like GUIGNARD and BONNIER, will think the use of these terms admissible. Although we incline towards this latter opinion, we shall not dwell on this point here.

But we think it desirable to point out that a closer study of unfertilised ovules, especially of dioecious plants will perhaps yield surprising results. Since we know through LOEB that chemical stimuli may cause the development of an egg, the possibility must be granted that this may also be the case with higher plants. When a normal fertilisation does not take place, such chemical stimuli would at any rate render a beginning of development possible. Looked at from this point of view the case of Dasylirion is perhaps important, but, as we stated already at the beginning of this communication, only an investigation in the natural place of occurrence of the plant can give an answer to this and allied questions.

Astronomy. — *"On the parallax of the nebulae"*. By Prof. J. C. KAPTEYN.

Up to the present time we know hardly anything about the distance of the nebulae. On the whole they do not allow of the most accurate measurement, and as a consequence direct determination of parallax is generally to be considered as hopeless. A few endeavours made for particularly regular nebulae have not led to any positive result.

The proper motions (p. m.) seem more promising, at least for the purpose of getting general notions about the distances of these objects.

Spectroscopic measurements of radial motion show that the real velocities of the nebulae are quite of the order of those of the stars. Therefore, as soon as we find the astronomical proper motion of any nebula, we conclude, with some degree of probability, that its distance is of the order of that of the stars with equal p. m.

Meanwhile it may be considered to be a fact, most clearly brought out just by the observations presently to be discussed, that as yet p. m. of a nebula has not been proved with certainty in a single case. It does not follow that these p. m. are necessarily very small. The time during which the position of these bodies has been determined with precision, is still short, the errors of the observations are large. The effect of these errors on the annual p. m. may easily amount to 0″2 or 0″3.

We might endeavour to lessen the influence of the errors of observation by determining *not* the individual motions but the *mean* p. m. of a considerable number of nebulae.

If this succeeded we might then compare this mean p. m. with the mean p. m. of different classes of stars, the mean distance of which is known with some approximation or, better perhaps, with the mean radial velocity of the nebulae determined by the spectroscope. The comparison would lead at once to ideas about the real distances.

Unfortunately the mean of a great number of *observed* p. m. will not be materially more correct than the individual values, if the total proper motion is small. The cause of this lies in the fact that in such a case the effect of a determined error of observation is not at all cancelled by an equal but opposite error of observation. Suppose for instance two nebulae both having in reality a p. m. of 0″01. For the first let the error of observation be 0″10 in the direction of the p. m. For the second assume an equal error in a direction opposed to the p. m. The *observed* p. m. of the first nebula will be 0″11, that of the second 0″09. Taking the mean of the two we are not brought nearer to the real value.

For this reason we shall not be led to any valuable result in this way, even if our material consists of very numerous objects, as long as the errors of observation exceed the real p. m.

The difficulty here considered would vanish if, instead of the total p. m., we could avail ourselves of some *component* of the p. m., which in different direction would have different sign. In this case, if systematic errors can be avoided or determined, the accuracy would increase as the square root of the number of objects included.

Such a *component* of the p. m. is that in the direction towards the Antapex. From this component we may derive the mean parallactic p. m. which is a measure of the mean parallax.

I will not here stop to consider the hypothesis involved. It must be sufficient to state that it assumes that the sum of the projections on some determined direction of the *peculiar* p. m. vanishes in the case of very numerous nebulae or, which comes much to the same that the peculiar p. m. may be treated as errors of observations.

Let

h be the linear annual motion of the solar system;

ϱ the distance of a nebula from that system;

λ the angular distance of this nebula from the Apex of the solar motion;

v, τ the components of the observed p. m. in the direction towards the Antapex and at right angles to that direction;

p the component of the *peculiar* p. m. in the direction towards the Antapex.

The parallactic p. m. shall then be:

$$\frac{h}{\varrho} \sin \lambda = v - p.$$

If this equation is written out for each individual nebula and if, after that, we take the mean of all the equations, the quantities p will disappear and we obtain the mean value of $\frac{h}{\varrho}$, which is the secular parallax.

Or rather:

As we may treat the quantities p as if they were errors of observation, which mix up with the real errors of the observed quantities v, we may write out for each nebula an equation of the form

$$\frac{h}{\varrho} \sin \lambda = v \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

If then we assume that the distance ϱ is the same for all the nebulae, we may solve the whole of the equations (1) by the method of least squares.

I have long wished to apply this method in order to get some more certainty about the position of the nebulae in space, but I have been restrained by the extent of the work connected with such an enterprise.

The difficulty has disappeared since the publication, a few years ago, of a paper by Dr. MONNICHMEYER assistant at the Observatory of Bonn (*Veröff. der Kon. Sternw. zu Bonn.* N°. 1). In this paper all the materials available at the time of its appearance have been brought together in a way which, for my purpose, leaves little to be desired.

This paper contains the observations of Dr. MÖNNICHMEYER himself. They bear on no less than 208 objects, mostly chosen among such nebulae as can be measured with considerable or at least moderate precision. Dr. MONNICHMEYER has collected besides, all previous observations of these objects. I have confined myself to the observations of those nebulae for which all the observers have used the same star or stars of comparison. I have further rejected the observations of those objects for which MÖNNICHMEYER did not succeed in determining the personal errors. The observations which thus have served for the investigation are those of MÖNNICHMEYER's paper pages 59—70, from which have been excluded, in the first place, those objects which in the list of pages 15—17, second column, have been denoted by the letter M; further the planetary nebulae, the clusters and the ring-nebula h 2023.

There remain 168 nebulae.

A good judgment about the accuracy of the observations may be obtained by the probable error derived by MÖNNICHMEYER for his own observations on page 9. For the other observers I have availed myself of the data contained on pages 18—25.

The accuracy was found little different for the several observers with the exception of RÜMKER.

I therefore simply assumed the weights to be proportional to the number of observations. For RÜMKER only the weight was reduced in the proportion of three to one. For SCHMIDT the number of observations is not given. For reasons given by MÖNNICHMEYER they are "immerhin etwas fraglich" (l. c. page 14). The results of SCHMIDT got the weight of only a single observation for that reason.

An overwhelming majority of the observations has been made between 1861—1869 and 1883—1893. It was possible therefore in nearly every case to contract all the observations in *two* normal differences from which the proper motion and its weight could be derived at once without any serious loss of accuracy.

From these p.m. I then derived the components τ and v, assuming for the position of the Apex, the coordinates

$$A_{75} = 273°, \quad D_{75} = +29°5.$$

The whole of the materials was divided into the three classes of MÖNNICHMEYER. They are described by him on page 9 of his paper in the following way:

Class I. Nebulae with starlike nucleus not fainter than 11$^{\text{th}}$ magnitude;

Class II. Nebulae with moderately condensed nucleus not fainter than 11$^{\text{th}}$ magnitude;

Class III. Difficult objects, in the first place irregular nebulae without any sharply marked point; furthermore all very faint objects and the very oblong nebulae.

Most of the objects have been classified by MÖNNICHMEYER himself on page 9 of his paper. The nebulae wanting in this list have been classified by myself, in accordance with the descriptions on p.p. 27—54, as follows: *h* 693, 1088, 1225 in Class I; *h* 421, 1017, 1212, 1221, 1251, 3683 in Class II; *h* 316, 1461 in Class III.

The p. m. as derived are relative p. m.; they are the motions relative to the comparison stars. MÖNNICHMEYER has investigated the p.m. of the comparison stars themselves; he has found a sensible p.m. for only 7 of the objects used for my investigation. The following table contains his results for these 7 stars.

Star of Comp.	mag.	used for nebula	μ_α	μ_δ	p. m. in arc gr. circle	v	Sin λ
			s	″	″	″	
15	6 0	h 132	+ 0.0140	— 0.089	0.227	+ 0.225	0.94
90	8.8	h 805	+ .0237	— .170	.352	— .156	1.00
129	6.1	h 1171	— .0170	— .127	.257	+ .255	0.97
164	7.1	h 1329	— .013	.00	.192	+ .167	0.99
168	9.5	M 90	+ .014	.00	.204	— .180	0.98
208	4.7	II 542	— .0050	+ .010	.075	+ .055	0 80
242	6.6	h 2050	— 0134	— .152	.199	— .197	0.45

These p. m. were applied by Mönnichmeyer before he derived his definitive differences in α and δ (Neb.-Star). In no other case a correction for the p. m. of the comparison stars was applied.

The majority of the observers used the ringmicrometer.

The principal error to be feared for observation with this micrometer is the personal error in right ascension. Mönnichmeyer has devoted the utmost care to their determination. Notwithstanding this it may be considered a fortunate circumstance that this error has no influence on the result for the mean parallactic motion, at least in the ideal case that the nebulae are distributed uniformly over the right ascensions from 0 to 24 hours.

For it seems highly probable that the distance of the nebulae is not systematically different in the different hours of right ascension. This being so the personal error will vitiate the parallactic p.m. of the nebulae at the same distance in right ascension on both sides of the apex, to the same extent but in opposite directions.

It is true that the distribution in right ascension is far from being uniform; still we may be sure that whatever residual personal errors may still exist in the materials of Mönnichmeyer, must appear considerably diminished in the result. Meanwhile I have tried to obtain some idea about the possible amount of these residual errors in the following way.

I computed the average proper motion in right ascension for each hour separately. Taking the simple mean of all these hourly averages we may expect to get a result in which not only the peculiar proper motions, but, as explained just now, also the parallactic motions shall have vanished.

This final result may therefore be assumed to represent the residual influence of the personal errors on the p.m.

For the value $\overline{\mu_\alpha}$ of this mean I find

$$\overline{\mu_\alpha} = -0.^s0004$$

In deriving this result the hours with many nebulae did not get any greater weight than the hours with only a few objects. Owing to this cause the final weight is found to be only 0.4 of what it would have been had the distribution been uniform.

We shall get a result of appreciably greater weight if in the first place we combine by twos the hours lying symmetrically in respect to the apex. In these mean values the parallactic motion is already eliminated; we may therefore further combine the twelve partial results having regard to their individual weights.

In this way I find

$$\overline{\mu_\alpha} = +0.^s0006.$$

It thus appears that MÖNNICHMEYER has succeeded remarkably well in getting rid of the influence of the personal errors.

As mentioned just now these errors appear still further diminished in the result for the parallactic motion.

There thus seems to be ample reason for neglecting any further consideration of them. In order to enable the reader to get at once a pretty good insight in the accuracy really obtained, I have divided the whole of the material not only into the three classes of MÖNNICHMEYER, but I have subdivided each of them into a certain number of sections, each of about the same weight.

I thus got the following summary. (See p. 697).

The values of τ have been included in the table merely in order to show that in them too no traces of any personal error are visible.

In order to get the yearly parallaxes π, I have divided the secular parallaxes $\frac{h}{\varrho}$ by 4.20; this number being, according to CAMPBELL's determination, the number of solar distances covered by the solar system in a year in its motion through space.

The probable errors were derived in the hypothesis that the component v is wholly due to errors of observation.

If we compute the probable error of one of our 13 results from their internal agreement we get 0."023. This number differs very little from the values directly found. Here again we have an indication that systematic errors must be small.

The last row of numbers contains the simple averages of the 13 individual results.

Class	α	numb of neb.	τ	$\frac{h}{\rho}$	p.e.	π	p.e.
	h m — h m		"	"	"	"	"
I	0.0 — 5.33	13	+ 0.014	— 0 039	± 0.023	— 0.009	± 0.005⁵
	5 33—10 57	12	— 043⁵	+ .051	.022	+ .012	5
	10.57—12.22	10	— .045	+ .034	.023	+ .008	5⁵
	12.22—12.45	9	— .004	— .027	.022	— .006⁵	5
	12.45— 0. 0	10	— .008	+ .013	.023	+ .003	5⁵
II	0.0 — 9.50	12	+ .021	+ 0.014	± 0.019	+ .003	4⁵
	9.50—11.10	10	— .004	— .016	.019	— .004	4⁵
	11.10—12 16	11	— .008	— .037	.020	— .009	5
	12.16—12.28	12	+ .019	— .040	.020	— .009⁵	5
	12.28— 0. 0	14	+ .000⁵	— .040	.020	— .009⁵	5
III	0.0 —12.14	20	+ .030	+ 0.016	± 0.019	+ .004	4⁵
	12.14—12.32	16	— .046	+ .038	.019	+ .009	4⁵
	12.32— 0. 0	19	+ .016	— .036⁵	.018	— .009	4
	Simple mean of 13 results	168	" — 0.004	" — 0.005	" ± 0.005	" — .0013	" ± 0 0012

We thus finally get for the mean yearly parallax

$$-0''0013 \pm 0''0012 \text{ (168 nebulae)} \quad . \quad . \quad . \quad . \quad . \quad (3)$$

This is the parallax relative to stars of comparison the mean magnitude of which is

8.75

Meanwhile, as mentioned before, MÖNNICHMEYER applied p. m. to 7 of his 183 stars of comparison.

If he had refrained from doing so, we should have found the parallax 0"0004 smaller. We thus have in conclusion:

Mean parallax of the 168 nebulae relative to stars of comparison of the mean magnitude 8.75.

$$\mathbf{-0''0017 \pm 0''012} \text{ (p. e.)} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (4)$$

In N°. 8 of the *Publ. of the Astr. Laboratory at Groningen* the mean parallax of the stars of magnitude 8.75 was found to be

$$0''0063 \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (5)$$

To this value we might apply two corrections:

1st. Because, since the publication of the paper mentioned, our knowledge about the sun's velocity has made considerable progress;

2nd. Because in its derivation a slight mistake was discovered.

I shall not apply any correction, however, because the two corrections nearly compensate each other for the magnitude 8.75. There is a fair prospect of the possibility of materially improving the values given in Publication 8 before long. It seems advisable to wait for such improvements before we alter these determinations.

If for this reason we provisionally adopt the value (5) we get:

Mean *absolute* parallax of the 168 nebulae

$$0''0046 \pm 0''0012 \text{ (p. e.)} \quad . \quad . \quad . \quad . \quad . \quad . \quad (6)$$

This result is somewhat less reliable, however, than (5) because of the additional uncertainty in the absolute parallax of the stars of comparison.

The value (6) agrees nearly with the mean parallax of the stars of the tenth magnitude.

I shall not insist on the exact amount brought out for the parallax. I shall only direct the attention to the fact that from observations covering only a period of somewhat over thirty years, we get a probable error of hardly over 0".001. If this is the case with visual observations we may look for really excellent results by photography.

The best measurable nebulae must be generally the smaller ones. The number of these which can be photographed is enormous.

With his Bruce-telescope (opening 40 centim., foc. dist. 202 centim.) MAX WOLFF obtained in 150 minutes a single photograph of the region near 31 Comae, containing 1528 measurable nebulae (Publ. Königstuhl I p. 127).

This richness of material will enable us to confine ourselves provisionally to those nebulae which allow of a very accurate measurement.

Personal errors must disappear because we shall certainly succeed in nearly every case in making our pointings on the same point for the several epochs. The peculiar p. m. will be the more thoroughly eliminated the more extensive our material; especially if this material is distributed over the whole of the sky. Errors in the precession have no influence at least on the value of the relative parallax.

I am convinced that by photography we may obtain, even within ten years, results which will far surpass in accuracy those of the present paper. Thus we may hope, in the near future, to reach a fairly satisfactory solution of the vexed question respecting the position of the nebulae in space.

The same treatment to which we have here subjected the nebulae may of course also be applied to other objects. We have already undertaken that of the Helium-stars and might perhaps afterwards try the same method for the stars of PICKERING's 5th Type.

In concluding it is only just to say that, whatever be the merit of the present investigation, it belongs mainly to Dr MONNICHMEYER. As compared with his careful and elaborate labour, that spent on the derivation of the present result is quite insignificant.

Chemistry. — "*On the course of melting-point curves for compounds which are partially dissociated in the liquid phase, the proportion of the products of dissociation being arbitrary*", by J. J. VAN LAAR. (Communicated by Prof. H. W. BAKHUIS ROOZEBOOM).

1. It is well known, that a liquid mixture of e. g. two components A and B, which can form a compound $A_{\nu_1} B_{\nu_2}$, reaches its *maximum point of solidification,* when the ratio of the molecular quantities of the two components is as $\nu_1 : \nu_2$, in other words when there is *no excess* of one of the products of dissociation of the compound $A_{\nu_1} B_{\nu_2}$.

Expressed differently: when we determine the points of solidification of a series of liquid mixtures of A, B and the compound with increasing excess x of one of the products of dissociation of the compound under consideration, then $\left(\frac{dT}{dx}\right)_0 = 0$ for the curve of solidification or melting-point line thus formed.

Hence the melting-point curve of a compound, with increasing addition x of one of the products of dissociation, will have an *horizontal* direction at $x = 0$, as soon as there is but the *slightest dissociation* of the compound in the liquid phase. If there is *no dissociation at all,* the admixture may be considered as an alien, indifferent substance, and the initial direction of the melting-point curve will show *all at once the normal descending course* at $x = 0$.

As will also appear from the following computation, the initial horizontal course will of course pass the sooner into a descending course, the slighter the dissociation of the compound is.

The peculiarity mentioned of $\left(\frac{dT}{dx}\right)_0$ becoming zero with the slightest trace of dissociation of the compound, was already proved by Prof. LORENTZ in 1892, on the occasion of an investigation of STORTENBEKER on chlorine-iodides [1]). Prof. VAN DER WAALS too has

[1]) Z. f. Ph. Ch. 10, bl. 194 et seq.