January 2, 1951

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

Dear Hank:

I am going out to sea in a couple of days and I shall make some hurried remarks to your manuscript before I leave.

It would seem to me that your classifications on Page 2 might involve an "evaporation type" as one of the four types. This would warrant some modification of the discussion on Page 3. In the case of the river flow type, the estuary water would be fresher; in the case of the evaporation type, it would be more saline than the open ocean.

Your definition of F on Page 5: I prefer one of the alternate forms you previously used involving the tidal period T rather than tidal frequency.

Regarding Page 6: Isn't the reason why B differs possibly related to a suitable definition of H. For example, if it were defined as the depth of mixing instead of the estuary depth, would the results be more consistent?

On Page 9 a bracket is missing in the first equation.

On Page 11, what is ϕ^2 very much smaller than?

The application of Rossby's ideas is certainly interesting and your results in that connection seem encouraging. However, I don't really understand the physics behind it at all clearly and I am therefore at a loss to make any definite comments.

Have I written you previously that I hoped you might take your general flushing equation as it appeared in the Compendium and make an initial value solution for some assumed distribution of concentration with x times zero.

With very best wishes for the coming year,

Yours,

Walter H. Munk

WHM:es

WOODS HOLE OCEANOGRAPHIC INSTITUTION

WOODS HOLE, MASSACHUSETTS

January 15, 1951

Dr. Walter Munk
Scripps Institution of Oceanography
La Jolla, Calif.

Dear Walter:

Thank you very much for your long and thoughtful letter and comments of a few weeks ago. I hope you had a good sea trip.

There is a matter which occurred to me yesterday which I would like to mention casually to you, just to get your off-hand impression as to whether it is a screwy idea or not.

As you know, the secular lengthening of the day by about 1/1000 of a second per century, as determined from lunar theory, is usually accounted for by the dispersal (*dissipation*) of the rotational energy of the earth through the mechanism of tidal friction.

Jeffreys and Taylor have studied the dissipation of energy by tidal friction in very shallow seas where the currents are strong and the dissipation of tidal energy largest. The numerical data seems to suggest that the frictional dissipation of energy in the Bering Sea accounts for the major portion of the necessary loss of rotational energy. About 1.5×10^9 hp is calcluated as being lost in the Bering Sea as compared to 2.1×10^9 hp, as computed from Fotheringham's study of ancient eclipse records.

Now, it seems to me that tidal energy could also be dissipated by lateral friction, especially in the areas along the western sides of the worlds' oceans, where there are large lateral eddies developed in the strong currents there, or possibly over the entire deep ocean.

Suppose we consider the magnitude of frictional terms on the tide in the two instances. For shallow water, where vertical friction predominates, the rate of dissipation of energy per unit volume is of the form

$$\mu_1 (\partial u_1/\partial z)^2$$

In the case of the deep ocean, the rate of dissipation of energy per unit volume is of the form

$$\mu_2 (\partial u_2/\partial x)^2$$

where μ_1 is the vertical eddy viscosity and μ_2 is the horizontal eddy viscosity.

The volume of sea water in the shallow seas over which the vertical friction dissipates tidal energy can be represented by V_1 . The volume of water in the deep ocean, where tidal energy is dissipated by large scale eddies associated with ocean currents is V_2 ; the average depth of these two different domains of dissipation are H_1 and H_2 respectively and their surface (geographic) areas S_1 and S_2 .

Very roughly speaking, we can evaluate the magnitude of the velocity gradients as follows:

Assume first in the shallow water the velocity gradient to be constant, and the maximum flow at the surface to be u_{01} we obtain

$$(\partial u_1/\partial z)^2 \cong (u_{01}/H_1)^2$$

In the deep ocean, if we assume the wave length of the tide to be λ , and the maximum velocity of the current to be u_{02}, we obtain

$$(\partial u_1/\partial x)^2 \cong (u_{02}\, 2\pi/\lambda)^2$$

However, the quantities u_{01} and u_{02} are not entirely independent because of mass continuity, and therefore are related roughly by

$$u_{01} H_1 \cong u_{02} H_2$$

Therefore, the ratio of total tidal dissipation by vertical friction in shallow seas to that by horizontal friction in the deep oceans

$$\frac{V_2 \mu_2}{V_1 \mu_1} \frac{\left(\frac{\partial u_2}{\partial x}\right)^2}{\left(\frac{\partial u_1}{\partial x}\right)^2}$$

is

$$\frac{S_2}{S_1} \cdot \frac{\mu_2}{\mu_1} \cdot \frac{40 H_1^3}{\lambda^2 H_2}$$

Suppose that $\mu_2 = 10^8 \text{ cm}^2\text{sec}^{-1}$ $\mu_1 = 10^2 \text{ cm}^2\text{sec}^{-1}$

$H_2 = 5 \times 10^4\text{cm}$

$H_1 = 10^4\text{cm}$

$\tau = 5 \times 10^8\text{cm}$

Then the ratio is

$$\frac{S_2}{S_1} \quad \frac{1}{300}$$

This means, that for the dissipation due to lateral friction to play a role of equal magnitude to that of vertical friction, the area of deep water dissipation needs to be about 300 times that of shallow water (Bering Sea).

Now, do you think that this could possibly be the case? Only about 3% of the whole ocean is shallow, and the Bering Sea is only a fraction of that.

Do you think this idea is worth pursuing further? It would be a big job to do carefully.

Sincerely yours,

Hank

Henry Stommel

HS:ds

1. Idea good
2. Not sure what is to be accomplished by care
Arctic Basin has shallow seas
3. Good agreement fortuitous

WOODS HOLE OCEANOGRAPHIC INSTITUTION
WOODS HOLE, MASSACHUSETTS

February 2, 1951

Dr. Walter H. Munk
Scripps Institution of Oceanography
La Jolla, Calif.

Dear Walter:

Thank you very much for your last letter. I am inclined to agree with you that it would be next to impossible to carry out an estimate of tidal energy losses over the world's oceans due to lateral eddy viscosity. As a matter of fact, upon looking over Jeffreys' treatment of tidal friction in "The Earth" I am rather surprised now that I think of it, that he was able to convince himself of the precision of his results. I wonder if it might not be a worthwhile thing to bring up this question in a one paragraph letter to "Nature".

Yes, I did receive an invitation to the Atmospheric Turbulence in the Boundary Layer Symposium. I will have, by the time of the Symposium, enough qualitative information on the occurrence of wind streaks on Ashumet Pond under varying conditions of wind, insolation, evaporation, to make an interesting presentation.

At present I am spending the first three days of every week in New Haven teaching a dreadfully elementary course on Meteorology and Oceanography. The rest of the week I am at home at Woods Hole. Although it is something of a nuisance to have to go down to New Haven all the time, it is great fun to see Gordon and the others there. On the other hand, the Bingham Laboratory is so quiet and peaceful, as compared to W.H.O.I., that I get a lot of work done there.

My best to you all at Scripps.

Yours very truly,

Henry

Henry Stommel
Central Bureau

HS:ds

27 February, 1951.

Mr; Henry Stommel,
Woods Hole Oceanographic Institution,
Woods Hole, Mass.

Re. Surface tension of Natural water.

Dear Henry,

I notice that in WHOI Ref 51-6 plans are being made to measure the surface tension of natural water surfaces by the spreading oil-alchohol method of N.K.Adam, with the purpose in mind of measuring tension depression in slicks. Walter and I will be most interested in the outcome of these experiments as they tie in to work in progress here. I presume that you are the power behind this project, and that you are planning the work for Ashumet Pond. The only previous work of this kind on lakes seems to be that of Yvette Hardman (Wisc acad sci Trans 33:395-404.)which is rather fragmentary and gives no seasonal variation. This last variation might be considerable as there is evidence that slicks depend on natural oils of biological origin. The method is sensitive to about 1 dyne/cm and I am not sure that this will reveal a tension drop in the slicks on your pond. In San Diego Bay, the drop in slicks that are caused by "downwelling" is of the order of 10 dynes, but there are other observable slicks in which the drop was apparently too small to be measured by this method. It is interesting to note that boats cause sufficient depression to be easily measured, the effect being due to compression of the surface film as the boat sails through it.

Well the reason for this long tarry-diddle is that we are planning a regional survey of the surface tension of the ocean to see how far at sea we can find the film that causes slicks. The slicks themselves have now been reported as far out as 400 miles (rarely on the High Seas). The plan is to use Adam's method at the outer limit of its sensitivity ie for a depression less than 5 dynes/cm. We worried some about the statement Adams has made on two occasions that his method might not be accurate where there was considerable adsorption at the surface of dissolved surface-active substances. I received a reassuring reply from Adam recently (which seems to be lost in what I laughingly call my file)on this point saying that he saw no reason for trouble on the ocean and suggesting a simple test to eliminate doubt as to this point. What worries me more is that failure to find ST depression does not mean that there is no film, but merely that it is not compressed. That would mean that we would have to dream up some way to put the surface under a (known) degree of pressure before we could find out whether there was fim present. I dont feel very happy about working with a hydrofilm balance at sea.

To come to the point: do you think it would be a sound idea to investigate the presence or absence of film on the sea by a uniform method on the Atlantic and the Pacific ~~a~~, and if so, would you be interested in joining with us in this study. Walter and I expect daily to hear that we have some Aifforce money to study the Physics and Chemistry of the Air Sea Boundary and so we might be able to help finance the work on the Atlantic. One problem on your side interests me very much. During Operation Cabot, Bill Kielhorn took some remarkable photos from the air that showed the Gulf Stream water much more ruffled than the adjoining slope water. The difference could be due to the presence of ~~a~~ film on the more productive slope water which might damp out the ripples, or the difference might be due to the existence of a boundary-layer of stable air over the colder slope water which protects the water suface from rippling by the turbulent wind. It would seem possible to clear up this point quite quickly by observations on the spot. This, by the way, has some possible military sigificance that we need not dwell on here. Some time I'll write to the Central Bureau about it and forward the letter by armoured car. Perhaps we could combine the Langmuir balance and the method of Adam using a Langmuir trough to comress a sample of seawater by a known amount and Adam's drops to read the tension . ~~This would be somewhat like Agnes Pockels original method where she used camphor~~. The advantage would be that the tension would be measured be a system not bothered by the accelerations of a bouncing ship. Another way might be to measure how much the film has to be compressed to damp out standard ripples generated by a tuning fork This trick has been used by Gorter & Seeder (Kolloid-zeitschrift <u>58</u>:257-260 (1932)) and gave pretty good quantitative results.Anyway, let me have your ideas. It seems somewhat foolish for us to pursue this independently of one another if we can get more significant results by a common effort.

By coincidence, while I was writing the above Walter came in with your letter to him of the 19th. He says to tell you that there is mass transport associated with capillary waves. It seems to me that as you say there must be some agency compressing surface film if it is to damp out ripples. The basis of my internal wave work was just this that the internal wave can sometimes do the trick. But there must be other agencies equally or more effective, particularly the wind at higher windspeeds. What we must find is some agency exerting <u>differential</u> stress on the sea surface, otherwise no compression occurs. I don't see how capillary waves could compress the film as a whole, so I imagine you are considering the local compression at the crest of each little wave. Yvetté Hardman measured the drop in surface tension at the crests of gravity waves, and I have repeated the observation. It is quite easily seen by Adam's technique. Undoubtedly the same thing must occur at the crests of ripples although the duration of the compression would be small compared to the modulus of decay of the ripples so that it is hard to see just how this would work. Half the time a ripple train would be subjected to the decay rate for compressed film and half the time it would decay at the rate for clear water. The most serious difficulty would be to explain why this effect only shows up at wind speeds much higher than required to form ripples. It seems somewhat more likely that the film is compressed on a larger scale by some variation in the wind stress and that the ripples make this situation visible by their reaction to the compressed state of the film. Otherwise it would seem necessary to assume that whenever there were ripples there would be compressed film damping, and I do not take it that this is what you have found. Furthermore, it seems hard to see why this effect would not affect the water surface uniformly instead of in streaks. If the streaks were the result of mass transport, one might imagine that the streaks would be normal to the wind rather than parallel. Could it not be that the wind structur is indeed cellular at these speeds but that this structure may be independent of convection. After all, we keep observing spiral modes of flow in many natural phenomena not all of which arise from convection. What they all do seem to have in common is the presence of some sort of boundary shear. Someday the answer will probably come into better focus than it is at present.

Please excuse the jumbled form of this letter. I sat down to drop you a brief note about our plans for ST measurement on the sea and have been rambling on and on. We all wish you were out here so that we could argue about these things over a mug of beer.

Yours,

August 6, 1951

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

Dear Hank:

Is it possible that I left two books, Byerly "Fourier Series" and Carlsaw and Jaeger "Heat Flow" at Woods Hole? Perhaps your secretary would know.

We have been partly successful in our glitter work at Monterey, but the weather isn't really good enough, so we are leaving for Hawaii next week and will be gone for about a month.

With best regards,

Yours,

Walter H. Munk

WHM:es

December 7, 1951

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

Dear Hank:

It has been so long since I have heard from you that I naturally wonder what and how you are doing.

Our work at Hawaii was reasonably successful and I hope that we shall have some numbers available in a couple of months. In the meantime I have tried to complete a few miscellaneous problems that have been lying around for some time. One has to do with the wobble of the earth induced by winds, one is on the absorption of nutrients by sinking diatoms. We also must get our annual changes in sea level out.

Michael Longuet-Higgins has been playing around with additional second order terms. I am trying to get him interested in doing some work on wind stress over ripples. Bill Van Dorn has been measuring the stress from the inclination of the 800 foot long yacht basin at Mission Bay. He can measure sea level changes to 0.1 mm. Soap knocks it down quite a bit.

Do let me hear from you some time.

Yours,

Walter H. Munk

WHM:es

WOODS HOLE OCEANOGRAPHIC INSTITUTION
WOODS HOLE, MASSACHUSETTS

December 12, 1951

Dr. Walter Munk
Scripps Institution of Oceanography
La Jolla, California.

Dear Walter:

I am very glad to hear how well your work is coming along. I am engaged in trying to run three projects at present. The first is the study of estuarine circulation. I think Harlow Farmer and I have really come quite a long way and are writing a report on two layer flow and other topics, which is growing to telephone directory size.

I am including in this letter a partial table of contents, and I am sending by slow post a few of the sections which are in more or less final form. There is still plenty of experimental work to do in the flume, but I believe I pretty well understand most of the phenomena in the first four chapters anyway. I have worked out a lot of examples, especially for Chapter 2, which are not quite ready yet for duplicating. I will send you these, bit by bit, as they come off the Multilith press. Eventually you should have a full copy. I imagine the final form of this work will not be ready for about six months.

I am also working very hard on the Infra-red these days, but have not gotten very far yet.

There is a new man by the name of David Frantz working for me now, who is going to try to put instruments out in the deep ocean. Our program is roughly as follows: first, to put temperature recorders out on the Continental Shelf for a year; second, to put a current and turbidity recorder on the Continental Shelf in a canyon (shades of Kuenen!), and thirdly, to place temperature and pressure recorders on a seamount, more or less in the path of the Gulf Stream. This program, we believe, is planned in steps of ascending difficulty. (The primary difficulty, of course, is to get the instruments back--not to design or build them).

Michael Longuet-Higgins and I did some work on the theory of the G.E.K. while he was here. I hope you will get him to give some talks on it at Scripps--say a four or five lecture series. Melvin Stern here has done a beautiful job on the solution of the G.E.K. theory for an ocean current of rectangular section, and also has discovered some very important integral relations from which one can obtain the total transport of an ocean current from <u>surface</u> G.E.K. and Loran measurements.

Sincerely yours,

Henry Stommel

HS:ds

ON THE NATURE OF ESTUARINE CIRCULATION

by

Henry Stommel and Harlow G. Farmer

Preliminary Table of Contents.

May 20, 1952

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

Dear Hank:

We are all very curious whether you have had your offspring yet. Please be sure to let us know.

I was of course terribly sorry to have missed you in Washington. Your summary of the Gulf Stream theory was well written and well received. My only objection is that you were overly modest with regard to your own work.

Thank you for spending some time with Bill Van Dorn. He came back from Woods Hole with his morale and self-confidence uprated. He talked to you about a possible loan of a long period current meter. I should be interested in making some measurements during the coming fall and winter for a period of about three months and would therefore very much like to borrow the current meter some time this summer so that we can gain some experience with its use. Do you suppose that we might carry out a modification whereby temperature might be recorded at the same time and on the same film?

What I would like to do is to moor the current meter about 100 m. beneath the surface on the eastern side of the California current and measure the shoreward eddy flux of momentum and heat. I would hope this would throw some light on the actual processes involved that lead to numerical values of 10^7. Two Coast Guard Officers, who are students here, are interested in this, and I think this would be a very good problem for them. Please let me hear your reaction.

There is also a matter with regard to a proposed symposium on oceanographic instrumentation, sponsored by ONR, in which I was invited to serve as moderator on the topic "Air-Sea Boundary Processes." I know no earthly reason why they have selected me for this, but as I have not turned them down so far I am afraid I shall have to try my best. One principal reason for being willing to accept this was that I was informed by Mr. Gibbs that you and Al Woodcock and Bob Reid were to be the discussants. I hear from Bill that you were somewhat uncertain whether you would come, not having been invited in due course. I might add that the way in which I received all information so far is by a few form letters and that I have had no part in formulating the program or naming the discussants. However, I do hope very strongly that you can come and not let some trivial bureaucracy interfere with what could turn out to be rather useful.

Yours,

Walter H. Munk

WHM:es

HENRY STOMMEL
Woods Hole, Mass.

July 28th, 1952

Dear Walter :-

I was very interested & pleased to get your letter.

Malkus & I have just returned from a two ship Gulf Stream Cruise. We made detailed velocity-vs-depth measurements in the Stream & the results look really good.

Enclosed is a copy of the talk —

My best to you & Martha
Hank

A Brief Review of Gulf Stream Investigations, 1949-1951

by

Henry Stommel

Woods Hole Oceanographic Institution
Woods Hole, Massachusetts

1952 is the fiftieth anniversary of the publication of Ekman's mathematical theory of the effect of the wind stress on the movement of ocean water. This famous theory has had a manifold influence on oceanographic science. It introduced oceanographers for the first time to the power and elegance of mathematical tools. It also introduced for the first time, so far as I am aware, the then new concept of eddy viscosity. The work of Osborne Reynolds on fluid turbulence was only a few years old. The theory of von Zoppritz, based on the assumption of laminar flow and no rotation was shown to be untenable.* Moreover, Ekman's theory convinced oceanographers that the Coriolis force is not negligible, even for phenomena confined to a shallow surface layer. The Ekman spiral has had a prominent place in every oceanographic text and lecture course. But in the real ocean this celebrated spiral has proved every bit as elusive a phenomenon, if not so zealously sought after, as the Holy Grail

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*Because of eddy viscosity and the earth's rotation it need not take an eternity for winds to produce ocean currents. This fatal neglect of the turbulent nature of ocean currents arose once again during attempts to estimate the dissipation of tidal energy to account for the acceleration of the moon, but was corrected by G. I. Taylor in his famous study of tidal dissipation in the Irish Sea. But it was Ekman's wind current theory which introduced oceanographers to the concept of eddy viscosity.

Thus Ekman's theory is a very good example of the difficult position which theoretical research occupies in a field so unexplored and little understood as the physical science of the sea. The oceanographic theory forces recognition of certain physical processes in the ocean. It suggests and leads to certain critical types of measurement, but the beautiful and over-simplified structures, such as the Ekman spiral, which it predicts on the basis of its analytic approach, often simply do not seem to exist. Oceanographers who have considered measuring Ekamn spirals have found themselves at a loss to find areas in the ocean whose area is sufficiently infinite, where the winds have blown steadily in one direction long enough for transients to disappear, but not long enough for the water to begin to pile up.

My reason for introducing this talk about recent investigations of the Gulf Stream with these remarks about Ekman's theory is intended to illustrate, in terms of a theory familiar to all, the ambiguous position which any theoretical study must occupy in a science such as ours, where so many of the important facts have not yet been observed. With these preliminary remarks on the inherent dangers in the uncritical acceptance of the results of theoretical investigations which involve simplified assumptions about the physical nature of the ocean which we are by no means justified in regarding as truth, I now turn to the period 1949 - 1951.

Unquestionably the most distinguished theoretical contribution during this period has been Munk's 1950 paper, "On the Wind-Driven Ocean Circulation". Built on the exploratory hypotheses of Sverdrup, Reid and Stommel, Munk's theory accounts for many of the gross features of the general ocean circulation, and some of its details, on the basis of the observed mean annual winds. Munk's theory is significant as the most successful example of the solution of a "mathematical field problem." The solution yields a transport of the Gulf Stream of the proper order of magnitude and leads to the conclusion that the horizontal circulation of the ocean is induced by the stresses exerted by the winds. (A)

The success of this theory is so evident that it has spurred further attempts in many quarters at deriving refinements. Attempts have been made to derive comparable results for a baroclinic model and to obtain a solution of the current system resulting from fluctuating winds, but little success seems to have attended these mathematical adventures. An extension of the model to oceans of triangular shape was made by Munk and Carrier in 1951. Moreover, there has been a tendency toward the extension of Munk's theory to situations not originally contemplated in its formulation. The theory was originally visualized as giving a statistical picture of the gross asymmetry of the wind driven ocean circulation. It is a mistake to try to fit the instantaneous synoptic picture of the ocean circulation at any one time with this statistical average. Professor Rossby has frequently mentioned his conviction that the extreme narrow, filament-like structure of the instantaneous Gulf Stream cannot possibly be the result of the planetary effect alone, and he seems to believe that there are some additional processes which are not included in the planetary theory, but which are needed to produce the high degree of intensification observed in the instantaneous synoptic picture. Rossby has called attention to analogies of the Gulf Stream with atmospheric jet streams. An example of the kind of current intensification process which Rossby has in mind is the sharp shear generated at the southern boundary of a zone of intense lateral mixing (Rossby, et al, 1947). We should be careful to make a clear distinction between the climatological mean or statistical average features of the ocean circulation, which the planetary theory purports to explain, and the instantaneous picture which we develop from the data taken on a single cruise.

Again, we must remember that the planetary theory is limited in many ways: it cannot tell us what the surface velocities of ocean currents are, and it is faced with a very serious difficulty in explaining the Antarctic Circumpolar Current, a dilemma which still remains to be entirely resolved, although Munk and Palmen (1951) have advanced one tentative explanation. The fact that the model of the planetary ocean theory does not involve the heterogeneous density structure of the real ocean is an indication of how it is not intended to describe or explain many of the features of the oceanic circulation. We may also ask, is there enough large scale eddy motion in the Gulf Stream to produce the lateral eddy viscosities required by Munk? Stommel (1951) attempted to show, on the basis of a chart by Fuglister, that the eddy viscosity in the climatological mean Gulf Stream is of the order of magnitude required by Munk, but I do not think it is very convincing. Inasmuch as the dispersion in position of the Gulf Stream has been shown by Fuglister and Worthington, (1951) to be chiefly associated with large scale wave motion rather than eddy formation, it seems likely that the diffusivity computed by Stommel is much too high. It seems very difficult to conjure up enough large scale turbulence near Hatteras to justify Munk's figure for the lateral eddy viscosity.

The CABOT cruise of 1950, which has been partly reported by Fuglister and Worthington (1951) has clearly demonstrated the existence of wave motions along the Gulf Stream with a wave length of about 300 kilometers, an amplitude of at least 50 kilometers, and a rate of progression toward the east of about 11 miles per day. These wave-like contours were observed on earlier cruises as well (Iselin and Fuglister, 1948), and we must now recognize them as regular features of the Gulf Stream. Further downstream these travelling waves appear to grow so large in amplitude that they break off in some fashion to form eddies. During the CABOT cruise the process of the birth of an eddy was thoroughly observed.

Rossby (1951) has made an investigation of theoretical processes leading to streakiness in ocean currents. Properly speaking, this paper was mainly a study of the vertical concentration of momentum in stratified currents, analogous to the Froudian analysis long used by hydraulic engineers in their treatment of open channel flow phenomena. Rossby has shown that the observed velocity distribution (in the vertical) of the Gulf Stream coincides with the computed profile of minimum momentum transfer. Rossby has promised a future paper to show that associated with the vertical concentration, there must also be a horizontal concentration, as a consequence of the earth's rotation.

Stern has investigated the vertical distribution of velocity, using the unintegrated form of the vorticity equation in the western current. He finds explanation for the shallowness of the current which depends upon a mechanism quite different from Rossby's. Stern finds reasons to suspect that the values of horizontal eddy viscosity used by Munk are several orders of magnitude too high to give the proper depth of the stream; he finds that the point of maximum velocity shifts toward the left of the current with depth; that increases of the width are accompanied by decreases of the depth of the current.

Preliminary field work in measuring the electric potential induced by the earth's magnetic field in the moving ocean water led von Arx (1950) to the hope that the G.E.K. would be capable of measuring ocean surface currents in the deep ocean to a high degree of precision. The results of the many hundred G.E.K. measurements made during the CABOT cruise have given disappointing evidence that this is not true. The theoretical studies on electric potentials induced in the sea, by Longuet-Higgins (1949), and Stommel, (1948), have not been particularly helpful for the analysis of G.E.K. records, but the recent work of Stern (as yet unpublished) on the electric potentials induced by currents of rectangular cross-section, promises to aid in the interpretation of G.E.K. measurements. Just what is the best way of using this powerful tool at sea and interpreting the results of its measurements is still very puzzling.

Malkus and Stern, in a paper to be published in the Journal of Marine Research (1952) have revealed some valuable integral theorems relating the potentials measured on opposite sides of an ocean stream to its total mass transport. In order to test and utilize these theorems, a joint project of potential measurement between Key West and Havana is being organized for this summer, as a combined effort of the Western Union Telegraph Co., the Woods Hole Oceanographic Institution, and the Marine Laboratory of the University of

Miami. Other special measurement projects, which are still in the planning stage, but which might be mentioned at this time, are the free-floating telemetering buoys being developed by Walden and von Arx, and the deep-sea anchored recording instruments by Frantz and Stommel. The use of the airplane for rapid Gulf Stream scanning has been stimulated by the amazing aerial photographs made by Kielhorn on the CABOT cruise.

Since the original studies of the fluctuation in the transport of the Gulf Stream by Iselin (1940) and Montgomery (1938), Fuglister (1951) has made a study of the monthly resultant current speeds in ten segments of the western North Atlantic from published data of the United States Navy Hydrographic Office. The annual variations of the current speeds, particularly in the Florida current and south of Hatteras, in the Caribbean Sea, and in the Trade Wind areas, are very marked, amounting to as much as 50% off Guiana, and 30% off the Florida coast. The maxima in these areas occur in the summer, and appear to be correlated to seasonal fluctuations in the wind with no appreciable time lag.

A very enlightening study of the irregularities in the circulation around the Bermuda-Azores High has been made by Chase (1951) in an unpublished Technical Report. The annual changes of pressure distribution, the frequency, persistence and detailed nature of interruptions to the normal circulation pattern in the Bermuda-Azores High pressure cell is exhaustively described. This work of Chase will rank high in importance when theoretical studies of transients in the wind-driven circulation can be successfully carried out.

As we have learned more about the regions of the stream north of Hatteras, we have gradually found ourselves groping further and further toward the east. Because of the tendency of the current to break up into eddies, as shown by the CABOT cruise, it has become more difficult to see how the current can continue as an uninterrupted band of water as it extends into the North Atlantic drift. Surveys of the North Atlantic drift have frequently shown isolated strong currents, and these were interpreted by Iselin (1936) and Sverdrup (1942) as indications of a branching of the Gulf Stream from a single large current into a number of diverging individual currents, the branching taking place somewhere east of the Grand Banks.

In 1951 Fuglister made the revolutionary discovery that he could draw very consistent and convincing current pictures, assuming that the stream in these areas was made up of discontinuous, unconnected filaments of high velocity, a form of synoptic analysis which has come to be called the "Multiple Current Hypothesis". These multiple streams run more or less parallel to one another, and bear only a superficial similarity to the venous structure of Bache's charts of 100 years ago. Fuglister's multiple stream type of analysis avoids the somewhat disconcerting tendency of the ocean current to follow the track of the ship, which has been evident in much of the recent detailed survey work of the Gulf Stream system.

Worthington, on last summer's cruise of the ALBATROSS III in the North Atlantic drift, found current patterns confirming the Multiple Stream Hypothesis. The Multiple Stream type of analysis has proved itself a serious rival of the continuous current school, and has served the healthy function of revealing the ambiguous nature of any analysis.

The discovery of these intense filaments of current in the West Wind drift poses many questions: are these filaments permanent features of the ocean, or do they drift about, some decaying and others growing; what are we to make of them in the light of Munk's theory; are they decay products or fragments of the Gulf Stream, or are they associated with the imparting of eastward momentum by the west winds; is it possible that they represent a mechanism by which large amounts of eastward momentum can be transported out of the West Wind area without increasing the mass flux, and are there similar multiple streams in other parts of the ocean?

The existence of these multiple streams may, if they prove to be a common feature of ocean circulation, have an important bearing on the present wind-driven, theoretical model. One of the grand features of the planetary theory is that all of the kinetic energy of the wind-driven circulation is dissipated in the intense western current. This extreme localization of dissipation of energy may be considerably modified if high shear zones exist in other parts of the ocean. For example, the existence of multiple streams in the Southern Hemisphere West Wind drift may resolve the dilemma mentioned before by providing a suitable brake for the Antarctic Circumpolar current. In view of the multiple streams the regions of energy dissipation in the ocean may be more widespread than contemplated in the planetary theory.

There is a curious problem involved in the horizontal circulation of the ocean in the West Wind drift area which I only vaguely apprehend, and for that reason may experience difficulty in communicating to you. It is this: the net eastward transport of mass is determined according to the wind-driven theory entirely by factors depending upon the wind, yet this transport is associated with a transverse density distribution, which must be largely determined by thermal and evaporative exchanges across the sea surface at different latitudes. Does this partial independence of the mass distribution from the wind stress distribution impose a constraint on the total mass transport in the west wind drift in such a fashion that an additional process (such as this tendency for concentration of momentum in narrow filaments) is necessary to carry the excess momentum imparted to the water by the wind out of the area? Because his statistical picture of ocean circulation yields such weak currents in the West Wind drift, Munk has neglected inertial terms in all areas of the ocean (except in the Gulf Stream itself where he has made a study of them: Munk, Groves and Carrier, 1950). When additional surveys of these areas have been made it may be interesting to re-examine the possibility of neglecting inertial terms in the West Wind drift.

I have avoided making any comparison between the Gulf Stream and the Kuroshio. The most recent Japanese work on the Kuroshio, (Nakamiya and Suda, 1950; Hidaka, 1949), has developed a picture so completely dissimilar to that of the Gulf Stream that there is at present no hope of comparing the detailed structure of these two streams. The statistical average circulation in the North Atlantic and North Pacific is similar, as required by Munk's theory, but there is little agreement so far between the detailed descriptions of these two major currents. Whether this difference is due to a discrepancy in the methods of analysis or the differences of instrumental techniques used in our survey work from those used by the Japanese, or whether there are fundamental differences in the detailed structures of these streams, is something which we will have to wait to answer.

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WOODS HOLE OCEANOGRAPHIC INSTITUTION
WOODS HOLE, MASSACHUSETTS

December 4, 1952

Dear Walter

I sent you a manuscript recently in which I tried to work out a dispersion law for meanders in the Gulf Stream. In talking over this idea with a mathematician the other day, I found that I made a terrible mistake in evaluating the roots of the equation on page 7, so that the table of phase velocities and growth rates is all wrong. Equation

$$(1-p)^3 - \frac{f^2 + gk^2\gamma D}{k^2 U^2}(1-p) + \frac{f^2}{k^2 U^2} = 0$$

is very interesting however. Unless I have made another mistake, I think it shows that there is only a narrow range of wavelengths which permit instability. This range centers around the wave number

$$k_0 = f / \sqrt{2g\gamma D}$$

and waves much longer or shorter are ~~un~~stable. Curiously enough, the limiting conditions at which the meander of wavenumber k_0 is the only unstable wavenumber in the whole spectrum is that

$$U^2 = g\gamma D$$

This result is so simple that I wonder if I have not been working on a trivial solution somehow, and will have to reinvestigate the whole matter. I'm afraid I have gotten all mixed up and confused, and I'll write you again later if I ever get things straightened out. The problem has its talons in me though now for sure.

Yours truly,

Hank Stommel

Note: There are few people here at Woods Hole at the moment with whom I can discuss this — so I've duplicated a few copies to circulate for criticism, suggestions, and ideas — Henry

The Role of Meanders in the Wind-Driven Circulation

Henry Stommel

December 17, 1952

1. Introduction

I intend to sketch in only the broadest strokes a possible mechanism by which meanders may be produced by, and in turn, control certain features of the wind-driven circulation of an enclosed ocean. The model of the ocean considered is only a very crude approximation to the true state of affairs and can be justified perhaps only by the exploratory and suggestive nature of this investigation.

The point of departure of our study is the fact that Munk's (1950) value of the total transport of the Gulf Stream is independent of the lateral eddy viscosity A over a large range of values of this parameter. For small values of A the velocities of that part of the circulation in Munk's solution which corresponds to the Gulf Stream are much too high and the Stream is too narrow as compared to observation. On the other hand, large values of A cause a broad slow Stream. Munk has chosen $A = 5 \times 10^7$ cm^2 sec^{-1} as a value which gives a cross stream profile of transport most nearly in accord with observation. There is little other ground, however, for choosing this particular value of the coefficient of lateral eddy viscosity. Munk's theory does not deal explicitly with a stratified ocean.

Rossby (1951) has shown that dissipative forces acting upon a current in a stratified medium tend to produce a concentration of velocity at the surface: in particular, in a two layer system (densities ρ and $\rho - \Delta\rho$) in which the upper layer is moving with velocity U, and the bottom layer is at rest, there is a tendency for the interface to slope upwards downstream and the velocity to approach a critical value

$$U_c = \sqrt{g \frac{\Delta\rho}{\rho} D}$$

where D is the thickness of the upper layer. The role of this critical condition at transitions in open channel flow has recently been studied by Stommel and Farmer (1952). We will now proceed to develop the perturbation theory for meanders in a stratified medium and will show these perturbations are all stable (and hence unlikely to develop into observable meanders) as long as $U < U_c$. When U approaches U_c a very narrow range of wavelength of perturbation becomes unstable. It is a remarkable fact that this unstable wavelength corresponds closely to the observed dimensions of a meander growing into an eddy (Fuglister and Worthington, 1951) and that this meander is stationary, which also is in accord with observation.

2. A simple meander theory

Suppose that there are two layers in the ocean, the lower of density ρ, and the upper of density $\rho - \Delta\rho$. The lower layer is very deep as compared to the upper, so that we shall assume that the accelerations and horizontal pressure gradients in it are vanishingly small at all times.

In the undisturbed state a steady current of velocity U flows in the x -direction in the upper layer. Associated with this is a horizontal pressure gradient transverse to the stream

$$fU = -g \frac{\partial d}{\partial y}$$

where d is the elevation of the free surface above what it would be if there were no motion. We denote the depth of the upper layer by D. From the assumption of no motion in the lower layer we see that

$$\frac{\partial D}{\partial y} = \left(1 + \frac{\rho}{\Delta\rho}\right)\frac{\partial d}{\partial y}$$

or

$$fU = -g\left(\frac{\nu}{1+\nu}\right)\frac{\partial D}{\partial y}, \qquad \nu = \frac{\Delta\rho}{\rho}$$

We now suppose that small perturbations u, v, the velocity components, and h and h', the elevation of the free surface and depression of the interface respectively occur. We assume further that these quantities are independent of y (!). The perturbation equations are written in the form

$$\left(\frac{\partial}{\partial t} + U\frac{\partial}{\partial x}\right)u - fv = -g\frac{\partial h}{\partial x}$$

$$\left(\frac{\partial}{\partial t} + U\frac{\partial}{\partial x}\right)v + fu = 0$$

$$\left(\frac{\partial}{\partial t} + U\frac{\partial}{\partial x}\right)(h + h') + D\frac{\partial u}{\partial x} + v\frac{\partial D}{\partial y} = 0$$

If we assume the perturbation to be of the form $e^{i(kx-\sigma t)}$ we obtain the following frequency equation

$$(1-p)^3 - \frac{[f^2 + gk^2\gamma D]}{k^2U^2}(1-p) + \frac{f^2}{k^2U^2} = 0$$

where $p = \frac{c}{U} = \frac{\sigma}{kU} = p' + ip''$, the real part p' of which is the ratio of velocity of the propagation of the wave, and the imaginary part p'' of which gives the instability of the wave motion.

For the particular range of parameters involved no one of these terms is small compared to the others. It is convenient to rewrite the equation in the form

$$y^3 + 2 = \mathbb{P}y$$

where

$$\mathbb{P} = 2\left(\frac{f^2}{2k^2U^2}\right)^{1/3}\left(1 + \frac{gk^2\gamma D}{f^2}\right)$$

and

$$y = \left(\frac{f^2}{2k^2U^2}\right)^{-\frac{1}{3}}(1-p)$$

see Jahnke + Emde

The roots of this equation are all real provided $\mathbb{P} \geq 3$, thus there are three types of stable wave possible. If $\mathbb{P} < 3$ there is a region of unstable waves. It is, perhaps, these unstable waves which interest us most because they seem to be the most likely to form on the Stream. If one regards $\mathbb{P}$ as a function of k^2 (for the moment f , D , and γ being regarded as fixed) we see by inspection that $\mathbb{P}$ passes through a minimum at some value k_0 , which presumably corresponds to the most unstable wave, and is found to be

$$k_0^2 = \frac{f^2}{2g\gamma D}$$

and in the case of marginal stability

$$U_0^2 = g\gamma D$$

Substituting these conditions for the "just-unstable" wave into the frequency equation we obtain that $p' = 0$, thus the wave is stationary.

3. Application to Gulf Stream theory

A realistic value of $\Delta\rho/\rho$ is 2×10^{-3}, and we may take the depth of the thermocline in the Stream as $D = 200$ meters. The critical velocity is 200 cm sec^{-1}. At this velocity a meander with a wavelength of 180 km becomes unstable and begins to grow. During the process of growth this meander diverts momentum of the Stream in a direction normal to its mean course. If the eddy detaches from the main Stream this momentum is dispersed in the flanks of the Stream, and from mixing length ideas we may calculate the order of magnitude of the local gross coefficient of lateral eddy viscosity due to this eddy:

$$A_e \cong U \frac{2\pi}{k_0} \qquad = 3.6 \times 10^9 \text{ cm}^2 \text{ sec}^{-1}$$

This value is much in excess of the value used by Munk. If it is recalled, however, that Munk has assumed a uniform viscosity over the entire length of the Gulf Stream System, whereas the observations of the CABOT cruise indicate the existence of only one detaching eddy, the discrepancy is not so glaring. If the value of A_e (acting on only 90 km of the length of Stream) is averaged over the entire length (about 4500 km) of the Stream a value of 7×10^7 cm^2 sec^{-1} is obtained which agrees well with Munk's average value 5×10^7 cm^2 sec^{-1}.

This theory imposes a very great stability on the climatological mean Gulf Stream because if on one hand the eddy viscosity diminishes the Munk theory requires the velocities to increase, but this causes unstable meanders to appear, and hence increases the lateral viscosity; on the other hand too much lateral turbulence tends to broaden the Stream and diminish the velocities below the critical, so that unstable meanders cease to form, thus reducing large scale lateral eddy motion. Although I am fully aware of the extreme crudeness of the theory employed here, I think perhaps it may provide a basis for reconciling the ideas of Munk and Rossby and that it may provide an understanding of the role and mechanism of Gulf Stream meanders. The Rossby theory describes the instantaneous picture of the Stream (the width of the instantaneous Stream being a consequence of Rossby's critical condition and Munk's fixed transport). In this theory the meanders are part of the mean motion (not turbulence). The Munk theory is an average picture only, considering meanders as turbulence. Perhaps it is a mistake to try to force the width of the current profile from Munk to be that of the instantaneous stream. If we simply assume A to be larger than 5×10^7 we can obtain a broader, weaker mean stream which fits the average Gulf Stream charts better.

a current transporting $70 \cdot 10^6$ m^3 sec^{-1} at 2 m sec^{-1} and 200 m depth must be about 200 km wide.

April 8 53

Dr. W H Munk SIO

La Jolla California

Dear Walter

In regard to the telephone conversation yesterday...

In deceing what values of parameters to use in Mr. Veronis' transient wind-driven ocean circulation theory it seems advisable to use the numerical values given by you in the 1950 J. of Met. paper so far as possible:

width of ocean 6500 km β as 1.9×10^{-13} per cm per sec

$A = 5 \times 10^{7}\ cm^{2} sec^{-1}$

Mr Veronis is using a wind stress distribution of the form

$$\tau = -(W + \Gamma \sin \omega t) \cos ny$$

where τ is the stress, W is the annual average amplitude, Γ is the annual perturbation amplitude, n is the wavenumber. What values do you suggest for these three constants, and where would you place the northern and southern boundaries of the ocean?

As to the two layer model... Do you think that the depths of the two layers might be 500m and 4000 km and $\Delta\rho/\rho = 3 \times 10^{-3}$? Please suggest other values if you think these are not quite the thing. Will you please communicate your thoughts oh these matters to

Mr. George Veronis, Grad. Div. Apllied Mathematics,
Brown Univ. Providence 3, R.I.

The computations are suffieiently intricate to preclude the likelihood of ever seeing the results for a wide variety of choices of the parameters so we ought to make the best possible choice of numbers right at this time.

My best wishes — Hank
(Stommel)

WOODS HOLE OCEANOGRAPHIC INSTITUTION
WOODS HOLE, MASSACHUSETTS

May 5, 1953

Dr. Walter H. Munk
The Scripps Institution of Oceanography
La Jolla, California

Dear Walter:

During the month of March I tried to make some measurements of the surface currents set up by the wind in the deep ocean remote from land under circumstances which as nearly as possible were similar to those assumed for the Ekman spiral. As you will see from the enclosed memorandum, which I certainly do not intend to publish until I have obtained very much more data, I ran into technical difficulties involving the relative size of ship and waves which made it impossible to gather enough data to be at all useful. However, I was able to obtain enough information to become surprised and somewhat dismayed by the complexity of the water motions in a region which at first hand should appear most simple. Knauss N. Holiday

Most of my effort at present is going into trying to devise methods and means of getting continuous series of data of the wind-driven currents offshore of Bermuda. These are purely engineering problems, and although somewhat formidable, I think can be solved; but this preliminary information that I have obtained, as woefully incomplete as it is, does suggest that even if I do obtain a continuous series of measurements of wind-driven currents in the deep ocean, I will have quite a lot of difficulty in explaining them in a rational way.

The primary difficulty, as I see it, is the presence of what appear to be irregular inertial motions of the upper layers of the ocean. These we are led to expect from Fredholm's transient solution of the Ekman problem in which the momentum imparted to the water by the wind is gradually diffused downward and gradually the transient inertial motions are damped out by vertical friction alone. In the Ekman model of the deep ocean where the latitude is everywhere the same, these inertial motions should always add up to a simple rotating (cum sole) vector with a period of 12 pendulum hours, and although we see some evidence of such rotary inertial motions such as shown in Figures 1a and 1b of the enclosed memorandum, the inertial motions are often apparently aperiodic as shown in Figure 1c, Figure 2, or as might be inferred, although not certainly so, from the peculiar mess of observations in Figure 3. Joe Reid

Now it seems to me that the complex, nearly-random nature of the motions in the upper layers might be explained by the dispersion of inertial motions originating from areas of the ocean of different latitude. After all, the wind is not uniform over the whole ocean and it seems to me in the case where the wind stress is applied to only a certain area, even in an infinite ocean, that inertial motions should disperse from that area. This is not, strictly speaking, a generalization of the Cauchy-Poisson problem to a rotating plane but it appears to have some of the properties. If these nearly-random features of the inertial motions are a common feature of observations as I accumulate more of them, I shall really be at a loss to discuss them quantitatively. Therefore I wonder if you have any idea of whether or not some of the more recent results of meteorological thinking might be useful to me. For example, I have heard indirectly that in the practical problem of numerical weather forecasting a good deal of attention has been given to the study of the spread of a point disturbance in various simplified models of the atmosphere. Perhaps some of these models are similar to a homogeneous ocean and the work that has been done on them might shed some light on the physics of the dispersal of inertial motions in a homogeneous ocean. P.L.C. Thomson

I certainly do not mean to press you for an immediate answer. If any suggestions or ideas do come to mind, however, I should certainly like to hear of them.

Yours very truly,

Hank

Henry Stommel

HS:ep

Enclosure

I already know of Cahns 1945 paper in J. Met.. This has constant f however.

By the way – do you have available tabulated ~~forms of~~ values of the Fredholm integral solution? The ~~depth~~ one for the surface is the ordinary Fresnel integral, but I do not recognize the form for any arbitrary depth.

1. Your Current meter
2. Coherence Study on CAPRICORN
3. Current drag on "

WOODS HOLE OCEANOGRAPHIC INSTITUTION
WOODS HOLE, MASSACHUSETTS

May 22, 1953

Dr. Walter H. Munk
Scripps Institution of Oceanography
La Jolla, California

Dear Walter:

Just a note to thank you for your letter about wind-driven surface currents. I most certainly hope that this background kind of surface velocity is not the result of the super position of a number of traveling dispersed inertial disturbances. It will make everything so complicated if it is. There is always a possibility, however, that in the period September-October we might get some nice clear-cut motions dispersed from a hurricane passing near to Bermuda.

During the months October through January I am hoping to make a continuous series of current measurements within a radius of 150 miles of Bermuda. I will use an anchored buoy and also some drifting ones. The chief problem is securing enough boat time so that I am in hopes of being able to release drifting buoys near the island of Bermuda and then having them radio in information for a period of several weeks, during which time they will drift far away from the island (and then they will be sunk).

Since I have such difficulty in getting the use of boats for any protracted period, I think it will be easier to release a number of these drifting buoys in sequence and not try to retrieve them. In that way I hope to be able to get information on currents some distance from Bermuda without having to go out there. We will have our first full dress buoy test by the end of June when I get back from making the Azores cable measurements.

It is very interesting to hear about your study of the coherence of surface currents made on CAPRICORN with G.E.K. on two vessels.

Best of luck on the annual changes in sea level. Gordon Groves was here and told me some of the baffling mysteries encountered such as the large amount of missing ocean water. Maybe there is a drain hole in the bottom of the ocean, after all.

Yours truly,

Hank

Henry Stommel

HS:ep

July 1, 1953

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

Dear Hank:

I finally got around to looking over your notes on the Gulf Stream meanders. I really don't know enough about this problem, but if it should turn out that only a narrow range of wave length is unstable and that this is in fact the range that has been observed, then you have certainly found something very interesting.

Perhaps I should mention to you that there has been a rather radical change in my personal life. Martha and I were divorced after I returned from CAPRICORN Expedition and I have married Judith Horton.

Yours,

Walter H. Munk

WHM:es

The Hydrosphere

Dear Walter,

Thanks a lot for the note & news. I will be around W.H.O.I for awhile until Sept 15. If you can possibly come to Bermuda from Sept 15 on I sure hope you will do so. You would like it a lot. I will be doing wind-driven current measurements. Please come to see us soon. We would love to have you both. We (Ruth + I) will be there til the end of January. There is plenty of room there for visitors. The round trip fare from N.Y. is \$95.00/100. There is no passport required.

I have 20 buoys already built. They have wind vanes, anemometers, current meters, etc.

that send out radio transmission every 3 hours with the dope. We are going to let them drift seaward from Bermuda.

BEAUTIFUL BERMUDA
BECKONS TO YOU

Your old friend
Hank

November 27, 1953

Mr. Henry Stommel
Oceanographical Laboratory
Biological Station
Bermuda

Dear Hank:

I am very glad you sent me your letter on the preliminary results on Bermuda and they look interesting indeed. My only offhand reaction is that the extent to which the current can follow the fluctuating wind (transient time constant) might depend critically on the depth of the mixed layer. If this is so a reasonably good knowledge of the density structure at the time of these experiments would be important. I am very interested in hearing further results and I hope you will write more letters such as the one I have just received.

We have just completed a somewhat amusing study on internal waves in the atmosphere. We found out about it by accident, having arranged for properly damped wind and pressure recorders to supplement the tsunami recording. It turns out that there are sometimes rather pronounced waves in the inversion layer that correspond very closely to long internal waves centered at the thermocline. On seven occasions last year, the wind as recorded at the surface was actually to a large extent nothing but the orbital velocity associated with these atmospheric waves.

Sincerely yours,

Walter H. Munk

WHM:es

I found this most interesting. AA

Arthur RSG

Eckart CE

Cox CSC

Groves GWG

please return to me

PAGE 1

Eckart
Arthur

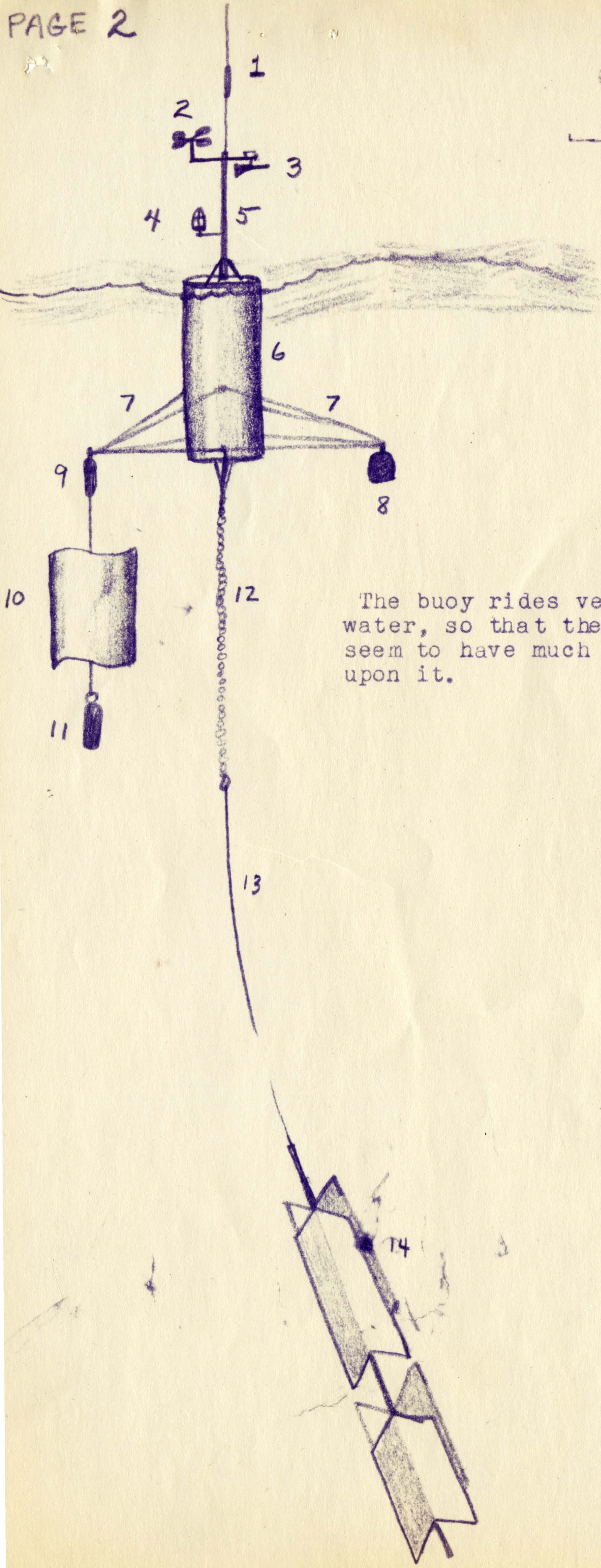
Oceanographic Observatory
c/o Biological Station
Bermuda
December 8, 1953

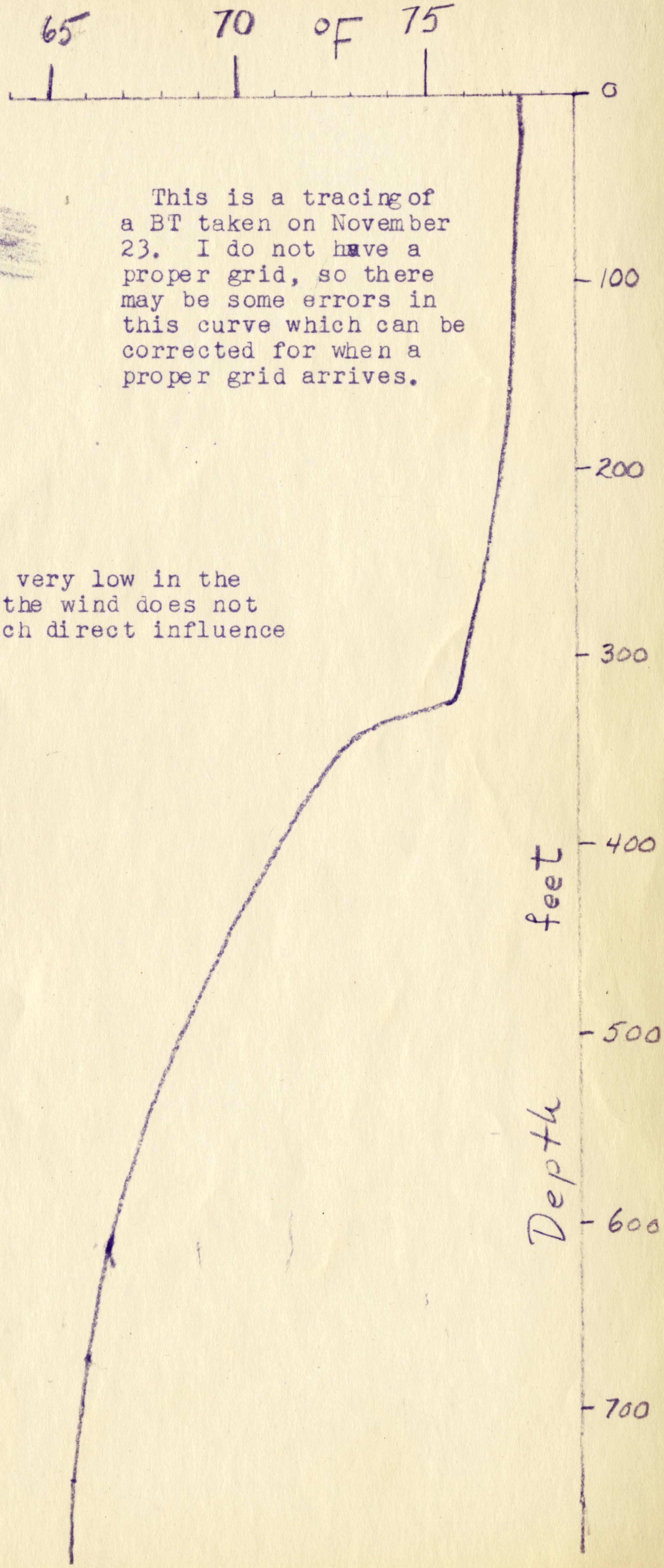
Dr. Columbus O'D Iselin
Dr. R.B. Montgomery
Dr. Walter H. Munk

My earlier letter to you described the results of measurements from the first drifting buoy that got successfully into deep water. This letter will describe the data obtained from another such buoy (number 2-C) which operated from November 23 until early on December 4, a total of about 15 days, when it apparently was sunk by gunfire as a suspicious looking object perilously near to the Big Three Bermuda Meeting. There have been a fearful nuisance of warships weaving about offshore for the past weeks. I put another buoy out on the 5th, but with a weaker riding light.

The RDF ing on Buoy 2-C has been awkward; the second RDF has just arrived in the airplane and ought to improve things a lot. However, the chart on the bottom of this page should give a rough indication of where the buoy was throughout its life. We put it off about 4 miles north of the northernmost part of the reefs and it drifted <u>into</u> the wind for several days until it got ENE of the island, and from the diminished strength of the signal, appeared to be at least 20 miles off the 1000 fm line. Then the signal grew stronger until it seemed very nearby and then suddenly it ceased abruptly.

Before giving the data, perhaps I should take this opportunity of describing the buoy itself a little. On page 2 is a figure showing the shape of the buoys used. (1) is an antenna six feet high (2) is an anemometer of the log type (3) a wind vane giving wind direction relative to the buoy. (4) is a navigation light (5) is a two inch diameter aluminum mast also six feet high (6) is a five foot high, two foot diameter steel buoy inside of which there are approximately 450 lbs of radio and battery equipment; (7) are outriggers (8) is a counterweight to balance the weight of the (9) log and (10) sheet metal rotor and (11) weight which measure the flow of water past the buoy. (12) is a length of chain and (13) is wire rope down to a (14) sheet metal cross or drogue. The entire arrangement rides not unlike a small vessel at a sea anchor. The motion of the surface layer over the deep water tends to orient the buoy with the left hand outrigger extended in the direction of flow of the upper layer relative to the deep layer. This direction is determined by a remote reading compass. The information from the meteorological instruments and current logs and compass is transmitted by radio to a shore installation where readings are made on a three-hourly schedule. At Mr. Iselin's suggestion, further readings will be made more frequently.

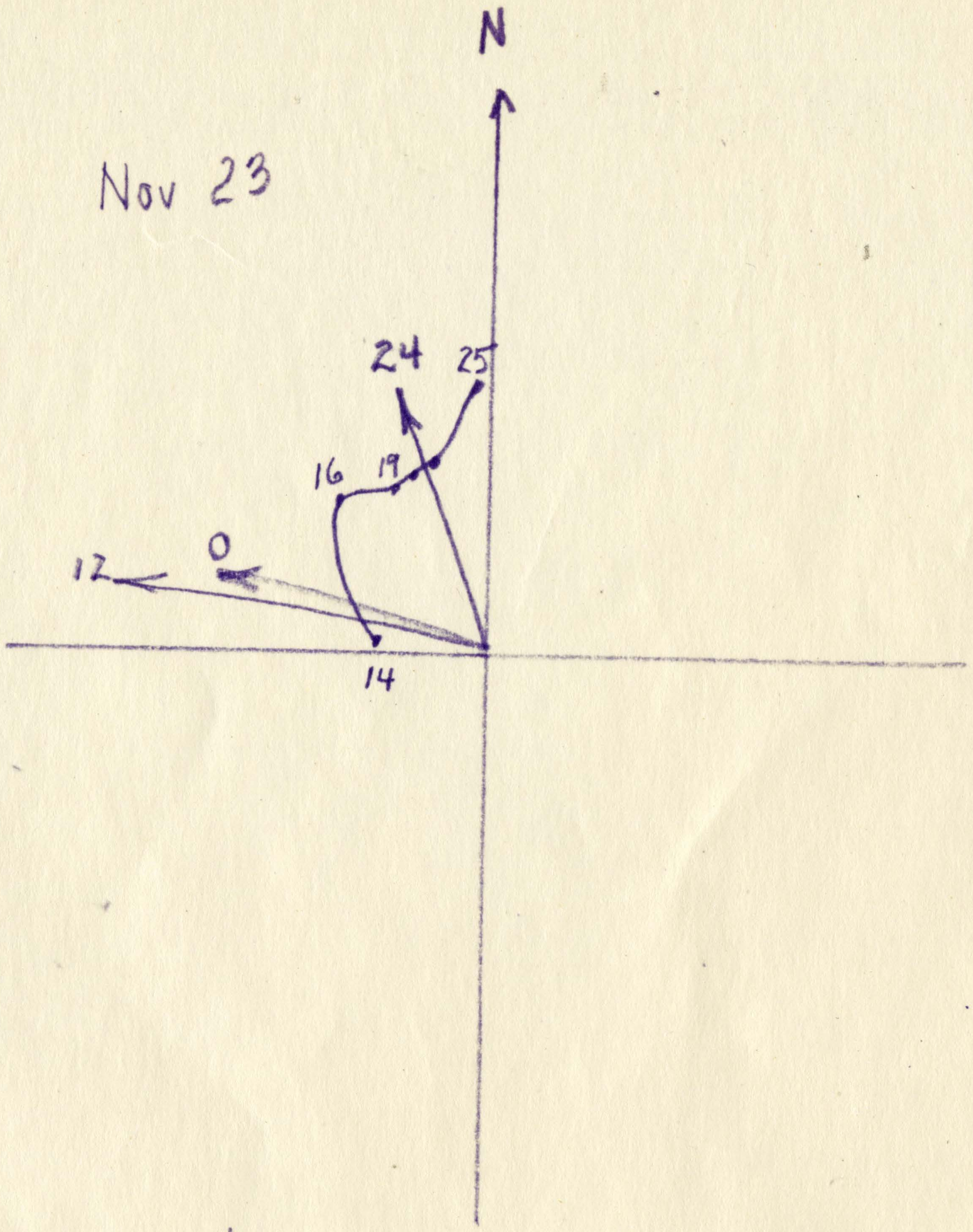


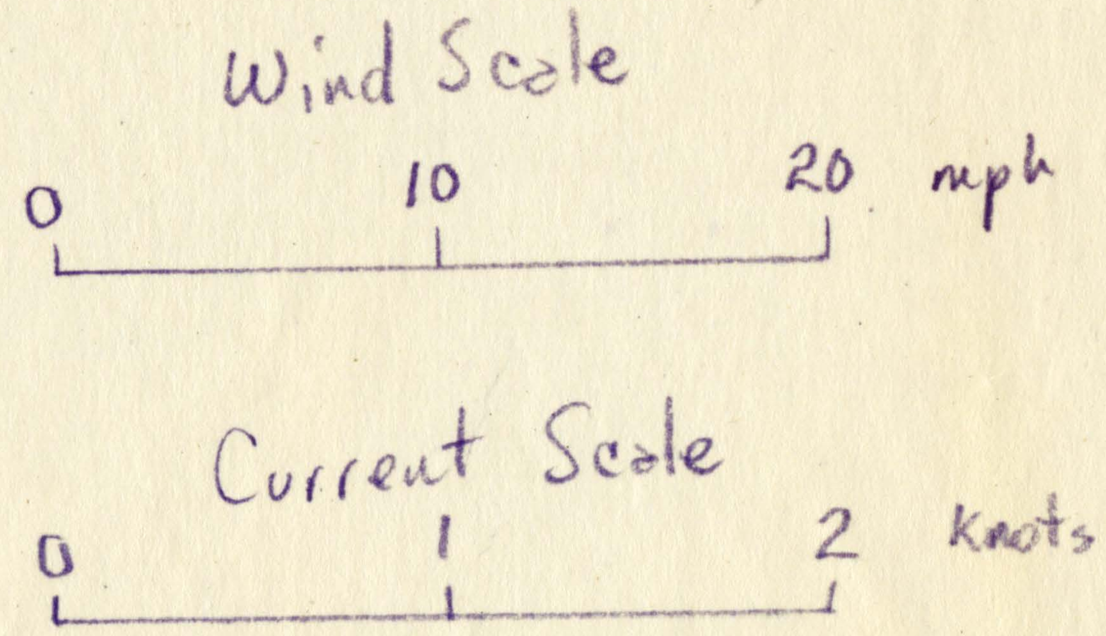


This is a tracing of a BT taken on November 23. I do not have a proper grid, so there may be some errors in this curve which can be corrected for when a proper grid arrives.

The buoy rides very low in the water, so that the wind does not seem to have much direct influence upon it.

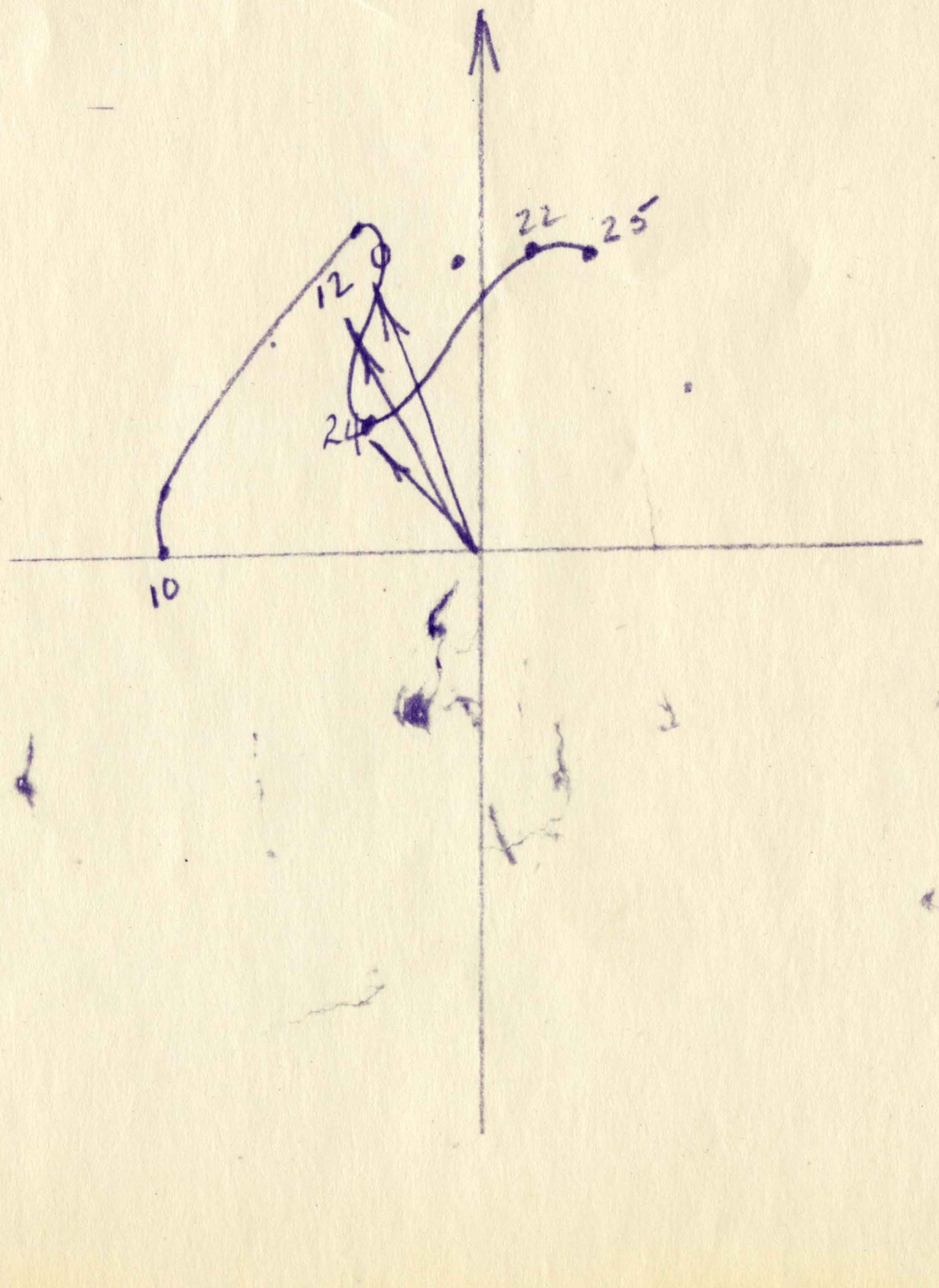
Nov 23



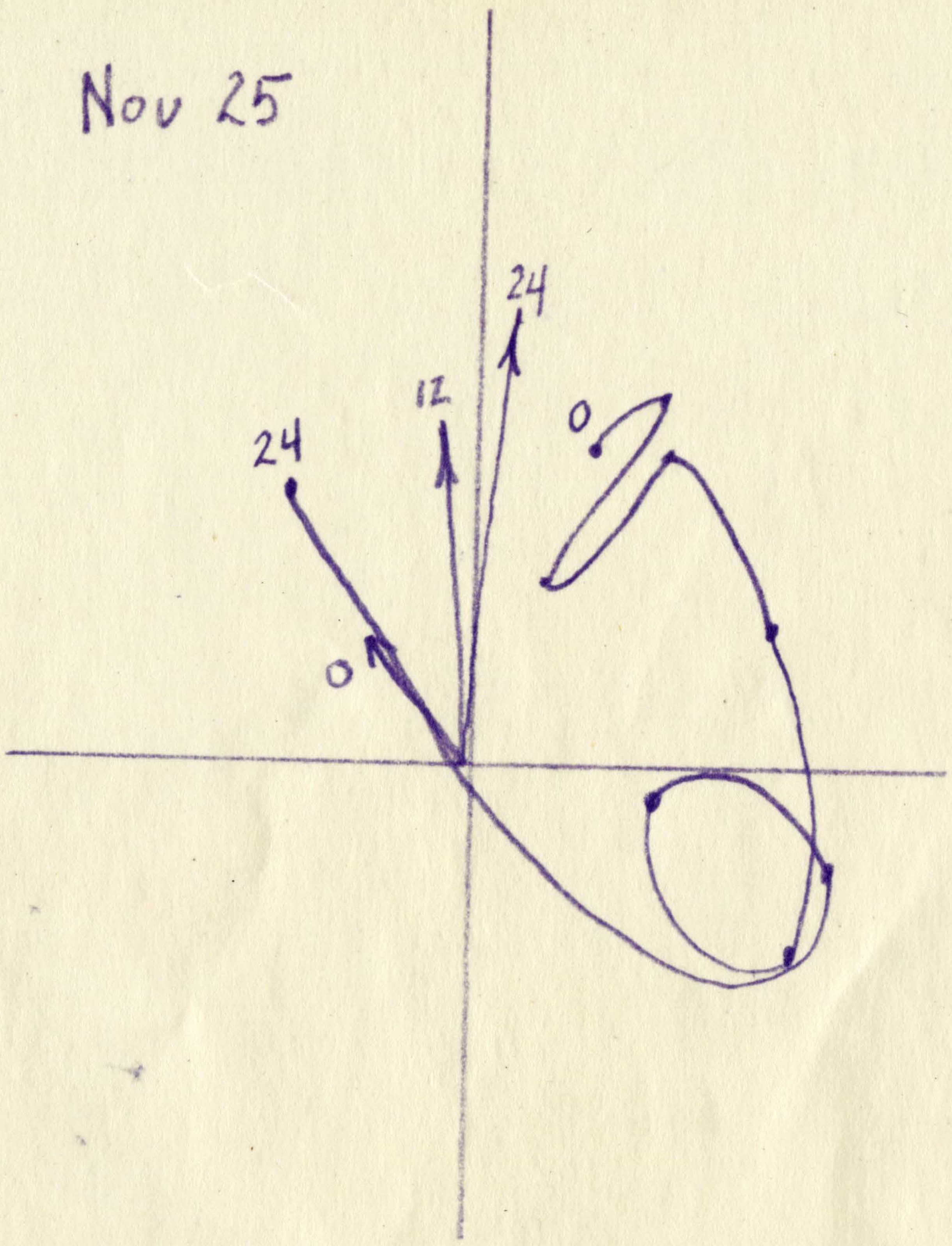


For the three days previous to Nov 23, when the buoy was launched, the wind blew more or less steadily from the East, a rather rare phenomenon here, at about 12 mph. During the afternoon of the 23rd it shifted toward the South. The current records begin at 1400.

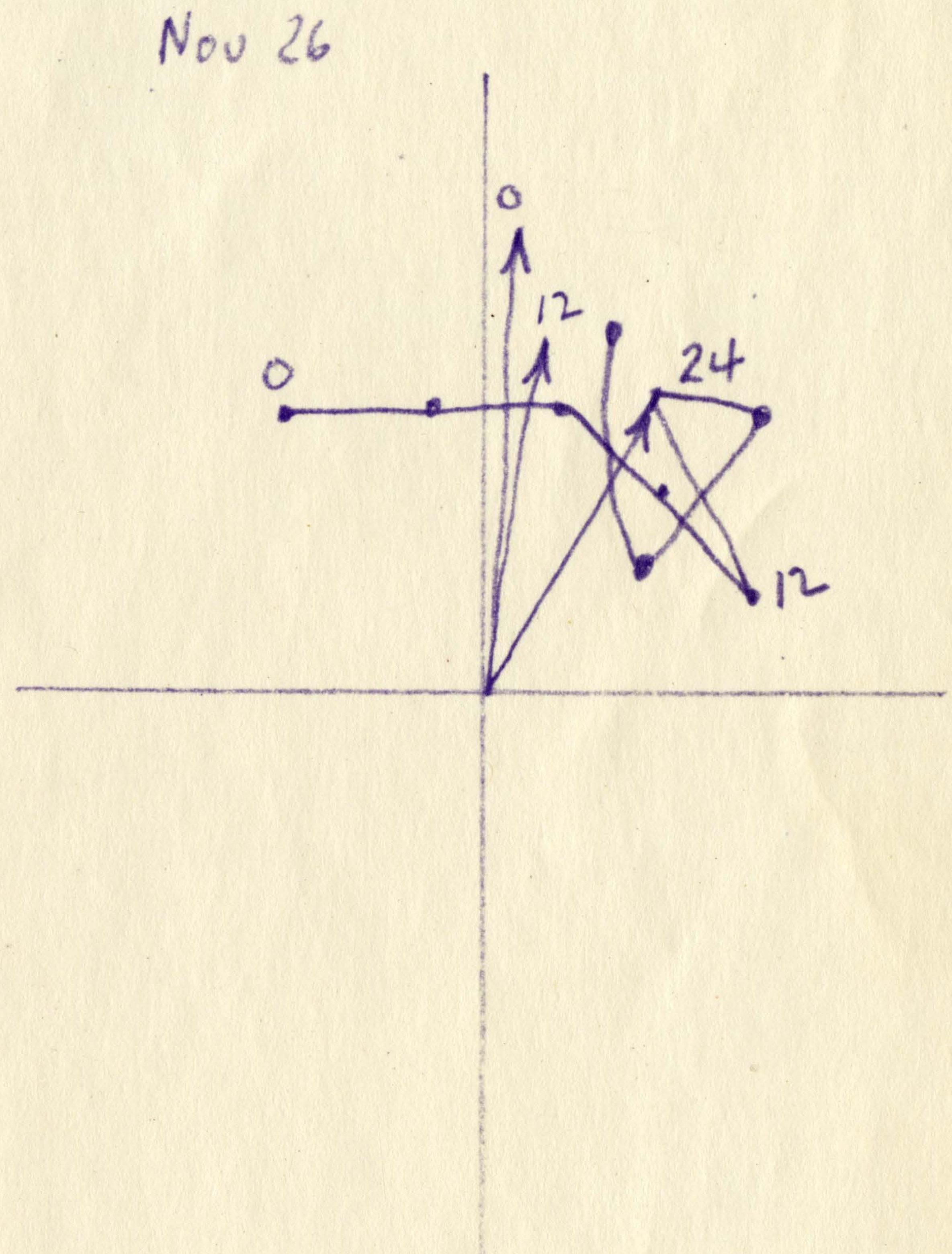
Nov 24



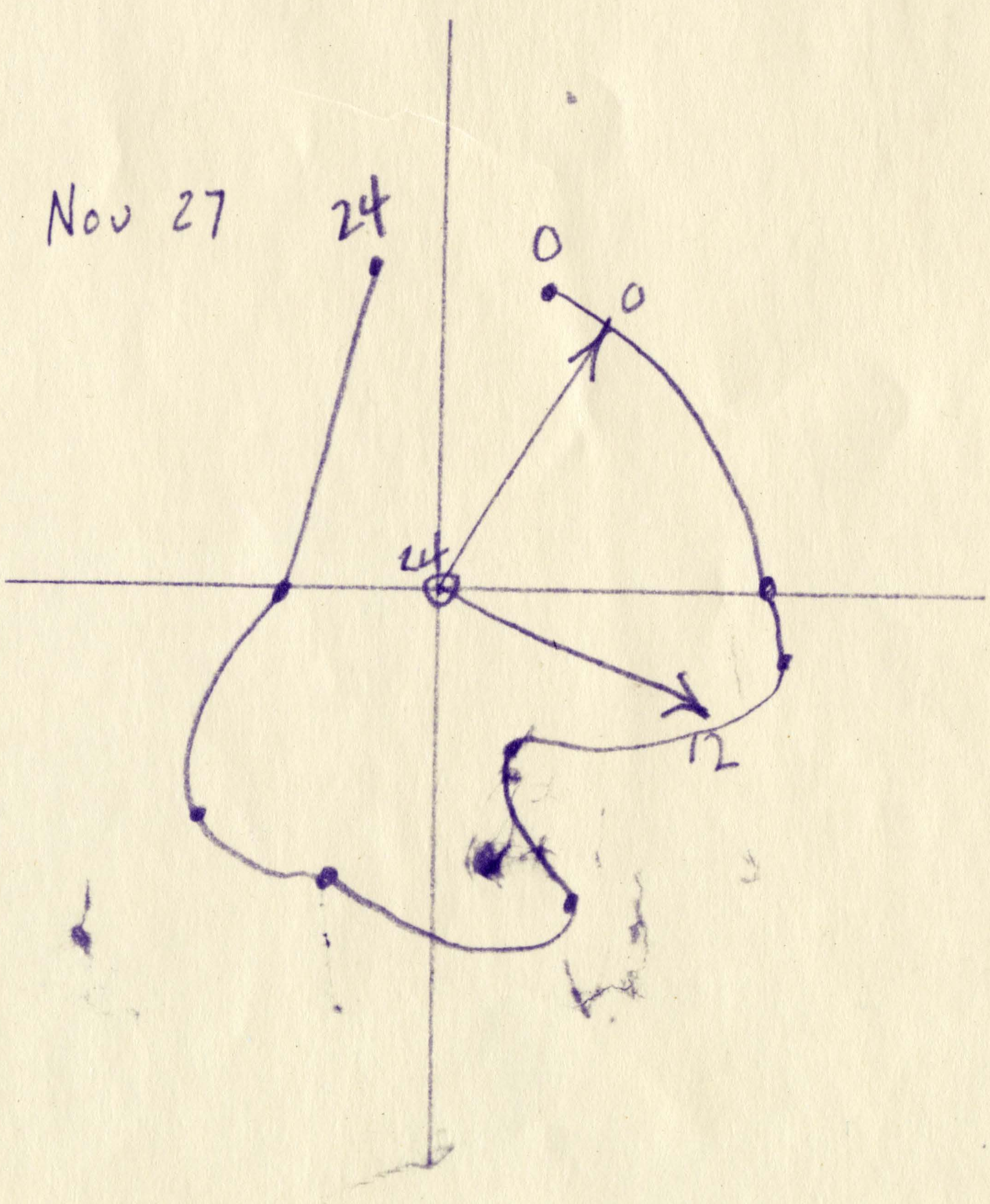
Some of the early morning records this day were lost due to electronic troubles in the wee hours.



Wind still veering and now a strong inertial current appears, it seems. The mean velocity is about 0.6 knots at 45° ±10° to the right of the mean wind.

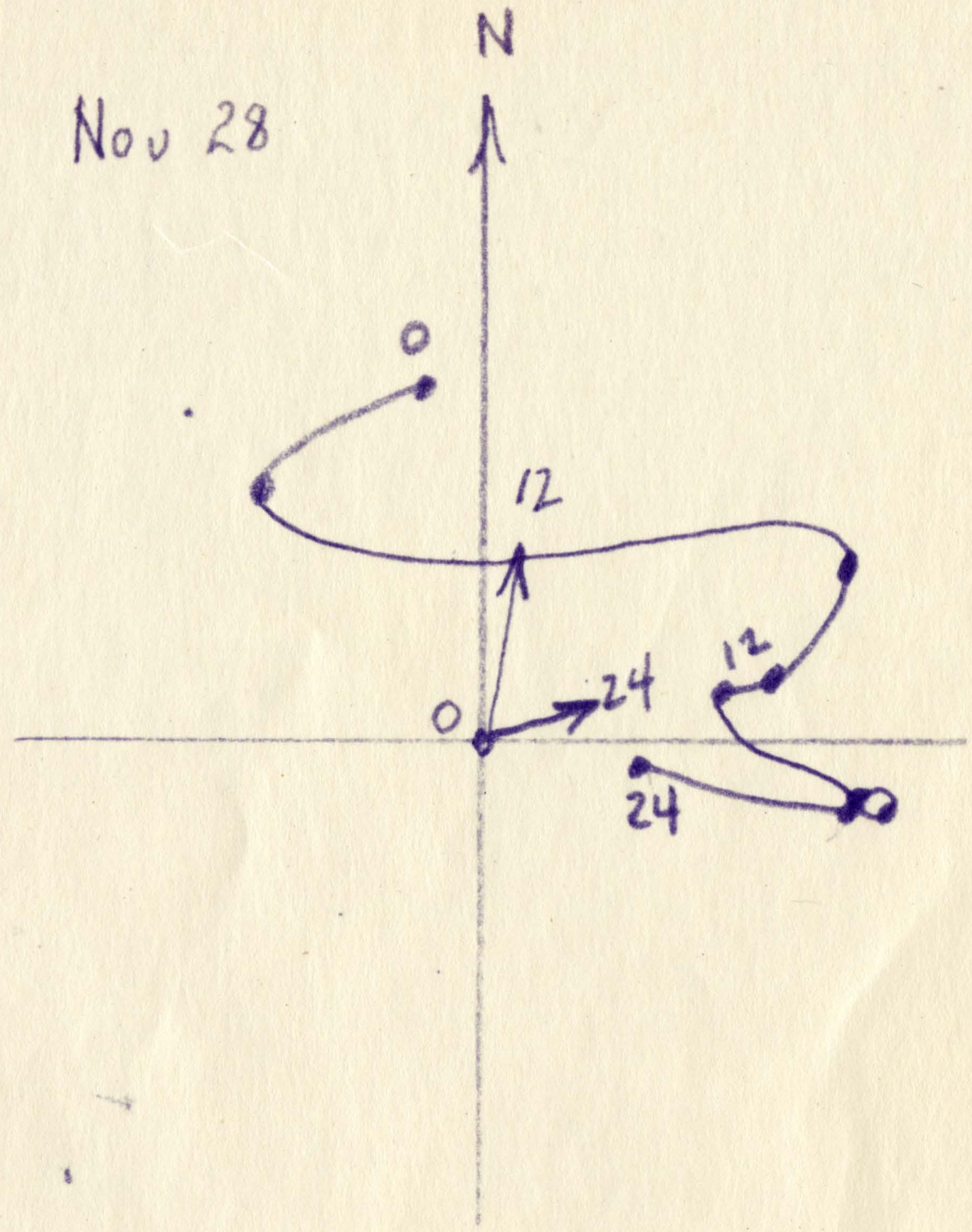


Wind still veering and currents complex



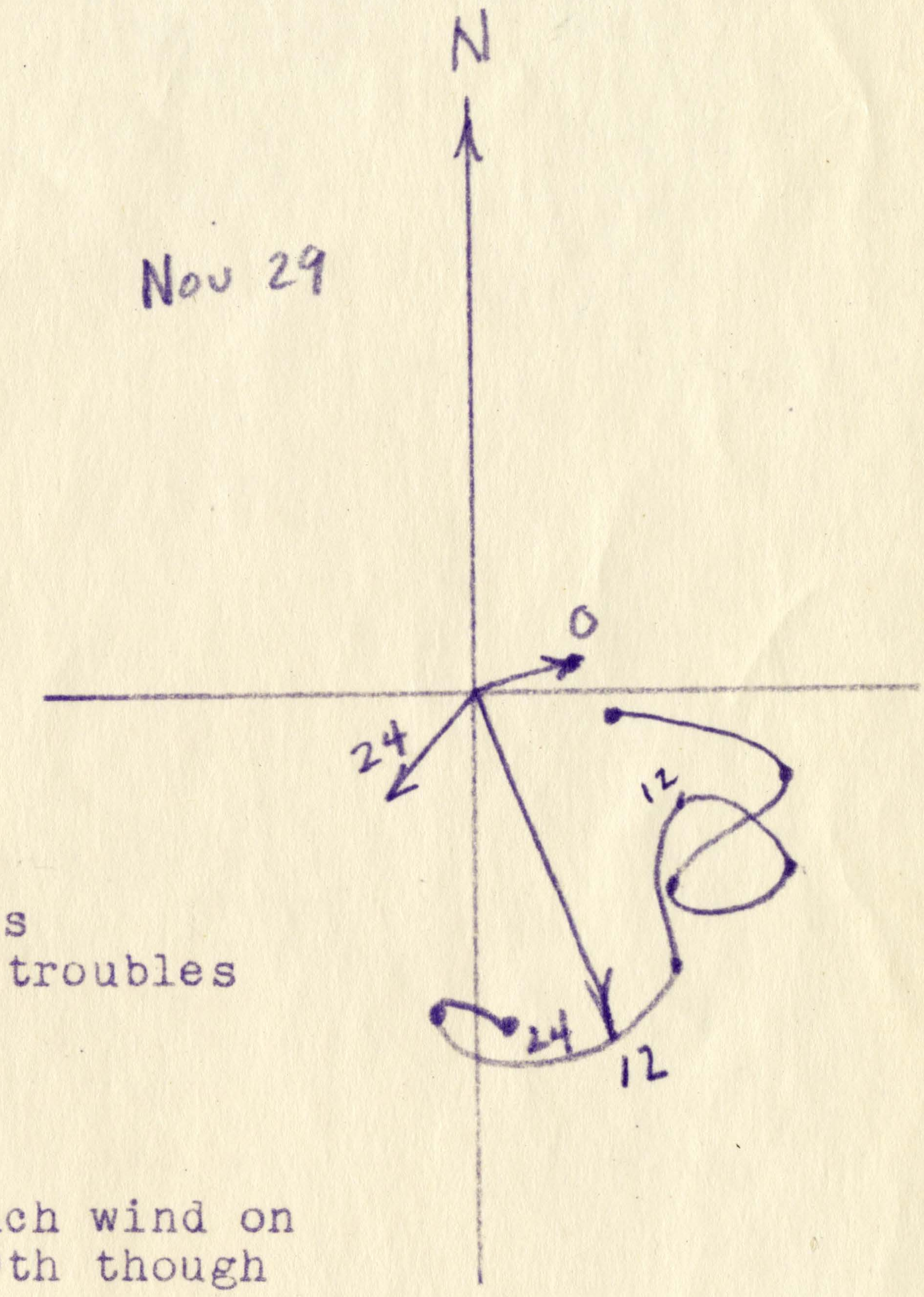
Wind veers and drops to flat calm during afternoon: Another inertial current, this time more or less about zero.

Nov 28



Calm in the early morning, gentle airs the rest of the day, irregular currents.

Nov 29

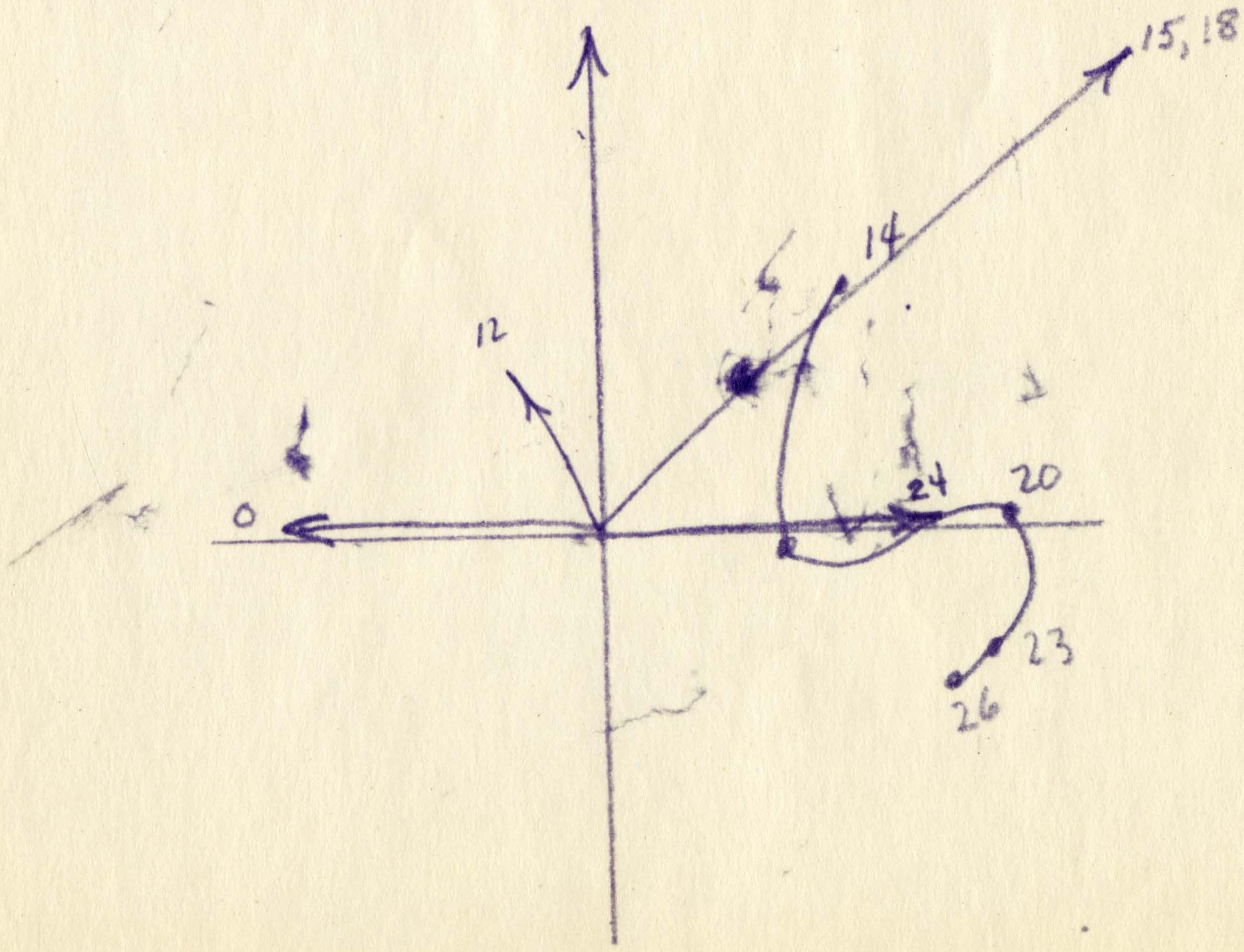


Wind from Northwest starts cunrent toward SE, but dies and the following day's data is missing due to more elctronic troubles

Nov 30 Data missing due to radio trouble.

Not much wind on the 30th though

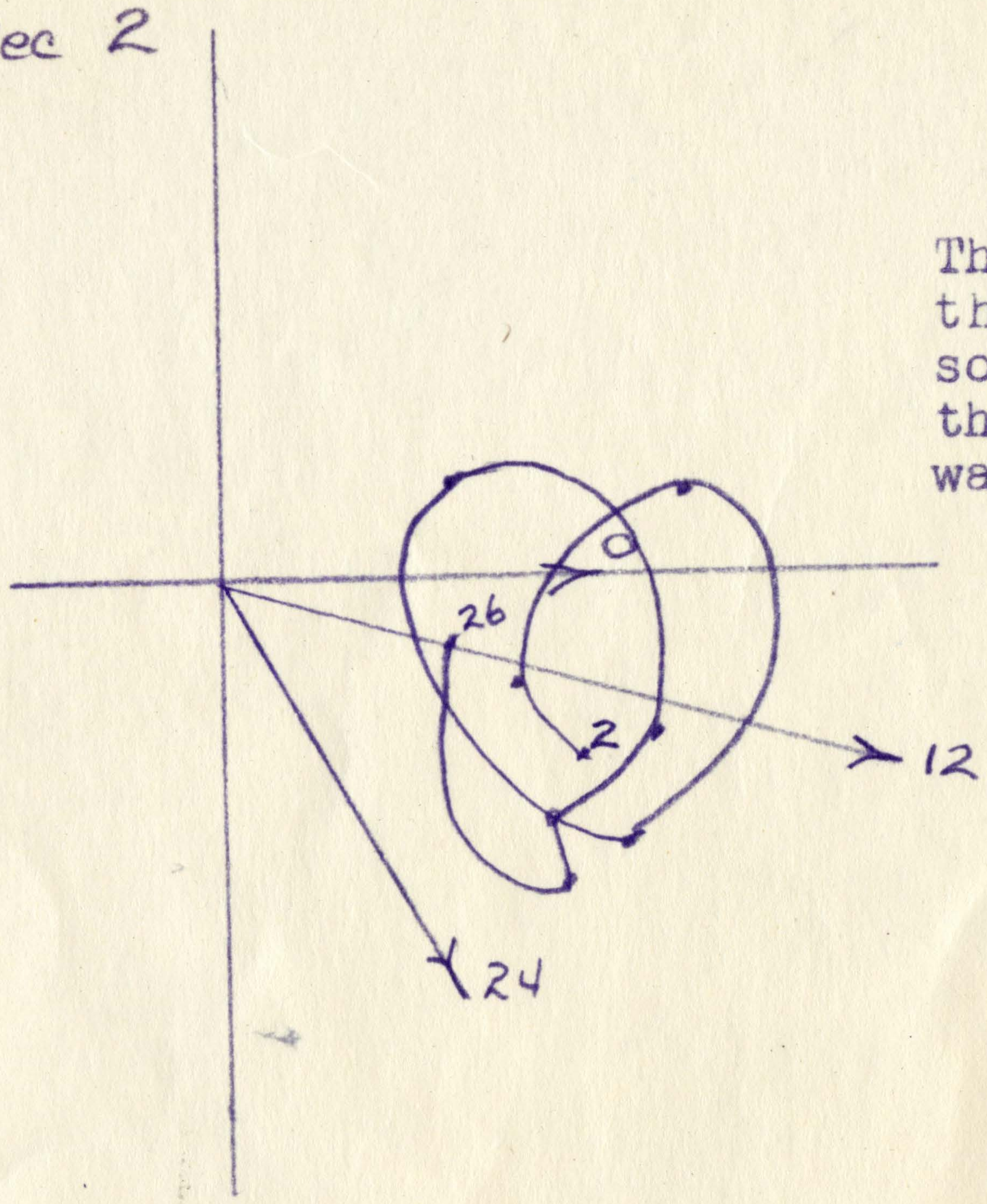
Dec 1



and very peculiar squally winds with strong SW blow in afternoon which gets currents flowing toward the east

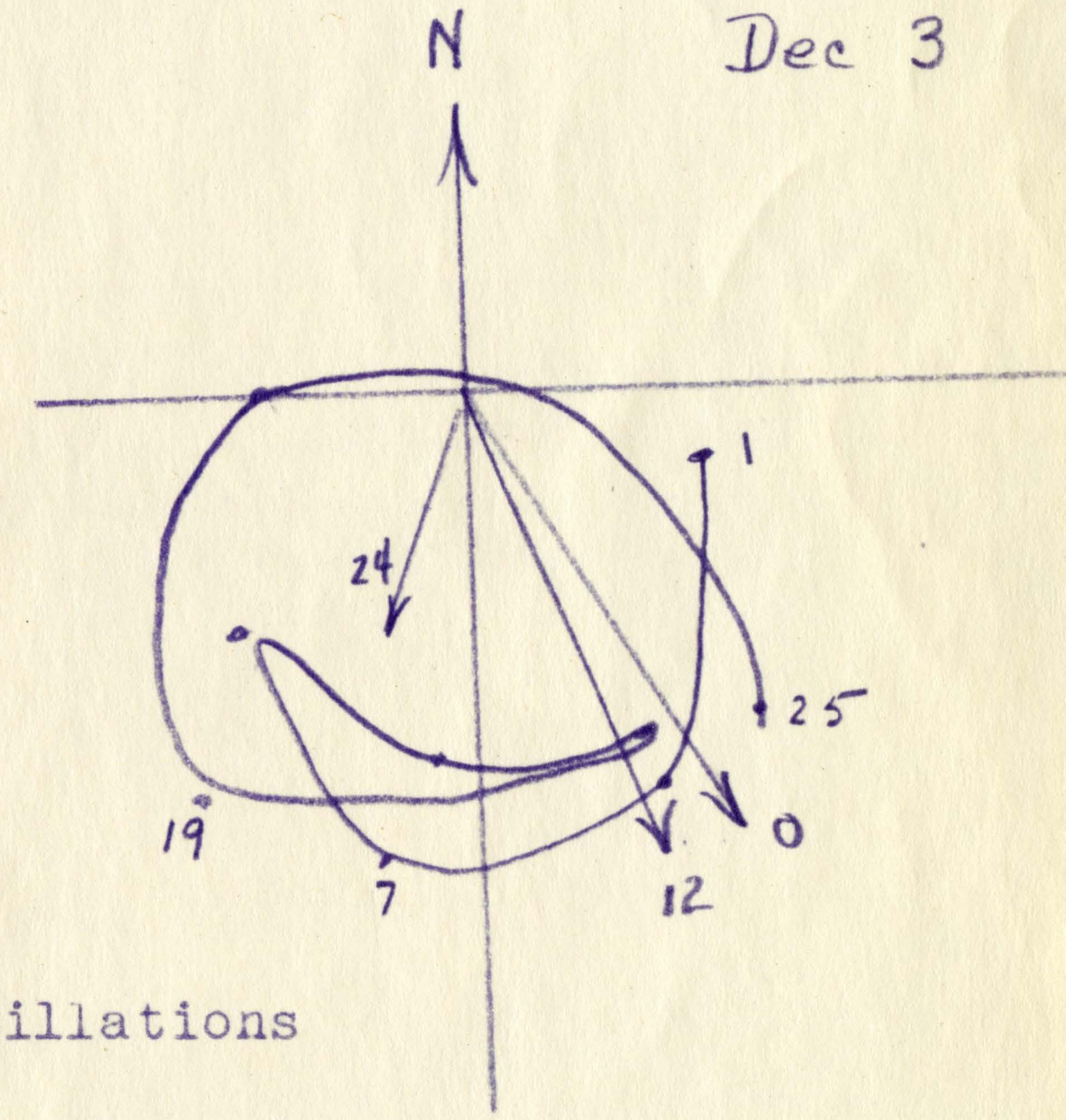
PAGE 6

Dec 2



This day looks somewhat peculiar. I think the buoy may now be near to the shore and some tidal currents are involved. Unfortunately the positions are not certain, but the signal was very strong, suggesting that the buoy was nearby.

Dec 3



This looks again like inertial currents following a drop in wind.

The deep drag was at 200 feet in all these measurements. In the buoy that will next be reported on the deep drag is at 560 feet, well below the seasonal thermocline, and the interval of data transmissions is every hour and a half. This may make the matter of the inertial oscillations more explicit.

Buoy lost early

Dec 4

I will send you more of this peculiar synoptic data in a later installment.

I do hope you will offer any suggestions or criticisms you may think of so that I can incorporate them in future measurements here. This program will probably go on until March 30.

With my very best regards,

Hank

Henry Stommel

December 22, 1953

Mr. Henry Stommel
Oceanographic Observatory
Biological Station
Bermuda

Dear Hank:

I certainly will try to help find someone who might work with you in Bermuda under the conditions stated. I personally feel that your present attack represents one of the most important developments in oceanography since the war and that it deserves all possible support. Finding someone is not the kind of thing that is done in a day, but I should think that chances for interesting one of the Scripps members to work with you should be rather good. I have several people vaguely in mind. I shall see how things develop.

We have just started building a house to take care of a baby expected in June.

Merry Christmas,

Walter H. Munk

WHM:es

Oceanographic Observatory
at the Biological Station
Bermuda
Dec 16, 1953

Dear Walter:—

I am very anxious to keep this Observatory going + am having a hell of a time getting people to stay here. All the married WHOI people are home-bodies, or lazy, & prefer to stay home. And now Parson, my very best instrument man, has to leave WHOI to go to Boston on account of his wife.

Do you have any graduate in Phys. Ocean at Scripps who might be willing to take part in this Observatory & maybe eventually take it over entirely? I think it is a very good opportunity to do original + worthwhile field work. He could come to WHOI during the Summer of 1954 + then come down to Bermuda in the Fall of 1954 for a year — + if he likes it, stay on indefinitely.

At present I have no prospect of any assistant at all for 1954.

Your friend

Hank Stommel

Oceanographic Observatory
located at the
Biological Station
Bermuda

December 24, 1953

Dr. C. O'D Iselin
Dr. R. B. Montgomery
Dr. W. H. Munk
Dr. Henry Charnock (National Institute of Oceanog. England)
Mr. Robert G. Walden

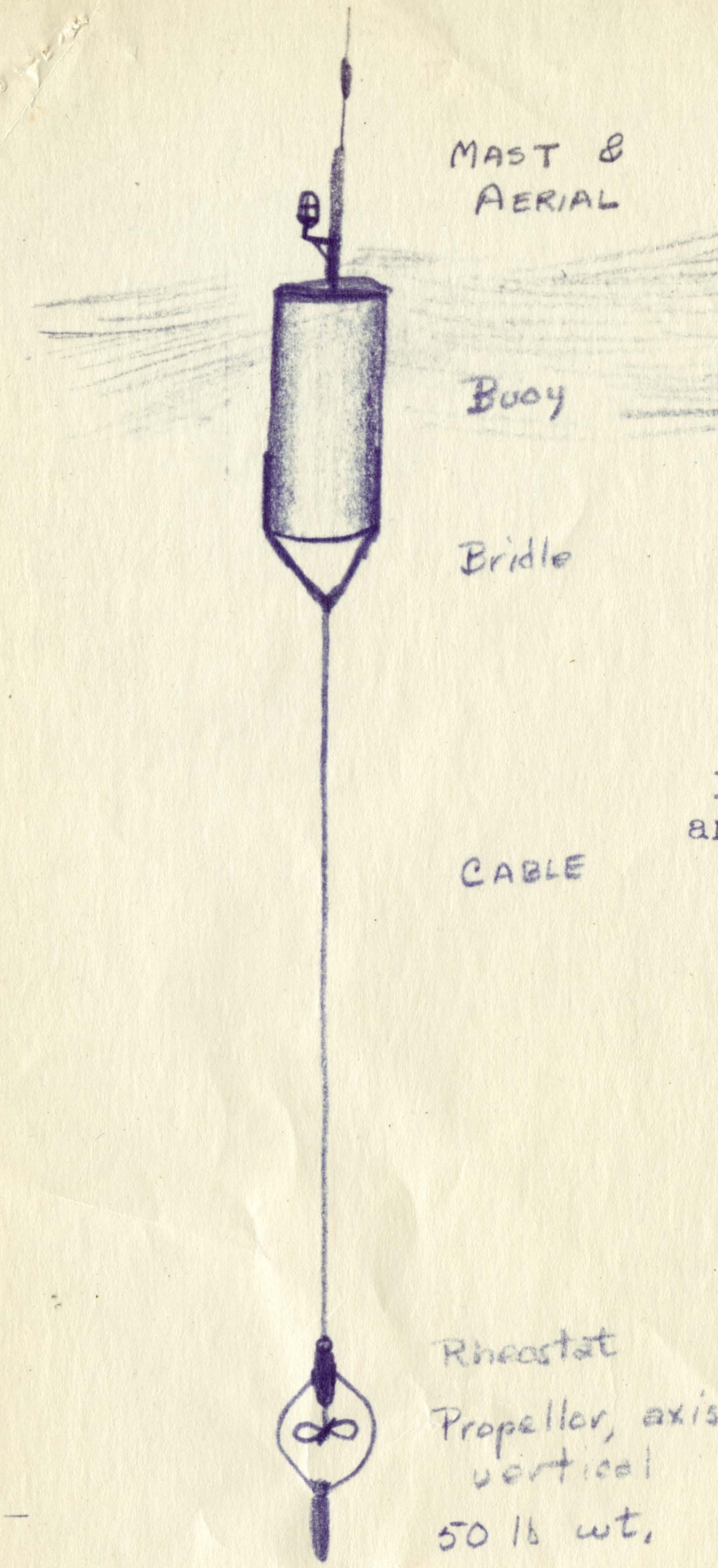
This third letter is a bit of a digression from wind drift current measurements, but we had an old buoy that had been washed up on the beach early in October, and we had some old batteries and a transmitter that had already seen service anchored on the 50 fathom line, and I thought it would be interesting to try something new with the buoys just to give an indication of their versatility and possible application to other problems besides current measurements. The measurement of thermal structure with buoys requires some refinements electronic-wise which were in progress of being developed, but nothing handy suggested itself, except that by taking out some gears from one of our current logs and putting on a propellor in a cage, with axis vertical, and suspending this by a long wire from the buoy, we could get a crude kind of deep-sea wave meter, which might be interesting as a tool for some future research. Accordingly Ted Spencer made up a propellor in a cage as shown in the drawing on page 2, and we suspended it from a bridle to a depth of about 300 feet below the buoy. The output of the radio transmitter is then a carrier modulated by a frequency of varying pitch, the actual pitch being a measure of the vertical displacement...in actual fact 1000 cycles per second equalled 10 feet. The signal received is recorded on magnetic tape and later played back through a frequency measuring cicuit onto a speedomax--- a portion of one of the records obtained by direct tracing from the speedomax record is shown on page 2 too. (Nov 9, 1530 60°W time) The buoy transmitted every three hours for about six days, and then apparently ran on a reef nearby. However, this was a mere trial run, and we did not even have fresh batteries in the buoy.

I think a buoy wave meter of this type is capable of measuring roughly the very largest storm waves in the ocean. However, since the buoy does not follow the sea surface exactly, it would be advisable to include a capacity type device, such as Walden has developed to give the position of the water surface relative to the buoy, or else, simply use a more buoyant buoy like a life raft or dory.

o o o o o

Now that I am digressing from wind drift measurements anyway, I would like to mention some ideas that I have had for the coming year here. I asked Bill Kielhorn if he could get me about ten miles of submarine cable to lay from Bermuda to the 1000 fathom line. I can then put resistance thermometers along it at various depths and get continuous data on thermal structure for a year or more. Such a technique would probably be OK for detecting internal waves in the seasonal and permanent thrmoclines, but thermometers suspended from dirfting buoys would probably be better for measurements of the diurnal heating.

Since these buoys are going out anyway, and since there are some extra contacts on the selector switches, I hope that you will tell anyone



who has some small gear that he would like to get synoptic oceanographic data with, that he is welcome to put it on these buoys. It should not weigh more than 50 lbs., nor consume more than 50 milliamperes 6 volts continuous. It must be disposable because the buoys are not retrieved. There are a variety of plate voltages available, up to 600VDC.

o o o o o o

I will be here in Bermuda until March 30, I think, and then at Woods Hole until the fall, at which time I'LL come back down here again. Visitors are very very welcome here. The place is an intellectual backwater... the only source of stimulation is the sea itself.

My very best wishes, and Season's

Greetings

Hank

Henry Stommel