AROUND SOME OBJECTIONS AGAINST THE BALLISTIC THEORY OF VARIABLE STARS

NOTE by M. LA ROSA

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1). - In the number 21 v. 179 of the Comptes Renàus de l'Acad. des Sciences Messrs. Ch. Nordmann and C. Le Morvan have drawn attention to the following fact, which, in their opinion, would be incompatible - or at least inexplicable - with my theory of variable stars deduced from the Ritz postulate.

Changes in light intensity, i.e., in apparent magnitude, are, for certain stars, dependent on the color of the light chosen to make the observations; that is, more exactly, the extent of the change of intensity it is a function of the average frequency of the spectral region to which it belongs the light taken to make the observations. Thus, e.g., for β Lyrae the change of apparent size it clearly increases from red to blue; it's the same at 0.66 (of the size step) in red, 0.94 in green, 1.34 in blue.

The reason why this fact seems incompatible with my theory is thus expressed by the aforementioned AA.: "If the luminous fluctuation of the stars with variation continuous was due to the mechanism invoked by Mr. La Rosa, the amplitude of the variation would necessarily be the same in all regions of the spectrum luminous".

The fact referred to is certainly indisputable and is worthy of all the attention of those involved in the study and interpretation of variable phenomena. In truth I do not know, if and how it can be explained in the theory of the eclipse and in the others so far given for these mysteries phenomena, but what is certain is that my theory *also in this case*, offers a clear reason for the fact in the clearest, most direct and most *spontaneous* way, that is, without artificial adaptations. And we will explain immediately how.

To this end, let us assume that these stars on which the phenomenon in word has been observed, exhibit light changes of *small amplitude* of the order of a step on the scale of magnitude, and we recall that hypothesis fundamental of the theory is *that all variables must be double stars o more complex*.

Given this we observe that the light that comes to us from a "variable" is due only partially to the revolving companion (or more exactly to the star that satisfies the limitation related to the constant *Kb*[1]) gives rise to the changes of light). whereby in determining the law of changes of intensity, one must take due account of the light emitted by the other (or other bodies); there which only in *particular, circumstances* can be negligible (for the variables long period, with very eccentric orbits and close to the maxima).

But this is certainly not the case in "variables" of small magnitude, for which account must be taken - what Messrs Nordmann and Le Morvan did not - of the light of the variable components and of that of invariable components.

Assumed for simplicity that the star in question is a "double", of which a single component satisfies the necessary condition for it to be able to give result in light changes of small amplitude (while the other is invariable), we find that the luminous intensity of this component must exhibit change of equal magnitude in all regions of the spectrum (as Nordmann and Le Morvan think) but this constancy of amplitude one cannot present oneself in total light, because in the light of one it is superimposed that of the other component.

Indicating with ir, iv, ib the luminous intensities that it would manifest to us the variable component in red, green, and blue respectively a1 moment of minimum, the corresponding intensities at the moment of maximum they would be mir, miv, mib, where m is a constant factor; but the reports of luminous intensities presented by the combination of the two stars at the moment of maximum and minimum, in these regions are:

Indicating with i_r, i_v, i_b the luminous intensities that it would manifest to us the variable component in red, green, and blue respectively a1 moment of minimum, the corresponding intensities at the moment of maximum they would be mi_r, mi_v, mi_b, where *m* is a constant factor; but the reports of luminous intensities presented by the combination of the two stars at the moment of maximum and minimum, in these regions are:

[1] $$\frac{I_r+mi_r}{I_r+i_r}; \qquad \frac{I_v+mi_v}{I_v+i_v}; \qquad \frac{I_b+mi_b}{I_b+i_b}$$

being I_r, I_v, I_b the corresponding intensities of the invariable component.

And it is clear that the three ratios [1] will generally be different, since their equality (since $m > 1$) presupposes that it is:

$$\frac{I_r}{i_r} = \frac{I_v}{i_v} = \frac{I_b}{i_b}$$

that is, it assumes that in the spectra of the two luminous bodies the distribution of intensity is done according to the same law; that is, it assumes that the two bodies *have the same temperature*, and also the same emission behavior.

In other words, *the ratio between the maximum and the minimum of light* presented from our variable, *must*, in general, *depend on the color of the light used* in photometric measurement; and only in the particular case, very unlikely, in

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([1]) Recall that the condition of "variability" is $1/30 < Kb < 5$; is that the amplitude of the change is large only near the condition $Kb = 1/2\ \pi$.

which the two emitting bodies have *the same temperature*, that relationship can be constant throughout the spectrum.

So, in general, *our relationship will be a function of the frequency*, so that the exact measurement of the values it presents in the different spectral regions, for any given star, coupled with a good understanding of the law of distribution of the radiant energy in the spectrum (at high temperatures) will do this give a new way to determine the temperatures of the two bodies issuers ([1]).

For this reason, the fact observed for the first time by Nordmann acquires, in the light of my theory, a singular importance, not only for the remarkable confirmation that it brings them, but for the new basis that it promises to give to the investigations on the temperatures of the celestial bodies.

2). - Another objection, made by Mr. Salet ([2]) concerns a contradiction that this A. would have detected between the observed phenomena and a deduction – in imperfect truth - which he causes to arise from a summary examination of the "principle ballistic".

The deduction is this: "...periods of maximum and minimum intensity (bright of the star) must correspond to times when the speed radial increases or decreases".

The contradictory facts enumerated by Mr. Salet; they are:

a) "For stars of the Algol type, the intensity is absolutely constant and the same before and after the abrupt minimum that has been attributed until now at an eclipse. The radial velocity should therefore (in my theory) be constant during these two time intervals and suddenly begin to vary at

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([1]) As an example, I wanted to calculate the values that the ratios take [1] for the wavelengths 6500, 5200, 3900, attributing to the two emitting bodies temperatures abs. of 6000° and 8000° respectively and admitting that the second only satisfies the necessary condition (relative to the *Kb*) for a variability restricted.

To calculate the amount of energy emitted in correspondence with the individual wavelengths I used the well-known formula of Planck, and I assumed even that the hotter star has twice the surface area of the other (in reason of its greatest mass) and that the ratio *m* was equal to 3.2. The found values they are: 2.27 in red; 2.82 in the green; 3.12 in violet; and these relationships correspond the magnitude changes 0.93; 1.12; 1.25.

A more exact verification cannot be expected until measurements have been made apposite of our relationship, and until the law is better known distribution of energy in the spectrum, at very high temperatures, which probably dominate in stars. Planck's law (the best for this today's science possesses) is applicable, with good foundation, only for rather low temperatures of the source, and in the region of great lengths waveform of the spectrum.

([2]) Bulletin of the Observ. of Lyons, v. VII, no. 3, March 1925.

moment of the minimum, which seems quite impossible".

b) "For the Cepheids, the maximum of the brightness should take place at the moment where the *increase* in radial velocity is maximum...", while "the maximum intensity takes place, on the contrary, generally *about the moment when the radial speed* is maximum. There is therefore a difference of a quarter of a period between the data of the new theory and reality".

c) (Here it is a reservation) "For long-period variables, it would remain to show that the variations of intensity are accompanied, in the direction predicted by the theory, of variations of the radial velocity".

If Mr. Salet could have read my original work ([1]) instead of the popularisation article, he would have ascertained that precisely for the "variables in the long run" the theory has demonstrated all its fecundi its the its value. Examining from the spectral point of view, the consequences to which the theory leads, in the field of radial speeds, we come to give full reason. of the existence of invariable stars, with strongly expanded line spectra; of the existence of variable stars with multiple lines, with moving components and of periodically variable number; of the existence of stars (variable or not) with simple and mobile lines, etc.; and all this in accordance with observed facts, but hitherto unexplained ([2]).

My theory is precisely in this field of long-term variables can boast a fortune that very few others have had, that of having received, a few months after their appearance, the most brilliant and *spontaneous* confirmation. I mean *the discovery of the periodic changes of the radial velocity* of "Mira Ceti" [flatly denied until December 1923, a few months after the publication of my first Note ([3])], and the discovery of a "companion" of Mira; this in full accord with the fundamental hypothesis, which I had to do - certainly boldly - around these stars, attributing their constitution of "complex" stars ([4]) assuming they have two or more companions.

Concerning doubt b), concerning the Cepheids, an examination is enough to remove it *careful* of the $T = f(t)$ curves, also reported in my article in *Scientia* ([5]).

([1]) *Memoirs of the Italian Astronomical Society*, vol. II, p. 324 (1923), and *Nuovo Cimento*, vol. I (new series), January 1924.

([2]) This topic is more fully developed in my Note entitled "Radial velocities and the ballistic theory of variables", in *Rend. Lincei*, vol. XXXIII, p. 446, December 1924.

([3]) *Rend. Lincei*, vol. XXXII, lst sem., p. 590, June 1923.

([4]) Yes, compare on this point my Note on "The constitution of variables of the "Mira Ceti" type according to the ballistics hypothesis on the velocity of propagation of the light", in *Rend. Lincei*, vol. XXXIII, 1st sem., p. 3, January 1924.

([5]) La Rosa, "Astronomical proofs contrary to Relativity", in *Scientia* vol. 36 (1924), p. 5 and p. 69.

We therefore recall that the banks of the strip within which each of the curve remains inscribed, they represent the law of correspondence $T = f(t)$ in the cases in which the speed of propagation was *constant*, and respectively equal a $c + v$, or a $c - v$; and which the straight line zz' (strip axis) represents the correspondence law $T = f(t)$ in the case where the velocity of propagation of the radii was constant and equal to c.

Given this, let's look at the first two curves of figure 2 of my paper, those corresponding to the values 0.1 and 0.16 of Kb. They have light maxima at their points of intersection with the straight line zz'; maxima that is, they fall in the moments T, in which *the component according to the line of sight of the tangential speed is zero*. And the analysis confirms that this must be done until the constant Kb considered by me is $\leq 1/2\ \pi$.

Instead, in the other curves (cases in which $Kb > 1/2\ \pi$) the light maxima fall at the maxima and minima of $T = f(t)$ and therefore in *proximity of the points of tangency of the curves with the banks* and that is *near the moments* where the radial velocity reaches extreme values. The particularities observed in the Cepheids, I therefore agree with this case; while not there are no facts that agree with the first. Here, too, the theory therefore boasts a real success.

Finally, coming to the variables of the Algol type, I must point out, although I can seem superfluous, that my theory at no point and in no way is opposes the "explanation of the eclipse" that has been given so far for these stars.

The fundamental hypothesis of my construction is this: *that all the variables whether they are double or complex* stars with one or more revolving companions *in planes slightly inclined with respect to the view. So, my theory no more disavows* that in certain particular cases the partner can come and place himself on the line of sight, and conceal, in whole or in part, the light of the central star.

The evidence for this point seemed so strong to me at the time I wrote my work, not to deserve a special comment.

The existence of stars, which have generally constant intensity, and affected by a single short and abrupt minimal and which present at the same time continuously variable radial speed, *doesn't contradict*, therefore *not at all to my theory*. The change of light remains, in these cases, independent by the ballistic effect, and therefore not subject to the conditions (relative to Kb) from which this effect is regulated.

But also, for the interpretation of the phenomena presented by the stars of this type, the aid of my theory is of great advantage. Not all "variables for eclipse" have in fact the schematic behavior, very simple, mentioned by Mr. Salet. Many of them show an increase, small, it is true, but continuous in luminosity, from the end of the principal minimum until a second and not very noticeable minimum occurs; several present also different intensity in the two phases of maximum constant, which fall before and after the second minimum; finally others (always supposed variables for eclipses) present a continuous variation (just like β Lyrae) which is difficult to reconcile with that hypothesis; finally, others have two real ones maxima which have *different intensity* (η Aquilae), a circumstance this is *irreconcilable* with the simple explanation for "eclipse".

For precisely these reasons I *stopped to study* these more complex cases (which I *didn't confuse* with the former) and tried to match - and not to replace - my theory with the hypothesis of the eclipse, deriving an explanation from it perfect of the most minute circumstances of the phenomenon; how can you make sure who wants to study my Memoir carefully, and how I will try to explain more fully in a forthcoming work.

3). - Another Note by the same Mr. Salet ([1]), aims to support the previous objection, relating to the "variables of the Algol type" of a "quantitative test", from which it would emerge that astronomical measurements (?) allow also to exclude that a fraction (≤ 1/200) of the speed of the source can add up with the normal speed of light.

Unfortunately, Mr. Salet tells us nothing about the conceptual line followed nor on the "data" used in the calculation. He will therefore try to make up for the defect, following that line which comes from my indicated theory, and making use of the few and uncertain data that is around Algol (the only example that Mr. Salet explicitly mentions) I could collect.

This star has a *period* of $68^h\ 48^m\ 55^s$. About 1/7 of this time comes taken by the change of light - the abrupt low -; in the remaining *interval* the splendor varies very weakly, presenting two maxima very much flat, and a secondary minimum, slightly accentuated. The extent of the change which takes place in this phase is slightly less than 0.1 of the steps on the scale of sizes. The total variation of light of this star can therefore be interpreted as the result of two changes, one abrupt that *happens* in jerks, the other not very large but of a continuous character.

The first change can be traced back to the eclipse hypothesis 1a which, as I have explained above, is necessarily admitted by my theory. And this change takes place in entirely independent ways propagation of light and therefore by the law of changes in velocity. The other change, which is not very large, is easily explained by my theory provided that it can be assumed that the quantity *Kb*, in the case of Algol, satisfies to the limitation:

$$0.02 < Kb < 5$$

([1]) P. SALET, "On the independence of the speed of light and that of light source", in *C.R.*, v. 180, pag. 747 (2 March 1925).

remaining, however, quite far from the value $Kb = 1/2\pi$ near which the amplitude of the light change becomes very large. So on to this only point can arise the disagreement between the theory and the numerical verification.

Therefore, according to what little Mr. Salet says in his Note, it should believe that he has found for Kb a value too close to $1/2\ \pi$, i.e., such as to predict continuous changes of light of great amplitude, which should be followed by similar changes in radial velocity.

Now that such a calculation can be made with serious foundation is to be excluded certainly; because the result is strongly affected by the grave uncertainty that dominates in parallax measurements, an element that of necessity appears in the calculation.

For the case of β. Persei, I found the following numbers:

π: = 0''.007 ± 0.027, Russel;

π: = 0''.122 ± 0.026, Flirt;

π: = 0''.037 ± 0.020, Chase,

perhaps discordant, despite the ostentatious equality of precision.

According to a more recent and more precise determination, made with the method photographic by Messrs. Lee and Joy at the Yerkes Observatory would be: π = 0''.027 ± 0''.010.

But according to more recent data kindly provided to me by prof. A. Bemporad would be: π = 0''.052 ([1]).

As can be seen from the set of numbers transcribed, we are far from the possess that *certain* element of fact that can lead us to a judgment *quality* unappealable, despising the numerous and beautiful confirmations that my theory has largely collected, in the field of photometric phenomena, spectroscopic and statistical data presented by variable stars.

Later, when it will be possible to measure the parallax with *sufficient accuracy* of certain variables, it will be possible to set up the quantitative control that the Mr. Salet want; and perhaps then my theory will be found to lead the way simpler and more direct for measuring stellar distances.

In conclusion, I take the liberty of reaffirming my firm conviction here on the soundness of my theoretical sketch.

([1]) As an orientation essay we can try to calculate the value of *Kb* that can be drawn from the above numbers. If we take the value for π 0''.027, considered more approximate, for the distance AIgol-Earth we find 121 years light, and therefore for *K* the value 1.54·10-4. And since *b* is 1.3·10-4 for the main star, and 3·10-4 for the companion, we find for *Kb* the values 2 for the main star and 4.62 for the mate. One should therefore conclude that the mate does not give rise a change of light, and that the main star gives rise to a change of small amplitude. But the uncertainty of π does not allow to rely on these results.

It does not lack the comfort of impressive confirmations; impressive by vastness of number, by richness and variety of coincident details, out of spontaneity and *sincerity*. The meaning and strength of these confirmations (infinitely vaster and more beautiful than those that the "theory of relativity" still goes alleging) would have appeared to everyone more than satisfactory, *really enthralling*, if "relativistic" concerns had not hindered the mere reading, and even printing, of my work.

But I'm confident that a large and peaceful discussion like this one will provoke from the criticisms examined here - will end up making everyone recognize its value and fruitfulness of the "ballistic principle", which not only promises a lot for Science of tomorrow - as the eminent opponents recognize ([1]) - but which gives, and not a little, for what is possible to build today.

Palermo, R. University, April 1925.

([1]) See P. SALET. loc. cit.; CHARLES NORDMANN and H. LE MORVAN. in *C.R.*, April 15, 1925.

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