March 1, 1949

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

Dear Hank:

I am very much impressed with your general discussion on diffusion in the ocean. I spent most of yesterday reading it and have now passed it on to Dr. Sverdrup. Although much remains to be done, as you say yourself, it seems quite certain that your work represents one of the few definite advances that have been made in oceanography during the last decade.

One of the nice features about your paper is its style. I find it very readable and much preferable to the dry, careful, scientific style which is fashionable now. And what you say at the end regarding the value of the deductive approach, that it is a question of morale more than anything else, I think really hits the nail on the head.

I have felt the need for this sort of approach ever since I worked on the diffusion problem at Bikini and I have since kept a folder named "Turbulence and Scale Factor" into which I occasionally drop some ideas. It was quite definite from the radio-active diffusivity data that the diffusion coefficient became appreciably larger with time as the general size of the contaminated area increased. Perhaps it may be worthwhile to really analyze the data. Since we are here dealing with the continuous distribution it would mean, I suppose, a certain generalization of Richardson's equation. You have stated, I believe, that Heisenberg expects to generalize his deductive approach to a continuous distribution. I suppose the Bikini bomb drop still represents, if not the best, nevertheless the most spectacular experience of oceanographic diffusion.

I wish I would understand a little better the meaning of the size of the eddy as far as computation of eddy viscosity. In the work I did in Norway I worked, as you know, on the horizontal ocean circulation with lateral stresses introduced in the general equations. In comparing the computed with the observed current pattern one finds that the eddy viscosity near the Gulf Stream must be a value of 10^8; also that its value increases somewhat as we go from shore toward the open ocean. In terms of Prandtl's theory I suppose this is the same thing as saying that the mixing length increases (linearly) with the distance from the boundary. Of course this increase holds only in a relatively narrow zone and must eventually flatten out.

Once again, congratulations for your work. I hope that I will see a lot of you while you are here at Scripps.

Yours,

Walter H. Munk

WHM:es

March 3, 1949

Dear Walter,

Thank you ever so much for your encouraging remarks about my work in ocean diffusion. I should like very much to have a chance sometime to see the Bikini data. Perhaps when I come out this summer would be a good time

My observations were made with discrete particles — floats — but Richardson's equation of diffusion

$$\frac{\partial q}{\partial t} = \frac{\partial}{\partial l}\left(F(l)\frac{\partial q}{\partial l}\right)$$

applies equally well to continuous distributions. It is necessary to transform the data from $\nu(x)$ to $q(l)$ [see p 11. of report for definition]. The integral of the equation if $F \propto l^{4/3}$ is easy, but then we still must transform $q(l)$ back to $\nu(x)$ for various t and that is where the rub is. However, I have received from Richardson a recent letter in which he has worked out the answer to this nasty problem, only it looks like some functions will have to be tabulated.

I hope you will be in the West most of the time I am there, despite the Scylla & Charybdis in New York.

The very best wishes to you and your wife.

Hank

March 29, 1949

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

Dear Hank:

I am a little late in replying to your letter enclosing the paper on the thermohaline circulation. I think I had a chance to look at some of your notes on this problem before I went to Norway.

As time goes on I am more and more certain that the wind-driven circulation accounts for, say within 15%, all the known shallow currents in all the oceans. I find that it is quite instructive to divide the subject into the zonal wind-driven circulation and the meridianal wind-driven circulation. The former accounts for most of the important facts in the ocean circulation.

From this point of view, the best way to classify ocean currents are, I believe, the wind-driven circulation and the thermohaline circulation, as you suggest. Since there is practically nothing known about the thermohaline circulation, your notes deserve, I think, particular attention. I was quite amazed to seehow complexing the problem becomes on account of its inherent non-linearity.

Bill Van Dorn has been trying for almost a year to develop a good method to measure deep ocean currents, and it would be interesting to compare some of his data, when he gets it, with some of your tentative conclusions.

With best regards,

Yours,

Walter H. Munk

WHM:es

November 18, 1949

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

Dear Hank:

I believe I am right in assuming that you will draw up an outline for our book for the University Press. As soon as I receive such an outline from you I will try to go over it as carefully as I can. I don't suppose it makes very much difference what we put in it as we will be quite free to change it at any time.

I wonder whether you could send me reprints of your papers with the exception of

The Westward Intensification of Wind-Driven Ocean Currents
Note on Eddy Diffusion in the Sea
Entrainment of Air into a Cumulus Cloud
The Theory of the Electric Field Induced in Deep Ocean Currents
Finite Difference Forms of the Equations of Physical Oceanography
and your paper with Gordon Riley and Bumpus.

Sincerely

Walter H. Munk

WHM:es

WOODS HOLE OCEANOGRAPHIC INSTITUTION
WOODS HOLE, MASSACHUSETTS

November 23, 1949

Dear Walter,

Many thanks for your letter. It was nice to know that you arrived safely.

I have already sent to you under separate cover a very sketchy table of contents. Upon looking over Teddy's letter it occurs to me that perhaps it was much too sketchy to be very useful.

Tom Duke has recently read the Barber and Ursell papers and has begun to see the light. He appears to have a genuine interest in oceanography and would like to have a chance to get started on a firm basis by going to Scripps. He also wants to get out from under HRS' yoke - that's just my guess. I find myself in the position of having to advise him as to what to do, and would therefore like to ask you:

1. Could Tom possibly start study for a degree at Scripps in the Spring term?
2. Could he obtain employment - he would need funds right from the start?

My best wishes to all of you at Scripps....

yours sincerely,

Hank

Henry Stommel

Montgomery

Coast guard

Daily

November 28, 1949

Professor Henry Stommel
Woods Hole Oceanographic Institution
Woods Hole, Massachusetts

Dear Hank:

I was tickled pink to get your brief note saying that you were interested in a part time position at Scripps. I am sorry that I cannot give you a definite answer at this time because of lack of budgetary information from the University. As an Assistant Professor, however, the University might be rather stuffy to the extent of wanting you to spend an entire semester here, or approximately 4½ months. Do you think this would be possible?

With best regards.

Very sincerely yours,

RR-b

Roger Revelle.

9 DEC 1949
1316
Director's Office

Henry Stommel

Dear Roger,

I understand quite clearly that you must go through a lot of rigamarole before you can get the University to give me an Asst. Prof. + that's perfectly O.K. with me. If it must be 4½ mos., then 4½ mos it will be.

Do you remember Johnny Holmes' sister Rosemary – well it looks like we are on the verge

of getting married —
and I cannot think
of a happier fate.
We probably will wait
a year, but it looks
pretty positive.

Yours truly,

Hank

December 13, 1949

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

Dear Hank:

This note is to tell you how delighted I am about your engagement to Rosemary Holmes. I must confess that my enthusiasm is slightly tinged with jealousy. I saw her only for a part of one afternoon but promptly fell in love, and only my age and previous commitments keep me from trying to beat your time.

With all best wishes.

Yours sincerely,

RR-b

Roger Revelle.

December 14, 1949

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

Dear Hank:

I wish I could say something definite and encouraging about Tom Duke. I have talked to Roger about it but the budget situation is such that a definite job could not be promised, at least not for the first semester. It has in all events been our policy not to give jobs during the first semester of studying, as we find that otherwise the student is overloaded. I know of only one exception to this rule that has been made. Would it be possible for Tom to take a chance on finding employment after he has been here one semester?

Another thing that comes to mind is that Tom might be a very suitable graduate student for Ray Montgomery. You know, of course, that Ray is in the market for some bright young graduate students. Is it not possible perhaps that Tom might find some employment at Brown. I know that the Department of Applied Mathematics, which is Ray's host at present, holds various contracts.

Defant is now planning to leave Scripps during the last days of February and to spend perhaps the first week of March at Woods Hole. Do you think Columbus might send him a letter inviting him to come at that time? He has never received any word from Woods Hole, and I felt that there is some doubt in his mind as to whether he is wanted at Woods Hole.

I think you have heard about the letter that Defant received regarding the purchase of METEOR volumes. I am sending you an ozalid copy of what is available.

With best regards,

Yours,

Walter H. Munk

WHM:es
Enc.

cc: Central Files

Fascinating job.

December 15, 1949

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

Dear Walter:

I am sending enclosed a set of the following literature on soaring of porpoises:

Reference: 1. Gray's paper on swimming of dolphins
2. Woodcock's Nature article
3. ~~My letter to Davidson~~
4. Davidson's reply
5. Woodcock and McBride manuscript
6. A short manuscript by me.

These various papers contain much that is mutually contradictory and perhaps it will be best to list these statements and then indicate the contradictions. I will not include Davidson's ideas in the scheme because I think they are wrong, what is your impression?

Statements:

A. Porpoises can soar in the forward portion of waves at speeds of 10 knots.
B. Porpoises can make themselves up to 9 lbs heavy in water.
C. Porpoises have drag-lift characteristics similar to a towed rigid form.
D. Porpoise wakes at Reynolds number of 5×10^6 are turbulent.
E. Porpoise muscles develop much more power per pound than other mammalian muscle tissue.

Reference 1 states if D is true, then E is true. Reference 2 states A is true, reference 5 states B is true, reference 6 states if C and D are both true, then A and B are not both true. Inverting this, if A and B are true, then C and D are both not true. It would seem therefore that porpoises have achieved something that practical hydraulic people should think on.

I would appreciate it if you can find time to see whether I have botched something up here.

Enclosed is a duplicate set of copies in case you think it worth while to forward this gathering storm to some authority like Hunter Rouse.

Yours truly,

Henry Stommel

Enc:

HS:ph

STUDIES IN ANIMAL LOCOMOTION

VI. THE PROPULSIVE POWERS OF THE DOLPHIN

By J. GRAY

(Sub-Department of Experimental Zoology, Cambridge)

(*Received August* 10, 1935)

(With Three Text-figures)

It is well known that certain aquatic vertebrates (notably dolphins and some of the larger teleostean fishes) are able to travel at surprisingly high speeds. The movements performed by such animals during rectilinear locomotion are all of the same type, for the hind end of the body vibrates rhythmically in a plane at right-angles to the axis of locomotion; the plane of vibration of the dolphin is dorso-ventral, whereas that of a fish is transverse to the long axis of the body. In all cases the orientation of the hind end of the body and of the caudal fin, in particular, is such that during both phases of each vibration the leading surface (relative to the direction of the vibration) is inclined at an angle to its own direction of motion through the water and is directed obliquely backwards relative to the head of the animal (Gray, 1933). The anatomical arrangements of the propulsive muscles of a dolphin appear to be simpler than those of a fish, since the locomotory movements are produced by four bands of musculature connected to the base of the caudal fin by strong tendons; on the ventral side of the vertebral column the two muscle bands extend forward to the region of the diaphragm, whereas the two dorsal bands extend over the whole back of the animal. The weight of the dorsal musculature is approximately twice that of the ventral muscles. The tail is deflected upwards by the contraction of the dorsal muscles and downwards by contraction of the ventral muscles. Reciprocity thus exists between the dorsal and ventral muscle groups, whereas, in a fish, reciprocity of this type is restricted to the right and left musculature of individual segments. Apart from these anatomical differences the propulsive mechanisms of a fish and of a dolphin appear to be of essentially the same type.

It is commonly stated that the streamlined form characteristic of rapidly swimming vertebrates enables them to move through the water with a minimum resistance. Attempts to measure this resistance (Houssay, 1912; Mangan, 1930) have been made on the assumption that when a fish is swimming freely in water it is overcoming a resistance which is equal to that encountered by an inert body of the same size and shape when towed through water at the same speed. The recent work of Richardson (1936) has shown that the towing resistance of an inert fish is not substantially different from that of a model of similar form, but observations of this type do not enable us to decide how closely this value is related to the resistance actually overcome by a free-swimming fish. The problem is of considerable interest,

since a reliable estimate of the "free-swimming" resistance of a large fish or dolphin would indicate whether the mechanism of swimming is or is not substantially more efficient than those, at present, available for the propulsion of a torpedo or airship.

Direct determination of the horse-power of a freely swimming fish or dolphin would involve very great technical difficulties. It is, however, possible to approach the problem from a theoretical point of view and from arguments based on the observation of models.

The velocity of a rapidly moving dolphin has seldom been determined with great accuracy, and no doubt it has often been exaggerated. The following observation made by Mr E. F. Thompson whilst in the Indian Ocean is therefore of interest. A dolphin swimming approximately 30 ft. from the side of the ship passed the ship in the direction of stern to bow in just under 7·0 sec. as timed by a stop-watch; the length of the ship was 136 ft. and its speed was logged at $8\frac{1}{2}$ knots. This dolphin must therefore have been travelling at 20 knots (=33 ft. per sec.). On many occasions dolphins have been seen to keep abreast of the ship's bows when the vessel was travelling at 15 knots, and so far as could be determined this speed could be maintained for considerable periods.

If the resistance (R) overcome during normal locomotion at a speed of 33 ft. per sec. is equal to the towing resistance of a rigid body of the same size and shape, then

$$R = \frac{d\rho AV^2}{g},$$

and the horse-power (H.P.) $= \dfrac{d\rho AV^3}{550g}$,

where d = drag coefficient, ρ = weight of 1 cu. ft. of water, A = surface area in sq. ft., V = velocity in ft. per sec., g = 32. The value of the drag coefficient varies with the velocity and length of the moving body, but for a 4-ft. porpoise travelling at 25 ft. per sec. the appropriate coefficient is about $1{\cdot}5\times10^{-3}$, while for a 6-ft. dolphin travelling at 33 ft. per sec. the coefficient is approximately $1{\cdot}3\times10^{-3}$.

The surface area of a 4-ft. porpoise (*Phocaena communis*) was found by direct measurement to be very nearly 7 sq. ft., whilst that of a model of a dolphin (*Delphinus delphus*) was 15 sq. ft. Using the equations given above, the towing resistance and requisite horse-power can be calculated; they are recorded in Table I.

Table I

Species	Total wt. lb.	Length ft.	Surface area sq. ft.	Speed ft. per sec.	Drag coeff.	Towing resistance lb.	H.P.	Wt. of muscles lb.	H.P. per lb. of muscle
Porpoise	53	4	7	25	$1{\cdot}5\times10^{-3}$	16	0·6	9	0·067
Dolphin	200	6	15	33	$1{\cdot}3\times10^{-3}$	42·5	2·6	(35)	0·074

Reliable estimates of the horse-power of mammalian muscle have been obtained in the case of man and of the dog. Henderson and Haggard (1925) showed that the output of very highly trained oarsmen was approximately 0·5 H.P. per man. If we assume a conservative estimate of the weight of muscle employed, namely 50 lb., the available horse-power per lb. of muscle is 0·01, a figure which is substantially the same as that obtained by Dill, Edwards and Talbot (1932) for the dog. If, therefore, the output of cetacean muscle is of the same order as that of other mammals the 4-ft. porpoise would require 60 lb. of muscle, and the 6-ft. dolphin would require 260 lb. Both of these figures are clearly fantastic; in fact, the total weight of the propulsive muscles of the porpoise was found to be 9 lb. and that of the dolphin was estimated to be 30–40 lb.[1] In order, therefore, to endow the cetaceans with their estimated horse-power we must assume that their muscles are approximately seven times more powerful than those of other mammals, in which case the ability of these animals to dissipate heat and to supply oxygen and nutritive substances to the active muscles must be very remarkable.

Before accepting an abnormally high horse-power for cetacean muscle, it is reasonable to reconsider the validity of the figure which has been accepted for the drag coefficient of the actively moving animal. The resistance per square foot of surface area will vary with the nature of the flow of water over the surface of the animal, and this, in turn, depends upon the size and velocity of the organism. So long as the product of the velocity and the length of a smooth rigid body does not exceed a critical value the flow past the surface may be expected to be of the laminar type and free from turbulence; if the critical value be exceeded the flow past the posterior end of the body becomes turbulent. The transition from laminar to turbulent flow sets in when the Reynolds' number[2] exceeds a value of 5×10^5 (see Ewald, Pöschl and Prandtl, 1930, p. 319).

Until the transitional value for Reynolds' number is exceeded, the value of the drag coefficient falls continuously for increasing speeds, but once the transitional point has been passed the additional resistance, introduced by the formation of eddies, leads to a marked rise in the value of the coefficient. Table II shows, for the sake of convenience, the drag coefficient for laminar flow and for the transitional change to turbulent flow for various values of Reynolds' number.

Table II

Reynolds' No.	Drag coefficient	
	Laminar flow	Turbulent flow
10^5	$2·1 \times 10^{-3}$	—
2×10^5	$1·5 \times$	—
3×10^5	$1·2 \times$	—
4×10^5	$1·0 \times$	—
5×10^5	$0·9 \times$	$1·0 \times 10^{-3}$
10^6	$0·7 \times$	$1·5 \times$
2×10^6	$0·5 \times$	$1·6 \times$
4×10^6	$0·3 \times$	$1·56 \times$
8×10^6	$0·2 \times$	$1·4 \times$
10^7	$0·2 \times$	$1·3 \times$
2×10^7	$0·15 \times$	$1·25 \times$

[1] The actual weight of the muscles of a dolphin (5 ft. 8 in. long) has since been found to be 33 lb. For this and other useful data I am indebted to Dr Frazer, British Museum (Nat. Hist.).

[2] Reynolds' No. $= \frac{Vl}{\nu}$, where $V =$ velocity, $l =$ length and $\nu =$ kinetic viscosity of water.

So long as a 6-ft. dolphin is travelling at speeds higher than 1 ft. per sec., the flow has been assumed to be turbulent, and the Reynolds' number for a 6-ft. dolphin travelling at 33 ft. per sec. is of the order of $1·6 \times 10^7$; in calculating the resistance of the animal the drag coefficient ($1·3 \times 10^{-3}$) characteristic of turbulent flow has therefore been used.[1] If, on the other hand, the flow past the body were laminar and free from turbulence, the resistance would be very much smaller, for d would become $0·15 \times 10^{-3}$. Under such circumstances the resistance of the dolphin would fall to 4·9 lb. and the horse-power to 0·3. This output of work could be maintained by 30 lb. of typical mammalian muscle—a figure in good agreement with the actual weight of muscle present. Similarly the drag coefficient for laminar flow past a porpoise 4 ft. long and travelling at 25 ft. per sec. would be $2·3 \times 10^{-4}$; the resistance would be 2·0 lb. and the calculated horse-power 0·09. This is equivalent to 9 lb. of typical mammalian muscle and happens to be the exact weight of the muscles of the porpoise itself. In view of these results, it is of interest to consider how far the flow over the surface of the body of an actively swimming fish or dolphin is determined by conditions which tend to eliminate the turbulence characteristic of the flow past a rigid body of similar form.

The nature of the movements executed by a fish or a dolphin are known with considerable accuracy, and their propulsive effect has been considered elsewhere (Gray, 1933). For the purpose of the present discussion, however, it is necessary to know in detail the type of flow which these movements generate in the surrounding water. Comparatively little information is available concerning the flow set up by vibrating systems immersed in a fluid (see Richardson, 1936), and so far it has proved impracticable to record the flow of water past the body of a living fish swimming freely in water. The present series of observations have therefore been made on models, composed of flexible rubber, whose movements were made to conform with those of a variety of fish. This was effected by inserting into the dorsal surface of the model (12–15 in. in length) a series of rigid rods which were capable of performing simple harmonic movements in a plane at right-angles to the long axis of the model. The amplitude of movement of each rod and the phase difference between itself and its neighbours were adjusted to conform with cinematograph records of an actively swimming fish of the same length as the model. The rods actuating the model were driven by a single shaft, so that the frequency of movement could readily be adjusted to any value. The model was half submerged in a large tank of water, and the movement of the water past the body of the model was detected by photographing the movement of particles lying at or near the surface of the water. By means of cinematograph pictures it was possible to obtain a tolerably complete picture of the flow past the model.

[1] In calculating the oxygen requirements of a blue whale (27 m. in length) travelling at 10 knots, Krogh (1934) accepts an estimate of the horse-power of 46·8. This appears to be based on a drag coefficient characteristic of turbulent flow: if the flow were laminar the oxygen requirements would obviously be very much less than those calculated by Krogh. It is unlikely that the flow past the body of a large whale is entirely free from turbulence, but it may well involve very much less than that past a rigid body.

In a normal fish the amplitude of the muscular waves increases as the waves pass backwards (Gray, 1933), but, for the moment, it is convenient to consider the movement of particles in the neighbourhood of a wave whose velocity and form remain constant. It is possible to summarise the flow of particles lying in the neighbourhood of such a wave as follows: (1) The direction and velocity (relative to the ground) of a particle depends upon its position relative to the crest (Fig. 1) of the wave and upon the velocity with which the wave is travelling relative to the ground. (2) A particle lying in the median plane of a wave trough (*e.g.* a particle situated on one of the lines gg_1, cc_1, jj_1 in Fig. 1) travels in the same direction as the wave but at a velocity which is always less than that of the wave itself and which decreases the farther the particle lies from the surface of the fish (Fig. 1, cc_1). (3) Particles lying in a transverse plane which cuts a leading surface of the body travel obliquely outwards from the longitudinal axis of the body, as can be seen from the direction of the arrows along the regions *fg*, *bc*, *hj*, *de* in Fig. 1, these regions each being leading surfaces of waves. (4) Particles lying in planes which cut trailing surfaces flow obliquely inwards as in Fig. 1, *ab*, *gh*, *cd*, *jk*. (5) Particles at positions near the outer crests of the waves flow in a direction opposite to that of the waves as in Fig. 1, *b*, *h*, *d*. Since the posterior[1] velocity of the particles relative to the ground is always less than that of the waves themselves, an individual particle is constantly changing its position relative to the crest of the wave; it is, in fact, constantly being overtaken by successive waves. This is illustrated in Fig. 2. The whole flow is determined by the fact that the leading surfaces of a wave are displacing water, while the trailing surfaces are acting as centres towards which water flows (see Fig. 1). In so far as water is prevented from flowing across the body of the fish the water displaced at a leading surface flows towards a trailing surface, lying posteriorly to itself. The leading surfaces represent regions of high pressure and the trailing surfaces represent regions of low pressure. As these regions pass towards the hind end of the fish, water is constantly moved backwards relative to the ground (Fig. 2), thereby giving a forward thrust to the body. These observations have been checked against the movement of particles lying in the vicinity of a slowly moving eel, and

[1] The propulsive waves travel over the body of a fish from the anterior to the posterior end of the body. The term "posterior" as used in respect of the model is therefore employed to denote the end of the model towards which the waves are travelling.

Fig. 1. Figure showing the direction of flow (relative to fixed axes) of particles in the neighbourhood of a series of waves of constant form passing over the body of a model fish in the direction of the large arrow. Leading surfaces are shown at *fg*, *bc*, *hj*, *de*. Trailing surfaces are shown at *ab*, *gh*, *cd*, *jk*. The relative velocities of particles lying in the plane of a wave crest are shown at the level hcc_1: the length and direction of the arrows at this level indicate the velocity and direction of movement of the particles.

Fig. 2. Figure showing the movement of a particle relative to a wave and to fixed axes respectively as a wave of constant form advances in the direction of the large arrow from A_{i} to A_{iii}. The particle (originally situated at position 1 near the leading edge of the wave) travels backwards relative to the wave along the line ——. When the crest of the wave has reached A_{iii} the particle is situated (relative to the wave) at the posterior edge of the wave (position 9). The motion of the particle relative to fixed axes is shown by the dotted line; the points marked on this line correspond with those showing the movement of the particle relative to the wave. The dotted wave indicates the position of the wave when the particle has reached position 5. The figures along the line A_{i}–A_{iii} indicate the position of the wave crest for each of the positions marked on the track of the particle.

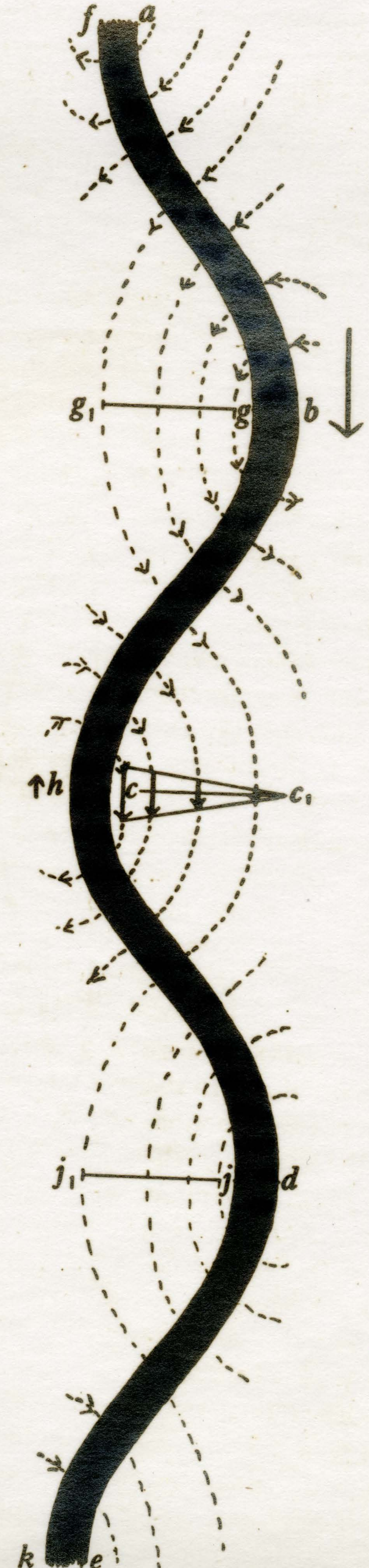

Fig. 1.

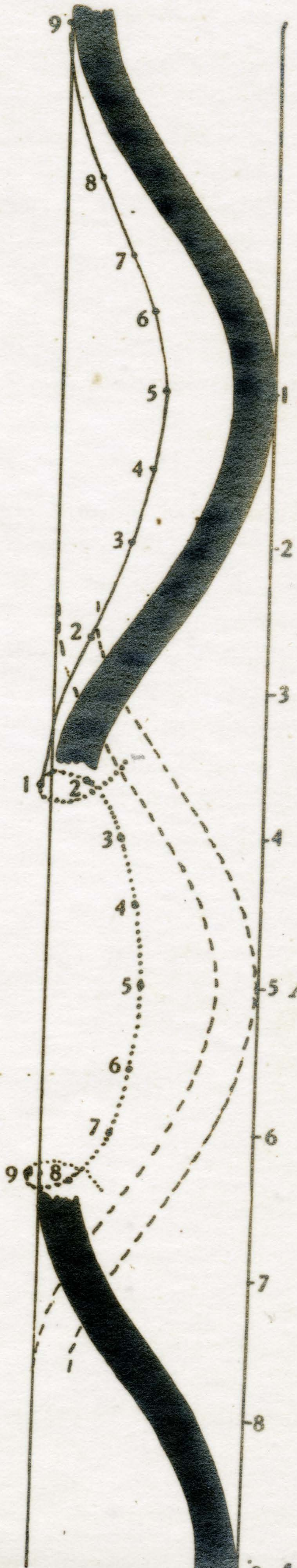

Fig. 2.

they can be applied, also, to the undulatory type of movement seen in active flagella. For present purposes they indicate that undulatory movements of the type seen in actively swimming fish may be expected to set up a type of flow quite distinct from that past a rigid body or past a dead fish towed through water.

When the form and movements of the model are made to approximate in form to those of a whiting or a mackerel the conditions are somewhat different, and it is more difficult to plot the flow with accuracy. It can, however, be seen that particles situated near the surface of the posterior regions of the body are accelerated backwards towards the trailing surface of the caudal fin (Fig. 3). This movement is particularly noticeable in the case of particles lying in the vicinity of the leading surface of the body, for when such particles reach the peduncle of the tail they pass rapidly over the dorsal or ventral surface of the body and are drawn in at the trailing surface of the fin (Fig. 3). It seems clear that the latter surface acts as a centre of low pressure which induces a backward acceleration of all the water lying in the vicinity of the whole of the posterior part of the body.

Probably the only safe conclusion to be drawn from these observations is that the flow of water in the vicinity of a body which is exhibiting undulatory movements of the type performed by a fish or a dolphin when in locomotion, differs substantially from that past a rigid body when being towed through the water, and consequently it is illegitimate to assume that the resistance to movement is the same in both cases. It is, however, tempting to go somewhat farther.

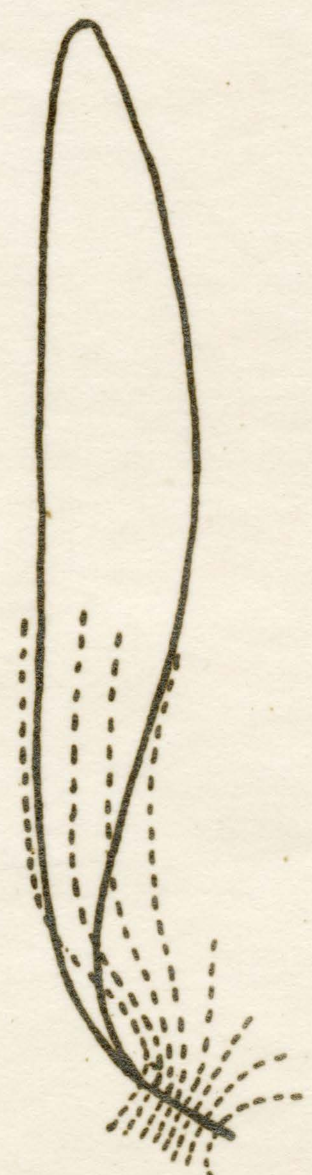

Fig. 3. Diagrammatic representation of the flow of water induced by the caudal fin of a dolphin or fish.

In the case of a rigid body anchored in a stream, the resistance due to turbulent flow is caused by frictional retardation of the flow in the vicinity of the boundary of the body. "If any accelerating or retarding pressure differences exist in the layers of water which adjoin the boundary layer these differences of pressure affect the fluid in the boundary layer also. If the external flow is accelerated by a fall of pressure in the direction of motion the fluid particles which are travelling more slowly in the boundary layer also receive an impulse in the direction of motion, hence all particles continue on their way past the surface of the body" (Ewald, Pöschl and Prandtl, 1930, p. 283). So long as such conditions persist the flow remains laminar and free from turbulence. Owing to the small dimensions of the models described in this paper, it has not been possible to determine by direct observation whether a turbulent flow past the model at rest is replaced by a laminar flow when the model is exhibiting typically propulsive movements, but the evidence suggests that the water in the vicinity of the hind end of the body of a fish or a dolphin is being influenced by such conditions in the external flow as are likely to represent a region or regions of low pressure acting in the direction of motion, and to this extent it seems conceivable that the flow past the surface of an actively moving dolphin is very much less

turbulent than is the case when the inert organism is towed through water at the same speed. In order to test the validity of this conclusion it would be necessary to observe the flow past the body of a much larger model than has so far been available: it would also be necessary to make the observations in an external flow of water whose velocity was approximately that of a free-swimming dolphin.

All fast-swimming fish and dolphins appear to possess a narrow but strong peduncle to the caudal fin, and the latter is expanded to a width approximately equal to the transverse diameter of the widest region of the body. If the suggestions made in this paper are valid, the narrow peduncle and expanded fin seem well adapted for a free flow of water from all the posterior regions of the body surface to the trailing surface of the fin.

SUMMARY

1. If the resistance of an actively swimming dolphin is equal to that of a rigid model towed at the same speed, the muscles must be capable of generating energy at a rate at least seven times greater than that of other types of mammalian muscle.

2. Observation of the flow of particles past the surface of models similar in form to a fish or dolphin shows that rhythmical movements, such as are characteristic of the body and caudal fin of the living animals, exert an accelerating effect on the surrounding water in the direction of the posterior end of the model. An effect of this type may be expected to prevent turbulence in the flow of water past the body.

3. If the flow of water past the body of a dolphin is free from turbulence, the horse-power developed per pound of muscle agrees closely with that of other types of mammalian muscle.

REFERENCES

DILL, D. B., EDWARDS, H. T. and TALBOT, J. H. (1932). *J. Physiol.* **77**, 49.

EWALD, P. P., PÖSCHL, T. and PRANDTL, L. (1930). *The Physics of Solids and Fluids* (English trans.). London.

GRAY, J. (1933). *J. exp. Biol.* **10**, 88.

HENDERSON, Y. and HAGGARD, H. W. (1925). *Amer. J. Physiol.* **72**, 264.

HOUSSAY, F. (1912). *Forme, Puissance et Stabilité des Poissons.* Paris.

KROGH, A. (1934). *Nature*, Lond., **133**, 635.

MANGAN, A. (1930). *Ann. Sci. nat.* **13**, 355.

RICHARDSON, E. G. (1936). *J. exp. Biol.* **13**, 63.

The Swimming of Dolphins

DOLPHINS in the Gulf of Panama have been seen moving through the sea at a speed of ten knots, their entire bodies showing no apparent swimming motion. This performance was confined, in my observations, to the area immediately forward of the stem (prow) of a sea-going tug and to an estimated depth of one metre or less. Elsewhere near the bow, vertical oscillations of dolphins' tails were readily timed with a stop watch.

When this 'motionless' swimming was first noticed, the animals were in the normal swimming position. In this position it was difficult to be sure that vertical motions of their tail surfaces were not occurring, since the direction of such motions would be nearly parallel with the line of sight of the observer. However, on several occasions dolphins were seen to turn on their sides during the 'motionless' swimming in such a position that their usual swimming motions would have been normal to the line of sight. No motion was visible in these animals, which were clearly seen just below the surface of the water. One animal remained on its side in this manner for 59 sec., which represented a distance, at 5·15 m./sec. (10 knots), of 304 metres. At this time dolphins swimming near by used 1·9 tail oscillations per second in keeping pace with the vessel.

These 'motionless' dolphins seemed to be riding the bow wave (that is, falling down the inclined surface). However, if dolphins are equal in weight to the weight of the water they displace, wave riding is not possible. No data have been found concerning their density. Kellogg[1] says, however, that dolphins usually sink when shot. Is it possible that they are dense enough to fall down the advancing slope of the bow wave, having achieved terminal velocity initially by swimming ? A low over-all resistance to motion through the water would seem to be required. Gray[2] has indicated that the work done by dolphin muscle in producing a speed of 10 m./sec. is comparable to the work of other mammalian muscle tissue, if laminar flow is assumed around the dolphin. With turbulent flow, Gray found that the work done by dolphin muscle at 10 m./sec. would be about seven times the work of other mammalian muscle. If a laminar regime exists at a speed of 5 m./sec., perhaps the over-all resistance to the motion of a dolphin is low enough to allow an animal, of sufficient negative buoyancy, to fall down the inclined water surface of a bow wave or other waves.

It may be thought that the dolphins were gaining a forward thrust by placing their tails against the ship's stem. Dolphins have been seen with their tails in momentary contact with the hull, but in the above observations of 'motionless' swimming the animals were clearly forward of the vessel.

Shoulejkin[3] suggests that the speed of motion of the dolphin's tail may be very rapid. Dolphins seen 'motionless' in the bow wave may have been using rapid tail oscillations of very small amplitude, causing them to appear to be motionless. If such swimming motions were in fact occurring, it is curious that the use of this propulsive technique should be confined to the immediate vicinity of the bow, while other dolphins a few metres away always used a large amplitude vertical tail motion with a period of about half a second (at 10 knots).

I would like to know whether other observers have seen this 'motionless' swimming, and what explanations may have been given.

ALFRED H. WOODCOCK
Woods Hole Oceanographic Institution,
Mass. Jan. 30.

[1] Kellogg, Remington (personal communication), U.S. National Museum, Washington, D.C., 1948.
[2] Gray, J., "Studies in Animal Locomotion. VI. The Propulsive Powers of the Dolphin", *J. Exp. Biol.*, **13**, 192 (1936).
[3] Shoulejkin, W. W., "Physics of the Sea", Publication Acad. Sci U.S.S.R., Moscow-Leningrad, see pp. 715–724 (1941).

COPY

EXPERIMENTAL TOWING TANK
Stevens Institute of Technology
711 Hudson Street Hoboken, New Jersey

Kenneth S. M. Davidson, Director
Allan B. Murray, Assistant Director
HOboken 3-8080

5 December 1949

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

Dear Mr. Stommel:

I am sorry to have been so long in getting down to answering your letter of 14 November, which related to what you call the "motionless" swimming of porpoises.

The fundamental point is, to my mind, that when moving in the forward part of a gravity wave, the "motionless" body will have acting upon it as a driving force a component of the "total" weight, and not merely a component of any excess of total weight over buoyancy. This can be demonstrated for a rigid body like a torpedo and I conceive there to be no basic difference between such a rigid body and a porpoise. Whatever the separate contributions to the total weight, of animal tissue, air, and water, within the porpoise, I take it as axiomatic that the over-all density must be substantially that of water. With this point established, there is of course no difficulty in accounting for the presence of a driving force sufficient to maintain the motion you describe. For a porpoise 6' long and 1' in diameter, the weight (displacement) must be something like 180 lbs, while the required driving force to maintain a speed of 10 knots is of the order of 14 lbs, presuming turbulent skin friction. Accordingly, the slope of the wave surface in which the motion occurs need be only something like 8%.

You will observe from the above that I do not subscribe to the statement in Mr. Woodcock's article in Nature: "However, if dolphins are equal in weight to the weight of the water they displace, wave riding is not possible." There is no fundamental difference that I can see between a submerged dolphin moving close to the surface of a wave and a surf board moving on the surface of a wave, and we know that wave riding is possible with a surf board.

I have been much interested in the performance of porpoises when swimming. Using the figures given above, it is clear that in still water the animal must supply a driving force of 14 lbs at 10 knots, assuming turbulent skin friction. Now, there is good evidence that the animal can easily supply this force, or a considerably larger force, by means of his tail. Nor is there any difficulty in deriving a satisfactory heat balance to account for this force, or a considerably larger one. The trouble arises from the physiological question of his ability to deliver such a force continuously over a reasonably long period of time (as he appears able to do) without crippling muscle fatigue. It is this question, according to my information, that has led to the suggestion that the skin friction resistance may be to a large extent laminar, rather than turbulent.

I shall be much interested to hear any further thoughts you may have on the subject.

Very sincerely yours,

Kenneth S. M. Davidson
Director

KSMD:mm

The Swimming of Dolphins*

by

A. H. Woodcock and A. F. McBride

Dec. 9, 1949.

In an effort to explain the apparent ability of dolphins to ride the bow wave of a ship at a speed of 10 knots (Woodcock, 1948) it was necessary to show that the animals had weight while immersed. Measurements of the weight of an immersed Stenella plagiodon have been made. First results indicate that a dead animal, which weighs 200 pounds in air, and is 6.5 ft. long weighs 9.2 pounds when immersed in sea water. This animal, which was in good physical condition, was killed by injection just before the weighings were made.

Irving, et. al. (1941, p. 152) has indicated that the lungs of dead Tursiops truncatus are collapsed. It is assumed therefore that the lungs of the dead Stenella were also collapsed at the time of weighing and that 9.2 pounds is near the maximum weight in sea water.

Irving (l.c.) indicates that the volume of tidal air per 100 pounds of body weight for Tursiops is 2.7 liters. Assuming the same proportions for Stenella, the tidal air volume for a 200 pound animal is 5.4 liters. At atmospheric pressure 5.4 liters of air will displace 5.54 kg. of sea water or 12.4 pounds. Hence the Stenella, with tidal air expelled and weighing 9.2 pounds, will weigh 9.2-12.4 pounds, or - 3.2 pounds in sea water with tidal air in his lungs. Thus these rough figures indicate that by breathing dolphins can become lighter or heavier than the water which they displace. This indication is amply supported by the behavior of Stenella in the tank at Marineland.

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* Contribution No. ___ from the Woods Hole Oceanographic Institution.

Stenella in the Marineland tank are observed to expel air from their lungs and to rest on the bottom for 4 to 6 minutes. Irving has said (personal communication) that dolphins can store oxygen in body fluids to such an extent that it is reasonable to suppose that they can stop breathing for several minutes with lungs empty of tidal air. It is therefore assumed that wave-riding dolphins remain "heavy" in water by expelling tidal air.

Can a dolphin, which weighs 9.2 pounds in sea water fall down a wave front (either wind or bow wave) at a speed of 10 knots without swimming effort and at the same time gain sufficient dynamic lift to maintain position against gravity?

In order to remain at constant depth and position within the advancing face of a moving wave it seems clear that the dolphin must receive a lift of 9.2 pounds from the water and that his drag at wave speed must not exceed the component of gravity acting at the angle of inclination of the water layers. That is, D must not exceed $W - F_b$; where D is the total drag and $W - F_b$ is weight minus the buoyant force of the sea water. If an inclination of the wave of 15° is assumed,[1] the component of the gravitational force acting will be W sin 15° (i.e., 9.2 x .259 pounds, or about 2.4 pounds). A force of 3.1 pounds results when an angle of 20° is assumed.

A restatement of the above question now takes the following form:

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(1) W. H. Munk of Scripps Institution of Oceanography has said, in a personal communication, that 15° is a reasonable angle to assume for the inclination of the water surface on the advancing face of the larger wind waves.

It would be interesting to know the slopes of bow waves of various sea-going vessels.

Does the drag of a 6.5 foot dolphin exceed 2.4 pounds when its speed is 10 knots and its lift is 9.2 pounds? Gray (1936) has given figures for computing the drag of a rigid body of the same size and shape as a dolphin, presumably a body having zero lift. Following Gray and assuming a negligible increase in resistance due to a lift of 9 pounds,[2] the drag is derived as follows.

$$D = \frac{d \rho A V^2}{g}$$

where

d = drag coefficient $\begin{pmatrix} 1.50 \times 10^{-3} \text{ turbulent flow} \\ 0.26 \times 10^{-3} \text{ laminar flow} \end{pmatrix}$

ρ = density (64 lbs ft^{-3})

A = surface area (15 ft^2)

V = speed (16.9 ft sec^{-1})

g = gravity (32 ft sec^{-2})

D = drag (lbs)

From the above equation and using Gray's tabulation of drag coefficients (see table 1), it is found that the drag on a rigid body having the general size and shape of the Stenella would be 12.9 pounds when turbulent flow is assumed and 2.3 pounds when laminar flow is assumed.

Gray has indicated that the force required per pound of muscle was excessive, if the resistance of an actively swimming dolphin is equal to that of a rigid model towed at the same speed. With the

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(2) See the annex to this paper in which Stommel gives a more detailed discussion of the lift-drag relationships of rigid bodies, and the probable meaning of these relationships when applied to dolphins.

Table I[3]

Reynolds' No.	Drag Coefficient	
	Laminar flow	Turbulent flow
10^5	2.1×10^{-3}	---
2×10^5	1.5 x	---
3×10^5	1.2 x	---
4×10^5	1.0 x	---
5×10^5	0.9 x	1.0×10^{-3}
10^6	0.7 x	1.5 x
2×10^6	0.5 x	1.6 x
4×10^6	0.3 x	1.56 x
8×10^6	0.2 x	1.4 x
10^7	0.2 x	1.3 x
2×10^7	0.15 x	1.25 x

Reynolds' No. = $\frac{Vl}{\nu}$, where V = velocity, l = length and ν = kinematic viscosity of water.

(3) From: Gray, J. Exp. Bio. 13, 192, 1936.

assumption of laminar flow, Gray found that the power developed per pound of muscle agreed closely with that developed by ~~of~~ other types of mammalian muscle.

If a reasonable balance of forces is to occur, it also seems necessary to assume laminar flow about the relatively motionless dolphin which is riding a bow wave. The total weight of the Stenella in water is inadequate to balance a drag of 12.9 pounds. This drag is about 5.4 times the resultant force of gravity acting at an inclination of 15° ($W - F_b \sin 15°$) and about 4.1 times this resultant at an angle of 20°. When the drag coefficient for laminar flow is used (see table 1) the computed drag of 2.3 pounds is slightly less than the available weight of 2.4 pounds. Force and drag are thus practically equal when the speed is 10 knots and when the angle is 15°. For a 20° wave slope force (3.1 pounds) equals drag at about 12.5 knots.

The above preliminary attempt to explain wave-riding of dolphins seems to give further support to the idea that the flow about their bodies is laminar.

Gray has suggested that, in contrast to rigid forms, dolphins may be able to prevent the onset of turbulence through effects arising from their swimming motions. Since wave-riding dolphins appear to be relatively motionless, the necessary assumption of laminar flow about their bodies at 10 knots implies that smooth flow may also occur with little or no swimming motion.

A further point, which arises from this study, concerns the significance of observations of the speed and time during which dolphins remained about the bows of vessels. If these animals can utilize gravitational force as a propelling aid on the inclined surfaces of bow waves, or other waves then it seems clear that this force should be considered in any estimate of the dolphins' capacity for sustained effort. It is reasonable to suppose that dolphins may, by a burst of effort, place themselves in the bow wave of a passing vessel and then, with the aid of gravity, maintain this high speed with a swimming effort which is reduced in proportion to the gravitational force. For the wave-riding dolphin which is making no swimming effort the force of gravity is assumed to be equal and opposite to the drag force at 10 knots, thus making propulsive effort unnecessary.

References

Gray, James

1936 Studies in animal locomotion. VI The propulsive powers of the dolphin.

Jour. Exp. Bio. 13, 192.

Irving, Lawrence, P. F. Scholander and S. W. Grinnell

1941 The respiration of the porpoise, Tursiops truncatus.

J. Cell. and Comp. Physiol. 17, 145-168.

Woodcock, A. H.

1948 The swimming of dolphins.

Nature, 161, 602.

Underwater Soaring of Porpoises

by
Henry Stommel
Dec 15, 1949

The purpose of this note is to point out some relationships that appear to be important in the soaring of porpoises in the forward portions of gravity or ship waves.

Suppose we fix our coordinates to the moving wave front, so that

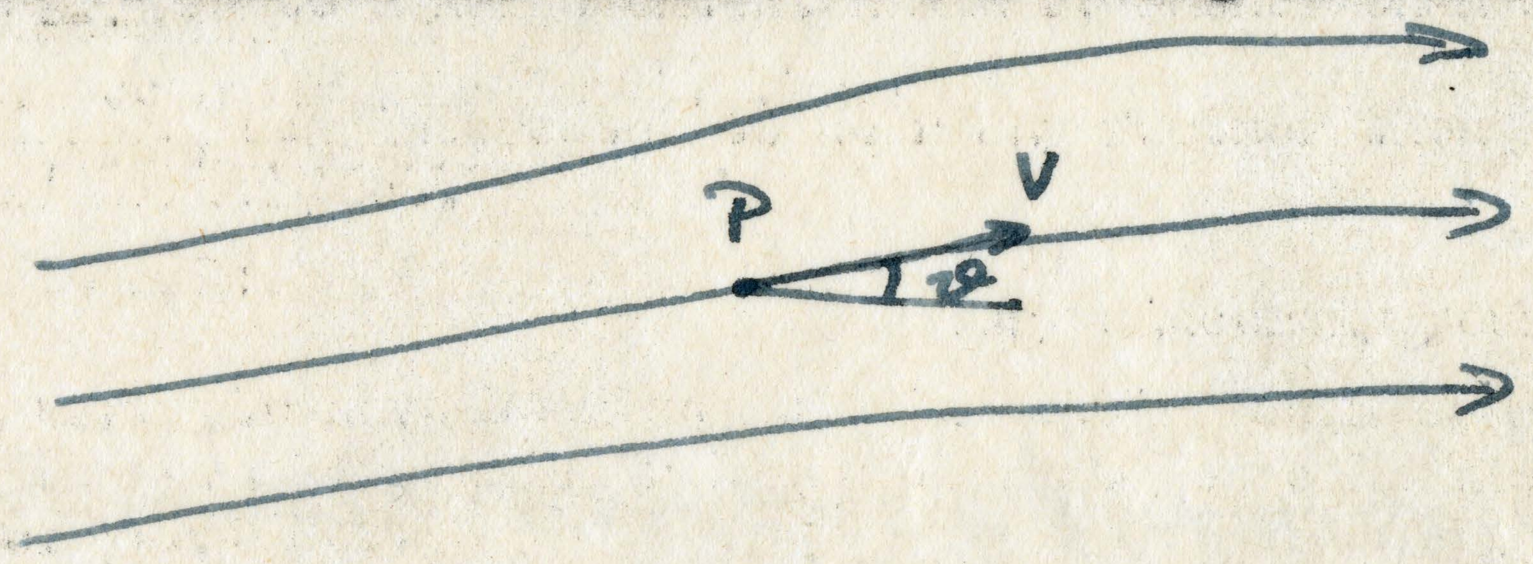


fig. 1

the streamline picture is constant with time as shown by the lines in figure 1. The inclination of the streamlines to the ~~vertical~~ horizontal may be designated as θ, which is, in our coordinate system a function only of position. The velocity of the water at P is denoted by V. We may imagine, now, a completely submerged porpoise to have reached a point P in the water. If he now stops swimming and trys to soar there, he must achieve a balance between three forces:

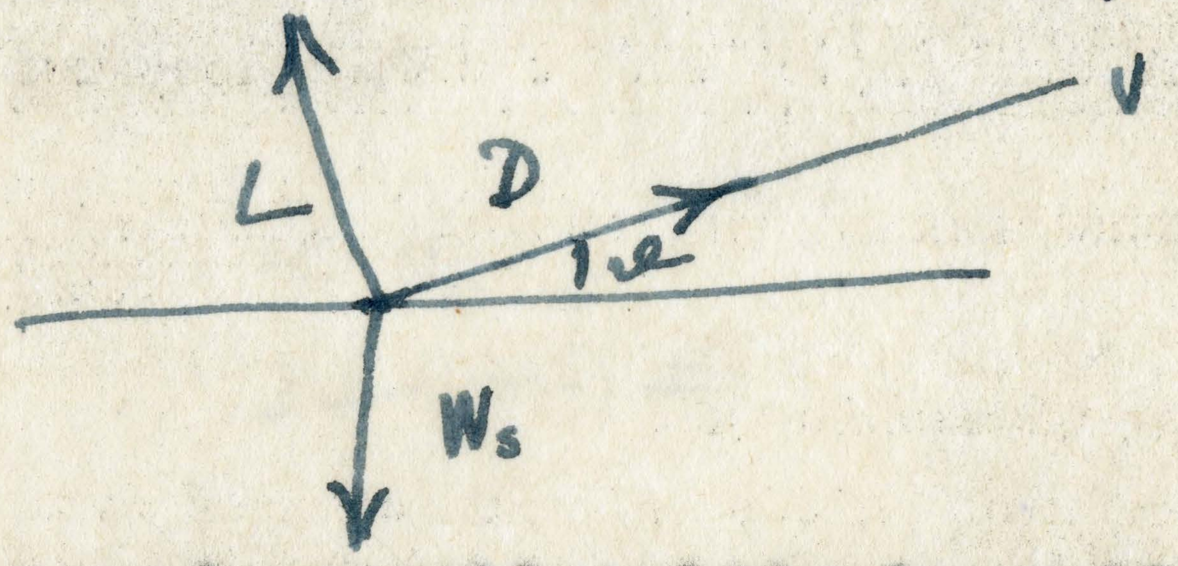
(i) W_s, his weight submerged which is directed vertically downward.

(ii) D, the drag due to the flow of water past him, which is tangent to the streamline at P.

(iii) L, the lift due to his orientation in the stream, which is normal to the streamline at P.

Because the porpoise is a living animal he may vary these three forces within certain limits.

Indeed some degree of control is a necessity because in nature there will always be some more or less random fluctuations in V and θ at P. Were the porpoise unable to control the balance of these forces within certain limits, he would clearly be swept away, or sink.



For example, he may increase his weight submerged by exhaling, but this can hardly be a useful means of control at P because while submerged he can only work it one way. He can also vary his drag by means of his swimming surfaces, although on the whole one might expect him to keep his drag to a minimum. By means of his swimming surfaces, or by orienting his entire body in a certain angle of attack α to the streamline at P, he can vary his lift through wide limits. It is possible that this variation of α is his primary means of control.

In addition the porpoise may choose not to stay exactly at P, but to move slightly about it, in this way operating in a certain range of θ.

One is tempted to make use of the experimental data from the towing of porpoise-like rigid bodies in a discussion of this sort, which is indeed what we will now proceed to do, but the versatility of control of which a living porpoise is capable must always be kept in mind.

The drag and lift, in equilibrium must be related to the weight submerged in the following way

$$D = W_s \sin \theta$$
$$L = W_s \cos \theta \qquad (1)$$

The drag and lift are usually expressed in terms of two coefficients C_D and C_L in the following manner

$$D = C_D S \tfrac{1}{2}\rho V^2$$
$$L = C_L S \tfrac{1}{2}\rho V^2 \qquad (2)$$

where S is the maximum cross-section and $\underline{\rho}$ is the density of the fluid.

Both $\underline{C_D}$ and $\underline{C_L}$ are functions of the Reynolds number $\underline{R}$ and especially $\underline{C_L}$ depend upon the angle of attack $\underline{\alpha}$.

An example of the determination of C_D and C_L for various angles of attack α is given by Durand.*

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* Durand, W. F. Aerodynamic Theory, Volume IV, p. 132.

The body was of a shape similar to that of a porpoise: 81 cm long and with a circular cross-section, the maximum being 136 cm^2. It was tested in a wind tunnel with $V = 18.5$ m sec^{-1}.

The effect on C_L and C_D of changing $\underline{\alpha}$ is shown on a graph of C_L against C_D

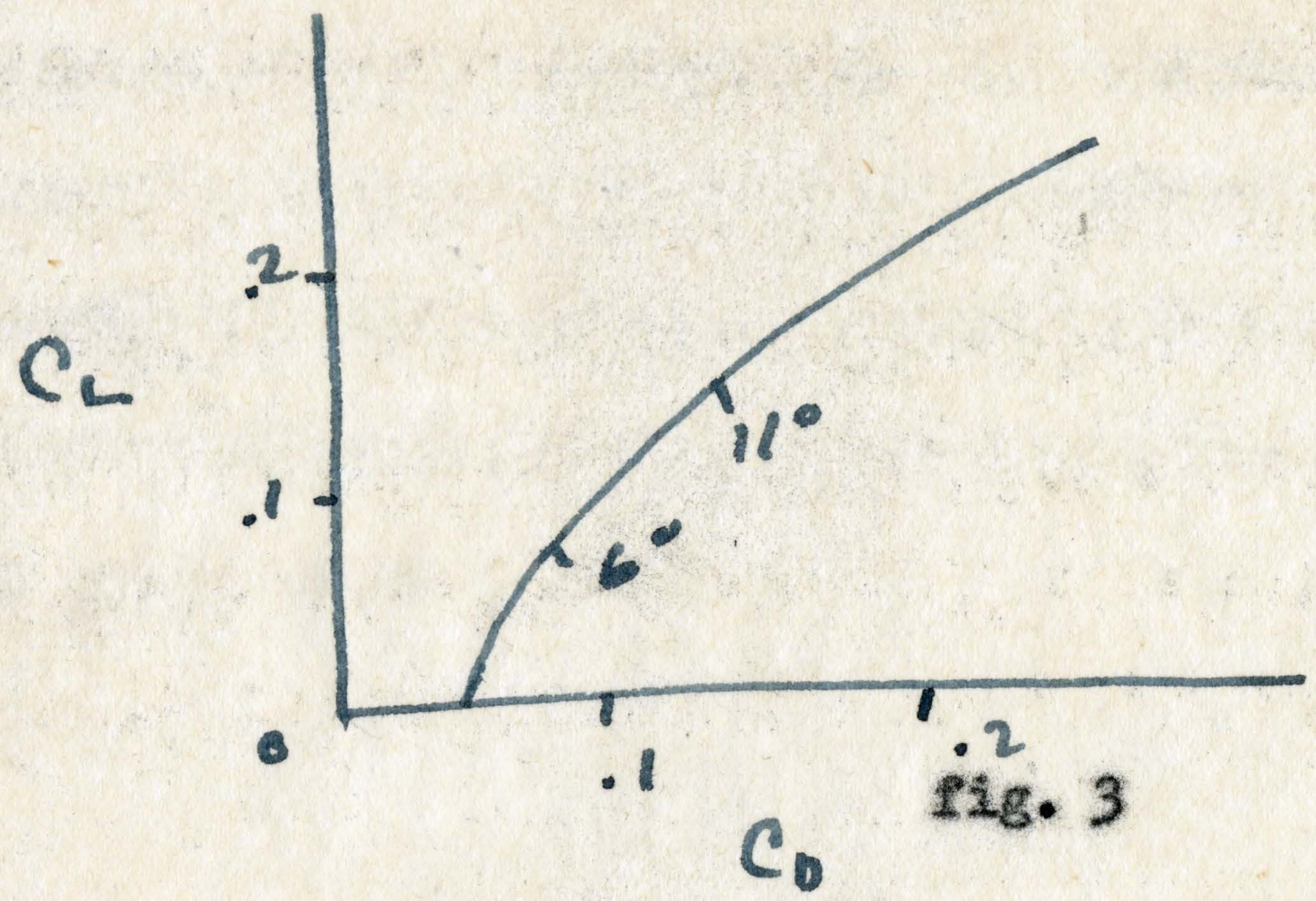

fig. 3

The values of $\underline{\alpha}$ for each point are indicated at these points and a curve drawn through them. Thus for a given value of $\underline{C_L}$, $\underline{C_D}$ and $\underline{\alpha}$ are fixed. This means that the drag and lift coefficients of the rigid body cannot be varied independently, and since, in equation (2) the quantities ρ, V, and S are not subject to change, the drag and lift are mutually dependent upon the angle of attack.

For soaring the drag and lift must also be related according to equation (1) .

Unless the drag and lift coefficients given by the formulas

$$C_D = \frac{W_s \sin \theta}{\frac{1}{2}\ S\ V^2}$$

$$C_L = \frac{W_s \cos \theta}{\frac{1}{2}\ V^2 \cdot S}$$

define a point which lies on the curve in fig. 3, soaring of the rigid body is impossible, and if the above point does lie on the curve then soaring occurs only if the angle of attack α is the correct value, for the curve at that point.

In the case of a living porpoise, the requirements for soaring are not so rigidly specifiable. For example, because the porpoise is capable of varying the swimming surfaces (as the elevators of an airship) the curve of fig. 3 depicting the relation of C_D to C_L for a rigid body must give way to an area (fig. 4) which is the porpoise's "drag-lift" range

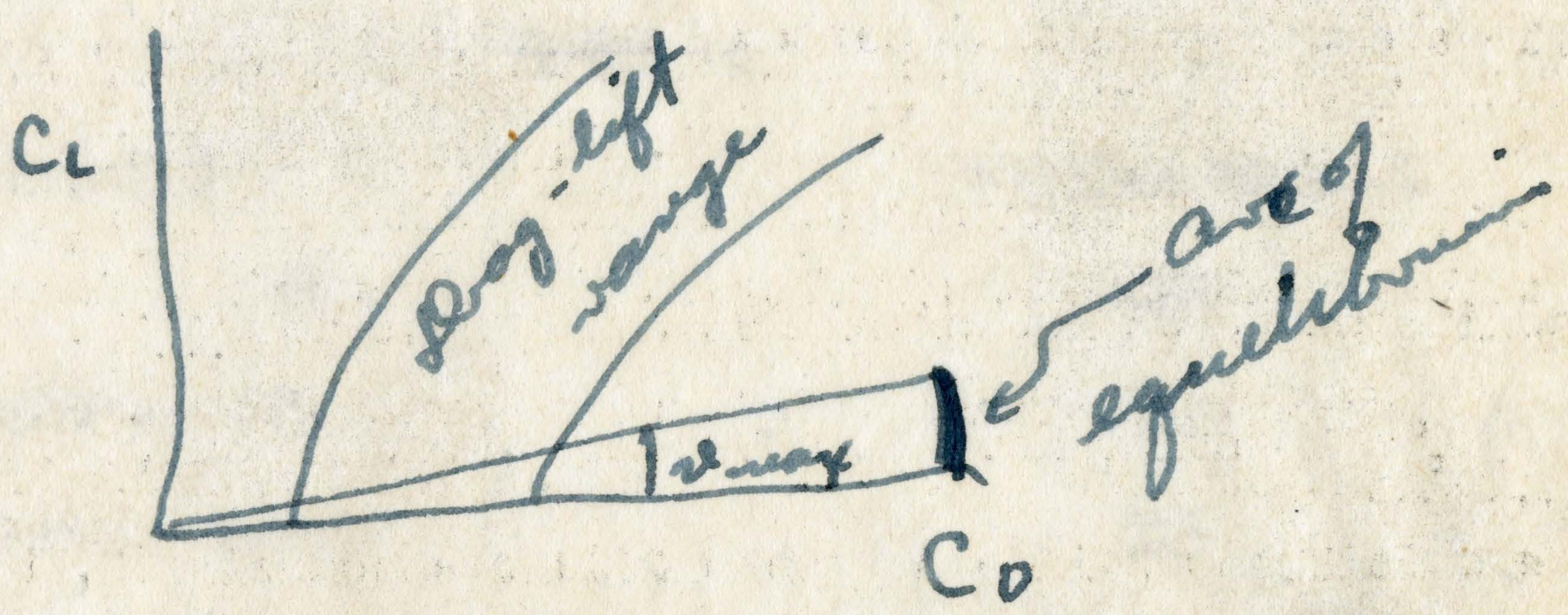

fig. 4

Although the porpoise probably cannot use his submerged weight W_s as an effective control, he can seek various values of within the wave which seem suitable to him. This means that there is a certain arc of a circle

$$C_D^2 + C_L^2 = \frac{W_s^2}{\frac{1}{4}\rho^2\rho^2 V^4}$$

in figure 4 extending from the C_D - axis to a certain angular height θ max which is the maximum angle of inclination to the horizontal of the streamlines at any point in the wave. By seeking the appropriate portion of the wave the porpoise may vary his equilibrium measurements along the arc. If this arc of equilibrium requirements coincides with his drag-lift range he may soar: that is the condition for soaring is that some portion of the arc lies within the drag-lift area.

The exact shape and disposition of these areas in the C_D, C_L plane is not yet known for a living porpoise, but for purposes of discussion let us consider a porpoise who will operate only in the drag-lift range of the rigid body of figure 3. The porpoise is 2 m long, 600 cm^2 cross section and travels at 500 cm sec^{-1} (10 knots).

Table 1.

	Model (air)	Porpoise (water)
l cm length	81	200
S cm^2 cross section	134	600
V cm sec^{-1} velocity	1,805	~~500~~ 200
ν cm^2 sec^{-1} kinematic viscosity	1.5×10^{-1}	10^{-2}
R Reynolds No.	10^6	$\cancel{10^7}$ 4×10^6

His Reynolds No. is sufficiently close to that of the model to justify using the curve of figure 3.

Let us suppose that the maximum value of θ in a wave is 15°, so that the arc of equilibrium will be limited by the line OB drawn with a slope of tan 15°. His weight submerged is taken as 9 lbs, and arcs drawn for various speeds. Only arcs for a very narrow range of velocity intersect the drag-lift curve, so that the range of wave speed in which this porpoise can soar is only approximately

3.8 ± 0.1 ~~9 ± 0.3~~ knots.

An actual porpoise probably has the ability to soar over a somewhat wider range of wave velocities. First, because his versatility of control widens the drag-lift curve into an area by change of form or suppression of turbulence and second because he may vary his weight submerged. In the above example he could soar in a ~~4 knot~~ slower wave if he retained more air in his lungs. ~~and reduced his weight submerged.~~

Walter: This is a copy of letter I sent Dr. Defant.
Did I tell you that Rosemary Holmes & I are going
to get married? We will I guess in about a
year when she gets her M.D.

Sincerely, Hank

December 19, 1949

Dr. Albert Defant
Scripps Institution of Oceanography
La Jolla, California

Dear Dr. Defant:

Dr. Iselin will be out of the country for a large portion of the spring of the coming year, and so he has delegated to me the very pleasant duty of making arrangements for your visit to Woods Hole. We should be delighted to have you stay with us a week or more in late February or early March. I will make all the necessary arrangements for local lodging and transportation. For example, I can easily drive to Boston by automobile to meet you at the airport; and we may quite simply drive over to Providence to see Dr. Montgomery at any time. Perhaps you will be willing to give a lecture on the breaking of internal waves at a Brown University Oceanography Colloquium.

It will be a great pleasure to welcome you here.

Yours truly,

Henry Stommel

HS:ph

WOODS HOLE OCEANOGRAPHIC INSTITUTION
WOODS HOLE, MASSACHUSETTS

December 22, 1949

Dr. Walter H. Munk
S∆I∆O∆
La Jolla, California

Dear Walter,

Thank you very much for your letter with all the news. I have passed on to Tom your advice about his looking into the possibility of studying with Montgomery.

You have probably received by now a copy of my letter to Defant. I will assume the responsibility for arrangements for his visit here.

The Colloquium at Brown seems to be working out swell. I gave the first one, and drew a bit of fire from Lettau. His aim was wild so I am still alive. In January Stetson will talk, in February, Riley, and possibly Defant in March.

I have carried out several days' runs on small laboratory scale thermoclines. I got hold of an old Atlantis garbage can; filled it with water and on top of that a layer of kerosene. I stir the layer of kerosene with a gentle 1 rpm rotary motion, and can heat it. The rotary motion is communicated downward, and so is the heat, and all the time I have a little recording thermocouple travelling up and down. I get a very definite thermocline after several hours of this treatment; dividing the garbage can liquid into two layers, the epican and the hypocan. There are what appear to be turbulent fluctuations in the temperature of the epican; but the hypocan remains fairly limpid and shows no turbulent fluctuations. I have found that the eddy thermometric conductivity in the hypocan is nearly molecular in magnitude; but in the epican it appears to be nearly 100 times as large. I have not measured the shear very carefully yet, but I naturally hope to do so: To get some empirical information about the relation of eddy coefficients; stability; and shear.

Gorbis H

Have you seen Kuelegan's remarkable paper in the N.B.S. Journal of Research - just out - on the instability of shearing motions in stratified liquids - some interesting empirical data.

My best wishes to you Walter, for a Happy Christmas, and to Bob Arthur, and the others......... I think of you all with nostalgia........

Yours sincerely,

Hank

December 28, 1949

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

Dear Hank:

First of all let me give you my heartiest congratulations on becoming engaged to Rosemary Holmes. She is such an intelligent and nice looking girl, I think you are very lucky, and the two of you will spend a good and successful life.

I find your paper on the porpoise very interesting. I have kept the duplicate set and have sent the original to Hunter Rouse with the request that either he or members of his staff look the papers over. They are so much better qualified for making critical remarks about your paper that I do not feel that I can add much, except to say that I find it very interesting.

A mimeographed set of Dr. Eckart's notes is now being prepared by Bob Reid, and I have put you down for a copy. Also I have noticed in one of Defant's recent papers a reference to a computation of tides by numerical processes: Hansen, 1948. It sounded quite similar to your work and I am planning to send you a complete reference when Defant returns after New Year.

I am also intrigued with your note to Bob Arthur concerning the effect of solar tides on levelling surfaces. I hope something comes of it. You are familiar, I suppose, with the fact that the water level along the California coast apparently rises toward Seattle.

Since you are going to take care of Defant's visit I suggest you make sure that you obtain a letter written by Roger Revelle to Columbus on the 23rd of December. It concerns certain details of Professor Defant's visit. By the way, your remark that Dr. Iselin will be out of the country has made me very curious. Where is he (or is the information highly classified)?

I found by chance that the variation in the angular velocity of the earth due to fluctuations in the westerlies, which I estimated to be of the order of one part in 100 million, have actually been observed and are of the right sign and right magnitude. Mil and I will rewrite our little note. I am quite excited about this as it does indicate the possibility <u>now</u> of using astronomic data as a tool in meteorology.

Best regards,

Walter H. Munk

WHM:es

Jan 5

Henry Stommel

Dear Walter —

Thank you for the letter + for sending on the porpoise work to Hunter Rouse.

The only thing I know about Columbus's being away is that he + his wife are planning a tour — a kind of extended vacation — and I think he needs one — he seems quite tired.

I was glad to hear that Your + Mil's paper now has good observational support in the correlation of fluctuations in the Westerlies + angular velocity of the Earth.

Right now I am in the midst of experimenting with the thermocline in the garbage can. It's lots of fun & I get skew, temp.; I plotted some of the A's against Richardson number & get a graph resembling some of those in your & Anderson's paper. Probably fortuitous, though.

My best to you — and I do hope Martha is feeling better.

Yours truly
Hank

January 13, 1950

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

Dear Hank:

Some time ago I mentioned a reference of someone who had computed tides by numerical processes. The reference is

> Hansen, W., Die Ermittlung der Gezeiten beliebig gestalteter Meeresgebiete mit Hilfe des Randwertverfahrens. Deutsche Hydrographische Zeitschrift, Heft 5/6, 1948.

I understand the same author had a subsequent article in the same journal.

Love,

Walter H. Munk

WHM:es

WOODS HOLE OCEANOGRAPHIC INSTITUTION
WOODS HOLE, MASSACHUSETTS

of the ocean circulation. You remember the ozalid copies I distributed and which came out with such terribly long times. By this time you probably have your own thoughts about this topic all written out, and I wonder if I might not have a copy. I promise to read them carefully as I really am interested and a little distraught as to understand why we cannot squeeze a little better agreement between our two approaches.

Has Eckart done anything on the transient current problem yet?

I wonder if we should publish some kind of collection of papers or symposium on transient ocean currents, or whether we should just save the whole thing for our book and then thrash it out as best as we can.

I was out to the University of Wisconsin to talk with Hasler and the limnological people about lakes in general and the circulation of water in lakes in particular. Mendota is a good lake to work on, I think, because they have all the streams gaged, and are going to commence an intensive bathythermograph survey. James Verber - a very nice young geographer - is doing the field work. He has been trying to measure currents at various depths, and has come out with some interesting results, among which are fast currents in the hypolimnion: 13 m sec-1 !

I met old E. A. Birge too. I was sure he was dead long ago. He retired from the presidency of the Univerisyt in 1927, and is now going on 99.

Saw Phleger there too... he was giving a lecture on his tour.

Please give my best regards to all my friends, and my best to you too...

Yours sincerely

Hank

Henry Stommel

WOODS HOLE OCEANOGRAPHIC INSTITUTION
WOODS HOLE, MASSACHUSETTS

February 9, 1950

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

Dear Walter,

Cmdr. Soule showed me a manuscript by Harry Carter entitled "On the circulation maintained by the lateral shearing stresses with application to the currents off the Grand Banks of Newfoundland". I assume that you have already got a copy from Harry, or will have one soon. I should like to outline my opinion of the paper to see if you concur, or whether I am sadly mistaken.

Carter starts by rederiving your equation which in the steady state must represent a prevailing balance between the planetary vorticity, the shear vorticity, and the wind stress curl. He then draws up a chart of the wind stress distribution over a small area off the Grand Banks for a two week period. I am not quite clear how this average wind stress was obtained, or why the period two weeks was chosen, or how changes in the winds during that time were taken into account. By this time I was fairly certain that the next step was going to be to drop the shear vorticity as too small and then use Sverdrup's 1947 relation between planetary vorticity and wind curl. To my surprise, however, he did just the opposite. He dropped the planetary vorticity and wind curl, and simply sets the stress vorticity equal to zero. This comes about because he assumes a large value (greater than 10^7 $cm^2 sec^{-1}$) for the lateral eddy viscosity. Because the phenomenon under discussion covers an area of only ~~a few hundred miles length (~~ 16 square degrees~~)~~ I think that the value of the lateral viscosity assumed is too large by a factor of, say, 100. In other words, I rather suspect that the thing has been done just backwards, and that it would have been better to use Sverdrup's 1947 equation. What he has really done is to solve a problem in viscous flow, quite independently of the wind distrubution - which in the end he does not use at all.

I should appreciate your comments on my comments, because I admit that I frequently miss the point in scientific papers in my hurry to scan them through, and I don't want to do Harry an injustice.

I have been struggling with my conscience to decide whether or not there is anything worth while in my work on time constants

February 14, 1950

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

Dear Hank:

Many thanks for your letter of February 9. Your comments on Harry Carter's paper interest me a great deal and I think you are quite correct. The curious thing is that I have not seen Carter's work except in the very early stages four or five months ago and I am a little distressed that he sent a manuscript to Comdr. Soule before I had a chance to go over it.

Regarding your questions on ocean transient currents, I have for my part not done any more about it until I would learn some more about Carl Eckart's thoughts on the subject. I expect he will have some more to say about it during the coming term. It might be a good idea, Hank, if you were to hold up publication of your notes until I could forward some information on Eckart's work. If and when this is forthcoming it might be a good thought to put our two approaches together. We might do then when I get East in connection with OPERATION DREAM. However, you may wish to send your notes in before that time, and this would, of course, be perfectly all right with me.

I am sorry that I have never written up my notes, but if I shall I will forward you a copy immediately.

Sincerely yours,

Walter H. Munk

WHM:es

P. S. Did you notice the quotation of your letter to Scientific Monthly (Sigma Xi), which appeared in the last issue?

February 28, 1950

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

Dear Hank:

I am enclosing Hunter Rouse's reply for what it is worth. I think that in a way he has somewhat missed the point and I hope you will not find his reply discouraging. It certainly would not be sound procedure to delay any consideration of the movement of animals through the water until we have completely solved the problems involving the flow of rigid bodies through fluids.

Best regards to you.

Sincerely yours

Walter H. Munk

WHM:es
Enc.

March 1, 1950

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

Dear Hank:

Thanks for your post card of February 25. I am fully aware that Carter has not used complete boundary conditions and I have pointed this out to him.

Please tell Dean Bumpus that as far as I can say now the dynamics of the Gulf Stream lead us to a circulation pattern <u>in-shore</u> of the Gulf Stream very similar to the one he observed. I hope to send him some preliminary results in the near future.

I am enclosing a copy of a paper I have submitted to Tellus.

Sincerely yours,

Walter H. Munk

WHM:es

Henry Stommel

Mar 8 '50

Dear Walter —

Many thanks for the extremely interesting manuscript copy of you & Carrier's work. You have really answered Sverdrup's question about triangular oceans in fine style.

My best to you & Martha

Hank

Henry Stommel

Dear Walter,

Thank you very much for sending the manuscripts on to Hunter Rouse & the reply.

I think I was very wrong to use the weight submerged. I overlooked the fact that the inclination of the pressure surfaces inclines the buoyancy from the vertical. My analysis is therefore void. [The # numbered (ii) on page 1 is wrong physically]

This has been a great help & I think perhaps I can straighten out the mess, after thinking a bit more about it.

My best to you & Martha

Hank.

April 6, 1950

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

Dear Hank:

Your suggestion concerning the effect of slippage against the western boundary has prompted me to add the following pages to my paper.

Thanks a lot.

Yours,

Walter H. Munk

WHM:es
Enc.

WOODS HOLE OCEANOGRAPHIC INSTITUTION
WOODS HOLE, MASSACHUSETTS

April 13

Dear Walter:

A combination of things – mostly Rosie – has influenced me to explore the possibility of my entering S.I.O. in the fall as a graduate student seeking a Ph.D. Perhaps you already have heard of it from Roger.

Is it possible for me to make any advance application for housing? If I could rent one of the cottages starting in the fall it would be marvelous, and Rosie would join me in the spring. Are you on the Housing Committee, or should I write directly to Dr. ZoBell?

Yours truly,
Hank Stommel

April 18, 1950

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

Dear Hank:

Beth Strong and I have sent out a housing application form for you and Rosie. I think the chances are fair that a vacancy might turn up by then. If nothing turns up on the campus at the time of your arrival, we certainly shall try also to find something in town for you.

We all have heard that a combination of things - mostly Rosie - have influenced you on coming to Scripps again. We all think it is swell that you should do so, although I agree with Roger that it would be appropriate to give you a degree sight unseen.

Hope to see you at the meeting at Washington.

Yours,

Walter H. Munk

WHM:es

July 28, 1950

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

Dear Hank:

I am pleased to see that you have translated my paper into English and that you will submit the translation to the Bulletin.

I have only two remarks on your manuscript. On Page 2, Line 15, add a comma after "system." Secondly, I would insist that you make reference to your AGU paper on the westward intensification. Actually the numerical values in Table 1 would have followed from your paper just as well. The principal modification is the substitution of lateral stress for vertical friction.

We are a bit mystified about your sudden change of plan? Do you want me to cancel your application for housing? What are you planning to do?

Yours,

Walter H. Munk

WHM:es

HENRY STOMMEL
Woods Hole, Mass.

Dear Walter,

Thank you ever so much for looking over the article so kindly. As you know from my writing to Roger I won't be coming to SIO this fall for study. I'm staying on at Woods Hole to work on my program here.

I should have written you earlier requesting to have my name removed from the housing list.

Rosemary gave me the boot. That's a closed chapter too.

My best to you all at Scripps.

I think of you often + the good easy going friendly atmosphere at Scripps.

Hank.

July 31, 1950

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

Dear Hank:

The attached letter has just come back to me. I am forwarding it to you for whatever you think should be done with it.

Yours,

Walter H. Munk

WHM:es

Dec 11

HENRY STOMMEL
Woods Hole, Mass.

Dear Walter,

Thank you very much for the translation.

I was married last Wednesday to Elizabeth Brown. You must stay with us at our new house when you are in Woods Hole again.

Hank

WOODS HOLE OCEANOGRAPHIC INSTITUTION
WOODS HOLE, MASSACHUSETTS

October 26, 1950.

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

Dear Walter:

As a result of the interest shown by a number of oceanographers in this country on the treatise on "Physics of the Sea" (Fizika Moria) by V. V. Shuleikin, published in Russian, Moscow, 1941, we are undertaking to translate portions of it here at Woods Hole, under the sponsorship of the Office of Naval Research.

Because large sections, especially those on waves and tides, for example, are believed to be duplicated in English texts on oceanography and hydrodynamics, a complete translation of the book is not contemplated at this time.

It would be a great help to us if you could inform us which sections of this treatise you would particularly care to have translated. We can set up some kind of priority arrangement, have a few duplicated copies of the translation run off as it is made, and send them to you piecemeal.

From a cursory examination of the Table of Contents I believe the material in each chapter is roughly as follows, but because I am unable to read Russian, I may be missing some important points in this summary:

Chapter 1. Dynamics of the Ocean. (Deals with the classical Bjerknes theory, Ekman's spiral, Ekman's theory of currents in shallow water and near the coasts, discusses Rossby's wake stream theory of the Gulf Stream, etc.)

Chapter 2. Dynamics of Tides. (Besides a theoretical study there is a discussion of some special deep sea tide gauges.)

Chapter 3. Surface Waves. (Contains what appears to be refraction diagrams and an instrument for determining certain characteristics of waves from the pattern of reflected sun and moon light.)

Chapter 4. Temperature of the Ocean. (Contains detailed discussion of radiation penetration into the sea, methods of measuring temperature and wind profiles above the sea, a discussion of mixing processes and an application of the method of Kernschichte to the North Siberian Shelf.)

Chapter 5. Physical Climatology. (There appear to be discussions of the climate of the Russian Arctic and its connection with arctic oceanography.

Chapter 6. Optics of the Sea.

Chapter 7. Acoustics of the Sea. (This chapter seems to be fairly antiquated and does not appear to contain even such things as refraction diagrams.)

Chapter 8. Molecular Physics of the Sea. (I am not quite certain how to evaluate this chapter, which seems to contain a lot of information usually not present in ordinary oceanographic texts.)

Chapter 9. Biological Physics of the Sea. (Contains among other things Shuleikin's extensive study on the swimming of porpoises.)

Chapter 10. Technical Physics of the Sea. (This appears to be an interesting chapter on methods of controlling the roll of ships, hull designs, and a number of other things which I am unable to make out from the Russian text.)

Very sincerely yours,

Hank

Henry Stommel

HS:ds

October 30, 1950

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

Dear Hank:

I was very pleased to hear about the plans for the translation of Schuleikin's book. Giff Ewing came back from Washington telling us that there was a chance that you might arrange for it, and we have hoped that it might materialize. Giff is away for a few days, but I know that he will be most pleased to hear about your plans.

We have already had translated pages 214 to 225 covering a portion of the section entitled "Several Methods of Measuring Wave features" in the chapter on Surface Waves. A copy of this will be sent to you some time during the next two weeks so that you might make use of it. We find at least in this section that his treatment of waves is stimulatingly different from the usual one. I would urge very strongly that this chapter be translated. From what I have been able to find out there is relatively little duplication with English texts.

Otherwise I find myself very interested in sections of Chapter 4. In addition, Chapters 9 and 10 look intensely interesting, although of course the work is outside my own field.

To summarize, the sections I am particularly anxious to have translated are Chapters 3, 4, 5, 6, 2 and 1, in that order.

Many thanks for your letter.

Yours,

Walter H. Munk

WHM:es

cc: Gifford Ewing

WOODS HOLE OCEANOGRAPHIC INSTITUTION
WOODS HOLE, MASSACHUSETTS

November 8, 1950

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

Dear Walter:

This coming week-end Rossby is going to be at Woods Hole to reexamine some of the CABOT cruise data in the light of some new ideas which he has been developing at the University of Chicago during the last month or so. I don't know right now just what he has in mind, but I understand that following his visit here he is going to spend some time out at Scripps, so we will probably hear many of the ideas in embryo form which you will hear in a few weeks.

You may be interested and happy to know that Bill von Arx is planning to construct a large rotating model for experiments on flow with Coriolis forces. I emphasized to him the extra value of such experimental studies if he could include a variable Coriolis ~~force~~ parameter and so the model that he has in mind now is a 10 foot diameter parabaloid of revolution which would rotate at about 23 RPM and would vary in slope from zero at the center of rotation to a slope of about 45 degrees at the extreme radius. The vertical (refer the normal to the plane tangent to the parabaloid at any point taken as a level surface) component of the Coriolis parameter would therefore vary from a maximum at the center of rotation to about 71% of the maximum at the rim of the parabaloid. This seems to be as much of a variation of the Coriolis parameter as is practically obtainable.

Bill then plans to fill the rotating parabaloid with water, say 2 or 3 centimeters deep, and to construct a system of fans for producing artificial winds, and to build in movable meridional barriers. He has many ingenious ideas about measuring the level of the free surface, about introducing two layer systems, etc. At present he is thinking in terms of Raynolds scaling. I wonder if it would not be more appropriate to use scaling according to the wave

Coriolis Number →

number $\lambda = \sqrt[3]{\beta / A}$ which after all determines the extent of western intensification rather than the Reynolds number.

bottom smooth
sides artificially rough
deep water as possible

Yes

A difficulty enters here, however, namely that bottom friction may conceivably be more important in such a model than lateral friction. As a matter of fact, do you think that there is a possibility that the bottom friction in such a model would be so large, as compared to the ocean, that the ~~bounce~~ between wind stress curl and planetary vorticity which occurs in the real ocean would be disturbed by the magnitude of the frictional terms? It is my opinion that every effort should be made in the model to reduce bottom friction as much as possible.

balance

Stability
dimensional
numbers

The prospect of having a rotating model at Woods Hole to play with is a very exciting one, and I am only bringing these questions up at this time, not to be discouraging but to be sure that maybe some very fundamental change in the design of the model might be necessary---for example, should the model ocean be made very deep rather than just a few centimeters. In other words, may not this rotating model be one of the few cases in which dynamic similarity is best achieved through an extreme exaggeration of the vertical scale (in contradistinction to what holds in most dynamic models).

My Theory

not in line theory

Upon looking over the results of the CABOT cruise I am more and more impressed by the difficulty of rationalizing the existence of the warm core with the equation of heat transfer and the vertical current sections. There seems to be some kind of fundamental difficulty in moving a narrow stream of warm water from low to higher latitudes. No matter what one does he always seems forced to make the right hand side of the warm core (Northern hemisphere) move more slowly than the left hand side, and this seems to contradict the notion of the current as determined by a warm tongue in which one would expect the strongest current to be in the middle of the core. I have tried setting up some theoretical models in which the heat transfer equation is combined with the ordinary geostrophic current equation, and I always come to an impasse.

Interesting

Rossby says that a steady warm tongue flowing in a northward direction is impossible and that it has to be an intermittent phenomen~~a~~on. As a matter of fact, Val Worthington has noticed, on plotting up the top 200 meter mean temperatures from the CABOT cruise, that the most central portion of the warm core occurs in blobs rather than extending continuously along the current. This seems to fit with Rossby's contention, but everywhere with the exceptions of these warm blobs, there is some indication of a warm core.

One would like to see some combination of the method of Kernschichte with the geostrophic current equation.

My best to you and Martha, and I hope you receive the 16 mm. movie film okay.

Jer

Sincerely yours,

Hank

Henry Stommel

HS:ds