VHE AND UHE GAMMA RAY ASTRONOMY. HISTORY AND PROBLEMS*

Short historical outline of VHE and UHE gamma ray astronomy is given including personal reminiscences of the author. Special emphasis is made on the development of experimental technique and methods of analysis. Main results obtained in the field up to date are critically reviewed.

1. INTRODUCTION

VHE and UHE gamma ray astronomy are dealing with the energy range 10^{11} *eV* and above. One calls usually VHE the range 0.1 *TeV* < E_γ < 10 *TeV* and UHE range E_γ > 30 *TeV* . If we look at the scale of electromagnetic waves available for astronomical observations, about a half of this scale is gamma ray range, and in its turn a half of entire gamma ray domain (about five decades of energy) comprises VHE and UHE ranges. But starting from UV wave length interval each further energy range seems to be less and less informative with the increase of energy.

This is partly due to continuously diminishing intensity and corresponding growth of technical difficulties of detection. Nevertheless, there is considerable enthusiasm in attempts to widen observational window at highest energies. It is stimulated by the hope to discover astrophysical objects of a new type, e. g., the sources generating cosmic rays.

Two mechanisms are known to be responsible for production of high energy gammas: interactions of electrons with matter, magnetic field, light (inverse Compton effect) and decays of neutral pions generated in cosmic ray nuclear interactions with matter. The latter process is perhaps unique for UHE photons and this connects directly their observation with cosmic ray sources.

Unlike gamma ray astronomy at 0.1 *MeV* - 10 *GeV* where spacecraft and balloons are used to expose gamma telescopes in space or at highest levels of the atmosphere, VHE and UHE observations are essentially ground based. This is not only because cascade showers generated by high energy photons give intensity of Cherenkov light (VHE) or number of particles (UHE) big enough to be recorded at ground level but mainly because necessary effective area ($\geq 10^4\ m^2$ and $\gg 10^4\ m^2$, respectively) so far cannot be realized in space. It seems natural to consider VHE and UHE experiments together. Both entangle with similar difficulties due to high background, hence similar methods are used to overcome them.

The aim of my historical excursion is not to give a thorough review of all experiments and results, but rather to point out main directions of development and real problems confronting us. Many recent experiments claim measurable fluxes of VHE and UHE photons from a dozen of astronomical objects. How reliable are these numerous

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* *Cosmic Gamma Rays, Neutrinos, and Related Astrophysics, Erice, 1988, NATO ASI series, ed. M. Shapiro and J. Wefel, p. 163.*

discoveries? My personal attitude is sceptical. The position of devil's advocate is rather unfavourable, but I hope my scepticism can be excused due to two reasons.

First, I carried out the first large scale VHE experiment at Katsiveli, Crimea in 1960-63 with purely negative result. Second, starting later UHE experiment at Baksan (1984-87) we could not help obtaining some positive indications as many others. So my scepticism is not the criticism of an outsider but self-criticism as well.

2. THE BEGINNING

As a starting point one can consider the proposal by Cocconi [1] to search for gamma ray sources as narrow-angle anisotropy in distribution of extensive air showers. Cocconi had in mind air shower arrays at mountain altitudes, near 1/2 of atmospheric depth with characteristic energy of 1 TeV and angular resolution ~ 1° . This idea was not realized so far in its original form, but certainly it stimulated further attempts to use angular anisotropy as a nondirect evidence of the presence of gamma ray sources. Another possible way is to use particular features of gamma initiated showers, e. g., presumed deficit of muons. This last idea was put forward approximately at the same time as G. T. Zatsepin proposed to use Cherenkov light of EAS instead of particles in Cocconi's experiment. The Cherenkov emission of EAS was discovered by Galbraith and Jelley in 1952 [2]. Next season the work on studying this phenomenon started in the USSR [3].

I personally did not hear Cocconi at 6th ICRC as I was at that moment involved in quite another business - radiation belts and other experiments in space. G. T. Zatsepin came to me and proposed to discuss the possibility of the use of Cherenkov light as he considered me to be an expert in Cherenkov technique after experiments at Pamir mountains in 1953-57. In 1960 the principles of VHE Cherenkov technique were clearly formulated [4] and four 1.5 *m* parabolic mirrors were mounted in Katsiveli, Crimea .

It is interesting to note that first results of this experiment [5] were published simultaneously with the results of two groups searching for muon poor showers [7,8] and thus the births of VHE and UHE divisions of gamma ray astronomy occurred at the same time.

One can note also that at that moment the prospects of VHE astronomy seemed to be rather modest and negative result was anticipated. It was written in [6] like this: "Present theoretical estimates... indicate a flux of high energy photons which does not encourage one to hope for a successful observation of such photons. Nevertheless, taking into account the significance of the problem, we made an attempt to observe this phenomenon experimentally..."

3. THE EXPERIMENT IN KATSIVELI, 1960-1963

In the season of 1960 the experiment was started with four mirrors on four independent rotating frames. Next year the number of mirrors was increased up to 12 and this basic version of the array I would like to describe briefly. Each parabolic mirror (fig. 1) had 155 *cm* diameter, and all 12 with parallel oriented optical axes (accuracy $\pm$ 0.2°) were divided in four independent channels (three parallel mirrors mounted on one of four independently rotating frames). PM tubes with 4.5 *cm* diameter were installed at the foci after correcting lenses which were used for the improvement of angular characteristics of the telescopes.

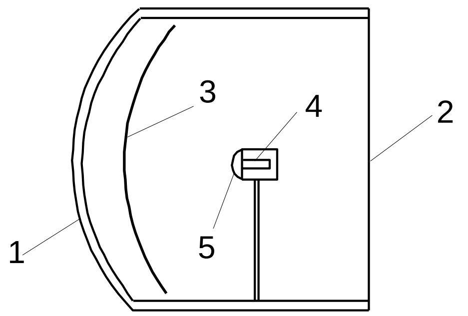

Fig. 1. The light receiver for Katsiveli experiment.

The signals of three PMs in each channel were summed and two-fold coincidences of pairs of channels as well as four-fold coincidences of the whole array were recorded. Only counting rate of four-fold coincidences was completely free from the contribution of random night sky light coincidences.

By calibrating procedure made using Cherenkov flashes of cosmic ray muons in plastic generator the threshold light intensity was measured as 280 *photons per* m^2 . Calculated energy thresholds were equal to 1.3 *TeV* and 3.4 *TeV* for primary protons and photons, respectively. Counting rate corresponding to this threshold was measured 200 *per min*. Effective opening angle was approximately 1.75° (FWHM). Zenith angle dependence of counting rate was observed, in agreement with theoretical estimation, to be of the form $Cos^n\theta$ with $n = 2.5$.

All observations were made during moonless clear nights using drift scan mode. Starting measurements the optical axes of all mirrors had been fixed to see the point on celestial sphere the source was going to cross after some time interval ΔT . After the observation lasting 2 ΔT all procedure could be repeated and several such scans for a given source were available during one night.

One should remember that in that remote epoch nothing was known about pulsars, X-ray sources and their periods. Radiosources were considered as best candidates for gamma ray emitters and hence radiosources were mainly looked at. The signals were searched from the following objects: Cygnus A (191 scans). Crab Nebula (47 scans), Cassiopeia A and Virgo A (20 scans in each case), Perseus A and Sagittarius A (4 and 7 scans respectively). Some random trial scans were made also for several clusters of galaxies (Ursa Major II, Corona Borealis, Bootes, Coma Berenices).

Statistically significant positive effect from any of these sources was never found. Relative excess value $\delta = (N_0 - N)/N$, where N_0 and N are the mean counting rates inside the time interval $\pm\, t_0$ and outside it, did not reach 2 σ in all cases. So no point source of VHE photons (> 5 *TeV*) was discovered. Typical upper limits obtained in this experiment are equal to $(2 - 5) \cdot 10^{-11}\ cm^{-2}\ sec^{-1}$.

The obvious preference of Cyg A having the biggest number of scans needs some explanations. The reason was some positive effect obtained during first scans that we were trying to confirm. But long observation resulted in disappearance of positive effect and only upper limits were obtained for all sources.

I would say that this was important result at that time. I do not know why but generation of high energy electrons via direct acceleration was very unpopular idea. People thought (and Cocconi estimating in his proposal [1] the flux from Crab Nebula was of such opinion) that electrons in Crab Nebula were of secondary origin, namely, the result of the chain of processes $pp \to \pi \to \mu \to e$. If so, considerable flux of gamma rays should be generated in the process $pp \to \pi^0 \to 2\gamma$ Upper limit obtained in Katsiveli experiment showed VHE gamma ray flux to be two orders of magnitude less than had been anticipated in the frame of this model. Thus direct acceleration of electrons was for the first time experimentally proved.

It is interesting to compare the parameters of this old array with that of modern Cherenkov telescopes. Let us recall that in 1986 authors of new giant project HERCULES [9] wrote: "As a standard for comparison we consider a conventional atmospheric Cherenkov detector with an energy threshold of 10^{12} eV with a system of three 1.5 m reflectors, operated in coincidence with a field of view (FWHM) of 1.7°, the background counting rate is ~ 1/sec." So in 1986 the "conventional" Cherenkov detector has all the same characteristics but the area four times less than Katsiveli experiment finished in 1963.

The experiment made by Durham group in Dugway, Utah, which claimed probably the biggest number of discovered sources, used the array that was not only of the same class and dimensions, but rather could be called a copy of Katsively array: the same four telescopes consisting of three paraxial mirrors of the same diameter.

Now the question arises: if the instruments of similar power are still in use, what has changed so drastically that instead of total absence of sources now there is multitude of them ? Very important problem of time variability will be discussed later. Now let us follow historical order of events.

The next experiment in VHE astronomy started near Dublin when the Katsiveli Cherenkov telescope had been already dismantled and results of final analysis of data had been published [10]. In 1964 Long et al. [11] observed several celestial objects, mainly quasars, using small directional Cherenkov telescope. The system consisted of two mirrors of 92 cm diameter, full geometrical field of view was at first 5° and typical counting rate was 40-100 counts per min.

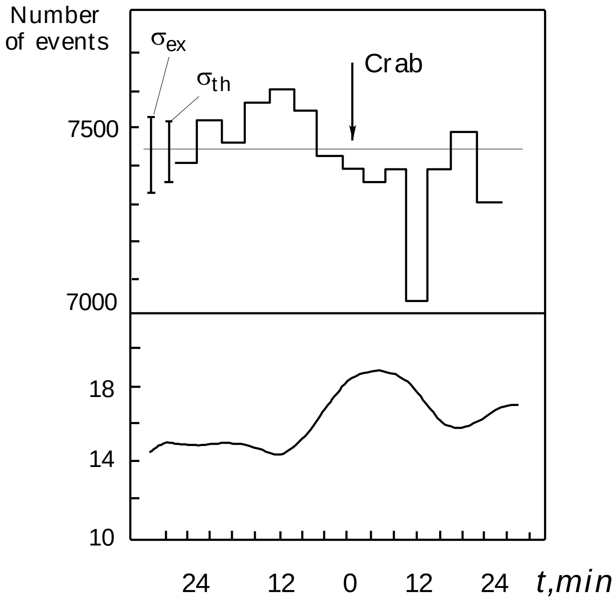

Fig. 2. The drift scan diagram for the Crab Nebula. The data of 1961. Average counting rate 143 per min. Below: total average output current of all 12 PM tubes.

Note that at this time another class of objects became popular. But luckily we have one that was and is being observed inevitably by every telescope. This is the famous Crab Nebula. Extremely interesting and instructive is the fact that after obtaining no signal with much more powerful array at Katsiveli (fig. 2 shows the results of Katsiveli experiment on the Crab Nebula), next experiment reported several years later positive effect from Crab [12]. In this paper the angular resolution was improved as compared to [11] but all effect was obtained during 9 drift scans (compare with 47 drift scans made for the source at Katsiveli). It is worthwhile to note that authors themselves did not insist on significance of this result estimating it as 1% (in fact even this figure was called in paper [12] unreliable). But nevertheless this experiment opened an era of discoveries in VHE gamma ray astronomy.

At the same 10th ICRC in Calgary where paper [12] was reported Fazio et al. [13] announced that big 10 m reflector was designed at Mount Hopkins, Arizona. This was the first attempt to obtain some advantage improving original technique.

4. DIRECTIONS OF DEVELOPMENT. ATTEMPTS TO IMPROVE VHE TECHNIQUE

4.1. The increase of diameter of reflectors

Big reflector of Whipple observatory at Mount Hopkins [13] was constructed as a mosaic from 248 2-ft hexagonal mirrors mounted on one frame. Similar construction was used in reflectors of Narrabri optical intensity stellar interferometer [14] that was operating as EAS Cherenkov light receiver in 1968-1974. This system has two paraboloidal dishes 6.5 m diameter from 252 hexagonal 38 cm mirrors. More recently very large mirrors of solar collectors of 11 *m* diameter were used by JPL-Iowa-Riverside collaboration at Edwards Air Force Base, California [15]. These are composed from 228 rectangular mirrors of average area 0.4 m^2.

Many new projects are going to use collectors of solar power stations as Cherenkov light receivers. This is not only because industrially produced solar collectors are easily available. There is a common belief that with lower threshold significant improvement of signal to noise ratio can be achieved. Indeed, if gamma rays have a spectrum of slope γ_g and cosmic rays have a slope γ_p , then (see e. g. [16])

$$\frac{S}{N} \propto E^{-(\gamma_g - \frac{\gamma_p}{2})}$$

and one can hope to have some improvement when $\gamma_g > \gamma_p/2$. But it is generally believed that gamma ray spectrum is flatter than that of cosmic rays. It well may be that there is no significant difference of γ_g and $\gamma_p/2$. In this case no improvement will be obtained, and I think this is really so. Let us analyse whether statistical significance of signals from the sources observed with large reflectors is better than in observations using conventional systems. The answer is probably no and we can conclude that the increase of diameter of mirrors up to now has not changed experimental situation considerably.

4.2 Long base paraxial mirror systems

The first attempt to use two reflectors with large separation as a gamma ray telescope was made by Hanbury Brown et al. [14] with Narrabri stellar interferometer. In 1968 they carried out scanning of the Crab Nebula and two southern sky pulsars. Later the same interferometer was used by Grindlay et al. [17] in "double beam" mode which will be described lower.

In 1978 Turver and Weekes [18] discussed the system of two big reflectors operated in parallel with a separation of ~ 100 *m*. University of Durham gamma ray facility at Dugway [19] used four independent telescopes located at the centre and apices of an equilateral triangle with a side 100 *m*. Tata Institute group at Ooty, India [16] constructed a system of 10 small (0.9 *m* in diameter) mirrors at the radius 55 m from the centre where 8 larger mirrors (1.5 *m*) array was deployed. New arrays Potchefstroom in South Africa [20] and White Cliffs in Australia [21] have separation of order 50 m. The same are planned for new arrays (future projects in survey by Turver [22]) .

There was a hope to improve angular resolution measuring time delays between separated detectors. It is possible in principle but fast timing in this case is going together with the reduction of coincidence rate and practically no improvement is achieved. As far as I understand Durham group used their separated array aggregating counting rates of four telescopes and this is the best argument against advantages of fast timing. Nevertheless, new projects of separated arrays are planned and to use big reflectors with large separation is a kind of general tendency in contemporary arrays (though recently Cawley [28] proposed so called distributed array of independent Cerenkov telescopes with rather modest mirrors).

4.3 Imaging systems

Imaging or multielement systems are also very popular. As long ago as in 1958-1960 Sekido in Japan used directional muon Cherenkov telescope with many PM tubes at the focal plane [23]. At the same time Brennan et al. [24] in Australia included first multielement light receiver in an air shower array in Sydney . The purpose of this system was to study the development of the shower in the atmosphere without connection with gamma ray astronomy. Later this technique was used to obtain shower images in observations of the fluorescence light from EAS.

In 1981 Weekes [25] proposed to use the system of 37 PM tubes for Mt. Hopkins 10 *m* reflector. This was done very soon at first with 19 PMs [25]. According to Weekes many advantages can be achieved with new system, e. g. energy measurement, discrimination against proton showers, simultaneous observation of ON and OFF regions and so on. Extensive Monte Carlo calculations were done by Hillas [27] to prove the possibilities of imaging technique.

However, it seems that in data published up to now none of these advantages was used. And it is clear that practical difficulties are considerable. Each image should be distorted due to fluctuations and problems of cuts and criteria, the choice of image parameters are not easy to overcome.

4.4 Double beam technique and others

Grindlay et al. [17] made an attempt of improvement of Cherenkov method using so called double beam technique. In this experiment two computer controlled reflectors of Narrabri interferometer had nonparallel orientation so as to observe the maximum of electron-photon shower from particular source. At the same time two off-axis PMs through the same reflectors were observing lower region in order to register Cerenkov emission of muon core of the same shower. According to authors in this way the rejection of 50% of proton shower background was achieved.

This method can be compared with separation of gamma and proton showers using muon detectors in UHE air shower experiments though efficiency of background rejection here is even worse. Similar situation can arise in imaging experiments described above, where, according to calculations by Hillas proton showers have secondary maxima generated by local muons.

Like in all other attempts with double beam technique there is no considerable improvement of experimental situation.

5. UHE EXPERIMENTS AND TECHNIQUE

According to abstracts of announced lectures of this school some of the lecturers (G. Yodh, J. Linsley) perhaps will discuss experimental details of UHE arrays. So I would like only to touch this subject in brief.

Comparing with VHE instruments one can see that air shower arrays used in UHE experiments are much more standard. Typical array is plastic scintillators of area 0.25 - 1 m^2 scattered over ground surface with separation from ten to several tens of meters. Usually only the number of detectors determines the quality of an array.

Two deviations from this common type are Baksan air shower array [29] and Top Gran Sasso [30] (under construction now) where instead of numerous small area scintillators several big area (~ 10 m^2) detectors are used. I think this latter type has certain advantages though up to now no careful comparison of two types of arrays has been made. In any case much more important is the presence and area of muon detector.

Probably the only nontraditional experimental method proposed recently by Poirier et al. [31] is trying to utilize angular distribution of particles measured by tracking devices. The authors hope to have good angular resolution and even separation of gamma and proton showers, but technical difficulties of this experiment seems to be enormous.

The first results of UHE observations were published in 1983 [32,33]. In five years many new arrays were created and some old EAS arrays became gamma ray telescopes. But up to now the first Cyg X-3 result of Kiel group [33] is the brightest and practically the only one where DC signal was statistically significant. And, nevertheless, there are many new sources. As in VHE range almost all progress here is connected not with the technical improvements but with sophisticated methods of data analysis.

6. REMARKS ON METHODS OF OBSERVATION AND DATA ANALYSIS

In Katsiveli experiment only drift scan mode was used. Now often tracking mode is preferred. In order to separate signal from background first method exploits suggestion of background isotropy, the second one uses constant intensity of background in time. Drift scan mode is most suitable to measure average excess of counting rate in the source direction above background counting rate. Now it is called sometimes DC signal. Alternative possibility can be called AC signal search. This can be done when signal is either periodic or sporadic (in the form of relatively short bursts). In this case tracking mode may have certain advantage for Cherenkov directional telescopes in VHE gamma ray astronomy. This advantage is connected with full and continuous utilization of all time available for observations (there is no necessity to spend time measuring background).

In UHE case when counting rate in a certain direction is obtained by off-line analysis of raw data the simultaneous analysis of background is not a problem. Nevertheless, in the UHE range after Kiel group initiative the analysis of phasograms prevails (at least in Cyg X-3 case).

I do not want to say that to discover a periodicity of a given source is of no interest. On the contrary, it well may be that just the light curve is most important for understanding the mechanism of gamma ray production. However, the main thing at the moment is to establish in most reliable and undoubtful way the very fact of each source emission. And the question is: what is the best way to do this, DC or AC mode ?

There is a case when AC mode is preferable, namely, when two conditions are valid:

1. The period of the source is well known and is convenient for observation being not too much less than 1 sec and not too much larger than 1 min.

2. The emission is concentrated in the small range of phases $\Delta\varphi \ll 1$ and there is practically no emission during all other phases of the period.

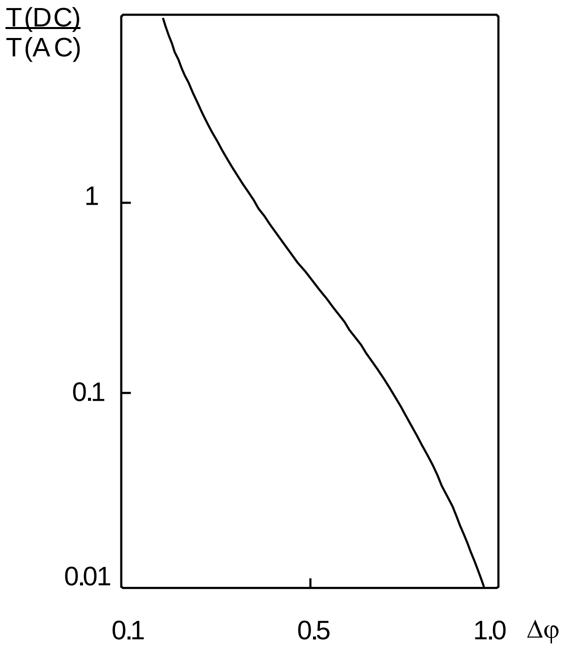

Fig. 3. The efficiency of the search by AC-mode (periodical signal) compared with DC-mode, as a function of duty cycle. T(DC)/T(AC) is the ratio of the observation time intervals (for equal confidence levels).

In order to compare the efficiencies of DC and AC methods let us estimate the time that is necessary to obtain a certain confidence level for different values of $\Delta\varphi$, active phase duration of the source.

The estimate of χ^2 value of a histogram with n bins is

$$\chi^2 = n + \frac{\lambda^2}{1+\frac{\lambda}{\sqrt{N}}}\left(\frac{1}{\Delta\varphi}-1\right)$$

where N is full background number of events, $\lambda = M/\sqrt{N}$ is the effect measured in units of standard deviations (M is the full number of counts from the source in DC mode). Corresponding value for AC mode is estimated as

$$\lambda_0 = \sqrt{2\chi^2} - \sqrt{2f-1} = \sqrt{2\chi^2} - \sqrt{2(n-1)-1}$$

(f is the number of degrees of freedom). Fig. 3 shows the ratio of observation times which are necessary to obtain a given confidence level (e. g., 5σ) by DC and phasogram methods, namely

$$\frac{T(DC)}{T(AC)} = \left(\frac{\lambda}{\lambda_0}\right)^2$$

In case of fig. 3 n is equal to 10 that is especially advantageous for $\Delta\varphi = 0.1$. If one chooses n = 100 then for $\Delta\varphi = 0.1$ *T(AC)* increases by a factor 2.5, and for $\Delta\varphi = 0.01$ drops 10 times. In this last case the advantage of AC method becomes really substantial and if indeed gamma ray sources emit periodically so short bursts, serious attention should be paid to this method.

There are indications that pulsars and X-ray binaries may be operating in this way. But if it is not proved definitely or the period is totally unknown then popular now enthusiastic search of periods can be misleading.

7. BRIEF OUTLINE OF RESULTS

7.1 Crab Nebula

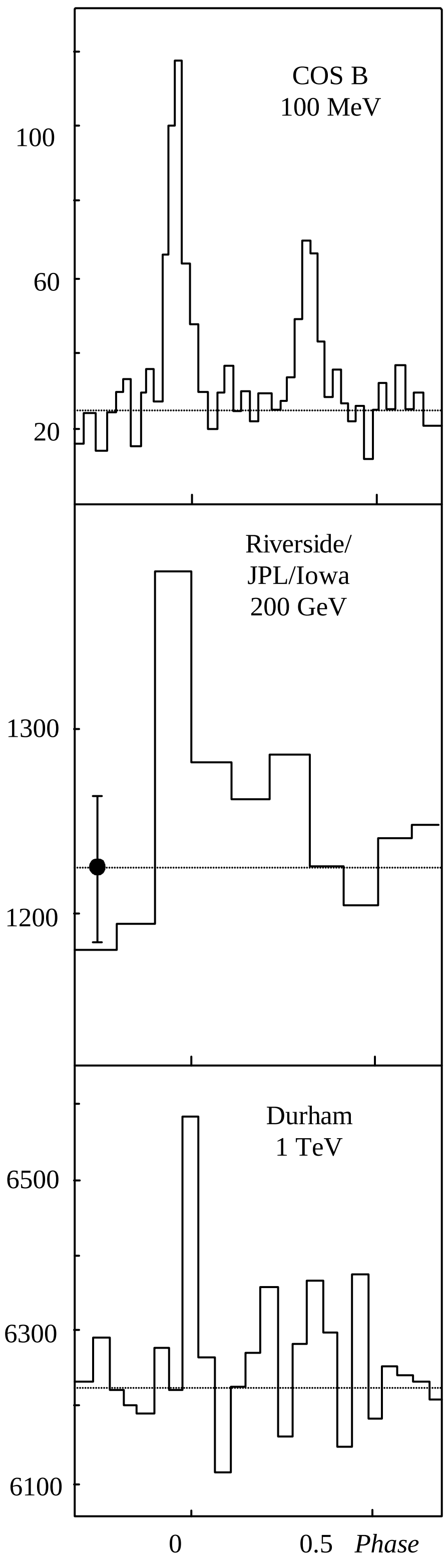

Fig. 4. Phasograms for Crab Pulsar at different energies.

This object is very suitable if one wants to demonstrate severe contradictions of VHE and UHE experimental data. Some people reported steady nonpulsed DC emission [34], others observed no DC signal but totally pulsed flux [35]. Light curves with only one pulse [35] and with interpulse [16] were published. TIFR group at Ooty pretended to measure energy spectrum of emission and obtained the integral slope γ = 1.2. Quite different value can be obtained if we compare the data of Durham group [35] and of Riverside-JPL-Iowa collaboration [15] (fig. 4). If one takes into account only the narrow peak at $\varphi = 0$ then total Riverside increase is 1.2% but in Durham data it equals to 0.22% and this corresponds to the integral slope of the increase $\gamma \sim 2.7$.

At the same time Lodz group [39] proposed the slope 0.4 in order to agree their data at 10^{16} *eV* with others.

The fluxes in every energy range are contradictory, as can be seen from fig. 5. Very high flux of Mukanov contradicts to the flux value of Jennings et al. [36], Tien Shan positive effect [37] is in direct contradiction with Baksan data [38], high flux of Lodz group [39] is not confirmed by Haverah Park results [40].

All this probably can be agreed in some complicated time variability. Who wants can try to construct extremely sophisticated model reconciling all observed features. But if we know that all published results are obtained near the threshold of detectability then natural demand seems to be possible: give us first fully reliable data.

Up to now I consider one result as most encouraging. According to Durham group (see fig. 4) the pulse width $\Delta\varphi$ is very small, may be as

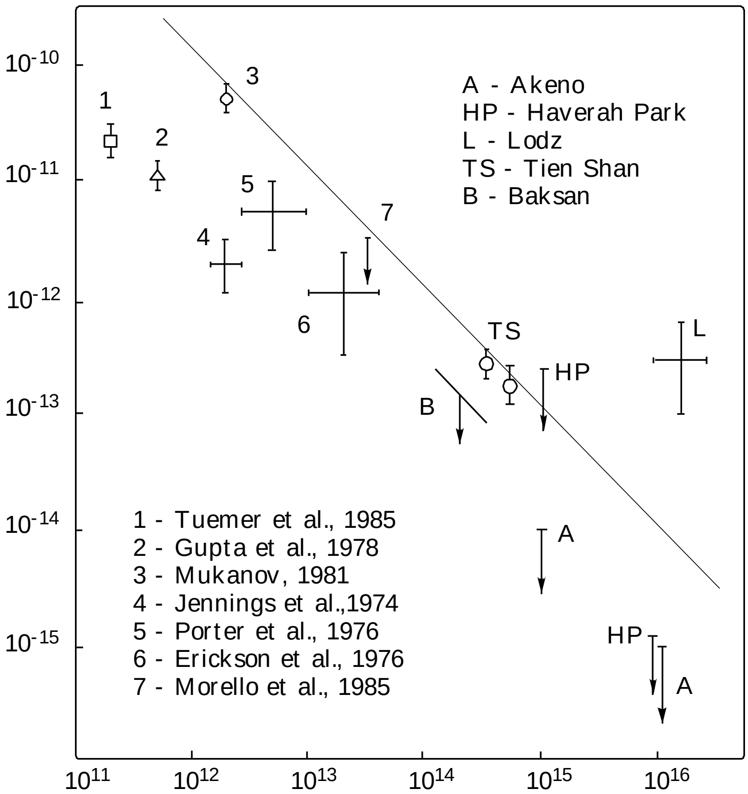

Fig. 5. Integral gamma ray fluxes from the Crab Nebula as measured by different groups at different energies.

small as 1%. If this is really so, then according to simple philosophy given above one can hope to improve considerably the statistical significance of data by improvement of timing and precision of Crab ephemeris.

7.2 Other pulsars and binaries

Not reviewing all claimed sources I would like to attract attention to several results which are most interesting in my opinion. Vela pulsar is the

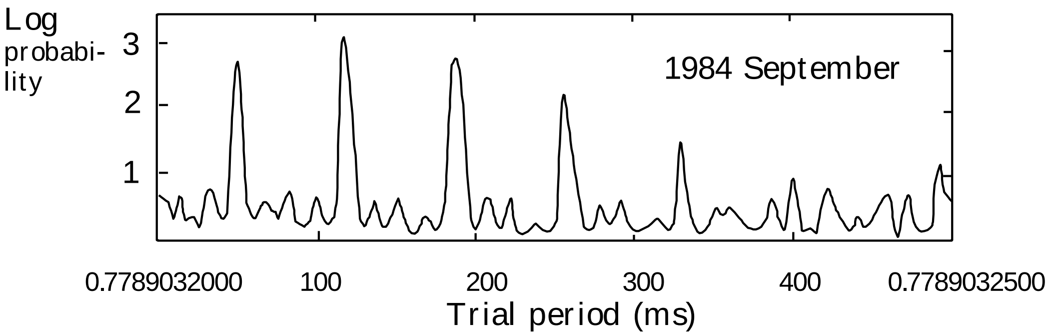

Fig. 6. Durham group Rayleigh probability for PSR 1937+21 pulsar.

most powerful gamma ray source at lower energies. It was observed twice in TeV region [16,41] and both times from the northern hemisphere, while there are many Cherenkov telescopes in southern hemisphere (the result of Grindlay et al. [17], sometimes cited as positive had only 2σ significance). Why so ?

Her X-l should be mentioned here as after the first publications of sporadic pulsed emission by Durham group [42] and Whipple observatory [43] now both of them announced simultaneous independent detection of one episode of such emission [44]. This is very serious argument (unique up to date), though both detections seem to be very near to the threshold of detection.

Most fantastic result was obtained from the shortest 1.5 ms pulsar PSR 1937+21. In the fig. 6 the frequency spectrum from [45] is presented which shows also characteristic patterns caused by 24 hours period of repetition of the data recording. For this a superstability of pulsar emission and timing in the recording system is to be believed. A pity that no data of DC signal from this source is available now.

7.3 Cygnus X-3

This object had put the most dramatic flavour to all the field. It happened not at the first claim by Stepanian et al. [46] and later observations by Cherenkov technique, but when Kiel group announced in 1983 that they see the source in the *PeV* energy range [33] by ordinary EAS technique. This was quite unexpected but rather convincing as the signal was recorded both in DC and in 4.8 *hr* phasogram (fig. 7). The result was obtained by a posteriori analysis of the data accumulated in 1976-1979 and confirmed immediately by Haverah Park array [47]. To the time of La Jolla ICRC in 1985 the excitement reached its maximum after the announcement of quite unbelievable result: Cyg X-3 signal was observed underground (first of all NUSEX and SOUDAN experiments).

At La Jolla I argued the NUSEX-SDUDAN claims not only as having no physical interpretation, but mainly as controversial to other data, Baksan underground data first. Later others (Frejus, Kamiokande, IMB) completed their analyses disapproving the sensation. All this demonstrated that the phasogram method as used by NUSEX group can provide a mirage on the level of 4-5 σ. Probably this stimulated Chardin and Gerbier to criticize all experimental evidences concerning Cyg X-3 [48].

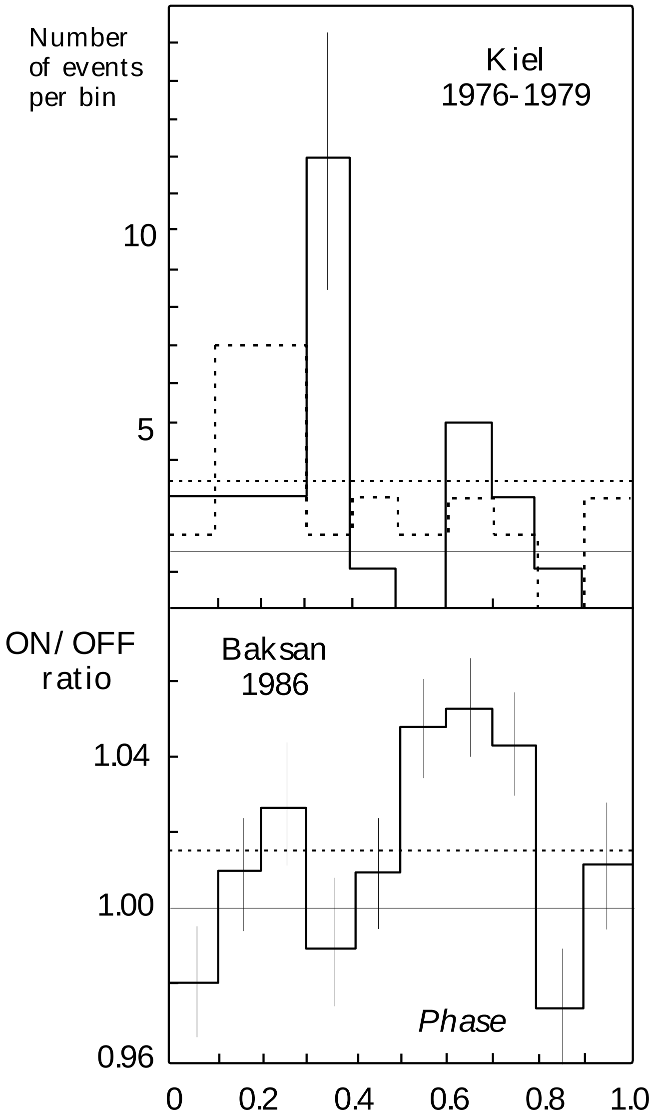

Fig. 7. Kiel and Baksan phasograms for Cyg X-3. Thin horizontzl lines represent the backgrounds. Dashed horizontal lines correspond to DC signals. Kiel: solid histogram - Parsignault ephemeris, dashed histogram - Van der Klis ephemeris.

Their criticism can easily be extended to all sources in VHE and UHE regions. Basically I agree with most of these doubts. But it is very difficult to disregard all numerous positive findings of Cyg X-3 observations, such as the DC signal and concentration of phasogram peaks at $\varphi = 0.2$ and $\varphi = 0.6$-0.7, especially if you are the author and cannot find the error in the data.

At La Jolla we presented the first data from Baksan EAS array, which was the first among detectors of a new type, combining good angular resolution ~ 1° and high counting rate ~ 1 per sec. In 1984 and 1985 we did not observe any average DC signal and because of this I regarded the peak at $\varphi = 0.6$-0.7 as nonsignificant. But afterwards in 1986 we had a steady DC signal, especially strong in March, May and October-November period. The total 1986 data are shown in fig. 7. Comparing Baksan and Kiel data one can see the difference in positions of maxima of phasograms. Independent of what version of Kiel is taken, using Parsignault or Van der Klis ephemerides (the latter is more precise, but gives less impressive phasogram), the difference is $\Delta\varphi \sim 0.5$ and is comfirmed by Haverah Park results.

The most strange in this comparison is the magnitude of the effect: ~ 100% in Kiel experiment and ~ 1% at Baksan. This is difficult to explain by the difference in energies (1 and 0.2 *PeV*) even for very flat Cyg X-3 spectrum assumed. Bhat et al. [49] suggested that Cyg X-3 is steadily dying, but I cannot regard the experimental evidence for this as convincing.

Now, there is a difficulty with 1986 Cyg X-3 Baksan data itself and it concerns the comparison with simultaneous data obtained at Los Alamos.

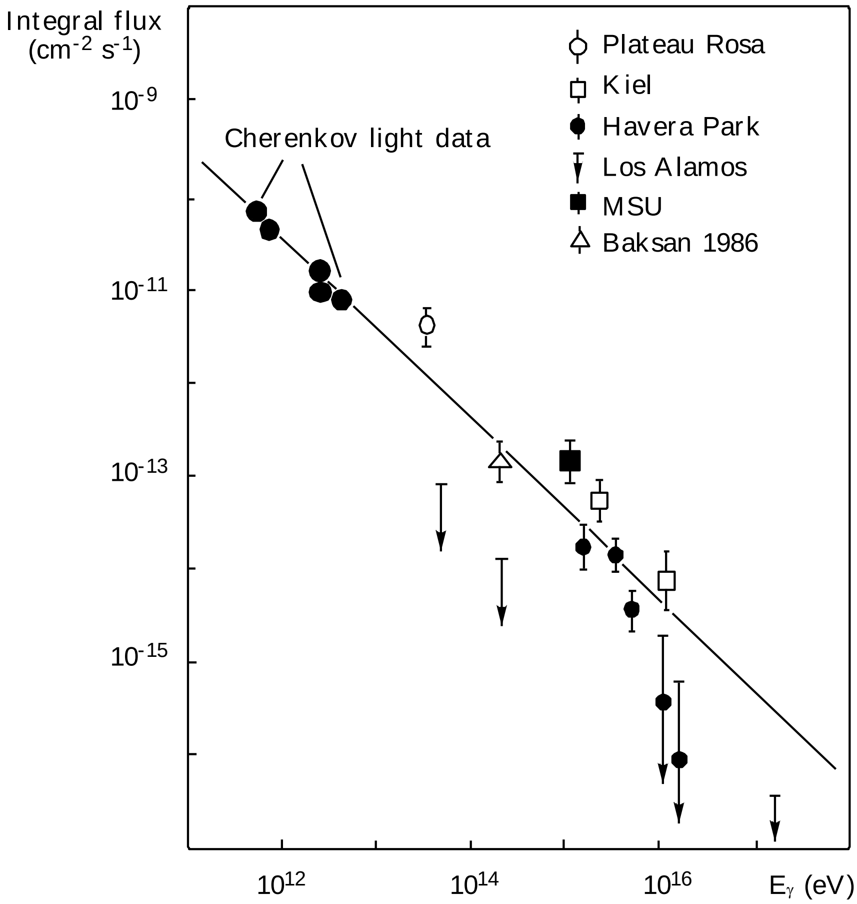

Fig. 8. The integral fluxes from Cygnus X-3 as measured by different groups at different energies.

The sensitivity of this new array called CYGNUS is similar to that of Baksan, the angular resolution is probably better. Los Alamos group accumulated 265 day data in 1985 and did not find a signal from Cyg X-3. This is a serious discrepancy which is not understood so far. But quantitatively the discrepancy is not as big as one can find from fig. 8 where results of many groups are plotted. Presented in fig. 8 are not the direct experimental data, but calculated fluxes. The result of such calculations depends on many details, such as suggested source energy spectrum, calibration of array by cosmic rays (used or not), analytical or MC calculations of the efficiency, taking or not into account absorption of gammas on the way from the source and so on. If we do the analysis of Baksan data in the same way as Los Alarms, the discrepancy of them in fig. 8 becomes smaller by a factor 5 or 7, but still the fact remains that Baksan sees a 3σ positive effect and Los Alamos does not.

For the future I would like to suggest the comparison of different data first of all in terms of the cosmic ray intensity, just ON-OFF ratio, indicating the angular window and the counting rate (the next important feature is the calculated effective area as a function of energy).

Finally, I would like to mention two evidences which could be in favour of reality of Cyg X-3 as a VHE-UHE gamma ray source. One is the striking recording by Baksan EAS array of a big increase in October 14-16, 1985, several days after the maximum of the biggest radio outburst (fig. 9). The probability of this to occur by chance is less than 10^{-3} and if the effect is real it demonstrates a new type of activity of Cyg X-3.

The other fact is the evidence in favour of 12.5 *msec* periodicity at TeV energies. If this is confirmed, it would certainly mean that TeV Cherenkov telescopes become a real tool in Astronomy.

7.4 Can the Milky Way be seen in VHE and UHE gamma rays ?

As everybody knows the first decisive experiment in gamma ray astronomy was made in late 60ies by Clark, Garmire and Kraushaar [50] on board of the OSO-3 satellite and discovered was ~ 100 *MeV* gamma ray emission from the Galactic plane. Investigation of Milky Way structure in 0.1-1 *GeV* range made by COS-B experiment is still the most important contribution to the HE gamma ray astronomy.

The detectability of the Milky Way depends of course on the assumed slope of gamma ray spectrum. If we assume extremely flat spectrum with integral exponent γ = 1, then there are no technical problems. As it was shown in [51] in that case all the measured at 10 TeV anisotropy can be attributed to gammas. Then several per cent increase is expected from the Galactic plane.

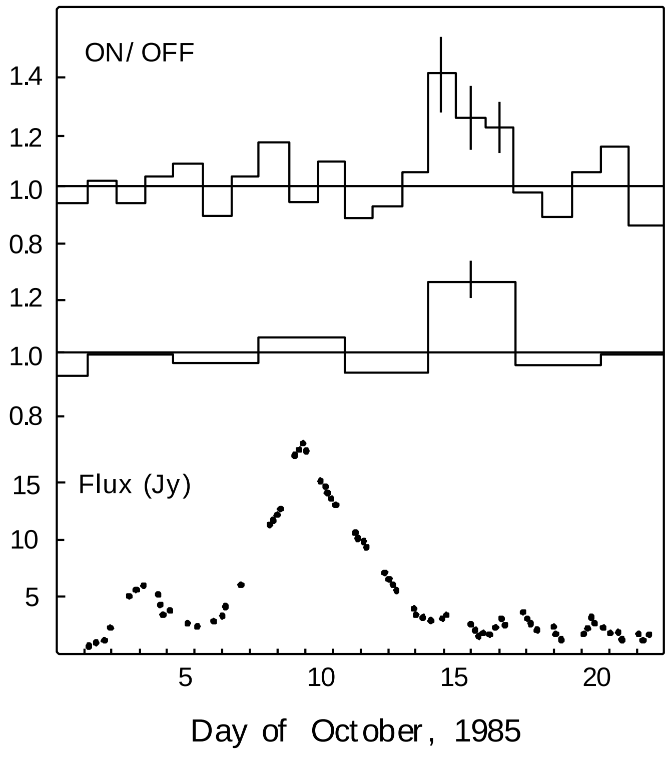

Figure 9. Baksan Cyg X-3 data and radio outburst flux in October, 1985.

If γ = 1.7 as for cosmic rays, the problem of detection is very severe. The slope γ being in between (1.3-1.4), it is worthwhile to try. In any case why should we believe that the spectrum from some exotic objects is flatter than for diffuse radiation ?

There is still a possibility that the flat spectrum from some favourable sources is not the spectrum of energies but the energy dependence of the ability of our instrumentation to fight the background. I believe that for the running UHE gamma ray detectors the search for the diffuse radiation from Milky Way should be performed.

8. CONCLUSIONS

1. We still do not have in VHE and UHE gamma ray energy ranges a steady candle in the sky, which is so important to encourage experimentalists and give them a practical possibility to check and test their instruments in angular and energy resolution.

2. If we believe that most of the potential sources are sporadic, we certainly need simultaneous measurements by independent installations. Unlike the case of the search for neutrino bursts from collapsing stars, the net of VHE-UHE gamma ray detectors should contain pairs of detectors separated by no more than several thousand kilometers to provide simultaneous measurements.

   My point is that not only giant installations like CASA project of Chicago-Michigan-Utah collaboration, certainly most promising, are of great importance, but also smaller ones, providing continuous observation and easy exchange of data.

3. As the prime and crucial evidence for VHE-UHE gamma ray source I consider the DC signal, not AC. In the case of UHE it means only the method of analysis. But in the case of VHE this means a new technique, - combination of tracking mode with a reliable background measurements.

4. A big muon detector is, of course, most complementary in the case of UHE gamma ray astronomy. The possibility of improving the signal to noise ratio in VHE gamma ray astronomy is not clear now.

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