EbE Vertexing for Mixing

(CDF-7673)

D_s^* and other “reflections”

Alessandro Cerri, Marjorie Shapiro

BERKELEY LAB

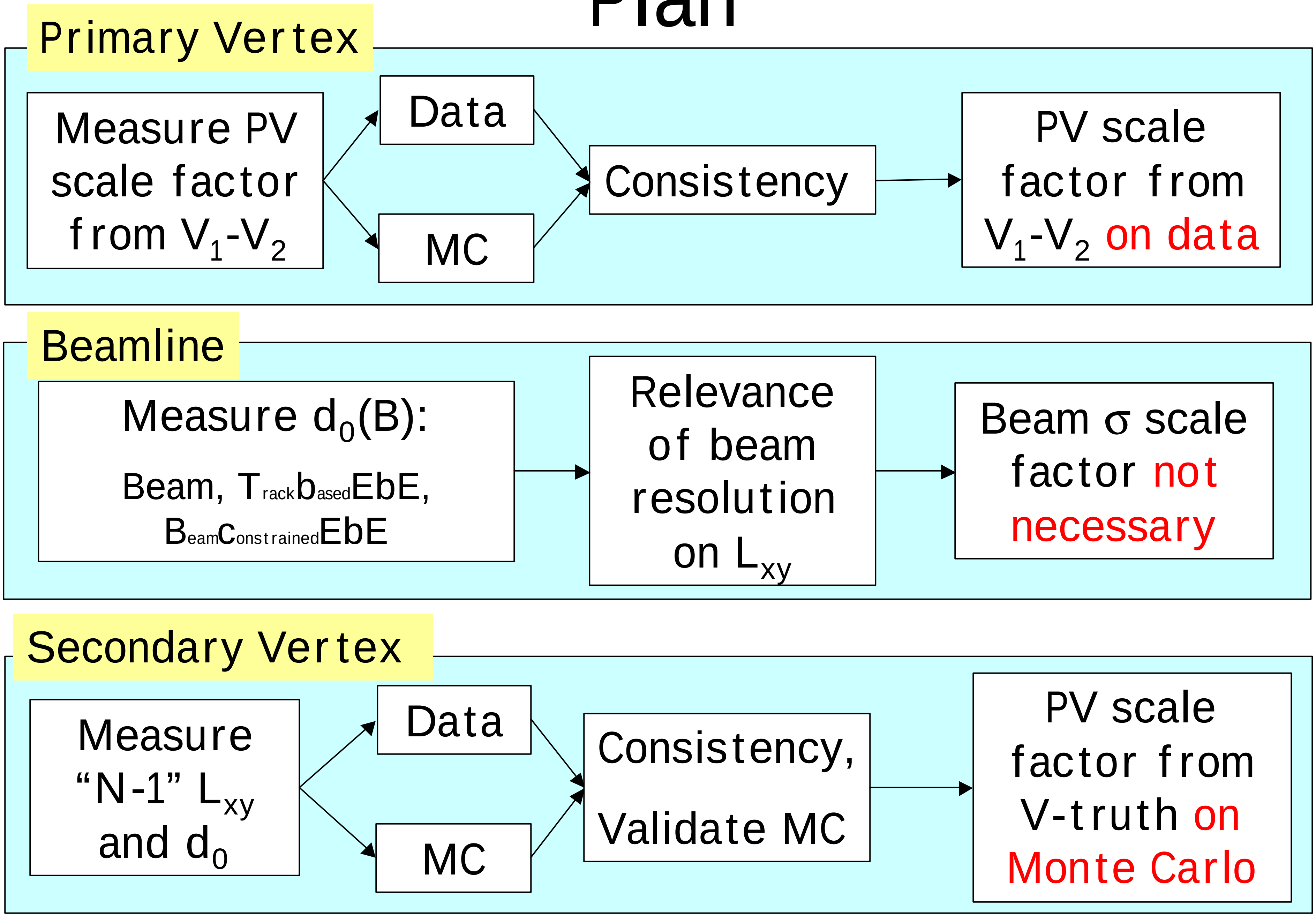
Aart Heijboer, Joe Kroll

UPenn

Two Talks in One

- EbE vertexing and ct scale factor:
  - We preblessed and didn't bless, why?
  - Where are we
  - Conclusions and plans
- Partially reconstructed Bs modes:
  - Why bother?
  - How to improve the naïve approach
  - Is it feasible?
  - Plans

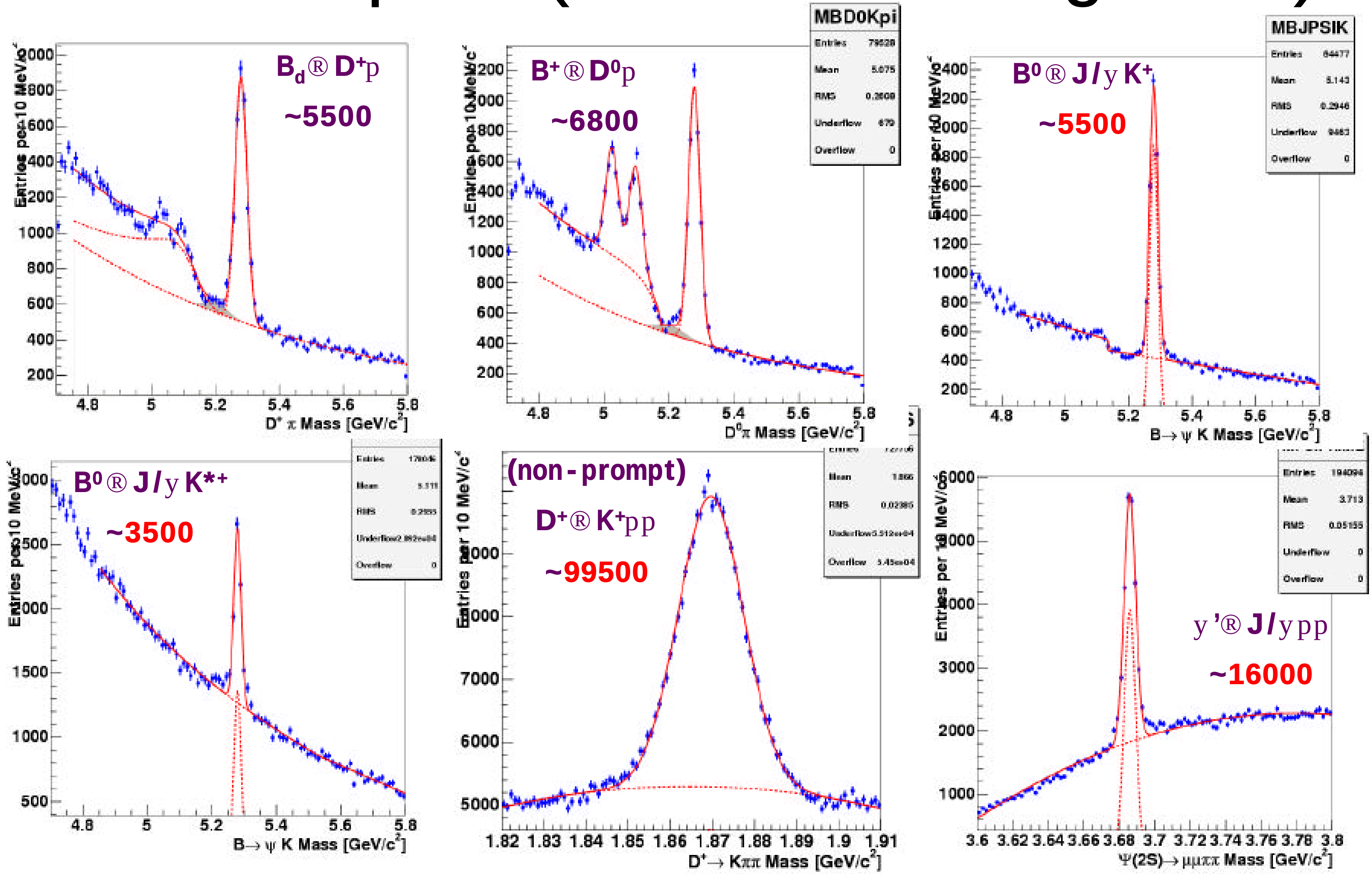
Plan


Primary Vertex
Measure PV scale factor from V1-V2
Data
MC
Consistency
PV scale factor from V1-V2 on data
Beamline
Measure d0(B):
Beam, TrackbasedEbE, BeamconstrainedEbE
Relevance of beam resolution on Lxy
Beam σ scale factor not necessary
Secondary Vertex
Measure "N-1" Lxy and d0
Data
MC
Consistency,
Validate MC
PV scale factor from V-truth on Monte Carlo

Problems after preblessing

- It was pointed out that our statistics is significantly lower than the standard analyses
- We identified the cuts responsible for that and relaxed them

The samples (after relaxing cuts)



~22000 fully reco'd B, ~100000 Fully reco'd D+, ~16000 fully reco'd ψ'

Montecarlo: mostly BGEN (basically all of the above+B_s), using Pythia if possible

This comes with a price though!

	$J/\psi K^+$ BGEN	$J/\psi K^+$ Pythia	$J/\psi K^+$ Data	$J/\psi K^{*+}$ MC	$J/\psi K^{*+}$ Data	$K^+\pi\pi$ MC	$K^+\pi\pi$ Data	$J/\psi\pi\pi$ MC	$J/\psi\pi\pi$ Data
N-1 L_{xy} Pull	1.18±0.02 ±0.4	1.24±0.016 ±0.12	1.35±0.017 ±0.4	1.18±0.02 ±0.3	1.56±0.02 ±0.2	1.14±0.009 ±0.02	1.22±0.004 ±0.03	1.16±0.02 ±0.1	1.21±0.01 ±0.2
N-1 d_0 Pull	0.97±0.02 ±0.3	1.13±0.014 ±0.07	1.19±0.014 ±0.4	0.99±1.3 ±0.2	1.31±0.02 ±0.2	1.08±0.00 8 ±0.02	1.02±0.003 ±0.03	1.04±0.02 ±0.1	1.11±0.008 ±0.3
MC X_{SV} pull	1.30±0.02 ±0.01			1.23±0.02 ±0.01		1.13±0.01 ±0.15		1.21±0.02 ±0.04	
MC Y_{SV} pull	1.25±0.02 ±0.2			1.28±0.02 ±0.09		1.14±0.01 ±0.2		1.27±0.02 ±0.15	
MC Z_{SV} pull	1.17±0.02 ±0.03			1.15±0.02 ±0.01		1.16±0.01 ±0.01		1.09±0.02 ±0.07	
MC Lxy Pull	1.15±0.02 ±0.04			1.18±0.02 ±0.04		1.17±0.01 ±0.15		1.20±0.02 ±0.01	

$<1\sigma$ (yellow)
$<2\sigma$ (green)
$<3\sigma$ (red)

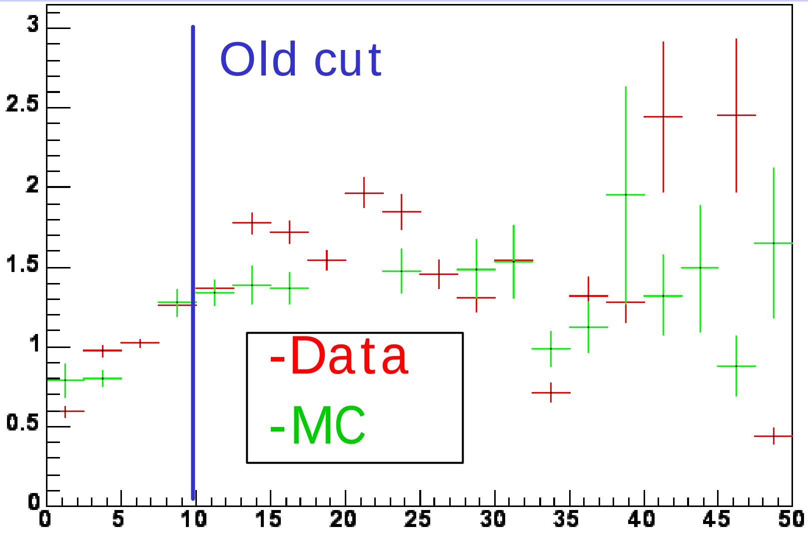
Large systematic uncertainties (up to 30%) and data/mc disagreement

Differences with Preblessing

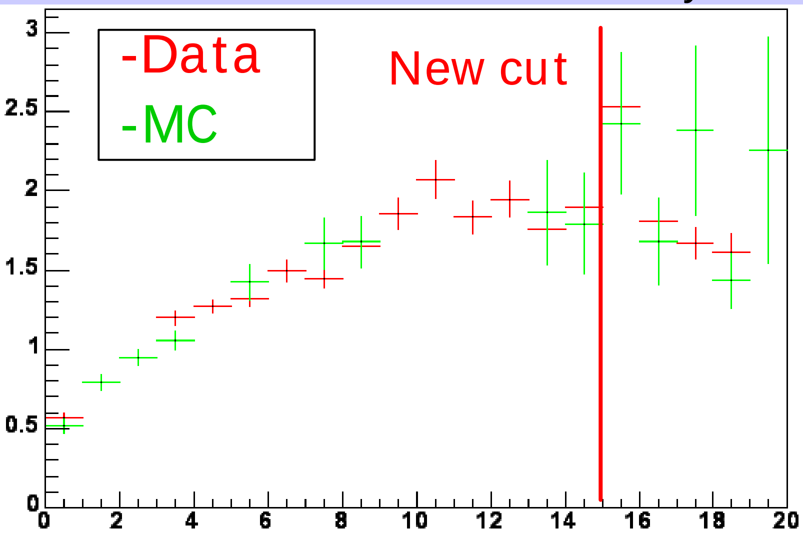
1) We gain in statistics: consistent signal yield with CCKM analyses
2) Looser cuts $\rightarrow$ secondary vertex pulls in general get larger
3) Pay another price: larger pull discrepancy between data and MC

- The main source of 1) and 2) seems to be the χ^2 cut:

N-1 Lxy Pull vs χ^2_{3D}



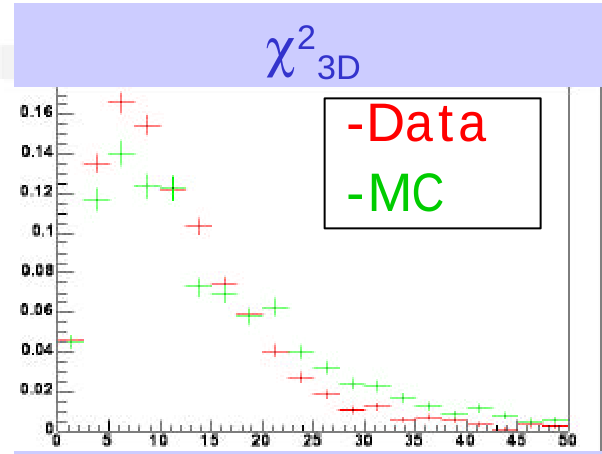
N-1 Lxy Pll vs χ^2_{xy}

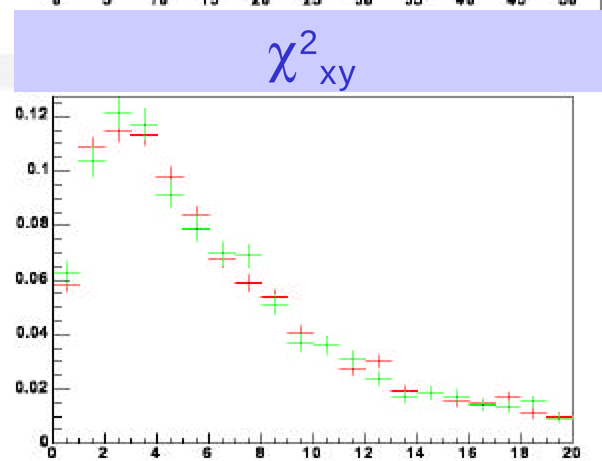


This does not quite explain 3), since agreement between data and MC seems pretty good!

Data-MC disgreement

- Disagreement is as large as O(30%)
- Can't be neglected
- A difference in the distributions? (kinematics, geometry, chi2 etc.)
- χ^2_{3D} is not well reproduced, but we moved to χ^2_{xy}
- Other discrepancies? No evident single-variable ones:





We compare systematically all the distributions and pull behaviors for the various samples, against MC

Bottomline

With larger statistics, relaxed cuts:

- No more dependence on ct/L_{xy}
- Kinematics MC and data differ significantly
- However Pulls don't seem to depend on those (individually)
- Pulls do depend on χ^2 but this is expected since χ^2 can be expressed as a linear function of the pulls themselves!
- Pulls generally larger but far from the '7500 numbers (~1.3)
- We are re-generating MC samples as close as possible to the data kinematics

Reproducing the ‘7500 approach

- We are able to roughly reproduce the ‘7500 quantity (L_{xy} of ‘fake’ B)
- We spent some time figuring out the discrepancies in our samples: skimming is in progress with selections as close as we can to the blessed result
- Remember this is a quantity which is DIFFERENT from what we usually use in our study
- For this sample there are reasons to believe that several variables (e.g. χ^2, isolation etc.) shouldn’t have the same distributions as the data:
  - Presence of D^+ and/or pions from secondaries will make it larger than in signal!
  - Trigger confirmation different (D daughters only vs all B daughters)
  - L_{xy} pull is bound to grow indefinitely with χ^2 for “background”!
  - Larger $\chi^2 \Rightarrow$ wider pull

In any approach: a tight cut on χ^2 (and any sensitive selection variable) will reflect in a modification of the expected L_{xy} pull, no matter what the definition is!

Conclusions on scale factors

- Changing cuts changes the scale factor
- Changing fit model changes the scale factor
- The scale factor is not really a “scale factor”: hidden dependencies
- A scale factor of 1.4 for the current analyses is “conservative” in terms of the limit we obtain
- For the future We know we can improve things!
- In progress:
    - Cross check of blessed result: final word on reproducing the ‘7500 numbers
    - MC generation to improve systematic uncertainty on PV and SV scale factors
    - What is the best way of correcting the χ^2 dependence?

D_s* & co.:
anything below $D_s\pi$
containing useful information
($[D_s\gamma]\pi$, $D_s l\nu$, $D_s\pi$, $D_s\rho$...)

A closer look at the MC mass distribution

•All histograms BGEN 5.3.1 with lum.avg. full simulation

•Below Main peak:

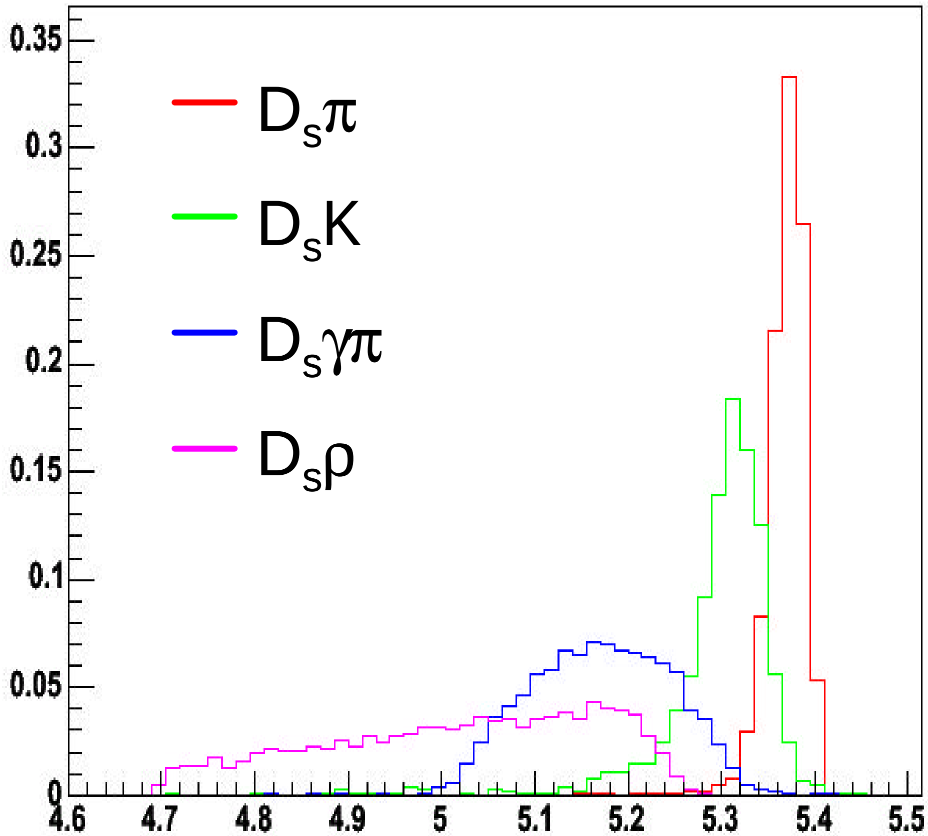
  •Photon background nastier than the rest: leaks in!

  ⇒Larger uncertainty and correlation in the fit

  ⇒Extend to B_s to use detailed fit developed by Hung-Chung for B^0 decays

☹Different backgrounds overlap in the 'satellite' region

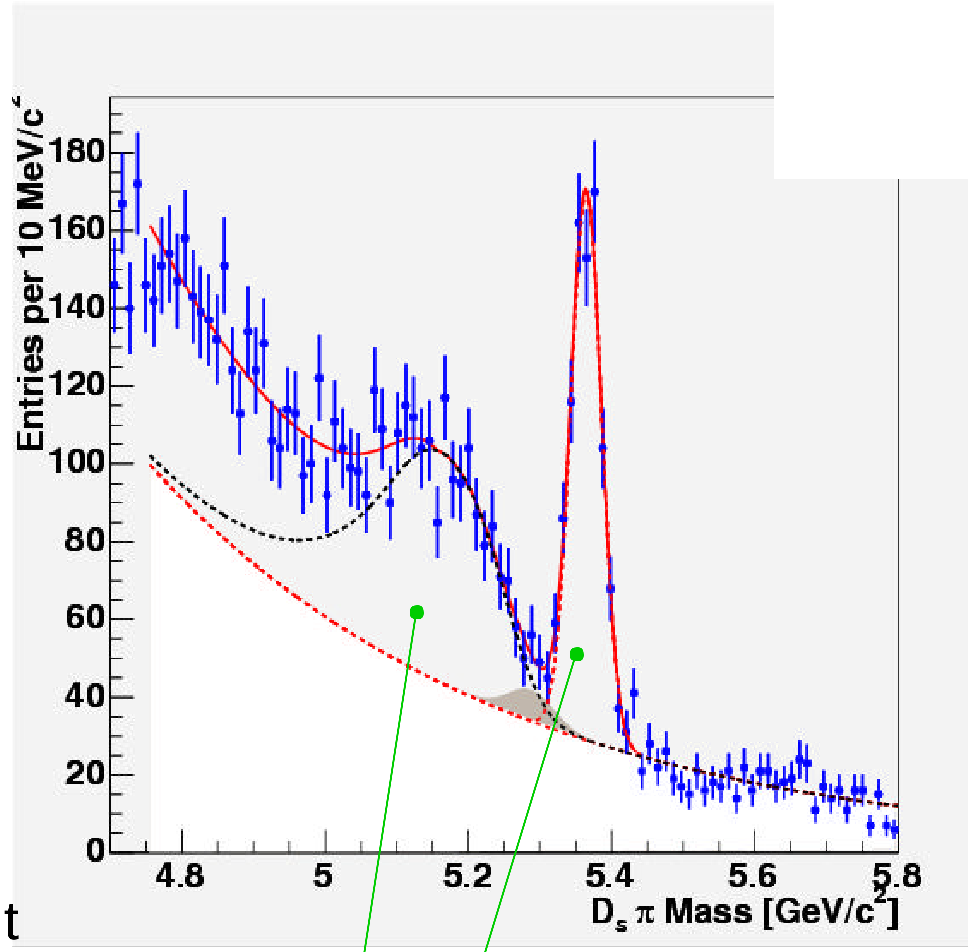
☺Mass cut should help rendering K factors similar



Once you get below the fully reco'd peak, you have to bite the bullet and deal with all the modes leaking in!

Yields from mass fit

- Fit analogous to CCKM: same sample, similar results
- Fit systematics are a problem, especially for non-fully reco'd stuff:
  - Combinatorial background could be a problem: d_0(EbE) helps!
  - B→DK has 200% uncertainty until we measure the BR!
  - Photon leak-in depends on detector resolution (can probably be controlled looking at the B^0 edge)
  - Keeping under control inter-mode correlations will be crucial!



$$\frac{N_{Satl}}{N_{D_s p}} \approx 2-4$$

D_s^*

(..and $D_s\rho$, and partially reconstructed more in general)

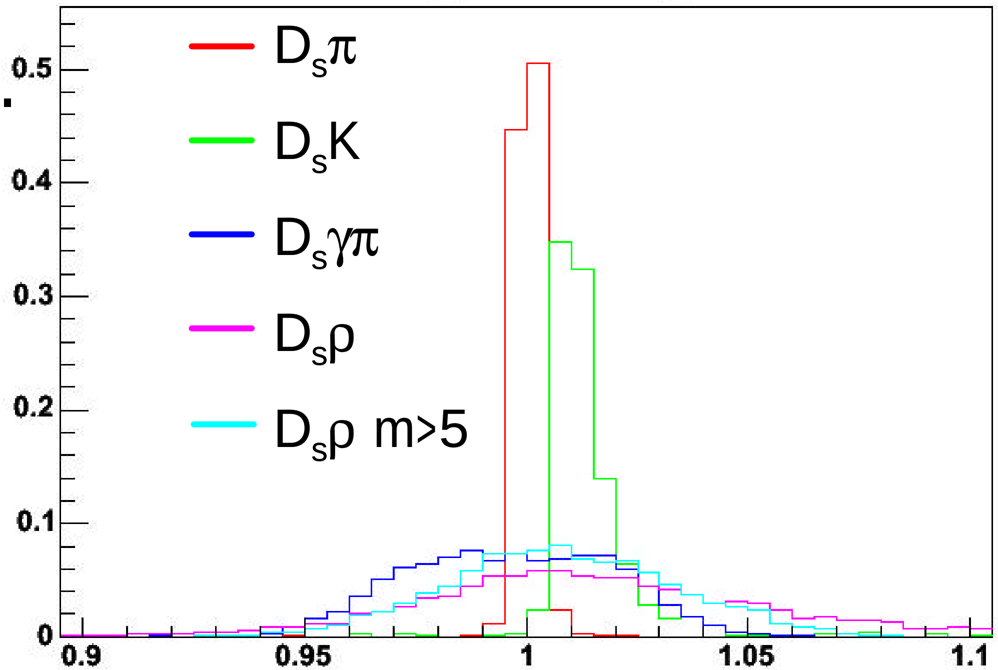
- Take this as an example: it is inevitable to have to deal with all the different modes at once!
- Interesting 'cheap' way of doubling our hadronic statistics
- $Bs \rightarrow D_s^* \pi \rightarrow [D_s \gamma] \pi$
  - Small q for in D_s^* decay (analogous to $D^* \rightarrow D\pi$)
    - Photon ~parallel to Ds
- What is the loss in momentum (ct) resolution?
  - ~~Negligible: happy hadronic-style analysis~~
  - Not Negligible: mixed semileptonic-like approach
    - K factor
    - There are interesting differences and analogies (focus of this talk)

Naïve approach

- K factor (M_{Bs}/P_t) distr. from realistic MC:
  - ~10% uncertainty

  On B momentum
  - Much better than

  Semileptonics
  - Still NOT negligible



- Technically, Bs mixing on this sample is like measurement on the semileptonics:
  - Better momentum resolution
  - Nastier background:
    - Oscillating
    - Many hard to disentangle components (ρ,D_s*,μ/e,D_sK,?), each with (at least in principle) different K factor

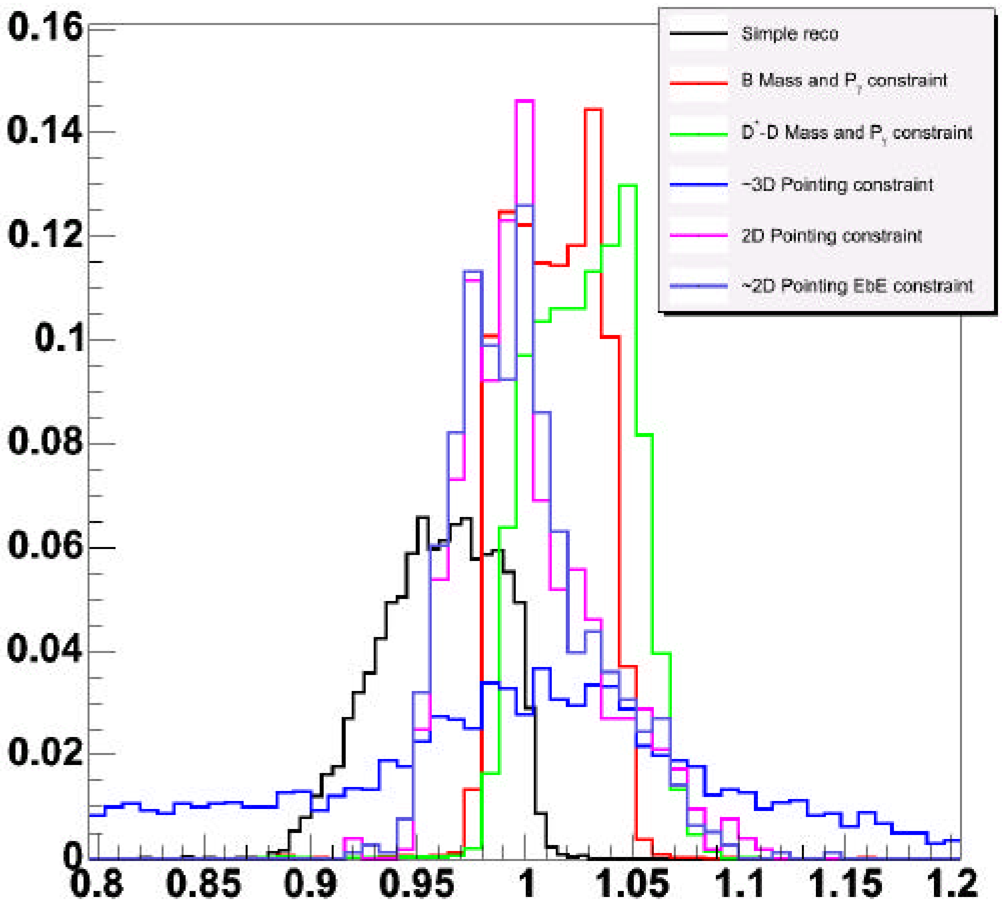
Improving the ct resolution

- Different strategies can be applied to ‘recover’ information on the missing momentum:
- $B_s \to D_s$ K: use the right mass assumption/don’t use M in the K factor!
- $B_s \to D_s$ X [4 unknowns]
  - Impose Bs invariant mass (1 constr.)
- $Bs \to D_s \rho$, $D_s \gamma \pi$ [3 unknowns]
  - D_s^* (or ρ) invariant mass (1 constr.)
  - D_s^* small q $\Rightarrow$ γ ~parallel (<0.1 rad) to D_s (~2 constr.)
- $L_{xy}/\sigma_{Lxy}>7$ $\Rightarrow$ reasonable lever arm for geometrical measurement of B direction
  - Beamline / 2D EbE (1 constr.)
  - 3D EbE (2 constr.)
- 2 Ways of applying each of this:
  - Explicitly solve equation and derive quantity (easier)
  - Refit topology (à la CTVMFT) with additional constraint(s) (more complicated)

Ds* Mode

- Used as benchmark, once the technology is there it can easily be expanded to semileptonic and hadronic modes
- Today's results based on explicit solution
- Feasibility of full refit under way
- We will explore the following cases:
  - Bs mass and $P_\gamma \propto P_{Ds}$ constraints (2+1)
  - Ds*-Ds mass and $P_\gamma \propto P_{Ds}$ constraints (2+1)
  - Full 3D pointing constraint plus a combination of Bs and Ds*-Ds constraint (2+1)
  - 2D pointing constraint using beamline plus full Bs and Ds*-Ds constraints (1+1+1)
  - 2D pointing constraint using EbE plus full Bs and Ds*-Ds constraints (1+1+1)
- Result evaluated looking at the derived 'K factor'

Comparison of K factors



Constraints	Mean	RMS
Default	0.959	0.0276
M(D*-D)	1.027	0.0255
M(Bs)	1.013	0.0199
~3D	0.996	0.147
2D (Beam)	1.002	0.0317
2D (EbE)	1	0.03202

- Imposing $P_\gamma \propto P_{Ds}$ yields overestimate of momentum, as expected
- 3D pointing alone is very poor constraint
- $P_\gamma \propto P_{Ds}$ already improves K factor

Naïve derivation in several cases worse than upfront uncertainty

Fully constrained refit is bound to be better, how much? (work in progress)

Conclusions

- Easiest inclusion of Ds* (& friends) is just a replica of the semileptonic approach
- Separation of modes and possible discrepancy in K factors could raise serious issues?
- Improvement on first-round is in principle possible:
  - several constraints at hand
  - Need lots of information (track parameters, covariances, vertices…)
- Aim:
  - get the last word on how much we can improve before proceeding to a full-fledged analysis
  - Reach a reasonable compromise between complication and improvement!
  - We want to measure the Bs lifetime including all these modes:
    - Important independent analysis
    - Fundamental cross-check for partially reconstructed modes
  - BR($B_s \to D_s K$) Will come "for free" (piggyback on $B^+ \to DK/\pi$)