Search for $K_L \to \pi^0\pi^0\mu^+\mu^-$ in the KTeV data

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A search for $K_L \to \pi^0\pi^0\mu^+\mu^-$ in the 1997 KTeV data with a sensitivity orders of magnitude better than would be needed to see the new physics suggested by the recent *HyperCP* $\Sigma^+ \to pe^+e^-$ result fails to observe the decay.

Introduction

In early 2005[1] the *HyperCP* collaboration announced the observation of $\Sigma^+ \to p\mu^+\mu^-$ with three unusual events. In the standard model, the decay proceeds through an off-shell γ, and the dimuon system does not have a mass resonance. However, all 3 of the *HyperCP* events had the same (214.3MeV) mass, to within the experimental resolution of ~0.5MeV. This outcome is unlikely; it is estimated to occur at only the 0.8% confidence level. Additionally, the *HyperCP* collaboration found that, using a detector acceptance as calculated for the standard model diagrams and form factors consistent with existing limits on $Br(\Sigma^+ \to pe^+e^-)$, the observed $Br(\Sigma^+ \to p\mu^+\mu^-)$ was higher than expected, albeit at about the 1σ level. They note that these anolomies could be explained by a new physics process mediated with a neutral boson (P^0) of mass 214.3MeV. Such a boson would be capable of changing quark flavor, at least for s to d transitions, but need not change lepton flavor. Should such a process exist,

$$Br\left(\Sigma^+ \to pP^0, P^0 \to \mu^+\mu^-\right) = \left[3.1^{+2.4}_{-1.9} \pm 1.5\right] \times 10^{-8},$$
$$\Gamma\left(\Sigma^+ \to pP^0, P^0 \to \mu^+\mu^-\right) = 2.5 \times 10^{-19}\ MeV\,.$$

A simple argument highlights, at least qualitatively, the relevance of the modes $K_L \to \pi^0\mu^+\mu^-$ and $K_L \to \pi^0\pi^0\mu^+\mu^-$ in determining if this result is indeed due to new physics. For a pointlike P^0, the $\mu^+\mu^-$ pair must materialize with no orbital angular momentum; then (as fermion and antifermion have opposite parities) this new boson must have $J^{(P)}$ of either $0^{(-)}$ or $1^{(-)}$ provided that the new interaction conserves parity.

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[1] FNAL Wine & Cheese, 21 Jan 2005; PRL **94**, 021801, the same date

For the vector boson case, the new particle should appear in the $0^{(-)} \rightarrow 0^{(-)} \oplus 1^{(-)} \oplus 1^{(+)}$ process $K_L \rightarrow \pi^0 P^0$, where the $1^{(+)}$ term is the contribution from orbital angular momentum. When followed by $P^0 \rightarrow \mu^+\mu^-$, the $J^{(P)} = 1^{(-)}$ case means that the new particle should contribute to $K_L \rightarrow \pi^0\mu^+\mu^-$. For the pseudoscalar case, the new particle should appear in the $0^{(-)} \rightarrow 0^{(-)} \oplus 0^{(-)} \oplus 0^{(-)}$ s-wave process $K_L \rightarrow \pi^0\pi^0 P^0$, again followed by $P^0 \rightarrow \mu^+\mu^-$; in other words, the new particle should contribute to $K_L \rightarrow \pi^0\pi^0\mu^+\mu^-$ (and also to $K_L \rightarrow \pi^+\pi^-\mu^+\mu^-$).

The two-body phase space for $\Sigma^+ \rightarrow pP^0$ is 5.00 x10^{-6} and so if the matrix element is a constant, its magnitude is 2.79 x10^{-7}. The phase space for $K_L \rightarrow \pi^0 P^0$ is 17.95 x10^{-6}, so the new physics would make a contribution to the partial width of $K_L \rightarrow \pi^0\mu^+\mu^-$ of 2.18 x10^{-21} MeV; that would be a branching ratio of 1.72 x10^{-4}. The existing *KTeV* limit is $Br(K_L \rightarrow \pi^0\mu^+\mu^-) < 3.8$ x10^{-10} at the 90% C.L. Similarly, the phase space for the three body decay $K_L \rightarrow \pi^0\pi^0 P^0$ is 33.36 x10^{-6}, and new physics with a constant matrix element corresponds to the rather large $Br(K_L \rightarrow \pi^0\pi^0\mu^+\mu^-) =$ 3.19 x10^{-4}.

Recently, two groups[2] have calculated branching ratios for $K_L \rightarrow \pi^0\pi^0 P^0$, $P^0 \rightarrow \mu^+\mu^-$ without assuming parity conservation, and using more realistic matrix elements. The later effect is important[3]. Both groups find that pseudoscalar couplings have the largest contribution, and that $Br(K_L \rightarrow \pi^0\pi^0\mu^+\mu^-) \approx 8$ x10^{-9}. A third group[4] has obtained a higher result of $Br(K_L \rightarrow \pi^0\pi^0\mu^+\mu^-) \approx 6$ x10^{-8}. This note describes a search with the 1997 *KTeV* data with a single event branching ratio sensitivity of 1.4 x10^{-10}. A search for $K_L \rightarrow \pi^+\pi^-\mu^+\mu^-$has yet to be attempted, but is quite suppressed compared to $K_L \rightarrow \pi^0\pi^0\mu^+\mu^-$; the phase space is smaller by a factor of 10, and the branching ratio is predicted to be smaller by about a factor of 5.

There is no calculation within the standard model for $Br(K_L \rightarrow \pi^0\pi^0\mu^+\mu^-)$, but the diagrams are similar to those of $K_L \rightarrow \pi^0\pi^0 e^+e^-$. There is an estimate[5] for $Br(K_L \rightarrow \pi^0\pi^0 e^+e^-)$ of ≈ 2 x10^{-10}; the four-body phase space suppression is a factor of 3480.

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[2] X-G. He, J.Tandean and G.Valencia, Phys. Lett. **B631** (2005) 100 and N.G. Deshpande, G.Eilam, and J.Jiang, eprint `hep-ph/0509081`.

[3] N.G. Deshpande, G.Valencia; private communications.

[4] D.S. Gorbunov and V.A. Rubakov, , eprint `hep-ph/0509147`.

[5] P.Heiliger and L.M. Sehgal, Phys. Lett. **B307** (1993) 182.

Analysis summary

The data taken in 1997 E799 running, with trigger 7,

2V*DC12*2MU3*PHVBAR1*2HCY_LOOSE*HCC_GE1

were used, corresponding to[6] about 2.68 x10^{11} K_L decays.

Reconstruction was straightforward. Events with two vertexable tracks of 7GeV or more and associated hard or soft clusters below 2GeV, and exactly 4 hardware clusters away from tracks were initially retained, provided that the hardware clusters formed a pair of π^0s both of which are within 15MeV of the PDG π^0 mass, using the charged vertex. These selection requirements brought the data set down to a very manageable size and would make a good crunch algorithm for the 1999 data.

The distribution of the signal Monte Carlo events in the ($P_\perp$, $M_{\pi\pi\mu\mu}$) plane is shown in Figure 1. The criteria $P_\perp <$ 0.1 – 2 |$M_{\pi\pi\mu\mu}$- 0.5|, with units of GeV, defines the signal box and includes a bit more than 99.8% of the signal. Data events outside this region are defined as background.

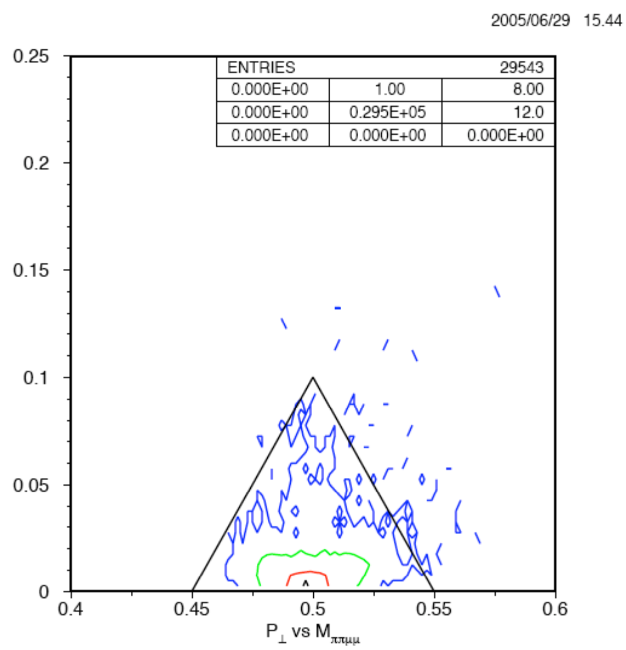


Figure 1. The distribution of the signal Monte Carlo events in

[6] This number is from Sada's thesis, which is our published $K_L \rightarrow \pi^0\mu^+\mu^-$ result.

the ($P_\perp$, $M_{\pi\pi\mu\mu}$) plane after initial reconstruction criteria.

Following examination of the reconstructed π^0 mass in the signal Monte Carlo events, the initial requirement of 15MeV was tightened to 9MeV.

The distribution of the variable KM3KIN, defined as

$$\frac{\left(M_K^2 - M_{\pi\mu}^2\right)^2 - 4M_K^2 P_\perp^2}{P_\perp^2 + M_{\pi\mu}^2}$$

is shown in Figure 2 for the signal Monte Carlo and for the data outside of the signal region. This quantity is the square of the longitudinal momentum of the neutrino, under the hypothesis that the charged particles are from $K_{\mu3}$, in the frame where $P_{//}$ of the charged particles is zero. In computing KM3KIN, the track of higher momentum was taken to be the muon, as is typically the case in $K_{\mu3}$ events with a tagged muon.

Figure 2 shows a very clear separation between simulated signal events and background data events. In the signal events, there is a very small Δm for the decay; $P_\perp$ is forced to be quite small and $M_{\mu\pi}$ is dominated by the assigned masses for the μ,π hypothesis. In the data, at least two background sources are visible in a bimodal distribution. Requiring KM3KINE to be over 0.45GeV leaves only 9 background events.

Figure 3 shows the distribution of reconstructed dimuon masses. The blue lines show a band of ±0.6MeV, which is about ±3σ of the Gaussian width of the

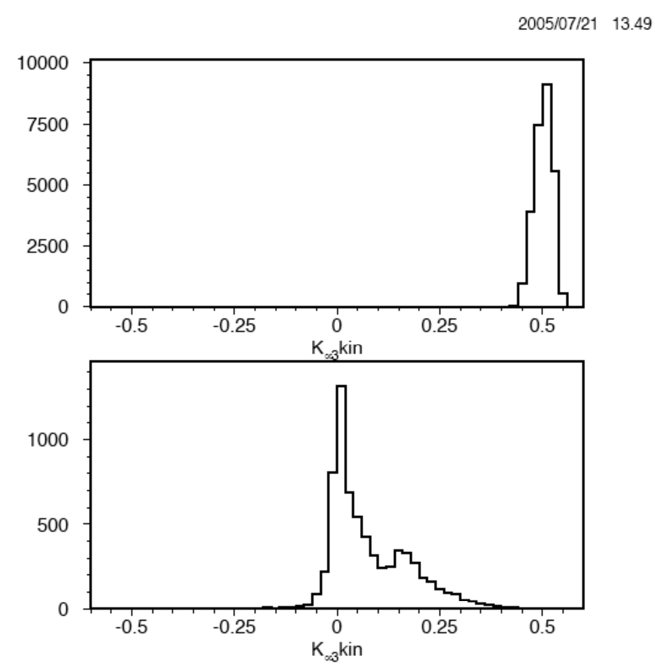


Figure 2. The distribution of KM3KIN in the signal Monte Carlo events and the data events outside the signal region

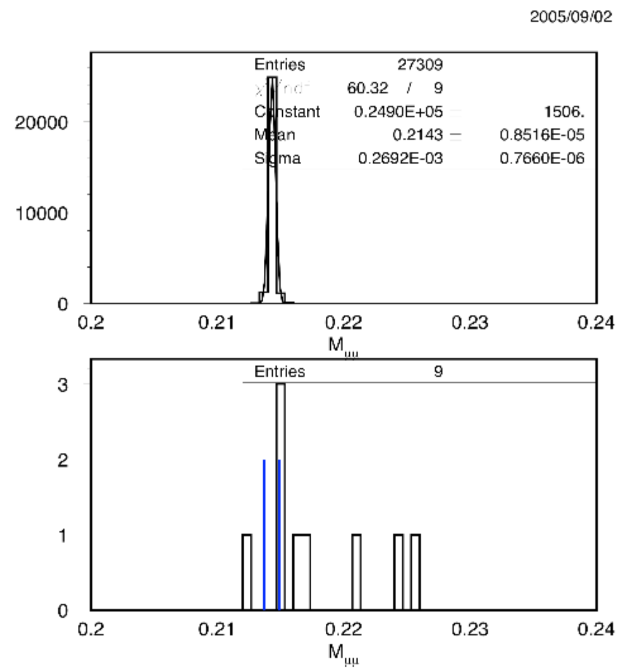


Figure 3. The dimuon mass distribution following the KM3KIN selection requirement, in signal Monte Carlo and background data.

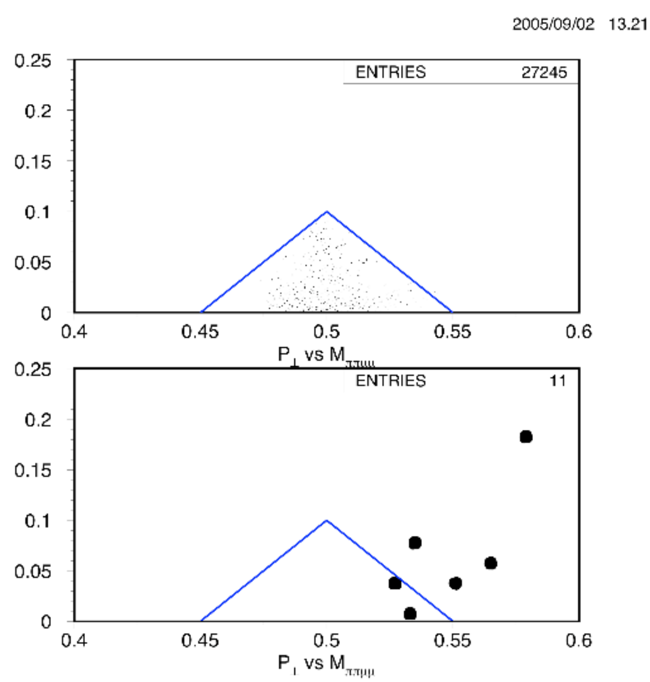


Figure 4. The distribution of simulated signal and data events in the ($P_{\perp}$, $M_{\pi\pi\mu\mu}$) plane after all selection criteria. There are five additional events outside and to the upper right of the data plot.

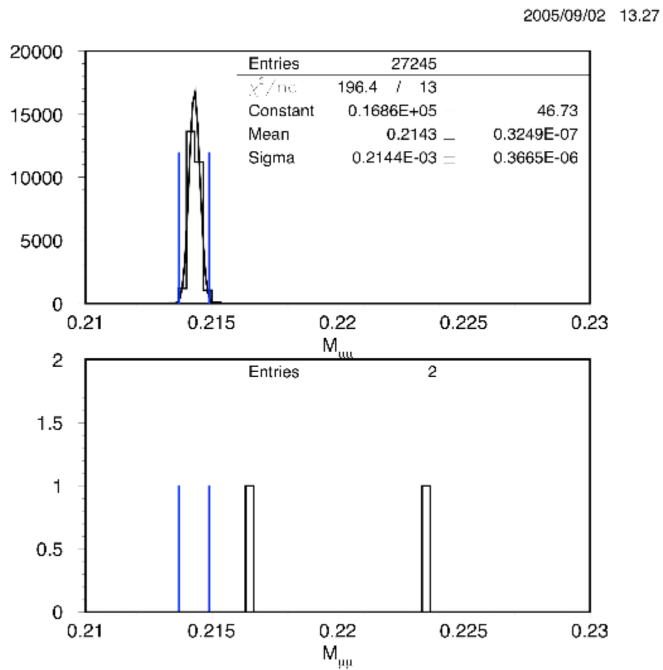


Figure 5. The dimuon mass distribution following all selection criteria.

simulated signal resonance. With this cut, the acceptance is about 2.73%, corresponding to a single event sensitivity of 1.4 $\times 10^{-10}$ in branching ratio, or 1.8 $\times 10^{-24}$MeV in Γ. As this is nearly 5 orders of magnitude below the hypothesized *HyperCP* level, the speculated-upon new physics should be quite unmistakeable.

Figure 4 shows that there are only two events in the signal region, and Figure 5 shows that they are not at 214.3MeV. We do not have a quantitative background estimate, but at this level of background the assumption that there is no background, while conservative, is not wholly unjustified. With no background and the estimated acceptance, the signal is less than 2.3 events, so at the 90% C.L., the partial width for the postulated new physics is less than 4.0 $\times 10^{-24}$ MeV.

The astute reader will have noticed a number of shortcuts taken in this analysis. The background has neither been quantified nor even clearly identified. No normalization analysis has been undertaken, and there is no analysis of systematic uncertainties in the sensitivity. Indeed, a few blunders that would cause the sensitivity to be incorrectly estimated by a percent or two of its value are known to remain in the calculation. Notwithstanding, the new physics scenario suggested by the *HyperCP* study is clearly ruled out.

Appendix. Details of the analysis: My notes.

Notes re HK's "observation":

By parity conservation he argues that P^0 should not appear in $K^+ \rightarrow \pi^+\gamma\gamma$, but might be in $K^+ \rightarrow \pi^+\pi^0\gamma\gamma$; this is relevant in light of the E787 result on $K^+ \rightarrow \pi^+\gamma\gamma$. However, it seems best to stick with $\pi\pi\mu\mu$ channels to start with, as this is where the anomaly was observed.

I stick with v6.00 of KTeVMC. Looks like JAWS is doing that too. I should set CDSERV to either /cdserv/hepdb_799_99 or /cdserv/hepdb_799 (my current default) I'll just try the 1997 data first. Looks like there was a trigger change in B05:

1997: 2V*DC12*2MU3 *PHVBAR1*2HCY_LOOSE*HCC_GE1
1999: 2V*DC12*2MU3_LOOSE*PHVBAR182HCY_LOOSE*HCC_GE2

where 2MU3 = 2+ hits in MU3X and 2+ hits in MU3Y but 2MU3_LOOSE is 1+ hit is MU3X with 2+ hits in MU3Y or the opposite combo: 2+ in MU3X and 1+ in MU3Y.

I take some samples from ~jaws/ktev/pi0mm/ana area. But I cannot find anything done to KTeVMC to allow for better muon simulation. Should it be there? Wasn't there some issue with punchthrough being simulated in L3 but it should be simulated earlier? Looks like the PIMUDK bug is fixed in the official version though.

I do the simulation by pirating the old kp0hdk deck in the DECAY patch.

Made an mrn file for 1997 from my old "six" files, scaled by Peter Mikelson's fluxes. This is incorrect, at least, in that it requires TRD-good runs only and this analysis won't use the TRDs at all. Later, I discover mrn files in Julie's area that she got from Jason Ladue. They are more likely to be correct, and I should check them out for the next generation. Took L3 namelist file from the same place… this is more likely to be correct.

Also, I allowed for exactly 4 hard clusters other than the clusters associated to the tracks. There will be some acceptance loss due to high energy in-time accidentals.

If analysis of 99 data is needed, Julie has the following lines in her code:

```
C JAWS: For E799_99
C The trigger latch information was lost during data taking
C Recover it using TDC information
C
         if ((inrun.ge.15220.and.inrun.le.15221).or.
     +        (inrun.ge.15268.and.inrun.le.15322)) then
            CALL TDCTOL1SRC
         endif
```

Plus also the routine TDCT0L1SRC, called here.

She has three different versions of MUMATCH; the one that I try is called MUMATCH2. Also, she has two different versions of MULAT; the one that I try is called MULAT2.

CSI799: Cluster and block thresholds changed – Julie says that this kills hot bits in the early 97 data. In my signal MC, with new thresholds, have 29543 signal after KTeVana, with energy means 0.4410 and 0.4401GeV; with old thresholds, have 29634 signal after KTeVana, and energy means of 0.4368 and 0.4363GeV. I guess the 0.3% loss in efficiency might be a good insurance.

For running on data, make sure the outfile is defined and that WRITEOPT is 3. Hafta figure out where the data is too I guess.

Trigger 5, the 2MU_LD trigger, is
GATE * 2V * DC12 * 2MU3 * PHVBAR1 * 2HCY_LOOSE * HCC_GE1
The 2MU_SD trigger was B06 in the winter,
GATE * 2V * VEWUD * 2MU3 * DC12 * !HA_SUPERHI * 2HCY
but in the summer it was changed to 2MU-LOOSE,
GATE * 2V * DC12 * 2MU3_LOOSE * PHVBAR1 * ET_THR1 //
                * 2HCY_LOOSE * HCC_GE1 : PS 1/5

Now ~offline/doc/tape_prefix.txt says 2MU_SD went on tape series NZS### for winter of 97 only; and from ~offline/tapeinfo/alltapes, the NZS series starts at run 8027 on tape NZS001 and goes up to run 8913 on tape NZS043.

The 2MU-LD ($\mu\mu\gamma$) crunch went onto NZL tapes, 001-066 for the winter data; 067 - 130 for the summer. Here is a table of their contents:

NZL001	8028	8128
NZL002	8129	8138
NZL003	8138	8180
NZL004	8181	8205
NZL005	8205	8223
NZL006	8223	8255
NZL007	8255	8268
NZL008	8268	8282
NZL009	8282	8283
NZL010	8285	8291
NZL011	8291	8327
NZL012	8327	8333
NZL013	8333	8354
NZL014	8354	8359
NZL015	8359	8364
NZL016	8364	8367
NZL017	8367	8370
NZL018	8371	8387
NZL019	8387	8397
NZL020	8397	8428
NZL021	8429	8443
NZL022	8443	8453
NZL023	8453	8473
NZL024	8473	8484
NZL025	8484	8504
NZL026	8504	8509
NZL027	8509	8518
NZL028	8518	8564
NZL029	8564	8567
NZL030	8568	8571
NZL031	8573	8576
NZL032	8576	8580
NZL033	8580	8584
NZL034	8586	8593
NZL035	8596	8607
NZL036	8607	8608
NZL037	8608	8613
NZL038	8613	8626
NZL039	8626	8628
NZL040	8628	8634
NZL041	8634	8671
NZL042	8671	8674
NZL043	8674	8678
NZL044	8678	8690
NZL045	8690	8697
NZL046	8697	8707
NZL047	8707	8722
NZL048	8722	8732
NZL049	8732	8746
NZL050	8746	8747
NZL051	8748	8750
NZL052	8750	8754
NZL053	8755	8769
NZL054	8769	8771
NZL055	8771	8773
NZL056	8773	8777
NZL057	8789	8792
NZL058	8792	8798
NZL059	8798	8821
NZL060	8822	8880
NZL061	8880	8889
NZL062	8900	8902
NZL063	8902	8907
NZL064	8907	8908
NZL065	8909	8913
NZL066	8913	8913
NZL067	10460	10482
NZL068	10482	10493
NZL069	10493	10540
NZL070	10541	10544
NZL070	10541	10544
NZL071	10544	10550
NZL072	10550	10552
NZL073	10552	10558
NZL074	10558	10563
NZL075	10563	10567
NZL076	10567	10590
NZL077	10590	10593
NZL078	10593	10594
NZL079	10594	10601
NZL080	10601	10602
NZL081	10602	10608
NZL082	10608	10610
NZL083	10610	10612
NZL084	10612	10619
NZL085	10619	10625
NZL086	10625	10635
NZL087	10635	10638
NZL088	10638	10643
NZL089	10643	10644
NZL090	10644	10649
NZL091	10649	10657
NZL092	10657	10658
NZL093	10658	10659
NZL094	10659	10664
NZL095	10664	10666
NZL096	10666	10672
NZL097	10672	10673
NZL098	10679	10682
NZL099	10682	10686
NZL100	10686	10703
NZL101	10704	10704
NZL102	10704	10706
NZL103	10706	10707
NZL104	10710	10716
NZL105	10716	10719
NZL106	10720	10720
NZL107	10721	10727
NZL108	10728	10732
NZL109	10732	10732
NZL110	10732	10736
NZL111	10736	10757
NZL112	10764	10769
NZL113	10769	10797
NZL114	10798	10819
NZL115	10819	10825
NZL116	10825	10896
NZL117	10896	10923
NZL118	10923	10933
NZL119	10933	10937
NZL120	10937	10937
NZL121	10937	10947
NZL122	10947	10948
NZL123	10948	10950
NZL124	10950	10950
NZL125	10950	10952
NZL126	10952	10957
NZL127	10959	10960
NZL128	10960	10970
NZL129	10970	10977
NZL130	10978	10978

Looks like Sada did a crunch onto tapes labeled NQDM01 - 44 for the entire 1997 data. Then a second pass, NQDM51-72, possible for the normalization mode?

In Julie's area of enstore, there are NQMM01-NQMM11 ; NQMM20-NQMM25 ; UPMM01-UPMM27 ; UPM001-UPM059. Looks like maybe UPMM, UPM0 are for 1999 data, NQMM series is 1997.

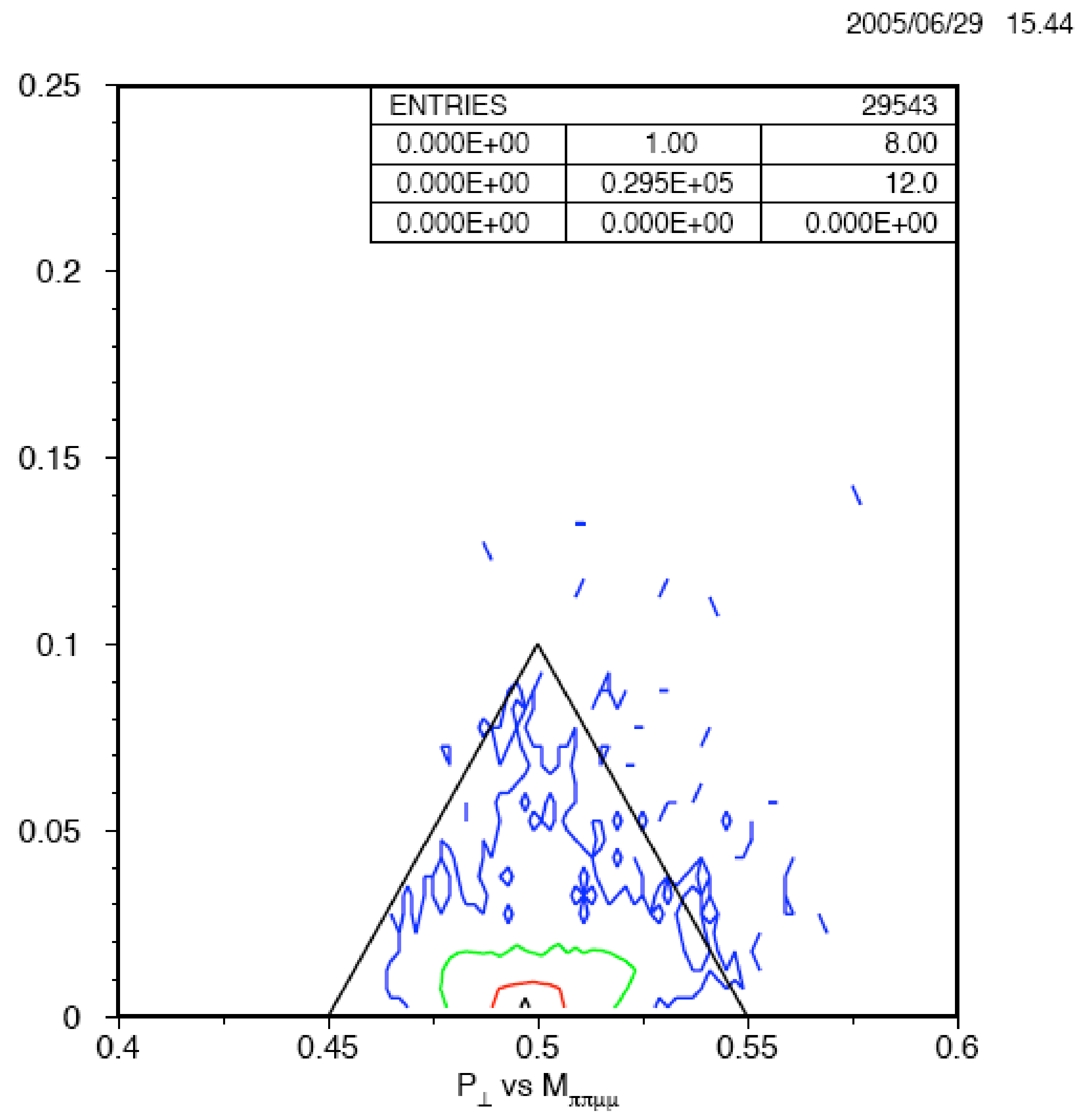
Hogan give me, as an example cmd file, ~/krand/em_cmd/em_00_146.cmd.

First 20 jobs take up little space indeed: 23Meg. DISK1 has something like 15Gig left.

Bug in code: I lose some data, not much, off the first tape. CSI799 finds that run 8028 is not a good E799 run, which is correct. So it kills the job. Instead I should either modify CSI799 to call with KTERR ranther than KTABORT level, or modify the KTeVana code to check the run number before calling CSI799. I leave this all for the next round to fix, as it really isn't much data lost. Uh. looks like I lose also the data on NZL067 for the same reason.

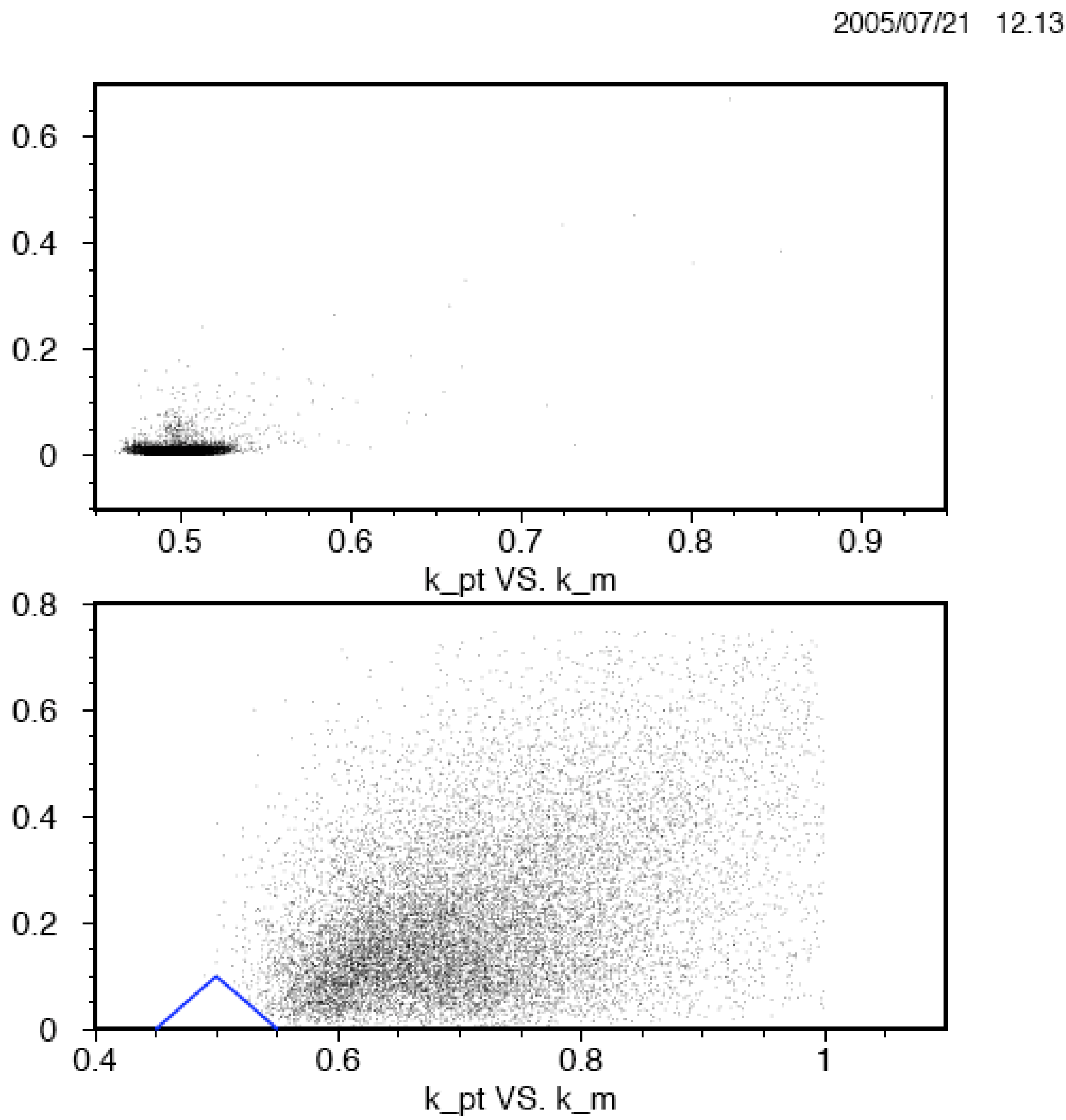
Missing runs: job NZL107 - 110, containing runs 10721 - 10736, run smoothly but have no output events selected. Why? I get some fragment of this data onto fnal07 off the staging area for later investigation. Looks like my GOODATA branch for runs between 9000 and 11000. Should I change it? Nahh.

Blind region definition: based on signal MC: $P_\perp < 0.1 - 2\,|M_{\pi\pi\mu\mu} - 0.5\text{GeV}|$



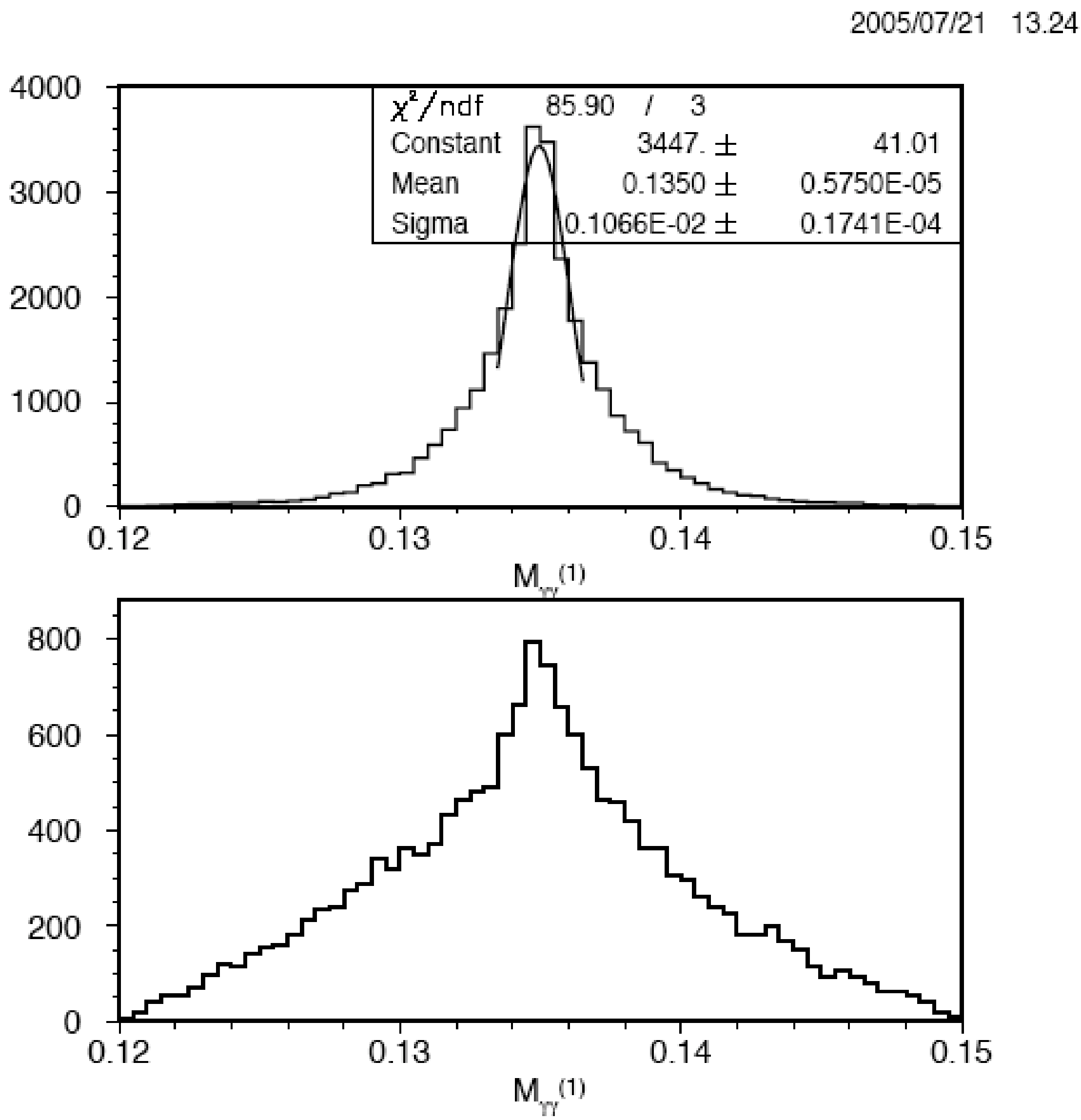
The blue contours are the 3σ level, based on the bin contents alone. There are 29522 entries in the bin, a bit off a tail off to the upper right that I do not understand, and this is from an MC run of 999991 events. In trigger 5, 188496 events got through L1, 179859 got through L2, and 97620 came out of L3, including 719 L3-rndm events. The total data file is 109753 events, so there are 12133 events from other triggers that are included in that file. 29542 of them got out of the KTeVana job. I seem to have lost quite a few events to the 4 unassociated hardware clusters requirement. I guess that is not very surprising. Marginal acceptance here for run 8397 is (0.443 ±0.017).

Killing the background: From the data events, with the blind region removed, I define the background. I do not think that I know what it is, but from my first plot, which uses just the KTeVana cuts (B05, 4 hardware clusters not associated to any tracks, 2 vertexable tracks of 7GeV or more momentum with matching soft or hard clusters below 2GeV energy, and 2 π^0 combos within 15MeV of the correct mass), I think that I am looking at accidental overlays of perhaps $K_{\mu 3}$ with $\pi^+\pi^-\pi^0$ fragments:

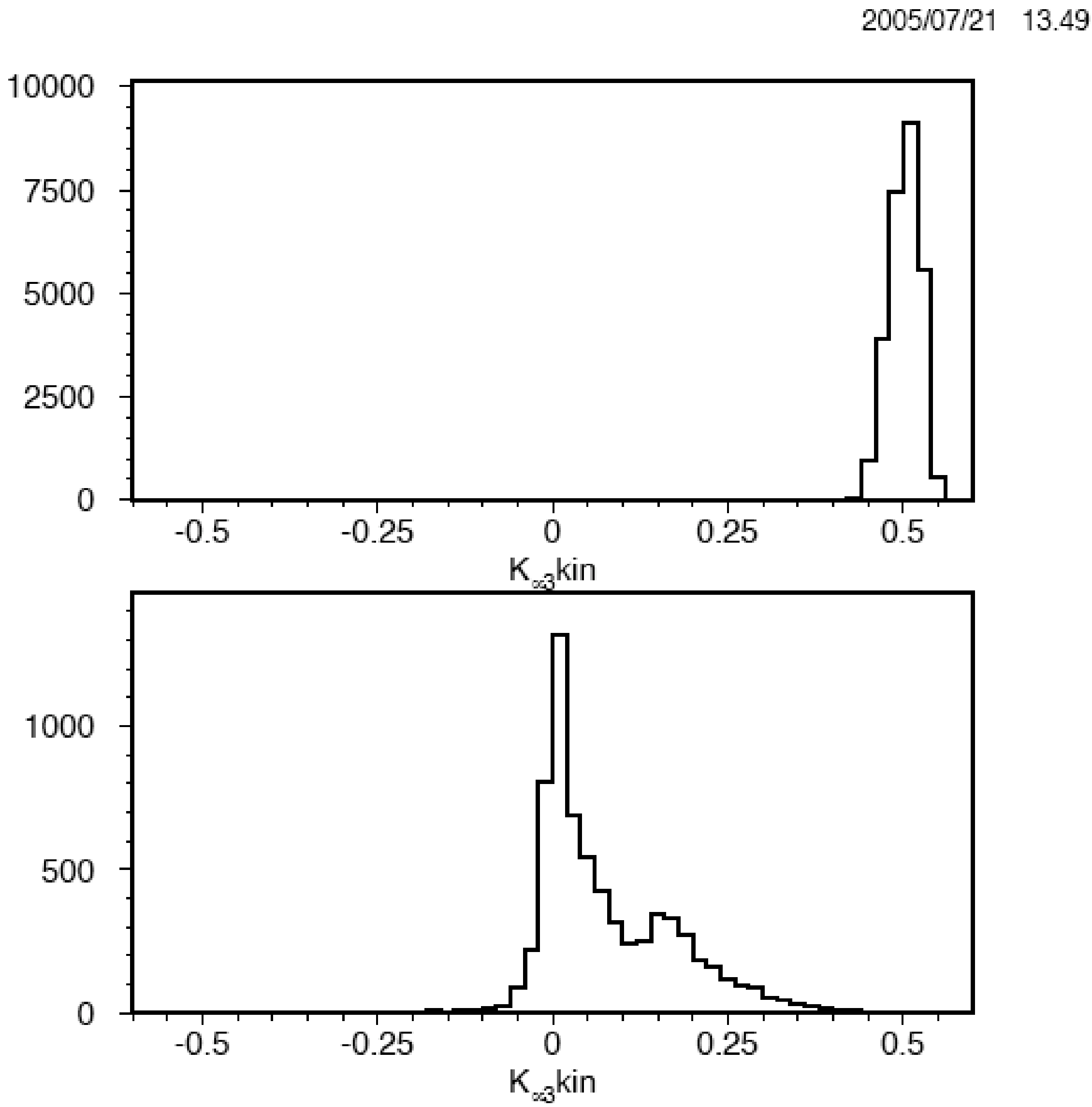


Additionally, I note that the momentum spectrum in the data is harder than in the M.C. signal. CsI says they are $\mu^{\pm}$ at 186m though.

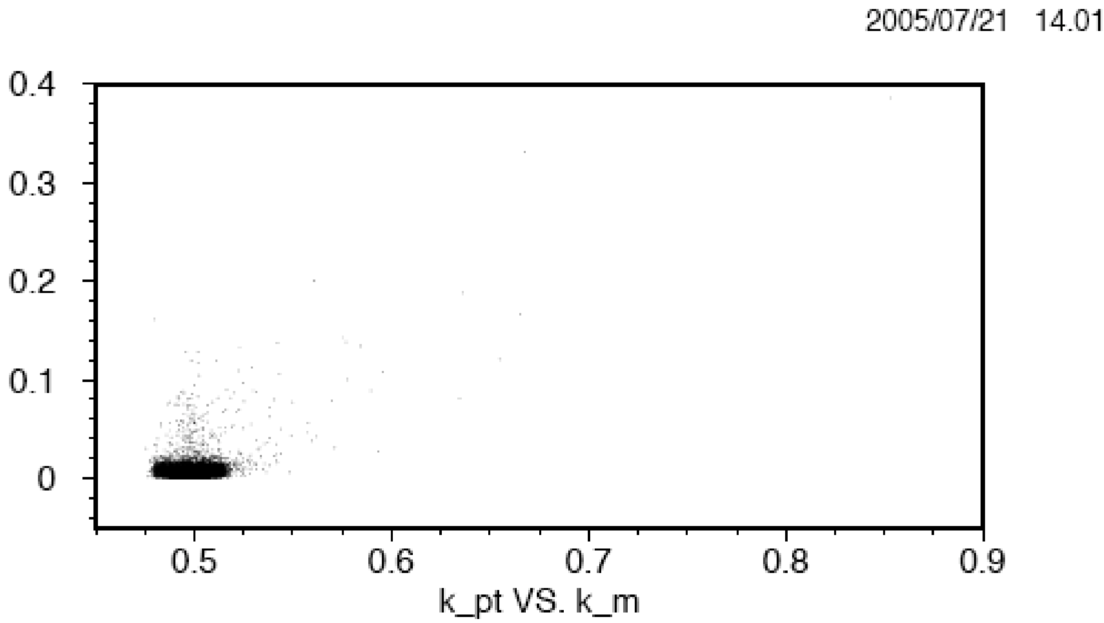
Cut on the π^0 mass: From the distribution of the 1st π^0, *i.e.*, the one that is closest to the PDG in reconstructed mass, I decide to require both π^0masses to be within ±9MeV, *i.e.*, in the range 126 to 144MeV.

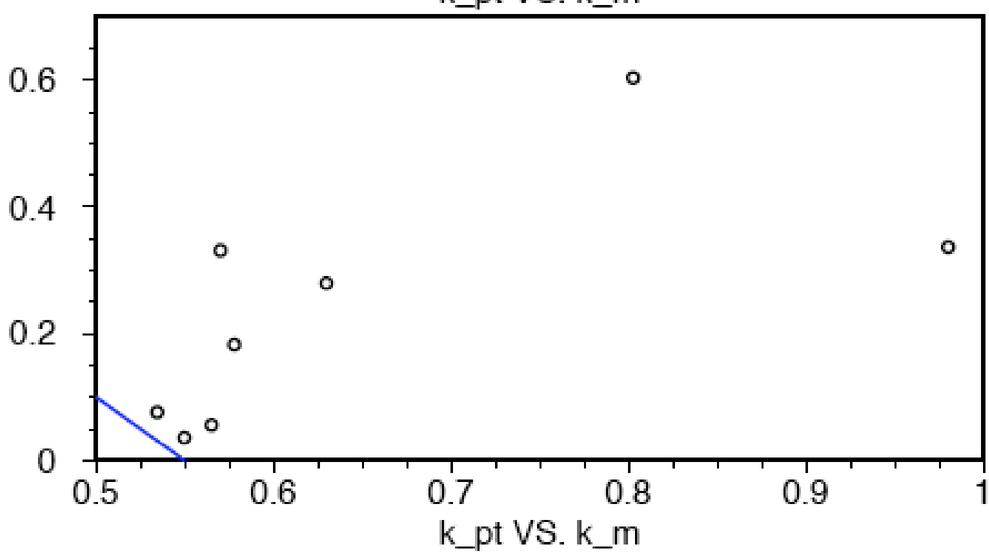


Cut on $K_{\mu3}$ kin: From the parameter to identify $K_{\mu3}$ events based only on the charged particle reconstruction comes a powerful selection criterion.

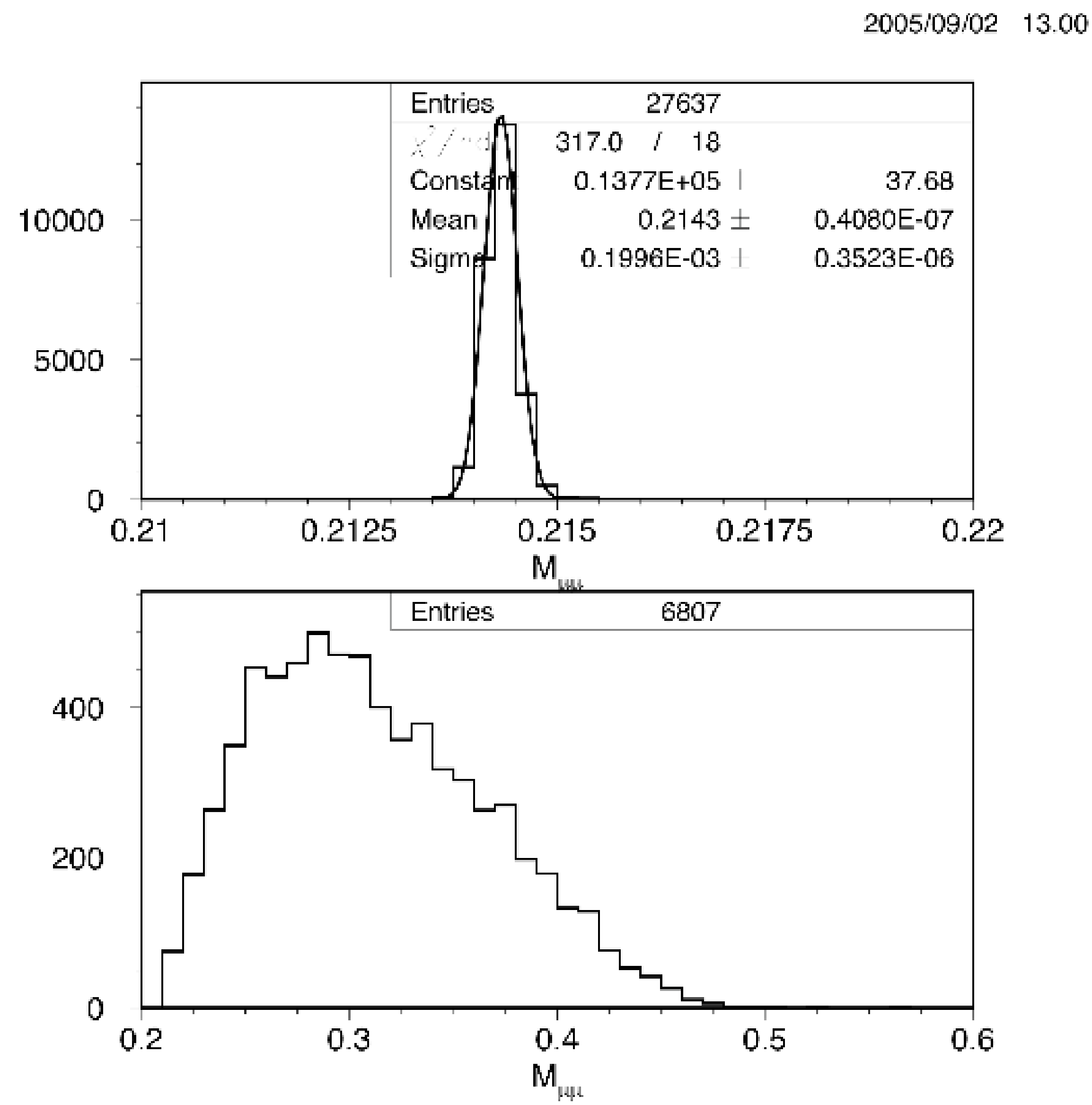


I decide to cut at 0.45. Here is what the background looks like at this point:



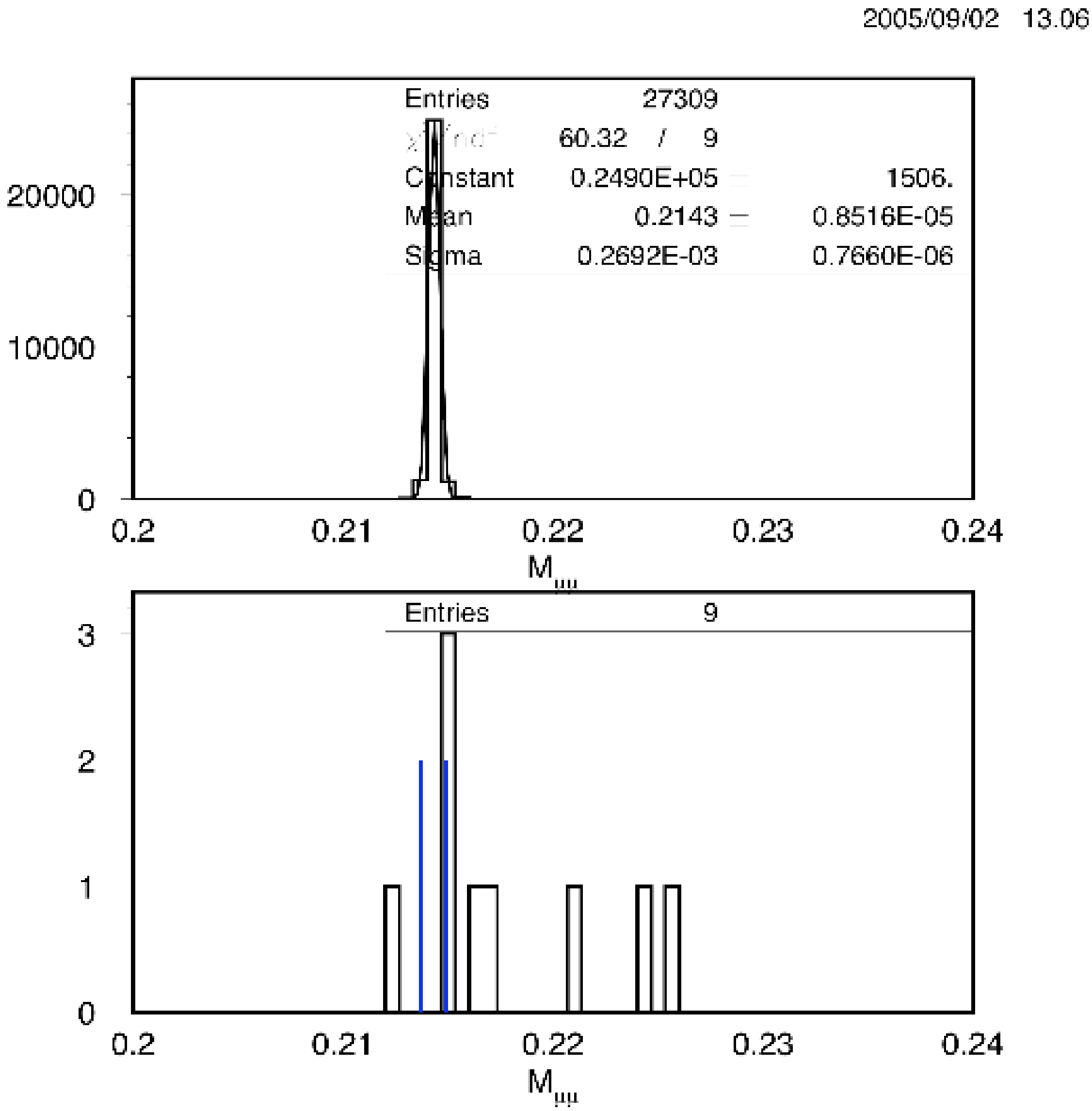


There is not much of it, but it seems to cluster near the region of interest. Another power cut is of course the dimuon mass, shown here with the π^0 mass cut only:



I decide to cut at ±3σ = ±0.6MeV. There is no background after this cut. I think that the background is mostly combinations of 2 $\pi^+\pi^-\pi^0$ events with $\pi^\pm \rightarrow \mu^\pm$ decays that happen fairly far upstream. The magnet offsets and χ^2 are similar, signal-to-data, and the vertex $\sigma(z)$ is actually better in the background data than in the signal, reflecting I think a larger opening angle in the background events after two $M(\pi^0)$ cuts.

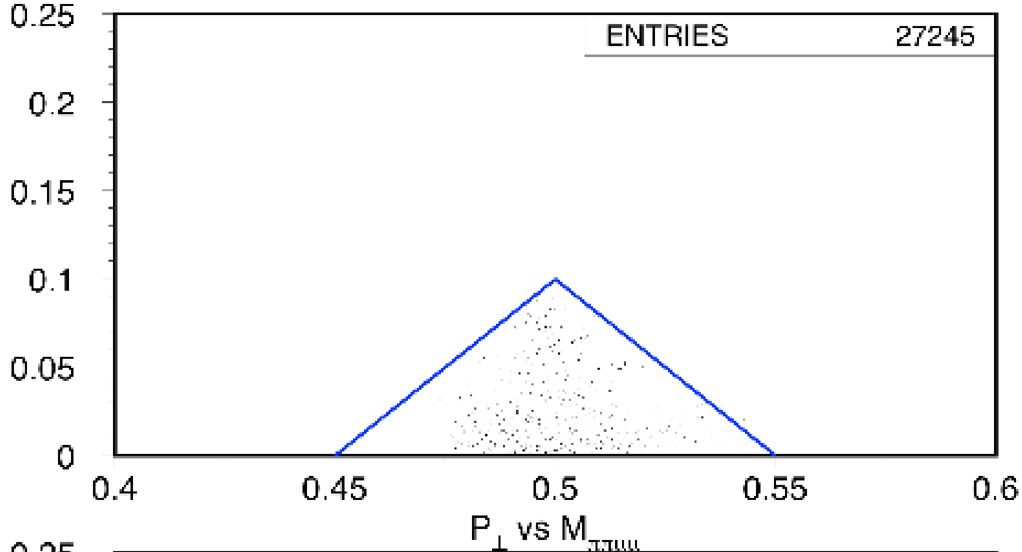
Let's go back and look at the dimuon mass with both the π^0 mass cut and the $K_{\mu 3}$ cut. There are 9 events in the data and some are close to 214.5MeV!

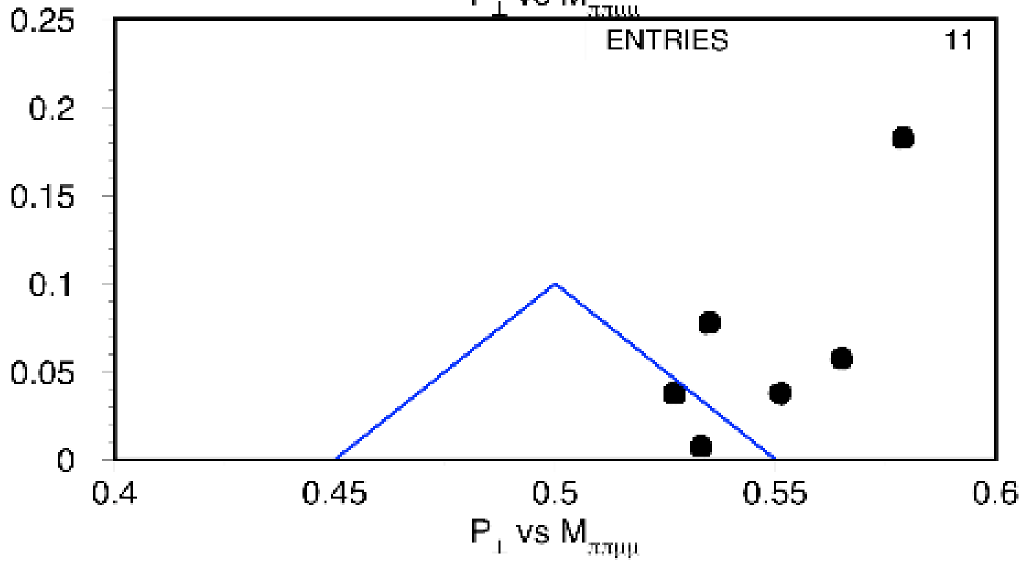


Acceptance is about 2.73% at this point. For a flux of 268 x10^{9} K_L (that is from Sada's analysis; Peter M got 265 x10^{9}) then the single event sensitivity is 1.37 x10^{-10} which, for 51.6ns lifetime is 1.75 x10^{-24} MeV. Compare HK's width of 2.5 x10^{-19}MeV.

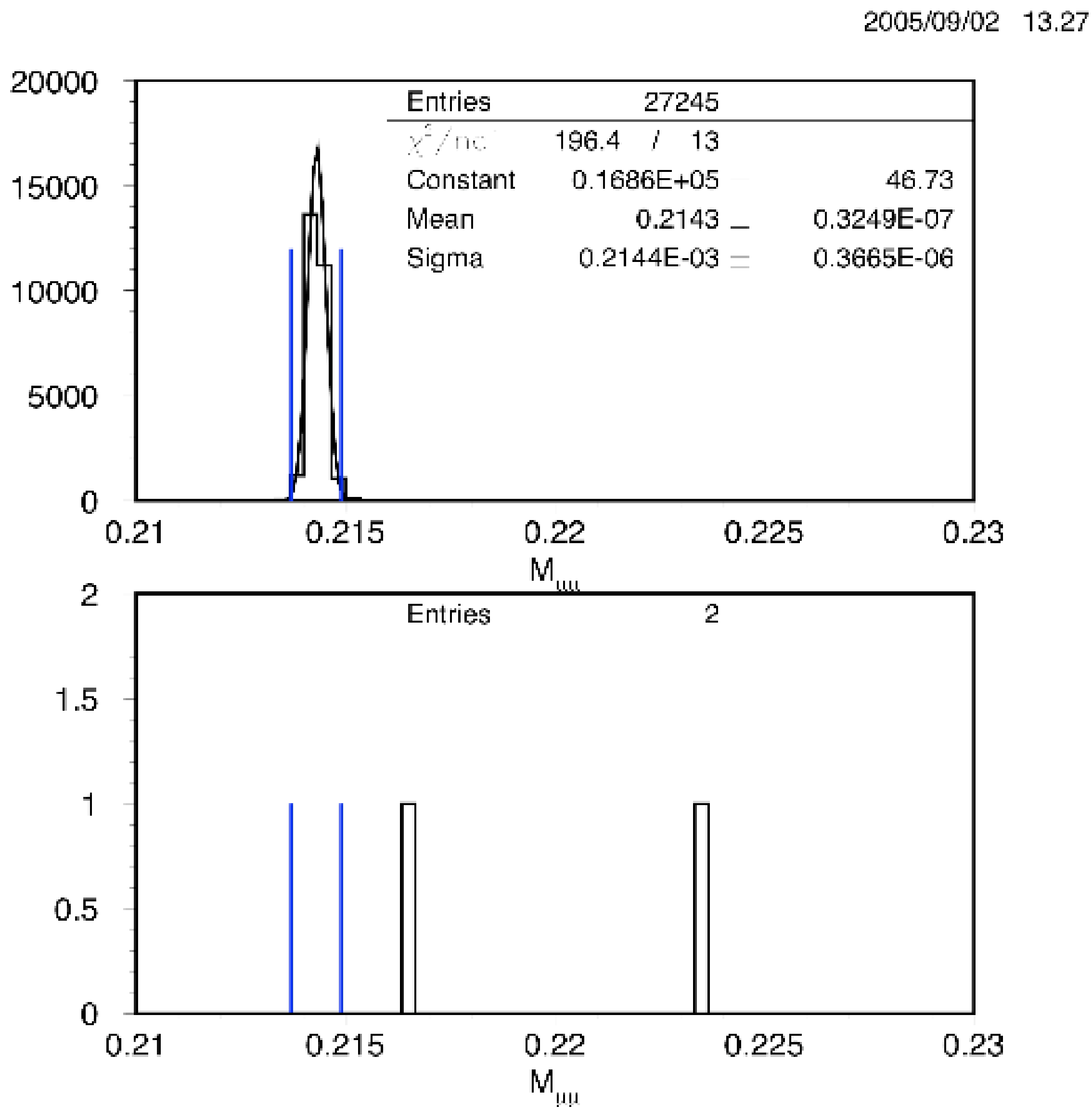
Opening the box: I don't have a background estimate but can reasonably set it, conservatively, to zero. I create an HNT file with all of the data events, and find 2 events inside the $P_{\perp}$, $M_{\mu\mu\pi\pi}$ box:

2005/09/02 13.21





These events are not in the interesting region of $M_{\mu\mu}$:



At the 90% C.L., the partial width for a new pseudoscalar quark but not lepton flavor changing boson is less than 4.0 x10^{-24}, which is almost 5 orders of magnitude below the HyperCP suggestion.

We should note that the possibility of a spatially extended weakly coupling intermediate state, with tensor couplings that put $\mu^+\mu^-$ into a J=2 state is not ruled out. Oh, maybe it is. Such a boson would also put the $\mu^+\mu^-$ into J=$0^{(+)}$ and $1^{(+)}$ states in addition to the $2^{(+)}$ state. These would contribute to $K_L\rightarrow\pi^0\mu^+\mu^-$, $K_L\rightarrow\pi^0\pi^0\mu^+\mu^-$, and $K_L\rightarrow\pi^0\mu^+\mu^-$, respectively. Not there!

10 Sept 2005: While preparing to present this to the KTeV collaboration meeting, I think as follows: $Br(K_L\rightarrow\pi^0\mu^+\mu^-)$ in the s.m. is 1.5 x10^{-11} according to Isidori *et a*l. which has roughly equal components from *CP*- conserving and *CP*- violating

amplitudeds. Now the π^0 is CP odd, so the previously conserving amplitude becomes a violating amplitude & vice versa, but the sum is gonna be similar. except for phase space corrections.

There is a formula for massless phase space:

$$\int d\Phi = (2\pi)^{-3N} (\pi/2)^{N-1} \frac{w^{2N-4}}{\Gamma(N)\Gamma(N-1)}$$

which I think I got from the RAMBO paper, or maybe one of it's references. So I evaluate this for w = 16MeV (that is, kaon mass minus 2 π masses and 2 μ masses) and 4 bodies; this is 2.22 $\times 10^{12}$. Then for 3 bodies, but initial state mass of M_K less 2 μ and 1 π mass, I get 1.79 $\times 10^{11}$. Ratio is 12.4 – so maybe the branching ratio in the s.m. for $K_L \rightarrow \pi^0\pi^0\mu^+\mu^-$ is something like 1.9 $\times 10^{-10}$ which is probably close to the SES for a well done analysis on the full dataset. Huh! Not! Because the 2γ CPC $\pi^0\mu^+\mu^-$ contribution to goes to a CPV and therefore small contribution, but the 1γ CPV $\pi^0\mu^+\mu^-$ term becomes CPC and dominates. Hence the correct way to get the Br is to take the $\pi^+\pi^- e^+ e^-$ Br and scale THAT by phase space. No, wait. Use $\pi^0\pi^0 e^+ e^-$ not $\pi^+\pi^- e^+ e^-$. Because it is an electromagnetic process, dumbkoffen!

P.Heiliger and L.M.Sehgal, Phys.Lett. B307 (1993) 182-186 says $Br(K_L \rightarrow \pi^0\pi^0 e^+ e^-)$ = (2.0 ±0.3) $\times 10^{-10}$.

Tasks procured in presentation to the KTeV collaboration meeting:

Recheck the KM3KIN variable. That plot just looks suspicious.
Do the correct RAMBO calculation for phase space ratio.

I go back to routine KMATS in PARK97.LEO and build a debug version and dump a signal event in detail. I observe that the higher momentum track is defined to be the muon. Reasonable, in light of the muon momentum cut, but I wonder how often this happens in real $K_{\mu 3}$ events. well, strictly speaking it doesn't matter. OK I debug at inevt=1234 which is in run 8101 in my signal MC file, with commands «dxladebug -iow park97.exe» , breaking at line 14900, after «leomake park97 97 deb». I'll put the numbers in a spreadsheet called KM3KIN.XLS. OK It's right. Checks all the way out to the ntuple used to make the plot.

Using massless phase space approximation, 4 body decay with 16.37 MeV of initial mass has less phase space than for 226.67 MeV by a factor of 6.376 $\times 10^4$. That's basically the ratio of the free energies to the 4th power. But my RAMBO calculation gives a very different result, with a ratio of 3480.6. Uhhhh. Ah. Bug in my spreadsheet. The right phase space numbers are 6.1265 x10^{-6} for 16.37 initial mass and 0.22521 for 226.67 initial mass; ratio for massless case is thus 3676.0, pretty close to the real result. So from the Heiliger & Sehgal result, one expects something like $Br(K_L \to \pi^0\pi^0\mu^+\mu^-) \approx (5.8 \pm 0.9)$ x10^{-10}.

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29 November 2005 – He, Tandean & Valencia have eprint `hep-ph/0509041`, which gives much lower rates for $Br(K_L \to \pi\pi\mu\mu)$, like ~10^{-8} for the pseudoscalar and 10^{-10} for the axial-vector case. I don't get it; I thought these numbers would be much bigger, like 2 x10^{-5}. Also, there is a paper by some Russians working in the SUSY model that should be cited; Deshpande, Eilam and Jiang in `hep-ph/0509041`, also get these sorts of rates using fairly textbook type methods.

(1) He allows for $J^{(P)}$ non-conservation at the $P \to \mu\mu$ vertex. I figured that for P to be on-shell, this could be neglected. How close to on-shell do I have to be? The *HyperCP* resolution is ~0.8 MeV, corresponds to 8 x10^{-22} m ⇔ 250 fm @ lightspeed. And if the line's true width is thinner, it could travel even further before decay.

(2) He uses chiral perturbation theory at the hadronic vertex. Can that produce any sizeable supression of the 4-body decay relative to the 3 body ones? Maybe due to forbidden angular momentum configurations?

(3) He calculates the couplings, matrix elements, and phase space separately. As I should have done. Let's fix that. From $d\Gamma = (2\pi)^4 |\mathcal{M}|^2 \, d\Phi / 2M$, where $|\mathcal{M}|^2$ is the square of the matrix element, I can use the branching ratio from HyperCP and an integrated phase space to get an average value for the square of the matrix element. That number is $< \mathcal{M} >^2$ = 2.8 x10^{-15} and when that is multiplied by $(2\pi)^4 \; \Phi / 2M$ for the $K_L \to \pi\pi\mu\mu$ decay, I get a partial width of 5.8 x10^{-19} MeV, which is branching ratio of 4.5 x10^{-5}. Huh. Still looks like a huge branching ratio to me!

Deshpande's reply argues that I should redo step 3 with three-body phase space using 214.3MeV. OK, I do that…

For $\Sigma \to pP^0$, phase space integral is 5.0015 x10^{-6}, and unit matrix element width is 3.2769 x10^{-6}. Since it is a 2 body decay, I can & do check these with PDG equations; they are high by 0.4 x10^{-3}. Good! Then for $K \to \pi\pi pP^0$, phase space integral is 33.3474 x10^{-6} and unit matrix element width is 52.2189 x10^{-6}.

For Σ lifetime of 8.018 x10^{-11} sec, Br of 3.1 x10^{-8} corresponds to Γ = 2.5448 x10^{-19} MeV and an average matrix element-squared of 7.7660 x10^{-14}

That makes the corresponding K_L width 4.0553 10^{-18} MeV, which is a $K_L \to \pi\pi\mu\mu$ branching ratio of 3.1791 x10^{-4}. I do the same calculation for one π^0 in the decay and get that the phase space is 17.95 x10^{-6}, the partial width of $K_L \to \pi^0\mu^+\mu^-$ of 2.18 x10^{-21} MeV; that would be a branching ratio of 1.72 x10^{-4}.

Valencia's paper gives $Br(K_L \to \pi^+\pi^-\mu^+\mu^-)$ lower by a factor of 4.6 or so, for the pseudoscalar case. (By doing the full complement of couplings he is doing the parity conserving and parity violating cases in a far better method than my old arm-waving). How much of that factor of 4.6 is phase space? From Rambo, I get like, a factor of 10 from phase space! In the massles approximation, that is about what I get too.

Valencia and Deshpande both reply, more or less, that close to the kinematic limit, a constant matrix element is a bad approximation. Here are my emails from them:

From: Nilendra Deshpande <desh@uoregon.edu>
Date: 30 November 2005 6:50:34 PM CST
To: Leo Bellantoni <bellanto@fnal.gov>
Cc: jing@uoregon.edu, Eilam Gad <eilam@physics.technion.ac.il>
Subject: Re: Your recent paper

Leo: Here are some thoughts on your question:
On Nov 29, 2005, at 4:46 PM, Leo Bellantoni wrote:

\> Dear authors of hep-ph/0509081
\>
\> I am a KTeV collaborator who has been interested in the implications of
\> the peculiar HyperCP result for a while. I am grappling with your recent paper.
\>
\> The resolution of the HyperCP detector is on the order of 0.8MeV; that
\> means that the postulated new particle would have c*tau of about 250 fm, at
\> least, and quite possible a lot more. In such a case, why not apply J^P
\> conservation at the mu+ mu- vertex and say that only new vector and
\> pseudoscalar bosons need be considered?
All four assignments are allowed. Scalar, pseudoscalar, vector and axial vector.
Since we do not know how X particle couples to muons, we cannot eliminate any of these.
We only considered scalar and psedoscalar in our paper.
(Coupling can be mu bar mu or mu bar gamma 5 mu)
No conclusion is possible simply by taking limit on width that you quote

>
> I tried to get a rough estimate of the contribution to the Klong -> pi pi mu
> mu rate from the hypothesized new physics. The HyperCP branching ratio,
3.1 e-8, corresponds to a partial width of 2.5 e-19 MeV. Starting from the well
> known formula (38.10) of the Particle Data Group 2004, I multiplied
> 2.5 e-19 MeV by twice the sigma hyperon's mass and divided by
> (2pi)^4, thus leaving an integral of the matrix element squared over the phase
> space. I then integrate the phase space alone, and use this to get an estimate
> of the matrix element squared. For the phase space integral I get 1.389e-4
> using RAMBO, so <M>^2 is 7.76e-15. Then I reverse the process, using the
> integrated phase space for Klong -> pi pi mu mu (which is 1.318e-4) and of
> course the Klong mass to get a partial width for the new physics in the Klong
> system of 5.77e-19MeV. That corresponds to a branching ratio of 4.5e-5,
> rather a few orders of magnitude over the results you have. Why? The only
> thing I can think of is that there is some kind of suppression at the hadronic >
vertex.

Your assumption amounts to no momentum dependence in the matrix element. This is particularly bad approximation for K_L decay which is highly momentum dependent because of chiral symmetry. Also, you need to calculate your phase space for Sigma to P X and K long to pi pi X, taking 214 Mev for X mass. (May be you have done it that way) The phase space available is pretty small for K_L.

>
> Thank you very much for your time.
>
> Leo
> ---
> Leo Bellantoni
> MS 122, Fermilab Batavia, IL 60510 (630)730-2155
>
>
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desh@uoregon.edu

From: "G. Valencia" <valencia@iastate.edu>
Date: 8 December 2005 5:04:54 PM CST
To: Leo Bellantoni <bellanto@fnal.gov>
Subject: Re: Your recent paper
Reply-To: valencia@iastate.edu

Hi, I took a quick look and it is not so easy to redo with "constant matrix elements" because there are intrinsic differences between the two processes in that the two amplitudes have different mass scales attached (through spinors for example). If I take everything constant but leave the spinors my answer gets bigger but not as big as yours.
So I suspect the difference is all in that what you did assumes an unrealistic form for the matrix elements.

On Mon, 2005-12-05 at 10:52, Leo Bellantoni wrote:
> OK. Thanks very much!
>   Leo
> --
> Leo Bellantoni
> MS 122, Fermilab Batavia, IL 60510   (630)730-2155
>
>
> On 5 Dec 2005, at 10:03 AM, G. Valencia wrote:
>
>> Hi,
>>>
>>>> Since the mode has a huge kinematic suppression it is possible that small
>>>> changes in the kinematic dependence of the amplitude will lead to big
>>>> differences in the rate.
>>> Uh, can you elaborate?
>>
>> For K --> pi pi mu mu there is very little phase space, so the actual answer is
>> very sensitive to things that would not matter much normally, such as the
>> such as the exact values of the K and pi mass. I need to rerun my programs
>> with constant matrix elements, I'll do it as soon as I get a chance.
>>
>> German