

Topics in Heavy Quark Expansion for Beauty

with the attempt to kindly respect the indicated time limits
in order to allow for adequate discussion time

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- We have the QCD-based theory of B decays handles only a limited range of problems, of course
- Thanks to BaBar, its predictions have already been confronted with reality in the good settings Hopefully similar checks will be available from Belle, CLEO, hadron colliders
- $b \rightarrow q$, viz. QCD on the light front and the OPE require development, not just refinements

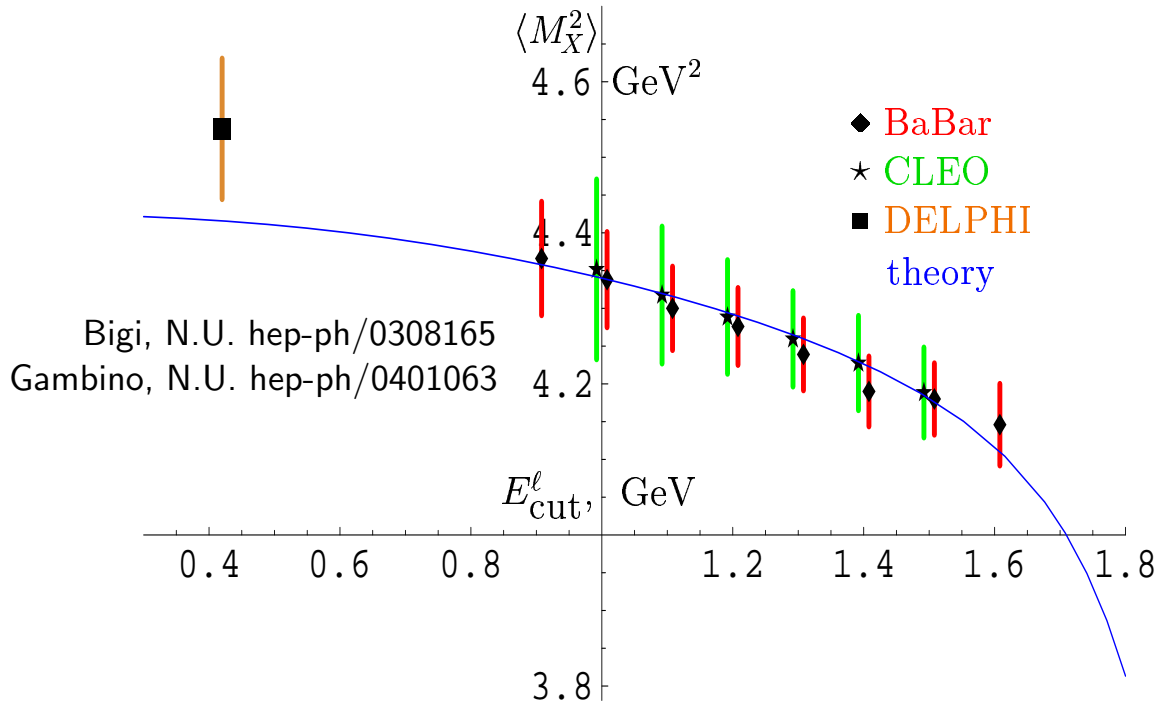
Inclusive semileptonic $b \rightarrow c l \nu$ distributions can/should be further explored

We have good control over inclusive $B \rightarrow X_s + \gamma$ and $B \rightarrow X_u l \nu$ decay characteristics with the today's experimental capabilities if use full power of the OPE, in particular utilizing OPE fit results from $B \rightarrow X_c l \nu$

• No apparent problem with $\langle M_X^2 \rangle$ vs. E_{cut}^ℓ

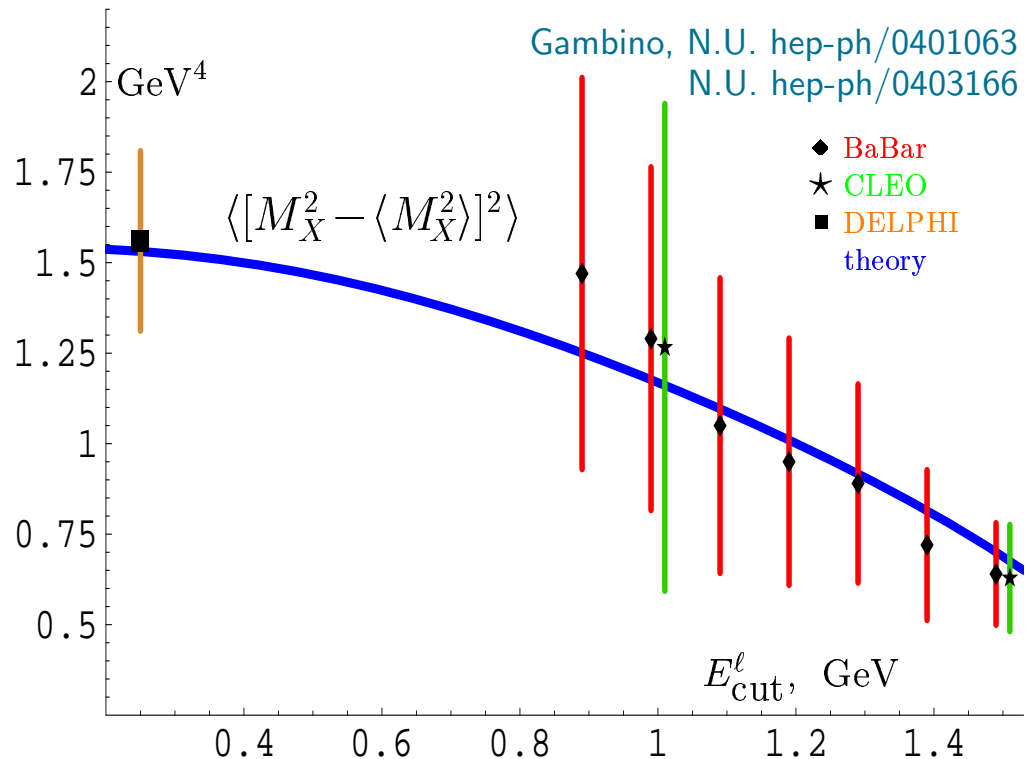
Robust OPE approach à la Wilson, $\mu = 1\text{GeV}$:

Data and expectations
as of July 2003



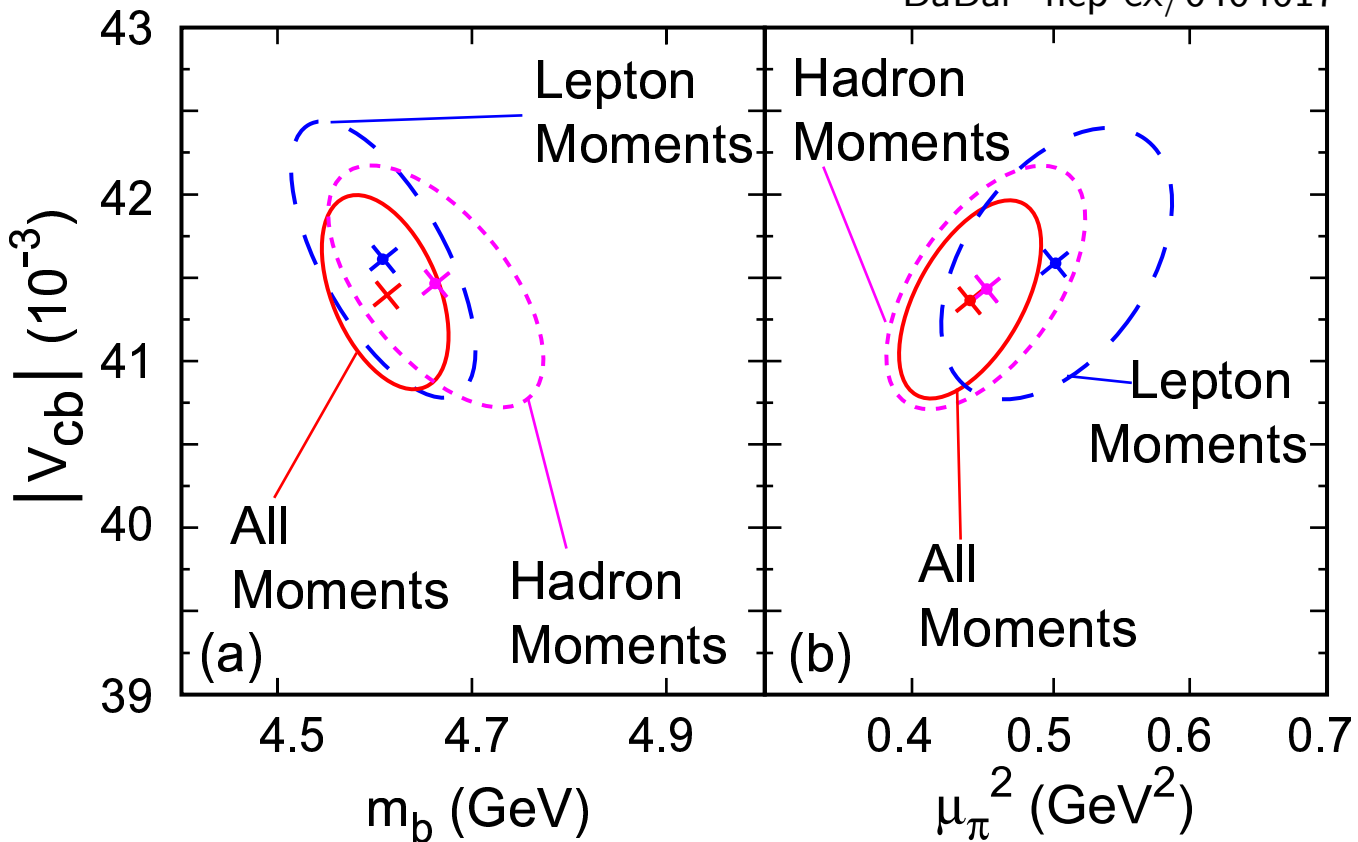
Second mass moment $\langle [M_X^2 - \langle M_X^2 \rangle]^2 \rangle$:

Parameters fixed from the BaBar fit
hep-ex/0404017



Good agreement where the right theory is used

OPE seems to work even where may be expected to break down



- ♠ Have an accurate and reliable determination of many HQ parameters from experiment
- ♣ Extracting $|V_{cb}|$ from $\Gamma_{sl}(B)$ has good accuracy and solid grounds
- ♥ Have precision checks of the OPE at the nonperturbative level

Overall there are many remarkable agreements with predictions

I think the most impressive is good consistency between $\langle M_X^2 \rangle$ and $\langle E_\ell \rangle$: A sensitive check of the nonperturbative sum rule for $M_B - m_b$

Important: the HQ values emerge in accord with the theoretical expectations: $m_b, \mu_\pi^2 > \mu_G^2, \dots$
the right scale for ρ_D^3

- $b \rightarrow s + \gamma$ moments?

Relying on relations *imprecise* with a high cut on E_γ

$$\langle E_\gamma \rangle = \frac{m_b}{2} + \dots \quad \langle [E_\gamma - \langle E_\gamma \rangle]^2 \rangle = \frac{\mu_\pi^2}{12} + \dots$$

A good way to accurately measure HQ parameters...

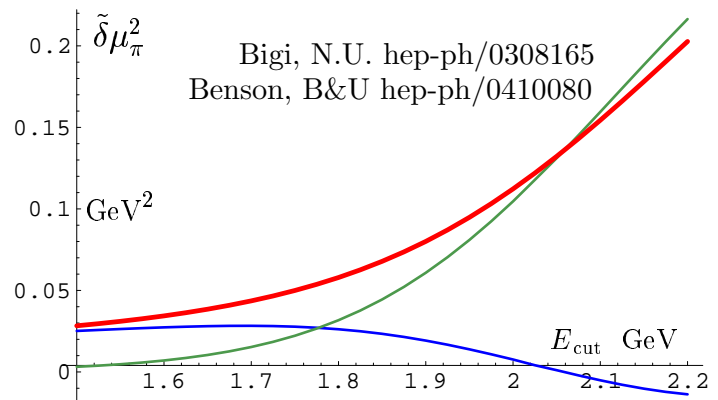
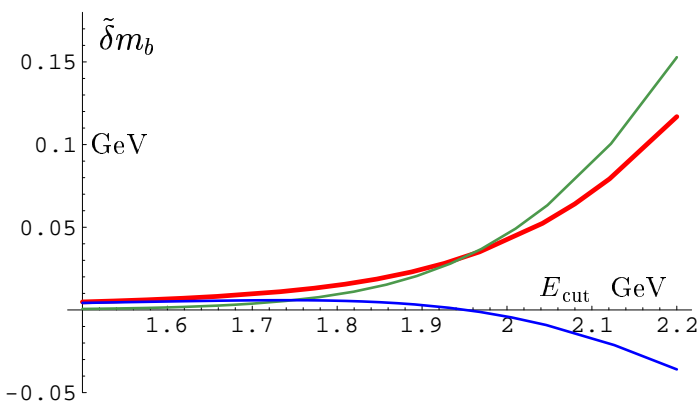
Bottle neck: 'Hardness' Q often gets too low with the cuts

even in $b \rightarrow c l \nu$ $Q \simeq m_b - m_c$ for total widths, but Q is below 1 GeV for $E_\ell > 1.7$ GeV

A complementary consideration suggests the expansion for M_X^2 loses sense for $E_{\text{cut}} \geq 1.7$ GeV

Terms appear $\propto e^{-\frac{Q}{\mu_{\text{hadr}}}}$

In $b \rightarrow s + \gamma$ $Q \simeq M_B - 2E_{\text{min}} \simeq 1.2$ GeV if the cut is at $E_\gamma = 2$ GeV



Accounting for these biases yielded a good agreement between all measurements

Perturbative corrections with the explicit Wilsonian cutoff have been calculated including all orders in BLM

Benson, Bigi, N.U. hep-ph/0410080

BELLE 2004: With $E_\gamma > 1.8$ GeV cut *biases* are not that much an issue

$$\begin{aligned}\langle E_\gamma \rangle &= 2.292 \pm 0.026_{\text{stat}} \pm 0.034_{\text{sys}} \text{ GeV} \\ \langle [E_\gamma - \langle E_\gamma \rangle]^2 \rangle &= 0.0305 \pm 0.0073_{\text{stat}} \pm 0.0063_{\text{sys}} \text{ GeV}^2\end{aligned}$$

For BaBar's HQ values we would obtain

$$\langle E_\gamma \rangle = 2.312 \text{ GeV} \quad \langle [E_\gamma - \langle E_\gamma \rangle]^2 \rangle = 0.033 \text{ GeV}^2$$

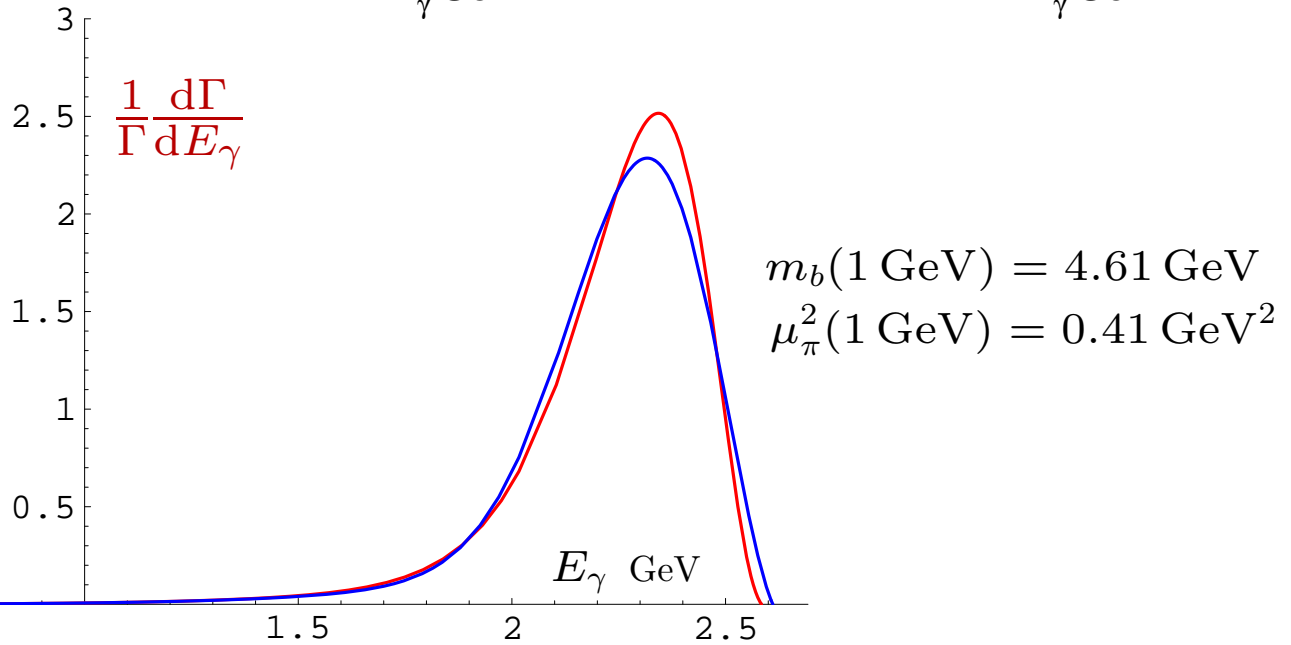
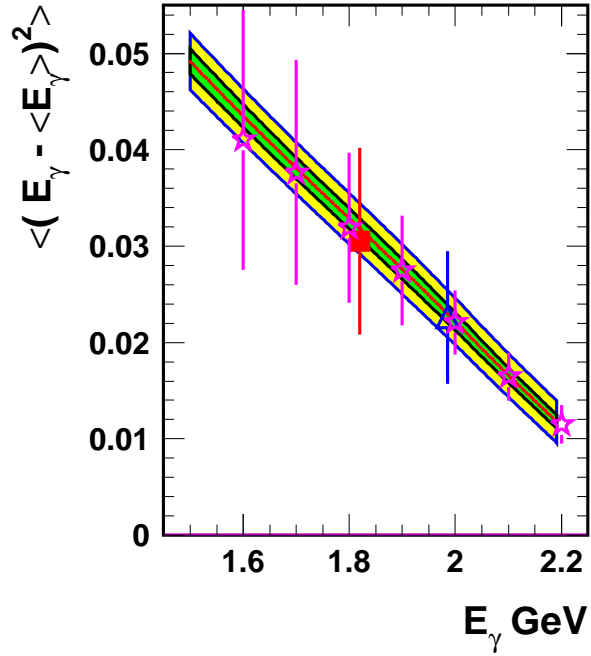
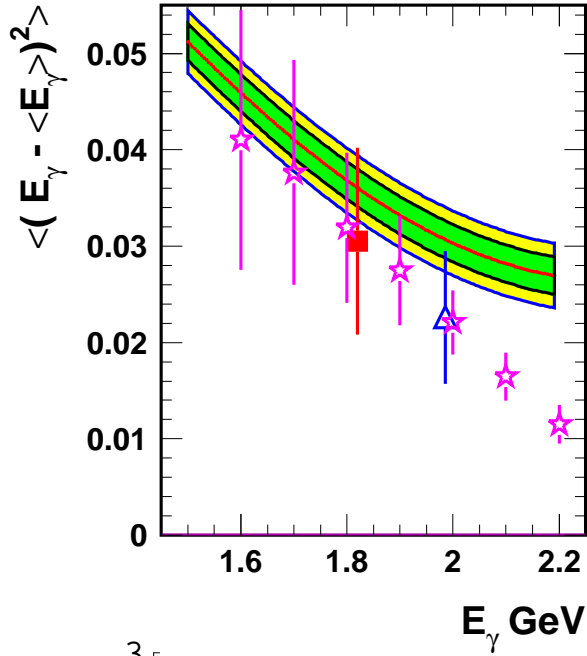
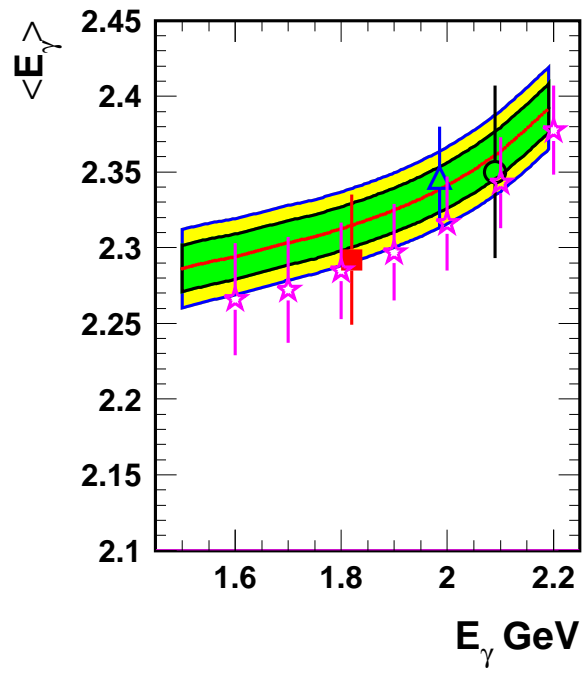
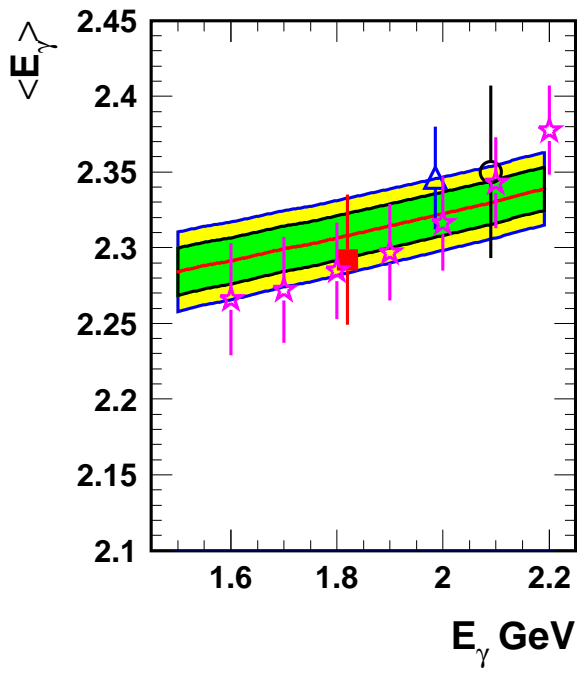
CLEO 2001: $E_{\text{cut}} = 2$ GeV

$$\begin{aligned}\langle E_\gamma \rangle &= 2.346 \pm 0.032_{\text{stat}} \pm 0.011_{\text{sys}} \text{ GeV} \\ \langle [E_\gamma - \langle E_\gamma \rangle]^2 \rangle &= 0.0226 \pm 0.0066_{\text{stat}} \pm 0.0020_{\text{sys}} \text{ GeV}^2\end{aligned}$$

vs.

$$\langle E_\gamma \rangle = 2.342 \text{ GeV} \quad \langle [E_\gamma - \langle E_\gamma \rangle]^2 \rangle = 0.0225 \text{ GeV}^2$$

Quite consistent!



Verdict:

OPE works well, the heavy quark parameters derived from experiment are consistent with the independent theoretical predictions

Perturbative corrections have been correctly calculated and are expected well behaved in the proper Wilsonian approach. No obstacles for precision calculations of truly inclusive short-distance observables

Need calculation of the perturbative corrections to the Wilson coefficients of power-suppressed operators (μ_π^2 , μ_G^2 , ρ_D^3)

This becomes a limiting factor

Kinetic value μ_π^2 emerges as theoretically expected

Does the precise value matter?

Precision m_b and μ_π^2 are instrumental for high accuracy in V_{ub} through the restrained inclusive rates

external slides \implies

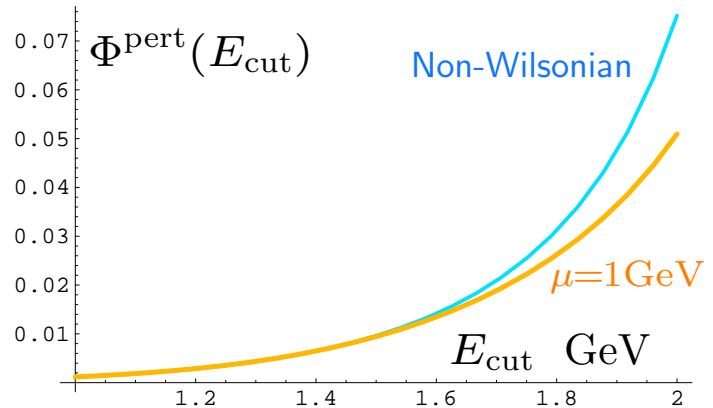
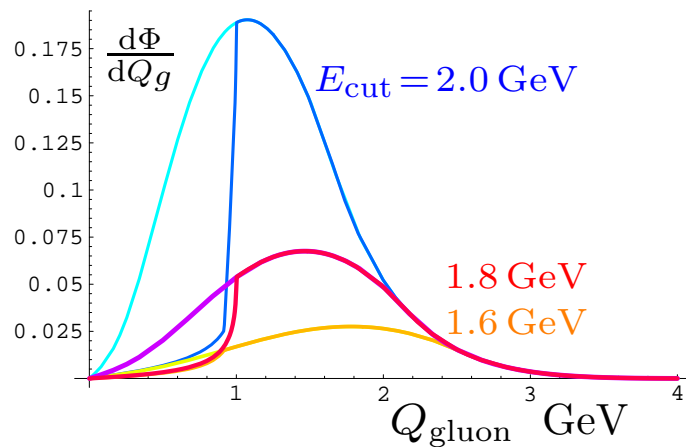
Values extracted from inclusive $B \rightarrow X_c \ell \nu$ can be used to constrain the $b \rightarrow u (s)$ distributions moments

Relevance is illustrated by our recent $b \rightarrow s + \gamma$ analysis

There is interesting physics indeed in $Q \rightarrow q$ when perturbative corrections are included!

SCET treatment is questionable

In the framework of the Wilsonian approach we can reliably evaluate the decay fraction $\Phi_\gamma(E_{\text{cut}})$ rejected by the cut on E_γ at known values of the HQ parameters extracted elsewhere

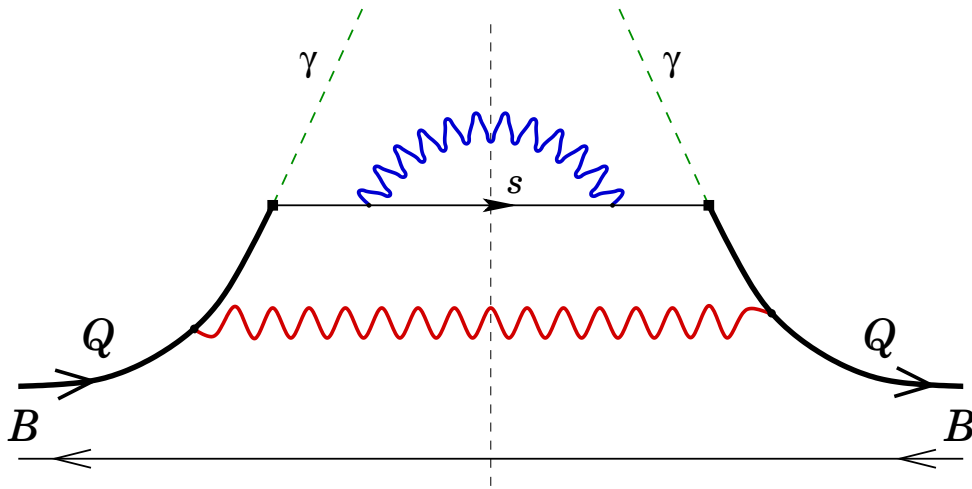


There are different schemes for masses, ... proposed for use in the analyses. None except the 'kinetic' (or 'SV' scheme) is satisfactory from theory viewpoint

Leaving aside theoretical fidelity, still the practical aspect: Such an accurate analysis for, say $b \rightarrow s + \gamma$ is not possible in the '1S scheme' or 'Υ-expansion'

Is this behind the claims of the radical difference in OPE with $b \rightarrow c$?

бэсККур



Soft gluons $|k_\mu| \lesssim \mu_{\text{hadr}}$ are included into HQ distribution function $F(x)$ (Fermi motion). Other, hard gluons are in the Wilson coefficients (kernel)

$$dW^{\text{pert}} = \int \frac{d\omega}{\omega} \int \frac{dk_\perp^2}{k_\perp^2} C_F \frac{\alpha_s(k_\perp^2)}{\pi} dW_{\text{born}} ,$$

However, 'hard gluons' may be energetic yet highly collinear, then $k_\perp \lesssim \mu_{\text{hadr}}$, in this case they are in fact *nonperturbative*

$$k_+ \sim \frac{k_\perp^2}{|\vec{k}|}$$

Nevertheless,

the integer moments of M_X^2 or E_γ are not affected by strong-coupling domain. They still are given by local heavy quark operators, plus genuinely short-distance perturbative corrections

N.U. hep-ph/0407359

Physics behind: growth of α_s is due to final state interactions

Yet the nonperturbative physics of an effective theory with only soft and collinear modes *is different* from actual QCD

some versions of SCET may be bogus

Some responses

Moment analysis

Uraltsev has recently argued that a lepton cut in $b \rightarrow c$ decays could potentially lower the effective "hardness" of the transition to values much smaller than m_b . If true, that would mean that we would have to rethink the naive OPE analysis of these moments.

We do not need, since this has been done, as follows from our papers

My questions are:

- Can the "effective hardness" Q be defined more precisely?

It has been ('02). Generally $Q \lesssim \omega_{\max}$ with ω_{\max} the threshold energy at which the process disappears if $m_b \rightarrow m_b - \omega$

In semileptonic decays

$Q \simeq m_b - E_{\min} - \sqrt{E_{\min}^2 + m_c^2}$
In practice the precise definition matters when Q is pushed down to a hadron scale, then it is more reasonable to define it using the hadron kinematics

- Does a lepton cut imply a hard upper cutoff on hadronic invariant mass?

Trivially, $M_X^2 \leq M_B^2 - 2M_B E_\ell^{\text{cut}}$, but of course

$$\theta(E_\ell - E_\ell^{\text{cut}}) \not\leftrightarrow \theta(M_B^2 - 2M_B E_\ell^{\text{cut}} - M_X^2)$$

Possibly, I misunderstood the question?

- Is the situation comparable to the $B \rightarrow Xs$ gamma moments, or is it different?

comparable?... Yes and no. Yes in the first respect, No in the second:
 $\theta(E_\gamma - E_\gamma^{\text{cut}}) \iff \theta(M_B^2 - 2M_B E_\gamma^{\text{cut}} - M_X^2)$

- Do power corrections scale like $(\Lambda/Q)^n$, or $(\Lambda/m_b)^n$?

Both are present. The point is appearance of the corrections that scale like μ_{hadr}/Q ; they can come with some powers of μ_{hadr}/m_b , or with $\alpha_s(m_b)$ or $\alpha_s(Q)$. There are well known examples, e.g. SV inclusive kinematics, or even $B \rightarrow D^{(*)}$ near zero recoil. The latter case is not 'hard', $Q \lesssim \mu_{\text{hadr}}$, yet the corrections are $0 \cdot 1 + 0 \cdot \frac{1}{m_b} + \frac{1}{m_b^2}(1 + 1 + 1 + \dots)$

Shape function:

The problem of deriving consistent, factorized expressions for inclusive spectra in the shape-function region, which are valid at NLO in perturbation theory, was recently solved in work by Bauer, Manohar and our group (Bosch, Lange, Neubert, Paz).

I disagree

At this order it is imperative to understand the renormalization properties of the shape function. This is needed for a consistent matching, and for a proper treatment of RG evolution effects.

A result of these studies was that, beyond tree level, the spectra are not given by a simple convolution of parton-model spectra with a "primordial" distribution function.

As a general statement, is trivial. But the concrete thing alluded to is in my opinion incorrect

Therefore, the naive treatment of Bigi-Uraltsev, Neubert-De Fazio, Kagan-Neubert etc. is not really correct beyond tree level.

Depends on what is understood by this level.

What has been treated, is correct

While I believe there can be very little doubt about the correctness of these papers (they agree with each other) (a poor argument), Bigi and Uraltsev still maintain that they have a "different scheme" of implementing shape function effects in a consistent way. Yet, this scheme is never defined in the literature. My questions are:

- How is the shape function defined in this scheme? (I really want an operator definition.)

$$F(k) = \int_{-\infty}^{\infty} \frac{dt}{2\pi} e^{ikt} \frac{1}{2M_B} \langle B | \bar{b}(nt) P \exp i \int_0^{nt} A_\mu dx^\mu b(0) | B \rangle$$

$n^2 = 1, |n| = 1$

Are there alternatives?

- What is the evolution equation obeyed by the shape function, and what is its anomalous dimension?

$$\frac{d}{d\mu} F(k; \mu) = \int dk' \frac{\alpha_s(\mu)}{\pi} g(k-k'; \mu) F(k'; \mu)$$

The concrete form of $g(k; \mu)$ depends on the scheme. As a general feature, it is singular at $k \rightarrow 0$ (has $1/k$ pieces), decrease fast at $k > \mu$. It does not vanish at $k > 0$

Anomalous dimensions are not too relevant things since the running is not logarithmic (similar to $\bar{\Lambda}(\mu)$, $\mu_\pi^2(\mu)$, ...)

- What is the evolution equation satisfied by the remaining hard part of the amplitude (the part that is convoluted with the shape function)?

$$\frac{d}{d\mu} \int dk' \frac{d\Gamma^{\text{pert}}(k-k'; \mu)}{d\mu} F(k'; \mu) = 0$$

The explicit form to order α_s and to all the BLM corrections, for the kinetic-type scheme can be read off from the Appendix in Benson et al., hep-ph/0410080. For instance, $\int dk g(k; \mu) = 0$, $\int dk \frac{\alpha_s}{\pi} k g(k; \mu) + \frac{dm_Q(\mu)}{d\mu} = 0$ (if m_Q is what enters the equation of motion for Q regularized in the same way, which we assumed), etc.

- What is the asymptotic (large-omega) behavior of the shape function in that scheme, and how is it derived?

Speaking naively, $F(k) \sim e^{-c(\mu)k/\mu}$ at $k \ll \mu$. Derived from the above evolution

naive is a good level, the SCET one would be *supernaive* in this nomenclature

These are prerequisites. Unless such properties are respected, the problem is not solved whatever is claimed, or one is left without the OPE. In this case I doubt one can say anything definite for nonperturbative effects

As long as such regularization exists and its physical properties are understood, the explicit form does not play a crucial role. Our perturbative calculations are approximations to something definite, and are used as it is normally done for other problems

to some of the Questions 4 theorists

4. Can the shape function moments for $b \rightarrow sg$ and/or $b \rightarrow ulnu$ be predicted from HQE parameters determined in $b \rightarrow clnu$ decays? If not, what prevents this? If the predicted moments match the data, can we use the $b \rightarrow clnu$ parameters in determinations of $|V_{ub}|$?

Yes. Should use.

5. Is there consensus on the necessity of “bias corrections” to $b \rightarrow sg$ moments (for E_{cut} in the range 1.8 to 2.0 GeV)?

These corrections to the naive OPE expressions do exist and I'm confident they are of the estimated scale. Biases are actual moments minus naive OPE expressions neglecting the cut over the tail of $F(k)$. One can do the same as we do first – obtain the actual moments – without mentioning the word ‘bias’. This is what is behind the ‘alternatives’

8. The experimental determination of spectral moments in semileptonic and radiative B decays will continue to improve. What improvements are needed in the calculations to keep pace? Which uncertainties do we not know how to reduce?

Need α_s -corrections to the power-suppressed Wilson coefficients
The effects of insufficiently low cuts
Adding to the point – what about higher hadronic moments or any related measurement?

10 What can we expect from SCET? What are the most pressing issues (conceptual and/or practical) to resolve in the near term?

The question to SCET. I said I'm somewhat skeptical that such a theory can be formulated as a complete field theory (back to 2002 Workshop here at SLAC). It depends on what one understands by this effective theory, if only counting rules, then ...

11 For which processes ($b \rightarrow clnu$, $b \rightarrow ulnu$, $b \rightarrow sg$) is it most useful to have separate measurements for B^0 and B^+ , taking account of the reduced experiment accuracy available in these tagged measurements? How can we use these measurements to quantify uncertainties due to Weak Annihilation?

In the SL decays, for $b \rightarrow u$. Note that this only addresses WA in the difference between B^+ and B^0 , possibly constrains B_s

12. Quark-hadron duality: how can we probe its region of validity? How should we treat the regions (e.g. at low hadron mass in $b \rightarrow sg$ or $b \rightarrow ulnu$) where it clearly breaks down? How would you assign an uncertainty?

Duality violation is typically difficult to detect, with rare exceptions: it is below the uncertainties due to unaccounted OPE effects (α_s^k , $1/m_b^4$, ...). In general, observation of oscillations, but not always easy in practice. It is normally negligible where a few resonances can contribute.

Low- M_X contribution is not really duality-violating piece – its integrated effect is known, say for $b \rightarrow s + \gamma$; for $b \rightarrow u \ell \nu$ it is given by $\langle B | \bar{b}u \bar{u}b | B \rangle$.

Clinton, when asked if he had intimate relationship with Lewinski:
"It depends on what you mean by intimate relationship."

9. How can the uncertainties associated with sub-leading SFs be quantified? Where could we expect to see an experimental indication of the importance of sub-leading SFs?

Rather a question in return: one may hope to get insights assuming reasonable physical properties (normalization, positivity etc.). Yet if in the SCET approach this is not the case already for the leading 'Shape function', what we can count on? In our approach we at least use these physical constraints automatically, which helps

6. How big are the leading uncertainties in the BF and moments for $b \rightarrow sg$ and what is their source?

Not as large as MN conjectured, assuming the HQ parameters are known. For the Br, I believe the uncertainty is subdominant to that of the 'total' rate. It is partially illustrated in the tables and can be further studied playing with parameters as is done with $b \rightarrow c \ell \nu$. I think the $b \rightarrow c \bar{c} s$ represents the brick wall. This part is not controlled by the OPE. In other words, here quark-hadron duality is used which does not rest on the OPE

7. Despite its known shortcomings, we still use the DeFazio-Neubert model (order α_s triple differential decay rate convoluted with a SF ansatz) to model our data in the M_x , q^2 - M_x , E_e and q^2 - E_e . We do this because we need to construct a realistic Monte Carlo generator with which to evaluate our experimental efficiencies. Are there viable alternative approaches? How would you quantify the theoretical uncertainty associated with this approach? Same questions regarding the use of the Kagan-Neubert model for $b \rightarrow sg$.

I maintain there are no conceptual problems here. Many concrete elements can or should be improved

Viable alternatives – we suggested applying the same approach

as we did in $b \rightarrow s + \gamma$. Quantifying theoretical uncertainty requires first playing with numbers varying obvious things like shape, effect of the higher-order terms, etc. Plus additional care!

1. Please list a set of measurements that would, when combined, result in the most precise and reliable determination of $|V_{ub}|$ with the current available data.

Depends on what is available / realistic. A responsible answer requires dedicated play with numbers within the good approach. I have not yet done this for many things. But this is feasible

3. The inclusive endpoint spectrum for $b \rightarrow ulnu$ decays has now been measured down to 2.0 GeV. How important would it be to reduce still further the E_e cut? How does this lower E_e cut impact the extraction of V_{ub} from the ratio of integrals of the lepton spectrum from $b \rightarrow ulnu$ and the photon spectrum from $b \rightarrow s \text{ gamma}$? Assuming we can measure the full E_e spectrum, can we extract the leading SF in $b \rightarrow ulnu$ decays?

Requires dedicated numerical analysis

Lower E_{cut} softens or eventually eliminates the need to obtain the shape from $b \rightarrow s + \gamma$, eventually requiring it only for the indirect cross checks like values of m_b , μ_π^2 , limits on the tails

hep-ph/0202175, p.14, Eq. (19):

Effect $1 - \frac{11}{6} \frac{\mu_G^2}{m_b - 2E_{\text{cut}}}$ may help to quantify

Even with the full spectrum the accuracy is limited for the distribution function, by m_b and not by the cut. But why would you need the leading SF?

2. What are the best ways of determining the uncertainty on $|V_{ub}|$ due to the HQE and SF? How big are these uncertainties and what is their source? What other sources of uncertainty on $|V_{ub}|$ need to be considered? No man pages for this question

RECYCLE

Predictions are more definite than in the analysis by MN

hep-ph/0408208, 179

There are both conceptual and technical differences

In fact, no need in 'Soft-Collinear Factorization' here; usual 'soft' factorization – Wilsonian OPE – is sufficient (small distances vs. large distances)

Bonsai OPE in SCET yields a loose unphysical shape function. Its properties reportedly are related to the true Wilsonian expectation values measured, e.g. in $b \rightarrow c \ell \nu$. In practice such relations are too crude and can hardly be improved

Speaking practically, $H \cdot J$ is simply the (Wilsonian) perturbative spectrum; it is known how to calculate it. That *has been done*

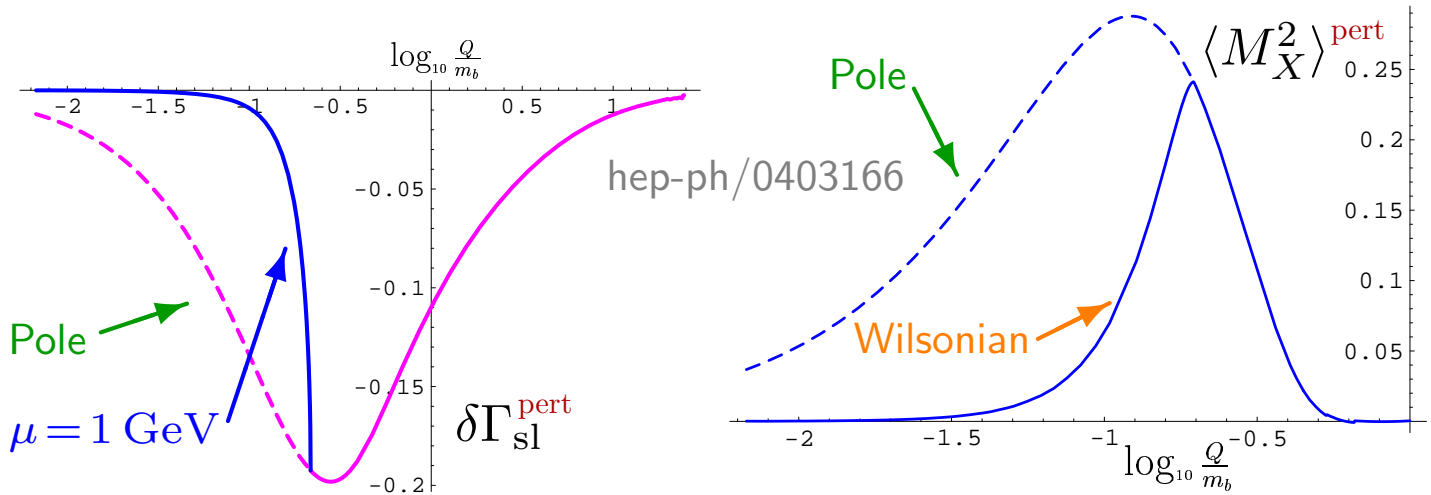
The moments of the Wilsonian heavy quark distribution functions $F(k_+)$ are directly related to measured moments in $b \rightarrow c \ell \nu$, no significant uncertainty here

The $1/m_b$ corrections have been automatically included, KLN-type relations respected, ...

The required anomalous dimensions, BLM corrections, 'SCET matching coefficients' (their actual part) are all contained in the full perturbative calculation. In the language discussed yesterday the correlations between α_s at different scales are automatically respected. Scales for α_s is resolved without the SCET ambiguity (e.g., $\alpha_s(\Delta)$ or $\alpha_s(2\Delta)$?)

The reported calculations are in all respects superior theoretically to the SCET-based evaluations, and are expectedly more accurate. Their further scrutiny is straightforward

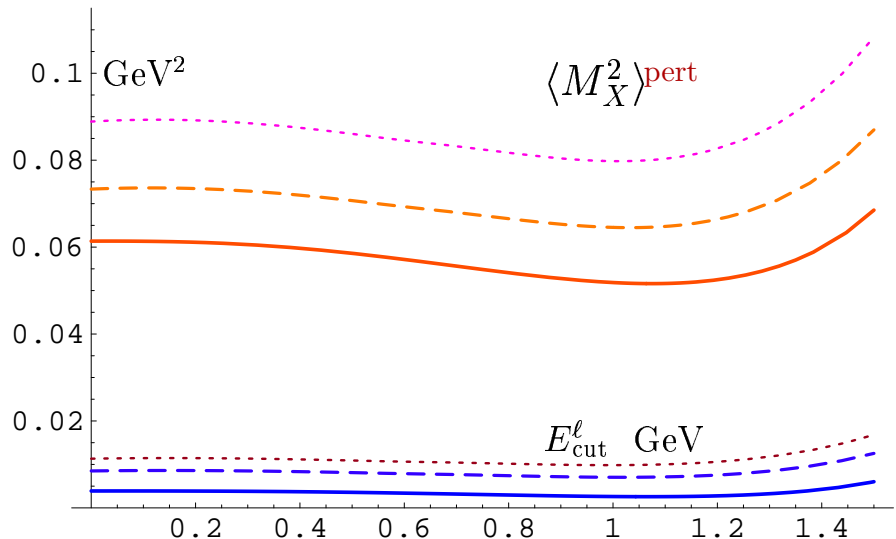
Perturbative corrections? ...



With the IR piece cut off according to Wilson we can work for precision!

Can a similar analysis be done in 'Y expansion' ('1S scheme')?

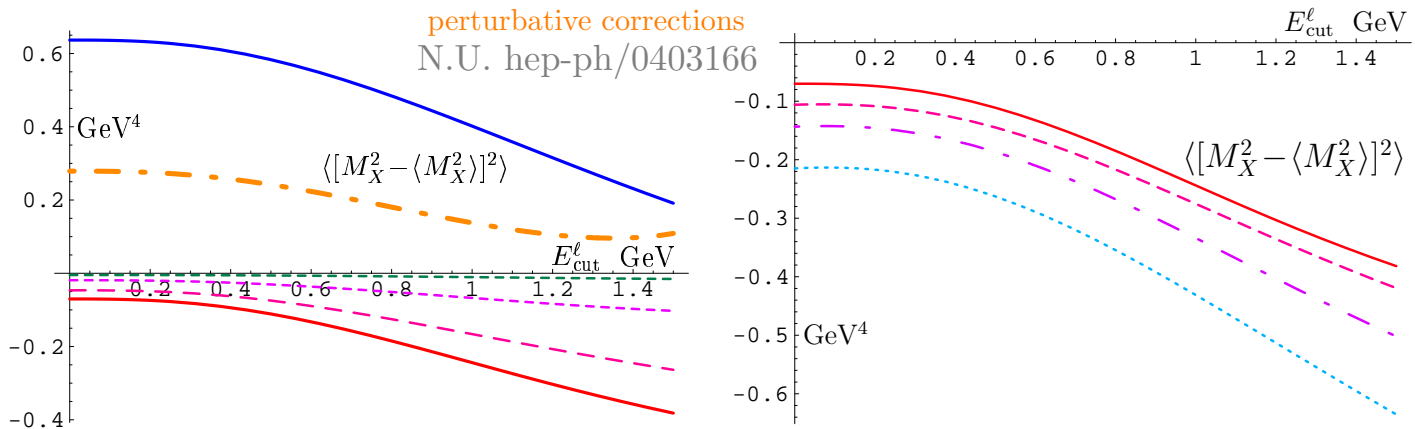
Corrections in the scheme with the hard cutoff, $\mu = 1 \text{ GeV}$. Within pole-type approaches the correction is 4-6 times larger and strongly decreases at larger E_{cut}^ℓ



Attn Z.L.: $\langle M_X^2 \rangle^{pert}$ does not depend on E_{cut} in the Wilsonian scheme, and in the pole scheme decreases for higher E_{cut}

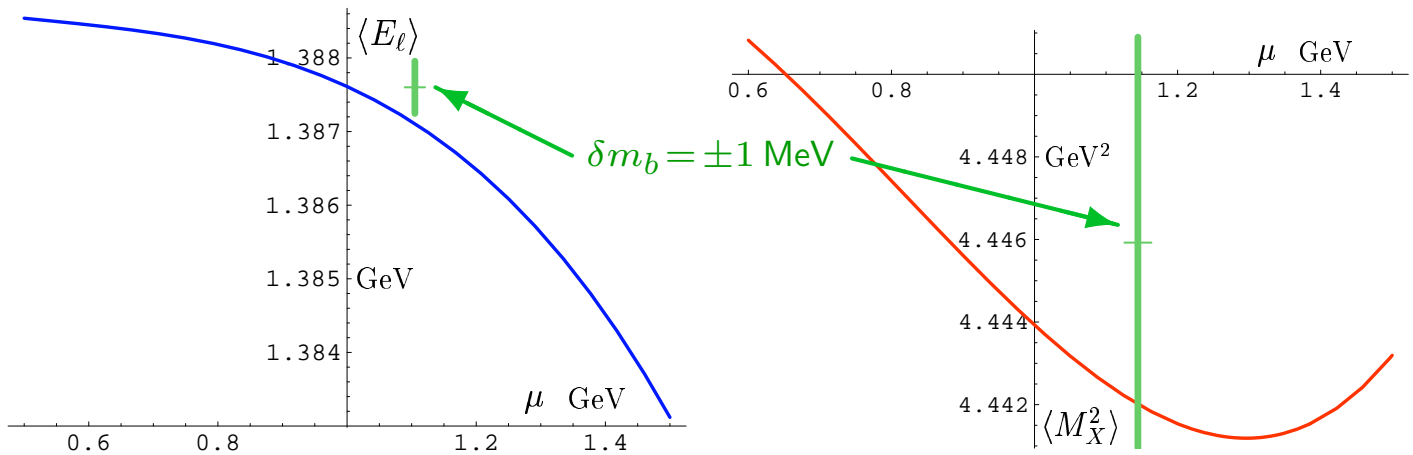
Now₂₀₀₄ all pure perturbative corrections have been calculated
N.U., P. Gambino; M. Trott

Perturbative corrections yield decreasing $\langle M_X^4 - \langle M_X^2 \rangle^2 \rangle$
 contrary to Bauer *et al.*



Bauer *et al.*, hep-ph/0408202:

Do not reproduce our predictions for hadronic moments
 Claim large 'scale-dependence' in our Wilsonian scheme
 Claim we underestimate theory uncertainty by up to 10 times



For other moments μ -dependence is even less significant

Suppressed μ -dependence ($\propto \alpha_s^2$) is a cross-check of algebra...

Power counting by Bauer *et al.* do not respect Wilsonian scale-independence
 holds in our scheme

'1S scheme': Use ' $m_b(\eta_b)$ ' instead of ' $m_b(1S)$ ', nothing changes
 yet $\delta m_b \simeq -25$ MeV much stronger scale dependence!

“ $\Upsilon(1S)$ ” mass and “ Υ -expansion”:

$$m_b(1 \text{ GeV}) \text{ appeared (1995) about } 4.6 \text{ GeV} \approx \frac{M(\Upsilon(1S))}{2}$$

As predicted by the OPE, it significantly improved perturbative expansion N.U. 1995

It was then (1998) tempting to define

$$m_b^{1S} \equiv \frac{M(\Upsilon(1S))}{2} = 4.730 \text{ GeV}, \quad \text{often } \frac{M^{\text{pert}}(\Upsilon(1S))}{2}$$

$M_{\Upsilon(1S)}$ in principle can be computed perturbatively in terms of fundamental short-distance mass, up to

$$\delta_{\text{np}} M_{\Upsilon(1S)} \sim 100 - 200 \text{ MeV}$$

Such mass is an observable, not a genuine short-distance mass. Its ‘hardness’ $\sim 1/R_{\Upsilon(1S)}$

$$\text{Physically } m_b^{1S} \approx m_b(R_{\Upsilon(1S)}^{-1})$$

$$R_{\Upsilon(1S)}^{-1} \sim m_b \cdot \alpha_s(\alpha_s m_b)$$

$$\text{hence } m_b^{1S} \sim m_b^{\text{pole}} - \alpha_s^2 m_b \quad \text{no } \beta_0!$$

$$m_b^{1S} = m_b^{\text{pole}} \left[1 - C_F^2 \frac{\alpha_s^2}{8} + \mathcal{O}(\alpha_s^3, \beta_0 \alpha_s^3 \ln \alpha_s) \right]$$

No term $\propto \alpha_s$, expansion runs in $\alpha_s(\alpha_s(m_b) m_b)$

This makes it an unsuitable object perturbation theory-wise for B physics

Following the rules of QCD perturbation theory, in all equations used in the analyses then $m_b^{1S} = m_b^{\text{pole}}$

“Upsilon expansion” was forged to ‘fix’ this

' Υ expansion' employs ad hoc reshuffling of different orders in perturbation theory motivated by a posteriori numerical observations. It would make no sense already in QED, say for μ -decay

One cannot make up for the numerically larger than $\alpha_s(m_b)$ value of $\alpha_s(Q)$ at the smaller momentum scale $Q = R_\Upsilon^{-1} \sim 1 \text{ GeV}$ by equating at will terms of different orders in α_s

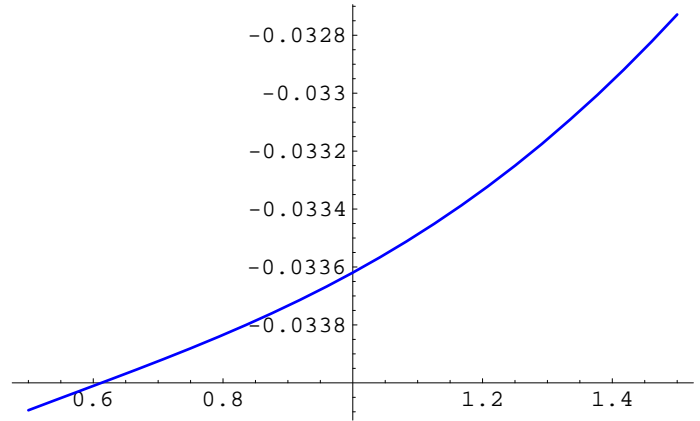
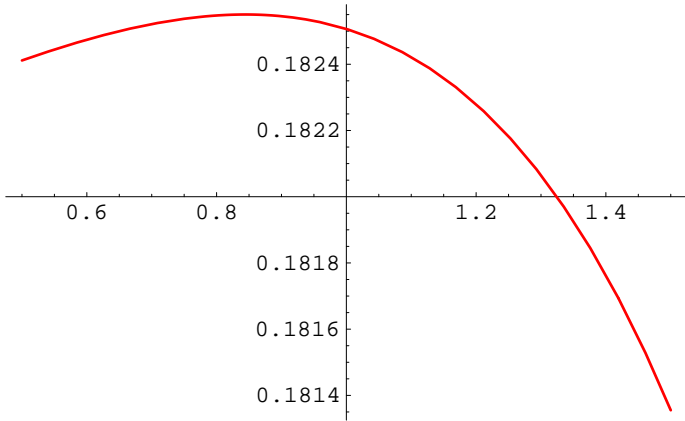
Introduces 'parameter' $\epsilon = 1$; since for such $\epsilon^k = 1$ for any k , powers of ϵ can be placed arbitrarily at any point. Introducing ϵ^k differently for different series Υ expansion mandates counting powers of α_s together with powers of ϵ . This means nothing but reshuffling the coefficients between different powers of α_s

In Coulomb bound-state problems meaning of ϵ is factor $1/v$

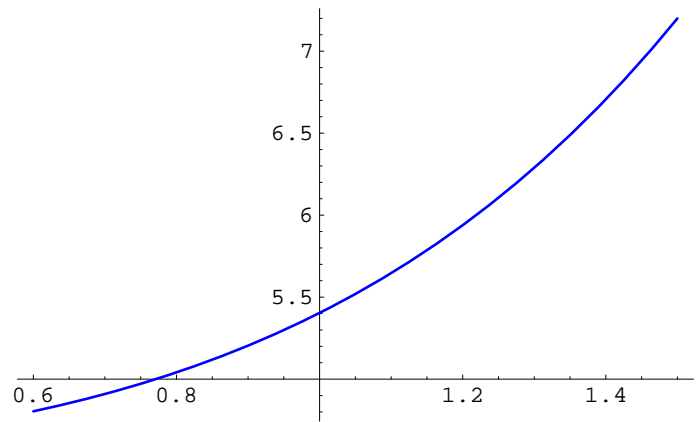
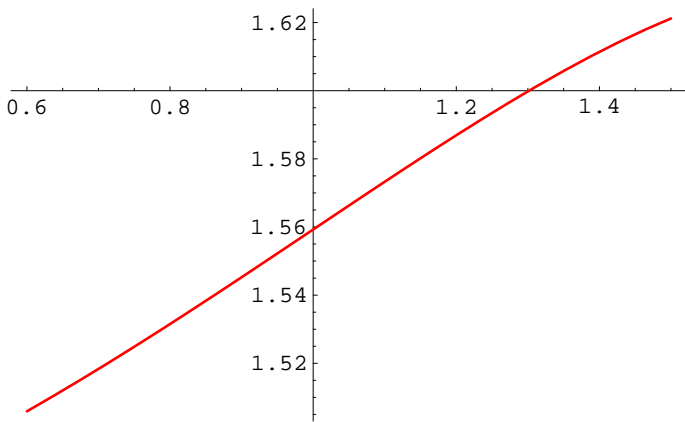
The scale $\alpha_s m_Q$ naturally appears in bound-state problems for heavy quarks since the perturbative expansion parameter for nonrelativistic particles is not necessarily α_s , but rather α_s/v , where v is their velocity. A 'bound-state' mass naturally appears there: powers of velocity make up for the missing powers of α_s . Nothing similar happens in B mesons or in their decays, the ϵ parameters introduced by the ' Υ expansion' is unity and can be placed ad hoc anywhere

More on 'scale-dependence' in our scheme

Higher lepton energy moments:



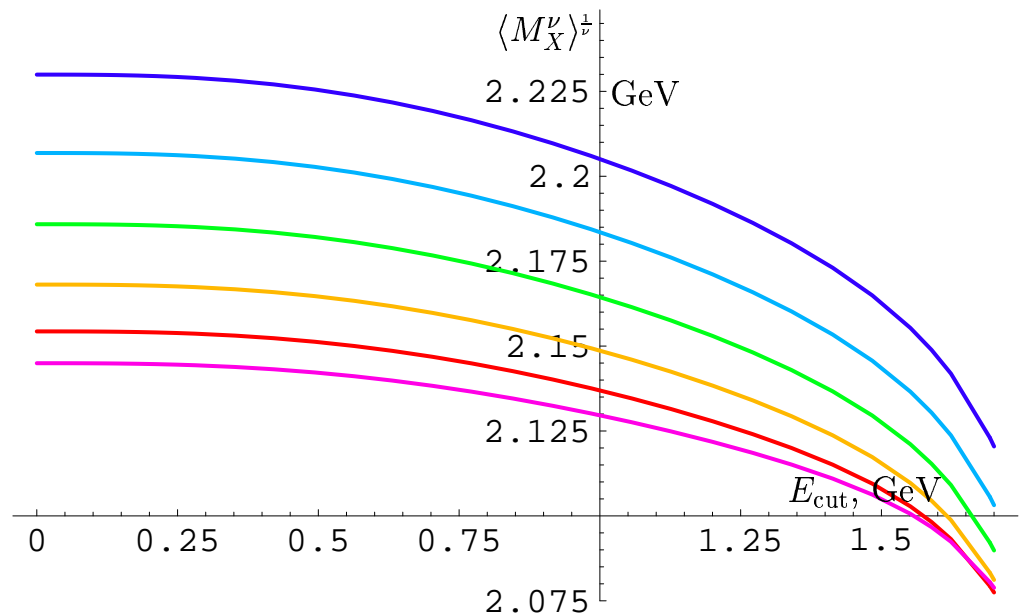
Higher hadron mass squared moments:



They do not arise naturally in the $1/m_b$ expansion, as illustrated by the limit $m_c \rightarrow 0$ which is analogous to the decay $B \rightarrow X_s + \gamma$; fractional photon energy moments are not given there by the expectation values of local heavy quark operators. For $B \rightarrow X_c \ell \nu$, the OPE would involve an expansion in $1/m_c$, as can also be seen from

$$\langle M_X^\nu \rangle = (\langle M_X^2 \rangle)^{\frac{\nu}{2}} \left[1 + \sum_{k=2}^{\infty} C_{\frac{\nu}{2}}^k \frac{\langle (M_X^2 - \langle M_X^2 \rangle)^k \rangle}{\langle M_X^2 \rangle^k} \right];$$

for integer moments with $\nu = 2n$ the sum contains only terms through $k=n$ and stops before $\langle M_X^2 \rangle$ enters the denominator.



Different hadronic mass moments $\langle M_X^\nu \rangle^{1/\nu}$ for $\nu = 1$ to 6 (from lowest to highest), vs. lepton energy cuts

NB to Bauer, Ligeti et al.: If you add or subtract 0.5 GeV^3 from $\langle M_X^3 \rangle$, you would get

$$[\langle M_X^3 \rangle]^{4/3} > \langle M_X^4 \rangle \quad \text{or} \quad [\langle M_X^3 \rangle]^{2/3} < \langle M_X^2 \rangle$$

Does criticism of G & U justify violating algebraic inequalities?