tion, and I hasten to reply to it.

I attended the session on "Women in Physics" at the APS Meeting in New York and took the liberty of expounding, in considerable length, some views I hold in this matter. I presume that Mrs Wood-Kyrala did not hear me, and read my remark "If I had been married to Pierre Curie, I would have been Marie Curie too" in PHYSICS TODAY, where it was quoted out of context, presumably precisely because of its shock value. To the extent that I created an opportunity for being misquoted, I indeed regret that statement by now.

The gist of my argument involving Mme Curie (or perhaps more properly, Sklodowska-Curie?) was as follows: Among umpteen other reasons, girls do not go into physics because they lack proper "images," whereas boys have a whole male galaxy to choose from; Marie Curie is the sole exception, et cetera, et cetera. This kind of "image" smacks of romanticism, I said. A better "image" is the inspiring, competent high-school physics teacher, and there are indeed too few women in that profession in the US. I said: "If you see Greer Garson as Marie Curie, you might want to become a physicist for two weeks; if you are inspired by your teacher, you might want to be a physicist for life." (The latter was true of me.) In short, I believe that there should be more inspiring high-school science teachers, in particular women.

I made several other points, but since they raise no objections, I need not discuss them here and now.

Upon re-reading Mrs Wood-Kyrala's letter carefully, I agree with her that intelligence is not transmitted by marriage. No doubt so will other attentive readers.

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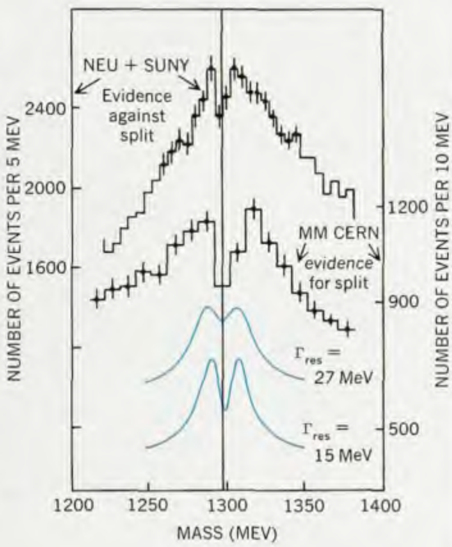
A_2 debate continued

Your "Search and Discovery" column perhaps inadvertently presented a somewhat one-sided report (July 1971, page 16) on the special session on the question of the split A_2 meson at the Washington APS Meeting. The PHYSICS TODAY readership deserves to be more fully informed of the factual situation in this controversy (a split A_2 means a breakdown of the quark model of bosons).

At the time of the Washington meeting there had been 24 investigations of the A_2 spectrum: 16 of them had reported a splitting, 8 not. At the session, the pros and cons of both kinds of experiments (split and non-split) were presented. However, only the *non*-split data were discussed in the July story and an earlier "Search and Discovery" story (April 1971, page 18) that dealt with the subject. Not even the existence of the opposing arguments was mentioned. Neither was the recent observation[1] of the split of the neutral A_2—the subject of an invited talk at that very session—mentioned. Yet this measurement is featured by more than an order-of-magnitude better statistics than the two non-split results to which an entire page was devoted in the April issue.

The one figure shown in July offered an incorrect comparison of the original CERN data with the data of the Northeastern–Stony Brook ("N-S") collaboration, which claims no structure.[2] The CERN observation of the split A_2-minus was made with good resolution, $\Gamma = 16$ MeV. But your figure compares the good resolution CERN data only with the poor resolution sample ($\Gamma = 21$ MeV) from the N-S experiment. Why was the good resolution N-S sample (claimed $\Gamma = 16$ MeV) not shown too?



The accompanying spectra of the A_2-minus meson shows the CERN data together with the best-resolution N-S data. The expected appearances of the split peak for different values of the instrumental resolution are also drawn. It can be easily shown that the N-S peak, presented[2] as the "evidence against structure," looks very much the way the split peak would look with a resolution greater than 22 MeV, rather than the 16 MeV claimed. Thus, the validity of the non-split result of the N-S experiment was challenged at the Washington session on the grounds that the resolution of the N-S spectrometer was at least 50% poorer than that of the CERN spectrometer. Since the latter was already marginal, the N-S experiment was unable to prove or deny the CERN result in spite of its higher statistics.[3]

It was also pointed out at the session

that the instrument of the N-S group had not demonstrated its ability to observe any other charged heavy meson, not even the ρ^-. The elastic π-peak appears to be 240 MeV wide in the spectrometer of the N-S group as opposed to 145 MeV in the CERN missing-mass spectrometer. A study[4] shows that the method of calculation[5] of the resolution employed by the N-S group gives an unrealistically narrow resolution probably because it arbitrarily sets to zero all cross-terms in the error matrix.

In addition it was pointed out that the poorer resolution in all three experiments that claimed no split is demonstrated by the fact that the widths of all the three non-split A_2's are unusually broad. The world average width of the A_2 is 90 ± 4 MeV. The N-S group lists the best-fit values of its width anywhere between 100 and 127 MeV.[2] The measurement of Foley *et al*[6] and Grayer *et al*[7] report non-split A_2's as 114 MeV and 123 MeV wide respectively.

Finally, it may be of interest to note that since the Washington session, in three out of the four most recent measurements the split in the A_2 is observed (Wisconsin,[8] Berkeley[9] and Rutherford[10,11] Lab).

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NU/SUNY comments: I would like to comment on the following points raised in Maglic's letter.

The large number of A_2 experiments that favor splitting. There are many low-statistics experiments that appear to find splitting—but these experiments also do not convincingly exclude the Breit–Wigner distribution. Nor is it clear that the apparent splits seen in these low-statistics experiments occur in the same place. These experiments would be significant if they could be unambiguously combined into a single distribution to determine if the dips add coherently or destructively. This procedure is not feasible because the experiments differ in the decay modes studied, backgrounds, resolution, and most likely in the calibration of the mass scale. The experiments vary considerably in their intrinsic ability to distinguish between a real effect and a statistical fluctuation. Thus to attach *a priori* equal significance to all experiments by simply counting those pro and those con is absurd. For example, the Northeastern-Stony-Brook experiment (NU/SUNY) contains 500 times the A_2's as one of the bubble-chamber experiment claiming to support the split, 15 times as many as the CERN Missing Mass experiment and five times the world's sample.

Two different studies have been made of the many A_2 experiments to determine their statistical reliability.[1,2] Both studies found, that to date:

- The only experiments that find a clear split and also convincingly reject the Breit–Wigner are the CERN MM experiments.
- All other experiments showing evidence for this split have marginal statistics.
- All the experiments that could reliably detect deviation from a Breit-Wigner found no deviation of the magnitude as originally reported.

Split in the neutral A_2. Although this experiment has ten times the number of total events as the K^-K^0 experiments, it detected only twice the number of A_2's, and in the peak region its statistical fluctuations were three times as large. This resulted from the large background present in the neutral spectrum and the lack of background in the K^-K^0 experiments. In addition, the mass resolution of the neutral experiment was 33% worse than the other two.

Reliability of the NU-SUNY mass resolution. The mass resolution of this experiment was measured from an analysis of elastic-scattering data and is described elsewhere.[3] Cross terms in the error matrix were justifiably set to zero, since ignoring them would only change the results by less than 5%. A 50% error as alleged by Maglic is ruled out by the observation of the width of the pion peak. It follows trivially from the missing-mass kinematics that the width of the pion peak, Γ_π, determines an upper limit on the A_2 mass resolution, Γ, by the relationship $\Gamma < m_\pi / m_{A_2} \cdot \Gamma_\pi$. For the NU/SUNY data with Γ = 16 MeV, the observed pion width of 150 MeV sets an upper

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limit of 19 MeV. (The 240-MeV pion width corresponds to the data taken with $\Gamma = 21$ MeV.) From Maglic's resolution curves it can be seen that a resolution of at least 27 MeV is required before the expected dip becomes 15% of the peak amplitude, which is the size of the apparent dip in the NU-SUNY good-resolution data.

The 145-MeV measured width claimed for the pion peak in the CERN-MM experiment produces an upper limit slightly less than the *calculated* resolution of 16 MeV. If the pion width is correct, then the 16 MeV must be incorrect or vice versa.

Furthermore, Maglic's assertion that the CERN-MM data shown in his letter were obtained with 16-MeV resolution is not supported by their publications.[4] The data have been combined from samples with resolutions of 16, 18 and 27 MeV with the 16-MeV data representing only about one fourth of the total.

Observation of the ρ^-. The NU-SUNY experiment has observed the π^-, ρ^-, and A^- but has not been able to duplicate the CERN MM group's early splitting of the ρ or their subsequent splitting of the A_2. However to test the apparatus for biases, measurements were made of the momentum transfer dependence of the elastic cross section and good agreement obtained.

Use of the A_2 width to test mass resolution. From Maglic's graph showing both the CERN and the NU-SUNY data it can be easily verified that the full width at half-maximum amplitude above background are identical for both experiments. Thus any discrepency in width arises from the procedure used to extract a width from the data, for example, how the backgrounds are drawn, the form of the resonance formula used, and the interval fitted.

So far there has been no opportunity for serious discussion of two other possible sources of A_2 splitting (or lack of it) —systematic errors in the detection apparatus and nonobjectivity in the data-selection procedures. Until these aspects of the experiments receive a fuller and more rational discussion there is little hope of resolving the A_2 controversy.

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What's going on in physics?

The article "Keeping up with what's going on in physics" (November 1971, page 23) is both very encouraging and very depressing. It demonstrates once again that physicists appear to have only one mode of response to problematical situations—the technological reflex.

What's going on in physics today? Certainly one of the most pervasive processes to be observed relates to the employment situation, which can with little difficulty be traced to the images that physicists have of themselves and the images that society as a whole has of physicists. *There is a real revolution taking place in science education, yet few physicists are at all aware of what is happening and what it means to the future of physics as a profession.*

Consider Table 1 on page 27 of the above article. The only journal in the list that even presumes to relate to science education is *American Journal of Physics*. Yet even this journal devotes itself mostly to the technological twitching of physics educators in distress. The focus is almost exclusively on more sophisticated ways of presenting the content of physics to an audience whose characteristics are unknown and of no concern to the writer. In particular the emotional response of a student seems never to be a matter for concern. Yet the future of physics will depend heavily on this kind of concern.

Thus I conclude that the sophisticated information system is totally irrelevant to all physicists whose driving concern is understanding and meeting the needs of today's children and tomorrow's physics teachers of physics in kindergarten through college. Yet the future of physics depends critically on these physicists knowing what's going on in physics.

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The author comments: Paul Smith is perfectly right in criticizing my title; I should have said "Keeping up with what's going on in physics research." I, like so many physicists, unconsciously associate the word "physics" with the subject matter of our discipline. And, of course, as I took pains to point out, when the planning for the American Institute of Physics new information program was initiated (in the mid 1960's), the dissemination of research information seemed the most pressing problem. I do not think that this problem is any less pressing today, but rather, that other problems have become much more pressing; one of these is pointed out by Smith.

I also agree with Smith that technol-