THE CO-EFFICIENT OF SAFETY IN NAVIGATION.*

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It is customary among engineers and architects, in making allowance for the strain to be borne by any part of a structure, to assign to the materials used, a strength sufficient to withstand a strain somewhat greater than the structure is ever likely to be subjected to. By experiment it can be found, for example, what is the "breaking load" of a wooden or iron beam of given dimensions, and an empirical law is established which will give us approximately the breaking load of any beam, when we know the dimensions, material, etc.; but in order to cover all possible differences which may exist in various beams, a *Coefficient of Safety* is either introduced into the formula itself, or is applied to the result obtained from the formula. This co-efficient should be large enough to cover, not only the largest possible deviation between experiment and theory, but also to meet all unforeseen emergencies, such as time and age inevitably bring.

Passing now to the consideration of the term "*Co-efficient of Safety*," as applied to navigation, it is our object to find the limits within which, under ordinary circumstances, a vessel can be located at sea, and then adduce some considerations which will enable us to form an intelligent judgment in regard to the range of error to which observations are liable. The quantity wanted is the average number of miles error in latitude and longitude, which we may fairly charge upon a single observation at sea, under ordinary circumstances. We have then to find the co-efficient by which this number must be multiplied in order to secure absolute safety, as far as safety depends upon human means and exertions.

By an examination of the "British Wreck Register" and the official inquiries made into the causes of disasters at sea, it will be seen that the ratio of loss compared with the increase of tonnage afloat, has for many years been steadily increasing. This inquiry is, therefore not an idle one. It is our purpose to examine only those causes of wrecks which, in a measure, seem to have escaped attention in official investigations. They are :—

I.—Wrecks produced by causes clearly beyond human control.

II.—Wrecks resulting directly or indirectly from over-insurance.

III.—Wrecks caused by the deviation of the compass.

IV.—Wrecks caused by errors of observation at sea.

The first inquiry is an important one, since, if we can find how many wrecks are beyond human control, we ascertain, at the same time, how many are *within* human control. The method of investigation is by the examination of records of Courts of Inquiry for twenty years. Between 1785 and 1813 no less than eight British ships were either wholly or partially disabled by lightning. Of course, vessels lost and never heard from should be added to this list. Between 1864 and 1869 we find from the insurance records that 9999 sailing vessels and 589 steamers, or a total of 10,588, were wrecked. Of this number, the end of 846 is entirely unknown, or one-eighteenth of the whole number. It is probable, therefore, that seven out of ten wrecks occur from preventable causes.

In regard to the second head, it is certain that more insured than uninsured vessels are lost, and in not a few cases it has been possible to convict shipowners of purposely destroying their vessels.

The compass problem is an intricate one, and has never been fully solved, though the researches of Flinders, Barlow, Scoresby, Airy and Harkness have done much to convert great uncertainty into tolerable certainty. The first observations on the variation were by Bond, in 1668. It is well known that the variation of the needle is very irregular. There are yearly, monthly and diurnal inequalities, the diurnal variation being discovered by Graham, in 1722. But the complexity of the problem does not stop here. The tendency of the present time is to build iron ships, and all ships now have more or less iron in their construction. These ships become, to a greater or less degree, themselves great magnets. In wholly iron ships the uncorrected deviation of the needle often amounts to 50°, thus rendering it utterly useless. The Admiralty Law in regard to "swinging" for the variation of the compass is a very clear statement of the case. It reads as follows:

"As the deviation or error of the compass caused by local attraction of the ships becomes changed in amount by any change in the ship's geographical position, and may be entirely reversed in its direction by the ship's proceeding from the Northern to the Southern Hemisphere it is to be invariably tested by azimuth and amplitude observations at sea, and the ship is to be swung for ascertaining the change of error on arrival at a foreign station, and also once a year, and the same is to be inserted in the log book and sent to the Admiralty with the quarterly return for December."

The next important discovery in this connection was by Barlow, who found that all the influence of iron bodies exerted on the compass resides on the surface. This discovery paved the way for Airy's method of correcting compasses, which is by swinging the ship in the usual way, and then correcting the local attraction of the ship by means of permanent magnets of soft iron conveniently placed with respect to the compass. But the most important discovery was made by Dr. Scoresby. He found that every iron ship is itself a magnet, and that it gets its magnetism while building by the inductive magnetism of the earth, the poles of the ship's magnetism depending on the position of the building yard and the direction of the keel in construction. Dr. Scoresby made the voyage of the world in the *Royal Charter*, to test his theory, and found it fully confirmed. Before starting, his compasses were corrected by Airy's plan. On arriving at Melbourne, it was found that a complete inversion of the ship's magnetic polarity had taken place. Every stanchion, every standard, every davit, every mass of iron about the deck had in its upper surface acquired a Northern, instead of a Southern polarity, and the starboard compass had lost nearly one-half of its original errors. On returning to the place of starting in the Northern Hemisphere, and swinging the ship, it was found that a re-inversion had taken place, but the compasses did not quite return to their original deviations, but retained a fraction of their errors. It has since been found that these changes are much greater in steam than in sailing vessels, as shown by observations on board the *Vulcan* (steam), and the *Pandora* (sailing).

In 1852-53, Dr. Scoresby, in a paper before the British Association, showed that there is a sensible difference in the deviation before and after steam is up. It has been said that the compasses of a steam vessel, when light and running before the wind, with a high sea, are practically useless.

More recent experience has shown that the magnetism of an iron ship does not attain its normal condition till some twelve months after launching, and that for some time the variation is very irregular. In the *Great Eastern* a fixed compass changed its deviation nearly 3 points in the first 9 months of service. The observations of Prof. Harkness on board a monitor seem to be conclusive on this point.

We come now to the consideration of the fourth point. As early as 1598, Spain offered a reward of 100,000 crowns for the discovery of a correct method of finding the Longitude at sea. The States of Holland, at an early date, offered a reward of 100,000 florins, and France a reward of 100,000 livres. In 1714 the British Government offered a reward of £10,000 to any one who should

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discover a method of finding the longitude at sea within 60 miles, £15,000 if with 45 miles, and £20,000 if within 30 miles. This offer did much to awaken interest in the subject. Though we have long since passed the lowest limit then mentioned, 30 miles, it is doubtful if any two navigators will agree as to what limit we have actually reached. The general testimony of sea-captains, in answer to my inquiries on this point, is that one mile is the ordinary limit within which the co-ordinates of a ship's place can be determined. A few placed the limit at half a mile. Only one navigator, with an experience of 30 years, placed the limit at 5 miles.

Two methods were proposed for the solution of the problem. Morin proposed what is now substantially the Lunar Method, and Maskelyne undertook the solution of the problem by observing the astronomical phenomena, such as eclipses of Jupiter's satellites. On the other hand, mechanicians devoted every energy to the mechanical problem. As the result of these labors, we have two essentially different methods for the determination of Longitude at sea.

I. By Lunar Distances, Occultations and Eclipses of Jupiter's Satellites, &c.

II. By Chronometers, assuming a rate at the beginning of the voyage.

The latter has for a long time been regarded as the more accurate method, but the difficulties to be overcome can be readily imagined, when we consider that even in the determination of the position of fixed observations, in which appliances of the utmost refinement are at hand, the places vary widely from the truth. For example, we find variations in the measured difference of longitude between Greenwich and Paris, as great as 5.5^s, or 1¼ miles, existing previous to the introduction of the telegraphic method of determining longitudes. The range between the earlier determinations of the difference of longitude between Greenwich and Brussels is 10 miles. Moon Culminations are more accurate than Lunars, but the same in principle. They are the more accurate when the longitude depends upon observations at each station, since the errors of Tables are thus eliminated. From a careful discussion of a long series of observations made at fixed observatories, with the most perfect instruments, it is found that we must expect from the Lunar Method an absolute error of six miles as the result of any number of observations. This corresponds in a general way with Prof. Peirce's investigation. He found that the ultimate limit, when one limb of the moon was observed, to be 0.55^s. "Beyond this," he says, "it is impossible to go with the utmost refinement. By heaping error upon error, it may crush the influence of each separate determination; but it does not diminish the relative height of the whole mass of discrepancy." But the discrepancy between the results for different limbs of the moon often amounts to 10^s in the mean determination of a year. The assumption that the ultimate limit of accuracy is as great as 1^s seems to be a very moderate widening of the limits. I find it to be 2.4^s.

For fixed observatories, using the moon's tabular place, we must expect an error of 3.1 miles, with a range of 12.9 miles. For Lunar Distances with sextant, on land, we must expect an error of 10.2 miles, with a range of 24.2 miles. For Lunar observations at sea these quantities should at least be doubled.

We now come to the subject of chronometers. The sources of errors are:

(*a*) Variations of rate arising from the action of magnetism. Airy's experiments show an extreme variation of 5.8^s in the daily rate of a chronometer, due to terrestrial magnetism.

(*b*) When chronometers are swung on the same support, it is probable that there is a sympathetic action between them, similar to the results recently found by Mr. Christie with the Transit of Venus Clocks.

(*c*) Variation on account of change of barometric pressure. This varies between 0.3^s and 0.8^s per day for every inch of change in the barometer.

(*d*) Variation between land and sea rates. Almost every chronometer will change its rate, when its circumstances, either of rest or motion, are changed. The Boston standard clock of Messrs. Bond & Son, almost invariably has a different rate on Sunday from any other day of the week. So, also, it has a change of rate when the streets are covered to any considerable depth with snow.

(*e*) Variation of rate at sea, on account of change of temperature. Mr. Hartnup, of Liverpool, was the first to give, not only the general rate of a chronometer, but also the rate for different temperatures.

An elaborate discussion of the errors of chronometers, from data collected at the Greenwich Observatory, from chronometric expeditions and from chronometers used in the Merchant and Naval services, the following result has been reached.

At the end of 20 days the navigator must expect an average error of 36 miles. He must look out for an error of 36 × 32 or 11.5 miles, and the amount of his error may prove to be twice this quantity, or 21 miles, all on the supposition that he has an average chronometer, and this is independent of the errors of observation, which must still be added.

We come finally to the consideration of the problem, —How near is it possible to find the place of a ship at sea by astronomical observations, taking into account all the errors to which observations are liable?

For the sake of simplicity we shall consider but one method, the method usually followed, viz.: by measurement of the altitude of the sun with a sextant, at a given time before it comes to the meridian for longitude, and the measurement of its culmination, for latitude.

We must first of all ascertain the magnitude of the errors to which observations with the sextant are liable. The following are some of the errors which we must ordinarily expect in observations with this instrument:

(*a*). Instrumental errors, such as eccentricity, errors of graduation, index error, &c. Errors of this class often exceed one minute of arc, even in a first-class instrument.

(*b*). Errors in noting time. No observer at sea pretends to note the time closer than one second. If we assume this low limit and multiply the co-efficient 3.5 already found, we have an error of nearly one mile.

(*c*). Errors arising from imperfect sea horizon.

(*d*). Errors arising from the use of approximate data.

(*e*). Errors depending on the latitude of the ship and the time of observation. By combination in the same direction, errors of this class may be very large, and, for the most part, they escape the attention of the navigator. The most favorable time for an observation of the sun for longitude is when it is exactly east or west. Here an error of one minute of arc in the observed altitude produces an error of the same amount in the resulting time, but if the observation is made 40 minutes from the meridian, an error of one minute in altitude may produce an error of 6 miles in the resulting position.

(*f*). Errors arising from the error in the estimated run of the ship between the morning and the noon observations.

The data for assigning a limit to the errors of observation with the sextant are as follows:

I. *Observations on Shore.* From a discussion of the observations by Williams in 1793, by Paine in 1831, by various observers at Willets Point in 1869, 1871 and 1872, by Hall and Tupman at Malta and Syracuse, and by Newcomb and Harkness at Des Moines, we find that for *latitude* the average error of observation with the sextant is 8″, that the average range between the greatest and the least results of a given series is 36″, the latter value having a range between 14″ and 59″. The coefficient comes out 4.4. For *time* the average error is 1.1^s, the range is 4.7^s, and the coefficient is 4.4.

II. *Observations at Sea.*

Under this head three distinct investigations have been made, as follows;

(*a.*) From an examination of the results obtained by chronometric longitude expeditions, we find that for a voyage of 15 days the average error is 5.3^s; the range between the greatest and the least results in each series is 18.0^s; the latter value has a range between 1.5^s and 55.0^s, and the coefficient is 3.4.

(*b.*) The longitudes of 36 stations have been determined by various British naval expeditions. The chronometers were rated at the Greenwich Observatory before starting, and the observations for time at the terminal stations were made in the usual way with the sextant. Evidently more than usual care was taken both with the observations and reductions. We find that the average difference between the results obtained by different chronometers is 4.4 miles with a range of 15.1 miles. The average range between the different results for longitude is 5.0 miles with a range of 31.6 miles. The average number of chronometers was 11, and the average duration of voyage was 11 days.

(*c.*) During the spring and summer of 1880 Officer W. H. Bacon, of the Cunard steamer "Scythia," kindly undertook for me a series of systematic observations from which the relative errors could be determined with considerable certainty. A complete series for a single day consisted of five sights at intervals of fifteen minutes, about 8 o'clock in the morning, five sights in the neighborhood of 11 o'clock, and five sights at the corresponding hours in the afternoon. Observations were also made when the ship was in known positions as often as possible.

This series of observations has an exceptional value on account of the conscientious fidelity with which the programme was adhered to and of the skill with which they were made. The relative errors were determined by comparing each position with the mean of the series, the rate being determined both from the morning and afternoon observations and from the log.

The results obtained are found in the following table:

LIMITS IN MILES.	Average Error from Observations at 9^h and 3^h.	Average Error from Log at 9^h and 3^h.	Average Error from Observations at 11^h and 1^h.	Average Error from Log at 11^h and 1^h.	Difference between Observation and Log at 9^h and 3^h.	Difference between Observation and Log at 11^h and 1^h.
	No. Cases.	No. Cases.	No. Cases.	No. Cases.	No. Cases.	No. Cases.
0.0- 0.5	1	0	0	0	7	6
0.5- 1.0	9	6	2	3	1	2
1.0- 1.5	8	13	3	5	3	3
1.5- 2.0	4	5	3	3	3	2
2.0- 2.5	6	4	6	5	2	3
2.5- 3.0	2	1	3	4	1	0
3.0- 3.5	2	2	6	5	7	2
3.5- 4.0	4	1	4	5	1	2
4.0- 5.0	1	3	6	5	4	4
5.0- 6.0	0	0	2	1	1	5
6.0- 7.0	0	0	2	1	2	2
7.0- 8.0	1	1	0	1	1	1
8.0- 9.0	2	0	1	1	0	2
9.0-10.0	0	1	0	0	1	2
10.0-11.0	0	0	0	0	1	1
11.0-12.0	0	0	0	0	2	1
12.0 +	1	1	0	0	0	0

QUERY.

A SUBSCRIBER would like to know the best method of mounting Triple phosphate crystals (dry) so as to tack them to the slide without interfering with definition. —Replies invited.

ON THE ACTION OF BACTERIA ON VARIOUS GASES.*

By F. HATTON.

The experiments were made to ascertain the nature of the action exerted by various gases on the life and increase of bacteria, and to observe what influence the bacteria had on the percentage composition of the gases. The bacteria were obtained by shaking fresh meat with distilled water. The aqueous extract was filtered and exposed to the air for twenty-four to thirty-six hours; it was always found to be full of bacteria. A small flask was half filled with mercury, filled up with the bacteria solution, and inverted in a mercury trough. The gas under examination was then passed up, a small glass vessel was introduced under the mouth of the flask, and the whole removed from the trough. The liquid was examined daily as to the condition of the bacteria, the sample being removed by a piece of bent glass tubing having an india rubber joint. After about a week the gas was pumped out by means of a Sprengle and analyzed. Atmospheric air was first tried. The bacteria lived well during the fifteen days of the experiment (T. 15° to 22°). A large absorption of oxygen took place, but it was not replaced by carbonic anhydride; in a second experiment (T. 25° to 26·50) 20 per cent. of the oxygen disappeared, and only 17 per cent. of CO_2 was formed. Pure hydrogen after fourteen days had no action on the bacteria; the gas contained 0.34 per cent. CO_2, 98.94 per cent. H. Pure oxygen after ten days was converted into CO_2 29.98 per cent., O 70.02 per cent. A mixture of CO 46.94 per cent., CO_2 1.27, O 1.27, N 50.51, was next tried after fourteen days; the gas contained CO_2 17.77, CO 0.55, H 7.58, CH_4 2.50, N 71.57. In all of the above cases the bacteria flourished well. Cyanogen was next tried. The solution of meat turned gradually to a thick black fluid. On the fifth day very few bacteria could be seen. From this time, however, they increased, and on the twelfth day were comparatively numerous. On the fifteenth day the gas was analyzed; it contained CN 5.35, CO_2 57.59, O 2.24, N 34.79; a second experiment gave similar results. It appears, therefore, that cyanogen is fatal to bacteria as long as it exists as such, but that it soon decomposes into ammonic oxalate, &c., and that the bacteria then revive, especially in sunlight. Sulphurous anhydride was next tried; the bacteria lived during the fifteen days: the gas contained CO_2 7.87, O 0.00, N 2.13, SO_2 90.10. Similar results were obtained with nitrogen, nitrous oxide, nitric oxide, carbonic anhydride, a mixture of H and O obtained by the electrolysis of water and coal gas. In all cases the bacteria lived well during the experment. The author next experimented with a solution of urea (0.98 per cent.) and phosphate of potash (0.4 per cent.), sowing it with bacteria. The bacteria lived well during the fourteen days of the experiment; small quantities of gas were evolved containing 0.53 per cent. CO_2, 2.64 per cent. O, and 96.82 per cent. N. An experiment was made with spongy iron, air, and bacteria. On the fourth day, all the bacteria had vanished; the air was analysed on the fifth day, and consisted of CO_2 0.26, O 0.00, and N 99.74 per cent. Experiments were also made with acetylene, salicylic acid, strychnine (10 per cent.), morphine, narcotine, and brucine; none of these substances had any effect on the bacteria. On the other hand, phenol, spongy iron, alcohol, and potassium permanganate were very destructive to these microscopic growths.

Mr. W. M. HAMLET said that these experiments confirmed some observations of his own. He had found that bacteria could exist in almost anything—in carbonic oxide, hydrogen, 1 per cent. creosote, phenol, methylamin, methylic alcohol, chloroform. Moreover, Crace-Calvert had shown that they could live in strong carbolic acid. In

* Read before Chemical Society, March 3, 81. This paper obtained for the author the Frankland Prize of £50 at the Institute of Chemistry.