MEASUREMENTS IN NUCLEAR EMULSIONS

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Most of the session to which this talk was intended to be an introduction has unfortunately evaporated because many of the colleagues whom we had hoped to welcome here were unable to come. Thus my subject has become less extensive, and I shall take the opportunity to give you some of my own opinions instead of the ones we shall not have the pleasure to hear today.

Let me go back thirty years, to the time when most physicists who worked in nuclear physics thought that the period in which emulsions were useful had come to an end. An example of that type of thinking was in a paper by H.J. Taylor[1] where it was said that:

> "In the writer's opinion this photographic method of investigating the ranges of fast particles involves inherent uncertainties which may give rise to large errors, and which may limit seriously its applications. It must be admitted, however, that this view is not held by those who have worked most on the subject. Frl. Blau and other workers appear to have regarded the method as capable of giving more accurate results than would seem justifiable if the considerations of the preceding paragraphs are valid."

In those days it was almost solely the Vienna group, under Marietta Blau, which continued to improve the technique, used it and maintained faith in it. It is a great pity that Miss Blau was unable to come to this conference to see the enthusiasm of her youth justified once more.

Since the days of that controversy very many things have happened; techniques and materials have improved, and quite a few important discoveries have been made with the aid of nuclear research emulsions. Yet, whilst the limits of what is possible have been pushed back a great deal since the pioneering work of Miss Blau and her collaborators, the causes of our difficulties are still the same. Nuclear emulsion is still an inhomogeneous medium, and it is still sensitive to humidity which causes fading and affects its density and shrinkage factor. The sensitivity of emulsion is still not a very well defined quantity. Our tracks, like those of Reinganum in 1911[2)], are subject to distortion that can be confused with scattering, and, like all the emulsion workers for more than fifty years, we have to be very careful in our processing if we are to obtain reproducible results and uniform sensitivity.

The word "reproducible" brings me to a point which is rarely made, perhaps because it is so obvious, but which is of great importance to all our work. It is that there are very few people indeed who know how to make nuclear research emulsions in large quantities in a reproducible fashion. That it is possible to obtain good results in such a messy medium is indeed a miracle, and we should never forget to give credit to the manufacturers, especially to the Ilford group of Dr. Waller and Mr. Vincent and, now, Mr. Ehrlich, and to Professor Bogomolov, who have succeeded in giving us a product of almost constant properties in which precision measurements can be made.

In all our work we rely on the Principle of Uniformity of Nature; that is, we believe that if the same experiment is performed several times in exactly the same way, then the results will be the same. However, I do not believe that there is also a Principle of Uniformity of Nuclear Emulsion, except by the grace of the manufacturers. Today there are physicists who have spent an appreciable part of their working lives investigating the properties of this medium. It is true that important results in physics have come out of such work, but I think it is useful to remind ourselves from time to time that nuclear emulsion is not a phenomenon of nature; it is a very powerful tool that we must apply to justify its existence.

Let me now come to the topics on which papers will be given. First there is the range-energy relation, which many of us took almost for granted even fifteen years or so ago. Others, fortunately, were more demanding and continued to investigate its dependence on emulsion composition, structure and the fundamental mechanisms involved. Professor Barkas and his group in particular have been most prominent and successful in this field and have made steady improvements in the precision with which the momentum of a particle can be established from measurement of its range in emulsion. Their methods, applied to the determination of the masses of unstable particles have provided us with the best available values for the charged K meson and the Σ^- hyperon, and discrepancies in this work led to their suggestion that there is a difference between the ranges of positive and negative particles of the same momentum. A paper in which experimental results on this difference are given will be presented at this session.

A related topic is the rate of energy loss in the extreme relativistic region. At the Munich conference, Professor Zhdanov's group reported that beyond the minimum of the energy-loss curve there is a maximum in dE/dx at a value of $\gamma = E/mc^2$ in the region of 100 to 300, predicted by Tsytovich[3]) and confirmed by Alekseyeva et al.[4]). Since then, a number of experimental groups have worked on this problem, for it has a bearing on the theory of energy loss as well as on measurements in nuclear emulsions, and we shall hear some of the results. The effect predicted by Tsytovich is not large, and to check it the rate of energy loss, or a quantity proportional to it, has to be measured with an accuracy of ±1% or better. Such precision has not been required in ionization measurements in emulsions up to now, and extreme care and many precautions are needed to reduce the effects of inhomogeneities in the emulsion and of variations in space and, in some experiments in time, of sensitivity and fading. It is well to realize that when one speaks of a 1% change in blob density, one speaks of ¼ blob in each 100 micron interval, a change that would be quite unnoticeable in the experiments in which one normally uses emulsions.

Extreme care and precautions cannot eliminate unsuspected sources of error, of which, it seems, some still remain. For example, the other day I was talking to Mr. Ehrlich of Ilford Limited, and I told him that we take great care to keep the emulsion packages well sealed, so that we can be sure that the water content is always that which corresponds to the relative humidity of 50% at which the emulsions are dried and packed at Ilford's. "Oh," said Mr. Ehrlich, "but we don't have the time to let the emulsions reach equilibrium ...". So the water content of the emulsions may vary from batch to batch, or even package to package and pellicle to pellicle. This is something I had not thought of before, and it seems to me that there may well be high-precision experiments in which one should let all the emulsions reach equilibrium at the same known relative humidity before they are used.

Then there is the question of temperature equilibrium. Emulsion is not a good conductor of heat, and it may take quite some time before a stack reaches temperature equilibrium. As far as I know, the temperature coefficient of sensitivity has not been measured with very high precision, but there is evidence to show that temperature non-uniformities affect results at the 1% level. Even if there are no temperature gradients, there still remains the possibility of variations in sensitivity from place to place, even in the same plate, and some years ago I heard of evidence that two regions only about 1 cm apart may exhibit detectable differences.

The next topic of interest is spurious scattering. At Munich, and at the meetings at Copenhagen and Lausanne, there was much discussion not only about the phenomenon itself but also about its definition. It now seems to be agreed generally that the mean second difference due to spurious scattering is defined by the relation

$$D_{ss}^2 = D_{corr}^2 - D_{th}^2$$

where D_{corr} is the observed mean second difference corrected for noise, distortion and other known spurious effects, and D_{th} is the mean second difference calculated from the theory. Thus spurious scattering is given by the difference between two quantities both of which are subject to some doubt. The first, D_{corr}, is affected by the level of distortion in the region of measurement, and by the method by which a correction is made for it. It is my personal opinion that, except in rare cases, the distortion corrections made by means of the usual methods are quite unreliable, for either they involve the assumption of a specific distortion contour (mainly the "C" shape), or the contour is determined by subjective comparison of the shapes of tracks in the same region of the emulsion. Thus it seems to me that the only way of obtaining good values of D_{corr} is to have emulsions with negligible distortion. Alternatively one may restrict attention to closely parallel neighbouring tracks and work with relative scattering measurements. The value of D_{th} one uses depends, of course, on the value of the scattering constant one chooses to adopt, and here, too, there is uncertainty; I shall return to that question a little later.

A quantity so ill-defined is difficult to discuss, and the best one can do may well be to determine its average value empirically and use it for correction. An alternative method, and perhaps a better one, is the one due to the Hungarian group, presented by Dr. Fenyves[5]. They divide spurious scattering into two components, one of which can be estimated by statistical techniques and used as part of the correction which leads to D_{corr}. In this way, the remaining "truly spurious" scattering is reduced, but an average correction may still be required for it.

Closely related is the problem of the value of the scattering constant. Some time ago, Hossain et al. here at CERN made measurements with very long cell lengths on tracks of particles of known momentum, and they found that the scattering was less than expected from theory[6]. Since then, several more experiments have been performed to find the behaviour of the scattering constant at long cell lengths, and the results are still conflicting. What is found is, essentially, that at very long cell lengths the spurious scattering becomes very small or imaginary (according to the definition I gave a while ago), and as consistently imaginary values seem absurd one has begun to suspect the theory. However, the situation is far from clear, and it may be that here, too, the answer will have to come from measurements of scattering which are free from the disturbing effects of emulsion distortion.

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