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LARGE SCALE DELIBERATE TRACER RELEASES IN THE OCEANIC THERMOCLINE

Final Report
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The Feasibility of Large Scale Deliberate Tracer Release Experiments in or below the Oceanic Thermocline

Report of work carried out under
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By

J.G. Shepherd and W.S. Broecker

Summary

The relative importance of lateral and vertical mixing in the interior of the oceans cannot be reliably determined from conventional oceanographic measurements, nor from observations of fallout radionuclides. It is suggested that the problem could be most effectively studied by an experiment in which a large quantity of a suitable tracer was deliberately released in deep water, and its subsequent dispersion was followed for at least one year.

The feasibility of such an experiment is examined. We conclude that it is indeed feasible, and that helium-3 would be the most suitable tracer, with tritium as a possible alternative.

About 30 g of Helium-3 would be needed for each release. Such a quantity may be dissolved in about 1 m^3 of water at pressures greater than 500 dbar, is easily available, and would cost about $100K. Concentrations would have fallen to unmeasurable levels by the time the patch had spread sufficiently to interfere with measurements of natural or radiogenic helium-3. The patch would need to be marked with dusters of neutrally buoyant floats in order that its movement may be followed. We estimate that it would be around 1000 km across after one year, and

anything from a few meters to a few hundred meters thick. Vertically integrating samples would probably be most suitable, used in conjunction with ship-board helium-3 mass spectrometer. Methods of dissolving the helium <u>in situ</u>, and making the release with minimum disturbance are also proposed. The cost would be several million dollars, spread over several years.

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Appendix A	The hazards associated with the release of tritium.
Appendix B	Preliminary design for helium-3 dissolution and release chamber.

1) INTRODUCTION

Conventional oceanographic observations of temperature, salinity, oxygen and nutrients have so far proved inadequate for us to estimate reliably the relative importance of lateral and vertical mixing in the interior of the oceans. The comprehensive and internally consistent data-set produced by the GEOSECS expeditions comes tantalisingly close to allowing a separation of the two effects in some places (Shepherd, work in progress) and more sophisticated methods of analysis may yet allow further progress to be made. However, the basic problem is that the sources and sinks of oceanographically interesting entities are distributed across a wide range of densities, and they can therefore reach the interior of the ocean either by movement along or across density surfaces. This problem is fundamental and will never go away: whatever deductions may be made from the distributions of these entities will never be incontrovertible.

Unfortunately the same difficulty applies to the transient tracers (fall-out radionuclides, etc.) recently released into the oceans by man. Their sources and sinks are also (naturally) at the surface and spread across a wide range of densities. Although the time history of their movement is yielding valuable information about the rates of ventilation of the oceans and more is to be expected from the Transient Tracers in the Oceans (TTO) expeditions presently being planned, such information on the relative rates of lateral and diapycnal* mixing as is gained from these observations will inevitably be inconclusive.

The question of these relative rates is however of crucial importance to many problems of great practical importance, notably the correct modelling of the fate of fossil fuel CO_2 or radioactive wastes in the oceans. Whilst some progress may be made with models in which the mixing processes are somewhat generalised (e.g., Peng and Broecker, work in progress) we cannot be sure that we have the correct answer until we have convinced ourselves that we have modelled the correct processes. The same is also true of models of the general circulation, on which our assessments of the fate of pollutants and climatic changes will ultimately depend. In these, incorrect modelling of mixing processes may lead to spurious dynamic effects (Veronis 1975), and incorrect results.

The whole question of mixing in the interior of the ocean has recently been reviewed by Garrett (1977). Briefly, the impression received from the study of large scale tracer distributions (Munk 1966; Reid, pers. comm.) is that diapycnal processes are significant, at least for some entities, some of the time, even though quantitative efforts to analyse such data have sometimes failed to confirm this impression (Rooth and Ostlund 1972; Needler and Heath 197). Further, physical oceanographic studies of diapycnal processes generally lead to much

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*Footnote: throughout this report we use the adjective "lateral" to describe processes occurring along surfaces of constant density, and "diapycnal" to describe processes occurring across (normal to) surfaces of constant density.

smaller numbers for diapycnal mixing than the "conventional" 1 cm^2/sec. In this category fall analysis of microstructure measurements (Gregg 1977; Osborne and Cox 1972), estimates of mixing by internal wave breaking (Garett and Munk 1975) and estimates of salt fingering and double diffusion (

Fluid dynamical opinion (Armi, pers. comm.) also has it that the stable stratification of the ocean interior is sufficient to suppress vertical overturning, although this is difficult to quantify because we know too little about local shearing rates. The difficulty with all such estimates is that they are only estimates of the vertical (diapycnal) mixing produced by particular processes, and there is no guarantee that the correct process is being examined. Indeed, it may well be that we have not yet thought of the most important processes. Certainly, during the course of this study we have thought of a new process which produces diapycnal mixing without vertical overturning (Shepherd in preparation). There may well be others.

How, then, can we best attempt to unravel the effects of lateral and diapycnal transport? The problem with conventional and existing man-made tracers is that their sources and sink intersect many density surfaces. We need to study a tracer whose sources (or sinks) lie as far as possible on a single density surface. This immediately rules out anything whose sources and sinks are at the surface, where the whole range of water densities are represented. Since few oceanographic entities have production or destruction rates strongly correlated with density, it also rules out all known tracers with sources in the interior. The closest approximations available would probably be helium-three and hydrothermally generated minerals released at geothermally active sites on the mid-ocean ridges (refs: Clarke _et_ _al_., 1969; Lupton and Craig 1975). Even so, the ridge crests extend over a wide range of depths and densities, and the source strengths are of course unknown.

All this leads one to wonder whether it might be possible to deliberately release a tracer in the deep ocean, and then study its dispersion directly. Some small-scale experiments of this type were carried out a few years ago (Ewart & Bendiner, 1974), but to our knowledge have not been continued. Small-scale experiments may tell us much about the detailed processes of vertical mixing, but information more relevant to our problems would be gained by large scale releases of tracer in sufficient quantity that it would remain measurable after at least one year, when it would probably have spread over a substantial fraction of an ocean basin. If this were possible, it would enable us to observe directly the relative rates of lateral and diapycnal transport in the interior, provided that one could insure that the release spanned only a very small range in density. This would not solve all our problems. Even if several such releases were made one would still have measurements only in a few places, and it would not be straight forward to extend these to the whole ocean. Measurements on patches also sample only part of the turbulent energy spectrum, and do not tell one immediately how to model transport in large-scale concentration gradients.

Nevertheless, such measurements should provide an immediate answer to the questions of whether or not the diapycnal mixing in the interior is negligible, whether it occurs primarily at topographic boundaries, whether or not the "classical" figure of 1 cm^2/sec for diapycnal mixing is at all meaningful. Most importantly, they should thus help us to decide how best to interpret other, more conventional data. They could provide the conceptual key to help us unlock the information in the data we already possess.

2) Experimental Concept

2.1) The spreading of the patch

The basic idea of the experiment is therefore to release, either in or below the main oceanic thermocline, enough tracer that it would still be measurable after a year or more, when it has spread over a substantial fraction of an ocean basin. The release might be instantaneous or prolonged: this question is considered below (section 4.2). First we discuss the way in which we might expect the patch to spread, according to our present very limited understanding of this matter.

There is now much evidence (refs. Reid, Pingree, Armi, etc. that mixing and movement in the deep ocean take place primarily along surfaces of approximately constant potential density (referred to the local mean pressure). In the course of this study we have been able to show that they cannot occur exclusively along such surfaces: diapycnal processes must exist (see Shepherd in preparation). Nevertheless, spreading and mixing along isopycnal or neutral surfaces is undoubtedly the best first approximation we can make. Direct measurements of diapycnal thickening and displacement are one of the principle goals of this experiment. However, we do not really know how fast, or in what manner the patch would spread -- this experiment would provide the first directly relevant information. As a starting point we might assume that the spreading process follows a Fickian diffusion law. Table 2.1 gives estimates of the diameter and thickness of the patch (twice its standard deviation) after various times, for a range of values of the turbulent diffusivity. We see that by the time (one or more years, perhaps) that the patch has spread across 1000 km or so, it could be as much as a few hundred metres thick.

Experiments on dye releases in shallow water, however, usually find that the patch spreads in a non-Fickian manner (see Okuko 19 for a review). The reason for this has been well-understood in principle since the work of Taylor ( ) and Richardson ( ) The spreading of a patch by turbulent diffusion is no more nor less than the repeated tearing apart of the patch by shear in the eddy field. and subsequent mingling of the fragments with water from outside. Eddies larger than the patch cannot tear it apart -- they merely move it around. Thus when one studies the spread of a patch one samples only that part of the eddy energy spectrum corresponding to eddy scales smaller than that of the patch. As the patch grows, one samples more of the spectrum, and thus observes a diffusivity which increases with the size of the patch, and thus also with time. This continues until the scale of the patch exceeds the maximum typical eddy size (if any), after which one would expect to see a relatively constant Fickian diffusivity.

There is no reason to suppose that the basic process of spreading along a neutral surface in the deep ocean is any different to that in shallow water. It is therefore most likely that the lateral spreading of our patch will follow a non-Fickian law, so that Table 2.2 is more likely to be appropriate.

Whether or not the diapycnal spreading will also be non-Fickian is quite unknown. As discussed in section (1), we do not know what the processes responsible for diapycnal mixing are. Much less do we understand how their efficacy would vary with the thickness of a patch. It seems prudent to allow the possibility that a non-Fickian law might apply, and carry this through when assessing the possible range of thicknesses one might conceivably encounter.

We comment in passing that if a non-Fickian law were appropriate, it might help to resolve the apparent discrepancy between the slow diapycnal growth of small-scale features (Armi, Rossby and the need for substantial diapycnal diffusion coefficients to explain the large scale distributions of tracers in the oceans (e.g. Fiadeiro 197).

Table 2.2 gives estimates of the size of a patch spreading according to the Okuko-Pritchard non-Fickian law (Okuko 19), for various diffusion parameters (those for diapycnal mixing being simply intelligent guesses).

We see from Tables 1 and 2 that after one year we may expect the patch to have spread to be a few hundred to a thousand kilometres across. It might be only a few metres thick, or might be a few hundred metres thick. A primary objective of the experiment should be to determine which of the latter possibilities in fact occurs. If the patch is still less than 50 m thick after one year, it is hardly likely that Fickian processes have been operating. Conversely, if the thickness exceeds 50 m it is most likely that they have.

We need some estimate of the maximum likely volume of the patch to estimate how much tracer needs to be injected so that it still will be measurable one year later (these estimates are made in section 3).

We adopt 1E 14 m^3 (corresponding to 1000 km x 1000 km x 100 m) as a sensible round number. It is not very likely that the patch would spread more than that. If it spreads less the concentrations within it would remain measurable for a longer time, and the time-scale of the experiment could and should be extended to cover several years.

2.2) Keeping track of the patch

We envisage the patch one day after release as being about 1 km across and one metre thick. It will be difficult to inject the tracer so that its initial thickness is any less than 1 m, and even if it were possible, it would be difficult to sample it effectively. This patch will move away with the deep current at a speed of perhaps 1 to 10 cm/sec, presumably following its own density surface. How is one to keep track of it?

It would clearly be highly desirable that the initial phases of patch movement should be followed. The ship should not leave the area without knowing at least the direction of movement when last seen, and the density surface on which the patch actually lies. It is therefore, highly desirable that it should be possible to begin sampling the patch immediately, and analyse the samples on board ship. If this can be done the initial spreading may also be studied, giving an extra bonus of information (although this is not the primary purpose of the study).

Since temperature measurements can be made more easily and more accurately than those of salinity, and we wish to follow the movement (if any) of the patch relative to density surfaces as accurately as possible, it would be best if the experiment were carried out in a region with a tight T/S relationship, where temperature is the main determinant of density.

This would also permit some refinement in the tracking of the patch. It would obviously be highly desirable if the patch were marked with neutrally buoyant floats. Passive floats most nearly follow surfaces of constant in-situ density rather than those of potential density, as one would wish, and in any case slowly creep deeper as their metal bodies yield under pressure. It would be advantageous if actively ballasted floats (Rossby, per. comm.) could be used to track surfaces of constant temperature. This would minimise (but not eliminate) the risk of the floats becoming detached from the patch by failing to track the correct density surface.

The most suitable location for the experiment would therefore probably be somewhere in the central North Atlantic, well to the east of the main gyre, so that the patch is unlikely to move too rapidly, but is in a region with a well-defined T/S relationship.

We therefore envisage that the ship that releases the tracer would mark it with a mixture of short-range and SOFAR neutrally buoyant floats, (preferably of a temperature-seeking variety) and with their aid would track and sample the patch for the remainder (several weeks) of the leg. The float cluster would be revisited three or four months later, and with a little luck the patch would be relocated, and its lateral and vertical extent determined by sampling. At this stage we guess that the patch would be maybe 100 km across and anything from 1 to 50 m thick. The concentration of tracer would probably have fallen 100-fold from the initial survey, so that ship-board measurements will be more difficult, although they should still be possible with suitable tracers, since concentrations should still be 100 to 1000 times the ultimate detection limit, if enough tracer is released.

Should the patch be lost at this stage the only resort would be to request the cooperation of the oceanographic community in obtaining samples from all ships in the right ocean, in the hope of finding it by chance. A tracer which can be sampled simply and easily without special equipment would thus be desirable.

Assuming however that the patch is indeed relocated, it should be re-marked with floats (unless the original ones have remained sufficiently close), and the temperature being tracked should be adjusted, since the patch will most likely have departed somewhat from the original temperature horizon.

Assuming that the experiment is progressing satisfactorily, it would at this point be possible to begin a "piggy-back" procedure, by making another release (at a different depth) whilst surveying the original one. This would ultimately yield more information for the money expended, since in the later stages, when the patches are ocean-wide and the main costs are ship-time for large-scale sampling surveys, more than one patch could be sampled simultaneously. Possibly three releases could be superimposed in this way. A tentative schedule for this strategy is outlined in Table 3. Logistics are discussed in more detail in section 3.4.

2.3) Interference with other work

There are those who would criticize purposeful He^3 injection because they would hamper efforts to use as a tracer of the He^3 generated by the decay of bomb-produced tritium. While we admit that careful consideration must be given this problem, we offer several reasons why this need not be the case.

1) If the He^3 injection is made in a region where the He^3 and H^3 distributions have been well mapped and if these distributions show smooth gradients in the vertical and along isopycnals, then the superposition of a

well defined anomaly on this background will not destroy its value. If He^3 time trends are desired, either the anomaly can be subtracted or a sufficient amount of time allowed to elapse until dispersal and outcrop loss has made the anomaly negligible.

2) It is possible that the consideration that the long-term perturbation of the He^3 field by the injection may be negligible will place upper limits on the amount of He^3 added.

3) Many people feared that the superimposition of bomb ^{14}C on the natural ^{14}C distribution would void much of the value of the former. In hindsight we see that the positive aspects of the bomb ^{14}C far outweigh any disadvantages.

Nevertheless, before any deliberate tracer scheme is agreed upon we will seek to obtain the approval and hope also to obtain the participation of those people engaged in H^3-He^3 measurement programs in the ocean.

2.4) Summary of Objectives

We conclude this section with a summary of the objectives of the experiment, in approximate order of priority. They are to determine

(a) the rate of diapycnal thickening as a function of time (and hence the nature and rate of diapycnal mixing).

(b) the magnitude of movement relative to isopycnal surfaces.

(c) the rate of lateral growth (and hence the nature and rate of lateral mixing).

(d) the rate of lateral movement.

In stating these objectives we have used the term 'isopycnal surface' loosely. It is most likely that the relevant surface is in fact a neutral surface (Ivers 197). The determination of such surfaces is discussed in section 4.3.

3) Feasibility: Possible Tracers

3.1) General Considerations

We determined in section 2.1 that we need to inject sufficient tracer that it remains easily measurable even when diluted into a volume of about 1E14 m^3 of water. We adopt as a standard of comparison the requirement that this concentration be ten times the detection limit* for the tracer. Ultimately, of course, more or less tracer than suggested by this calculation might be used. We shall find however that this alone is sufficient to rule out most of the conceivable tracers.

It would also be desirable if the tracer were

a) non-hazardous

b) cheap

c) easy to sample and store

d) analysable on board ship (in less than one day)

e) conservative (i.e., not degraded in, or removed from sea-water) except for predictable decay.

The requirement for properties (a) and (b) is obvious. Property (c) would be particularly desirable if a widespread search for a patch had to be undertaken, and it would also permit maximum information return as institutions and nations not directly involved in the experiment might be persuaded to collect samples, particularly in the late stages of the experiment.

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* Footnote: we here use "detection limit" to mean the minimum detectable difference of concentration from background.

Property (d) is highly desirable: without it a sampling survey would have to be conducted "blind", and the probability of losing the patch would be enormously increased. Property (e) is required in order that the tracer should accurately track water movements. Only approximate conservation is however necessary, as much would be learned even from a somewhat non-conservative tracer, provided it lasted for at least one year.

We now discuss the properties of various possible tracers according to these criteria. The conclusions are summarised in Table 4.

3.2 <u>Tritium</u>

Tritium is one of the least toxic radionuclides, an isotope of hydrogen, and therefore highly conservative (as HTO) in water (apart from its predictable radioactive decay). It can be detected rapidly at moderate concentrations (say 300 pCi/l ≡ 100 T.U.) by simple distillation and liquid scintillation counting, and most sensitively (but only slowly) by allowing growth of its daughter (He-3) which is detected by mass-sprectometry. The detection limit is about 0.1 T.U. (0.3 pCi/l), so that 300,000 Ci would be required. No unusual sampling methods are required, and 100 ml samples are sufficient. The cost of so much tritium is difficult to determine. Through regular channels it would be about $1 million, but alternative sources of supply might be found for a large quantity. Tritium thus satisfies properties (c) through (e), and possibly also (b). It is not entirely non-hazardous, although it is unquestionably the least hazardous radionuclide in the marine environment. Approximately

2,000 MCi are in the oceans already as a result of nuclear weapons tests, and do not constitute an appreciable hazard. Natural processes produce about 2 MCi each year, giving a natural inventory of about 30 MCi. Conservative calculations indicate that as much as 100,000 MCi could be jected each year into the deep sea (IAEA 1978), and 1000 MCi could be injected each year even into coastal waters without leading to a substantial health hazard. Quantities of the order of 0.1 MCi per year are in practice discharged into European coastal waters by nuclear fuel reprocessing plants. It is reasonable to conclude that the release of 0.3 MCi in the deep ocean would lead to a negligible health hazard, once it had been safely released. Detailed estimate of individual and collective doses and detriment are given in Appendix A . The transport and actual release of such a quantity of tritium would of course require stringent safety precautions, since the maximum permissible annual intake of tritium is about 3 mCi (for a member of the general public). This should not present any serious problem, however, as tritium is an extremely weak β-emitter, and therefore leads to no external radiation hazard. Its emission is in fact insufficiently energetic even to penetrate the skin, and the only hazard arises as a result of ingestion or inhalation.

Nevertheless, the release of radioactivity to the oceans is an emotive matter, and a scientific assessment of the hazards is unfortunately hardly relevant. A proposal to make releases of the size contemplated would undoubtedly lead to uproar in certain quarters, and would require careful justification. Although the resulting arguments might in the long run be educational and beneficial for the world in general, it is tempting to conclude that if it is possible to avoid the hassle by using a non-radioactive material, it would be sensible to do so. The political and legal position is however discussed in a little more detail in Appendix A.

3.3) Helium-3

Helium-3 is the less abundant (0.00013%) stable isotope of helium. It occurs at slightly elevated concentrations in the oceans because it is the daughter product of decaying tritium (whether natural or radiogenic: see Jenkins and Clarke 1976), and because it is exhaled during geothermal processes at the midocean ridges (Lupton and Craig 1975). It is measured by gas extraction and mass spectrometry (Clarke *et al.*, 1969, Jenkins *et al.*, 1972). The detection limit is about 0.3% $\delta(^3He)$ which corresponds to 3E-20 g/g, since the natural ^4He concentration is about 4E-5 ml/l, or 7E-12 g/g (Riley and Skirrow). The quantity of tracer needed is therefore only 30 g (about 250 gaseous litres at STP). Since the solubility of helium is about 8 ml/l (1E-3 g/h) at 1 atmosphere pressure, this quantity could be dissolved in about 600 l of water at 50 atmospheres (corresponding to 500 m depth), and correspondingly less water would be needed at greater depths.

Helium-3 is available without difficulty in gram quantities, is completely non-toxic, and is absolutely conservative in subsurface water.

Only small samples (∿100 ml) are required, and storage requires care but no undue difficulty. There seems to be no reason why a helium-3 mass spectrometer should not be made to work on board ship, (Jenkins, per. comm.), and the cost of 30 g of He-3 would be around $100,000. The cost of the sea-going mass spectrometer would probably be around $400,000 for a fully installed and operable system.

It appears that helium-3 satisfies all the requirements for the tracer most satisfactorily.

3.4) Rhodamine-B

Rhodamine-B is a fluorescent dye routinely used in shallow water dispersion studies (see e.g. RHENO). It has also been used in the only deep releases known to us (Ewart & Bendiner 1974). It is measured by fluorometry, and has the great advantage that sub-surface, in-situ and real-time measurements can be made. The detection limit is however only about 1 ppb (1E-9 g/g). Our experiment would therefore require 1 million tons of Rhodamine to be released, which is obviously impossible. We conclude that Rhodamine and similar substances are unsuitable for this experiment.

3.5) Deuterium

Deuterium is the less common stable isotope of hydrogen (abundance 0.015%), available as D_2O (heavy water). It is non-toxic and at first sight might provide a non-radioactive substitute for tritium. It can be detected by mass spectrometry at concentration anomalies of about 0.1% (δD). Unfortunately, however, most of the atoms in the ocean are hydrogen, so that this corresponds to a mass confrontation of about 1E-7 g/g. On would therefore need 100 million tons of heavy water for the experiment, which is obviously impossible.

3.6 Freons

Freons (fluorocarbons) have recently been investigated as tracers in the oceans. They can be measured by gas chromatography using an electron capturedetector with a detection limit of about 10ppb. Some 10 million tons would be needed for this experiment which is impossible. We comment that their interest as oceanic tracers arises not so much because of the sensitivity of detection, as because of the large quantities (million tonnes) which have been released during the last 30 years.

3.7 Caesium - 137

Caesium-137 is a β/γ emitting radionuclide, chemically somewhat analogous to sodium. It is not perfectly conservative in sea-water, since it is adsorbed onto particulate material (NAS. 1971) but particulate concentrations and settling velocities in the deep ocean are sufficiently small that the activity would only be scrubbed out of the patch on a timescale of about 100 years, so that this would not entirely invalidate its use.

It can be measured at levels of about 2 pCi/m^3, using chemical extraction and beta-counting techniques (Bowen *et al*., 19 ; Kupferman and Livingstone 1979) so that about 2000 Ci would need to be released. It is produced in large quantities as a waste product of the nuclear power cycle, and its specific activity is about 100 Ci/g, so that if it could be obtained carrier-free only about 20g of a caesium salt need be injected into (say) 1 m^3 of sea water, giving a 0.02 g/kg density charge, which could easily be compensated for.

However, Cs-137 is approximately 10,000 times more radiotoxic in the marine environment than tritium (allowable intakes are about 100 times smaller and it concentrates by a factor of 100 or so in many foodstuffs).

Thus the hazards associated with a caesium release would (although still small) be about around 100 times greater than for tritium (even though the curie quantity is much less). Such a quantity of Cs-137 would also generate substantial external radiation dose rates and therefore require shielding and special handling procedures during transport and release.

Since much larger (∿100 litre) samples are required, and the analysis is more difficult, it appears that Cs-137 (or similar β/γ emitting radionuclides) would have no advantages over tritium for a large scale release. This might not be true for a small scale experiment, where it could be detected in situ using submerged scintillation counters, at concentrations of about 100 pCi/l (D.F. Jefferies and E. Reynolds, Pers. comm.).

3.8 Discussion

We have identified only three tracers for which the experiment proposed is feasible. These are tritium, helium-3, and caesium-137. The latter has no advantages over tritium and will not be considered further.

Both tritium and helium-3 fulfill satisfactorily all our criteria of suitability. We select helium-3 in preference to tritium because:

(a) it is non-radioactive, and would avoid the political, administrative and public relations problems which would surely arise over the release of a large quantity of tritium.

(b) it can be stored and handled without special safety precautions.

(c) the release mechanism etc., may be tested and observed in action as often as desired using cheap helium-4 instead of expensive helium-3.

(d) It may be measured rapidly, with the highest precision, on board ship.

Nevertheless we stress that we believe that the hazards associated with the release of up to 1 MCi of tritium would be acceptable small (see Appendix B), and recommend that it be retained as an alternative should helium-3 ultimately prove to be unsuitable for some unforeseen reason. Tritium would have a possible advantage in that helium-3 measurements could also be made on the same samples. Since helium-3 (its daughter) would be lost to the atmosphere when the patch intersected the surface, whereas tritium would not, some additional information would thereby be gained about gas exchange with the atmosphere, and rates of mixing near the surface. We do not consider this possibility sufficiently important to outweigh the other advantages of helium-3.

The enormous advantage of helium-3 (or tritium, measured as helium-3) for this experiment arises from the conjunction of four factors, namely

(a) the low abundance of helium-3 compared with helium-4

(b) the low solubility of helium in water

(c) the ability to achieve one million-fold concentration enhancement by simple gas extraction.

(d) the ease and sensitivity of mass spectrometric detection for low mass number.

Factors (a) and (b) together lead to a low background, and thus contribute to the low detection limit. This is further enhanced by factors (e) and (d), which would apply only for a rare gas of low mass number. There are no other rare gases of low mass number with low abundance stable isotopes. We conclude that helium-3 is uniquely suitable for this experiment. Since the sensitivity obtainable corresponds to the detection of only about one hundred thousand atoms, we also conclude that it is most unlikely that any other tracer could be detected more sensitively.

4) Experimental Method

4.1) Location of Release

In order to minimise the difficulty of keeping track of the patch, it would be sensible to carry out the experiment in a region where deep currents are not unusually energetic. As pointed out in section 2.2, it would also be of some technical convenience if the T/S relationship were fairly tight, with temperature the main determinant of density (although this might conceivably mean that the diapycnal mixing rates could be atypical. In the first instance it would also be prudent to avoid the equatorial region at latitudes lower than 15°, and the polar regions at latitudes greater than 50° (these interesting regions could perhaps be studied by later experiments, if the first is successful). There would be no point in going further than necessary from the U.S. coast, and in order to facilitate collaboration by other nations the Atlantic would be preferable to the Pacific. These considerations together suggest that the experiment should be carried out in the N. Atlantic somewhere between latitudes 20°N and 40°N, and longitudes 30°W and 60°W. We propose that the optimum location for the experiment should be discussed by a meeting of interested oceanographers before a decision is reached.

The depth of the release (or releases) should be sufficient to avoid the near surface regions (down to a few hundred metres) which would not be typical of the interior of the oceans, and from which loss of helium-3 to the atmosphere would be likely. The region below about 2000 m occupied by the lower N. Atlantic Deep Water in the western Atlantic should also be avoided, to prevent interference with work already in progress on the movement of radiogenic helium in these regions. Within these limits we can identify three depths which would be of particular interest:

(a) about 500 m: this would be fairly representative of the oceanic thermocline, would afford the possibility of eventually studying the interaction of the patch with the surface if it moved sufficiently far north or west (and remained observable), and would enable stations to be occupied and samples to be taken fairly rapidly. 700 m might in fact be a good choice, as it has been extensively used for SOFAR float work, and is ideal for such work (Rossby, pers. comm.).

(b) about 1000 m: this is approximately the depth of the base of the main thermocline.

(c) about 1500 m: this is about the depth of the upper North Atlantic Deep Water, and would be reasonably characteristic of the deeper, less highly stratified ocean.

Releases at these depths would be sufficiently far separated vertically that they would not be expected to interfere with one another for several years, by which time tracer concentrations would probably have fallen below measurable levels.

As suggested in section 2.2 it should be possible to make releases at these depths in "piggy-back" fashion, getting three times the amount of information for less than twice the cost (see sections 4.5 and 5). We suggest however that the optimum depths for the release or releases be also discussed by interested scientists before a decision is taken.

4.2 Method of release

Most of what has been said so far applies with equal force whether the release is envisaged as a pulse (completed with a day or less, perhaps) or as quasi-continuous, extending over a month or more. The method of release would obviously differ completely, however.

Either method of release has advantages and disadvantages. A pulse release is conceptually simpler,and would use less ship time. It is also probably easier to arrange that a pulse release is confined to a very narrow range of density surfaces (the practical aspects are discussed below). The interpretation of the results of a pulse release is probably also more straightforward, using the methods familiar from shallow-water research (see e.g., Okubo). The disadvantage is that mixing processes in the deep ocean are probably somewhat episodic, and Murphy's Law makes it likely that a single release would be made under atypical conditions. One might, for example, happen to make the release just as the local internal waves decided to break, or as the largest eddy of the year came by. Thus at least the initial behavior of the patch could not be guaranteed to be typical. However, as the lateral scale of the patch will probably increase rapidly, within a few weeks it would be averaging over a considerable part of the spatial spectrum, so that this problem is probably not too severe so far as the long-term results are concerned. A related practical difficulty is that the initial eddy field may shear the patch into two or more almost separate pieces, which in spite of the increasing lateral scale never really coalesce again. This happened, for example, in the 196 RHENO experiment in the North Sea (ICES). This possibility must simply be accepted as a fact of life.

A quasi-continuous release would partially overcome this problem, by adding an element of time averaging to the experiment. This would seriously complicate the interpretation of the results, however, because the mean currents and eddy field would draw out the release into a tortuous snake-like pattern, perhaps ten times as long as wide, and one would always need to know which part of the snake one was looking at in order to estimate how much it had grown. In the long term lateral spreading would reduce the problem, but by then the two types of release would become similar in any case.

We rather favor a pulse release because of its conceptual and practical simplicity, but suggest that this question should be a further subject for discussion before a decision is taken. We discuss the practical aspects of both types of release below.

(a) Pulse release

For a pulse release, whether of tritium or helium-3, we assume that one starts with the tracer already dissolved in about 1 m^3 of sea-water (possibly artificial) of the appropriate density. This is necessary because otherwise its density would be so far different that it would rise or sink as a blob, spreading onto a range of density surfaces as it did so. Also if one were to release gaseous helium it would inevitably form bubbles, which would rise, dissolving as they did so, and fail to produce the desired "spike" injection.

There are two ways in which the initial $1m^3$ of tagged water could be obtained. Firstly, by taking real or artificial sea-water of the correct salinity, and dissolving the tracer in it either in the laboratory or on board ship, within a suitable container the container would then be lowered to the desired depth, allowed to reach ambient temperature, and released. The disadvantage of this method is that the release container would have to be rather carefully constructed. In the case of tritium it would have to be designed to avoid leakage of tritium and consequent radiological hazard. In the case of helium-3 it would have to be pressurized in order to prevent helium (dissolved under 50 atmospheres or more) from escaping into the atmosphere. In both cases the container would nevertheless have to be capable of allowing the tagged water to escape without hindrance at the appropriate time. This would pose considerable engineering problems.

The second alternative is to dissolve the tracer in situ. In this case the container serves only to trap ambient water long enough to dissolve the tracer. It need not be pressurized or provide a high degree of containment, and its design should be fairly straightforward. This method could not be used unless the dissolution of the tracer does not change the density significantly (otherwise the tracer would "fall" - up or down - out of the container rather than spreading laterally as desired). It would therefore be unsuitable for tritium unless this can be obtained as virtually carrier-free tritiated water. For helium, however, the dissolution almost certainly could be carried out in situ, and we recommend this as the most suitable method.

The rate of dissolution of helium in water is likely to be limited by diffusion through the liquid boundary layer at the helium/water interface.

Piston velocities of a few metre/day are commonly observed at the sea surface under fairly turbulent conditions (Broecker and Peng To saturate $1m^3$ of water through a surface contact area of (say) 1000 cm^2 could therefore take several days, even with substantial agitation. We conclude that a special bubbler mechanism would need to be designed to ensure dissolution within (say) one day. This could of course easily be tested in the laboratory using helium-4. A preliminary design is discussed in Appendix B.

If in situ dissolution of helium is used the density change of $1m^3$ of water should be insignificant. 30g (10 mole) of helium-3 atoms are added. They will occupy approximately the same volume as 10 mole (180 g) of water molecules, so the density will be decreased by a factor (iEG+30)/(1EG+180) corresponding to a change from Co = 27.00 to Co = 26.85, for example. Such a change is unlikely to cause violent vertical motion of the initial "blob". It corresponds to a buoyancy force of 150 g.w.t. on a mass of 1EGg, and therefore to an acceleration (neglecting viscous drag) of 0.15 cm^2/sec. It would therefore take at least 36 sec for the blob to rise one metre, by which time it would probably have at least doubled in volume (and thereby reduced its density contrast). Drag forces would reduce the extent of vertical movement. However, to minimise such vertical displacement, (and consequent diapycnal mixing) it would be prudent to raise the container to the depth where its new density would be in equilibrium, using data from simultaneous STD measurements. This displacement might amount to about one hundred metres at five hundred metres depth. Alternatively, and preferably, about 150 g of salt could be added.

In any case great care must be taken to ensure that the tagged water has the same density as that into which it is to be released.

Using in situ dissolution it should be possible to design a simple container with doors which would spring open when an electrically or mechanically triggered latch is released (an explosive release would be undesirable because of the heat and turbulence generated). A preliminary design is suggested in Appendix B. Such a device should of course be subjected to stringent "swimming pool" tests using colored dye during the development of a final design.

(b) Continuous release

For a continuous release the dissolution of the tracer presents no particular problem, since one may simply inject tritiated or helium-saturated water at a metered rate. The latter could be obtained by slowly pumping ambient water through a helium reservoir, using a low-power submersible pump such as that developed by Weiss for the MANOP program). There would however be substantial difficulty in ensuring that the tracer is released into the same density surface (within ± 50 cm, say). Any tethered release system would move relative to density surfaces because of (a) ship motion (b) internal waves (c) variable currents tilting the mooring. Problem (a) could be overcome using a bottom mooring, but problems (b) and (c) cannot. We suggest that it would be preferable to work directly from a ship, using an injector which is controlled so that tracer is released only when the temperature (or possibly density, using a computer/STD system) lies between suitable narrow limits. This might work reasonably well at 500m where the temperature limits might be 0.005 °C apart, but may not be feasible at 2000m, where they would need to be less than 0.001 °C apart. The response time of the thermometer, as well as its precision, is of course important, particularly if one is dealing with rapid movements produced by ship motion. Since fairly low-frequency variations in the depths of isotherms/isopycnals is also to be expected, some manual or automatic control of the release depth would also be necessary. Ideally, perhaps, a temperature-seeking servo-controlled winch should be used. Whatever procedure is adopted there is no doubt that it would be technically more difficult to carry out a continuous release onto a single density surface. The difficulties are probably not insuperable, however, and should not be permitted to disallow a continuous release if this were thought to be a more desirable experiment.

4.3) Tracking the patch

We have already determined in section 2.2 that it will be essential to mark the patch, whether from a pulse or continuous release, with neutrally buoyant floats in order to have some remote indication of its whereabouts. Unless this can be done successfully the chance of finding the patch again, even after a year, is rather small. As discussed in section 2.2, actively-ballasted temperature-seeking floats would be most suitable. We suggest that perhaps 6 SOFAR floats should be used with each release, together with about 12 of the deep-sea drifters whose development is presently being planned at the University of Rhode Island (Rossby, pers. comm.). These cheap devices would surface after a predetermined time, and emit radio signals receivable by satellite. This would give the best available indication of the location of the patch immediately prior to a survey cruise. The SOFAR floats yield additional information about how the patch reached this position, and its dynamical environment (existence of eddies etc.). We also propose that about 12 Swallow floats (which respond locally to a ship on station) be used to provide real-time information on the location and motion of the patch. This would be extremely valuable to a chief scientist attempting to plan and execute a sampling survey of a moving and deforming target. If floats prove to provide sufficiently good information of patch location, it would also be possible to use the locations of such a number of floats to define the local frame of reference in which the survey is carried out.

If a pulse release is used, the release should be made close to the middle of the cluster of floats. If a continuous release is used, floats should be released regularly, perhaps one each day, so that an outline of the "snake" is obtained.

The success or failure of the experiment will depend crucially on the ability to relocate the patch. It would be most desirable to carry out a preliminary experiment to determine whether or not the floats do satisfactorily track the patch. This should combine the simultaneous release of neutrally buoyant floats, fluorescent dye (so that the operation of the release mechanism can be observed, possibly from a manned submersible) and a small quantity of helium-3, so that the efficacy of the sampling and survey techniques may be tested. In this way the entire procedure may be checked out before a large release is made. Such an experiment would be most worthwhile in its own right, as it would provide thee first direct indication of the extent to which neutrally buoyant floats behave as trully Lagrangian tracers.

In order to determine the expected depth of the patch, and thus also to determine movement relevant to neutral surfaces, it will be necessary to calculate the depth of the neutral surface on which is patch is presumed to lie. A Fortran program to follow neutral surfaces on a station by station basis has been written as part of this study, and is appended at Appendix D. It has been tested on the GEOSECS W. Atlantic Section, and the results for several surfaces are illustrated in a paper being prepared by one of us (J. Shepherd).

4.4) Method of sampling

It will not be an entirely straightforward matter to sample effectively a patch which may possibly be no more than a few metres thick, and will almost certainly be somewhat lumpy in structure. We believe that the vertical location can probably be determined quite accurately by identifying the depth of the appropriate density surface from an initial STD cast. The best possible data on the horizontal extension of the patch would then be obtained from vertically integrated samples over an appropriate depth range spanning the expected depth. These should be obtained by pulling a continuously running sampler through the patch at a constant rate. The sampler could be pumped, or operated by sucking through a controlled leak into an evacuated bottle, or a spring-operated piston sampler. Such a system would need to be developed and tested.

This technique, using stacks of vertically integrating samplers (say ten, one above the other, so arranged that the samples obtained do not quite overlap in depth) would also be the most effective way of locating the patch in depth to the nearest 10m or so. Thereafter one would use a combination of conventional bottle samples (for maximum spatial resolution) and vertically integrated samples depending on the structure and development of the patch actually encountered.

We note again, however, that samples must (probably) be taken on and around a particular density surface (rather than a particular depth) and that this would probably be most reliably located using temperature measurements. One must therefore expect to have an STD attached to the sampling device throughout any sampling operations, and note that a temperature-controlled winch would be a valuable asset.

Preliminary enquiries indicate that such a device is feasible, and would probably involve the modification of a D.C. electrically driven winch by the addition of a suitable temperature sensor and control unit (Markee Machinery Co., pers. comm.). Substantial development and testing would however, be necessary to determine the efficacy of the device, and the maximum frequency of fluctuations it could be expected to handle.

4.5 Logistics

If the sampling system is attached to the STD cable, then lowering to (say) 1000m sampling and raising the system again need only take about one hour. Samples could be outgassed and analysed whilst steaming to the next station. During the initial survey, (see tables 2.3 and 2.2) when the patch will probably be less than 30 km across (but moving at up to 1 km/hr) steaming time will be short, and one might hope to work about 8 stations per day. This would allow a 5x5 grid (probably the minimum to delineate the patch effectively) to be worked every 3 days, and a 10x10 grid in 10 days. One would attempt to carry out perhaps two 5x5 surveys, and one final 10x10 survey, during the initial phase.

During the second (3-4 month) survey, when the patch is perhaps 100 km across, steaming time would be more significant, but the time developement would be slow, so that one might attempt to work only one 15x15 grid. During one-year and subsequent surveys steaming time would be the limiting factor (say 100 km or 3 hours between stations), and one would probably work no more than 4 or 5 stations per day, and hence put down a single 10x10 grid during one leg. The sampling schedule outlined here is not intended to be definitive, only to give some idea of the general procedure and numbers of stations involved.

Should the "piggy-back" arrangement of successive releases mentioned in section 2.2 be adopted, initial surveys after the first could be contracted (one would hopefully have established by then whether or not floats were satisfactory indicators of patch position). One might combine a single 5x5 initial survey with a 10x10 second survey of the previous patch. The schedule for various legs might be similar to that shown below.

Leg 1:	Preparatory hydrography, Float preparation Release preparation	3 days
	Float Cluster Release Tracer Release #1	1 day
	Ad-hoc survey	3 days
	First 5x5 survey	3 days
	Second 5x5 survey	3 days
	10x10 survey	10 days
	Steaming (to and from port)	6 days
		29 days

Leg 2 & 3	10x10 survey of Patch #1	10 days
	Preparatory work	3 days
	Float & Tracer Release #2	1 day
	Ad hoc survey	3 days
	5x5 survey of Patch #2	3 days
	Steaming	8 days
		28 days
Leg 4 et		
	10x10 survey of Patch	20 days
	Steaming	10 days
		30 days

A substantial allowance for steaming time is made because the port of departure would probably have to be decided before the location of the patch was known.

4.6 Ancillary Experiments

The information on rates of lateral and diapycnal mixing obtained will be vastly more useful if they are accompained by concurrent measurements of other oceanographic processes which may be related. We have in mind particularly , observations of temperature/salinity microstructure, and of the internal wave field. The detailed geometry of the isopycnal surfaces is also of interest, and would be determined naturally during the course of sampling surveys. Current measurements will also be available from float tracking. It would probably not be worthwhile to mount special ancillary experiments on each survey leg, but they should certainly be carried out on at least one leg (possibly in conjunction with the one-year survey), using another ship if necessary. Some observations from moored current meters might also be worthwhile at this stage, in order to study the high-frequency end of the current/turbulence spectrum which is not accessible to measurement by floats.

5.) Costs

We here make tentative estimates of the costs of the experiment as we envisage it. These are intended only for general guidance, since detailed costs cannot be estimated until firm proposals are prepared. They are expressed in 1979 dollars, and will also need to be corrected to allow for inflation. We have also made estimates for two experiments: firstly, a basic experiment involving a single release, and secondly, an extended experiment involving three releases at different depths, carried out in "piggy-back" fashion (see section 2.2). We have broken down the costs to show that part associated with the development and construction of new equipment, and also identified separately the costs associated with the pilot experiment. These costs would fall during the development phase (perhaps two years) of the experiment. The remainder would fall during the third and possibly the fourth year.

These tentative cost estimates are detailed in Table 5. The basic experiment is likely to cost several million dollars, and the extended experiment about 50% more. The largest item in both cases is ship's time for sampling surveys, as would be expected.

Table 5: COSTS

	$K
Development	
Design & Construction of release system	150
Design & construction of sampling system	30
Temperature-controlled winch modifications	50
Shipboard helium-3 system	400
Salaries	100
	$ 730 K
Pilot Experiment	
Tracer	negligible
12 Swallow floats	60
12 Deep-sea drifters	20
6 SOFAR floats	100
Float-tracking equipment, charges, etc.	30
Ship-time (one leg)	150
Salaries (scientific & support staff)	100
Travel, shipping, expendables, etc.	20
	$ 480 K
Basic Experiment	
Tracer	100
12 Swallow Floats) from pilot experiment	0
6 SOFAR Floats)	
12 Deep-sea drifters	20
Ship-time (4 legs)	600
Salaries	400
Travel, shipping expendables, etc.	100
	$ 1220 K
Total Cost for Basic Expt.	$ 2430 K
Extra Cost for Extended Experiment	
Tracer (2 extra shots)	200
24 Swallow floats	120
12 SOFAR floats	200
24 Deep-sea drifters	40
Ship-time (3 extra legs)	450
Salaries	200
Travel, Shipping, Expendables, etc.	100
	$ 1310 K
Total Cost for Extended Expt.	$ 3740 K

6.) Conclusions and Recommendations

1) The deliberate release of a sufficient quantity of a tracer in the deep ocean to remain measurable after it has spread over a substantial fraction of the ocean is feasible.

2) The most suitable tracer would be (stable) helium-3, with (radioactive) tritium as a possible but unlikely alternative.

3) About 30g of helium-3 at a cost of about $100K would be required. for each release.

4) There would be no hazard associated with such a release of helium-3.

5) Alternatively, about 300,000 Ci of tritium at a cost of about $1M would be required, and some small hazard would be involved.

6) It would be feasible to release helium-3 (or tritium) either as a pulse, with an initial volume of about $1m^3$, or continuously over a period of a month or more.

7) A scientific meeting should be convened to discuss the relative advantages of a pulsed or continuous release, and the most suitable location for the release, together with all other aspects of this experiment. This report should serve as a basis for discussion.

8) It would be necessary to mark the "patch" with a mixture of locally responding and SOFAR (or pop-up) floats, and these should preferably

be actively-ballasted temperature-seeking types.

9) Shipboard analysis of tracer samples will be almost essential to avoid losing the patch. This should be possible for helium-3 even at very low concentrations. A specially designed and constructed helium-3 machine would be needed to carry out many analyses as fast as possible.

10) Vertically-integrated samples would be an essential complement to regular bottle samples, and a suitable sampling system would need to be designed and built.

11) The patch is likely to be about 30 km across after 1 month, 100 km across after 3 months and several hundred km across after one year. Its thickness might increase to no more than a few metres during one year, but might also increase to several hundred metres. The principal goals of the experiment should be to determine:

(a) the rate and extent of thickening.

(b) the rate of diapycnal displacement;

(c) the rate of lateral growth.

(d) the rate of lateral displacement, vis-a-vis floats.

12) We tentatively suggest that the best location for the experiment would be the central North Atlantic; between 20°N and 40°N, and between 30°W and 60°W. Suitable depths for the release would be 500m, 1000m and 1500m, in order of preference.

13) If a pulse release is used, <u>in situ</u> dissolution of the tracer would be preferable. With helium-3 it would be essential to simultaneously dissolve some salt to maintain neutral buoyancy.

14) If a continuous release is used, great care will be necessary to ensure that tracer is released only into water with densities in the appropriate narrow range, and this may be a tricky engineering problem.

15) With sufficiently rapid shipboard sample analysis (three per hour) it should be possible to work 100 stations in ten days on early surveys, or twenty days on later cruises. This should permit the patch to be reasonabley well delineated by single=leg surveys during the first year. Thereafter (if the patch is still observable) multi-ship or multi-leg surveys would be required.

16) Up to three releases would probably be incorporated in a "piggy-back" scheme, in which surveys of one patch are combined with the release of another at a different depth. This would ultimately yield three times the information for less than twice the cost.

Table 1

The Spread of a Patch under Fickian Diffusion

		Lateral Diameter (km)					Vertical Thickness (n)				
		Diffusivity					Diffusivity				
sec	Time	1E6	3E6	1E7	3E7	1E8	0.1	0.3	1	3	10
8.6E4	1 day	8	14	26	45	80	3	5	8	14	26
6.0E5	1 week	22	38	69	120	220	7	12	22	38	69
2.6E6	1 month	46	79	144	250	456	14	25	46	79	144
7.9E6	3 months	79	137	251	435	790	25	44	79	137	251
3.2E7	1 year	159	277	506	876	1590	51	88	159	277	506
1E8	3 years	283	490	894	1549	2830	89	155	283	490	894

Table 2

The Spread of a Patch under Okubo-Pritchard Diffusion

Time		Lateral diameter (km)					Vertical Thickness* (n)				
		Diffusion parameter (cm/sec)					Diffusion Parameter (cm/secx1E5)				
sec		0.1	0.2	0.5	1	2	0.2	0.5	1	2	5
8.6E4	1 day	0.17	0.3	0.9	1.7	3.4	-	0.01	0.02	0.03	0.09
6.0E5	1 week	1.2	2.4	6.0	12	24	0.02	0.06	0.12	0.24	0.60
2.6E6	1 month	5	10	26	52	100	0.1	0.3	0.5	1.0	2.6
7.9E6	3 months	16	32	79	158	316	0.3	0.8	1.6	3.2	8
3.2E7	1 year	64	128	320	640	1280	1.3	3.2	6.4	13	32
1.0E8	3 years	200	400	1000	2000	4000	4	10	20	40	100

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* Note: We have virtually no idea what values of diffusion parameter would be appropriate for vertical mixing of this form. The values here are ranged around 1E-5 cm/sec, which would be roughly equivalent to a vertical diffusivity of 1 cm^2/sec at a length scale of 1 km.

Table 3 Tentative Schedule

Time	Patch 1 500m	Patch 2 1000m	Patch 3 2000m
Leg 1 Zero to 1 month	Release & initials survey (30 km)	--	--
Leg 2 4 months	Survey 2 (100 km)	Release & initial survey	--
Leg 3 8 months	--	Survey 2	Release & initial survey
Leg 4 1 year	Survey 3 (300 km)	Survey 3	Survey 2
Leg 5 2 years	Survey 4	Survey 4	Survey 3
3 years onward	Further Surveys?		

Table 4

Summary of Tracer Properties

Tracer	Method of Det.^{n}	Det.^{n} Limit	Quantity Requ'd	Initial Water Vol.	Toxicity	Cost	Sampling	Shipboard Analysis
Tritium	He-3 ingrowth	0.1TU(0.3pCi/1	300,000 Ci	1 m^3	Slight	$1 M	Simple	Yes (at high conc.)
Helium-3	Mass Spect.	0.3% δHe-3 ≡ 3E-20 g/g	30g	<1m^3(@ 50 atm)	None	$30 K	Simple	Yes
Rhodamine B	Fluorometry	1 ppb.	1000000 tons	Enormous*	Slight	Enormous*	Simple	Yes
Deuterium	Mass Spect.	0.1%δD ≡ 1E-6g/g	1E8 tons (D_2O)	Enormous	None	Enormous	Simple	No
Cs-137	Concentration/ gamma spectrom.	2 pCi/m^3	2000 Ci	∿ 1m^3	Slight	$6 M	Large vol.	No
Freons	Gas chromatography	10 ppb	10 M tons	Enormous	??	Enormous	--	Perhaps

*Note: Enormous means anything in excess of 1 million m^3, or $100 million.

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Appendix A

The Hazards Associated with the Release of Tritium

A 1) General, Legal and Political Considerations

In this Appendix we make rough estimates of the possible radiological hazards associated with the release of tritium to the sea, using essentially the methods recommended in the context of radioactive wastes by the IAEA (1960). First however, it is worthwhile to sketch the legal and political framework within which such a release would have to be contemplated.

There are at present no international conventions governing the release of radioactivity for scientific purposes. There are however two conventions which are almost relevant. These are the Landen Convention which prohibits the dumping of high-level radioactive waste at sea, and the Convention, not yet in existence, which will presumably one day result from the U.N. Law of the Sea Conference..

The London Convention is not strictly relevant because it relates only to "dumping" of wastes, and a planned scientific release is certainly not "dumping" within the meaning of the Convention. Nevertheless, it makes absolutely no difference from the point of view of radiological hazard whether the radioactive material is unwanted waste, or has been specially manufactured at great cost. We believe that it would be essential to comply with the spirit of the Convention, even though the letter of the law might not apply. In this case this would mean preparing an assessment of the hazards associated with the release, and submitting this to the competent national authority (the EPA) for approval. We sketch such an assessment in the remainder of this Appendix, and believe that there is no technical reason why a release should not be approved. The U.S. government has however adopted a policy refraining from radioactive waste disposal at sea, and whether it would be prepared to countenance a release for scientific purposes is not known.

The deliberations of the UN Law of the Sea Conference have not so far resulted in a convention. Should one be agreed upon in the near future, its implications would have to be considered.

A2) Maximum dose to an individual

We now proceed to estimate the radiological hazards pertaining to a release of tritium. We have taken general guidance from the methods used by the IAEA in assessing the hazards associated with waste disposal in the deep sea (IAEA 1978a, 1978b) and note that their estimate of the maximum permissible release rate of tritium to an ocean the size of the N. Atlantic is 100,000 M Ci/year.

The relevant radiological limits for individual dose are the HRP dose limits for a member of the general public, and from these are derived (IAEA, 19) maximum permissible annual intakes. For tritium ingestion the figure is 2.6 mCi (per year), and we work with this figure (although various amendments have been suggested subsequently, none has yet been officially adopted).

Suppose R curies of tritium disperse in the course of the first year across an area A (in subsequent years it will have spread further, diluting the activity and reducing the maximum individual dose). We shall calculate the dose associated with consumption of contaminated fish: IAEA experience suggests that doses through other less well-established pathways would be similar). Even if the activity were confined to a layer a few metres thick, the fish would not be. They would sample the activity averaged over a depth (d) of not less than 100 m, say. The release would be at a depth of 500m or more (where there is in fact little fishing, especially in the region proposed). To obtain an upper limit for this dose calculation we shall assume that the activity rises by some magical and unspecified process to the surface. The concentration of activity in sea-water (assumed at the surface) would thus be R/Ad. That in fish would be the same (tritium does not concentrate in biological materials), if they stayed in the same place long enough to build up equilibrium concentration. The annual intake of someone eating a quantity Q of fish per year would then be QR/Ad. Taking A = 1E16 cm^2 (i.e. 1000 km x 1000 km), d = 1E4 cm (100m), Rc = 300000 Ci, and Q = 1E5 g/yr (corresponding to 300 g/day, a tolerably high consumption rate), we find that the maximum intake would be 3E-10Ci. Even three such releases would therefore lead to only about one millionth part of the maximum permitted, at most. The dose involved would be less than 0.001 mrem, which is utterly insignificant, by any standard at all.

A3) Collective dose and detriment

Using similar assumptions we can estimate roughly an upper limit for the collective dose involved, integrated over the entire world population and all time. Suppose the rate of fish catch from the area involved is a quantity F per unit area per year. Then the total quantity of contaminated fish consumed (by somebody or other) is AF. Since the concentratio is R/Ad, the total activity ingested each year is RF/d, which is independent of A, the area of spreading. This will therefore be the same each year, except that it reduces because of radioactive decay. The total activity ingested over all time is thus RFT/d, where T is the mean lifetime of tritium (1.4 x the half-life or about 17 years). A sensible number for F is 1E-4 g/cm^{2}yr (for coastal waters), so the total activity is 3E5x1E-4x 17/1E4 Ci, or 0.05Ci. This is 20 times the MPAI, and thus corresponds to 20x0.5 rem, or a collective dose of 10 manrem, at most. We note that only a tiny fraction of the activity release is ever ingested by man: most decays away in the sea. A collective dose of 10 man rem is not large. It is the quantity which would be received by two people receiving the maximum permissible occupational exposure for one year. The probability that anyone would die as a result of ingesting tritium released from the experiment may be roughly assessed as less than 1 in 1000 (UNSCEAR 19).

A4) Discussion

The simple calculations above do not purport to be a complete assessment of the hazards associated with a release of tritium. They do serve as an example of the type of calculation which would be necessary in a complete hazard assessment, and of how such an assessment might be approached. There are of course other pathways to be considered, but experience suggests that these are unlikely to be dramatically more important. We believe that they show that if tritium were after all chosen as the tracer for this experiment it would be possible to establish beyond reasonable doubt that the hazards would be acceptably small.

This system clearly could and should be tested in the laboratory using helium-4 (preferably at various pressures). The recirculation pump would probably need to have a capacity of about 30 litre/min, and develop a pressure difference of about 1 atmosphere at the ambient pressure of the release, but this should be determined experimentally. We have carried out some preliminary laboratory tests which demonstrated that such a devise can indeed be made to work.

B′-2) Release

With the in situ dissolution method the chamber need not withstand any substantial pressure differential, as all operations can be carried out under ambient pressure, and its construction is therefore simplified. The main requirement is to eject the contents of the chamber as clearly as possible, and then to remove the chamber with minimum disturbance of the resultant patch.

Bearing in mind that the release will probably be made into water moving with a current of several cm/sec, we suggest the following arrangement. The chamber should be square in plan, and be equipped with a pair of vanes (one above and one below, so as to avoid interference with the patch once ejected) so that it orients itself with the current. The chamber should be equipped with two pairs of spring-actuated doors, one pair on the upstream face, and one on the downstream face. The release may then be effected by opening the upstream doors inward, and the downstream doors outward (not necessarily very fast) as illustrated in Fig. C-2. This should start the contents moving out in the downstream direction, and the ejection will be completed as the current sweeps through the interior of the chamber. This must therefore be designed to present minimum obstruction to the current when the doors are open. Finally, to permit withdrawal with minimum

Appendix B

Preliminary Design for Helium-3 dissolution and Release Chamber

B-1) Dissolution

We require a bubble system which will produce a bubble surface area in excess of 1000 cm^2 (corresponding to about 10 thousand bubbles 3mm in diameter at any one time) and substantial turbulence (to minimise boundary layer resistance). Since it cannot be guaranteed that all the bubbles would dissolve during the first pass through the system, a recirculating system will be required. The general layout of such a system is indicated in Fig. C-1.

It is 50 cm high, and 140 cm square. Helium is blown by a recirculating pump through a distribution plate which forms the base of the chamber. In this are drilled about 20 thousand holes spaced on 1 cm centres.
The roof of the chamber is pitched, and leads up to a central outlet through which undissolved gas is returned to the pump. Provision must be made for

(a) venting the chamber during descent to allow pressure equalisation.

(b) flushing with ambient water at the release depth (perhaps using the recirculation pump.

(c) closing the vent before injecting helium.

(d) injection of concentrated salt solution for buoyancy compensation.

(e) injection of gaseous helium-3 from a pressurised cylinder.

(f) pressure eaualization using a scaled bladder during injection and dissoution (sealed to prevent premature release of helium-3 tagged water).

(g) recirculation of helium-3 until all is dissolved.

(h) release of the contents to the exterior (see next section).

disturbance, the whole system (included perhaps an associated STD) should be equipped with conical fairings above and below, as illustrated in Figure C-3.

The doors should presumably close onto synthetic rubber gaskets to ensure a good seal. If the front doors are spring-loaded to open, but held closed by a catch which can be released on command, they could in turn hold the rear doors closed by means of struts under tension (see Fig. C-2).

This design is only a tentative proposal. The efficacy of prototypes should certainly be tested using colored dyes in shallow water, with visual observations by divers.