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Eclectic Modelling of the North Atlantic. II. Transient Tracers and the Ventilation of the Eastern Basin Thermocline [and Discussion]

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Eclectic modelling of the North Atlantic. II. Transient tracers and the ventilation of the Eastern Basin thermocline†

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Renewal rates of the waters of the thermocline in the eastern North Atlantic are estimated by combining linear quasi-geostrophic dynamics with steady and transient tracers into a unified eclectic, reservoir model. The two-dimensional model first employed is finally rejected when it is found that it generates oxygen-utilization rates (OUR) that are, by conventional biological wisdom, too high. The three-dimensional model that replaces the two-dimensional one shows that the OUR is indeterminate, with possible ranges from zero to unacceptably high values. The region is flushed primarily from the north and east.

The problem of using transient tracers is mathematically equivalent to that of distributed-system boundary-control theory, the open-ocean boundary conditions playing the role of the unknown control variables. The missing time histories of this new set of unknowns means that tritium and helium-3 distributions are only comparatively weak constraints on the flow field, but do set upper bounds on the vertical exchange with surface waters. Surface Ekman pumping is adequate to explain the interior distributions without additional buoyancy ventilation, although this latter process is possible. Some speculation is made about conditions under which transient tracers might play a more definitive role.

1. Introduction

We have termed 'eclectic modelling' the pooling of many different types of oceanographic observation into internally consistent models of the general circulation. Oceanographic observations seem inevitably disparate in type, duration and sampling strategy, rendering difficult the conventional comparison with model results. In pursuit of procedures for making the type of synthesis that is required, we examined in part 1 (Wunsch 1984*a*) the meridional heat flux of the North Atlantic Ocean by combining a variety of different observations and hypotheses into an eclectic model. The central data were the International Geophysical Year (IGY) zonal hydrographic lines, and estimates of the wind-stress driven Ekman flux. The central hypotheses were geostrophy and a quasi-steady state. Auxiliary observations and hypotheses involved a great many estimates of deep velocities in the ocean, the flux of the Florida Current, Mediterranean outflow, etc. Despite the several hundred constraints that were written down, the system remained underdetermined and the strategy for exploration was to seek upper and lower bounds upon the meridional flux of heat as a function of season and as an annual mean. The major feature of part 1, apart from the specific numerical estimates of flow and heat flux obtained, was the demonstration that it is possible to combine quantitatively the inevitably fragmentary and diverse measurements that characterize oceanographic observations.

The original intention had been that part 2 would combine all the data of part 1 with the

† Part 1 appeared in *J. phys. Oceanogr.* **14**, 1712–1733 (1984).

transient and steady-tracer data that is extant in the North Atlantic to further refine the estimates of heat flux and other flow properties. A number of events have led however, to deferral of that goal. Of most importance, an entirely new data base for the North Atlantic is gradually becoming available, a data base consisting of several higher density, higher quality hydrographic lines (see, for example, Roemmich & Wunsch 1985). These new lines also contain, as the IGY did not, a suite of accurate oxygen and nutrient data, as well as information on some of the transient tracers (tritium, helium-3, chlorofluoromethanes) (see, for example, Brewer *et al.* 1985). Because the labour involved in constructing an eclectic model is very great, it seemed best to delay further computations until this newer and more useful data set becomes fully available.

Further, experimentation with transient-tracer data suggested strongly that there was a great deal to be learned about how to employ data describing time-evolving tracer fields, where the issues of undersampling in time were clearly much more serious than for fields that on some large scale could be regarded as in a steady state. In particular, the combination of radiocarbon data with the eclectic model, as described in Wunsch (1984*b*), was not obviously extracting the full information content of the radiocarbon data.

A first step towards solving the general problem of making inferences from sparse transient-tracer information was taken in Wunsch (1987*a*). There, it was shown that the process involves solving three related problems: the forward problem, the inverse problem, and what was called the regularization problem. In brief, consider an advection–diffusion equation

$$(\partial C/\partial t)+\boldsymbol{u}\cdot\nabla C-\alpha\nabla^2 C=Q, \tag{1}$$

where α is a mixing coefficient and Q a source or sink of tracer C. The forward problem consists of determining C given $\boldsymbol{u}$, α, Q and appropriate (in the Cauchy–Hadamard sense) initial/boundary data. The inverse problem consists of determining $\boldsymbol{u}$, α and Q given C, including the initial/boundary data. In this second problem, in practice, one must estimate the unknowns when the available data for C are fragmentary and noisy. In Wunsch (1987*a*), it was shown that one was led in solving the inverse problem, to solving (1) in unstable directions, backwards in time and/or upstream; it is a forward problem for an unstable equation and hence is an issue of what in the mathematical literature is called 'regularization' (see, for example, Tikhonov & Arsenin 1977).

In this present paper, we seek to employ the general methods set out in Wunsch (1984*a*, 1987*a*) to study the ventilation rates of the eastern Atlantic. This is a region, the β-triangle area described by Armi & Stommel (1983), of considerable present attention as a testing ground for ideas about the processes and rates governing the renewal of near-surface water masses through exchange with the atmosphere. As a consequence, there exists an unusually complete set of estimates for both geostrophic velocity and of a suite of tracers, both steady and transient. The challenge is to combine these so as to exploit the information content of all of them to estimate ventilation rates, and their relative utility as a guide for future expeditions.

2. The data and the model

2.1. *The data*

Jenkins *et al.* (1985) have reported standard hydrographic variables and the tritium–helium-3 pair (^{3}H and ^{3}He) in the region depicted in figure 1. Potential temperature, salinity, oxygen,

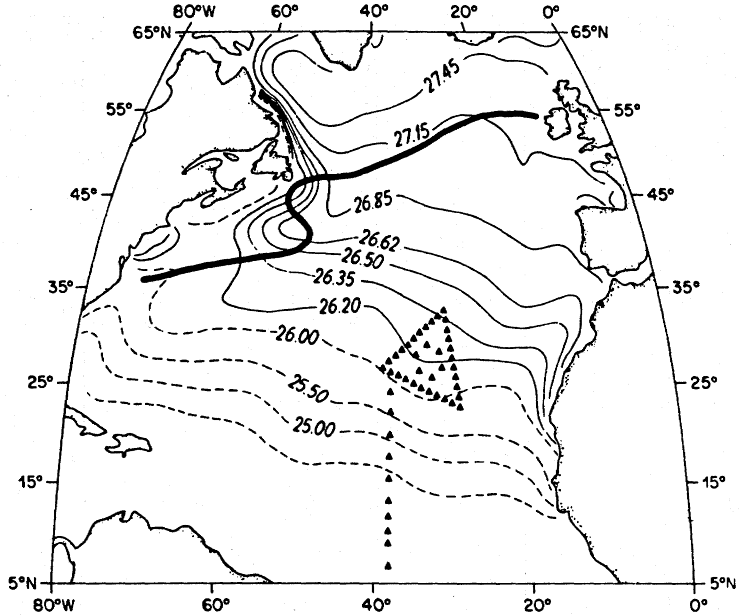

FIGURE 1. Geography of the station data used (taken from Jenkins 1987). The data in the 'β-triangle' stations were used to form a composite meridional section along 38° W as depicted in subsequent figures. The data used combined stations from late 1979 and early 1980.

^{3}H and ^{3}He, and the ^{3}H–^{3}He age are shown in figures 2–6 projected onto the meridional line at 38° W. (I am grateful to Dr Jenkins for making these data available, and for discussions of how best to use them.) The ^{3}H–^{3}He age τ is defined as

$$\tau = \lambda^{-1} \ln\left(1 + \frac{[^3\mathrm{He}]}{[^3\mathrm{H}]}\right), \tag{2}$$

where $\lambda = 0.056\ \mathrm{a}^{-1}$ is the decay constant for tritium into ^{3}He. The square brackets mean 'concentration of'. τ is an *apparent* age since the water was in contact with the atmosphere (where $[^3\mathrm{He}] = 0$). The interpretation of τ is simple only if no mixing occurs (Jenkins 1987).

This meridional section will be the focus of the present study, i.e. we will address the oceanic circulation initially as though it were two-dimensional and as though the data were truly representative of the oceans. The two-dimensional model of the region will be shown to fail, in an interesting way, driving us to a fully three-dimensional one. Nonetheless, the two-dimensional model is sufficiently complex that it is worth examining first. In the spirit of eclectic modelling, we wish to bring to bear as much independent information as possible about the behaviour of the ocean in this region.

A number of direct estimates of velocity in this area exist. They include the β-spiral calculations of Armi & Stommel (1983), the climatological β-spiral estimates of Olbers *et al.* (1985), the inversions along zonal lines at 24° N and 36° N of Wunsch & Grant (1982) (from the IGY data) and of Roemmich & Wunsch (1985) for the modern (1981) sections. An examination of all of these estimates, keeping in mind the questions of representativeness of all of them, leads us to take as an *initial* estimate that the velocity at latitude 30° N (a latitude near the centre of the triangle) is as depicted in figure 7*a*, i.e. a linear function of depth, directed

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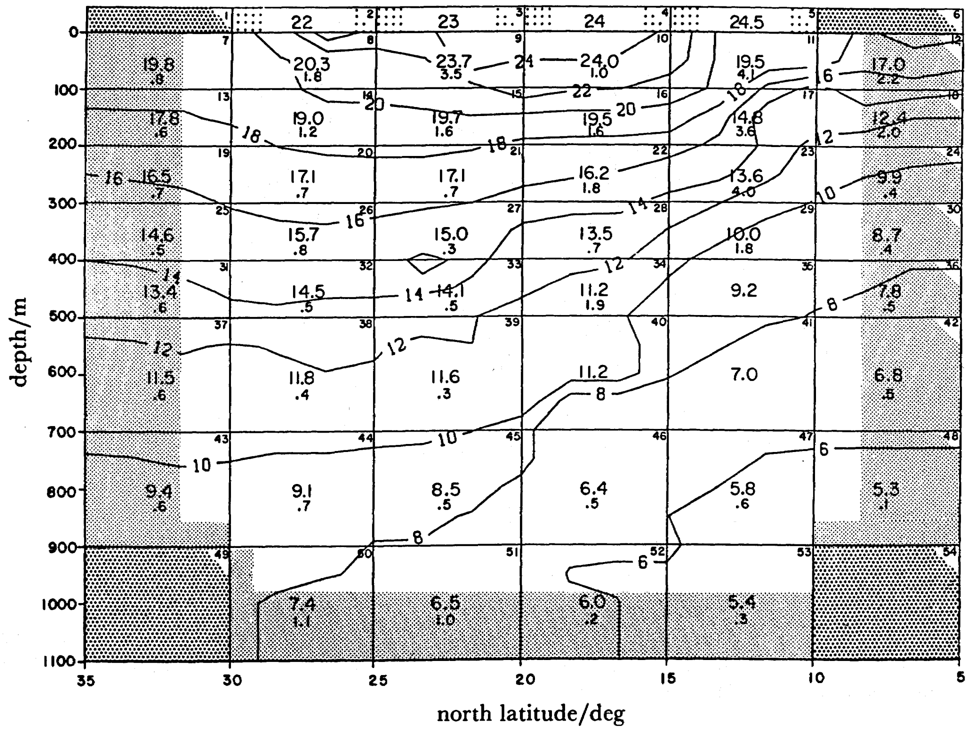

FIGURE 2. Potential-temperature contours along the meridional section. Little smoothing was done by the computer contouring routine (to show the real noisiness of the field). Large numbers in each box are the average temperature assigned to that box for purposes of the model. The smaller number underneath is the estimated standard deviation of the box mean, where the data were adequate to compute one. Values with no standard deviations tend to be interpolations from neighbouring regions. For reference purposes, each box has been given a simple number, in the upper right-hand corner. The boxes fall into five distinct categories. Corners (1, 6, 49, 54) play no role. Stippled boxes (13, 18, ...) are regions used to specify boundary conditions; there are no budgeting constraints applied. The top layer shown (2, 3, ...), is an artifice used to represent the very upper ocean (e.g. an Ekman layer). The thickness of this layer is indeterminate and carries, by way of example, the surface concentration of tritium. Clear boxes are the interior domain of the model for which various balance constraints are applied. The very surface tier, 8–11, is intermediate in that some constraints are imposed there (e.g. salt balance), but others (temperature) are not.

towards the south above 1100 m reaching a value of 1.5 cm s^{-1} at the surface. The formal uncertainty, as a subjective estimate based upon comparison of the above sources, is probably about 10 % at any given depth.

Leetmaa & Bunker (1978) estimated the annual average Ekman pumping rate, w_E, in this area. Crudely scaling their figure 7, we estimate w_E in 5° latitude bands at this longitude as shown in figure 7*b*. It is difficult to estimate the uncertainty of these values. A preliminary guess is ±10 %, to be changed later if necessary.

The boundary conditions for ^{3}H involve the description of the atmospheric transfer as a function of latitude and time. Dreisigacker & Roether (1978) and Weiss *et al.* (1979) have compiled estimates of North Atlantic surface concentration from 1952 to 1972. These are shown as a function of latitude and time in figure 8. Because the tracer surveys were made in the autumn of 1979 and late winter of 1980, the curves of figure 8 were linearly extrapolated, based upon the last two years of observation. These extrapolations brought the apparent surface concentrations of ^{3}H close to zero in 1976 at all latitudes of present concern. No model results are sensitive to this extrapolation.

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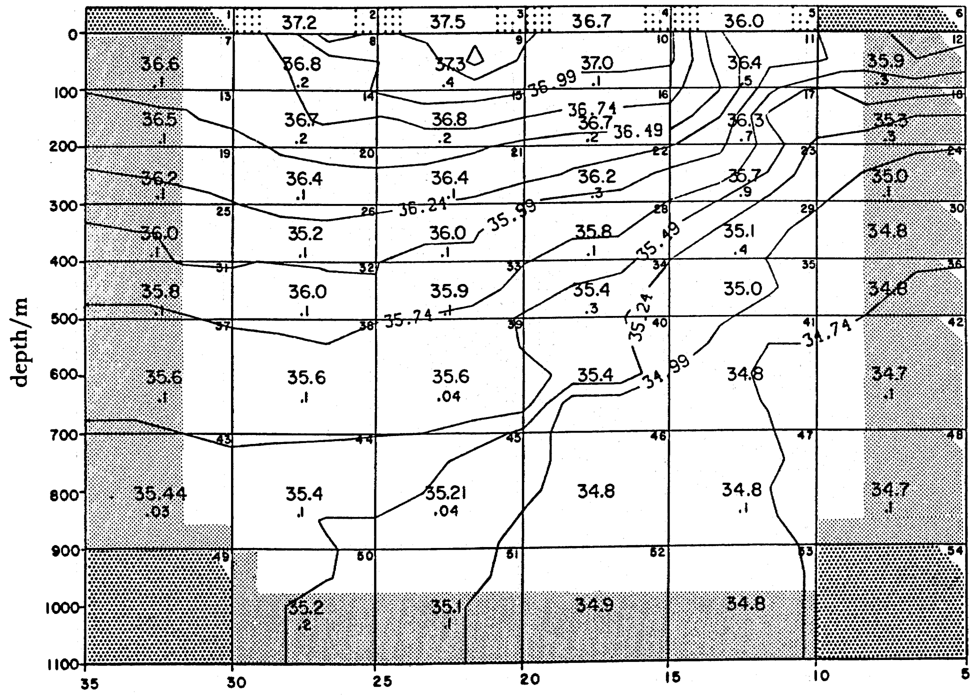

FIGURE 3. As in figure 2, but for salinity.

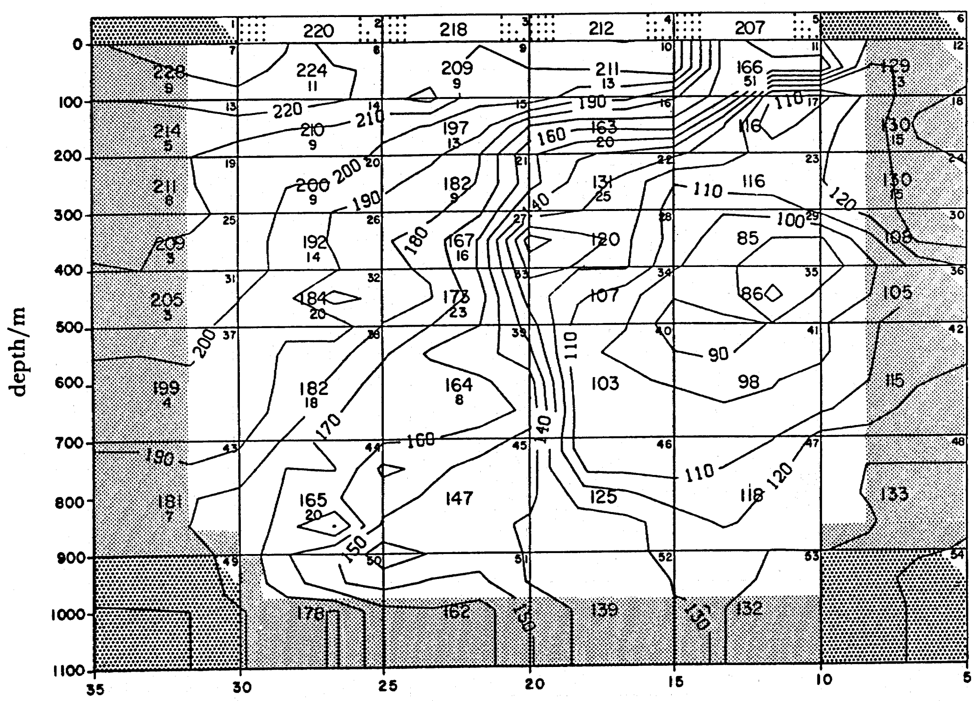

FIGURE 4. As in figure 2, but for oxygen concentration (in units of micromoles per kilogram).

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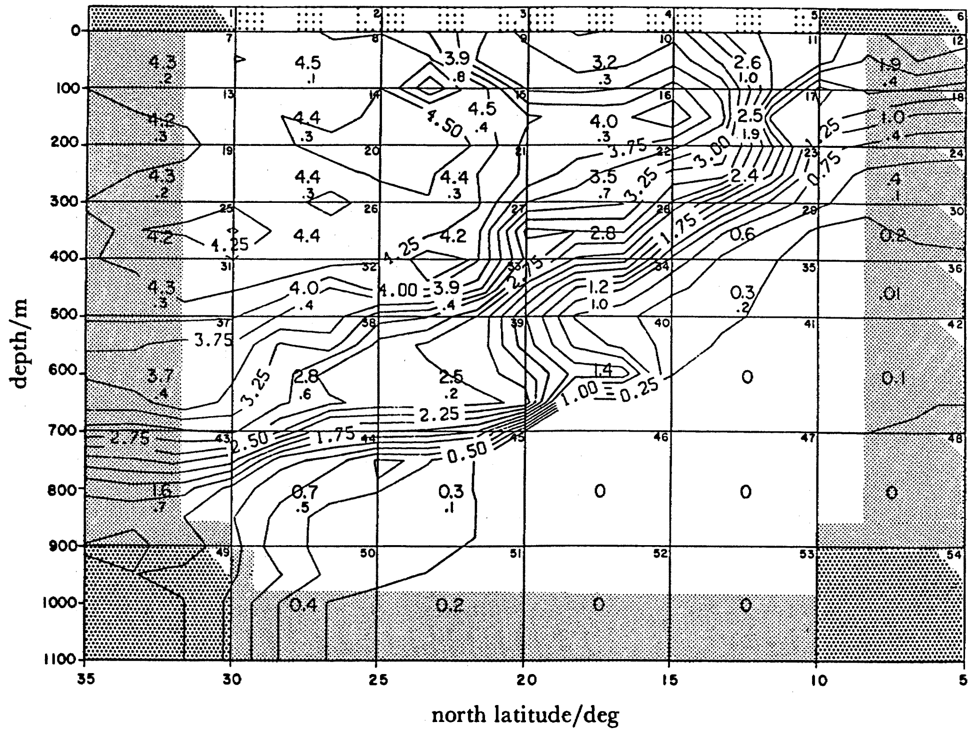

FIGURE 5. As in figure 2, but for the ^{3}H distribution in 1979–1980. Note the very sharp front.

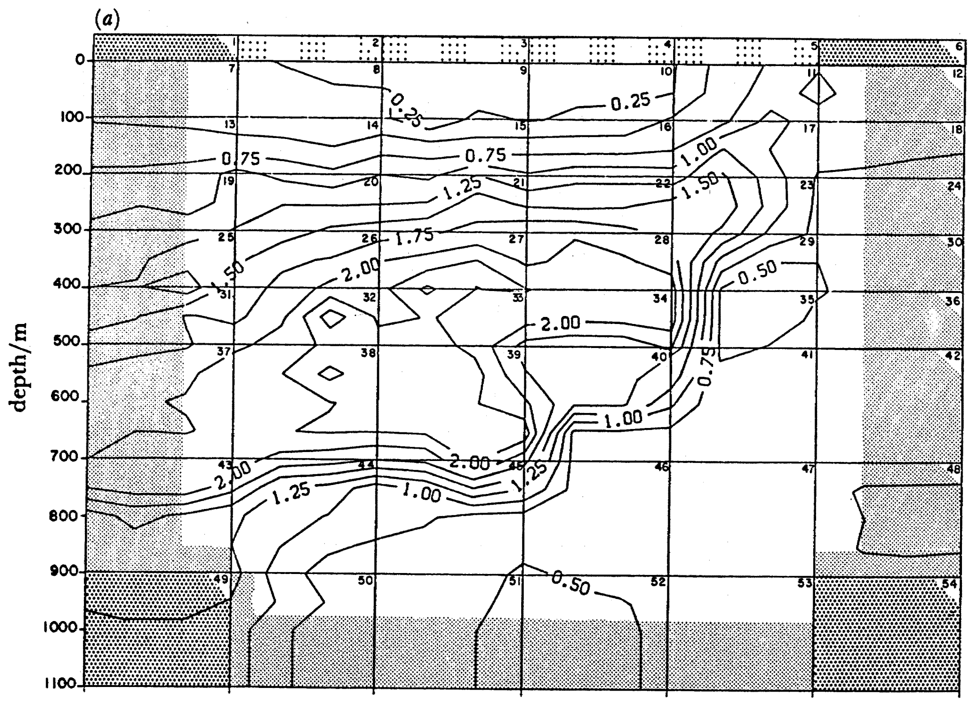

FIGURE 6(a). For legend see facing page.

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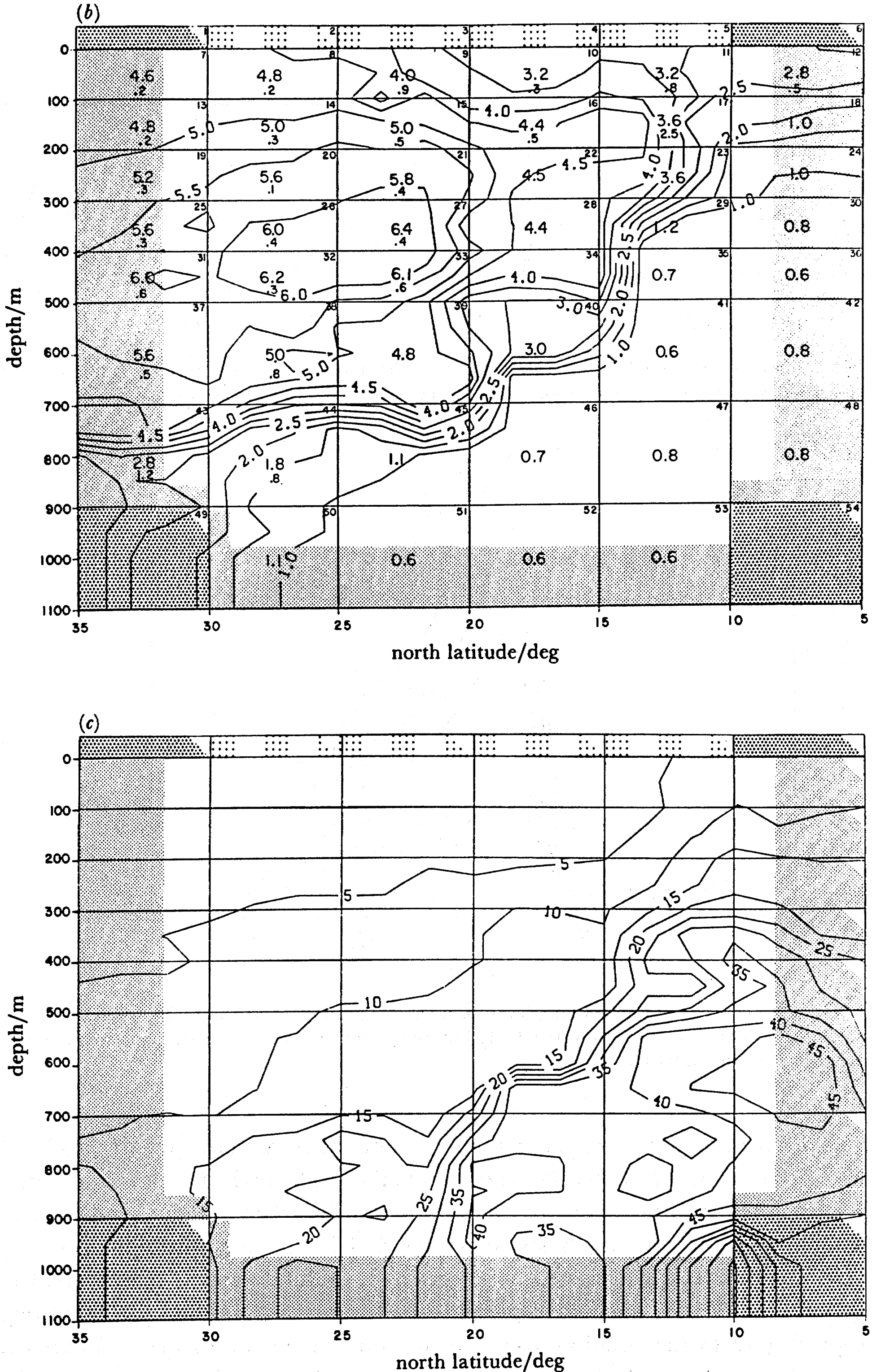

FIGURE 6. (*a*) The estimated ^{3}He distribution in 1979–1980. Box values were not assigned because ^{3}He was used only in the form of stable tritium. (*b*) The stable tritium distribution (see text). (*c*) The ^{3}H–^{3}He age (see text) of the water in the model in 1979–1980.

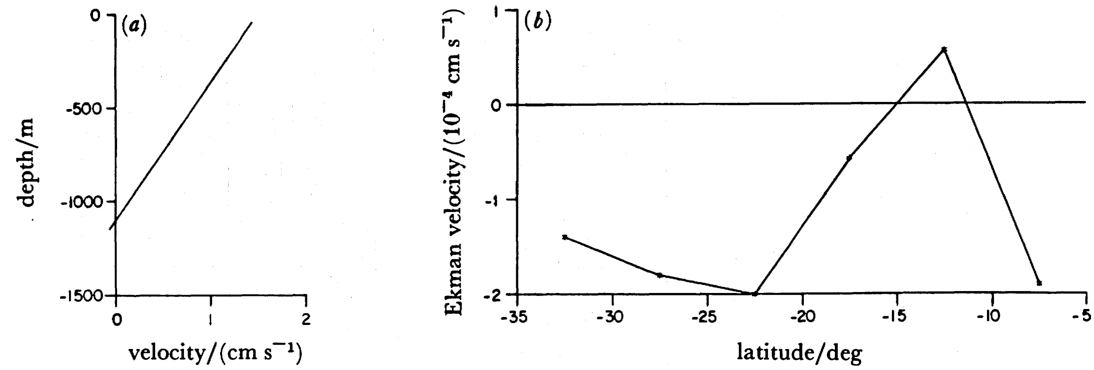

FIGURE 7. (*a*) Estimate of meridional geostrophic velocity at the northern boundary as a function of depth. (*b*) Estimate of Ekman pumping velocity along the top boundary.

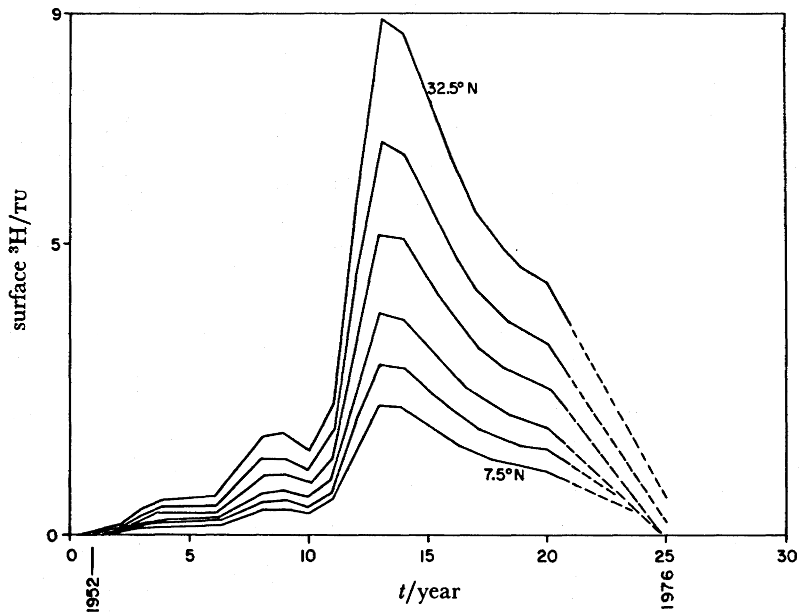

FIGURE 8. Surface tritium concentration as a function of time and latitude as taken from Dreisigacker & Roether (1978). The highest values are at the most northerly latitudes. Broken lines show extrapolation made between the latest published value and 1979.

For ^{3}He, the surface-boundary concentrations may be taken as zero (Jenkins 1987). The annual average surface concentrations of oxygen were taken to be slightly supersaturated, from the observed surface temperatures and the compilation of Broecker & Peng (1982).

The most striking feature of the charts in figures 2–6 is the sharp-front-like outcrop of properties occurring near 15° N discussed by Broecker & Östlund (1979) and others. Cox & Bryan (1984) suggest that the boundary represents the transition from the 'ventilated region' of the sub-tropical gyre to the 'shadow zone' to the south as in the theory of Luyten *et al.* (1983). But note that the front-like feature appears at precisely the latitude where the Ekman velocity estimate of Leetmaa & Bunker (1978) shows a band of Ekman suction rather than the pumping generally characteristic of a sub-tropical gyre. Whether this feature of the wind field is real or not, and if real, whether it is a cause or effect of the property front is indeterminate. We will make a limited effort to understand the extent to which the local wind field controls the tracer properties of the upper ocean in this region.

2.2. *The two-dimensional steady model*

Although the discussion of Wunsch (1988*a*) for the solution of (1) was in terms of a finite difference formulation, we have chosen here to simplify the model even further. Consider the sections in figures 2–6. As displayed, the section was divided into a series of boxes consisting of 5° of latitude and either 100 or 200 m depth intervals. To avoid twice differentiating noisy data we have opted to employ a box-model version of (1). In the chemical-engineering literature such a system is called a 'compartments model' (see Wen & Fan 1975) and is what Keeling & Bolin (1967) described as a 'reservoir model'. Following these latter authors, we abandon the attempt directly to separate advective flows from mixing effects, and simply describe mass transfers between boxes i and j by a single parameter $J_{i,j}$. By choosing to represent the transfer of properties, C, by $C_i J_{i,j}$, we are assuming that this underparametrization of the system will be adequate to describe all the data. We must remain aware that if incompatibilities arise, different properties are surely being mixed at different rates, and it may become necessary to introduce a more complete parametrization. The general philosophy remains as using models as simple as conceivable, until such time as they become in conflict with the data, at which point, more structure is added one step at a time. (Originally the problem was formulated in terms of conventional advection–diffusion as in Wunsch (1988*a*). But then the data reduction is much more complex, involving numerical laplacians, the dimensionality of the mixing field is very high and, in general, there is much more to keep track of. Given the crudity of the data a highly sophisticated form seems unreasonable; cf. the comments by Salmon (1986).)

Depth was used as the vertical coordinate rather than the perhaps more obvious isopycnal one, because it made possible the straightforward employment of the linear vorticity balance described below. Vorticity turns out to be among the more important constraints.

The observed property data, west of longitude 30°, were averaged over the depth range of each box depicted in the figures and this single number was assigned as the particular property value in the box. The variance of each of these averages was also recorded as an estimate of the formal uncertainty where enough data existed in a box to make the calculation useful. Both the box mean and the variances (where they were computable) are depicted in the figures. In addition, a set of surface-layer boxes (of indeterminate thickness), chosen to represent the surface property reservoir was overlain on top of the sections already shown. The obvious uncertainty over the behaviour of the Ekman layer – mixed layer – subduction layer has led us to simply regard the surface properties as defined by a reservoir of unknown physics and vertical exchange through pumping into the box below. The figures display these reservoir values.

2.3. *Conventional tracers*

The traditional tracers of oceanography are temperature, salinity and oxygen. Before grappling with transient tracers, we ask whether the observed fields of these classical tracers can be described consistently. Consider the θ, S and O (potential temperature, salinity and oxygen) fields in figures 2–4. With the time derivative term set equal to zero in (1), the demand is for a flow field J in the box model that satisfies the following constraints for any interior box, labelled i:

$$\text{conservation of mass in the form,} \quad \sum_{j \in N_i} J_{i,j} - \sum_{j \in N_i} J_{j,i} = 0,$$

$$\text{conservation of salt,} \quad \sum_{j \in N_i} S_i J_{i,j} - \sum_{j \in N_i} S_j J_{j,i} = 0,$$

$$\text{conservation of heat,} \quad \sum_{j \in N_i} \theta_i J_{i,j} - \sum \theta_j J_{j,i} = 0,$$

$$\text{conservation of oxygen,} \quad \sum_{j \in N_i} O_i J_{i,j} - \sum O_j J_{j,i} = 0,$$

where $j \in N_i$ denotes the set of indices of the boxes neighbouring box i. All $J_{i,j} \geqslant 0$.

Mass is conserved in each interior box, and for reasons to be made clear presently, an additional statement of mass conservation into the entire interior region was added. Salt conservation was implemented in the same way. Heat conservation was applied only to those boxes below the top two levels.

Finally, oxygen conservation in a steady state was *not* assumed, but a sink term, Q, the oxygen utilization rate (OUR), was introduced for each box as a new set of unknowns. The oxygen boundary conditions were set as slightly supersaturated, as already described.

The final set of relations employed in this initial steady calculation was obtained from the physical estimates of mass flux: the Leetmaa & Bunker (1978) estimates for Ekman pumping of figure 7*b* were imposed at the base of the top layer of boxes. Finally, the geostrophic velocities of figure 7*a* were imposed at the box boundaries at 30° N.

The model was then extended to include the linear vorticity balance

$$\beta v = f \, \partial w / \partial z \tag{3}$$

whose validity in the region is discussed by Armi & Stommel (1983) and Olbers *et al.* (1985). This relation can be applied only approximately. Integrating (3) in the vertical,

$$\beta J'_{i-1,i} = f[J'_{i+m,i} - J'_{i,i-m}], \tag{4}$$

where $J'_{i-1,i}$ represents the net flow into box i through its northern boundary, $J'_{i+m,i}$ the flow from the box below, and $J'_{i,i-m}$ the flow into the box just above. In the top tier of boxes, $J'_{i,i-m}$ was set equal to the Ekman influx from the surface layer; in the northern tier of boxes, $J'_{i-1,i}$ was set equal to the geostrophic flow already described. Each of the J' is of the form

$$J'_{i,j} = J_{i,j} - J_{j,i}, \tag{5}$$

i.e. representing the net flow across the particular boundary into the box.

Wunsch & Roemmich (1985) have questioned the validity of the Sverdrup relation in oceanic modelling, suggesting that evidence for its quantitative applicability was thin. They concluded that Sverdrup balance was unlikely to hold on an Atlantic-wide basis, and that *quantitative* demonstration of a point-wise validity was probably not possible with existing data, but that it could not be disproved either. The present constraint, with w set equal to the Ekman flux, is a demand for approximate Sverdrup balance, and what we are doing is testing it locally. Wunsch & Roemmich (1985) did suggest that at least in the eastern North Atlantic, Sverdrup balance might be plausible in the upper ocean, the chief difficulties coming as one went into the abyss with the bottom-induced w probably becoming dominant. The present data set lies

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in the thermocline and above, and the relation should work here if it applies anywhere. In the present test, no reason was found to reject (4), although that is a very different matter from proving its validity.

None of the constraints just described can be expected to hold exactly. There are errors expected in every one of them arising from observational inaccuracies (e.g. the geostrophic flow), to model incompleteness (e.g. the assumption of steady two-dimensional flow), or both. Thus for every constraint we make an estimate of the error involved to the extent possible. The bounds used are listed in table 1.

TABLE 1

	row constraints	
mass		
each box	0.05×10^9 kg s^{-1}	
overall	0.025×10^9 kg s^{-1}	
salt		
each box	0.15×10^9 kg s^{-1}	
overall	0.1×10^9 kg s^{-1}	
temperature		
each box	0.5 °C $\times 10^4$ kg s^{-1}	not in boxes 7–12 not in three dimensions
oxygen		
production in boxes 8–11		
consumption in boxes below 8–11		
geostrophic velocity		
northern-boundary boxes		10% of nominal (figure 7*a*)
eastern boxes		10% of nominal (figure 12) three dimensions only
Ekman fluxes		
top tier to second tier		10% of nominal (figure 7*b*)
	column constraints	
all fluxes positive		
all fluxes less than about 10×10^9 kg s^{-1}		

The resulting system consisted then of 128 budgeting, linear-vorticity, and northern-boundary geostrophic constraints in 186 unknowns, $J_{i,j}$, subject to the positivity constraints $J_{ij} > 0$ and an overall constraint that the horizontal velocity equivalent of $J_{i,j} \leqslant 6$ cm s^{-1} in the 100 m thick upper layers (3 cm s^{-1} in the deeper, 200 m thick layers). The resulting system, being underdetermined, was solved by linear programming in the same manner as in Wunsch (1984*a*, *b*). Because of the nature of linear programming solutions, there is a tendency to push all interior-box balances to limits of one sign; to prevent this behaviour, strong constraints requiring overall mass and salt conservation were added. The accounting for constraints is necessarily only approximate, because all constraints have inequalities applied, and others, such as the demands on the Ekman fluxes and on the sign of the OUR, have not been counted.

To make a connection with more conventional models, consider the following. We regard the total flux across an interface as the analogue of the dynamical velocity, u

$$\rho A u = J_{ij} + (-J_{ji}), \tag{6}$$

where A is the area of an interface and ρ is the density (recall that all J_{ij} are positive). The net flux, F, of a tracer, C, across that interface is

$$F_{ij} = C_i J_{ij} - C_j J_{ji}. \tag{7}$$

For those who like to think in terms of eddy coefficients for tracer flux, K, the net flux in a conventional tracer conservation equation is

$$F_{ij} = (\rho C u - \rho K\,(\partial C/\partial n))\,A, \tag{8}$$

where $\partial C/\partial n$ is the normal derivative. Equating (7) to (8) and with the definition (6), we obtain

$$K\frac{\partial C}{\partial n} = \frac{(C_j - C_i)}{2\rho A}(J_{ij} + J_{ji}) \tag{9}$$

having put $C = \frac{1}{2}(C_i + C_j)$. 'Mixing' will be large where $C_j \neq C_i$, and advection will dominate where they are comparable. Minimizing $\Sigma_{i,j}(J_{ij}+J_{ji})$ can be thought of as a search for a minimum mixing solution.

There is a mixing within each box, of course, that in our formulation is sub-grid scale. This mixing includes an implicit component owing to the upstream differencing we are using (Roache 1976). The emphasis in this paper is, however, on integral properties (e.g. the oxygen consumption rates), and we will not focus on distinctions between advective and diffusive transports, much of which can be regarded as largely semantic anyway.

When initially run, the system was 'infeasible', i.e. had no solution at all capable of satisfying all the constraints, despite the considerable under-determinancy. The infeasibility was resolved by permitting the net meridional geostrophic flow from box 7 to 8 to be reduced to 0.5 cm s^{-1}. In view of the origins of these estimates, this variation was deemed physically acceptable. A solution to the system is shown in figure 9 for which $\Sigma_{ij} J_{ij}$ was minimized.

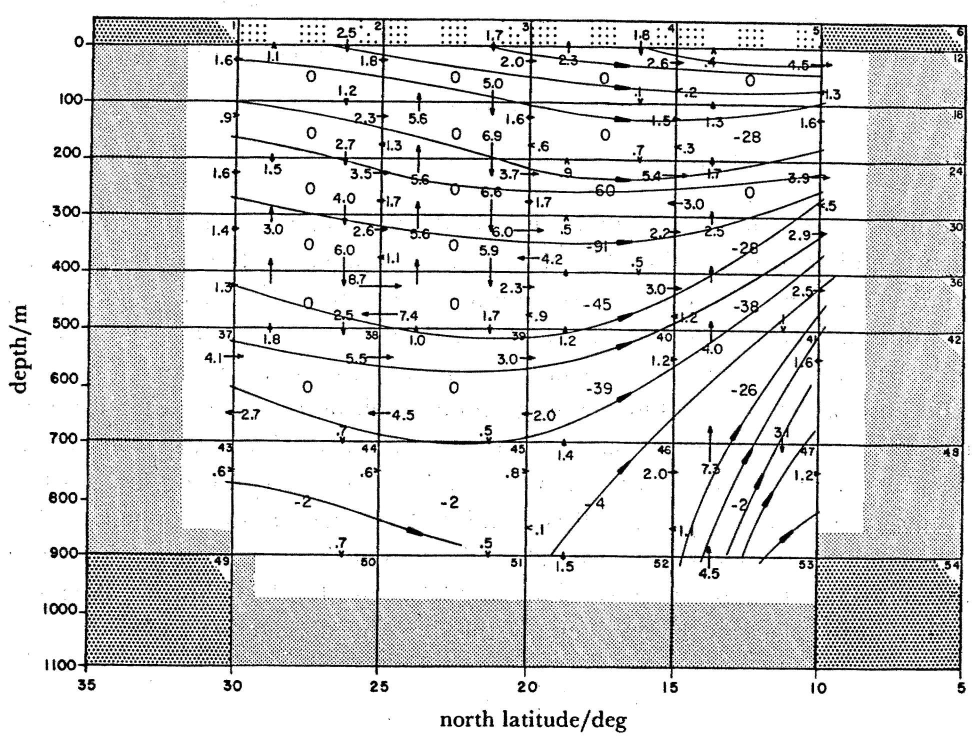

FIGURE 9. Flow in two-dimensional model with objective function minimizing the overall sum of the J and the meridional average OUR as a function of depth. Arrows are labelled with transports in Sverdrups and a schematic interpretation of the net flow (J') across each interface in terms of a stream function is displayed. Contour interval is approximately 1 Sverdrup. Oxygen utilization rate in units of micromoles per kilogram per year is displayed in the centre of each box (negative numbers corresponding to consumption). (1 Sverdrup = 10^6 m^3 s^{-1}.)

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2.4. *First results of the steady model*

To recapitulate, this purely steady model demands consistency with the meridional geostrophic flux at the northern boundary, estimates of the Ekman convergence – divergence at the sea surface, overall mass and salt conservation, mass, heat and salt conservation within the interior boxes, and the linear vorticity balance within each box. For oxygen, it was demanded that there should be a net injection of oxygen into the top tier of boxes, and below that tier, oxygen should only be consumed. Because of various errors of observation, zero oxygen consumption (production) was deemed observationally equivalent to net production (consumption) of up to 0.5 μmol kg^{-1} a^{-1}.

With these constraints as just described, there is no difficulty in finding a solution; indeed, infinitely many solutions exist. Two of them are shown in figures 9 and 10. However, it was quickly realized that all the flows that were found to be consistent with these constraints had very large oxygen consumption below the surface tier, with values reaching higher than 70 μmol kg^{-1} a^{-1} in some boxes, and overall averages below 200 m of 15 μmol kg^{-1} a^{-1}. The values of OUR have been discussed most recently by Jenkins (1987) and Sarmiento *et al.* (1988).

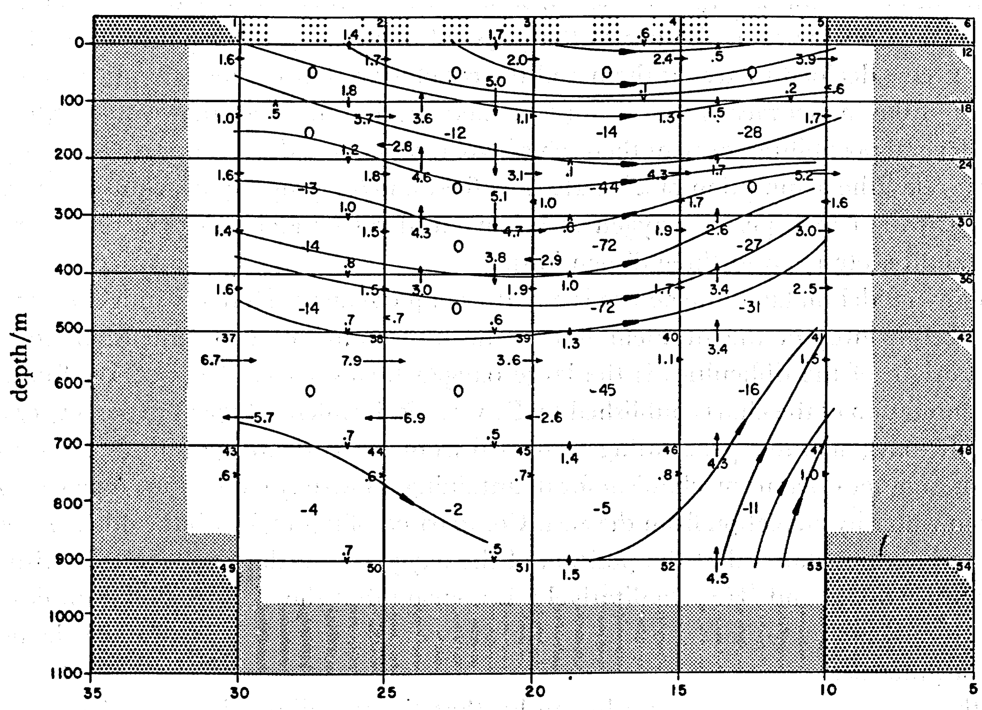

FIGURE 10. As in figure 9, but for a solution in which an attempt was made to minimize the OUR below 200 m.

It would appear that: (a) the actual rates of oxygen consumption (OUR) are very poorly known and, that (b) the biological oceanography community has difficulty in accepting rates even as high as 10 μmol kg^{-1} a^{-1}. Given (a), one might question whether (b) should be given very much weight. However (W. J. Jenkins, personal communication 1987), there do appear to be serious questions as to whether an adequate carbon flux is available to support a very high OUR, and it thus became important to see whether the OUR could be driven down to rates consistent with biological conventional wisdom. This question was explored by making the linear programming objective function (cost function) equal to the average OUR below 200 m and seeking the minimum possible value.

A solution that resulted from the minimum value is depicted in figure 10. The average OUR below 200 m is 14 μmol kg^{-1} a^{-1}. The value of the minimum is unique (up to the constraints imposed), however the flow giving rise to that minimum is not unique. But in some individual boxes the OUR remains over 70 μmol kg^{-1} a^{-1}.

The origins of this very high OUR are unmistakable if figure 10 is compared with the oxygen distribution in figure 4. What is happening is that the geostrophic plus vorticity constraints drive a very strong flow across the oxygen front lying at about 15° N. Thus high-oxygen water is being transported into a region of low oxygen concentration and the only way to maintain the low oxygen concentration is to rapidly consume the incoming flux. Without removing or modifying these dynamical constraints, there is no way to reduce the required consumption.

If we accept the conventional biological wisdom (which says that even Jenkins's (1987) best estimates of 10 μmol kg^{-1} a^{-1} are too high), then the present model must be deemed to have failed. Perhaps the most interesting aspect of the failure is that it only occurs when we couple together the dynamical constraints with a quasi-biological constraint. Either set of constraints by itself would be self consistent. The most troubling question is whether the OUR constraints should be placed on the same footing as the physical ones; for present purposes, with some hesitation, I have decided to accept them and the conclusion that the model must be rejected.

Rejection of the model means that it must be modified in some way that would reduce the high OUR. Several possibilities present themselves: greatly increase the error bars on the physical constraints; relax the assumption of a steady flow field and/or property distribution; increase the parametrization (e.g. permit oxygen to be mixed at rates independent of those for temperature, salinity, etc.); go to three dimensions.

As with any model making, the choice is a matter of judgement. Here my judgement is that the most likely source of the problem lies in the assumption of two dimensionality. The immediate origin of the difficulties is the large oxygen fluxes across the property front. But cursory examination of the charts published by Kawase & Sarmiento (1985) shows a very large tongue of low-oxygen water penetrating westward from the African upwelling region to the east. Here is another obvious mechanism for maintaining a low-oxygen region: replacement of high-oxygen water by advection from the east. Comparison of figure 4 with the other property-distribution diagrams shows that the position of the oxygen front does not coincide with that of either density or ^{3}H and ^{3}He. Qualitatively, it appears that the oxygen minimum lies near the axis of the dynamical front, consistent with the view that low-oxygen water is being fed along it from the east.

It was thus concluded, extremely reluctantly, that the two-dimensional model was indefensible, and had to be replaced by a three-dimensional one. The decision was made reluctantly because the computational load in three dimensions is much greater than in two (especially when time variation is added in §4) and the book-keeping burden grows enormously. Furthermore, the addition of property fluxes in the third dimension increases the parametrization of the model so that the ratio of constraints to unknowns decreases. Nonetheless, the conclusion was that a three-dimensional flow field was inescapable.

3. The three-dimensional steady model

The three-dimensional model was made as simple as seemed plausible while still resolving the OUR contradiction of the two-dimensional flow. The model is shown as depicted schematically in figure 11. The original section is now envisioned as representing a set of three-

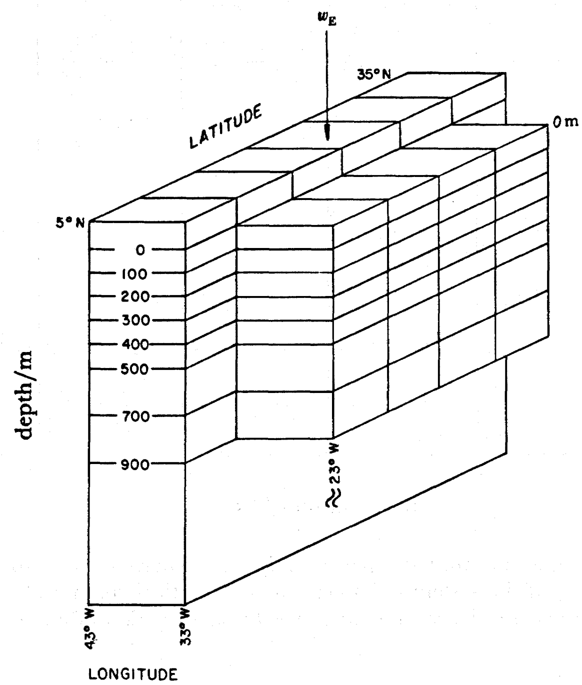

Figure 11. Schematic diagram of the three-dimensional model in which the eastern tier of boxes feeds fluid into the interior boxes to its west at rates determined by the meridional geostrophic shear. A third group of boxes, to the west, is not displayed and serves only as a receiving reservoir for the central tier. No backflow from west to east is permitted.

dimensional boxes sandwiched between the two other such sets of boxes to the east and west. The zonal extent of each of the central boxes was taken as 5° to the east and west of the original section as depicted. The hydrographic data of Jenkins *et al.* (1985) from the region shown in figure 1 were used to estimate the zonal thermal wind relative to the deepest available station pairs as shown in figure 12. A value of the thermal wind was assigned to the zonal flow in each box and was assumed to be the same on both eastern and western boundaries. For each column of boxes, an unknown reference level velocity, b_p, was added to the system. Only flows from east to west were permitted; thus the western tier of boxes is a purely passive recipient of westward flows from the central tier. Divergence in b_p is permitted across the central tier.

The properties assigned to the eastern tier were taken from the charts of Kawase & Sarmiento (1985). These values must be regarded as crude approximations to the true average properties. The constraints of the two-dimensional model were thus modified as follows. Mass, salt and oxygen conservation constraints in each box, and overall, include a zonal flux of properties from the east composed of the known thermal wind plus unknown reference-level velocities. The heat-balance equations were made free (i.e. non-binding) as they are nearly redundant if both mass and salt are conserved. The vorticity constraints are unchanged as (3) does not

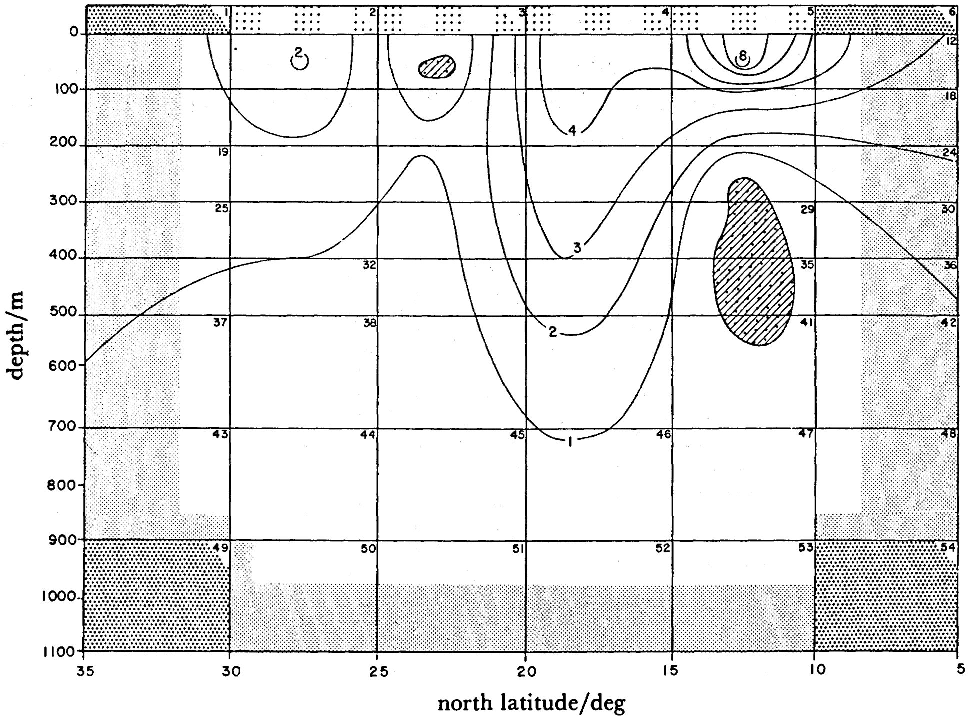

FIGURE 12. Geostrophic flow (in units of centimetres per second) used to drive the east-to-west fluxes. An unknown reference-level velocity is part of the system unknowns in each vertical column of boxes. The reference-level velocity was forced to be sufficiently large so as to drive the small region of eastward flow (stippled) to the west.

involve the zonal flux directly (but the meridional and vertical velocities will be changed from their previous values by the presence of a zonal divergence).

This simplified three-dimensional model nominally contains 181 constraints (each with error bars) in 195 unknowns (all the previous ones plus 8 zonal reference level velocities). When run, it was completely consistent and the problem with the OUR disappeared completely: there are consistent solutions in which there is now no net OUR at depth (within the error bars). No changes had to be made in the northern geostrophic flux; the values in figure 7*a* were used within the stipulated 10% error. This result might be thought of as a lesson in the difficulty of deriving oceanographically useful numbers from models of dimension lower than three.

With this consistent steady-flow model in hand, we now turn temporarily to the transient-tracer problem, returning later to examining the ventilation rates in the eastern North Atlantic.

4. TRANSIENT TRACERS

Consider the original tracer equation (1). If the velocity and mixing coefficients are specified along with the source/sink terms, this equation permits computation of the tracer distribution $C(x, t)$ in any domain if, and only if, appropriate boundary conditions are specified. We could compute the distribution of tracer C in any one of the boxes, i.e. $C_i(t)$ for any time t if all the J_{ij} are known, and if $C_{i0}(t)$ are known in all the boundary boxes, $i_0 \in B$, from which a flow

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occurs. Furthermore, a set of initial conditions $C_i(0)$, all i is required. That is, the reservoir model is

$$\partial C_i/\partial t + \sum_{j\in N_i} C_i J_{i,j} - \sum_{j\in N_i} C_j J_{j,i} - Q_i = 0, i\in I, \tag{10}$$

$$C_{i0}(t) = \bar{C}_{i0}(t),\ i_0\in B,\ C_i(0) = \bar{C}_i(0),\ i\in I,$$

where $i_0 \in B$ is the index of all boundary boxes having a flow into the interior, $i\in I$ is the set of all interior boxes, $j\in N_i$ is the set of all neighbour boxes of box i. The forward problem is well-posed, and conventionally solvable, if and only if the governing evolution equation (10), a full set of boundary value time histories, and initial conditions, are available.

Consider a single transient tracer $C = {}^3H$. At the surface of the models in figures 5 and 11, the boundary history is given approximately by the curves in figure 8. Denote these special surfaces boxes as $i\in B_S$, and the remaining boundary boxes as $i\in B_I$. These other boundaries, $i\in B_I$, north, south, east and bottom, are in the open ocean. Apart from a few isolated observations, taken up below, our only available observations are the values on $i\in B_I$ in 1979–1980, plus a set of interior observations, $i\in I$, in that same year. We may reasonably suppose that the initial conditions $C_i(0) = 0$ everywhere, where zero concentration was certainly appropriate before 1950. Whether it was still an appropriate initial condition as late as 1963 (say) is less obvious.

Here is the crux of the transient-tracer problem as defined by a data set like the present one. Even under the drastic and sweeping assumption that all of the $\boldsymbol{J}$ are steady and known exactly, the absence of the open-ocean boundary data (plus the uncertainty in the sea-surface boundary conditions) means that the forward problem is not fully posed and the extent to which inference could be made about the $\boldsymbol{J}$ (which after all is our real goal) from ^{3}H is not clear at all.

We now formalize the problem under the assumption that the $\boldsymbol{J}$ are known. Discretize in time as well as space, and write the system as

$$C_i(n+1) = \left[1-\lambda\Delta t - \sum_{j\in N_i} J_{i,j}\frac{\Delta t}{V_i}\right] C_i(n) + \sum_{j\in N_i} J_{j,i}\frac{C_i(n)\,\Delta t}{V_i}$$

or

$$\boldsymbol{C}(n+1) = \boldsymbol{A}\boldsymbol{C}(n) + \boldsymbol{Bq}(n),\quad t = n\Delta t,\ t_f = n_f\Delta t, \tag{11}$$

where Δt is the time step, λ is the decay constant (1/17.7 a) for ^{3}H, V_i is the volume of box i,

$$A_{ii} = \left[1-\lambda\Delta t - \sum_{j\in N_i} J_{i,j}\frac{\Delta t}{V_i}\right],\ i\in I,$$

$$A_{ij} = \left[J_{ji}\frac{\Delta t}{V_i}\right],\ j\in N_i,$$

$$\{\boldsymbol{Bq}\}_i = \bar{C}_i(n),\ i\in B_I \text{ or } B_S.$$

The matrix $\boldsymbol{A}$, when multiplied by the tracer distribution at time n, gives the distribution one time step later if the boundary values vanish (I will ignore the distinction between n and t). The quantity $\boldsymbol{Bq}$ represents the boundary conditions of this problem; this separation of boundary and interior components is made for convenience.

Then, subject to the evolution equations (11), we must find the time histories $\boldsymbol{C}(n)$ for the interior boxes, subject to partial initial/boundary data. In the classification system of Wunsch (1988*b*), this problem is that of 'state estimation'; it is *not* an inverse problem.

By writing the problem in the form (7) we have emphasized a point taken up at length elsewhere (Wunsch 1988*b*), that the transient-tracer problem coincides mathematically with the problem of control of distributed systems, in which the missing data correspond to the need to find a 'control' function. Many of the results of control theory become available for interpreting the present problem. In particular, the tracer observations at time t_f, corresponding to 1979, represent what is called a 'terminal constraint': the time evolution of the system from the initial conditions, under the 'control', or boundary conditions, must result in the tracer concentrations observed at time t_f subject to various demands on the system state $\boldsymbol{C}(n)$ and on the control functions. The formal control problem proceeds by establishing a cost or objective function in general form (see, for example, Luenberger 1979)

$$H = \psi(\boldsymbol{C}_{t_f}) + \sum_{t=0}^{t_f} L[\boldsymbol{C}, \boldsymbol{q}], \tag{12}$$

where L is any function of $\boldsymbol{q}$ and $\boldsymbol{C}$, often quadratic, and ψ is the terminal constraint demand. General discussion of this system is taken up in Wunsch (1987*b*); here the discussion is confined to specifics.

The formal unknowns of this problem are $\boldsymbol{C}(t)$ and $\boldsymbol{q}(t)$ $0 \leqslant t \leqslant t_f$; the solution is rendered unique by driving the cost function to an extreme (as was done above for the steady problem). In the discussion in Wunsch (1988*a*) the problem was formulated by writing the cost function as (typically)

$$\min: H = \sum_{i \in B} \sum_{n=0}^{n_f} q_i^2(n)$$

or

$$\min: H = \sum_{i \in B} \sum_{n} |q_i(n+1) - q_i(n)|.$$

In Wunsch (1988*a*) the entire system of (11) was solved explicitly in the whole time-space domain for all the unknowns at once by using linear programming. This procedure is practical for the one-dimensional (diffusive pipe flow) situation described there. In three dimensions the system size is unwieldly. More troublesome is the huge disparity between the number of calculations required to solve the model and the very small number of actual observations to which we can compare the result. It seems unreasonable that thousands of separate numerical operations must be carried out to test the consistency of a handful of tracer observations from 1979–1980.

We are led to try and reduce the number of degrees of freedom in the model to be more consistent with the number of testable observations.

4.1. *Stability and system size*

The first problem is determining the size of the time step Δt. This value is governed by the stability of the time evolution of (11). The criterion can be derived on simple physical grounds as the demand that in any time step of size Δt, that the tracer concentration in none of the boxes become negative. Examination shows that this requirement in turn demands that the elements of $\boldsymbol{A}$ should be non-negative:

$$1 - \lambda\Delta t - \sum_{j \in N_i} J_{i,j} \frac{\Delta t}{V_i} > 0, \quad \text{or} \quad \Delta t < 1 \Big/ \left(\lambda + \sum_{j \in N_i} J_{i,j} \frac{1}{V_i}\right). \tag{13}$$

This criterion determines the computational load. For values of J_{ij} found in part 2 above, we find that Δt normally must be less than about one month. If (13) is satisfied, the system (11) is one of non-negative system matrix $\boldsymbol{A}$, permitting the application of powerful theorems (Luenberger 1979) that describe the system behaviour; these will not be pursued here. The paradox of transient tracers is that we have no confidence at all in details of the input on monthly time scales, but it appears that we must nonetheless specify $\boldsymbol{q}$ at monthly intervals and time step the system repeatedly to arrive at the observations in 1979.

To reduce the system size the following procedure was adopted. Let the tracer concentration in boundary box i_0 be unity at the time $t = 0$ for one physical year and zero thereafter. We then compute the evolution of the tracer through the system for all times $0 \leqslant t \leqslant t_f$. Call the result $g_{i_0}(n)$ and make the computation for all boundary boxes. The computation must be carried out for Δt chosen in accord with (11); but the results of the calculation were then averaged year by year to yield the set $\bar{g}_{i_0}(t = n\Delta t') \equiv G_{i_0}(n)$ where $\Delta t' = 1$ year. Two examples are shown in figure 13.

Given G_{i_0} (which depends directly upon the J_{ij}), the general solution of time t_f can be written as

$$C(t_f) = \sum_{i_0 \in B} \sum_{n'=0}^{n_f} \alpha_{i_0}(n')\, G_{i_0}(n-n'); \tag{14}$$

g_{i_0} or G_{i_0} will be recognized as a special form of Green's function; it is related to the solution of the adjoint of system (11) and is the key to system sensitivity testing. The α_{i_0} are the concentration time histories in each boundary box and are the new unknowns.

We now have reduced the formal unknowns of the problem from the complete internal history, $C(n)$ plus boundary history $\boldsymbol{q}(n)$, to just the time histories of the boundary boxes alone. Computation of the G_{i_0} for the present problem suggested that the system tends to flush out all significant memory of previous states in less than about 15 years. If we take 15 years as the duration of the computation and permit the boundary concentrations to vary independently each year and independently in each boundary box, there are 15 years* (18+28 boundary boxes) = 690 unknowns plus the boundary values in the sea-surface boxes (which are partly uncertain). This number accounts for the fluid being fed from the eastern boxes in the full three dimensional calculation. Because the tritium data are represented by 28 interior values in 1979, it still seems unreasonable to have to work with so many unknowns and only 28 constraints. The problem was therefore further reduced in degrees of freedom by writing the concentration time history in each boundary box as

$$C_{i_0}(t) \equiv \alpha_{i_0}(t) = \gamma_{i_0,1} f_1(t) + \gamma_{i_0,2} f_2(t) + \gamma_{i_0,3} f_3(t), \quad t = n\Delta t', \tag{15}$$

with the coefficients γ_{i_0} as constants. For tritium this form still demands 3*(18+28) = 138 unknowns. The three f_i can be chosen as any convenient set of functions. Here they were the coefficients of the Taylor series expansion:

$$f_1(t) = t, \quad f_2(t) = \tfrac{1}{2}t^2, \quad f_3(t) = \tfrac{1}{6}t^3. \tag{16}$$

This reduced form is known as 'parametric control' (see, for example, Stengel 1986) and it clearly restricts the range of optimization of the cost function.

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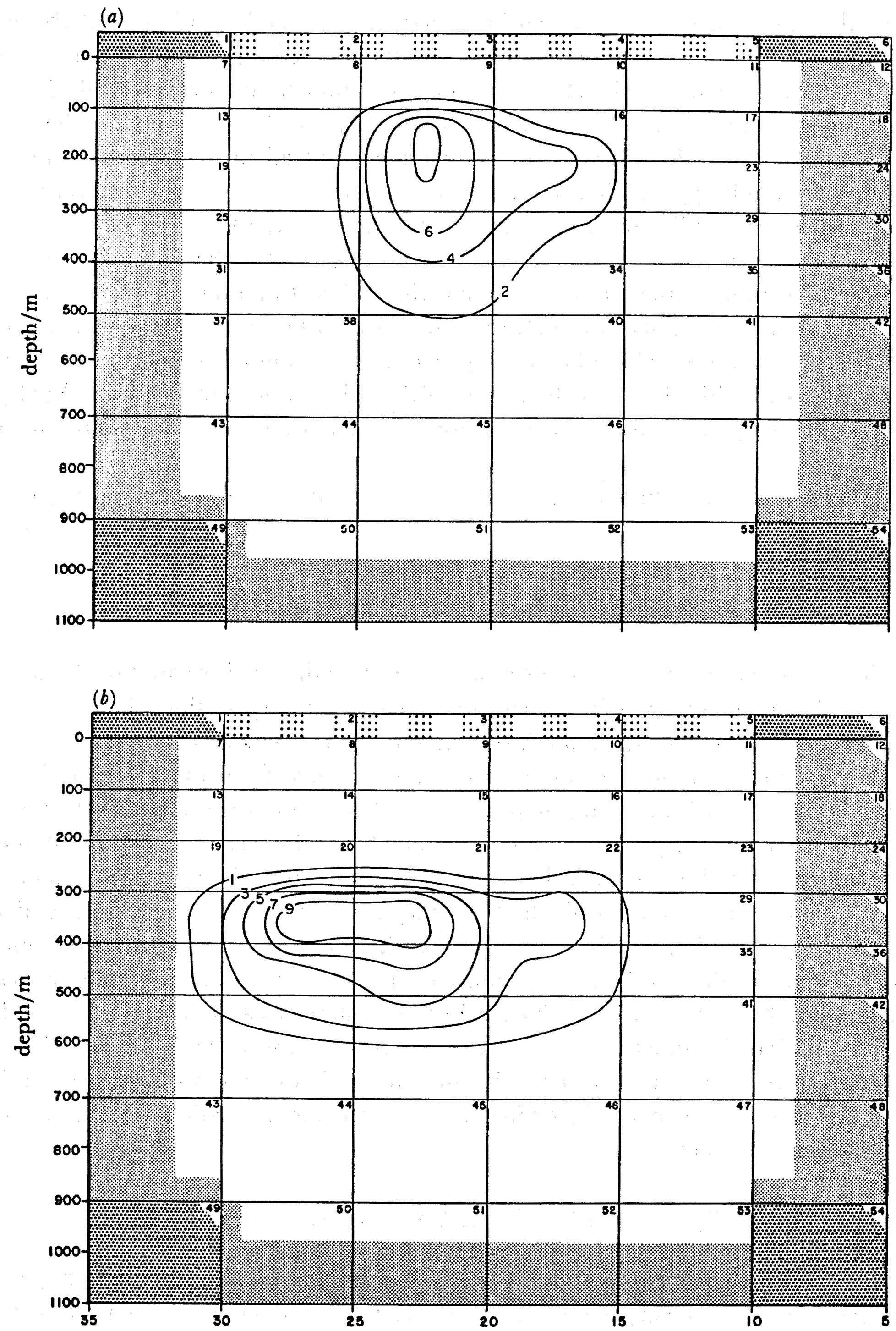

FIGURE 13. Green's function showing the tracer concentration that results when unit value is introduced into boundary boxes 3 (figure 13*a*) and 25 (figure 13*b*) after 5 years, for a given flow field (see figure 16).

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In terms of (11), the vector

$$\boldsymbol{q}(t) = \begin{bmatrix} f_1 \\ f_2 \\ f_3 \end{bmatrix},$$

and the matrix $\boldsymbol{B}$ has non-zero rows for $\mathrm{i}_0 \in B$, consisting of $[\gamma_{\mathrm{i}_0,1}, \gamma_{\mathrm{i}_0,2}, \gamma_{\mathrm{i}_0,3}]$ plus appropriate zeroes.

What should be apparent at this stage would be the very great underdeterminacy, if the transient-tracer inverse problem were being posed. Discussion of the inverse calculation is deferred to the end, but it is worth making two points at this stage. The transient-tracer equations (11) place demands on the flow field $\boldsymbol{J}$ only if the control variables can be specified, and if the time-derivative terms can be estimated on timescales consistent with equation accuracy and stability requirements. Neither of these requirements is trivial.

4.2. *Transient solution*

In the present case, we chose as objective function the sum of the time history coefficients

$$H = \sum_{\mathrm{i}_0 \in B} (\gamma_{\mathrm{i}_0,1} + \gamma_{\mathrm{i}_0,2} + \gamma_{\mathrm{i}_0,3}), \tag{17}$$

and solved the system (14)–(17) by linear programming, which remains convenient because of the non-negativity requirement for tracer distributions.

^{3}He was included in the system by forming 'stable tritium' ST $= {}^3\mathrm{H} + {}^3\mathrm{He}$, a conservative quantity (with ^{3}He expressed in ^{3}H units). Appropriate $\boldsymbol{G}_{\mathrm{i}_0}$ were computed for ST (its decay constant λ vanishes in (11)), where the surface boundary conditions for ^{3}He are vanishing concentration so that the ST surface boundary conditions are the same as for tritium. A requirement ST $\geqslant {}^3$H is also demanded in each boundary box, and the equivalent of (14) is imposed with the stable tritium concentration appearing on the left. The entire concatenated tritium plus stable tritium system was solved simultaneously.

The unknowns of this system are now the coefficients of the three expansion functions in each of the boundary boxes, one set each for ^{3}H and ST. The nominal knowns are the observed [^{3}H], [ST] in 1979, including the boundary values themselves. We anticipate, should an infeasibility appear, that we would need to choose between permitting more structure in the boundary time histories, and modifying the transport field $\boldsymbol{J}$. We obviously have much scope for accommodating the available data.

For the steady-flow field depicted in figure 9 (a minimum mass flux solution), a slight infeasibility *did* occur: it took the form of an inability to raise the ^{3}H and ST concentrations above zero in box 29. Because the concentration in that box is indeed very low, the infeasibility was most simply accommodated by increasing the error bar very slightly to permit zero there. The system was then entirely feasible and for this fixed-flow field and terminal constraint, it became possible to explore the different boundary time histories that are consistent. Two of them are shown in figure 14, for which the cost function (17) was a minimum, and the other a maximum. The isotopic age τ may be expressed as

$$\tau = \lambda^{-1} \ln([\mathrm{ST}]/[^3\mathrm{H}]) \tag{18}$$

and is displayed in figure 14 for these two time histories.

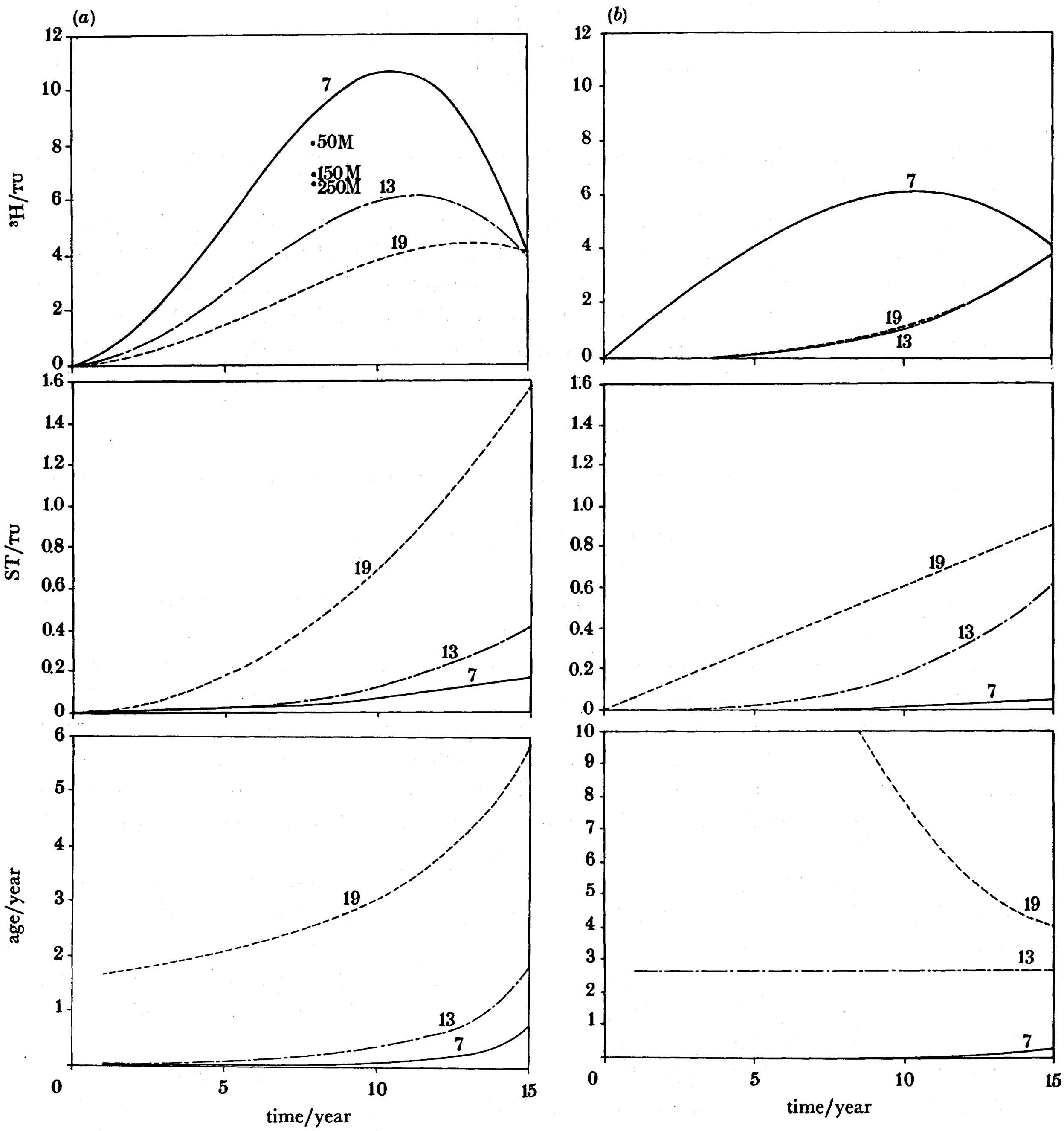

FIGURE 14. (*a*) Time histories in three of the northern-boundary boxes that lead to the final, observed ^{3}H and ST distributions. Box number is shown in the figure. Values determined by Östlund (1984) for 1972 are shown for ^{3}H in upper ocean (top three boxes). (*b*) Another time history in the northern-boundary boxes also leading to the final tracer distributions. The flow field was the same as in (*a*), yet despite the greatly restricted forms of these time histories, they remain underdetermined and put little restriction on the interior flow fields.

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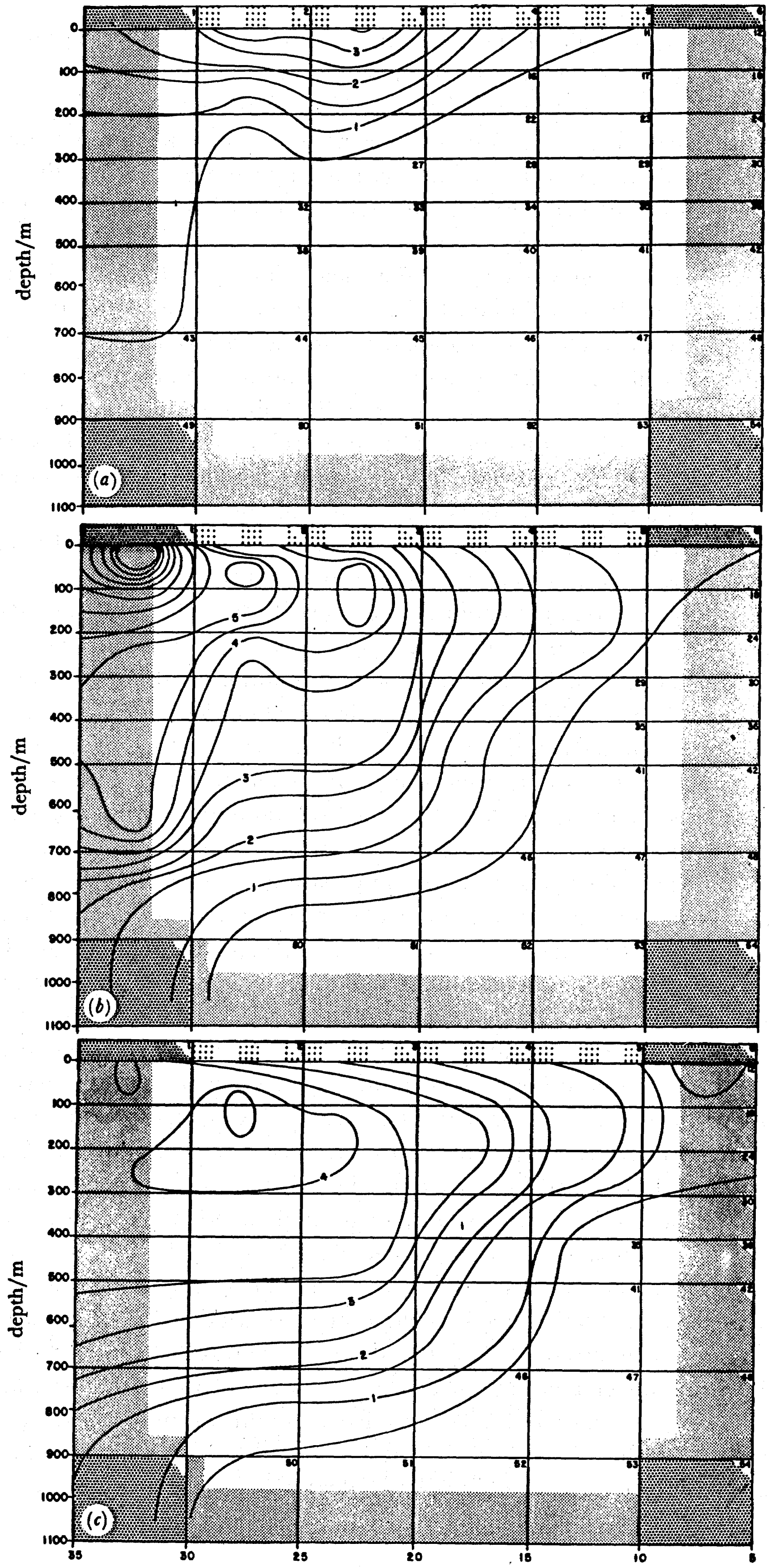

FIGURE 15. Interior distribution of ^3H corresponding to figure 14*a* and the flow field of figure 16, for three years (*a*) year 3, (*b*) year 12, (*c*) year 15, including the final year (1979). The initial interior distribution at $t = 0$ was taken to be zero for purposes of this calculation. If non-zero, it would have flushed through before the end. Contours in tritium units.

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The actual interior ^{3}H distribution is displayed in figure 15 after 3, 12 and 15 years, the latter corresponding, within error bars, with the terminal constraints.

The role of the pair of transient tracers ^{3}H and ^{3}He is thus seen to be, in this particular problem, a rather weak consistency check on the flow determination previously made from the steady system. Not only does the available information only weakly constrain the $\boldsymbol{J}$, but two rather different time histories (there are infinitely many possible) in the boundary boxes are consistent with the terminal-tracer constraints.

About the only information we gained from the calculation apart from the demonstration of consistency, was that the ^{3}H distribution had to have peaked before 1979 at the northern boundary, because when a completely monotonic rise to the observed values was forced, the system was infeasible. The histories calculated suggest that the maximum should have occurred between about 1969 and 1974, depending upon depth. But this conclusion should not be taken as a very strong one: the width and location of the peak is largely determined by restrictions on the most negative value of the cubic term in (15). More terms in (15) and fewer restrictions on the coefficients would almost surely permit later, sharper maxima.

A certain amount of transient-tracer data in the β-triangle region exists for the time period before 1979. One can ask whether this extra information would make a qualitative change in the degree of constraint placed on the solutions. Figure 14*a* displays the values in the boundary boxes in 1972 (year 8) as inferred from the data reported by Östlund (1984). The 50 m value in 1972 is lower than that calculated from the reference solution, and the values below 50 m are a good deal higher. The question of whether forcing the boundary boxes to these values in 1972 would place strong demands on the system is easily answered (strictly speaking, the 1972 values could have been ascribed to the interior column underneath box 8 (latitude 27.5° N), which is closer to the nominal latitude (30° N) of the Östlund data. But forcing the boundary conditions, with only three parameters governing each box, is much more demanding than forcing the interior boxes, which respond to many parameters distributed over the entire system). The set of constraints that $\boldsymbol{C}(8)$ should equal the Östlund values (only three of which are shown in figure 14*a*) was added to the system (14) and then re-solved. These new observations were easily accommodated, without any difficulty, and thus the interior flow field remains essentially unconstrained.

The Östlund (1984) data also include values that could be assigned either to the southernmost column, or to the southern-boundary conditions. Assignment to the southern boundary adds no information because for the particular flow field being considered, there is no northward transfer into the interior. If the values are assigned to the column at 12.5° N, the necessary extra ^{3}H is easily supplied from the east (the values of the Östlund observations are somewhat higher than shown in figure 15) and again no demand is made on the flow field.

The calculated time histories of the isotopic age are intriguing. If the histories displayed (figure 14) are examined, they suggest a complex variation in the exposures of the waters entering the boxes over time. Should one be able to place restrictions on the age, it holds out hope of writing stronger demands on the $\boldsymbol{J}$ field. However, the water feeding into the box over a decadal period presumably emanates from a variety of 'sources' originating over much of the ocean. Second, the age calculation is very sensitive to slight variations in [^{3}He] at small concentrations and the significance of the apparent source variations seen in figure 14 is unclear, as the water was surely exposed to the surface at different times in the tritium transient and at different locations. Furthermore, as Jenkins (1987) shows, the interpretation of the age is far from straightforward as soon as mixing processes are admitted into the equations.

In what follows then, we return to the steady-box calculations for estimates of ventilation rates, regarding the transient calculation as a check to be made at the very end. It will be seen that the consistency check is not wholly trivial. Some of the more extreme cases generated below will be rejected as inconsistent with the transient tracers. But the point remains that the necessity of regarding the boundary conditions as a part of the problem unknowns weakens the constraining power of the transients.

5. Rates of ventilation

We have show that, in general, we can expect the ^{3}H and ^{3}He distributions to be consistent with any reasonable flow distribution deduced from the steady-tracer distributions (although actual consistency should be checked in each case). We therefore return to the steady box model of §3 and explore the so-called ventilation of this part of the ocean.

'Ventilation' is a term that seems to be more or less synonymous with 'renewal' as used for example by Dietrich *et al.* (1980). More recently, the word has come into prominence through the title of the paper by Luyten *et al.* (1983) in which the context is the rate at which water pumped down from the surface by Ekman-layer convergence reaches the thermocline. This latter process is a special one that is probably best labelled 'Ekman ventilation', to distinguish it from, for example, water being forced downward through buoyancy (convective) effects, perhaps to be called 'convective ventilation'. Almost certainly there are other processes of importance (e.g. small-scale mixing) which also 'ventilate' the interior ocean.

In the present case, the initial motivation had been to focus the question specifically on the rate at which oxygen was being consumed and replaced. But as we have already seen, once the model is carried into three dimensions, OUR can vary between approximately zero, and biologically unreasonable values. In effect, we learn that improving estimates of OUR will depend upon physical constraints that have yet to be measured, or preferably, from a physical oceanographer's point of view, from fundamental biochemical measurements. If the latter were a reasonable prospect, then oxygen distributions could be used to help determine the circulation, rather than vice versa.

For the present, we will define ventilation as the rates at which water moves across the boundaries from outside the volume, with a focus on the northern, and near-surface boundaries.

5.1. *A reference solution*

As a reference state, we minimize the overall fluxes within the region, i.e. we minimize

$$H = \sum_{\text{all } i,j} J_{i,j}$$

in the meridional plane. This solution can be thought of as being the one of maximum simplicity, although this identification is only an analogy and is not rigorous, or as a search for minimum mixing as defined above. A solution that minimized this H (it is not unique) is shown in figure 16*a* in the y–z plane. There are some instructive elements of the solution. Overall, the flow is being forced directly by the geostrophic fluxes from the north; no 'backflow' from the

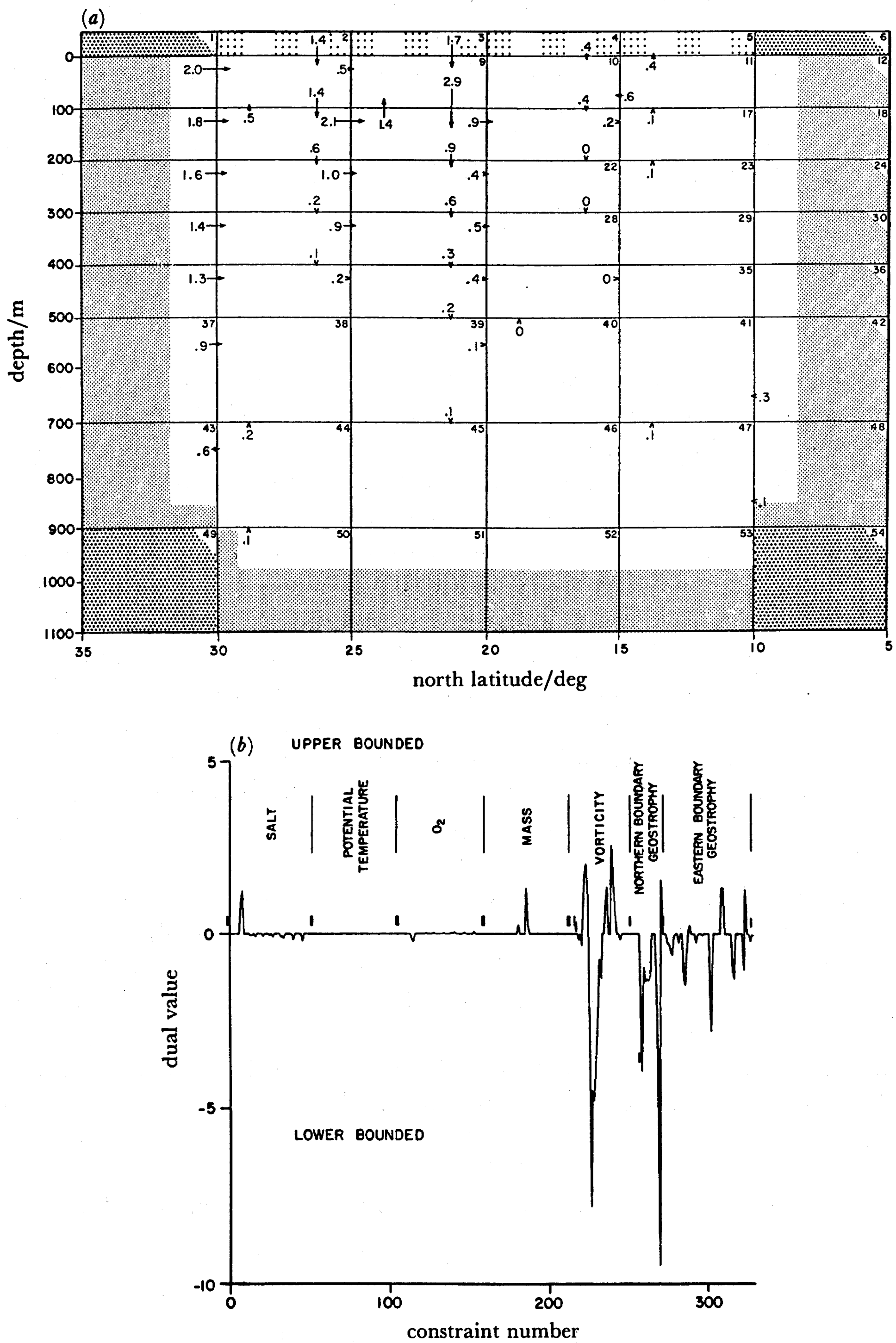

FIGURE 16. (*a*) A reference solution, with minimum $\Sigma_{i,j} J_{ij}$, for which the tracer distributions in figures 14 and 15 were computed. (*b*) Value of the dual to the solution of (*a*). Positive values denote constraints at their upper limits, negative at the lower limits.

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interior boxes to the northern boundary is required. Injection of fluid from the Ekman boxes is also unidirectional and downward, apart from the flow into box 5 demanded by the Ekman suction there. There is little or no flow across the property front, with much of the flow having been ejected to the west before reaching the strong-gradient region. A quite strong flow along the front (6 Sverdrups) is demanded in box 11 with a strong zonal divergence. The high OUR in the two-dimensional flow is thus reduced through the physical divergence now permitted.

The dual solution found by the linear-programming procedure (Simplex method) provides at least a partial answer to the question of which of the constraints were important in determining the value of the objective function. The dual for this particular solution is shown in figure 16*b*. Each equation in the system has a non-zero dual if the constraint is 'binding', i.e. if the particular balance has been forced to either its upper or lower limit. In figure 16*b*, positive values correspond to box balances at their upper limits, negative values to those at their lower limits. The value of the dual is the sensitivity of the objective function to perturbation in the bound, and can be interpreted as a measure of the relative importance of the particular balance. Non-binding, 'basic', constraints have zero dual value. The dual can be interpreted as the Lagrange multipliers of the system.

For the solution in figure 16*a*, the most important factors in setting the value of the minimum of H are, first, mass balance, led by that for box 21, followed by the geostrophic velocities at the northern boundary and the vorticity balances. Increases or decreases in these binding equalities would make a much larger change in the particular solution than would for example, a modification of the salt balances.

Although each of the solutions we will display has a different dual, the general characterization given by figure 16*b* seems universal; the quantities that really determine the fluxes in the system are geostrophy, mass balance, and the linear-vorticity balances.

5.3. *Ventilation extremes*

We can now explore the limits, within this model, of movement of water in and out. The first solution to be compared to figure 16 is the one shown in figure 17 in which we sought to maximize and in figure 18, to minimize the downward mass flux from the surface layer into the layer below (that is from boxes 8–11 into boxes 14–17, remembering that the top tier of boxes is an artefact). One might crudely view these limits as those reflecting buoyancy injection of fluid into the interior. Whether the Ekman flow, imposed at the tops of these boxes penetrates through them vertically, or becomes deflected to the adjacent box on the north or south, is determined by the model itself. This approach was taken because the correct driven depth of Ekman flux is unknown. The flow in figure 17 was just feasible when tested against the [^{3}H]/[ST] terminal distribution. A more extreme solution, when the vertical J_{ij} were permitted to grow to 10 Sverdrups (*ca.* 10^{-3} cm s^{-1}) was seriously inconsistent with the ^{3}He data. In that solution (which is not displayed), so much low ^{3}He water was carried into the interior that the system was unable to supply enough high ^{3}He water from elsewhere without violating the ^{3}H distributions. The transient tracers thus permit us to rule out solutions with extremely large vertical transfers (it appeared unlikely, from the form of the infeasibility, that the introduction of additional degrees of freedom in the boundary conditions would have made the solution acceptable).

From (9), we can relate this result to conventional vertical eddy coefficients. We take the flows at the base of box 9 in figure 17 as an example. Substituting into (9), we obtain

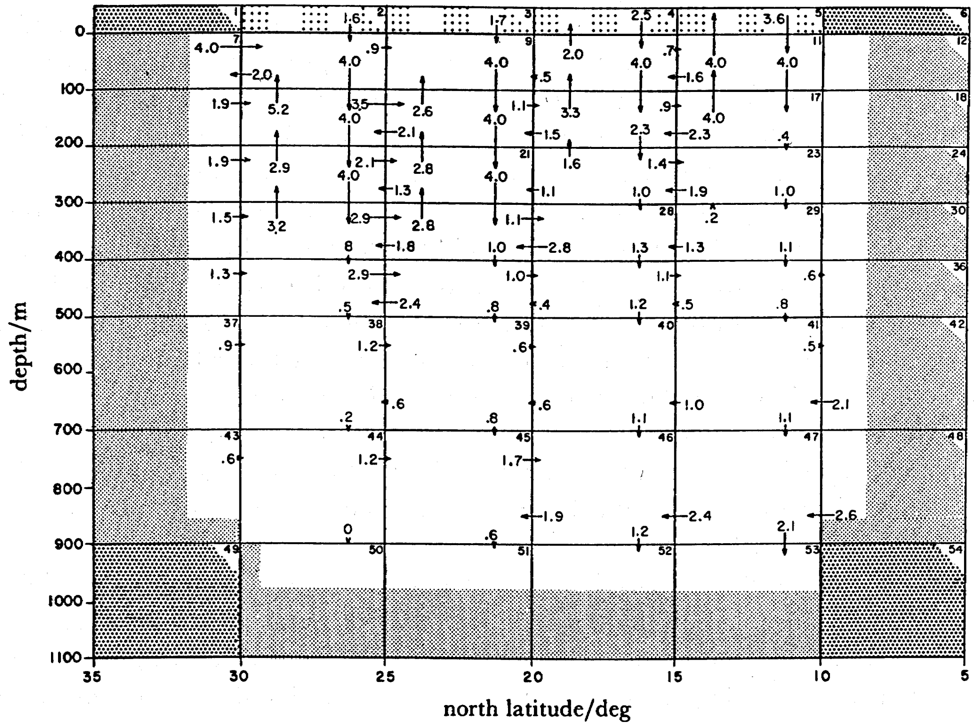

FIGURE 17. Solution in which the injection from the top layer was maximized (i.e. from boxes 8–11 into the layer beneath).

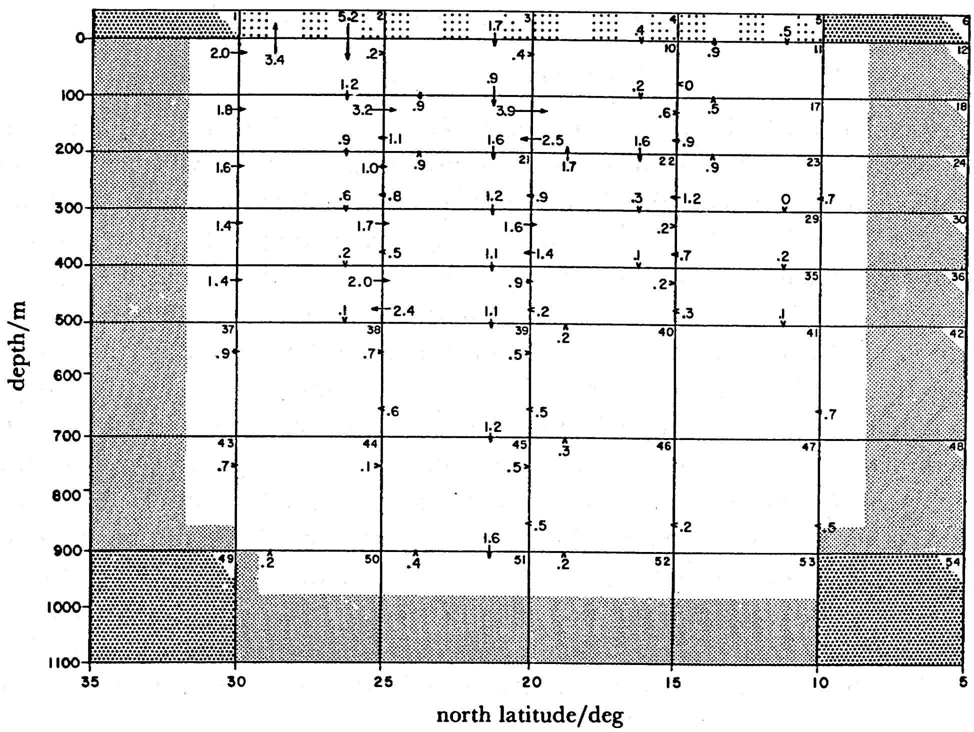

FIGURE 18. Same as figure 17, but a minimum flux.

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$K \approx 3\ \mathrm{cm^2\ s^{-1}}$. Because figure 17 is close to an upper bound for vertical transfer rates, the $^3H/^3He$ data suggestion that the upper-ocean apparent vertical eddy coefficient on this spatial scale must be less than about $3\ \mathrm{cm^2\ s^{-1}}$.

Figures 19 and 20 display the maximization and minimization of ventilation from the north.

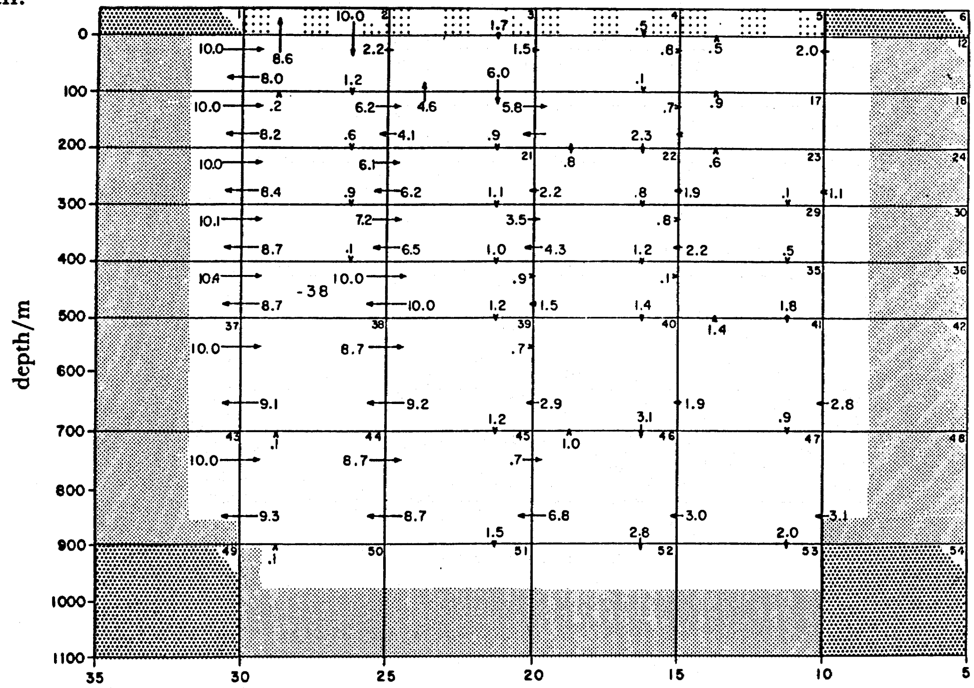

FIGURE 19. Solution showing maximum input from the northern boundary.

The difference between the minimizing and maximizing solutions is largely the difference between the flows that are unidirectional from the surface and northern boundary boxes and those in which a substantial amount of 'backflow' occurs. For example, the difference between figures 17 and 18 is essentially that in the former, the 'Ekman layer' receives substantial amounts of fluid from the layer below, whereas in the latter, the amount of export of fluid into the surface layer is much less. In both cases, the net transfer is set by the Ekman-flux values.

Comparing figures 16*a* and 20, we see that the solution of minimum overall circulation has a northern injection which is the same as the minimum northern injection, but the interior solutions are (slightly) different because of the different objective functions.

5.4. *Sarmiento's estimates*

Sarmiento (1983), using a box model of the tritium distribution in the sub-tropical North Atlantic, concluded that exchange between the surface waters and the upper thermocline had to be much greater (by a factor of 5) than that provided by Ekman pumping alone. He suggested that buoyancy forcing provided much of the excess injection. Because his model includes, for example, the region of 18 °C water formation and the Gulf Stream, the inference is reasonable.

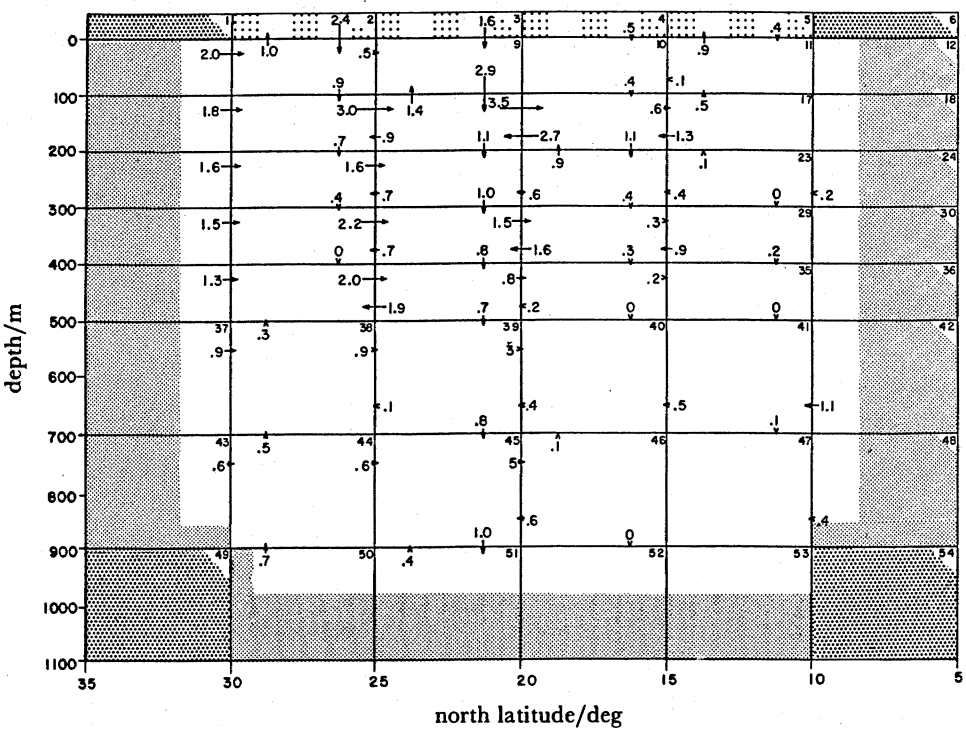

FIGURE 20. Solution of minimum input from the northern boundary.

But in the much more localized box model used in this present paper, there is no need to provide extra injection over and above that given by the estimates of Ekman pumping. As is clear from figure 14, most of the ^3H–^3He enters from the north (and east) rather than from above. This is not to say that excess invasion from above (as in the solution in figure 17) is not possible, merely that the ^3H–^3He distribution does not require it. Given the geometry of Sarmiento's (1983) model (his figure 2), it is possible that, even in the much larger domain over which he calculates the inventories, the missing time history at the northern isopycnal base of his outcropping layers could greatly influence the estimates of the injection necessary from above.

6. Discussion

The specific result of this paper is an estimate of the possible range of renewal rates of the water in the Eastern Atlantic thermocline. Although these may prove useful, the most interesting part of the work has been the formulation of a system in which transient tracers could be used to constrain and test the fluid flow. A formal identification can be made with the problem of distributed-system boundary control in which the 1979–1980 ^3H–^3He survey provides a terminal constraint on the system evolution. The open-ocean boundary conditions, and the uncertainties in the surface-boundary conditions correspond with the need to find a control function to drive the system to the observed terminal state within observational errors and subject to various auxiliary constraints (e.g. positivity). In the tracer problem we are not seeking to actually control the system, but asking for realizable boundary conditions that could have driven it to the observed terminal state.

The analogue with control problems makes it possible to adapt many of the methods and deductions from that subject. In the present particular case, we drew the conclusion that the transient tracers would serve mainly as a not very stringent consistency check on flows more tightly constrained by dynamical measurements (geostrophy) and steady tracer balances. They do serve to cap the maximum near-surface vertical exchange.

For the present region, with the particular model, and the data as they exist, it is difficult to escape the conclusion that the transient-tracer observations are less informative than the dynamical and steady balances. It is premature, however, on the basis of this one example, to conclude that this relative importance is universal. Almost surely there are situations in which the consistency check will fail badly, thus constraining the circulation directly. We can, even in the present context, examine ways in which the existing drastic underdeterminancy owing to the control variables could be reduced.

It is clear that as the number of boxes used increases, that the ratio of boundary/control unknowns to interior flow parameters decreases rapidly. The distance (in non-dimensional terms) of a given interior box from the boundary boxes would also increase. It may well be that a point would be reached in which the interior time histories would remain unaffected by the boundary histories for sufficiently long times that severe constraints would be placed on the interior fields. But one cannot arbitrarily decrease the box size to bring about this reduction in underdeterminancy. In the present case, the [^{3}H]/[^{3}He] eddy noise visible in Jenkins's (1987) maps means that the average concentration variances would increase as the box size decreased, and the inequalities would widen, with the result that the underdeterminancy may well remain unchanged or even worsen.

It should be pointed out, moreover, that there has been an implicit, enormous reduction in system degrees of freedom through the expedient assumption of a steady flow field. Relaxation of this assumption opens up vast new vistas of difficulty, the conquest of which is better postponed.

In the formal inverse problem, observations of the transient tracer C are used to make demands upon the flow field $\boldsymbol{J}$. That practical transient-tracer data are less demanding of $\boldsymbol{J}$ than are steady tracers is manifested in two ways: the difficulty of defining the control variables, and the necessity for having an estimate of the $\partial C/\partial t$ term, neither of which is an issue for the steady tracers. Constraints on $\boldsymbol{J}$ from the steady nutrients or oxygen are of course, gravely weakened by uncertainty over the biological source/sink terms that appear in the equations. The inert transient tracers are not subject to this latter uncertainty. Overall, the impression that one gains from the present exercise is that physical constraints involving neither biology nor chemistry (geostrophy, vorticity, mass and salt conservation) are much more powerful general tools for estimating $\boldsymbol{J}$ than are any of the biologically active, or transient, tracers.

The determination of a strategy for transient-tracer observations thus remains an important challenge. Perhaps a few tentative, nearly speculative, conclusions are permissible. The greatest difficulties emerged when the boundary-value histories had to be computed as part of the set of unknowns. This problem would be much ameliorated by mapping distributions over regions bounded by physical régimes (e.g. the continents) where the time histories can either be measured easily, or inferred with little uncertainty. It may be sensible to focus the observational efforts on developing time histories critical to large interior volumes. For example, the ^{3}H–^{3}He the history of the narrow Iceland–Scotland overflows would constrain any model of the flow in the interior to the south. Such histories would also be demanding of any model that claimed to compute the wintertime injection of fluid from the surface poleward of this region.

Finally, the unique capability of transient tracers is their ability to provide estimates of the very-large-scale, global, transfers from atmosphere to ocean, as a function of time. For climate purposes, and the CO_2 problem, a strategy of sampling directed at accurate inventory estimates, as opposed to interior flow estimates, seems plausible. To the extent that global inventories can be estimated over decadal timescales, their changes would appear to powerfully constrain air–sea transfers of properties.

Charmaine King provided all the computational help. The paper would not have been possible without extensive discussions with Dr William Jenkins. The work was supported in part by grant OCE8521685 of the U.S. National Science Foundation, and N00014-85-G-0241 of the U.S. Office of Naval Research.

Appendix

Table 1 listed the constraints and the formal error used in most of the steady inverse calculations described in this paper. Minor deviations permitted in some cases are described in the text. Unless otherwise specified, the row constraints apply to the interior boxes of figures 2–6.

Table A1 describes the constraints used in the transient tracer control problems. Again, minor deviations are described in the text.

Table A1

row constraints

tritium
- all interior boxes within 0.5 TU[1] of observed means in 1979
- surface box concentration within 0.5 TU of observed history (figure 8)
- open boundary boxes with 0.5 TU of 1979 observations at end of integration

stable tritium
- all interior boxes within 0.5 TU of observed means in 1979
- surface box concentration with 0.5 of observed ^{3}H concentration
- open boundary boxes with 0.5 TU of 1979 values
- $[ST] \geqslant [^3H]$ on all boundaries
- $[ST] = [^3H]$ on surface boundary

column constraints

$0 \leqslant \gamma_{1,2} \leqslant 10 \qquad -0.1 \leqslant \gamma_3 \leqslant 10$

[1] TU represents tritium units.

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Discussion

M. Whitfield (*Marine Biological Association, Plymouth, U.K.*). Would not Professor Wunsch's model be more closely constrained if OUR values had been obtained? Does this not suggest that a closer coupling between tracer movements and process-orientated studies would be valuable?

C. Wunsch. I would say 'yes' to both questions. If OUR could be reliably specified it would be very useful in constraining the physical flow field. However, my impression is that most OUR estimates have been obtained from physical flow models not unlike my own, and are therefore not independent pieces of information. I am unable to say whether there is any real prospect of purely biochemical estimates in which one would have quantitatively useful confidence.

D. J. Webb (*Institute of Oceanographic Sciences, Wormley, Surrey, U.K.*). Is it not slightly unfair to concentrate on a section across the main current flow, where the information on the velocities and diffusion rates is negligible compared with the geostrophic velocity-information gained from the density field? With a section taken along the core of a major current, where errors due to tracer advection out of the plane of the section are less, the tracer data would be much more useful in determining diffusion rates and velocities within the plane of the section.

C. Wunsch. Exactly to whom or what I am being unfair is not clear to me. I took the data that existed and tried to make inferences from them. The challenge to modellers is to use real data distributions, not those that exist only in some theoretical world.

It is certainly true that if one knew where the axis of a flow was, and if one could then apply the tracer distributions wholly to determining (say) the mixing, that the inferences about mixing rates would be stronger. But the assumptions that the axis of flow is known *a priori* to the observer who made the measurements, and that two dimensional tracer distributions necessarily imply steady two-dimensional flow fields, are very stringent ones. In practice we prefer to deduce the flow axis from the tracers, rather than to assume it.

W. J. Jenkins (*Woods Hole Oceanographic Institution, Massachusetts, U.S.A.*). I understand the main message of Professor Wunsch's presentation here is the limitations imposed by the lack of tracer data, and the strength of better-known physical constraints such as quasi-geostrophic balance and conservation of potential vorticity. As he correctly pointed out, we are hampered by incomplete knowledge of time variations in the transient-tracer distributions. All these point to a pressing need to increase the database in this regard.

My major reservation regarding Professor Wunsch's calculation stems from the physical construction of his model. To generalize, I feel that the model as posed is not sufficiently 'expert' to answer the questions he asks of it. His calculations represent an important first step toward quantitative evaluation of the utility (or lack thereof) of tracers in studying ocean circulation and mixing, but I would think it fair to stress that they are clearly preliminary and should not be regarded as a final judgement.

I think that the primary structure of his model should be oriented along isopycnals. Most of us believe that flow, to a first approximation, occurs along isopycnals. Diapycnic processes can be introduced as a perturbation. Second, I would be inclined to ignore the section data and concentrate on the β-triangle itself, where the data are more fully three dimensional. The section, aside from being two dimensional, crosses a property front in the ocean which is known from the days of Wüst and Defant to be controlled by the flow of waters from regions far outside the purview or this limited data set. I would expect any tracer analysis in such a situation to fail.

The choice of a box-model formulation, although mathematically 'reasonable', is not optimal for the task at hand. Professor Wunsch's procedure or averaging over box domains and ascribing an uncertainty associated with the internal variance is destroying important information. The primary information in the ^{3}He distribution lies in its gradients. There are gradients within the box domains that are not only lost by the averaging process, but contribute to his estimated 'noise', thus weakening confidence in the larger scale gradient estimates. I would go further to state that the ^{3}H/^{3}He age, although complicated by mixing does offer a unique advantage of having a built-in normalization regarding the time history. Put another way, as evidenced by Professor Roether's presentation, the ^{3}H–^{3}He age is relatively steady on the shallow isopycnals and hence may be easier to use in the inverse calculations.

The presence of eddy noise in the data makes it difficult to estimate gradients on small scales in a reliable way. None the less, the mean gradients across the triangle are determinable to order 10% in ^{3}He. Perhaps a more sensible approach would be to reduce the degrees of freedom with a least-squares or objective mapping procedure.

Finally, my original purpose in sampling the β-triangle area was to use it as a test bed for the ^3H–^3He dating technique because the area was so well characterized hydrographically. As I have tried to demonstrate in my recent study, the age data are approximately consistent with the climatologically averaged geostrophic velocities. The small inconsistencies are in themselves interesting, but I should also point out that if one were to reduce the problem to one of cost-benefit analysis, one must weigh on the one side the cost of acquiring and analysing the tracer data, and on the other side the cost of the many hydrographic cruises that were necessary to achieve the climatological mean estimates. More importantly, the tracers yield information not simply related to the velocity field *per se*, but also to the complex amalgam of processes involved in ventilation. The data set that Professor Wunsch has used does not contain sufficient information to solve all the problems, but this does not mean that one has placed firm limits on what tracers in general can do.

C. Wunsch. I am not aware of any problem in which the answer is determined by the coordinate system chosen. Proper choice of coordinate system can make the solution much easier to get, but the actual answer should be independent of it. In the present case, I wanted to use the vorticity constraints, which are much simpler to apply in geographical coordinates. If one wishes to add constraints that cross-isopycnal mixing is very weak, those statements are additional linear constraints upon the J_{ij}, involving local projections onto the isopycnals in each box. One can be confident that addition of such constraints would tighten the solution bounds. They would be easier to write down in isopycnal coordinates, but then the vorticity constraints would have to be written through projections onto the geographical coordinate system. A decision as to whether one set of constraints or the other is more fundamental is somewhat arbitrary. My own view is that a conclusion that cross-isopycnal mixing is small is something that should be deduced from the observations, not imposed, whereas the vorticity constraints are inferred systematically from Newton's laws of motion and are more difficult to escape.

Certainly one could reduce the box size somewhat. But the model is a climatological one referring to multi-year integrated behaviour. I chose the box size used on the basis of a subjective judgement as to the smallest region over which I was comfortable that I was not just looking at eddy noise. Given the importance of the heat, salt and vorticity constraints, and that we know so much more about spatial scales of eddy variability for these physical fields, I would be reluctant to reduce the box size by very much. A different set of boxes could be used for ^3He than for other tracers, within the same model, but in the last analysis, there will be no substitute for a good time series of ^3He, at mesoscale time–space resolution, to demonstrate that reduction in scale does not increase the noise faster than information is being gained.

I have tried to be very careful not to overemphasize the generality of these results. In that sense, I regard this model as a test bed too. In my mind, the major results are two. (1) In the spirit of what I have called 'eclectic modelling', one can use quantitatively all sorts of different data with irregular distributions in space and time. I recognize the realities of oceanic observations, and the whole eclectic idea was to learn how to employ observations as we actually get them, not as a mathematician classically assumes them to be. (2) That surveys of transient tracers provide a 'terminal constraint' seems to be a useful mathematical concept now permitting us to use the control mathematics in other situations to better understand the relative information content of different types of observation. The mathematical issue would be no different had the calculation been restricted to the triangle area itself, although the result might have been more constraining. Reduction in degrees of freedom would also obviously lead to tighter bounds from the tracers, but which degrees of freedom depicted in the figures in the paper should be rejected as physically unreasonable?

Nothing would have pleased me more than to find in the present situation that the existing ^{3}H–^{3}He data strongly constrained the flow, mixing, OUR, etc. There may be some much cleverer way to use these data than I have thought of (but just making many more restrictive assumptions about how the ocean behaves is not what I mean), and different regions may yield very different results. The challenge now is to demonstrate that.

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