January 17, 1955

Mr. Henry Stommel and/or
Mr. Jules Charney
Institute for Advanced Study
Princeton, New Jersey

Dear Hank and Jules:

Either I am nutty, or Neumann is wrong. The last two terms in his equation (8) are

$$\frac{\partial \bar{p}}{\partial x} D, \qquad \frac{\partial \bar{p}}{\partial y} D,$$

The notation is misleading. The terms follow directly from the integration of (6) and are therefore

$$\int_{-D}^{0} \frac{\partial p}{\partial x}\, dz, \qquad \int_{-D}^{0} \frac{\partial p}{\partial y}\, dz$$

In going to (9), we cross-differentiate as follows:

$$\frac{\partial}{\partial y}\left[\int_{-D}^{0} \frac{\partial p}{\partial x}\, dz\right] - \frac{\partial}{\partial x}\left[\int_{-D}^{0} \frac{\partial p}{\partial y}\, dz\right]$$

$$= \left\{\int_{-D}^{0} \frac{\partial^2 p}{\partial x\,\partial y}\, dz - \frac{\partial(-D)}{\partial y}\left(\frac{\partial p}{\partial x}\right)_{z=-D}\right\} - \int_{-D}^{0} \frac{\partial^2 p}{\partial y\,\partial x}\, dz - \frac{\partial(-D)}{\partial x}\left(\frac{\partial p}{\partial y}\right)_{z=-D}.$$

The sum of the first and third term vanish because $\partial^2 p/\partial x\, \partial y = \partial^2 p/\partial y\, \partial x = 0$; the second and fourth terms vanish separately because the pressure gradients are zero at $z = -D$ by hypothesis. (At least I assumed this explicitly; it follows of course from the geostrophic approximation.) Thus the whole expression is zero, and <u>not</u>

$$\frac{\partial D}{\partial y}\frac{\partial \bar{p}}{\partial x} - \frac{\partial D}{\partial x}\frac{\partial \bar{p}}{\partial y}$$

as obtained in Neuman's (9). What remains of equation (9) is identical with the equations used by Hank and myself.

Let's take a simple example. Let

$$p = x\, y(z + D)^2$$

$$p_x = y\,(z + D)^2 + 2xy(z + D)$$

$$p_y = x(z + D)^2 + 2xy(z + D)$$

All these vanish at $z = -D$. Now

$$\int_{-D}^{0} p_x \, dz = y \int_{-D}^{0} (z + D)^2 \, d(z + D) + 2xy\, D_x \int_{-D}^{0} (z + D)\, d(z + D)$$

$$= \tfrac{1}{3}\, y\, D^3 + x\, y\, D_x D^2$$

$$\int_{-D}^{0} p_y \, dz = \tfrac{1}{3}\, x\, D^3 + xy\, D_y D^2$$

Hence

$$\frac{\partial}{\partial y}\left[\int_{-D}^{0} p_x \, dz\right] - \frac{\partial}{\partial x}\left[\int_{-D}^{0} p_y \, dz\right]$$

$$= \tfrac{1}{3} D^3 + y\, D^2 D_y + x\, D^2 D_x + xy\, D^2 D_{xy} + xy\, D_x D^2_y$$

$$- \left[\tfrac{1}{3} D^3 + x\, D^2 D_x + y\, D^2 D_y + xy\, D^2 D_{xy} + xy\, D_y D^2_x\right.$$

$$= 0$$

but following Neumann, we have by definition

$$\frac{\partial \bar{p}}{\partial x} = \frac{1}{D}\int_{-D}^{0} p_x \, dz = \tfrac{1}{3}\, y\, D^2 + xy\, D_x D$$

$$\frac{\partial \bar{p}}{\partial y} = \frac{1}{D}\int_{-D}^{0} p_y \, dz = \tfrac{1}{3}\, x\, D^2 + xy\, D_y D$$

His expression for the cross product in (9) is

$$\bar{p}_x D_y - \bar{p}_y D_x =$$

$$= \tfrac{1}{3} D^2 \left[\, y\, D_y - x\, D_x \right] \neq 0.$$

q. e. d.

Yours,

Walter H. Munk

P.S. Of course, in von Arx's trough where $z = -D$ is the solid bottom, eastern intensification could be generated, but there the pressure gradients do not vanish at $z = -D$.

Hurray for

WESTERN DEFENSIFICATION

PPS. Please write me at once what would be convenient times for my visit to Princeton. I have to do some ship scheduling.

4918

Jan 28

Walter,

No, no, no. The torque of the stress on the western boundary is proportional to the ~~stress~~ ~~...~~ (stress) $\frac{r}{2}$ lever arm to center of mass.

$$A\frac{\partial v}{\partial x} \cong A\frac{\partial^2 X}{\partial x^2}$$

<u>not</u>

$$A\frac{\partial^3 v}{\partial x} \cong A\frac{\partial^4 X}{\partial x^4}$$

Now

$$A\left(\frac{\partial^2 X}{\partial x^2}\right)_{x=0} = A\left(-\frac{1}{2}k + \frac{i\sqrt{3}}{2}k\right)^2 e^{i\left(\frac{\sqrt{3}}{kr} - \frac{\pi}{6}\right)}$$

$$= A\frac{k^2}{4}\left(-1 + i\sqrt{3}\right)^2\left[\cos(\ ) + i\sin(\ )\right]$$

$$= -\frac{Ak^2}{\cancel{4}2}\left(\cancel{\ } 1 + i\sqrt{3}\right)\left[\cos i(\ ) + i\sin(\ )\right]$$

$$= -\frac{Ak^2}{2}\left(\cos\left(\frac{\sqrt{3}}{kr} - \frac{\pi}{6}\right) - \sqrt{3}\sin\left(\frac{\sqrt{3}}{kr} - \frac{\pi}{6}\right)\right]$$

$$= -\frac{Ak^2}{2}\left[\cos\frac{\sqrt{3}}{kr}\cos\frac{\pi}{6} + \sin\frac{\sqrt{3}}{kr}\sin\frac{\pi}{6} - \sqrt{3}\sin\frac{\sqrt{3}}{kr}\cos\frac{\pi}{6} + \sqrt{3}\sin\frac{\pi}{6}\cos\frac{\sqrt{3}}{kr}\right]$$

(But $\sin\pi/6 = \frac{1}{2}$; $\cos\pi/6 = \sqrt{3}/2$)

$$= -\frac{Ak^2}{2}\left[\cos\frac{\sqrt{3}}{kr}\left(\frac{\sqrt{3}}{2} + \frac{\sqrt{3}}{2}\right) + \sin\frac{\sqrt{3}}{kr}\left(\frac{1}{2} - \frac{3}{2}\right)\right]$$

but for $\frac{\sqrt{3}}{kr} \ll 1$

$$= -\frac{Ak^2}{2}\left(\sqrt{3} - \frac{\sqrt{3}}{1kr}\right) \cong -\frac{\sqrt{3}}{2}Ak^2$$

$$= f(A)\ !$$

I certainly look forward to seeing you and Judy etc. Will tell Jake about the date

— Hank

THE INSTITUTE FOR ADVANCED STUDY
ELECTRONIC COMPUTER PROJECT
PRINCETON, NEW JERSEY

March 12, 1955

Dr. Columbus Iselin.
Woods Hole Oceanographic Institution
Woods Hole, Mass.

Dear Columbus:

Now that my stay here in Princeton is coming to a close, I have been trying to form in my own mind a picture of the present state of recent theoretical work on the wind-driven ocean circulation. There has been considerable progress, I think, in certain limited areas. Other broad aspects of the problem, some perhaps of more importance than what has been cleared up, are still only dimly apprehended. In this latter category I am referring to such fundamental problems as the reason for the existence of the main thermocline, and the role of thermodynamic processes in the Sea. I am sorry to report that we cannot shed any light on these most vexing questions as a result of the work, thought, and discussions of the past year's theoretical activity.

There are two main results which emerge from the welter of brain storms in pretty good form. I think they are major steps forward. They are due to Charney and Morgan separately, and I will try to describe them briefly.

Result No 1: Charney's two layer quasi-geostrophic theory of the Growth Region of the Gulf Stream. As you will remember, Rossby never felt that the Munk wind-driven theory could give a detailed picture of the filament-like structure of the Gulf Stream. Munk's general overall picture of the wind-driven circulation stands, of course, but one should not try to look at it with a microscope to search out minute detailed features such as cross-stream current profile, etc. Montgomery evidently was worried by the way eddy viscosity was employed, and he suggested to Fofonoff that he look for frictionless free solutions. My own little yellow booklet tries to do without friction; and Morgan has investigated flow patterns in a western oceanic boundary layer in the absence of friction in a homogeneous ocean (no density stratification). But Charney, I think, has copped the prize by coming up with a theory of the region of the growth of the Gulf Stream (from the Florida Straits to Cape Hatteras) which really looks like the real thing... the thermocline tilts up at the right angle, the width and velocities of the stream are good fits, and proper account is taken of accelerative processes within the stream, so that it should fit fairly well with the results of precise levelling along the coast. When he has it finished it will be a considerable contribution. It has grown out of his contact with WHOI last summer, and I think Admiral Smith and the Woods Hole Associates should be gratified at these indirect results of their awarding the Lectureship to Charney last summer.

Morgan has shown that the boundary stream in a frictionless homogeneous ocean cannot have a countercurrent on the east; I do not know whether the Charney theory will give one. Munk's frictional theory gives one. And I can think of at least one other process which must favor a countercurrent: the fact that advection of heat from lower latitudes leads to the formation of the warm core, and hence requires some isotherms to come back to the surface on the right hand side of the stream. Malkus and I looked at this warm core quite intently last summer, but we never were able to decide how important it is to the dynamics of the stream. It bears watching, but would introduce a much more involved theoretical model than any we have yet considered.

There are, as I see it, two phenomena which can be called countercurrents on the eastern side of the Stream, and which tend to get confused. The first is the narrow (say 100 km wide) countercurrent on the right hand side of the warm core. This is the countercurrent of the towed electrode, bathythermograph, Loran navigation type surveys. The other countercurrent, and perhaps the most important one really, is manifest in the recurvature of the southern branch (sic) of the Gulf Stream to rejoin the Stream near Hatteras. This is manifest of the 10° isotherm chart as the elongated elliptical depression of the isothermal surface between the Stream and Bermuda. The thing also shows up very clearly on the Meteor Atlas chart " Dichte in 1000 m Tiefe"....the Big Green Sausage. The first countercurrent is associated only with isotherms >18° C. The other is more gradual, and extends to all depths.

Result No 2. Morgan's Formulation of the Integrated Equations of Motion, and its application to the central regions of the ocean. You will recall the bombshell that Neumann quietly cast into the arena last spring, when he suggested that variations in depth of the moving surface layer might alter the picture of the central oceanic wind-driven circulation used by Sverdrup, Reid, Munk, and myself. At first we found a few slips in the original formulation as put forward by Neumann, but we never were able to convince him (or ourselves) that he wasn't basically right. The whole question of the effect of variation in depth of the moving surface layer has been very tormenting to everyone. Morgan has re-examined the whole question ab initio, (the WHOI technical report) and has found that there is indeed an extra term in the vorticity equation for the central ocean. This term has important consequences for the calculation of the circulation and topography of the thermocline in the central ocean. Morgan has applied the new form of the equations to a two-layer model.

We have been using your chart of the 10° isotherm as a guide in our thinking. The most striking feature of this chart is the abrupt transistion from very smooth regular topography south of 30° N to extremely rugged irregular topography north of that latitude. Morgan's analysis applies to the southern half. He suggests that we try to deal only with the southern half at present, and replace the upper half with a black box symbolic of our ignorance of this region of decaying Gulf Stream, wind drift, and winter convectional overturn and mixing. Now that we have tried to construct a two layer ocean we are seeing for the first time the impossibility of ignoring turbulent and thermodynamic processes north of 30° . Most remarkable of all is the fact that the flow over most of 30° N is mostly to the South..... and yet by some miracle, this very irregular thermal topography suddenly becomes transformed into a very regular, uniform one.

I think there is every reason to hope that the essential features of the 10° isotherm chart south of 30°N will be explained in terms of Morgan's work, and this will certainly be a very big step forward...

Finally, a word or two about the investigation that Veronis and I (mostly Veronis) have been carryying out concerning the response of the sea to variable winds. It is all written up and being typed in final form. You will recall that we originally had great hopes of getting a theory that might deal with the fluctuations in Florida Current observed on the Western Union cable. Well, we can't really do that properly yet. To tell the truth, our work probably only applies to the initial stages of the response of the sea to a storm far away from coasts. You see, our ocean is infinite, darn it. But it has lots of interesting features, and maybe someday Veronis will be able to fence the ocean in with coasts. He has applied for a Fellowship for the month of August, so that we can try to carry the thing a little further forward.

Yours sincerely,

Henry Stommel

cc G H Morgan
J G Charney
G Veronis
W H Munk
W Malkus
G Neumann

THE INSTITUTE FOR ADVANCED STUDY
ELECTRONIC COMPUTER PROJECT
PRINCETON, NEW JERSEY

Dear Walter :-

If there is one thing which changes every week it is the status of the integrated equations of motion for wind generated currents.

Veronis has found an inconsistency in Morgan's results (WHOI Tech Rept.) - which goes a long way toward cancelling the optimistic note I sounded in my letter to Iselin.

Will see you soon

Hank.

March 22, 1955

WOODS HOLE OCEANOGRAPHIC INSTITUTION
WOODS HOLE, MASSACHUSETTS

Dear Walter,

Thanks for the note. I am returning the Fofonoff letter. Also am sending you a Russian paper on ocean currents by one Sarkisyan. My best to Judy – and my apologies for running out on you – I am a goose.

Yours as ever,

Hank

April 25, 1955

Mr. Henry Stommel
Institute for Advanced Study
Princeton, New Jersey

sent to:
Woods Hole Oceanographic Institution
Woods Hole, Massachusetts

Dear Hank:

Many thanks for your note and the Russian translation. Concerning the Russian paper, George Carrier has made the following very plausible comments. There are, in the solution, two additional terms as compared to what we had in our paper. If the wind curl is a function of latutude only and these become proportional to two of the other terms in the solution, then our results are correct as they stand. If, on the other hand, the wind curl varies with longitude, then there are two possibilities.

If such variations are important over distances of the order of the distance of the Gulf Stream from shore, or even shorter distances, then the additional terms are important. If the important meridional fluctuations in wind curl take place over distances large compared to the Gulf Stream's distance from shore, then the additional terms are negligible.

I think that the latter condition is the reasonable one to take, and that therefore the objections raised in the paper are not valid. George expresses himself more strongly.

Love,

Walter H. Munk

WHM:es

May 5, 1955

May 5, 1955

Mr. Henry Stommel
Woods Hole Oceanographic Institution
Woods Hole, Massachusetts

Dear Hank:

Your WORKSHOP sounds wonderful and I should be glad to take part in it. Why don't we do it at Scripps?

Certainly NSF could be approached but it would seem to me that most of the people you mentioned could just do this as part of their regular work. In a way a request to NSF would be a bit touchy since it would come to me for action. I suppose I could disqualify myself from having to decide this but I am almost certain that anyone else who could act on this would act favorably.

You may not agree with me but I have become doubtful as to whether one can collaborate with Michael. He is a lone wolf and does not really know how to work with anyone else or with a group of people. He is certainly most able, but I am not altogether enthusiastic about the chance of having him take part in an effective bull session. However, you make the decision any way you wish and keep these doubts confidential.

I was naturally sorry that you didn't show up at AGU. I suggested that Gordon come up and see you and I hope that you had a fine time together. I thought he gave a fine paper at Washington and I also was impressed with papers by Redfield and Maurice Ewing.

Don and I thought we would have one more go at trying to do something at the AGU meeting that would attract people like yourself. After next year our tenure will be over, thank God. I would propose a one day session on ocean currents with four invited papers in the morning on critical new observations and four invited papers in the afternoon on theory. The eight papers must be given by people who have contributed something essential during the last two years. There should be a half hour of discussion for every half hour of paper. Do you think we might get you not to boycott such a meeting? If you do I am about ready to give up having anything more to do with AGU.

One bit of gossip; President Fleming has not paid his dues for three years. Also, Don Pritchard has won his battle and the Transactions will come out in a much improved format starting next year.

Sincerely yours,

WHM:es

Dear Walter:

Do you think that it would be a good idea, for the Summer of 1956, to try to arrange a WORKSHOP in THEORETICAL CIRCULATION STUDIES, at which YOU, Charney, Veronis, Ichiye, Wm Malkus ~~myself~~, Morgan, and Longuet-Higgins would assemble for a month (or two) and work on problems together? We could have a wonderful time, and as long as we limited the study to theoretical stuff & did not let the group get much bigger than 6 or 8, it would be fascinating & profitable.

Do you think NSF might underwrite such a group effort? Do you think we could do it at Scripps or Woods Hole? or should we have it far from interruption — say in the mountains somewhere?

Yours sincerely

XX

Hank

May 3 '55

May 9 55

Dear Walter :-

I am so glad to hear that you like the idea of a theoretical circulation **WORKSHOP** for the summer of 1956.

Scripps would be a swell place to have it. If you think, on second thought, that you could swing travel expenses, etc. - I would love to bring my family - over & above the minimum that ONR & WHOI allow, you are elected HOST.

Hank.

May 24, 1955

Mr. Henry Stommel
Woods Hole Oceanographic Institution
Woods Hole, Massachusetts

Dear Hank:

I am intrigued about your preliminary agreement between the non-steric change in sea level and the wind curl calculation. Perhaps this is it. We actually had a look at the non-steric component with the possible effects of shifts of water mass on the earth rotation. It is, of course, only that component that is important, as it represents an actual shift in mass. But it never occurred to me to compare the non-steric component to see whether it might follow your barotropic theory.

Two comments. Part of the non-steric component, perhaps about 2 cm, represents a seasonal shift of water mass from ocean to land (ground water and snow), I think. Secondly, the non-wiggly part of my solution for the barotropic square ocean case should be somewhat similar to your calculation.

I have a favor to ask you. You recall the Klebba automatic 14 months current meter? Could I have on loan or permanently the WHOI blue report describing this instrument? I have misplaced my copy.

With best regards,

Sincerely,

Walter H. Munk

WHM:es

Dear Walter:

The reason I did not come to the AGU meeting was that I just felt rather tired, and the meeting did not look very good as a whole. With a few exceptions, in fact, I would guess it was LOUSY. There are so many utterly half baked papers that one's patience is exhausted.

The influence of the old-timers is still very strong, but the vein of qualitative reasoning is pretty well run out. We do not seem to be able to attract many really first rate people into the field despite our millions of dollars budgets.

Honestly, I'm not boycotting the AGU or ASLO. I just can't bear them.

Hank

June 15, 1955

Dear Walter, Gordon, and George V.

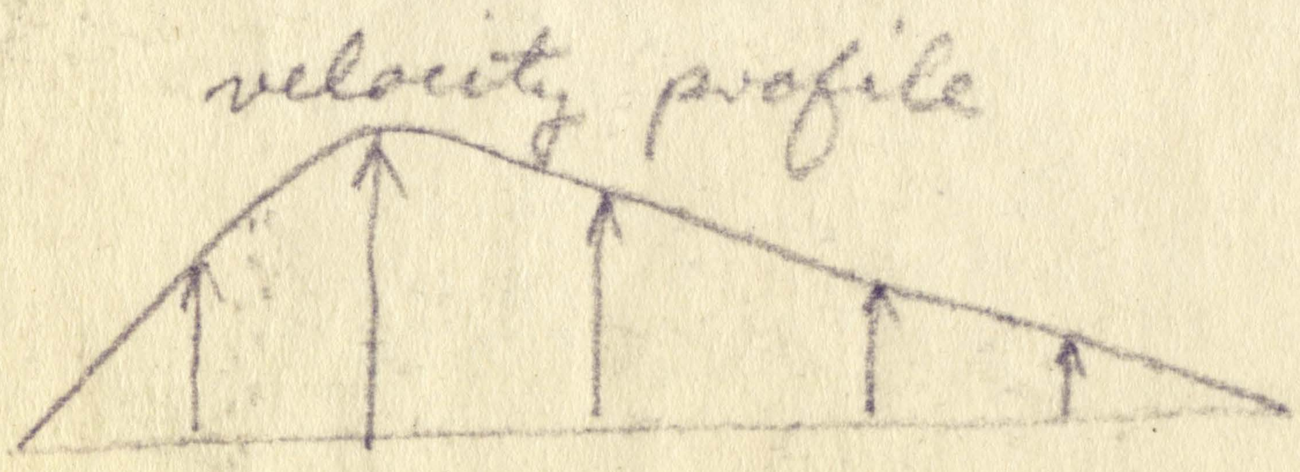
I returned from the Imperial City, where I was able to find enough consecutive Pillsbury current data made at the same station (700 surface readings during 1 wk.) (off Fowey Rks) to play around with trying to find the eddy coefficient in the Florida Sts. There is not enough dope to do it in the region of anticyolonic shear off Hatteras where it would really be worth doing some time. Its not worked up yet, but offhand looks like 10^5 $cm^2 sec^{-1}$.

I saw George V. (Veronis, not Fifth) on the way back, and Jule. We talked over various things, such as.

One of the interesting results of the Veronis-Stommel transient paper was the relative insensitivity of the thermocline to seasonal wind variations in mid latitudes. I looked at the monsoon region in the Indian ocean to see if it is so there, where there is a really good reversal of wind. The 1950-51 Discovery stations seem to shew that the thermocline really does respond to the wind in 5° to 10° S. But this is rather better than it looks, because if you redraw our frequency-wavelength diagram for such low latitudes the theory then predicts response of thermocline. Two comments are then makable: the monsoon area so near the equator does not constitute a test; and second, our theory is not much good near the equator. Now GORDON, your equatorial wave theory DOES apply near the equator, and you could easily discover what the response of the thermocline should be under an equatorial monsoon. In fact, your theory is "a very nice complement near the equator to ours far from the equator" quotes Geo V.

I think I would be putting it mildly to say that Gordon and I are having trouble finding clearcut evidence of day-to-day barotropic mode sea-level changes corresponding to ocean storms. Now I wonder (you will remember Munk's suggestion that the mid-Atlantic ridge might be a wave-guide?) whether we simply cannot ignore the bottom topography in discussing the dispersion of the barotropic mode. Is this the reason that the tide-gauge observations and storms correspond so poorly. For example, the Bermuda tide gauge is sitting on a cone, with tip 5 km above the bottom, with a base 130 km in diameter. Can this possibly act as to isolate Bermuda from barotropic sea-level changes? in the outside sea?

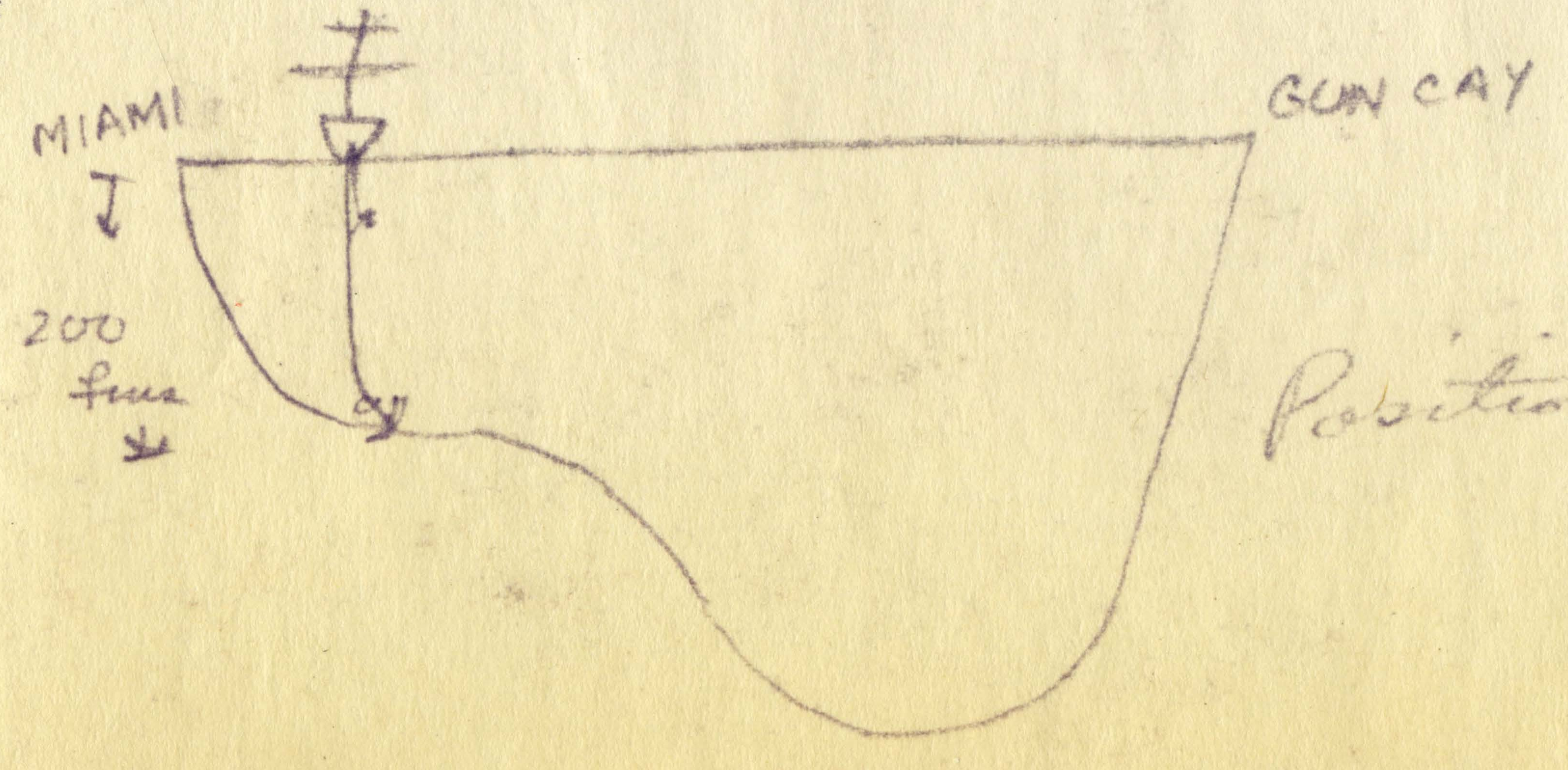
Bows, scrapings, and apologies...unconditional surrender and sorrow, vales of weeping, mists of sadness. I made a mistake plotting the Bermuda tide gauge data. Mr. McKay at USCGS challenged the big Nov sea-level bump, we looked over the data, and I slipped in the Nov plotting. There is still a bump, but not nearly so big as I drew it. It is like other years. It comes down about 0.4 feet. Maybe I should be glad.



Yours repentfully

Hank

Henry Stommel



Position of Pillsbury's week-long station in Florida Strait, relative to current profile.

Drs. Gordon Groves and Walter Munk

July 28, 1955
Woods Hole

Dear Walter and Gordon:

Ever since our confab in Princeton I have been looking a t tide gauge data etc., and thinking about the quasi-geostrophic model of forced motion in closed oceanic basins and getting very nearly nowhere. I talked about these things with Dr. Arnold Arons and he suggested that we look into the free periods of quasi-geostrophic barotropic motions in an ocean governed by the vorticity equation

$$\lambda^2 \eta_{xxt} + \beta \lambda^2 \eta_x - \eta_t = 0$$ η is elevation of surface

with boundary conditions $\eta_{xt} = 0$ ~~of the form~~ at $x = \pm L/2$

and waves of the form $\eta = \eta' \sin(\omega t + k' x) + \eta'' \sin(\omega t + k'' x)$
The result is that there are only certain frequencies permitted ω_0, ω_1, ω_2, ω_3 defined by the equation

$$\omega_n = \frac{\beta \lambda}{2} \left(1 + \frac{n^2 \pi^2 \lambda^2}{L^2}\right)^{-1/2}$$

where

$$\lambda = \frac{\sqrt{gD}}{f}, \quad n = 0, 1, 2, 3 \ldots \qquad \beta = \frac{\partial f}{\partial y}$$

These quasi-geostrophic seiches consist of a combination of two free Rossby waves each of the same frequency, but of different two wavelengths, obtainable from the ordinary frequency equation (which has two wavelengths for each frequency):

$$\omega = (\beta / k) / (1 + 1/k^2 \lambda^2)$$

On account of the restrictive nature of the boundary conditions at $x = \pm L/2$ there are only certain eigenvalues of frequency permitted (given above) and the ratio of the amplitude of the two components is also fixed... the longer wavelength has the greater amplitude. The absolute value of the amplitudes of course is arbitrary.

I think Arons' suggestion gives a much more interesting model to think of than the forced one which we were diddling with, with walls. The following little table gives some of the computed periods. The period corresponding to frequency ω_0 is not given because it is a degenerate mode. IT IS IMPORTANT TO REMEMBER THAT THESE FREQUENCIES ARE COMPUTED ACCORDING TO THE INFINITELY N-S EXTENDED MODEL ON THE BETA-PLANE, and hence may be misleading so far as an ocean bounded by latitude circles as well is concerned.

Ocean	Latitude	Mean Depth(km)	Mean width	Period (days) First mode n = 1	Second mode n = 2	Third mode n = 3
Atlantic	30 N	4.5	5770	4.7	8.2	11.5
	45 N	3.5	3520	9.5	16.6	23.0
	60 N	2.0	1660	26.2	49.5	63.0
	30 S	4.0	5770	4.7	8.3	11.8
Pacific	30 N	5.5	11540	3.0	4.6	6.0
	45 N	5.0	6670	6.0	9.5	12.8
	30 S	4.5	9630	3.4	5.3	7.1
Indian	30 S	4.0	7700	4.0	6.6	8.8
Antarctic	50 S	4.0	24200	5.7	6.1	6.8

These look rather like the kind of periods that you, Gordnn, were seeing in the Pacific.

When Arnold Arons and I had got this far we were really quite excited and the subject of geostrophic seiches looked like a road to glory, when all of a sudden I found a paper by Goldsbrough (Proc Roy Soc A vol 140, p 241, 1933) in which he obtains the solution of the problem of free oscillations in an ocean with meridional coasts 60* apart on a rotating globe. There are two periods nearly equal to tidal periods, but there is a longer period... 7.2 days ... associated with a geostrophic type motion. Now as a matter of fact this long period type of free oscillation associated with what we would call barotropic transient current systems has long been known to tidal theoreticians (see Hough) as motions of the Second Class, and it is apparently actually the same thing as Rossby waves on a globe without a basic zonal flow. Arons and I were somewhat taken aback to find our work so clearly anticipated by 22 years; but I think our physical understanding is somewhat superior to Goldsbrough's. He was not really interested in the long period. Moreover, his method does not given a very good shape or map of the successive wave form over the ocean. On the other side of the ledger of course he has the ineffable advantage of being on a spherical rotating earth rather than on a limitless beta-plane. You will be vexed to know, if you do not already know, that he tried to do the problem for the Pacific (120° apart) but decided against it on account of the necessity of dealing with a more complicated form of associated Legendre function. We are really so sorry that the Pacific should be mathematically unfriendly as compared to the Atlantic.

Our approximate beta-plane theory of course is just as easy for the Pacific as for the Atlantic. It gingerly skirts equatorial regions. We are going to look into dissipative boundary conditions for the forced and free motions. It ought to be interesting.

Are we duplicating something that has already been done more than once before? If you have any further references for us please come across with them.

All this excitement about the barotropic mode makes me very anxious to try out for several years a deep-sea pressure gage at Bermuda at a depth of about 3000 meters. That is the reason why I wrote Walter. I will write a separate letter to Frank Snodgrass about the instrumentation problems. I certainly hope that stability of the instrument will not be a stumbling block as Frank suggests may turn out to be the case.

My best wishes to everybody...and have a good trip Walter.

Hank

Henry Stommel

Dear Walter:-

I think Hasson will be glad to do a bit of drudgery or stewing over records for you. He is not a fancy mathematician, & I am not sure he has a very good feeling for the ocean yet, but he is industrious & intelligent. He had two years with Proudman. I judge he is about 28 yrs old. I think it has been a rather unfortunate thing for him to be so exclusively under Neumann's influence so long. Neumann genuinely believes that he (Neumann) has a personal intuitive understanding of the ocean, and his students take him too seriously. I want to see Hasson emancipated from that influence.

My understanding is that Hasson may be going to Stockholm in Feb. ~~[illegible]~~. I asked him how long he wants to stay at Scripps - he says Oct thru Jan — 3 months.

I am quite fond of Hasson. I think you will like him personally, although he may not be able to enter into your present theoretical thought & research.

Incidentally, we got <u>Ichiye</u> a Rockefeller Fellowship for 1957. He will stop by at Scripps for a day or two, the end of Oct 1956, — to attend some kind of whing ding you are having there.

Dear Walter :-

Thank you very much indeed for the nice letter about edge waves — especially the analogy with Love waves.

The data which is given in the technical report is half-hourly. Since Oct 3 '55 (data not yet published) I have <u>5 minute</u> interval temperature at 500 meters for about 6 months. This data is not drawn up or read yet, but if ever and whenever you say the word, I'll read off 3000 consecutive temperatures at 5 minute intervals for you, and send it to you.

I can't figure out why the Väisälä frequency

$$\omega = \sqrt{\frac{g}{\rho}\frac{\partial \rho}{\partial z}}$$

should be the <u>maximum possible</u> one in stable water column. I can see that it is the frequency for a wave disturbance of the form

$$\rho = \rho_0 \sin \omega t \sin lx \sin mz$$

as $l/m \to 1$, but why is it the max possible?

Life is just one long series of puzzles. Can you give me an easy answer or reference?

—Hank.

WOODS HOLE OCEANOGRAPHIC INSTITUTION
WOODS HOLE, MASSACHUSETTS

Dear Walter:

Many thanks for the reprints. Arnold Arons and I are going to put a vibrotron on our Bermuda deep cable — the Byron Jackson Co is making us a copy of your deep one.

Incidentally, can you or Snodgrass spare us a foot or two of the Navy 200 series wire that you use? — as a sample

The Navy Property man at Boston claims he does not know of the 200 series, & we ought to get a look at it, and then show him too. Up til now we have used old Spiral 4 & found it adequate, but of course the number of conductors is rather small.

X

Hank

WOODS HOLE OCEANOGRAPHIC INSTITUTION
WOODS HOLE, MASSACHUSETTS

Dear Walter:-

Your letter catches me just as I depart for Curacao, and points south to repair a Caribbean electrode installation.

The experiments Cromwell is doing sound very interesting to me — they sound a little like the garbage can experiment. I also enclose a reprint for Rouse which is somewhat similar, only he is maintaining a steady density field.

No — we are not in Bermuda, this year, but will be there, I hope, the coming winter.

You will be glad to know that Veronis is coming to WHOI permanently June 15. At last I have a theoretical colleague to whom I can turn for mathematical advice & help!

Morgan and I are trying to develop some kind of theoretical framework which would give us some idea of just what it is that determines the depth of the baroclinic wind

~~driven~~ geostrophic currents in the sea. – ~~first~~

I think I once sent you a translation of a rather interesting paper by Linneykin [Russian] on the subject. I think we have improved on this basic idea pretty much now.

So many lines of qualitative analysis seem to point to an vertical eddy diffusivity of about 1 to 10 cm^2/sec in the deep ocean (beneath the thermocline) that I am getting bolder and more sold on my funny notions about the thermohaline circulation – but I simply cannot imagine what there is to produce so much turbulence in deep water, if indeed it is really there.

I now have about 18 months of nearly continuous temperature measurement on the bottom at 500 meters off Bermuda. This is in the top of the main thermocline. There is quite a lot of variation – especially short period – apparently 10-30 meter vertical displacements with a period of from 1 to 6 hours. I cannot explain this kind of internal wave (or granular structure floating by?) and wonder sometimes whether or not it is simply a local feature. My motto being, "when in doubt, publish" I will make up a blue report of the data so that you & Snodgrass can see it, and perhaps you will be able to tell me whether you see similar muck on your deep thermistors.

My very best to you
and Judy

—— Hank

May 21, 1956

Mr. Henry Stommel
Woods Hole Oceanographic Institution
Woods Hole, Massachusetts

Dear Hank:

I am returning Rouse's paper. It is definitely connected with what Towney Cromwell is doing, and he was aware of it. In a way, Towney confirms what might be the principal conclusion: an existing thermocline can propagate downward as a result of stirring in the upper layer.

Sincerely yours,

Walter H. Munk

WHM:es

July 27, 1956

Mr. Henry Stommel
Woods Hole Oceanographic Institution
Woods Hole, Massachusetts

Dear Hank:

I have come back this morning from the heat and smog of Los Angeles. I spent a couple weeks there taking a course in how to program an electronic computer.

There are two letters from you and a copy of a letter you wrote to Gordon Lill. Certainly temperature fluctuations below the seasonal thermocline such as you describe make it impossible to sample these adequately by going out every couple weeks and taking a deep BT. I would still suspect that shallow measurements (up to 1000 feet) taken once every two weeks are meaningful, even if they don't give the whole story.

When things settle a little I shall have a look at your report Reference 56-43.

Regarding Mr. Kurt Jacoby, I had originally thought he wanted me to proofread articles that had been prepared for his book, and I was willing to consider this. It later turned out that the editor was supposed to be responsible for selecting topics and finding people to write on these topics. This would take more time than I should like to spend on such an undertaking.

I quite agree with you about the untimeliness of a treatise on oceanography. You will recall this was my feeling when we thought about doing a book for Oxford Press together some years ago. It seems as if the subject matter is getting more untimely as time goes on.

I have no strong feeling on your suggestion on a volume "Modern Developments in Oceanography" with 20 to 40 scholarly review articles. I did like the recent volume dedicated to G. I. Taylor, which was somewhat along these lines.

With best regards,

Yours,

Walter H. Munk

WHM:es

July 5

Dear Walter:—

I now have monthly hydrographic data (T + S to 2000 m) for Bermuda since June 1954. You can get the Bermuda tide gauge data from Geodetic Survey. If you make computations on steric sea-level, ~~so~~ you can have a preview of what might be expected from the IGY program.

Shall I have copies made + sent to you?

— Hank Stommel

June wants them, so I wrote and said yes
Have been received

WOODS HOLE OCEANOGRAPHIC INSTITUTION
WOODS HOLE, MASSACHUSETTS

Dr. Walter Munk
SIO
La Jolla California

Dear Walter:

Mr. Kurt Jacoby visited me here the other day to talk about the proposed treatise in oceanography which he previously discussed with you. I said that I thought it was a bit premature, that the newer developments were in such a fluid state that a definitive treatise might better wait 10-15 years... at least until there is some evidence of crystallization of ideas.

Dr. Kullenberg tells me that Dietrich et. al. are actually preparing a Handbuch der Ozeanographie. I've written to find out about it.

On the other hand, it might still be advisable to consider a volume entitled " Modern Developments in Oceanography".... taking up where The Oceans leaves off. That might be a useful thing to do...get maybe 20-40 review articles..... really scholarly ones.... not like these interminable symposium articles......

Pass on to me any thoughts you may have .

Yours,

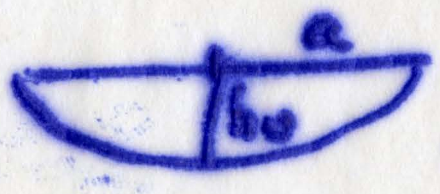
Hank

Dear Walter :— Aug 1, 1956

I plan to be in Bermuda part of the Spring of 1957. and can install a tsunami recorder or any other piece of gear you would like me to. I am putting down the 500 m. depth Vibrotron. I think I have enough dough to buy a tsunami recorder from you $10 if you think it worthwhile. We'd get a slight head start on the IGY that way. —

Many people have had a bit of intuitive difficulty in trying to visualize the standard treatment of edge waves within the framework of surface waves (Lamb 446 ff.) and since, on the shelf, the frequency is so low, have wondered why they aren't treated from the beginning like "tidal waves" — that is, using the hydrostatic approximation from the beginning. Have you noticed the dispersive nature of the waves at the edge of a parabolic basin (Tidal Wave chaps, Lamb p 292, equat[n] 14) where if we write $\lambda = \frac{2\pi}{k} = \frac{2\pi a}{s}$,

$$\frac{\sigma^2}{k^2} = gh_0 \frac{1}{k^2}\frac{2s}{a^2} = \frac{2gh_0}{k\,a}\ ;$$ then, $c^2 = \frac{g}{k}(\text{slope})$

if slope $\doteq \frac{2h_0}{a}$ 

.

Or again, if we start with the long wave formula for basin of variable depth (p 291, equat 2) and take a bottom of the form $h = lx$, where l is the slope, the coast at $x=0$. Now we seek a solution of the form $\zeta = \zeta_0\, X \cos(ky - \sigma t)$

(like edge waves), and we find that we must have $X = \frac{a}{a_0} e^{-kx}$ and that $\sigma^2 = g l k$, the only difference from the surface wave theory being, that $l = \tan\beta$ rather than $\sin\beta$ — in any case quite small. Since the amplitude diminishes with x, there is not any reason to worry about getting into trouble with the hydrostatic approx. for large x and k. Also, you will observe that there is a roughly intuitive explanation of the dispersive character of the waves. Waves with large $\underline{k}$ are confined to the shallowest water on the average, and hence must move slowest.

Your old friend

Hank (Stommel).

WALTER .... APOLOGIES.... I SEE BY IGY PROGRAM THAT EWING ALREADY HAS A BERMUDA VAN DORN TYPE LONG WAVE RECORDER INSTALLED..... THEREFORE THERE DOES NOT APPEAR TO BE MUCH REASON FOR ME TO PUT IN FOR ONE TOO.... ... SO PLEASE FORGET THE FIRST PART OF MY LETTER OF TWO DAYS AGO...HANK

WOODS HOLE OCEANOGRAPHIC INSTITUTION
WOODS HOLE, MASSACHUSETTS

August 3, 1956

Dear Walter:

Mr. Hassan wrote a letter several days ago to Roger asking whether he can obtain some financial support to help him visit Scripps this Fall. He has spent two summers at Woods Hole, this past one with me in my office here ... and he is finishing his Doctorate at NYU.

As you know, there is a serious bifurcation in oceanographic education/reseach in the East U.S. ... i.e., the educational, degree-granting places NYU, MIT, are really quite completely separated from the active research centers, WHOI, Lamont. Therefore, Mr. Hassan must make an extra effort to acquaint himself with the programs, goals, techniques, pace, etc. of the research centers, and I think he would find it very advantageous to visit Scripps as well as WHOI. Mr. Hassan does not ask to be enrolled as a student, and if he should therefore be barred from fellowship or scholarship aid, he would be quite willing to accept temporary employment on a project, where his mathematical training and skills would doubtless prove useful to SIO. Mr. Hassan received a small stipend from the Egyptian Govt., but it is a very slender resource to meet the expenses of a cross-country trip with wife and two children ... (he will drive)... and he would very much like to have a grant-in-aid or part-time employment.

Henry Stommel

Hank

August 16, 1956

Mr. Henry Stommel
Woods Hole Oceanographic Institution
Woods Hole, Massachusetts

Dear Hank:

I talked to Roger today about getting some support for Hasson. In the light of what you said I would like him to visit us for three months. But Roger did not commit any funds and said, in fact, that the Director's budget for this purpose has been pretty nearly spent for this year. He asked me, however, to find out exactly how much support Hasson would need, and then I would discuss this with him again.

Please ask Hasson to write me what would be his minimum requirements. Tell him that we would enjoy his visit very much and that we shall arrange so if it is financially possible.

With best regards,

Yours,

Walter H. Munk

WHM:es

September 19, 1956

Mr. Henry Stommel
Woods Hole Oceanographic Institution
Woods Hole, Massachusetts

Dear Hank:

I have not forgotten about my offer to run a spectrum on the Bermuda temperature records. If you don't mind, I would suggest that we consider it actively in about a month. By then I will have finished with the analyses of my long period wave records, and the desk will be clear. I hate to start something new before the present things are cleared up.

As far as the long period wave records are concerned I am enthusiastic with what one can do with such analyses. But the problems of analysis are touchy, and it is taking me a very long time to learn how to do things so that one can really have confidence in the results. I am beginning to understand why some of the earlier analyses yielded such strange answers.

By the way, I have two dates in Washington in November, one for the 16th and 17th and one for the 28th to 30th. The dates are preliminary, but if they hold could I park myself at Woods Hole in the in-between week. What I would like to do then would be to park in your office and just see what you and your friends have done and are doing.

With best regards,

Yours,

Walter H. Munk

WHM:es

October 10, 1956

Mr. Henry Stommel
Woods Hole Oceanographic Institution
Woods Hole, Massachusetts

Dear Hank:

I am leaving Washington on the Federal on Saturday night, November 10th and will get to Woods Hole whenever I get there on Sunday morning. I would like to stay there until Thursday, the 15th, leaving that evening, again on the Federal.

Could you be so very kind and see if I could stay at the Challenger Inn or some other nearby place. I don't want to bother Columbus Iselin or Admiral Smith, but if there is a convenient chance you might tell them that I would like to be there during these four days.

Perhaps you could talk about your interesting temperature data. By that time I shall be, I think, quite ready to start getting your problem on the computer.

With best regards,

Yours,

Walter H. Munk

WHM:es

P.S. We returned last night from long period wave recording on Guadalupe Island. The trip was pleasant and quite successful. John Swallow came with us.

December 27, 1956

Mr. Henry Stommel
Woods Hole Oceanographic Institution
Woods Hole, Massachusetts

Dear Hank:

Your survey of the Ocean Current Theory is a service to all interested parties. Would you consider it an endorsal if I say that I found much in it that was new to me? The relationship to the tidal theories, the exchangeabilities between the evaporation-precipitation and the wind-driven models are news to me. Figure 10 is wonderful. What are the vertical velocities to give the required internal mode that makes the Gulf Stream and Brazil current come out right? Are these vertical velocities then a part of the thermohaline convection between polar and equatorial regions? You have given the thermohaline people a new lease on life.

Carl Eckart is worried about the work of the great tidal theorists, and it may turn out that some of their results are wrong. But this is indefinite, and should not concern you at the moment.

I am left with the impression that one of the really essential things to be done at this moment is to look for empirical evidence concerning the various possible barotropic and baroclinic waves. Do you think it would be worthwhile to do the following thing: I could take hourly sea levels for a station for 3 - 10 years, pass it through Gorden Groves tide killing convolution, and end up with 1000 to 3000 mean daily levels, good to about 1 cm. From these I can get meaningful spectra for all periods between 2 days and 2 months. The spectrum has about 60 values, each good to a few percent. The work to do this is not excessive. As an example of the method I enclose spectra of sea level at Guadalupe Island for two successive days. I think we have been able to catch the normal modes of the island, but note that amazing and repeatable fine structure! The actual oscillations were less than a millimeter, the Vibrotron at 100 meter depth.

With regards to your remarks on internal modes, p. 8, it might help to point out that your evaporation-precipitation model (previously discussed) can be considered an internal mode of the hydrosphere (ocean plus atmosphere).

There are many small points. Perhaps I can save you a few errors if I list them.

Do send me a copy, if you can spare one.

Merry, merry Christmas,

Walter Munk

WHM:es
cc: R.S. Arthur

HENRY STOMMEL
Woods Hole, Mass.

Dear Walter :—

The prospect of having you in Woods Hole for a week in November is a very happy one indeed! — and we all (Fuglister, Veronis, Malkus, et. al.) hope some incident – such as extra committee meetings in Washington — won't keep you away, the last minute. You are most welcome here.

HENRY STOMMEL
Woods Hole, Mass.

I see by Tripper's Round Robin letter that you are now PAPA of TWO.

Congratulations.

Hank.

BERMUDA BIOLOGICAL STATION
ST. GEORGE'S WEST
BERMUDA

CABLE ADDRESS:
BIOSTATION

Dear Walter and Bob:

You will be interested, I think to hear a little sneak preview of the results of Swallow's and Worthington's joint ATLANTIS-DISCOVERY II survey of deep currents under the Gulf Stream using Swallow's neutrally-buoyant floats. I was along on the first two weeks.... Godry Volkamnn was on the second half --- arriving Woods Hole about April 5.

I am not sure of exact number.

About 7 deep floats in all quite clearly establish the reference level at about 2000 meters, with strong countercurrent underneath. At 2800 meters there were even some strong filaments moving as much as 20 miles in 66 hours.

This of course was a kind of crucial test of some of the pictures shown in my Survey article... and as you can well imagine goes a long way toward reducing the net transport of the Gulf Stream, and hence bringing it much more in line with the transports deducible from the wind-stress theory. Hooray for Munk!

I hope Swallow will give a full account of the expedition at the Toronto IUGG meeting.

Hank.

Henry Stommel

Dear Walter,—

Just a note to say that I would be very pleased to offer my home to any Scripps family that wanted to exchange with me next winter. It would save money for both of us. They could also use my car.

Incidentally, it is certain that the idea of a joint Rossby-Sverdrup volume does not fit in with the plans of the other Committee members.

I've had to cut my trip to Sweden short because Elizabeth's brother has leukemia, & she wants very much to spend this last Christmas at home with him & her mother. He'll die in a month or so — very very sad — he's only 18.

— Hank.

WOODS HOLE OCEANOGRAPHIC INSTITUTION
WOODS HOLE, MASSACHUSETTS

Dear Walter:-

Thank you for your letter and nice compliment. I heard from Rossby what a wonderful place the new Mexican University is — he visited Adem about a year ago. Have a good time!

This Nov 15, Fuglister & I are going to go together on the DISCOVERY II from W.H. to Gibraltar on the last of the IGY east west profiles. We will then have completed deep hydrographic sections across the Atlantic at every 8° of lat from 60°N to 16°S — and more work further south is planned for the Spring. We'll spend a little time at Stockholm & at Lacombe' place in Paris, and be back Jan 20.

I am at a bit of a loss as to how to make best use of myself in the next year. I think that the most useful

— and key — thing to do is to try to make direct measurements of the currents in deep water in the central ocean areas. Swallow is carrying the burden of the development of a longer-life neutral buoy — and perhaps in May-June we will be able to make some sea-trials. Actually there is little I can do to help this program along except to be enthusiastic and to go along.

Life with three small children seems to be rather difficult for Elizabeth, and she can't help but rub off her frustration a bit on me. The trip to Bermuda last year seemed to help her a bit. I have been toying with the idea of finding some excuse to take her & the family on another trip — to Stockholm or even St. Helena — to help her vary the monotony of caring for the little ones.

WOODS HOLE OCEANOGRAPHIC INSTITUTION
WOODS HOLE, MASSACHUSETTS

I sent the combined manuscript and your long letter to Bernhard and he promise to return it soon. Then we can try to submit it by the Feb 1 dead-line.

My very best wishes
Hank.

Dear Walter:

Thank you for your notes. I forwarded them to Veronis & Ichiye, and have been trying to reason out the trouble of the wiggles. As I see it the trouble is that we treat the western boundary by a different physical technique in the steady vs the transient problem. As the period $\rightarrow \infty$ the transient should merge into the steady solution, and in the interior, as we know from Sverdrup's analysis, this solution is determined independently of the western boundary condition. Thus I have been trying to set up a mathematical solution that is independent of the western boundary (like the figure in my Survey article - I forget the fig. no., but it was the last in the transient section). So far no luck.

I am very unhappy to hear about the drift of the pressure gauge - maybe this will kill my chances of getting the yearly cycle. 1 cm/day. My goodness! I sort of expected some such effect - but nothing so big.

Of course, I will be glad to get you the short period readings when I finally get all the equipment installed - probably about June or July

The cable will be in long before that but I have to buy the recording gear on next year's allotment.

My best wishes and thanks for your information.

Hank.

Jan 3 '57

Dear Walter,

Thank you very much for your kind words concerning the Survey article - and the helpful errors you found.

Your comments on the possibility of analyzing hourly tide data to get small amplitude second class waves interests me very much and I think you ought to try it. The ~~[illegible]~~ analysis for Guadalupe - which I herein return - is amazing! I take it that c/ks is cycles/kilosecond? And the period your refer to for the island is the circumisland edge wave?

Chicky, I and the babies plan to go to Bermuda on Feb 1 for three months. I am putting down a Vibrotron at 500 meters (I hope) and bringing in a cable to shore, with the hope of getting several years' pressure readings to compare with Elliott Stevens' tide gauge. Then perhaps I will have something a little more tangible to offer you and June to play with apropos of isostatic adjustment.

Your question about the magnitude of vertical velocities necessary to be consistent with transports in deep water of about 20×10^6 m^3/sec is a fair one — and it rather bothers me. I require vertical velocities at mid depths of about 10 cm/<u>day</u>. This is sort of big, and I worry about it off and on.

Swallow is bringing the DISCOVERY to the Gulf Stream in Feb 1957 to try direct current measurements of the deep current. He is going to take me along — and we shall soon find out if there is really a deep counter flow!

Yours —

Hank (Stommel)

February 12, 1957

Mr. Henry Stommel
Oceanographic Observatory
Biological Station
St. George's, Bermuda

Dear Hank:

We enjoyed the visit from Ketchum and Walden. They are awfully nice fellows and obviously competent instrumental people. I continue to be worried about long term instrumental drift, as far as you people are concerned. Frank has now had a Vibrotron in the water for several weeks, and the frequency has increased all this time. The rate of increase has dropped somewhat and now is equivalent to a lowering of sea level by 1 cm per day. One, of course, would gather that this is some sort of an aging process, and perhaps eventually things will settle down. In our work, dealing with periods of one hour and less, this is not too serious.

When you first get Bermuda records I should like to talk you into taking readings at fairly close intervals, say one every four seconds, for a day or two. This would give me a chance to have a look at the long period wave spectrum of another island. Perhaps we can talk about this when the time comes.

I am very concerned by the lack of theoretical model for the steric sea level program. You will recall that I thought about this at the time when I visited you at Princeton. On my return here I did work through the theory for a two layer ocean following your scheme, and the trouble I got into was that I came out with an extremely wiggly solution. You had previously remarked on this difficulty, and I have had no thoughts on what to do about it. Mohammed, too, has looked over these notes but could not think of an obvious cure. I would certainly welcome it if you or Ichiye would find it worthwhile to consider this problem. I wonder whether some notes of mine would be at least initially helpful. I am enclosing them.

Finally, about changes in the depth of the thermocline in the Atlantic and Pacific, I intend to keep my eyes open about internal waves at our San Clemente station.

With best regards,

Yours,

Walter Munk

WHM:es

OCEANOGRAPHIC OBSERVATORY
LOCATED AT THE BIOLOGICAL STATION
ST. GEORGE'S, BERMUDA

A FIELD STATION OF THE WOODS HOLE OCEANOGRAPHIC INSTITUTION. WOODS HOLE, MASS., U. S. A., DEVOTED TO SYNOPTIC STUDIES OF THE NORTHWESTERN SARGASSO SEA.

Feb 6.

Dear Walter:-

Clark and I and the family are spending the next few months in Bermuda partly for the sake of a "change" and for the fun of it, and partly to put in the vibration cable.

I have been wondering whether you or anyone else have been trying to set up a theoretical model for the steric sea level IGY program. At the equator there is little steric level change, but in mid-latitudes there is quite a respectable seasonal change; therefore there must be meridional pressure gradients at levels near the surface. We must therefore expect zonal geostrophic currents. The meridional coastal barriers prevent purely zonal currents from flowing indefinitely and so at some stage the influence of the coast must be felt. I should think that this would force a readjustment of the mass field, and that the bottom pressure might then begin to change. But I do not have a definite understanding of the time scale involved — if the influence of the coast is propagated at close velocity of the non-dispersive long baroclinic second class wave it is only about 2 cm/sec and would not penetrate very far into mid-ocean in one season. Is anyone working on this problem? It is almost a moral necessity on account of the IGY program. I've tried to interest Veronis, but he is all excited about Malkus's turbulence theory right now. Perhaps I could interest Ichiye in it.

///

Another thing, as you have seen from the Bermuda thermometric cable data there is a great deal of vertical oscillation (about 20 m. amplitude waves) at 500 m in this part of the Atlantic. I suspect that this activity is somehow a cause of mixing at mid-depths, and as

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you know, the primitive theoretical efforts of Veronis and myself suggest that the depth of the thermocline in the ocean is proportional to the (vertical eddy diffusivity)$^{1/4}$. One would expect that in an ocean like the Pacific with about half the depth of main thermocline as that of the Atlantic, that the eddy diffusivity would be markedly less, and the amplitude of internal waves smaller. Perhaps this is something that you may be able to keep an eye open for when you begin to get data from your new 10 mile cable.

My best wishes
Hank

February 15, 1957

Mr. Henry Stommel
Oceanographic Observatory
Biological Station
St. George's, Bermuda

Dear Hank:

I want to return to the problem of a "theory" of the annual steric departures. June is writing her thesis on the seasonal changes in the heat content of oceans, on a global scale. Her main effort is to (1) describe these from BT and Nansen data, and (2) compare the results to what has been inferred by the energy budget method, using radiation, evaporation, etc., the usual stuff. She is not yet finished, but the result seems to be that for averages taken over the subtropical and subpolar gyres of the North Atlantic and North Pacific there is no convincing difference between the two methods (the overall mean ratio is 1.4, but can be explained away by various means).

Suppose we accept that such changes of heat content within the entire gyres are due to local heating. Then this explains the seasonal departures in steric levels as well. All we need the wind to do is to account for the non-steric departures, i.e., recorded minus steric.

We now have some Russian sea levels in the Arctic (given to Roger at Goteborg by Kort) and it seems as if the whole Arctic and subpolar region is lower by 20 cm in spring compared to fall. My rough calculations, which I sent you, indicate that in amount this can be accounted for by changes in the Westerlies. In winter when these blow the hardest, the water is driven southward, and reaches its lowest level 3 months later.

This leads to the following hypothesis: Energy flux through the surface accounts for the <u>steric</u> departures in sea level, because it can't do anything else. Wind stress accounts for the non-steric departures, because it hasn't time to affect the steric departures. The result of wind stress is a movement of water from the polar and subpolar gyre to the subtropical gyre during winter, and reversed during summer. There you are.

This says nothing about how the slopes of the sea surface are maintained.

What do you think?

Walter H. Munk

WHM:es

HENRY STOMMEL
Woods Hole, Mass.

Aug 20 '57

Dear Walter:-

Just a note of condolence to you on the death of Sverdrup. I know he meant a great deal to you, and that you doubtless feel his loss keenly.

Following so closely upon Rossby's death it was a great shock to us here at W.H.O.I.

Yours as ever

Hank.

Nov 7 '57

WOODS HOLE OCEANOGRAPHIC INSTITUTION
WOODS HOLE, MASSACHUSETTS

Dear Walter:

Your suggestion that I visit Scripps next winter sounds like great fun to me. and I mentioned it to Elizabeth and she seemed to brighten up at the prospect.

If there is someone at Scripps who would like to bring his family to Woods Hole we could trade homes, cars, etc. and in that way hold costs to a minimum — just travel really. I did that last winter with Sutcliffe in Bermuda & it worked fine.

During the period Nov 15 to Jan 8 my address will be NIO, England — although I'll go to Stockholm for a week or so to see old Defant who finally is going ahead with his book (!) Pergamon Press has taken it on.

Hank.

P.S. Your suggestion about combining Sverdrup & Rossby does come rather late — after all the Swedes (Bolin et. al.) have been designing

Nov 7 '72

the whole thing around Rossby, + they have translated a long manuscript of his to start the book off. But I phoned Geo. Platzman about it + he promised to write the other committee members. So we'll get their reaction later. —

Hank