the DUMAND project: writing the formal proposal for its construction. It is of course possible to delay that step, but after so long a gestation period and the present feasibility study, that is beset with danger. It could be justified only if it can be shown that there is something crucial we don't know yet, which is essential to writing the proposal; or if we have not satisified the requirements stated above. It is now time to consider those in more detail.

HOW TO SUCCEED...

<u>Requirement No. 1.</u> Existence of Clear, Valuable, and Preferably Unique Goals. We have had difficulty with this, not because of the absence of such goals, but because there are perhaps too many possibilities, and there has been confusion as a result. Among the possibile goals are the following:

A. Neutrino Astronomy.

1. Search for neutrinos from gravitational stellar collapse outside our galaxy.

2. Search for extraterrestrial sources of high-energy neutrinos.

B. High Energy Physics Experiments Using Cosmic Rays.

1. Study of neutrinos produced in the atmosphere by cosmic rays. Search for new particles.

2. Neutrino Oscillations.

3. Studies of cosmic ray events at energies beyond the accelerator range.

A. NEUTRINO ASTRONOMY

1. Choice of Energy Range.

Not all these goals are mutually compatible. In particular, in neutrino astronomy a clear choice has to be made between studying the 10-MeV energy range appropriate to GSC, or the GeV- to TeV range relevant to the study of the existence of both diffuse and point sources of high-energy neutrinos.

This choice has already been made, in favor of the latter alternative. Since it has recently been questioned, we review the arguments in its favor. It was made on the basis of Tammann's calculations, at the 1976 Honolulu Workshop, of the observable rate of such events, and the size of detector needed to obtain a minimum acceptable rate. That calculation has been reviewed since then, without changing the conclusion. A recently proposed change, by a factor of 2, in the Hubble constant, which would tend to reduce the volume of detector required, (the Virgo cluster being closer than previously thought), would reduce the necessary volume by a factor of four, to a few times 10E7 tons. However, the cost of a GSC detector of even 10E7 tons still exceeds that of a high-energy neutrino detector by one or two orders of magnitude. Thus the original objection to it, that of excessive cost, still stands; and I will assume that our goal remains the high-energy domain.

However, even with a detector designed for high-energy neutrinos, there is some sensitivity to lower-energy events. In the DUMAND array, the volume occupied by a Sea Urchin, (perhaps a little more) might be thought of as sensitive to such low-energy events, provided it turns out that there is no large interfering background in either the ocean or the Sea Urchin module itself. Such backgrounds might include capture gamma-rays of about 8 MeV from neutrons due to spontaneous fission of the natural uranium content of the ocean (this is the largest ocean background) and signals due to radioactive decays of the materials of Sea Urchin itself. These have not all been evaluated as yet, but do not appear too serious.

Accordingly, with 6615 modules, each with a sensitive volume of about 50m3, a total effective low-energy neutrino detector volume of 3E5 tons is available. Since this is only one percent of the required volume, the range would only be one-tenth as great as that required to see the Virgo cluster, or about 1 Mpc on the current distance scale. Within that volume the only external galaxies detectable would be the local galactic cluster, including M31-33 and the Magellanic clouds. The expected event rate would be one event every four years.

The foregoing argument, reviewed in perhaps excessive detail, explains the difficulties associated with a search for GSC neutrinos from great distances. We have not repeated the arguments in favor of TeV neutrino astronomy; they are known to us all. As time goes by, they become steadily stronger, as new and stranger objects are discovered (like SS-433). As astrophysicists think more about possible neutrino sources, they invent new and more interesting hypothetical sources, all of which are grist for our mill.

B. HEP WITH COSMIC RAYS.

1.Study Of Atmospheric Neutrinos, New Particles.

The second part, high-energy physics using cosmic rays, has given the most difficulty. Any physics goal that can be reached by experiments on accelerators had best be left to them; especially in the range near 1 TeV. Thus we have very strong competition from accelerators on experiments that search for W and Z bosons, and for new particles that could be produced in the accelerator energy range (which we take as presently defined by colliding beams at 1 TeV each, or 2 x 10E15 eV laboratory energy for a stationary target.) In addition, plans are now apparently definite for the construction of a colliding electron-proton facility, of somewhat lower cm energy, which would be capable of studying directly the weak interaction. Thus our extensive efforts to prove that we could use DUMAND to do an interesting study of neutrino-proton inelastic scattering, although still valid, are not likely to carry much weight. Neither are the undeniable possibilities of observing Z's or other new particles; these cannot be considered as primary aims.

What remains to us, then, in this field? To my way of thinking, three major possibilities: neutrino oscillations, muon physics, and energies above 10E17 eV. Let us consider these.

2. Neutrino Oscillations.

If neutrinos do indeed have mass and neutrino oscillations exist, then DUMAND is in a position to observe the effects, particularly as a function of distance, by observing neutrinos produced anywhere in the earth's atmosphere. The distance between the source and detector varies from 20 to 13,000 km, In principle, this makes DUMAND a good tool for such observations; at 0.5 TeV, for example, the L/E ratio varies from 0.04 to 27, in units of m/MeV. However, the subject is one of keen current interest, and a considerable number of experiments will have been done by the time that DUMAND comes on line. Thus, unless DUMAND has some unique advantage, oscillations may not be a very good talking point.

Of course, if extraterrestrial neutrinos are observed, then their composition becomes a most important check on neutrino oscillations. This argument applies to both GSC neutrinos and high-energy neutrinos. For very long path lengths, the equilibrium ratios of the different neutrino types will depend, not on the mechanism of production, but only on the mass differences of the various neutrino types. In that case the equilibrium mixture will be independent of the nature of the source, and the mix will convey no information about the source - except for the neutrino/antineutrino ratio, which is relevant to the possible existence of antimatter sources. Unfortunately, this is a matter over which we have no control.

The total muon-neutrino flux observed at sea-level from atmospheric neutrinos will constitute a measure of whether oscillations can be observed; the normalization here is the number of observed muons. If muon neutrinos (97% of atmospheric neutrino production below 50 to 100 TeV) are not equal in number to the observed muon spectrum, which is known to about 10%, this is evidence for their disappearance (as pointed out by Reines et al.) Varying the zenith angle from zero to 180 changes the path length from 20 to 13000 km. The ability to measure neutrino energy can conceivably allow the direct observation of oscillations.

Monte Carlo calculations by V. J. Stenger seem to indicate that the steeply falling atmospheric neutrino spectrum (E-3.5) has the effect of approximating a monochromatic spectrum, providing one has a sharp detection threshold. Consequently a plot of neutrino intensities vs. zenith angle will show sharp minima, provided the mass differences fall in the appropriate range. It also appears that if there are oscillations involving tau neutrinos, DUMAND has a range of L/E far exceeding that available at a fixed location near an accelerator. These considerations require further development.

3. Cosmic Rays at Energies Above the Accelerator Range.
a. Cosmic-ray Muon Physics.

Another field available to DUMAND is that of conventional muon physics: depth-intensity relations, muon spectra, angular dependences, modes of energy loss at high energy, etc. These are attractive to a specialized fraction of the cosmic-ray community, which includes many of our best friends: Allkofer, Miyake, Zatsepin, to name a few. These people do not need to be wooed; any possible DUMAND array will be of very great interest to them. Unfortunately, the physics involved is often far from fundamental; its major current interest is in connection with neutrino oscillations; and there, investigations at considerably lower energies than DUMAND would be more informative (i.e. the GeV region.)

b. Cosmic Rays at Energies Above 10E17 eV.

In this region, the undisputed kingdom of cosmic rays, there are two areas of investigation open to DUMAND. One is the study of multiple muon events. To reach the DUMAND depth, muons need over 2 TeV at the surface. Multiple muon events, with 10 or more such muons, thus belong to the primary energy domain of about 10E15 eV and above. An interesting point to make about such muons is that they are probably all directly produced. Even at 1 TeV the decay length of a pion is 55 km, and the chance of seeing 10 decays is vanishingly small. The muons are thus concerned with charmed and other exotic quarks, or other processes not found at lower energies.

Since "directly" produced muons are either decay products of short- lived parents (e.g. charmed hadrons), or of still shorter-lived field mesons like the Z0, they constitute an effective probe of little-known processes at very high energies. Directly produced muons are readily distinguished from decay muons by their zenith angle dependence.

An interesting area to explore is the "decoherence" curve of such muons. This is cosmic ray jargon for the radial distribution with respect to the shower axis. Muons from atmospheric interactions are expected to have mean separations of about 5m at DUMAND depths. It is therefore easy to search for multiple cores, or wide-ranging single muons, and thus to identify large transverse momenta. The ability to measure muon energies for such events is clearly useful.

c. <u>The Combined DUMAND - Fly's-Eye Detector</u>

An even more interesting possibility is the combination of the DUMAND array with a "Fly's-Eye" array that looks at the atmospheric showers that initiate multi-muon events in DUMAND. The combination of DUMAND with a surface detector of such a nature that the energy and rate of development of the high-energy cascade could be observed has long been recognized as interesting. However, the difficulties of locating detectors on the ocean surface above the array were daunting. A possible way out of this dilemma may have been opened by the observation that, of the two possible DUMAND sites under consideration, one is only 40 km from shore, and might be moved closer, conceivably as close as 25 km. The range of the Fly's-Eye depends both on the energy of the shower and the gain and resolution of the optics. This problem has been examined by J. Elbert and N. Stanev of the University of Utah; the investigation is still incomplete. Preliminary indications are to the effect that at 25 km, a system with a threshold at 3E17 eV could be built on shore to look at the sky just over DUMAND. A rate of 100 or more events per year might be expected. As suspected, the combination of the deep muon detector and shower detector is much more powerful than either alone, and appears to give promise of distinguishing primary protons from heavier primaries. Any information about multiplicities will be interesting, if it can be transformed back into information about exotic quark production, or any other novel processes at very high energies.

SUMMARY.

This brief discussion shows four major possibilities for justifying the project. In descending order of importance, (my personal, biased opinion), they are:

1. The inauguration of high-energy neutrino astronomy.

2. High-energy cosmic ray studies.

3. Neutrino oscillations.

4. Cosmic-ray muon physics.

I would not care to argue about the relative priorities of 3 and 4. My main point is that it will probably be No. 1 that drives the project, with an assist from No.2, with 3 and 4 helping to recruit supporters. The last two are in a sense fall-back positions that guarantee, in the absence of any extraterrestrial neutrinos, that there would still be useful cosmic-ray data from the array. Considering current theoretical expectations, an absence of any extraterrestrial neutrino signal would itself be a remarkable piece of information, as was Ray Davis's surprisingly small solar neutrino signal. A suitable fallback position is a requirement not only for the sponsoring agencies, but also for the physicists asked to devote a large portion of their working life to the project. No. 2 seems to me a secondary major aim, not a fall-back position.

The foregoing discussion is necessarily superficial and incomplete. It needs a more complete and authoritative statement, and probably correction of errors of fact or emphasis. But since it is here that our hopes are pinned, it must be done as well and persuasively as possible.

DISCUSSION OF REMAINING REQUIREMENTS

Requirement No. 2. Demonstration of the feasibility of new technology required.

Following is a list of the new technology required by DUMAND, as best I can construct it.

1. New, highly sensitive detectors for Cerenkov light in the ocean: threshold sensitivity 50 quanta/m2 for a 3-electron trigger. Cost to be about $1k each; to satisfy specifications on noise, in-water weight, power consumption, reliability, and lifetime, etc.

2.Satisfactory signal processing technology. The signals from several thousand (originally 23000) modules must be processed to find all through-going muons (about 30/sec),and determine their trajectories; trigger on neutrino events, distinguishing them from muons; recognize multiple-muon events; and be sufficiently redundant to continue operating for ten years without catastrophic failure. there are obvious subsidiary requirements concerning cost, reliability, efficiency,redundancy, etc. There must be sufficient filtering capacity to avoid swamping the shore station with irrelevant data. It is clear that new PMT'S are required, but these require no new technology.

3. Deployment Technology. It was the unanimous opinion of all the experts who considered the problem in the 1978 workshop that deployment requires no new technology, but only applications

of existing technology. However, it is the largest undersea deployment ever undertaken, so it does break new ground.

This opinion relates primarily to the use of special ships like oil-well drill ships. It does not apply to the problems involved in towing the DUMAND modules, collected into strings and suitably packaged, to the site, transferring them to the ocean bottom, connecting them together, and testing the array. The technology for those operations also appears to be available; but again, the combination and scale contemplated introduce a considerable element of novelty, thus giving rise to a desire for some sort of experimental verification of the deployment concepts.

It now appears safe to say that the foregoing problems have all been solved in principle, and many in practice; that is, equipment satisfying the requirements can now be designed and constructed at predictable costs.

1. The Sea Urchin: the unknowns here concern questions like the mounting of the spines, their design for pressure tolerance, and their mechanical support. Solutions are available, though not yet necessarily final. No new technology will be needed.

2. The signal processing problem is a complex one; the final answer has not yet been selected. But we now have a choice among several possibilities, as shown by the 1980 Signal Processing Workshop. The fact that fiber optics communication is now an assured fact is of great help; it widens the possibilities, and may make it possible to end up with far less electronics on the ocean bottom than we originally thought necessary.

3. As noted above, existing deployment techniques appear to be adequate.

<u>Ocean Environment.</u>

There remains one technological problem of paramount importance, which physicists tend not to think about because they have little to contribute. This is the entire area of ocean environment. It is absolutely essential to a successful DUMAND program that we be be able to state authoritatively, that there exists a site where DUMAND can successfully be deployed; where the water transparency is adequate for the project; that biofouling and other interference from marine organisms is not a serious problem; and that for the proposed lifetime of the project - nominally 10 years - there is good reason to believe that the environment is sufficiently benign to allow the array to continue to function. We have far to go before these statements can be made.

<u>Requirement No. 3.</u> The approval of the scientific community is an absolute essential. It need not be unanimous; but it must be widespread and impressive. We cannot win with a majority, or even a strong minority, of negative reviews.

There are two communities involved. One is that of high-energy astrophysics - and it is for this reason that so much time is devoted at our meetings and summer studies to the subject. The prospect of detecting extraterrestrial neutrinos is inherently exciting to astrophysicists; it is our task to make it appear plausible. The cost of the project must also appear reasonable in relation to the possible knowledge to be gained; and in that regard the new DUMAND 80 Standard Array should be helpful.

The other community - and it is really a dual one - is that of high-energy physics and cosmic rays. These are not really identical, although in principle they should be. (Some individuals belong to both.) We need the support of both. We have strong elements of support in both communities, but it is not clear whether it is sufficiently widespread and enthusiastic. I am inclined to doubt; I think more is required. Reducing the cost of DUMAND is one of the best possible weapons; much of the opposition is of the simple, gut variety: "not out of my pocket, Buster", based on the zero sum-rule hypothesis.

Another potent weapon would be ability to demonstrate that the funding would not come exclusively out of the high-energy physics kitty. In cosmic rays I think the problem is primarily one of competition for funding. Here a strong push toward collaboration with the Utah group, and hopefully others as well, would be very helpful in showing that DUMAND can bring money into the cosmic-ray community.

The remaining problem of approval concerns the high-energy physics community. Opposition here will be partially disarmed by the cost reduction. Reducing the emphasis on high-energy physics experiments that compete, or are seen as competing with accelerator physics will be most helpful. A budget comparable to that of a colliding-beam detector allows a comparison of possible achievements in which I think a satisfactory case can be made for DUMAND.

<u>Local Support.</u> Another factor closely related to support from the Federal agencies is support from the parent organization, i.e. whoever it is under whose auspices the DUMAND project will operate. At present this is the University of Hawaii; and it is clear that the support given the project to date by both the University and State of Hawaii are largely responsible for the setting up of the DUMAND Hawaii Center. This kind of support will be essential for the entire duration of the project. safely through the initial approval and funding stages, even after scientific approval has been won.

<u>Requirement No. 4.</u> Existence of adequate personnel and leadership. This could be a serious problem. So far - quite understandably - few people have been willing to devote large fractions of their time to a project as chancy as DUMAND has been in the past. When, however, we reach the point of proposing the construction of a DUMAND array - be it mini- or full-scale - the

time will be at hand for all men to stand up and be counted.

Closely related is the problem of leadership. Either the project must be taken on by an existing group already involved in the project, or a new one must be started. From all viewpoints, the former is preferable. Certainly the funding agencies will prefer not to start a new organization to acquire a life and momentum of its own, and therefore hard to kill when its mission is accomplished. This viewpoint is particularly strong in the early stages; when DUMAND has achieved some real results, things may change very markedly.

If the mission is taken on by an existing group, the leadership, if not obviously available from outside, can be obtained from within that group. It must be plausible: the leaders must be neither too young nor too old, with a record of scientific achievement sufficient to give plausibility to the selection. Of course, the smaller the project the less stringent the requirements.

<u>Requirement No. 5.</u> The requirement for a clearly-defined goal appears to be obvious; but in the case of DUMAND that is not as easy as it sounds, since there are many possible choices. If the primary aim is to be neutrino astronomy, this must be apparent in the staffing of the project, its leadership, and its status in the astronomical community. The proposed modus operandi must be explicitly outlined, and successive stages clearly defined. In the case of DUMAND, where arrays of different sizes and types are involved, it must be clear what arrays are desired, when, and how much they cost, as well as what they contribute toward the aims of DUMAND.

<u>Requirement No. 6.</u>: Conforming to the Mission of a Federal Agency. This is a difficult one. There is no Federal funding agency that is the obvious recipient for our application, with the possible exception of NSF. However, the current status of NSF is not particularly favorable to us. There is a history of failure to fund earlier proposals; there is presently zero permanent staff in high-energy physics; and there is a lack of communication between different departments, as well as a strong predilection for small projects requiring small grants.

The alternatives are difficult; ONR is too mission-minded; NASA is confined to space; and DOE, in practice the best of all possibilities, is a bit outside its obvious concerns (remembering that DOE is in high-energy physics by historical accident only, not for organizational reasons.) In fact, our DOE friends have already warned us not to expect them to take the major responsibility for the project. With sufficient enthusiasm for the project in government circles, this difficulty can be overcome; but it is difficult to generate that enthusiasm, since no one as yet sees DUMAND as his baby.

<u>Requirement No. 7.</u> This is to be interpreted as saying: for God's sake keep the cost down to a minimum. I want to take

this as a signal that it is now time to come out with the annoouncement that through superior intelligence, hard work, divine inspiration, blood,sweat, tears, and a little bit of luck (not necessarily in that order), the DUMAND staff has been able to redesign the array so that all its objectives can now be achieved with a somewhat smaller array (a factor of two), at a cost now estimated as below \$20M. That announcement should be made at an appropriate time and place - perhaps the Summer Workshop - and be buttressed by an analysis of the new 1980 DUMAND G2 Standard Array and a comparison with the 1978 DUMAND G. It does not follow from this that our subsequent request for funding must be necessarily for this full amount, should we elect to start with a mini-; but it will change the entire scale of perception of DUMAND. In the language of high-energy physics, it is comparable with a colliding beam detector; in the language of astrophysics, a similar comparison must be found. We are at a disadvantage in that the fields of radio, x- ray, and gamma-ray astronomy, when new, could be entered at much less initial investment than neutrino astronomy, which necessarily needs very large detectors. At \$19M and .6E9 tons, DUMAND G2 costs three cents per ton! This is to be compared with the \$1K/ton characteristic of electronic neutrino detectors at large accelerators.

The End.