HIGH ENERGY PHYSICS

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THE Twelfth International Conference on High Energy Physics was held at Dubna, U.S.S.R., from August 5 to August 15, 1964. The following is a brief survey of some of the interesting recent developments in particle physics against the background of this Conference.

Perhaps the most interesting new development reported at this Conference was the work of the Princeton group consisting of Christenson, Cronin, Fitch and Turlay. They announced the discovery of the 2π decay mode of K_2°, indicating a violation of CP invariance. They had over forty events of the type $K_2^\circ \to \pi^+ + \pi^-$, giving a branching ratio

$$\frac{K_2^\circ \to \pi^+ + \pi^-}{K_2^\circ \to \text{all charged modes}} \simeq 2 \cdot 0 \pm 0 \cdot 4 \times 10^{-3}.$$

Some additional experimental evidence in support of this observation was reported by an Illinois group. Various attempts have been made to maintain CP in spite of this observation, *e.g.*, one has raised the question as to whether unstable particles do really decay exponentially over time intervals that are very long compared to the mean lifetime, *e.g.*, if the decay rate of K_1° at large t is slower than the exponential rate valid at relatively small t, one might have a long-lived K_1° component interfering with the observation of K_2°. Another suggestion (Okun) is that K_2° may sometimes decay first into an as yet unobserved heavy neutral lepton X° of mass $\simeq$ 480 MeV ($K_2^\circ \to X^\circ + \nu$) and that this X° then decays according to $X^\circ \to \pi^\mp + \mu^\pm$, the $\mu^\pm$ being mistaken for a $\pi^\pm$. Some authors (*e.g.*, Bernstein, Cabibbo and Lee) have invoked a new kind of long-range vector interaction in order to explain the experimental observation. In a somewhat similar fashion, Lévy and Nauenberg have hypothesized the existence of a new extremely light neutral vector boson s of mass smaller than the $K_1^\circ - K_2^\circ$ mass difference, so that the decay $K_2^\circ \to K_1^\circ + s$ is possible. (Incidentally, one does not yet know definitely whether K_2° is heavier than K_1° or *vice versa*, though such experimental evidence as exists favours the former alternative.) This suggestion is reminiscent of the proposal for the existence of the neutrino, which was made to save angular momentum conservation in β-decay. The reasons *do not, however, appear to be as compelling,* and it seems best to take the experimental observation at its face value as implying CP violation.

If this interpretation is correct, several questions arise. Is CPT still valid ? If so, there must be a violation of time-reversal invariance, and in that case another time-honoured invariance principle will have been thrown overboard, in so far as weak interactions are concerned. One will of course search for other independent tests of both CP and T invariance in various weak interactions. There is also the possibility that it is CPT, and not necessarily T, that is violated. But then it is not clear how one could ensure the exact equality of the masses and lifetimes of particles and antiparticles. One may therefore ask if the masses and lifetimes of particles and antiparticles are indeed exactly equal, and so it is desirable to have more accurate experiments to test these equalities. Another very pertinent question is whether the small branching ratio for the 2π decay mode of K_2° implies a small or a large violation of CP. One asks this because one has been accustomed to a large violation of C and P, when they are violated at all. A note of caution is however sounded by Cabibbo who reminds us that non-leptonic decays of strange particles are not at all well understood, and that the decay $\Sigma^- \to n + \pi^-$ is nearly parity conserving. In any case, Sachs has suggested that the experimentally observed small branching ratio may be an indirect manifestation of a maximal CP violation in the leptonic interactions of K°. Such a leptonic mechanism, however, requires that the $\Delta S = \Delta Q$ rule be strongly violated too. In fact, "maximal violation" is then defined by the statement that the $\Delta S = -\Delta Q$ interaction is out of phase with the $\Delta S = \Delta Q$ interaction by $\pi/2$. Though there was supposed to be a substantial evidence in favour of a violation of the $\Delta S = \Delta Q$ rule a couple of years ago, more recent experiments are consistent with a rather small violation of this rule, and therefore the suggestion of Sachs does not seem to be in the right direction. Instead, Cabibbo proposes a modification of the non-leptonic part of the current, and suggests that the so-called second-class currents, usually supposed to be absent, are in fact present and are CP violating. One then gets a CP or T violation which is small for ordinary β-decay, including neutron decay, purely for kinematic reasons. To observe other T-violating effects one then has to study high Q-value β-decays, *e.g.*, β-decays of hyperons or K-mesons, or μ-capture, or high-energy neutrino interactions. Another interesting suggestion is that of Amati and of Truong, that only the $|\Delta T| = 3/2$ part of the decay interaction is CP-violating. It should be possible to test this suggestion by measuring the branching ratio

$$\frac{K_2^\circ \rightarrow \pi^+ + \pi^-}{K_2^\circ \rightarrow \pi^0 + \pi^0}.$$

One may remark on the 'philosophical' implications of CP violation. The overthrow of parity a few years ago came as a shock to many, to whom it was obvious on general grounds that there must exist mirror reflection symmetry in nature. It was possible to absorb this shock by following the suggestion of Landau and of Lee and Yang, that one may still have an extended mirror reflection symmetry in which 'particle reflection' or charge conjugation is carried out along with space reflection. This was the reason for the interest in CP invariance. Thus one proposed that there was a symmetry between the decay of a particle in one co-ordinate system and that of its antiparticle in the mirror system. Now if CP goes, and even if CPT is still valid, would one still insist that there is an extended symmetry with respect to mirror reflections ?

Before leaving weak interactions we must refer to another dominant theme in the current discussions of weak interactions, namely the introduction of the SU (3) idea into the theories of weak decays. Here Cabibbo's theory has been remarkably successful, especially in removing some of the difficulties in the way of Universal Fermi Interaction, presented by the leptonic hyperon decays. There is, however, no satisfactory understanding of the Cabibbo angle. We may also summarize the present status of some of the other conservation laws and selection rules that have served to bring order into the large body of experimental information in this field. μ-e universality continues to be fulfilled in leptonic decays of pions, K-mesons and hyperons. The conserved vector current hypothesis has been borne out by several experiments specially designed to test it. Doubts had been raised about the validity of the $\Delta S = \Delta Q$ and $|\Delta T| = 1/2$ rules at the time of the last conference, but more recent experiments have shown that there are no marked departures from these rules. One still does not have a satisfactory understanding of the $|\Delta T| = 1/2$ rule. On the current-current hypothesis, it can be understood rigorously only if one adds to the usual charged lepton currents some additional currents. The other possibility is that only charged currents are involved, but the $|\Delta T| = 1/2$ part of the interaction is somehow enhanced by strong interaction effects, *i.e.*, the rule depends on the dynamics of the decay processes. Along these lines, and within the framework of SU (3), Dashen, Frautschi, Gell-Mann and Hara have proposed an octet enhancement model, and have discussed ways in which the two possibilities may be distinguished experimentally. It may be mentioned here that fresh limits have been put on neutral currents by recent experiments on the branching ratios of K^+ : there is no event of the type $K^+ \rightarrow \pi^+ + e^+ + e^-$, implying that

$$\frac{\text{neutral currents}}{\text{charged currents}} \lesssim 2 \times 10^{-5}.$$

The intermediate vector boson continues to elude detection, and the present lower limit on its mass is 1·6 BeV.

During the last few years there has been a very substantial increase in the experimental information available on high energy scattering, thanks to the impetus provided by the predictions based on simple applications of the idea of Regge poles. It may be appropriate to list some of the questions about which one has been worried with regard to high energy cross-sections during the last few years: (1) What is the behaviour of σ_{tot} as $E \rightarrow \infty$? Do they really tend to a constant value as $E \rightarrow \infty$ as assumed by Pomeranchuk ? Have any of the total cross-sections, *e.g.*, σ_{tot} (pp), really reached a constant value already ? (2) The Pomeranchuk Theorems : Do the cross-sections of particles and antiparticles, as well as of members of an isospin multiplet, tend to become equal as $E \rightarrow \infty$? If so, how do they approach equality ? (3) What is the behaviour of σ_{el} ? Does σ_{el}/σ_{tot} tend to a constant value as in several optical models, or does it tend to zero as in a simple Regge pole picture based on dominance of the Pomeranchuk Regge pole ? (4) Does Re $f(o)$/ Im $f(o)$ tend to zero as $E \rightarrow \infty$, as has often been assumed in connection with the Pomeranchuk theorems ? (5) More strongly, does Re $f(o) \rightarrow 0$ as $E \rightarrow \infty$, as has often been assumed on the basis of simple models of diffraction scattering ? (6) What is the shape of the diffraction peak ? Is it energy-dependent, in particular shrinking with energy, as first suggested by Regge polology, or is it energy-independent as was believed earlier ? (7) Is the contribution of spin-dependent terms really negligible at high energies ? (8) More recently, there have appeared generalizations of Pomeranchuk theorems to differential cross-sections based on Phragmen-Lindelöf theorem. Are they obeyed by experimental cross-sections ?

The proton-proton total cross-sections were so far generally believed to be constant above 10 BeV, though analysis based on Regge poles had suggested that the constancy was only apparent, arising from the approximate cancellations of the contributions of different poles. At this Conference, however, Kycia from Brookhaven presented new and very accurate data (error $\sim$ 1%) on several total cross-sections, and showed that σ_{tot} (*pp*) is still falling slowly at 20 BeV. In fact, it falls by about 1·5 mb between 10 BeV and 20 BeV. σ_{tot} (K^+p) and σ_{tot} (K^+d), however, still seem to be constant in this energy interval. On the other hand, σ_{tot} ($\bar{p}p$), σ_{tot} (π^+p), σ_{tot} (K^-p), σ_{tot} (K^-d), and σ_{tot} (*np*) are all falling in this energy range. It is clear, as has been emphasized again and again, that one has to be cautious in applying simple asymptotic formulæ in this region.

The π^-d and π^+d total cross-sections have been found to be equal all the way up to 20 BeV, thus providing a good verification of charge symmetry. Using these together with the π^-p and π^+p cross-sections measured by them, the BNL group has proceeded to test the Glauber formula for the screening of one nucleon by another in the deuteron. They find that the Glauber formula fits the measured $\pi\pm p$, $\pi\pm d$ cross-sections very well, so that one can now apply it with confidence to find K^+n cross-section from K^+p, K^+d cross-sections and *np*, $n\bar{p}$ cross-sections from *pp*, *pd*, and $\bar{p}p$, $\bar{p}d$ cross-sections respectively. It then turns out that the total cross-sections for *np* and *pp* cross each other at about 4 BeV, and that the *np* cross-section approaches the *pp* cross-section from above. This approach from above agrees with one's naive ideas about the repulsive character of the contribution of ρ_0 to *pp* scattering. On the other hand, the crossing of *np* and *pp* cross-sections at about 4 BeV means that the explanation provided by Muzinich for the sharp peak observed by Palevsky *et al.* in forward elastic charge exchange *np* scattering at 2·04 and 2·85 BeV/c. in terms of a single ρ Regge pole, cannot be right: these energies are just not high enough to apply single Regge pole expressions. Incidentally, there have been more recent experiments on *np* elastic charge exchange scattering at higher energies, and the sharp peak observed by Palevsky *et al.* is seen to persist.

One may at this point refer to modified one particle exchange models proposed recently by several authors to account for the strong peaking in this reaction as well as in various other production reactions. The main feature of these models is that they avoid violation of unitarity in the low partial waves and include initial and final state interactions, usually as a strongly elastic scattering with a diffraction character. It is claimed that these corrections are capable of explaining the deviations from OPE customarily ascribed to phenomenological form factors. These models are, however, still rather crude, and it would be worthwhile to develop them further in view of the large mass of detailed experimental information that has now begun to be available on several production processes, especially on isobar production.

Accurate experiments on the angular distribution in the near-forward direction for $\pi^\pm p$ and *pp* scattering have been pushed to very small angles, with the result that one now has a strong evidence for a substantial real part in the forward scattering amplitude of all these processes. The fact that the interference of this real part with the Coulomb scattering amplitude is constructive for π^+p and *pp* and destructive for π^-p shows that the *real part corresponds to repulsion* in all these scatterings. The ratio $Re\, f(o)/Im\, f(o)$ seems to be of the order of $-0{\cdot}25$ for all these scattering amplitudes in the region of $\sim$ 10 BeV. One may add that the interpretation in the case of $\pi^\pm p$ is unambiguous but that in the case of *pp* is not so unique since the data could also be understood in terms of a substantial spin-dependent part in the amplitude. Spin dependence is moreover also indicated by the fact that polarization in *pp* scattering is dependent on energy. Lindenbaum believes, however, that $Re\, f(o)$ in *pp* scattering is a real effect and not just an apparent effect arising from the spin dependence of the amplitude. If the present results persist, $Re\, f(o)$ seems to be a *slowly varying function* of energy for both $\pi^\pm p$ and *pp*.

The experimental results on the real part have been compared with forward dispersion relations by Soding and Levintov *et al.* for *pp* and by Barashenkov for $\pi^\pm p$. The agreement is reasonably good, but in view of the importtance of such a comparison, one looks forward to a refinement of both the theoretical analysis and the experimental measurements.

There is no change in the situation with regard to the shrinkage of diffraction peaks except for the fact that there is now a substantial evidence for an expansion of the diffraction pattern for $\bar{p}$. pp and K^+p shrink. $\pi^\pm p$, K^-p do not shrink or shrink very little. The data could still be consistent with a model based on several Regge poles, but the comparison is no longer easy. In this connection one may mention a calculation by Igi which shows a strong energy dependence of the residue of the Pomeranchuk Regge pole.

There have been several attempts at empirical fits to the angular distributions at high energies. Guided by the optical model, Serber has suggested a scaling law for comparing the results for different incident particles. According to this scaling law, we are to compare the values of $\left[\frac{d\sigma}{dt} \Big/ \left(\frac{d\sigma}{dt}\right)_0\right]$ as functions of the momentum transfer measured in dimensionless units: $t' = (\sigma_{tot}/4\pi)\ t$. In terms of this dimensionless quantity, angular distributions of pp and $\bar{p}p$ are virtually identical for small t' and so also those of $\pi^\pm p$ and $K^\pm p$. Another empirical attempt worth mentioning refers to large angle scattering outside the diffraction peak. The suggestion is to express the angular distribution in terms of $p_\perp$ instead of t, and was first made by Sankaranarayana and Sarma. This kind of fit has been carried out more recently by Kirsch and Orear and they claim that $\ln\left(s\frac{d\sigma}{dt}\right) \propto p_\perp$ gives a straight line over a very large region—in which $d\sigma/dt$ changes by as many as ten decades.

The reason as to why $p_\perp$ should be the right variable in terms of which to discuss large angle scattering is not at all clear. Yang, in a letter to R. Wilson, has suggested a semi-classical picture in which the proton is regarded as an extended object, and the rapid drop of $d\sigma/d\Omega$ with $p_\perp$ is attributed to the difficulty of maintaining the proton intact in a scattering involving large transverse momentum transfer. In the same picture, eP scattering at large angles should drop roughly as fast as $\sqrt{(d\sigma/d\Omega)_{pp}}$ or the form factors G as $\sqrt[4]{(d\sigma/d\Omega)_{pp}}$. If one takes the right variable, *viz.*, total momentum transfer squared q^2, for describing this scattering, such a behaviour does not seem to be in violent disagreement with the Cornell data.

A by-product of the very accurate measurements on total cross-sections carried out by the BNL group has been the discovery of two more nucleon isobars, one in the $I = 1/2$ state at 2645 MeV and the other in the $I = 3/2$ state at 2825 MeV. These show up as tiny bumps with a height of only 0·3–0·4 mb. in the total cross-section curves for $\pi^\pm p$. There is no sign of any new structure in the K^-p scattering, except for a possible shoulder between 3 and 4 BeV/c. The bumps in total cross-sections being so small in this region, one would have to wait for them to show up in some specific reactions before being quite sure of their existence. In the meantime, of course, one has gone ahead to speculate about the Regge trajectories on which they should lie. In this way one gets impressive near-straight-line trajectories joining the four $T = 1/2$ particles lying at 938 MeV, 1688 MeV, 2190 MeV, and 2645 MeV, and the four $T = 3/2$ particles lying at 1236 MeV, 1924 MeV, 2360 MeV, 2825 MeV. What is even more striking is the fact that these two trajectories seem to more or less overlap each other.

As an experiment where one looks for a magnified effect produced by a small bump one may mention the work of Wahlig *et al.* These authors survey $\left(\frac{d\sigma}{d\Omega}\right)^{CEX}_{0°}$, the forward differential elastic charge exchange cross-section for π^-p, over the range 2·4 BeV/c to 18 BeV/c. The charge exchange scattering amplitude is proportional to the difference between the $I = 3/2$ and $I = 1/2$ amplitudes. Making use of the optical theorem one has

$$\left(\frac{d\sigma}{d\Omega}\right)^{CEX}_{0°} = \frac{1}{2}\left(D^- - D^+\right)^2 + \frac{1}{2}\left(\frac{k}{4\pi}\right)^2\left(\sigma^- - \sigma^+\right)^2$$

Since above 2·4 BeV/c, the difference $\sigma^- - \sigma^+$ is much smaller than the magnitude of each, the forward charge exchange cross-section should reflect resonances in this region with a much more favourable signal-to-noise ratio than would a total cross-section experiment.

The explosion in the population of resonances continues without any check in sight. The need for classifying them into multiplets on the basis of some symmetry scheme has therefore become all the more imperative. The SU (3) scheme has had some spectacular successes in this regard during the last couple of years. Here one thinks of the Gell-Mann Okubo mass formula, in particular for the spin $3/2^+$ baryon decuplet which led to the prediction and then the discovery of the Ω^-. Apart from this multiplet [which comprises of $N_{3/2}^*$ (1236), Y_1^* (1382), $\Xi_{1/2}^*$ (1529) and Ω^-(1675)], other known (or suspected) baryon multiplets are the spin $1/2^+$ octet (consisting of N, Λ, Σ, and Ξ), and the spin $3/2^-$ octet [consisting of $N_{1/2}^*$ (1518), Y_1^* (1660), Y_0^* (1680), $\Xi_{1/2}^*$ (1810)]. It is remarkable that all the known baryons can be accommodated within these multiplets, a few singlets [namely Y_0^* (1405), Y_0^* (1520)], and their Regge recurrences. We have already listed the possible Regge recurrences of N and N^* (1238).

Y_0^* (1815) could be a recurrence of Λ. There still remain a few baryons, whose existence has been suggested but not confirmed and which do not fall easily into the above pattern. In this category, the strongest candidate perhaps is the $1/2^+$ πN resonance at 1480 MeV, suggested by Roper. One watches with interest if this resonance, and a few others, do indeed establish themselves. There is also the $N_{5/2}^*$ (1560). Here an attractive suggestion due to Abers, Balázs and Hara is that the nucleon the $N_{3/2}^*$ (1236) and the $N_{5/2}^*$ (1560) are the first three members of a family of particles with $J = T = 1/2, 3/2, 5/2, \ldots\ldots$

While the classification of the baryons is thus reasonably satisfactory, the same cannot yet be said about the bosons. So perhaps one has to wait for the discovery of a few more bosons before they also fall into definite patterns. And the experimentalists promise to be quite obliging. The well-established boson multiplets are the pseudoscalar octet (π, η, K, $\bar{K}$), and the vector octet and singlet which mix with each other (ρ, ω, K^*, $\bar{K}^*$ and ϕ). Among the bosons whose multiplet assignments are not yet known, one has f^0 (1250), B (1215), κ (725), A_1 (1080), A_2 (1310), $\eta 2\pi$ (960), which are fairly well established, and a host of other candidates which await further confirmation. One may remark in passing that B (which decays first into ω and π), A_1 and A_2 (which decay first into ρ and π) and $\eta 2\pi$ (as also $N_{5/2}^*$) are instances of cascade decays of resonances which would not show up in two-body scattering or in two-body final state interactions.

The success of the mass formula for the SU (3) multiplets mentioned above, as also for the electromagnetic mass differences (Coleman and Glashow), is quite impressive. One also notes the success of the sum rule involving the decay amplitudes for the 3/2+ resonances (V. Gupta and V. Singh; also C. Becchi, E. Eberle and G. Morpurgo). One does not really understand why all these relations work so well. The scheme has no outright failures. One of the reasons for this is that, unlike the many previous schemes which have failed, it does not have built into it any absolute selection rules that would forbid processes allowed by isospin and strangeness selection rules. Moreover, symmetry breaking terms (however ill-understood) are an integral part of the scheme.

The successes of SU (3) accentuate a puzzle which has existed since its inception, though one chose to ignore it in the beginning. Why is it that nature does not make use of triplets? Or can it be that triplets do indeed exist but may have so far eluded detection? A revolutionary suggestion incorporating triplets, made independently by Gell-Mann and Zweig, endows them with fractional charges ($2e/3$, $-e/3$, where $-e$ is the charge of the electron) and a fractional baryon number. If they exist, they would constitute a new form of stable matter. There have been searches for these 'quarks' or 'aces', so far in vain. There are other alternative schemes involving triplets, which are conventional as far as charge or baryon number are concerned, but are endowed with a new quantum number C, which has been variously called 'triality', 'peculiarity', 'supercharge', 'charm', etc. For ordinary matter, $C = 0$. The triplets would then be stable, weakly decaying or strongly decaying, depending on the degree to which C is conserved. Schwartz has planned an experiment to look for possible stable triplets.

Encouraged by the successes of SU (3), several extensions to higher symmetries have been carried out and were reported at the Conference. The most interesting extension, however, seems to be the SU (6) scheme, which appeared after the Conference (Pais, Gursey and Radicatti; Bég and V. Singh).

Having seen that a particular symmetry scheme is so successful in providing a classification of the known particles, one naturally asks the question: what is the dynamics behind all this? In other words, how does this symmetry arise? and why not some other symmetry? The idea of the so-called bootstrap mechanism promises to provide an answer to this question. The bootstrap idea arose through the early work of Chew and Mandelstam on $\pi\pi$ scattering, where they noticed that the exchange of a ρ-meson was capable of providing the force necessary for binding two pions into a ρ-meson. The idea has since developed into a philosophy in which all the strongly interacting particles—whether stable ones or resonances—are regarded as being on the same dynamical footing, each a composite of all possible ones, bound together by the forces due to the exchange of all possible particles, including itself. None is regarded as more elementary than the others. The idea has a very considerable æsthetic appeal, especially in the face of the rapidly increasing population of particles, and has met with semi-quantitative success in predicting the parameters of several of the low-lying particle states. It has also been applied to the problem of predicting the octet of vector boson resonances in the scattering of a pseudoscalar boson octet by itself, or of predicting the octet and the decuplet in meson baryon scattering, etc. Through this kind of work, initiated by Capps, Cutkosky, and Martin and Wali, one hopes to understand how symmetries arise, and how they are broken. The tools one uses in these investigations are the analyticity properties of the S-matrix, and general properties like unitarity and crossing symmetry. However, at present, one is forced to introduce very drastic simplifying approximations in carrying out the programme, even in single channel problems, and all the more so in the multi-channel problem relevant to the question of symmetries. The semi-quantitative success achieved so far has therefore been very encouraging, and has stimulated considerable activity in this field. If these attempts succeed, one will have travelled quite far from one's naive conception of what elementary particles are.

CHEMICAL COMPONENTS OF THE *LOBARIA* LICHENS FROM THE WESTERN HIMALAYAS

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AMONG lichens, members of the genus *Lobaria* are generally found in abundance on the Himalayas at altitudes above 8,000 ft. They are foliose, fairly large in size and are readily recognised by the presence of white spots on a dark violet background when dry and olive-green when moist. They have been considered to be important in perfumery and tanning and have been used as vegetable drugs for the cure of eczema and for lung troubles.

In an earlier investigation[1] of the lichen *Lobaria isidiosa* from Darjeeling, it was found to contain a triterpene and thelephoric acid, a dark violet quinone pigment. Aghoramurthy, Sarma and Seshadri[2] used this lichen as a source of thelephoric acid for a detailed study of its constitution. Samples collected during the summers of 1953, 1958 and 1959 showed variation in their chemical components.[3] The presence of fairly good amounts of terpenoid compounds was noted in this lichen and four terpenoids A, B, C and D were recorded. This prompted us to study other samples of this and other *Lobaria* species collected mainly from the Western Himalayas. The terpenes isolated in these cases have been compared with the above four terpenoids.

Lobaria isidiosa

Of the two samples of *L. isidiosa* now examined, one was collected near Darjeeling (8,000 ft.) during the summer of 1961 while the other was collected in June 1962 from a pine tree near Ganghariya (10,000 ft.) in Western Himalayas. The general extraction procedure adopted was the same as described by Aghoramurthy *et al.*[4] The results are presented in Table I, and the details of separation and identification are given below.

TABLE I
Chemical components of L. isidiosa

Year and place of collection	Petroleum extract (Yield %)	Ether extract	Acetone extract
1961 Darjeeling ..	Wax and carotenoids	(i) Triterpene (D) (0·57%) (ii) Stictic acid (0·38%)	(i) Thelephoric acid (0·23%) (ii) Stictic acid (0·51%) (iii) D-Mannitol (0·20%)
1962 Ganghariya ..	,,	(i) Triterpene (D) (0·40%) (ii) Stictic acid (0·28%)	(i) Thelephoric acid (0·17%) (ii) Stictic acid (0·24%) (iii) D-Arabitol (0·17%)

Petroleum ether extract was concentrated and chromatographed on alumina. Elution with various solvents gave only wax and carotenoids with both lichen samples.

Ether extract was evaporated to dryness. Preliminary examination showed that it consisted of a phenolic and a non-phenolic compound separable with 80% acetone. Hence the mixture was repeatedly extracted with boiling 80% acetone. The extract on concentration yielded a colourless solid which on repeated crystallisation from 80% acetone gave stictic