Citation:

J.A.C. Oudemans, On the contents of the sixth and last part of the Report 'Die Triangulation von Java', in:
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Whether they do so also petrographically, I cannot decide, having no specimens of the last mentioned place to compare them with. So I cannot ascertain whether, and if so, where, limestone, with remains of Leperditia grandis, found in the Groningen diluvium, still exists in a compact rocky mass. —

Geodesy. — On the contents of the sixth and last part of the Report "die Triangulation von Java", lately presented to the Academy, in the name of the Netherlands Government, by Prof. J. A. C. OUDEMANS.

(Read February 23, 1900).

As I had the honour to state at the meeting of January 2, 1897, the fifth part of this Report contains the complete results of the triangulation of Java, primary and secondary [1]). It contains the length in metres and the azimuths at both ends of every side of the triangles. Finally a table exhibits the geodetic longitudes and latitudes of all the stations, as they were calculated, starting from the station Genoek in the Residency of Djepara, where the late Geographical Engineer SOETERS made an accurate astronomical determination of latitude and azimuth; the same table contains also the heights above the mean level of the sea, (see IV Abth., p. 206, line 4 from the bottom.)

How these heights were obtained had hitherto not been explained; it is done in the now completed 6th part.

I beg to communicate in a few words some particulars about these calculations. At any station near the seaboard the height above the mean sea level can be measured directly, taking as far as possible the tides into account. This was done for 19 stations, 8 primary and 11 secondary ones, and these were the starting points for obtaining the heights of the stations situated further inland, but for the last a knowledge of the refraction is required.

Owing to the diminution, of the density of the atmosphere with increasing elevation, a ray of light traversing it in a nearly horizontal direction is bent downwards.

Now if at one station be measured the zenith distance of

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[1]) The primary triangulation was already treated of in the 4th part, but the results were reprinted in the 5th.

another, whose distance is known, the difference of height may be calculated, provided the mean radius of curvature of the ray of light between the two stations be known. If that radius be n times that of the earth's surface between the two stations, $\frac{1}{n}$ is generally called the coefficient of terrestrial refraction, and its half, which is used in the calculation of the difference of height, if the path of the ray is supposed to be circular, will here be called the *factor of refraction* [1]).

This factor may be found, either, as BAUERNFEIND did in Bavaria, by measuring the zenith distance of a distant station, whose height above the observing station has been accurately determined by spirit levelling — or by measuring, at each station, the zenith distance of the other. Both these methods, especially the latter, have been repeatedly applied, but the results were very discordant. Theoretically the factor is known to depend on the indications of barometer and thermometer, but still more, nay principally, on the law of diminution of the density of the air with increasing height.

As this law was unknown for Java, and as the factor was suspected to be very variable, being dependent on the time of the day and on atmospheric conditions, the design was long ago formed in the Geographical Service to make a special determination of this factor, by reciprocal and simultaneous observations of zenith distances at different hours of the day. This design has been carried out since I left Java, under the direction of the late engineers WOLDRINGH and SOETERS.

In 1876 Engineer WOLDRINGH made two determinations, assisted by Assistant JACQUES OUDEMANS. On one day the reciprocal zenith distances were simultaneously measured five times between the south end of the Simplak base and the summit of the Salak, and shortly afterwards three times between the same base end and the station Tjitjadas; the difference of height being 2015 metres in the first, and only 44 metres in the last case. (The heights above sea level were: base end 195, Salak 2210, Tjitjadas 235 metres). The results were different, and in the last named determination there existed from half past seven till half past ten a. m. a considerable diminution of the value of the factor of refraction. This led to the arrangement of systematic series of observations. A first series

[1]) In Clark's Geodesy, the half of $\frac{1}{n}$, which we call the factor, is called the coefficient of refraction

was observed between the stations Banjoepahit, Penoenggalan and Basé in Middle Java, by Messrs SOETERS, JACQUES OUDEMANS and DE VLETTER. Unhappily the observations of the last named observer are lost; perhaps this is the reason, why the same observers performed new series between three other summits in East Java, viz. in March and April 1878 between the stations Djoerangsapi, Poetri and Tanahwoelan, and in November of the same year between Petjaloengan, Poetri and Tanahwoelan. (The heights were: Djoerangsapi 230, Poetri 976, Tanahwoelan 761, Petjaloengan 534 metres).

Between each pair of stations the zenith distances were repeatedly measured strictly simultaneously, e.g. at 8 h. between the first and second, at 8 h. 20 m. between the first and third, at 8 h. 40 m. between the second and third station, and so on. The observations were commenced early in the morning and continued as long as rising clouds did not interfere.

The general conclusion from these researches was, that the factor of refraction diminished from early in the morning till noon, at first rapidly, and afterwards more slowly; of course this is easily explained by the rising of the heated inferior layers of air.

But a remarkable result was, that each pair of stations gave a different value for the factor of refraction; thus, while in March 1877 Penoenggalan and Banjoepahit gave for the mean 0,0547, (minimum at noon 0,0510,) the mean result from Petjaloengan and Poetri was 0,0882, (maximum at 8 h. 0,0973).

It was thought that these differences could be accounted for by deviations of the plumbline, caused by local attractions; for such attractions displace the zeniths and so alter the zenith distances; but the attempt to explain the observed differences in this way, proved unsuccessful; at least it would have been necessary to assume very large deviations of the plumbline, different in sign, besides as has been already stated, in March and April of 1878 the observations were made at Tanahwoelan, Poetri and *Djoerangsapi*, and in November of the same year at Tanahwoelan, Poetri and *Petjaloengan*; now the deviations of the plumblines at Tanahwoelan and Poetri, needed to render concordant the observations of March and April, did not agree, either in direction or magnitude, with those which would have harmonized the observations of November. Attempts to explain the variations of the factor of refraction by the indications of barometer and thermometer, were equally unsuccessful and it seemed necessary to conclude that the layers of air of equal density are by no means parallel to the spherical or ellipsoidal surface of the earth, but that they follow the sinuosities of the ground, so that every pair of mountaintops or rather of stations has its own factor of refraction.

As it was nevertheless desired to obtain a mean factor of refraction, my zealous coadjutor, Coronel M. L. J. van Asperen, late of the R. D. N., who had executed most of these calculations, proposed to me to select all the pairs of stations between which reciprocal zenith distances had been observed, whether simultaneously or not. In this research East and West Java were kept separated, but they gave very nearly the same result, namely 0,068. In all there were 114 results, ranging from 0,0392 to 0,0879.

These 114 values were then divided and ranged in 11 groups, in three different ways; 1st. according to distances, varying from 23,9 to 69,5 kilometres; 2nd. according to mean heights, ranging from 123 to 2673 metres, and 3rd. according to zenith distances, taking for each pair of stations the arithmetical mean between the smaller zenith distance and the supplement of the other.

My reason for arranging the results according to distances, was that in the triangulation of Hanover, Gauss found the factor of refraction to be dependent on the distance between the stations, the greater distances giving a larger factor, but this was easily explained by the flatness of the country.

Between two stations that were not far apart, the ray of light almost grazed the surface of the ground, and it is a known fact, that in this case, by the heating of the soil and the so caused diminution of density of the undermost layers of the air, the factor of refraction often diminishes, and not seldom becomes negative, in which case the convex instead of the concave side of the ray of light is turned to the earth. Of course in Hanover great distances were only to be found between relatively high summits, and so the ray of light between such stations was often free from this influence.

In Java no influence either of distance or of zenith distance on the refraction was remarked. As regards mean height, its increase was accompanied by a feeble diminution of the factor of refraction, but not until the mean height amounted to 2000 metres. For this reason the *theoretical diminution of the factor* with increasing height has been ignored.

As we now came to deduce the differences of heights from the zenith distances, and to calculate from these, in connection with the heights measured directly at the seaboards, the heights of the inland stations above sea level, it seemed proper to use the 114 cases, in which, as above stated, reciprocal zenith distances were observed, but not simultaneously; in such cases the difference of height may be calculated by a known formula [1]), not involving the factor of refraction.

For calculating the remaining differences of height the value of the factor of refraction had to be taken into account. Fortunately its influence on the shorter sides of the secondary triangulation was always small; very often a method was used which had the advantage of diminishing considerably the influence of abnormal refraction, viz: measuring the zenith distances of both the stations from a third station simultaneously, or at least the one immediately after the other.

As the general rule was, to measure from every station zenith distances of all the surrounding stations, there were many more given quantities than were necessary to determine the heights of all the stations, primary as well as secondary; so the differences of height had to be deduced by the method of least squares; but as the stations were too numerous to be all included in one solution, it was necessary to combine them in groups. The manner in which the division of the work took place is clearly indicated by a map printed in colours, of which I give a pair of reprints to circulate [2]). As already mentioned, the heights of all stations near the seaboard were measured directly, the heights of the other stations being derived from these. The heights given in the Report are those of the tops of the triangulation pillars, generally built up 1,1 metre above the ground, upon a foundation of one meter depth.

In the deduction of the most probable values the weights of the measured differences of height were taken into account; it was therefore necessary to distinguish between the three ways in which these differences had been found, viz. by measuring from one of the two stations concerned or from both reciprocally, or from a third station. The report gives these particulars. In the primary work reciprocal zenith distances were almost exclusively used.

After the heights of the primary stations had been fixed, the heights of the secondary stations were deduced, taking into account the weights of the several determinations.

The result of an elaborate examination was, that the average mean error of a deduced difference of height was about half a metre.

I also computed the mean error (in seconds) of a single zenith

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[1]) Viz. the formula: $h' - h = S\left(1 + \frac{h' + h}{2R}\right) tg \tfrac{1}{2}(z' - z)$.

[2]) Plate III of the Report. As the stones, that have served for this plate, as also for the plates I and II, have been ground out, it has been impossible to join them to the printed abstract.

distance, such as had been used in determining the differences in height. The result found was ± 9",6, whereas the mean error of the reciprocal simultaneous observations was only ± 2",2. The inference is, that, if we wish to find the accurate difference in height between two stations, between which the ground is too hilly for spirit levelling, *simultaneous reciprocal* zenith distances ought exclusively to be taken.

In Java the determination of the heights was of quite secondary interest, and the results (which were confirmed by further researches) were sufficient for the purposes of the Survey; but at other times and in other regions there have been cases, where the difference of height was the chief aim, and this rule was not observed.

In the operations executed in 1825, under the direction of colonel CORABOEUF, for the geodetic determination of the difference of height between the Atlantic and the Mediterranean, every care was bestowed on the triangulation of the summits of the Pyrenees; the zenith distances too were often repeated, three or five series, each consisting of ten observations, being taken on each occasion; but the reciprocal zenith distances were not observed simultaneously, though the double staff rendered it practicable. The result might have been more satisfactory, if this precaution had been taken, but it must be allowed that at that time attention had not yet been drawn to the point.

A separate chapter of the Report is consecrated to the dip of the horizon, observed at 53 stations. It soon appeared that these observations could not compete with those already mentioned for determining the heights above mean sea level; they have accordingly not been used for this purpose.

But it was quite another thing, being given the already fixed heights of those 53 stations and the observed dips, to deduce *a posteriori* a formula by which the height above the sea might be calculated from the dip.

This problem also initiated several inquiries. The height is best calculated by a series containing the even powers of $tg\, d$. Now, in trying to determine, by the observed dips, the coefficient of $tg^2\, d$, (on which the 5½ times smaller one of $tg^4 d$ depends), the computed heights of the low stations were generally too great, a result easily explained by the already mentioned inversion of the curvature of the ray of light near the level of the sea. Indeed, following a ray of light from the sea to a distant mountaintop and supposing a point of inflexion to occur near the sea level, then it is easy to see, that in determining the height of any point on the ray by the dip, the error made will be the same for all points on the ray. Indeed, as a diagram immediately shows, this error is equal to the linear quantity by which that part of the ray of light, which is concave on the side turned to the earth, dips under the level of the sea, if prolonged at the sea-end.

By the method of least squares, it was easy to find this so called constant, as well as the coefficient of $tg^2\, d$, and thence the factor of refraction.

The first step was to take the arithmetical mean of all the dips observed at the same station; the second term, containing $tg^4 d$, proved small enough to be neglected if the height did not exceed 2000 metres; and so the problem was reduced to the solving of 53 equations with 2 unknown quantities. The coefficient of $tg^2 d$ being thus determined, the factor of refraction was calculated [1]), and agreed almost perfectly with the value found before; finally the coefficient of $tg^1 d$ was deduced. The result was

$$h = (6{,}56546)\, tg^2 d - (5{,}82716)\, tg^4 d - 2{,}475,$$

h being in metres, and the numbers in the brackets being logarithms of the actual coefficients. The value of the factor of refraction, deduced from the first coefficient, is 0,0692, whereas the reciprocal zenith distances gave 0,068. Considering the diverging values that are found for this number, the agreement is more than sufficient.

The same material, however, seemed to invite a more detailed inquiry. Often, at the same station, the dip has been observed at different hours of the day, and it seemed to be worth while to examine the changes of the factor of refraction during the hours of observation. The result is shown graphically on plate II. At 11 a. m. both methods give nearly the same value, viz. 0,0680; before and after, the dips give a smaller value than the reciprocal zenith distances.

The same plate shows in a little map at the bottom, all the stations where the dip has been observed. They appear to be very regularly distributed along the south coast of Java and in the eastern part of the north coast; we also see that the dips were observed in different azimuths. Now, as the radius of curvature of the earth, (or of the ocean), differs with the azimuth, it is

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[1]) Calling the coefficient of $tg^2 d$ a, the factor of refraction k and the radius of the earth R, we have $a = \frac{R}{2-4k}$, or $k = 1/_2 - \frac{R}{4a}$.

necessary, if we wish to calculate with precision, to take this into account and to reduce the observed dips (for instance) to dips belonging to a mean radius of curvature.

I was sorry not to be able to take the tides into consideration, though they have been thoroughly investigated[1]), for a great number of stations, by our honoured Correspondent Dr. J. P. VAN DER STOK, who embodied his results in tide tables. But these tables commence with the year 1891, and it would have been a laborious operation to extend them to the period containing the observations (1865—1880) and to deduce, by the cotidal lines, (also drawn by the same author), the height of the sea at the moments of the observations.

In most cases neither the azimuth was noted, nor the hour of the observation; and it was necessary to make assumptions with regard to both; as to the azimuth, the dip observation was supposed to be made in a direction perpendicular to the coast line; as to the hour of observation, it was supposed that the horizontal observations commenced at half past six, and that every observation, horizontal or vertical, occupied five minutes. This gave the time of the dip observation.

This investigation led me to frame the following rules for future observations of dip.

1. At every observation the apparent time is to be noted, as also the indications of barometer, thermometer and psychrometer.

2. Repeating the observations on several days is recommended, in order to neutralize accidental deviations and to discover extraordinary perturbations.

3. It is useful to take the measures in different azimuths, especially, if possible, in the meridian and in a direction perpendicular to the meridian, in order to examine whether the theoretically existing difference of the dips in these two directions, which may amount to one part in 150, or to 24″ in a dip of one degree, is confirmed by experience.

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1) These researches have been published in fifteen papers, 13 of these in the "Tijdschrift van het Koninklijk Instituut van Ingenieurs, Afdeeling Ned. Indie, 1890—1896". The title of the first paper, translated, is "The Harmonic Analysis of Tides, applied to Observations made at Tjilatjap"; that of all the following: "Studies of the Tides in the Indian Archipelago"; II and III contain theoretical matter, IV—XIII the results of the harmonic analysis, applied to the indications of the tide gauges established in different parts of the East-Indian Archipelago. XIV (Statistics) and XV (Predictions) have been published in the Journal of the Royal Physical Society of Batavia, Vol. LVI, 1896.

4. The stations are to be so chosen, that the height of the tide may be calculated by the tide-tables.

I have thought that a review of previous investigations of terrestrial refraction, would not be unwelcome to those interested in the subject. It includes an account of the work of JEAN DOMINIQUE CASSINI in 1661, CESAR FRANÇOIS CASSINI in 1742, TOBIAS MAYER in 1751, LAMBERT in 1759, BOUGUER in 1749, ROY and MUDGE from 1787 to 1799, MÉCHAIN and DELAMBRE from 1792 to 1797, VON ZACH in 1795, WARREN in 1804, VON LINDENAU in 1805, TRALLES in 1806, GAUSS in 1823, WILHELM STRUVE in 1826—27, CORABOEUF in 1827, CACCIATORE in 1831, BESSEL in 1834, BAEYER in 1835, SABLER and STRUVE in 1838, ATKINSON in 1825, DENZLER in 1842, PILAAR in 1846. Then follows a description of the theories of LAPLACE, BAEYER, BABINET, SAWITSCH, BAUERNFEIND, LINDHAGEN, HELMERT, JORDAN and WALTER. The chapter closes by mentioning the value of the factor of refraction which on my suggestion Coronel M. L. J. VAN ASPEREN has deduced from the observations of the Peruvian Committee for measuring a degree in Peru in 1735, and finally by summing up a number of papers on terrestrial refraction, published since 1850 in different periodicals.

The second section of this sixth part contains the determinations of latitudes and differences of longitude, made for comparison with the results of the triangulation, with a view to detecting deviations of the plumbline by local attraction.

Determinations of latitude by circummeridian zenith distances were made at 63 stations. For reducing these observations the knowledge of the declinations of the stars employed was requisite; and though most of the stars were taken from the Nautical Almanac, whose places for the northern stars rest almost exclusively on observations made at Greenwich, it was thought desirable to take into account some other determinations, especially as the declinations of those southern stars which could not be observed at Greenwich were very uncertain.

For the determinations of azimuths made, (in absence of our Pole star), by KAISER's method, by measuring the difference in azimuth between stars in the east or west and some visible station, the precise knowledge of the declinations of the observed stars was also of much interest; sometimes stars were employed for the azimuths which had also served for latitude, yet the choice of stars was guided by different considerations in the two cases and stars fit for the determination of latitude, were not always appropriate for that of the azimuth [1]).

The late Dr. N. M. KAM, whose catalogue of stars is well known, undertook, at my request, to furnish a list of the declinations of the stars employed; the numerous star catalogues which he had to consult being forwarded to him from the library of the Utrecht Observatory. He carried out this task very thoroughly, including corrections for the systematic differences of the catalogues. I must add, that when KAM undertook this work, it was not known that AUWERS, of Berlin, was about to undertake a similar one.

Neither for the determination of the latitudes, nor of the azimuths, was it deemed necessary to submit the right ascensions to an equally severe examination. In the determination of latitude a small error in the R. A. was entirely eliminated by the circum-meridian observations; in the determinations of the azimuths, which were made for 20 stations, the influence of such an error was always either 0, or so small that the Right Ascensions could be taken directly from the Nautical Almanac.

That a careful determination of the declinations was not without utility, may be shown by the following instances: the correction of the declination of α Andromedae in the Nautical Almanac for 1854 was $-0'',86$, in that of 1871 $+0'',03$; that of the declination of γ Pegasi was $+0'',7$ in 1876, 78 and 79; that of ε Leporis, from 1871 to 1880, was nearly a second; that of the declination of Sirius varied from $+1'',30$ to $-1'',89$, that of the declination of Procyon from $-0'',55$ to $+1'',58$, and so on.

Dr. KAM also charged himself with the deduction of the definitive

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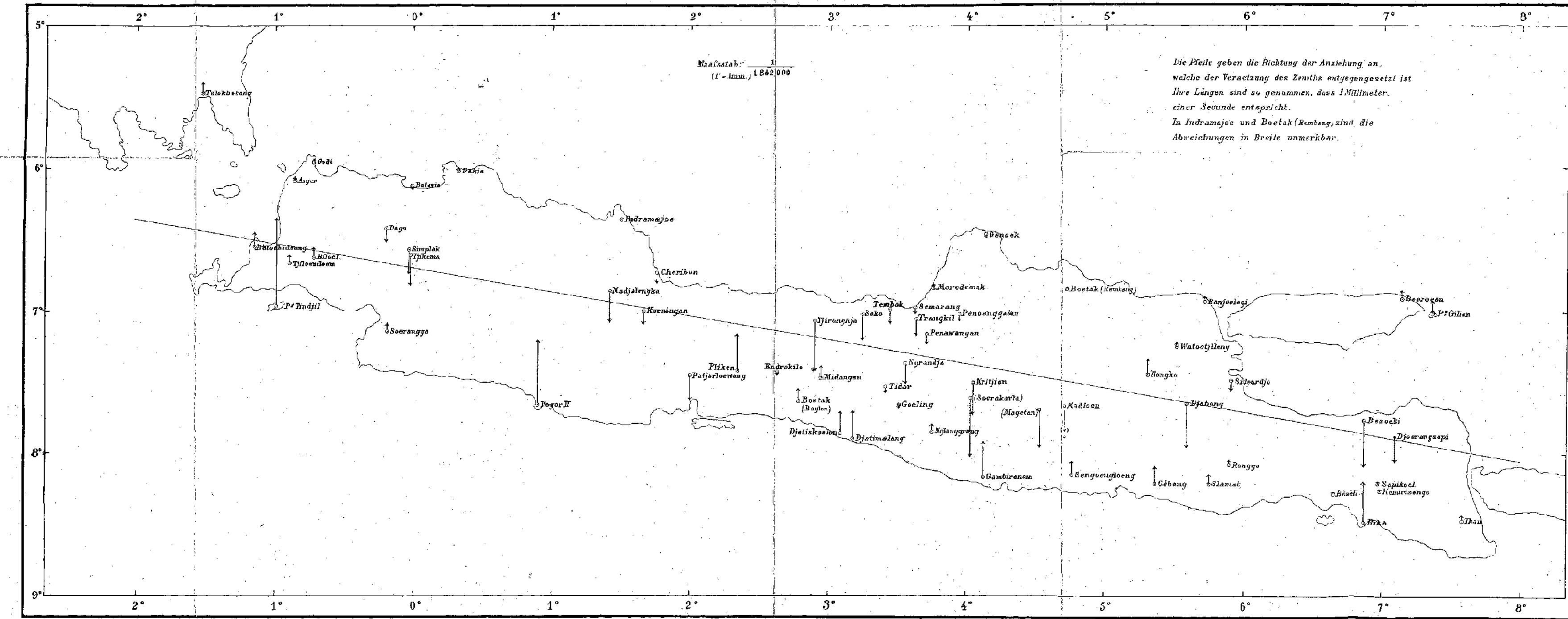
[1]) For the determination of a latitude generally stars were employed (commonly four on the north and four on the south,) that culminated at 20° or more from the zenith; the rule was to choose them so that the sums of the zenith distances on the two sides of the zenith were nearly equal; whereas for the determination of an azimuth, the stars ought to be so chosen that at a low altitude their azimuths changed as little as possible. Therefore KAISER, in his "Treatise on the astronomical determination of geographical positions in the East Indian Archipelago" proposed to choose, for this purpose, stars, which, having an altitude of from 10° to 30°, were either in the east or in the west.

Those stars were of course preferable, that moved vertically; (astronomically: whose parallactic angle was 90°;) which was only possible if the star's southern declination exceeded the latitude of the station; but even if the star be not in this most favorable situation, still, provided it stands not higher than 30°, a possible uncertainty of the time, as well as of the latitude, has only a very small influence on the azimuth.

DIE LOTHABWEICHUNGEN AUF JAVA,
IN DER RICHTUNG DES MERIDIANS.



latitudes and azimuths, taking into account the flexure of the telescope[1]).

All these determinations of latitude were made for the purpose of finding the differences between the astronomical and geodetic latitudes, due to deviations of the direction of gravity from the normal to the adopted ellipsoidal surface, on which the whole network of triangles is supposed to be projected. In German these differences are called "*Lothabweichungen*"; I think an unequivocal Dutch expression would be: "*afwijking van het paslood*" or "*pasloodafwijking*". It may arise *either* from the stronger attraction of mountains or heavy underground masses, *or* from the feebler attraction of less heavy masses on the opposite side. Which cause is the true one in any particular instance, may be decided with more or less probability by measuring the force of attraction by pendulum experiments.

The deviations are represented on plate IV[2]) by arrows, directed to the north or to the south, according to the direction of the attraction. It must be kept in mind, that if the arrow points to the north, the astronomical observations indicate a greater (meridional) latitude then the geodetical ones.

Though attraction by the mountainous country is undeniably indicated, the arrows on the south coast being all directed to the north, and those on the north coast almost all to the south, yet some decided irregularities may be noticed. Though, for instance, seven stations, situated within or near to the Residency of Semarang, indicate an attraction to the south, we find at Genoek, near the north point of Djepara, very little or no attraction, notwithstanding the vicinity of the mountain Moeria. Again, while several stations situated on the north coast, Anjer, Gedé, Batavia, Pakis, Indramajoe, Boetak (in the Residency of Rembang), show an absence of attraction, which might have been expected from their great distance from high mountains, the enormous deviations, 37″ in Poelo Tindjil and 26″,5 in Pogor II, both near the south coast, are very remarkable. An explanation, however, of this phenomenon may be found, not in the attraction of a high mountain on the north, but in the lack of

1) Dr. KAM examined also, at my request, the changes that would result in these latitudes, (*A*,) if the flexure of the telescope was not calculated for each night separately, but its mean value for the whole period was employed; (*B*,) if the flexure was simply put $=0$, so that the arithmetical mean of all the determinations was adopted as the definitive value of the latitude.

The result was, that, owing to the suitable choice of the stars on both sides of the zenith, the differences were extremely small, and in the great majority of cases did not reach a tenth of a second.

2) As the stone, on which this plate was engraved, was not yet ground out, it has been appended to this abstract.

attraction to the south by the deep Indian Ocean. The southward attraction at Patjarloewong is strange, there being no remarkable mountain to the south of this station, and the near lying Pliken showing already clearly the attraction of the Slamat on the north east. We are also struck by the southward arrow at Magetan, which has the mighty Lawoe rising near it on the northwest.

Other remarks could be made with regard to the directions and lengths of the arrows on this map, but I think these are sufficient At all events I think the conclusion is evident, that the deviations of the plumbline, detected by the determinations of latitude, show the desirability of a much larger number of such determinations. If we wish to combine the study of plumbline deviations with triangulation, it is desirable to make a determination of latitude at every station; it is then not necessary to attain a high degree of precision; a single series of say, eight circummeridian zenith distances of a star south of the zenith, combined with an equal number of a star at nearly the same distance to the north of the zenith, would be sufficient. If I were to have again under my direction a triangulation of Java or a similar mountainous country, I certainly should prefer this method to that which has been followed. Moreover every mountain offers a wide field of research; and very interesting results might be attained by executing a large number of measures of plumbline deviation and gravity in the whole region affected by a vulcano.

As YVON VILLARCEAU has shown, the deviations of the astronomical azimuths from the geodetical ones may be expressed in deviations of longitude: unfortunately they must then be multiplied by the cosecant of the latitude, a factor, having for the 20 stations, where azimuths were determined, a mean value of more than 8. Moreover the said differences were too much affected by the accumulation of errors in the horizontal measures; so the results of this reduction were untrustworthy. They gave, (and this is not to be wondered at) improbably large deviations in longitude.

Much smaller discrepancies were obtained by comparing with the triangulation the differences of longitude which I determined, in conjunction with the Assistants JAEGER and VOSWINKEL DORSELEN, by the telegraph, in the years 1859—1863, before the resumption of the suspended triangulation. Our stations were not identical with those of the triangulation, but the relative positions have since been determined and allowed for. Though the comparisons are only eight in number, they show clearly the attraction by land.

Utrecht, February 19, 1901.