Theory Summary

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TOP2015, Ischia, 18 September 2015

At the conference

- Inspiring introduction by M. Peskin
- Review of boosted top methods (Spannowsky)
- Generators for top physics at the LHC (Re)
- Quark properties (Schulze)
- Single top in NLO+PS framework (Papanastasiou)
- New leptonic observable for top mass measurement (Sayaka Kawabata)
- Theoretical problems with top mass determination (G. Corcella)
- $t\bar{t}H$ and tH (M. Zaro)
- Vacuum stability (Espinosa)
- BSM top and Higgs interactions (De Andrea)
- Differential NNLO (Heymes)
- EW corrections to top physics (Uwer+Pagani)
- High $m_{t\bar{t}}$ log resummation (Pecjak)
- PDF's (Thorne)

All very interesting, lots of new results.
I will pick only a few topics, based upon

- Recognized importance (NNLO distributions for $t\bar{t}$)
- Personal taste (`NLO+PS` for resonances)
- Whether I have something to say about it (pole mass)

Outline

- General remark about where we stand now
- Top mass issues
- NLO+PS generators including decay of resonances into coloured objects and interference effects
- NNLO differential distributions

Where we stand now

From talks By Peskin, De Andrea:

- Top quark physics has a lot of potential for new physics discoveries:
  - Top loop in Higgs propagator: who's canceling it, if anything at all? urgent need for top partners (whether from Susy or other sources)
  - Top abundance at LHC invites us to search for rare processes, constrain anomalous couplings, etc. (talk by M. Shulze).

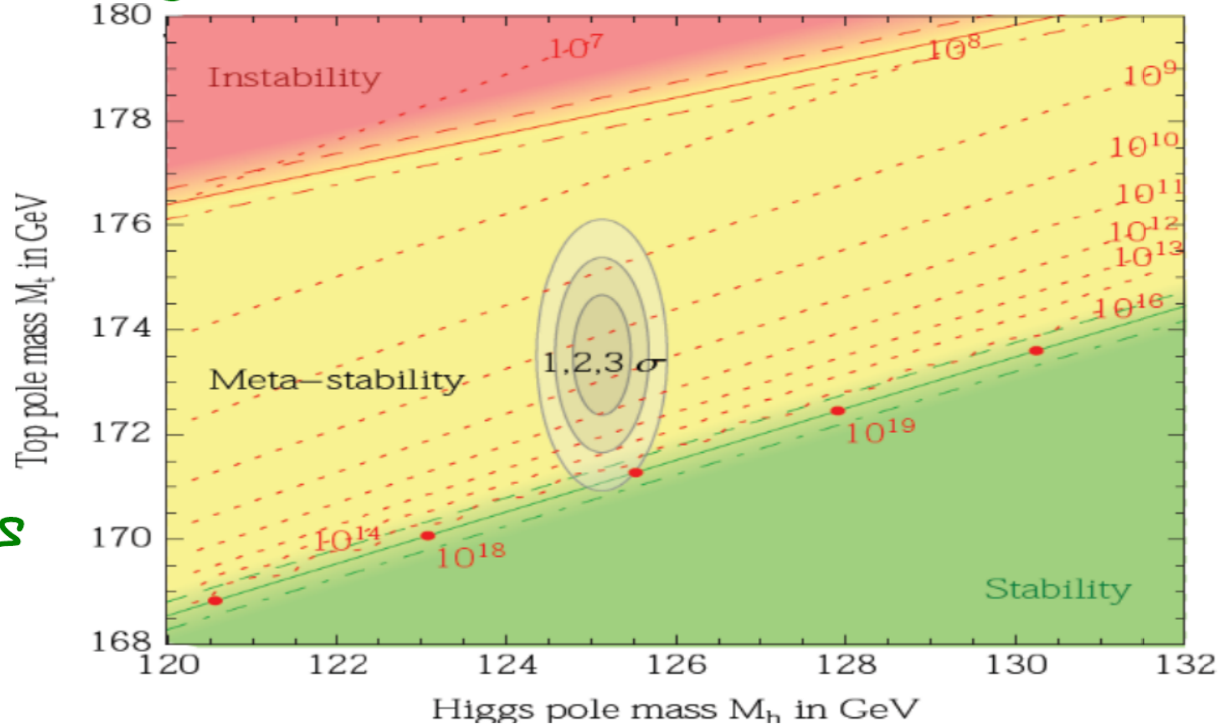
From Espinosa talk:

AiM OF THE TALK

Degrassi et al '12, Buttazzo et al. '13

Explain this plot →

- Ingredients
- Assumptions
- Implications



With emphasis on the crucial role of Mtop

Studies prompted by 2 facts:

- Higgs discovery
- No new physics signals

The possibility that the Standard Model may hold up to very high energy, or at least that new physics may be much more economical than we would have liked becomes an option.

So:

- If we have new physics nearby, very likely to be "near" the top and Higgs.
- If there are no new physics signals, the top and Higgs are still trying to tell us something. So: measure the top mass AND look for new physics.

Top mass

Marquard,A.V.Smirnov,V.A.Smirnov,Steinhauser, Feb.2015:

$$m_{t,\text{pole}} = m_{t,\overline{\text{MS}}}\,(1 + 0.4244\,\alpha_s + 0.8345\,\alpha_s^2 + 2.375\,\alpha_s^3 + (8.49 \pm 0.25)\alpha_s^4)$$

$$m_{b,\text{pole}} = m_{b,\overline{\text{MS}}}\,(1 + 0.4244\,\alpha_s + 0.9401\,\alpha_s^2 + 3.045\,\alpha_s^3 + (12.57 \pm 0.38)\alpha_s^4)$$

For the top formula, α_s is the 6-flavour $\alpha_s(m_{t,\overline{\text{MS}}})$.

Pole mass affected by IR renormalons:

$$m_{t,\text{pole}} = m_{t,\overline{\text{MS}}}\left(1 + \sum_{n=0}^{\infty} r_n \alpha_s^{n+1}\right),$$

For large n (Beneke,Braun,1994; Beneke 1994): $(\partial\alpha_s/\partial\log\mu^2 = -b_0\alpha_s^2 - b_1\alpha_s^3\ldots)$

$$r_n \to N\, m_t\,(2b_0)^n\,\Gamma(n+1+b)\left(1 + \sum_{k=1}^{\infty} \frac{s_k}{n^k}\right), \qquad b = \frac{b_1}{b_0^2}\,,$$

REMINDER: Factorial growth $\Longrightarrow$ Power Corrections

Factorially divergent perturbative expansion: breaks down when $(2b_0)^n \alpha_s^n n!$ for some n stops decreasing:

$$\frac{(2b_0)^{n+1}\alpha_s^{n+1}(n+1)!}{(2b_0)^n \alpha_s^n n!} \approx 1 \Longrightarrow (2b_0)\alpha_s n \approx 1 \Longrightarrow n \approx \frac{1}{2b_0 \alpha_s}$$

Size of the last good term (using $n! \approx n^n e^{-n}$ and $\alpha_s = \frac{1}{b_0 \log \frac{\mu^2}{\Lambda^2}}$):

$$(2b_0)^n \alpha_s^n n! \approx \underbrace{(2b_0)^n \alpha_s^n n^n}_{\approx 1} e^{-n} \approx \exp(-n) = \exp\left(-\frac{1}{2b_0\, \alpha_s}\right) \approx \frac{\Lambda_{\rm QCD}}{\mu}$$

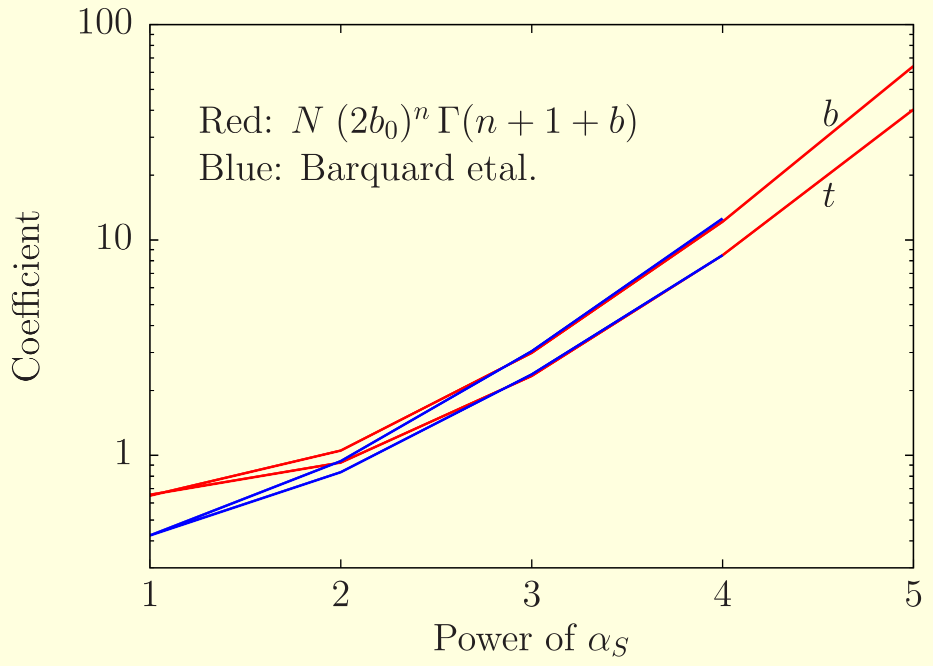
From Beneke, 1994: exactly as above, no extra powers of $\log \frac{\mu}{\Lambda}$;
(easy to check using the two loop α_s and Stirling relation in Beneke's formula)

Normalization not determined.

Does Beneke's formula show consistency with the calculation of Marquard etal?

Fitting α_s^4 coefficient with Beneke's formula, we get $N = 0.726$, and fit well α_s^3 coeff. for t and b, and α^4 for b,

Can use it to predicts $c_j,\ j \geqslant 5$!!!



Assuming $\alpha_s = 0.1088$, we get $\mathcal{O}(\alpha_s^5)$ contribution:

$$M_{\rm pole} = 163.643 + 7.557 + 1.617 + 0.501 + 0.195 + 0.10\,\text{GeV}$$

The terms in the perturbative expansion reach their minimum at order $8 \sim 9$, with last correction ≈ 0.043 GeV.

Alternatively: $\Lambda_6 = 0.094$, $N\Lambda_6 = 0.068\,\text{GeV}$ can be considered an estimate of the renormalon ambiguity in the determination of the pole mass.

Comments

- IR Renormalons come up in most QCD computed quantities:
  - Total cross section for $Z/\gamma^\star \to \mathrm{hadrons}$ ($\frac{\Lambda^4}{Q^4}$ effects)
  - DIS ($\frac{\Lambda^2}{Q^2}$ effects)
  - Jets ($\frac{\Lambda}{Q}$ effects; bound to affect top mass determination)
  - renormalon in $m_{t,\mathrm{pole}}$: ultimate limit in precision for pole mass
- They appear as a consequence of our inability to treat long distance phenomena in QCD.
- Besides the formal treatment, also intuition takes us there:
  In the case of the top mass, the fact that the top, being coloured, cannot concievably exist if not surrounded by other coloured particles gives an intuitive argument towards the presence of a mass ambiguity of order Λ.

What can we do about renormalons?

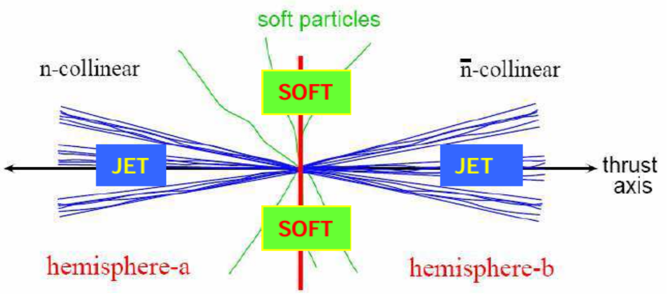
Almost by definition, renormalons are smaller than perturbative corrections.

But this is not always the case:

If we compute perturbatively the maximum energy of the be^+ system as a function of the pole mass, we get $E_{be^+}^{\max} = m_{t,\mathrm{pole}}$ with no perturbative corrections (if we use instead $m_{t,\overline{\mathrm{MS}}}$ we do get of course large perturbative corrections). But we know, both intuitively and in a more formal sense, that we have an error due to a renormalon (although, presumably, as shown earlier, a fairly small one).

From Gennaro's talk: Hoangs attempt using SCET:

Attempt to address the MC mass using the SCET formalism $Q \gg m_t \gg \Gamma_t \gg \Lambda_{\rm QCD}$



Factorization theorem $(e^+e^- \to t\bar{t})$: A.H. Hoang and I.Stewart '08

$$\frac{d\sigma}{dM_t^2 dM_{\bar{t}}^2} \sim H_Q(Q,\mu_m) H_m\left(m, \frac{Q}{m}, \mu_m, \mu\right) \int d\ell^+ d\ell^- B_+\left(\hat{s}_t - \frac{Q\ell^+}{m}, \Gamma, \mu\right) B_+\left(\hat{s}_{\bar{t}} - \frac{Q\ell^-}{m}, \Gamma, \mu\right) S(\ell^+, \ell)$$

H_Q, H_m: hard scattering; $B_\pm$: jet function; S: soft function

Jet mass: short-distance (resonance) mass with $R \sim \Gamma_t$; $Q_0 = 3^{+6}_{-2}$ GeV shower cutoff

$$m_J(\mu) \sim \left[\frac{d \ln \tilde{B}(y,\mu)}{dy}\right]_{y=-ie^{-\gamma_E}/R} \Rightarrow m_{\rm pole} = m_J(\mu) + e^{\gamma_E}\Gamma_t \frac{\alpha_S(\mu) C_F}{\pi}\left(\ln\frac{\mu}{\Gamma_t} + \frac{1}{2}\right) + \mathcal{O}(\alpha_S^2)$$

MC mass is interpreted as $m_J(Q_0) \simeq 173$ GeV and then R-evolved to $\bar{m}(\bar{m}) \simeq 163$ GeV

– Typeset by FoilTEX –

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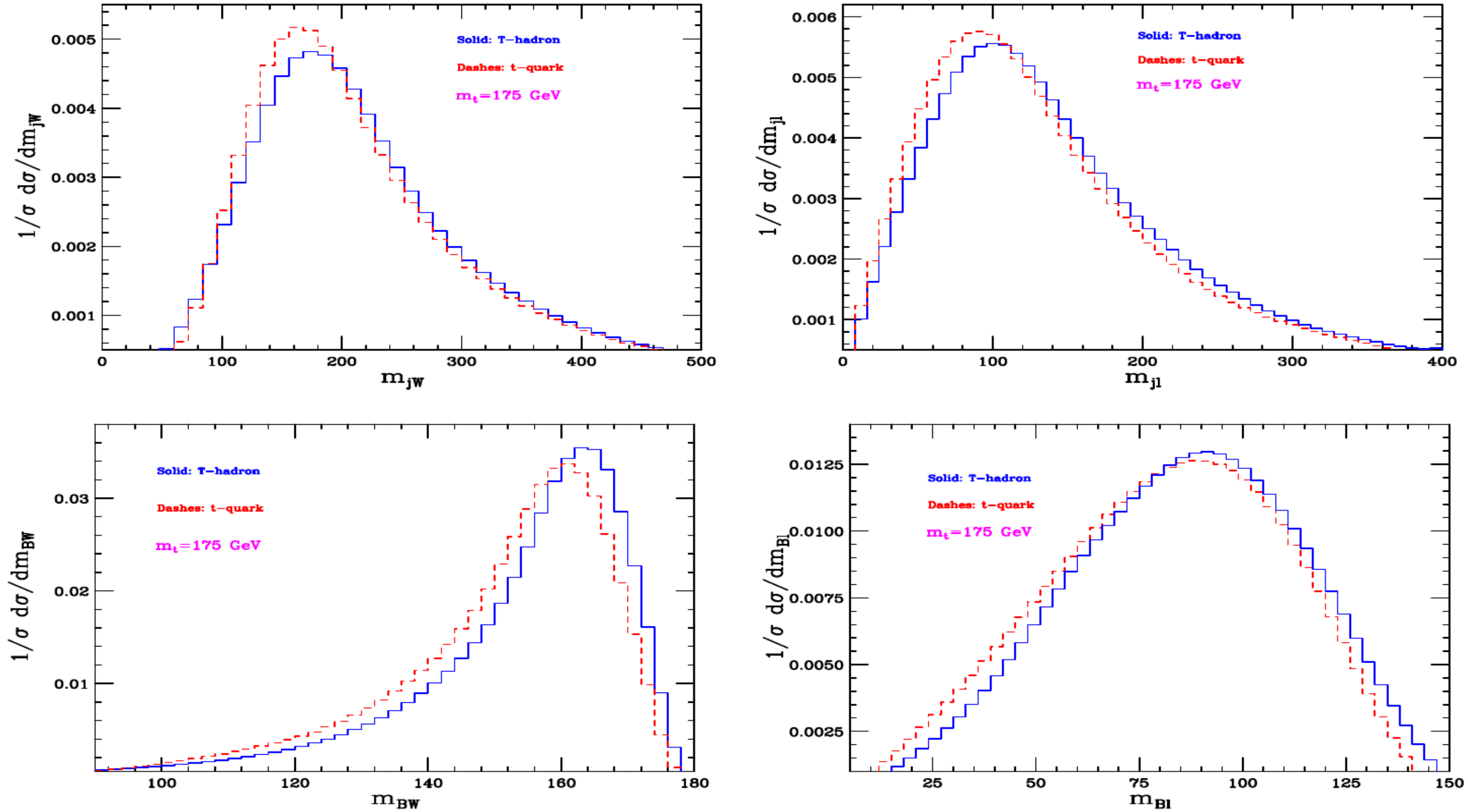
More practical: (Corcella, Mangano)

pp collisions at $\sqrt{s} = 8$ TeV, dilepton channel, k_T algorithm, $R = 0.7$, $p_{T,j} > 30$ GeV, $p_{T,\ell} > 20$ GeV, MET> 20 GeV, $|\eta_{j,\ell,\nu}| < 2.5$ (preliminary)



Many questions: if we let the t hadronize, its gluon radiation should be cutoff at a scale Λ rather than Γ_t. Thus, T and t distributions are perturbatively different. But with a Monte Carlo we can treat them in the same way.
So:

- In view of the fact that the pole mass ambiguity may be very small
- In view of the fact that (after all) the pole mass is a complex quantity (see also Hoang MSR mass)
- In view of the fact that experimentalists have severe constraints on what they can measure, so that there are often other non-perturbative corrections besides the pole mass renormalon

may be we should focus more on clever Monte Carlo studies to estimate non-perturbative uncertainties.

A question that should never arise: what kind of mass are we measuring anyhow?

A top mass measurement is always done by comparing theoretical predictions with measured distributions. The mass is a parameter of the theoretical prediction. Examples:

- The lepton distribution (like in Sayaka Kawabata talk). You measure the distribution, and compute it. The mass is the mass that you use in your calculation
- An end point method: you are measuring a pole mass (no radiative corrections! but beware of non-perturbative effects)
- Some sort of $Wb\,\mathrm{jet}$ reconstruction: you are after the pole mass, relying to some extent upon the fact that the mass of the decay products receive no radiative corrections. You should have low sensitivity to theoretical ambiguity in production and decay. You should compute the process using the pole mass, and test for this sensitivity

NLO+PS

The purpose of the NLO+PS was precisely this: being able to predict exactly the same distributions that are measured, with the same cuts and detector effects, in terms of the fundamental parameters of the theory.

Most recent developments: include also corrections in decays:

(talks by Re and Papanastasiou)

From Re talk: problems with radiation in resonance decays

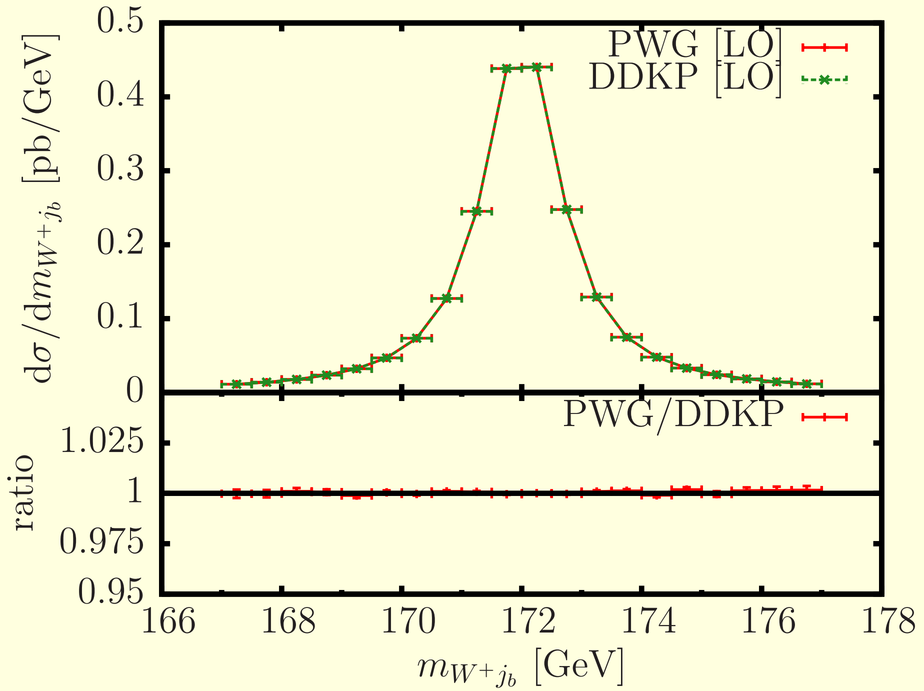
$WWbb$ at NLO+PS

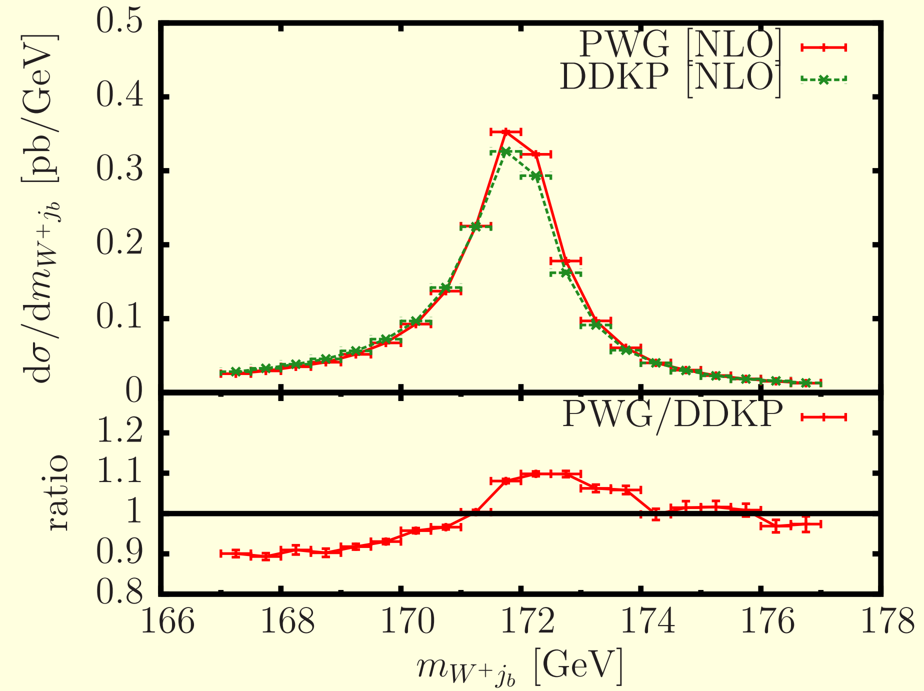
- at NLO+PS, things get more serious:

$$d\sigma = d\Phi_{\rm rad} \bar{B}(\Phi_B) \frac{R(\Phi_B, \Phi_{\rm rad})}{B(\Phi_B)} \exp\left[- \int \frac{R(\Phi_B, \Phi_{\rm rad})}{B(\Phi_B)} d\Phi_{\rm rad} \right]$$

- because virtuality is not preserved, $\bar{B}/B$ is suppressed or ennhanced, if $(\Phi_B, \Phi_{\rm rad})$ or Φ_B are off-shell, respectively.
- these effects don't mutually compensate, because if Φ_B is off-shell, the Sudakov factor always yield large suppression (the converse is true only if m^2_{bg} is small).
- expect distorsion of b-jet mass when $m^2/E \approx \Gamma_t$, *i.e.* $m_j \simeq 8$ GeV
- a POWHEG implementation for the full $WWb\bar{b}$ computation exists [Garzelli,Kardos,Trocsanyi '14]. It'll be interesting to investigate further.
- In the meanwhile, POWHEG-BOX was improved: now it can deal with radiation in resonance decays, in the zero-width limit, in a fully general way. First step towards exact $WWb\bar{b}$ at NLO+PS...

Not totally satisfactory:

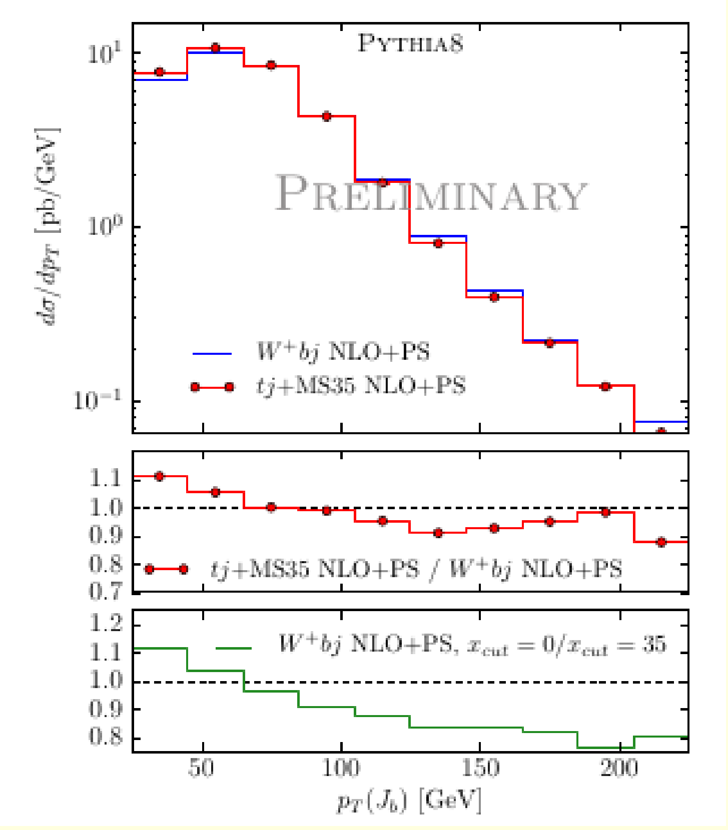


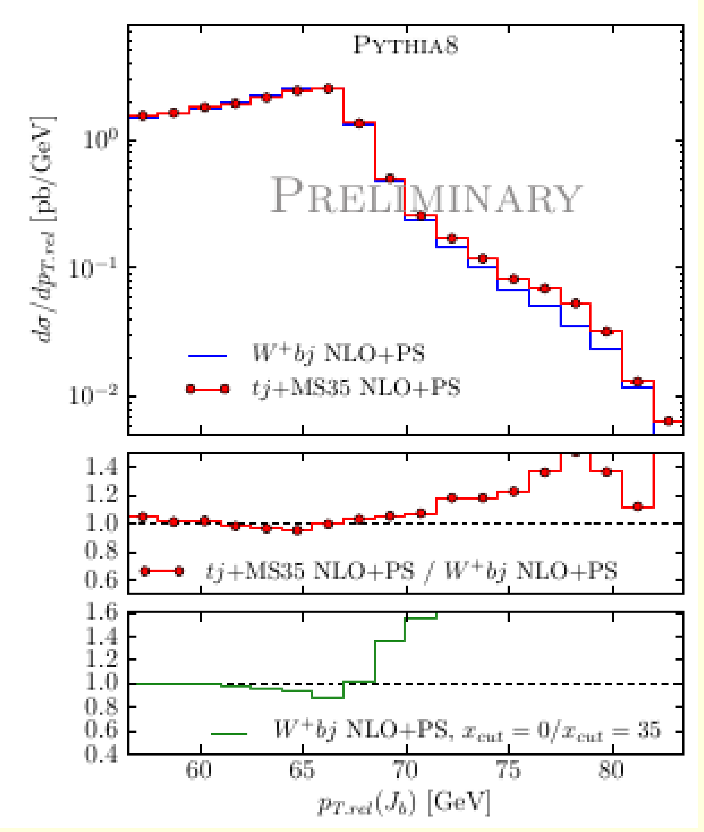


(Ellis,Campbell,Re,P.N. 2014, shown by Corcella at this conference)

Our LO correction for finite width and interference effects work perfectly at LO, but it looks like there is a shape distorsion at NLO that we do not capture.

Papanastasiou has shown first results for a calculation of $pp \to W^+ j_b j$, using the MC@NLO method, matched to a shower, with all finite width and interference effects included (Frederix, Frixione, Papanastasiou, Prestel, Torrielli):

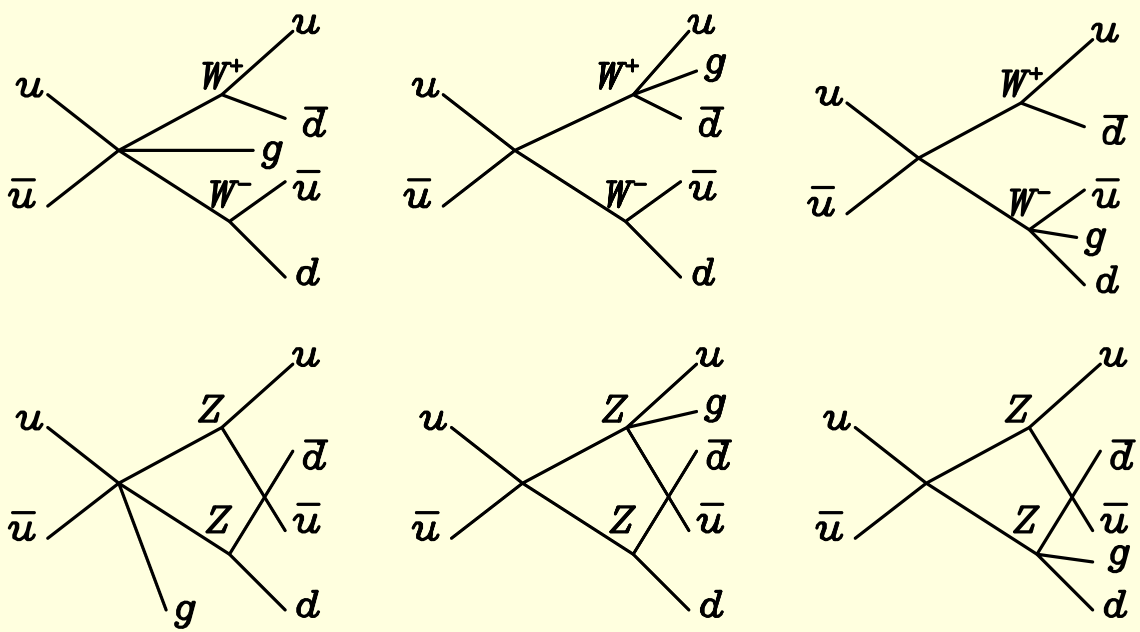




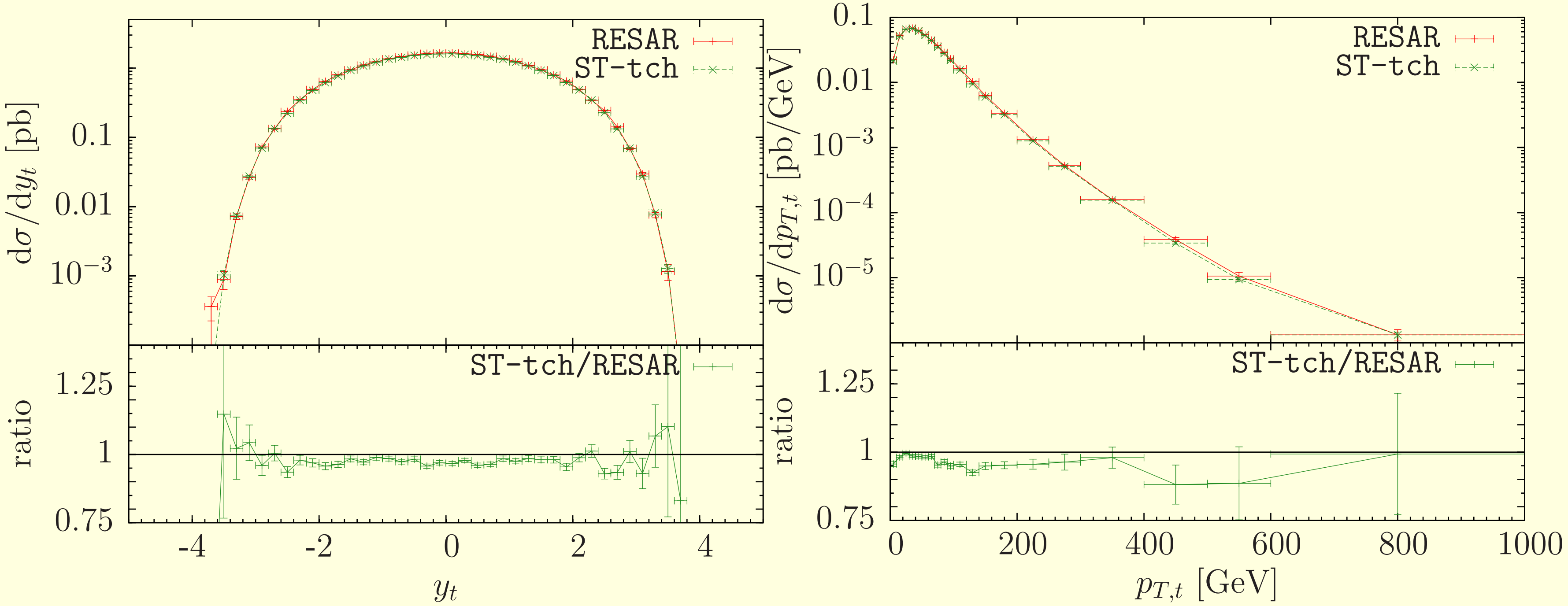
In the framework of `POWHEG`: a general method for the treatment of processes with intermediate resonances, with the inclusion of finite width, interference effects has been implemented in a new version of `POWHEG` (Jezo,P.N.). Was presented at CERN in June and at the MITP Workshop in July.

In essence, the partition of the Real cross section in terms of contributions that have only one singular collinear region is extended into a partition where each contribution has only one possible resonance history structure.

$u\bar{u} \to u\bar{d}u\bar{d}$
illustrative
example

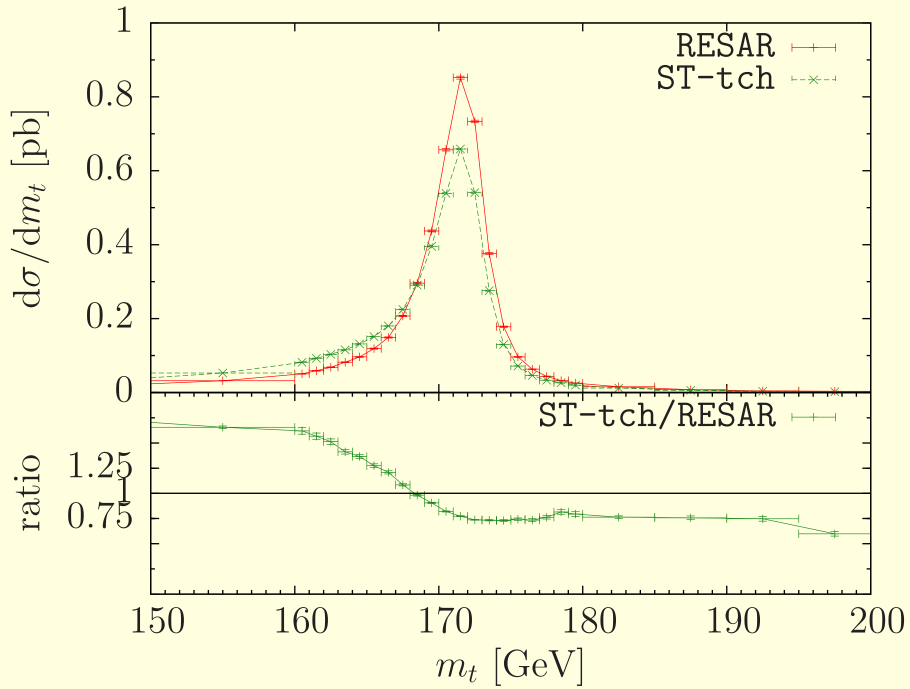


The method has been applied to single top production, and there is ongoing work on the $pp \to W^+ b W^- \bar{b}$ process. For single top, it compares well with `POWHEG ST-tch`:



But we have some indications of important effects of peak distorsion.

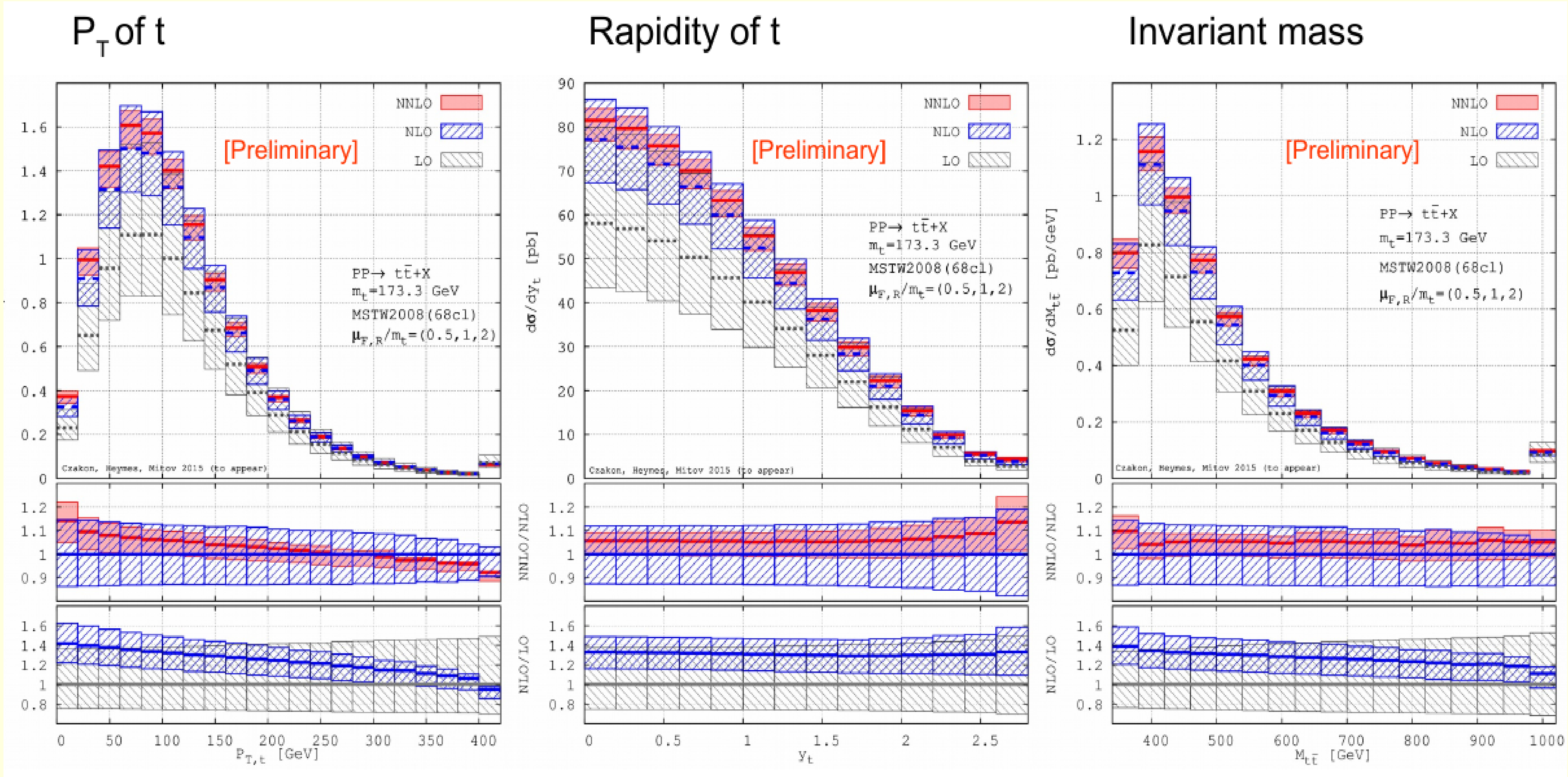
Still preliminary and not totally understood.



Since we aim at a precision below 1 GeV, we need to be able to understand all effects that can affect the top peak in a consistent framework. These are the tools to do it.

NNLO distributions for $t\bar{t}$ production finally shown!!!

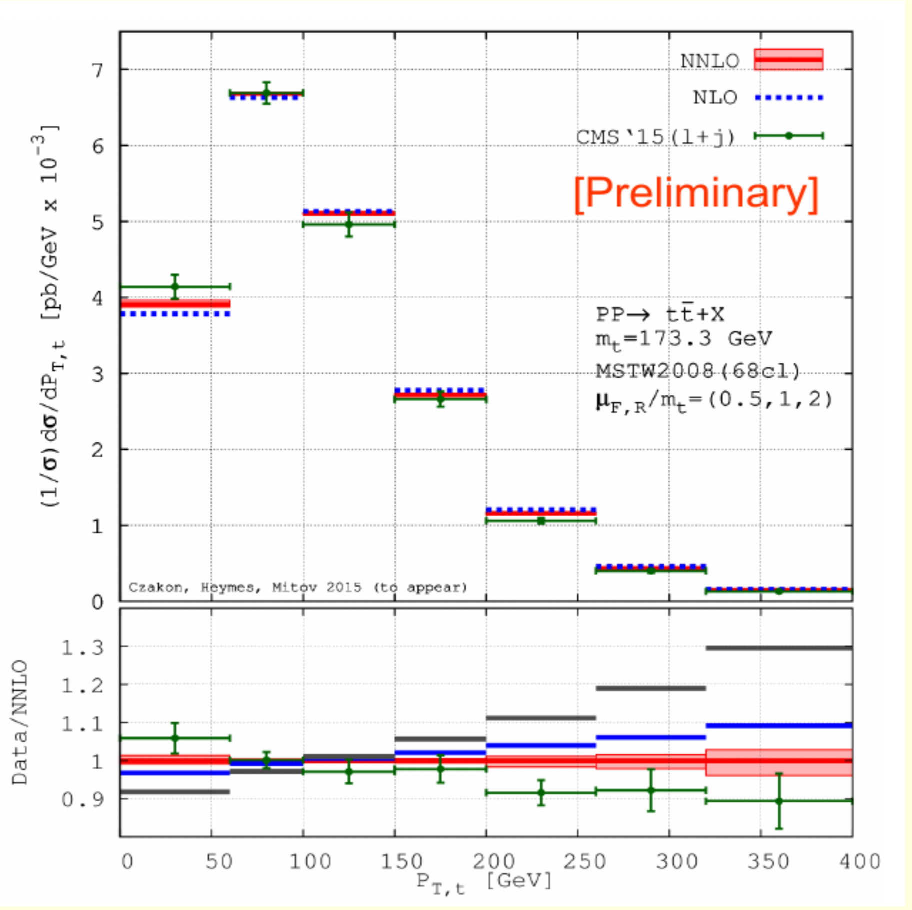
By Czakon,Fiedler,Heymes,Mitov. Long awaited for; but we knew it is very demanding work!



Timely result, in view of the problems in modeling the transverse momentum distribution of the top

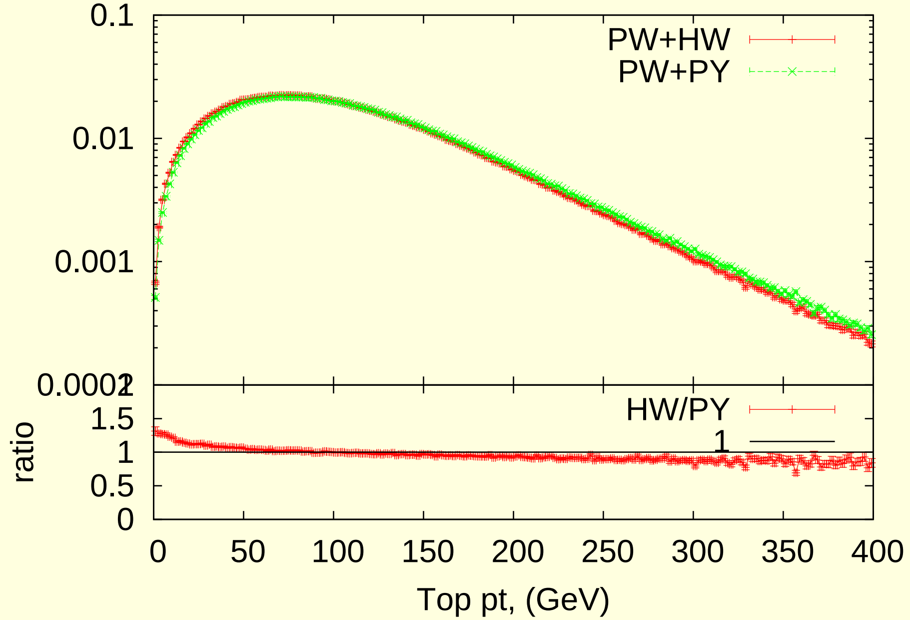
Consistency in the choice of scales was questioned at this conference.

No doubt these questions will be quickly solved.



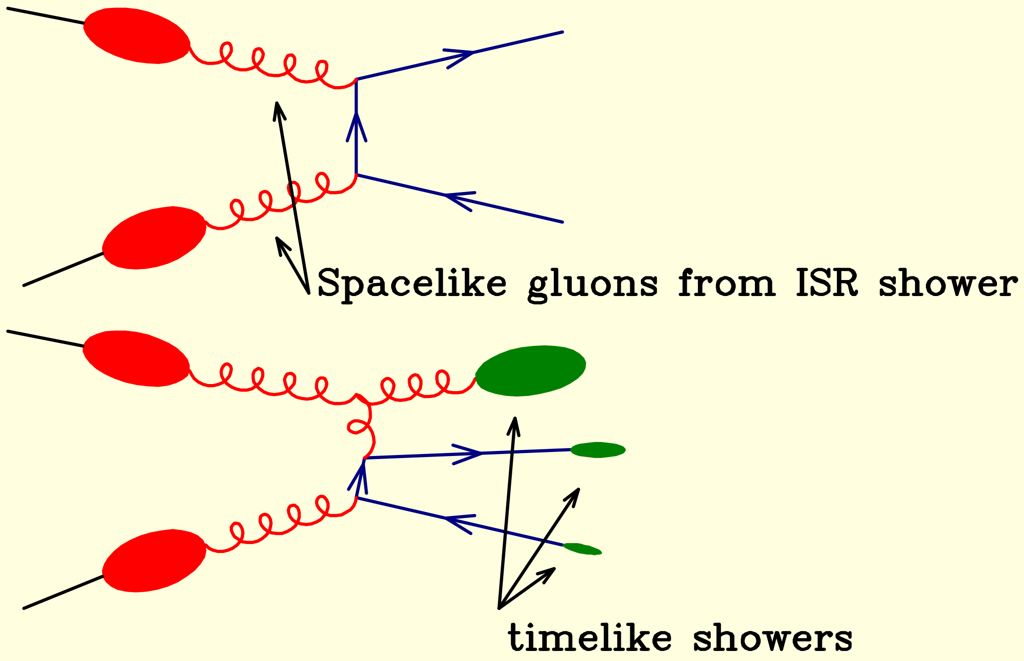
Furthermore: more groups are computing it: Abelof, Gehrmann-DeRidder, Maierhofer, Pozzorini; '14, '15; Bonciani, Catani, Grazzini, Sargsyan, Torre; '14. '15

In the framework of the TOPWG at CERN, ini 2014:



Intensively discussed, since `PW+HW` seemed to fit the data better.

The cause: MOMENTUM RESHUFFLING



ISR shower throws off shell the incoming gluon. In order to conserve 4-momentum, the final state is boosted. The mass $m_{t\bar{t}}$ is preserved

FSR shower changes the mass of final state partons. In order to conserve 4-momentum, the final state momenta are rescaled.

More specifically (HERWIG manual and private communication by B. Webber), one goes to the CM of the system of timelike showers and rescales all their 3-momenta by a common factor, so that the energy of the system matches the hard process energy.

So: Formally a NNLO effect. P. Richardson implemented alternative recoil scheme in `Herwig++` in order to study this uncertainty.
No longer an uncertainty now

Something to learn here:

- Theoretical uncertainties tend to increase with time.
- unless we achieve real progress.

Conclusions

- Theoretical progress in several areas: NNLO, resummation, EW corrections, NLO+PS methods.
- We are still struggling with problems related to the top mass at the LHC. More work is needed there, but some progress has taken place.