Note on Dr. Rosenhead's Papers, "The Annual Variation of Latitude" and "Tides on a Two-layer Earth"

By

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Abstract

In both the papers cited above, Dr. Rosenhead refers to my work[1] on "the causes of the annual variation of mean sea level along the Japanese coast", and states, in the former, that the annual variation of latitude can be explained completely by the application of my results, and in the latter that the tides on the yielding earth would be in conflict with my evidence.

If those papers are carefully examined, however, it will be found that the statements mentioned ought to be reversed. That is to say, Rosenhead's results for the tides on a two-layer earth are in harmony with my own results, not in conflict at all, while the paper on latitude variation contains two serious errors—mis-uses of my results—which render his calculations useless. The present paper will show and discuss those misunderstandings and errors.

I. On the Paper on Tides

§ 1. Dr. Rosenhead published an investigation[2] upon the "tides on a two-layer earth" in the Geophysical Supplements to M. N. R. A. S., and stated in it that

> "The work of the present paper appears to indicate that the equilibrium theory can account for the phenomena of the long-period tides, whereas the evidence of Nomitsu and Okamoto indicates the *reverse*—at least for the North Pacific,"

and in the summary also that

1. These Memoirs, A, **10**, 125—161 (1927).
2. Geophys. Suppl. M. N. R. A. S., **2**, 171—196 (1929).

"The values of k and $(1+k-h)$ derived from the long-period tides appear to indicate that the equilibrium theory can account for the long-period tides. This is *in direct conflict* with the experimental evidence of Nomitsu and Okamoto which deals with the annual variation of sea level in the North Pacific."

Seeing these words in much question, however, I examined his paper carefully and found that his work and mine had no direct relation with each other and that the two results are in rather good agreement. It is entirely a misunderstanding on his part to consider that the two are in conflict.

§ 2. Let me here pick out the essential parts of Rosenhead's paper. Assuming the earth as a two-layer globe of Jeffreys' type i. e.

	Outer Shell	Inner Core
Radius	6.37×10^8 cm. $=a$	$0.545a$
Density	4.27 gm./cm.^{3}	12.04 gm./cm.^{3}
Rigidity	1.695×10^{12} dynes/cm.^{2}	0

Rosenhead investigated the effect on the ocean tide of the deformation of the yielding earth produced by the external disturbing forces, once statically and once dynamically. Then he calculated the values of Love's numbers,* h and k, and obtained the following results.

	Statical Theory		Dynamical Theory			
	Including the effect of ocean	Neglecting the ocean effect	Fortnightly, Ocean depth 2162 m.	Fortnightly, Ocean depth 8852 m.	Semidiurnal 2162 m.	Semidiurnal 8852 m.
h	0.550	0.667	0.583	0.569	0.651	1.706
$c=1+k-h$	0.720	0.697	0.705	0.710	0.468	0.093
k	0.270	0.364	0.288	0.279	0.119	0.799

His conclusion from these results is that the equilibrium theory can account for the phenomena of the long-period tides, although it differs greatly from the real (dynamical) for short-period tides like the semidiurnal.

§ 3. That is true so far, but he goes too far in saying that the evi-

* h=Earth tide/Equilibrium tide corresponding to the disturbing force.
k=Secondary tide due to the deformation of the crust/Equilibrium tide.

dence of Nomitsu and Okamoto indicates the reverse. As the investigation of Rosenhead is an application of Herglotz's method[1] to the yielding earth covered with a *homogeneous* ocean and deals with only such forces as may produce some disturbing action upon the earth crust itself, e. g., the pressure on the sea-bottom, it can have no direct connection with the actual amount of seasonal variation of mean sea level, of which there are various causes but the chief cause is the difference in water-density.

The reason why Rosenhead, notwithstanding this, goes so far beyond the limit to say is probably his misunderstanding of our result that along the Japanese coast the statical effect of the atmospheric pressure is only about a third of the actual variation of mean sea level. He has indeed supposed this to mean that the actual fluctuation of sea level is about three times that of the equilibrium tide corresponding to the disturbing forces—very far from the statical theory.

But that is clearly a surprising misconception of our result unless the barometric variation were the only cause of the whole fluctuation of the sea level. I have however shown that the annual variation of sea level along the Japanese coast is much more effected by the change of water density than by the barometric fluctuation, and that the conjoint effects of these causes all considered *statically* account for the actual motion of the sea level. Moreover as to the influence of water-density, I have preparatorily given a theoretical proof[2] that it might be calculated *statically* if Coriolis' force was neglected. The case where Coriolis' force is considered will be discussed in a forthcoming paper[3].

Thus, the two papers on tides by Rosenhead and myself have no direct connection and they are not opposed at all, but rather in accord in the view that the statical theory can account for the long-period tides.

II. The Paper on Latitude-variation

§ 4. In the Geophysical Supplements Dr. Rosenhead has written also another paper[4] dealing with "the annual variation of latitude" in

1. Zeitschr. f. math. u. phys., **52**, 275 (1905).
2. These Memoirs, A, **10**, 111 (1927).
3. These Memoirs, A, **15**, 131 (1932).
4. Geophy. Suppl. M. N. R. A. S., **2**, 140—170 (1929).

which he states that this phenomenon could be completely explained if, in addition to the causes enumerated in Jeffreys' theory,[1] another and new correction due to oceanic motion were made, employing the results in my paper on the annual variation of mean sea-level.

But examining his paper carefully, one will find two fundamental errors—misuses of my results—which render his calculation futile.

§ 5. In order to explain the points, let us first trace the outline of his work briefly.

Let λ and μ denote angular displacements of the momental axis of the earth from its undisturbed position towards and perpendicular to the Greenwich meridian. Accepting Jeffreys' view that the yearly portion of the latitude variation corresponds to the change of moment of inertia caused by the transference of mass over the earth's surface, Rosenhead obtained from Pollack's analysis[2] of 35-year observations the following values of λ and μ as observed:

$$\left.\begin{aligned} \lambda_{\text{obs.}} &= -0''.0104 \sin\odot - 0''.0028 \cos\odot \\ \mu_{\text{obs.}} &= +0''.0368 \sin\odot - 0''.0010 \cos\odot \end{aligned}\right\} \qquad (1)$$

where $\odot$ represents the sun's longitude.

On the other hand, suppose the annual component of the variable portion of mass per unit area on the earth's surface to be

$$m = \frac{1}{2}\sigma\cos\odot + \frac{1}{2}\sigma'\sin\odot \qquad (2)$$

where σ and σ' are functions of θ the colatitude, and ϕ the longitude east of Greenwich. Then, if the earth were rigid, the corresponding values of λ and μ would be

$$\left.\begin{aligned} \lambda_0 &= -18''.2 \times 10^{-3} \int_0^{\pi}\int_0^{2\pi} m.\ \sin^2\theta\cos\theta\cdot\cos\phi\ d\theta\ d\phi \\ \mu_0 &= -18''.2 \times 10^{-3} \int_0^{\pi}\int_0^{2\pi} m.\ \sin^2\theta\cos\theta\cdot\sin\phi\ d\theta\ d\phi \end{aligned}\right\} \qquad (3)$$

For the actual earth, however, by virtue of its yielding character, these values must be somewhat reduced to such as

$$\lambda = c\lambda_0, \qquad \mu = c\mu_0, \qquad (4)$$

where c is a reduction factor less than 1. According to Rosenhead

1. Monthly Notices Roy. Astr. Soc., **76**, 499—525 (1916); See also "The Earth," 238—249 (1924).

2. Gerlands Beiträge f. Geophys., **16**, 108—194 (1927).

it will be 0.696 for a two-layer earth of Jeffreys' type and approach to 0.6 for many-layer earths.

As such annual changes in the distribution of mass over the earth's surface that could contribute to the latitude variation, Jeffreys enumerated the change of atmospheric pressure, precipitation of snow, and periodic change in vegetation. Besides these three, Rosenhead presumed the change of sea level as a fourth cause, and calculated separately all their contributions to λ_0 and μ_0 by formula (3) getting the following results:—

Causes	λ_0		μ_0	
	Coef. of sin⊙	Coef. of cos⊙	Coef. of sin⊙	Coef. of cos⊙
Precipitation of Snow	+0″.0175	−0″.0238	+0″.0093	−0″.0126
Vegetation	−0″.0038	+0″.0017	−0″.0055	+0″.0024
Atmosphere	+0″.0102	−0″.0020	+0″.0509	−0″.0077
Oceanic Motion	−0″.0393	+0″.0189	+0″.0069	+0″.0165
Sum	−0″.0154	−0″.0052	+0″.0616	−0″.0014

Finally adopting $c=0.6$ in eq. (4), Rosenhead obtained the result

$$\left.\begin{aligned}\lambda=c\lambda_0&=-0''.0092\sin\odot-0''.0031\cos\odot\\ \mu=c\mu_0&=+0''.0370\sin\odot-0''.0008\cos\odot\end{aligned}\right\}\qquad(5)$$

This is so near to eq. (1) that it makes him consider the problem perfectly solved.

§ 6. Now for the contributions from snow and vegetation in the foregoing table, Jeffreys' values have been accepted without any alteration; but the remaining contributions from the atmosphere and oceanic motion are the result of Rosenhead's recalculations, in which I perceive two serious mistakes.

One mistake lies in his conception of the contribution from atmospheric pressure. To calculate the barometric contribution he employed also Jeffreys' ingenious method[1] of using only the land values of atmospheric variation, in which it is unnecessary to know the sea values. Namely if m_1 and m_2 be the variable parts of mass due to the barometric variation per unit area over any point of the land and

1. Loc. cit.

sea respectively, then by the law of indestructibility of matter it follows that

$$\iint_{\text{land}} m_1 dS + \iint_{\text{sea}} m_2 dS = 0.$$

If we here notice the atmospheric air only, the sea value m_2 as well as the land value m_1 will differ according to locality; but if we consider, after Jeffreys, the atmospheric variation together with the sea motion, which will balance each other *hydrostatically*, then m_2 must be constant all over the ocean and it can be determined by the land values of m_1 only, i. e., from the above equation

$$m_2 = -\frac{\iint_{\text{land}} m_1 dS}{\iint_{\text{sea}} dS} = -\overline{m}_1 \frac{\text{Area of land}}{\text{Area of sea}} = -0.40\overline{m}_1, \qquad (6)$$

where $\overline{m}_1$ denotes the mean value of m_1 over the land.

As Rosenhead employed relation (6) to calculate the barometric contribution with eq. (3), his result must necessarily include that part of oceanic motion which keeps *in hydrostatic equilibrium* with the atmospheric pressure. Notwithstanding this, he takes it to represent the effect of the *air only*, and does not subtract that part of oceanic motion taken already into account from his own "correction due to oceanic motion."

§ 7. Secondly his new "correction due to oceanic motion" is entirely a mistake. After introducing the outline and summary of my paper on the annual variation of sea level, Rosenhead surprisingly misinterpret the fact that the whole variation of sea level amounts to about three times that due to the statical effect of barometric pressure. He says

"The essential point of this paper is that, while the actual and theoretical motions (of ocean water) agree in phase, the amplitude of the actual motion is about three times that of the equilibrium tide,"

as if only the barometric pressure were the total cause of the whole fluctuation of sea level.

This misunderstanding causes him to state

"The actual variation of sea level is three times that due to barometric variation."

Refering to the surface loading contributing to λ_0 and μ_0, he goes to say, "An increase of atmospheric pressure will have two effects. The *first effect* will be to increase the surface loading on account of the increased weight of the superposed column of *air*. This effect has already been accounted for in § 4, 2..............

"The *second effect* of the increase in atmospheric pressure is to depress the level of the sea and thus diminish the surface loading. Hence if we analyse the pressure variation over the sea as we have analysed the pressure variation over the land, then we have to *multiply the result by* **-3** to obtain the exact correction to allow for the annual variation of sea level."

In this way indeed, the new correction due to oceanic motion in the foregoing table has been introduced.

But my paper differs absolutely from his understanding of it. I have shown that along the Japanese coast the annual variation of the sea level owes only about one third to the barometric variation, and the remaining greater part to the variations in temperature and salinity, accordingly to the change in the density of water. The one third accompanying the barometric variation has been already accounted for in the contribution to λ_0 and μ_0 in the preceding article. The main part of the fluctuation in level due to the change in water-density does not mean mass-transference over the ocean because it has been proved that the phenomenon can be treated hydrostatically, just like the isostacy in the earth crust, i. e, the sea level stands higher with smaller density and lower with greater density, so that no change in the surface loading contributing to λ_0 and μ_0 will occur.

Although that part of the fluctuation in level due to wind and tributary land water will change the surface loading influencing λ_0 and μ_0, yet their effect will be limited to relatively small coastal seas, and may be negligible in the wide ocean.

From the above discussion it will now be clear that Rosenhead's "correction due to oceanic motion" is entirely erroneous and ought to be erased. Hence as the explanation of latitude variation, his paper marks no further advance than Jeffreys', excepting that it has the merit of having given theoretically the reduction factor c in eq. (4) for yielding earths.