Theoretical Uncertainties in Experiments

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why should we care?

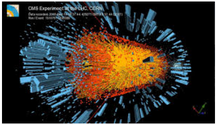
motivation: precision era at the LHC (and beyond)

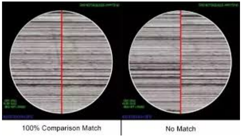
- entering percent precision era at LHC:
  - no more “simple wins” (resonances? large deviations?)
  - focus increasingly on minute deviations
  - must measure and constrain 0’s

(EFT’s as perfect framework to communicate 0’s in consistent way)

- name of the game: precision tests of the Standard Model
  - in dozens or hundreds of processes
  - over huge regions of phase space
  - in subtle quantum effects

(too often forgotten: we **do** precision tests of QM)





CSI LHC: need precise & accurate tools for precision physics

This is a **massive challenge for the theory community**:

- develop new techniques (multi-loop, IR subtraction, . . .)
- improve computational efficiency (HPC, GPU's, . . .)
- understand & appreciate limiting factors
- necessitates **long-term investment**

(and possibly creation of new career & funding paths: "theory technicians")

where are we?

some general thoughts (focus on LHC/hadron colliders)

- challenge: huge phase-space coverage of large variety of observables
  - $\longrightarrow$ huge scale ratios: resummation vs. fixed order
  - $\longrightarrow$ theoretically controlled matching of different approaches
  - $\longrightarrow$ large number of processes: (semi-)automated solutions necessary
- increasingly important: "find the right tool for the job"

  (or don't use a hammer if you want to pull nails out of the wall)
  - $\longrightarrow$ fixed order vs. resummation, analytic vs. numerical, ...
  - $\longrightarrow$ choice informed by size of first missing order and importance of potentially large variety of physics effects

    (this means we need to know what we look at, just running favourite MC is not going to cut it)
  - $\longrightarrow$ need robust uncertainty estimates across tools palette

    (this presents a veritable problem when considering soft non-perturbative physics effects, or parton showers)
- rough parametric uncertainty estimate: $(\alpha_S/\pi)^2 \simeq (\alpha_W/\pi) \simeq 1\%$

  (often overlooked: this is for observable calculated at the corresponding order – total cross section vs. differential distribution)

current status of theory (focus on LHC/hadron colliders)

- $\text{NNLO}_{(\text{QCD})}$, $\text{NLO}_{(\text{QCD})}\otimes\text{NLO}_{(EW)}$(approx) & $\text{NLO}_{(EW)}$ (exact) are the new baseline precision for many observables
- $\text{NLO}_{(\text{QCD},EW)}$ automated & validated but:

  (lots of successes: OpenLoops, Recola, aMC@Nlo _ MadGraph , McFm, ...)

  - FS multiplicity limited by computing power

    (...and may be improved with smarter/better computational algorithms: phase space integration? ML??)
  - need better "taxonomy" when communicating
- NNLO_{QCD}: $2\to 2$ sorted, ingredients for $2\to\geq 3$ missing

  (master integrals: analytic vs. numerical)
- $\text{N}^3\text{LO}_{\text{QCD}}$ available for $2\to 1$ only

theory precision in MC event generators

- MC: major conversation pathway between theory & experiment

  (quite often for our experimenters, they are "theory")

- current "accuracy standard(s)":
  - matching: NNLOPS (NNLO + parton shower)
  - merging: MEPS@NLO (combined NLO's + parton shower)
- dominating QCD effects: $\mathcal{O}(10\text{-}30\%)$
  - low-$p_\perp$ region dominated by parton shower
  - high-$p_\perp$ region dominated by (multi-) jet topologies
  - higher accuracy in rate (and some shapes) through NNLO matching
- add EW corrections for %-level precision
  - EW correction at large scales $\mathcal{O}(10\%)$
  - QED FSR + EW for V line shapes at $\mathcal{O}(1\%)$

what is achievable?

(in the next 5-10 years)

"predictions" for analytic tools

- expect progress in higher-order calculations
  - (semi-)automated $\mathrm{NNLO_{QCD}}$ for multi-particle FS & mixed exact $\mathrm{NLO_{QCD}} \otimes \mathrm{NLO}_{EW}$ for multi-particle FS
    (I expect this to be driven by constructing efficient basis of master integrals and improving and automating IR subtraction methods)
  - approximate and/or exact $\mathrm{N^3LO_{QCD}}$ beyond $2 \to 1$ topologies
    (similar problems as before: master integrals, ...)
  - higher precision in PDF's
- don't know enough about resummation to have a clear picture, will concentrate on parton shower simulations instead

simulation

- reminders/lessons from past decade:
  - NLO matching with parton showers has been trivial

    (parton shower kernels equal NLO subtraction kernels)
  - reminder 2: multi-jet merged MEPS@LO and MEPS@NLO usually agree in central values to better than 10%, main effect is reduced scale uncertainties

    (this is not yet fully explored when searching for efficient event generation)
- extrapolation to higher-orders in simulation:
  - NNLO will not be quite as simple - will need $\mathcal{O}(\alpha_S^2)$ kernels

    (tricky, think about it as fully automated NNLO subtraction kernels)
  - opens parton shower for systematic uncertainty analysis
    $\longrightarrow$ expect main effect in reduction of scale uncertainties
  - non-trivial impact of choices (kinematics) on log accuracy

where does it stop?

theory limitations

"The great advances in science usually result from new tools rather than from new doctrines"
(Freeman Dyson)

- technical challenges: speed & stability of fixed-order calculations
  - numerical issues 1: phase-space integration efficiency

    (current approach: multi-channel with process-specific mappings)

    (hard to see how ML can make a massive difference – until now only "blanks")
  - numerical issues 2: special functions in multi-loop master integrals

    (convergence of series expansion, maybe need to go to quadruple precision)
  - numerical issues 3: stability of numerical $(N-1)$-loop results

    (example: NLO inputs to NNLO calculation, supremacy of compact analytic expressions)

$\longrightarrow$ this will necessitate highly technical, barely publishable work

(traditionally this is often a dead-end for careers in theory)

theory limitations

- physics challenges:
  - physics challenge 1: sources of uncertainty
    (simple scale variations by a factor of two may not be sufficient: central scale in multi-scale processes?
    in addition: new sources of uncertainty. example: kinematics scheme in parton showers and impact on log accuracy)

    $\longrightarrow$ community effort: agree on robust procedures
  - physics challenge 2: complexity & control
    (as the calculations become increasingly complex we need more and more independent – algorithmic, implementation, ... – checks. examples: two calculations for fixed order, two implementations of parton showers, etc.)

    $\longrightarrow$ community effort: robust and open (cross-)validation procedures
  - physics challenge 3: input parameters as source of uncertainty
    (my favourite example: PDF's with different values for $\alpha_S(M_Z)$
    and, yes, there's more: PDF's etc.)

    $\longrightarrow$ community effort: harvest "old data"

theory limitations

- "philosophical" challenges:
  - philosophical challenge 1: breakdown of factorisation
    (studies suggest that at some high order factorisation breaks down for basic $2 \longrightarrow 2$ QCD processes
    it is not clear how this translates to more complicated processes)

    $\longrightarrow$ don't know if we should care
  - philosophical challenge 2: impact of higher-twist
    (this is closely related to 1.
    simple back-of-the envelope: at DY $m_P/M_Z \approx 1\%$ – is this a problem?)

    $\longrightarrow$ this may need a careful analysis – is it linear or quadratic?
  - philosophical challenge 3: impact of soft physics uncertainties
    (lots of them a a hadron collider: MPI/underlying event, hadronization, . . . –
    quite often models aiming to describe these effects are too similar, and based on identical data)

    $\longrightarrow$ only more data will help here

?

instead of a summary

- dominance of efforts to measure, constrain & communicate 0's
  (no question, this is important, but
  we need to be careful in how we communicate this to ourselves, next generation & public
  discussing a high-energy LHC of 27 TeV for the year 2050 is not helping to recruit the brightest students
  chase exciting unorthodox ideas: non-perturbative physics at high energies)
- important to develop a positive & exciting vision of the field
  (in my opinion we sound too often quite corporate)
- identify ways to port our ideas, techniques & practices
  (there are a lot of neighbouring fields, where many of our techniques would make a huge difference: HI collisions, cosmic rays, EIC, . . .)