Alan Weinstein
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Quick Study of the D^{*+} Width

In early April, Ian Stewart, a student of Mark Wise's, gave a talk at Caltech on his predictions for the D^* width from chiral perturbation theory[1]. He showed that a good measurement of the D^* width provides a powerful test of chiral perturbation theory. He noted that the best limit on the D^{*+} width was 131 keV, from ACCMOR, based on 110 events. He asked me what the CLEO result was, and I was very embarrased to not have a good answer, so I started looking into it (knowing nothing about what is going on in CLEO on this front).

I worried that the extraction of the D^* width would be difficult if the detector resolution was at all uncertain (which it of course is). I wondered if the shape of the D^* peak would be sufficient to distinguish the natural width from the non-Gaussian tails in the detector resolution, especially in the presence of background. Unfortunately, after playing around, I concluded that the Breit Wigner tails (for the relevant range of Γ and σ) were not big enough to warrant optimism; see Figure 1.

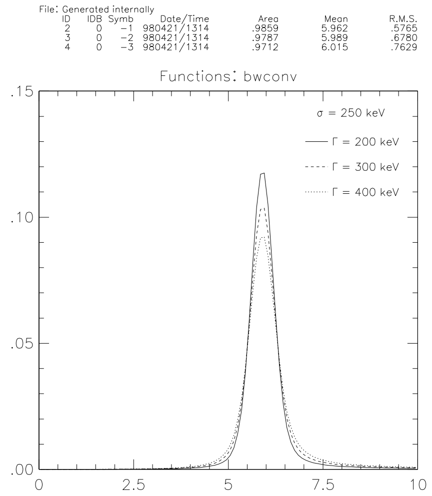

Figure 1: The quantity $M(D\pi) - M(D) - M(\pi)$ in MeV/c^2, modelled as a convolution of a simple Gaussian (with $\sigma = 0.25$ MeV) with a Breit-Wigner with a P-wave mass-dependent width of pole values $\Gamma(M(D^*))$ of 0.2, 0.3, and 0.4 MeV.

A few days later, Harry Nelson advertised David Asner's APS talk transparencies[2] which included a very nice $D^* - D$ peak from CLEO-II.V. I promptly digitized it and fitted it to a convolution of a D^* Breit Wigner (with a mass dependent width) and a single Gaussian (clearly a poor approximation to the actual resolution). The results are shown in Figure 2.

I conclude that the mass could be determined to better than 5 keV (statistical only!), and the width could be determined to something like 20% of itself.

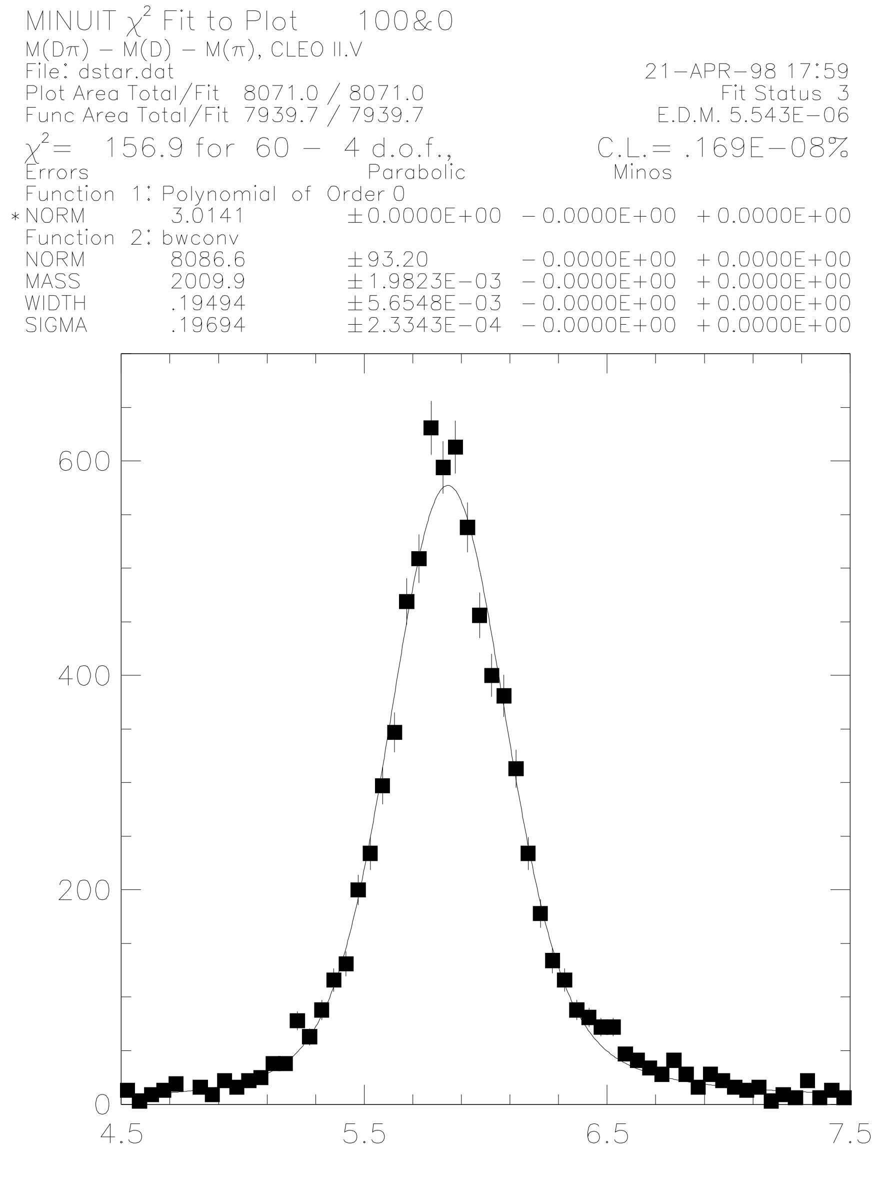

Figure 2: Results of a fit of the CLEO II.V data for $M(D\pi) - M(D) - M(\pi)$ in MeV/c^2 (courtesy of Dave Asner and Harry Nelson), to a convolution of a D^* Breit Wigner (with a mass dependent width) and a single Gaussian.

Some discussion:

- The determination of the width depends critically on our understanding of the detector resolution. Our absolute uncertainty on the CLEO resolution feeds directly into the systematic error on the width.

- I know of no clean way to determine the CLEO resolution in this momentum range, using the data. However, the Kalman error matrix is expected to be much better than pre-compress, and the CLEOG simulation of our resolution has been pretty damn good in pre-compress CLEO II.

- Thus, a better approach would be to do an unbinned maximum likelihood fit, using an event-by-event gaussian sigma as determined by the Kalman error matrix.

- Further improvement could be obtained by modelling the deviations from the Kalman prediction using the CLEOG monte carlo. Systematic errors can be evaluated conservatively by scaling the width of the MC-determined response function by, say, 20% of itself. I have set up a fitter that can do that, working off of a histogram of the detector response.

- I have no idea what cuts went into the plots Dave Asner showed at APS. however, it is clear that we are not lacking in statistics. Thus, one could reduce systematics by cutting hard on track quality of the slow pion.

- As for the mass, or more precisely, $M(D\pi) - M(D) - M(\pi)$, the error on the PDG96 average is 40 keV. Asner's plot is sufficient statistics to acheive a statistical error of as low as 5 keV. Of course, our result will depend entirely on our systematic error on the slow pion momentum. I imagine that it will be a challenge to get the systematic error below 50 keV!

References

[1] I. Stewart, hep-ph/9803227.

[2] D. Asner, http://hep.ucsb.edu/people/hnn/wrongd/ .