Lessons for EIC from LHC: PDF discussion

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Studies of the potential of the HL-LHC have been made

The extra luminosity does not help when uncertainties are already systematics limited

Estimates are made of the size of future systematics

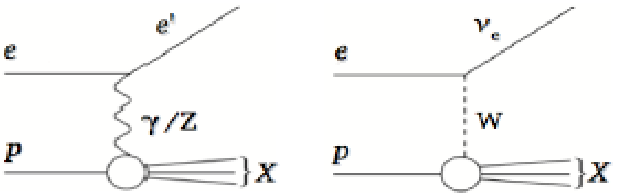
But these tend to be idealised

Let’s look at some problems with systematics within the context of PDF fits for some real LHC data

Suggestions for the EIC to avoid some problems

Deep Inelastic Scattering (DIS) is the best tool to probe proton structure

NC : $e\,p \rightarrow e'\,X$ CC : $e\,p \rightarrow \nu_e\,X$



- Kinematic variables:

$Q^2 = -q^2 = -(k-k')^2$ Virtuality of the exchanged boson

$x = \frac{Q^2}{2p\cdot q}$ Bjorken scaling parameter

$y = \frac{p\cdot q}{p\cdot k}$ Inelasticity parameter

$s = (k+p)^2 = \frac{Q^2}{xy}$ Invariant c.o.m.

Neutral current:

$$\frac{d^2\sigma^{\pm}_{NC}}{dx\,dQ^2} = \frac{2\alpha\pi^2}{xQ^4}\left(Y_+ F_2 \mp Y_- xF_3 - y^2 F_L\right)$$

$F_2 \propto \sum_i e_i^2 (xq_i + x\bar{q}_i)$ quark distributions

$xF_3 \propto \sum_i (xq_i - x\bar{q}_i)$ valence quarks

$F_L \propto \alpha_s \times g$ gluon at NLO

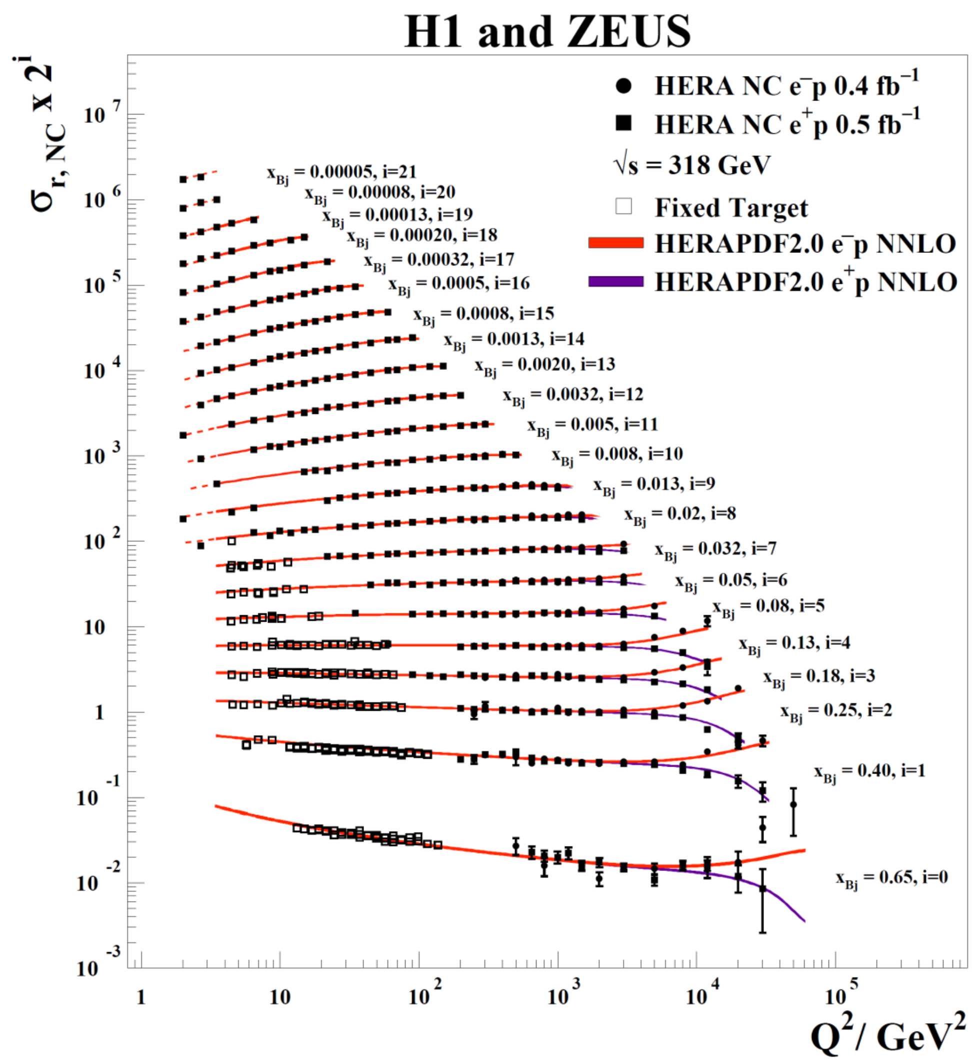
LO expressions

Charged current:

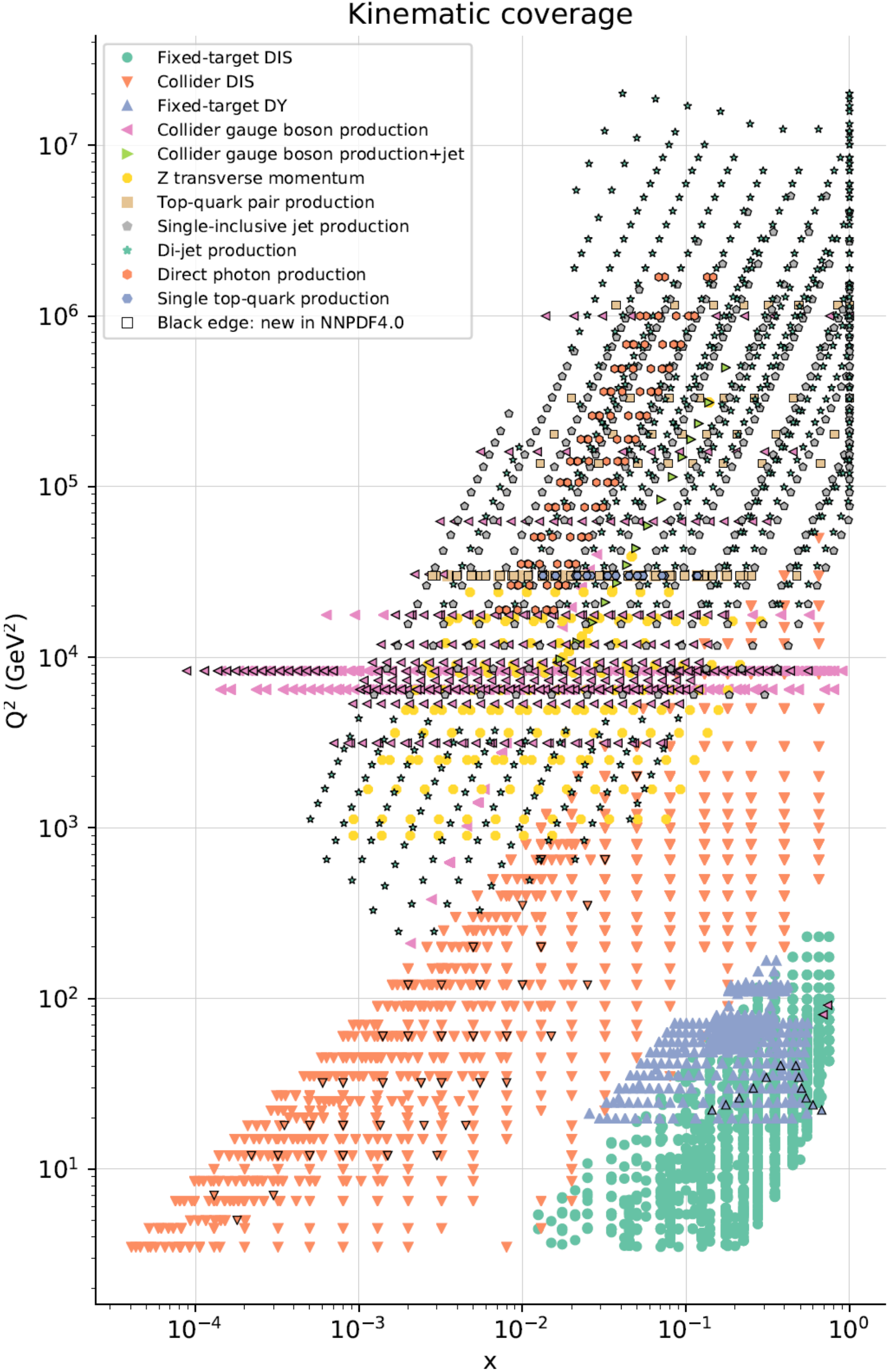
$$\frac{d^2\sigma^{-}_{CC}}{dx\,dQ^2} = \frac{G_F^2}{2\pi}\frac{M_W^2}{M_W^2+Q^2}\left(u + c + (1-y^2)(\bar{d}+\bar{s})\right)$$

$$\frac{d^2\sigma^{-}_{CC}}{dx\,dQ^2} = \frac{G_F^2}{2\pi}\frac{M_W^2}{M_W^2+Q^2}\left(\bar{u} + \bar{c} + (1-y^2)(d+s)\right)$$

flavour decomposition



Gluon from the scaling violations: DGLAP equations tell us how the partons evolve

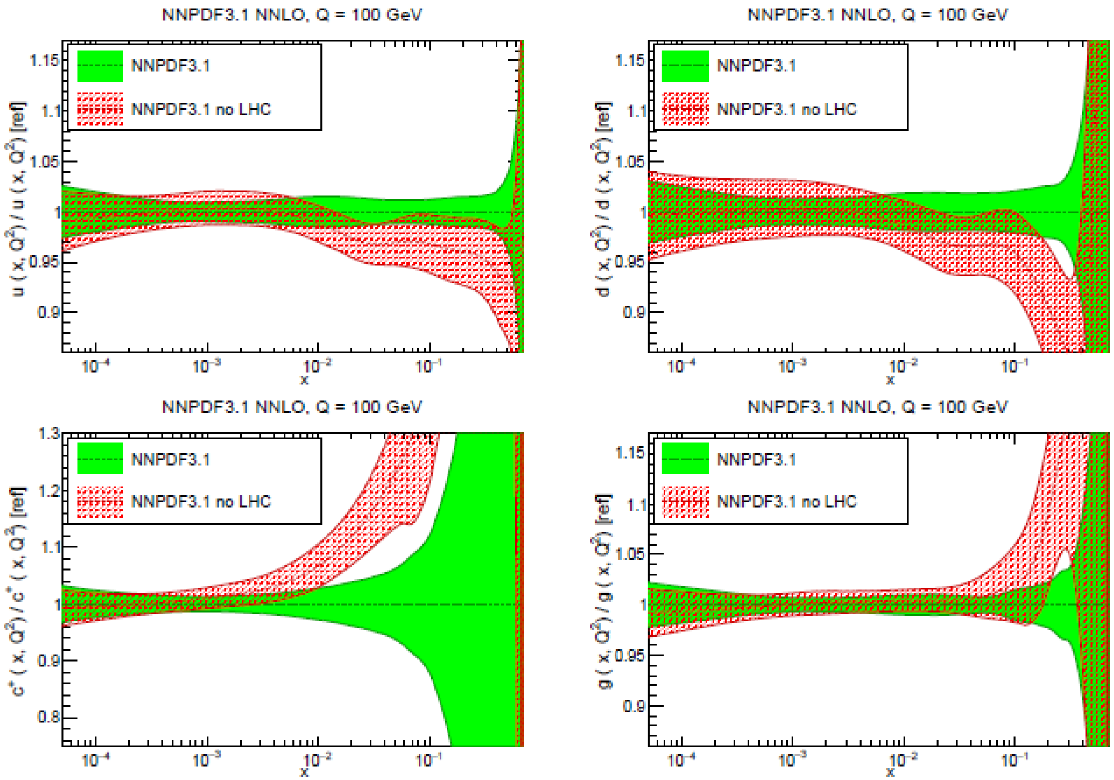


We now use many other processes than deep-inelastic scattering for the determination of PDFs:

- Drell-Yan data from fixed targets and the Tevatron and LHC
- W,Z rapidity spectra from Tevatron and LHC
- Jet pT spectra from Tevatron and LHC
- Top-anti-top differential cross-sections
- W and Z +jet spectra, or W,Z pt spectra
- W and Z +heavy flavours
- These are all processes that can be calculated at fixed order, currently NNLO
- But beware: there may be new physics at high scale that we ‘fit away’

So let's see how much LHC data is improving PDFs

NNPDF3.1 includes modern LHC data on W,Z + jets + top + Zpt from 7 and 8 TeV running.   Compare PDFs with and without LHC



Some of the data input to NNPDF3.1 –like the **ATLAS W,Z data have already reached a limit of how accurate they could be.**

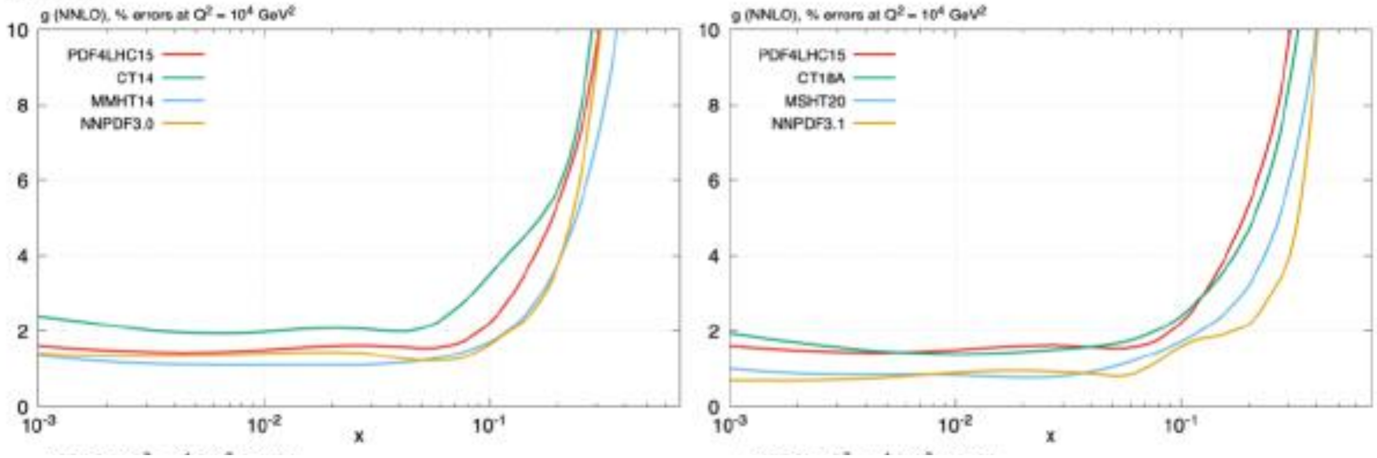
The W,Z uncertainties of *O*(1%) are limited by experimental systematics not by statistics. This will not get better in the foreseeable future with the High Luminosity LHC

FURTHERMORE, this looks good BUT specific choices were made by NNPDF e.g which top-quark differential distributions are used and of which jet data distributions are used etc.

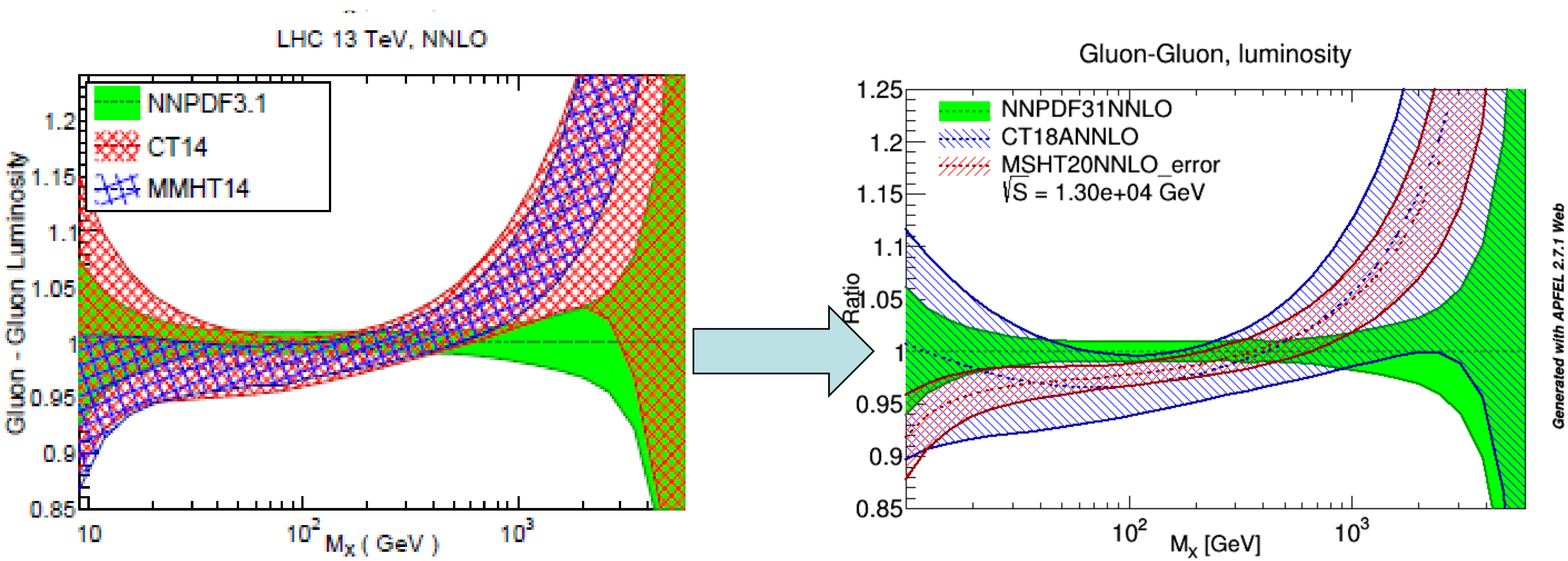
NNPDF4.0 improves this further… but this is not yet agreed by other groups.

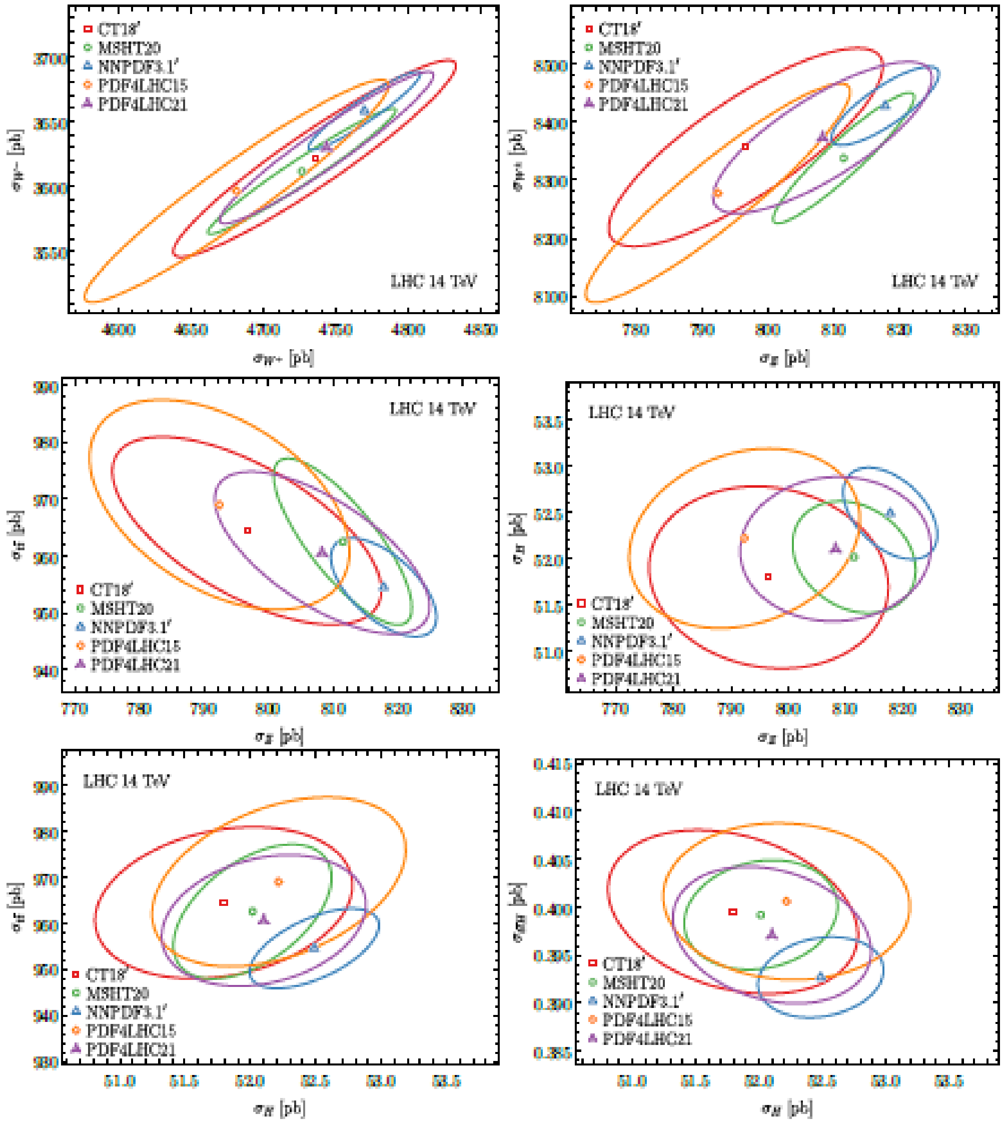
Other PDF groups are making other choices--this could even increase the total uncertainty due to differences between PDF sets

Gluon



As the uncertainties of each individual PDF decrease with the input of more information, the divergence of the PDFs from each other has increased





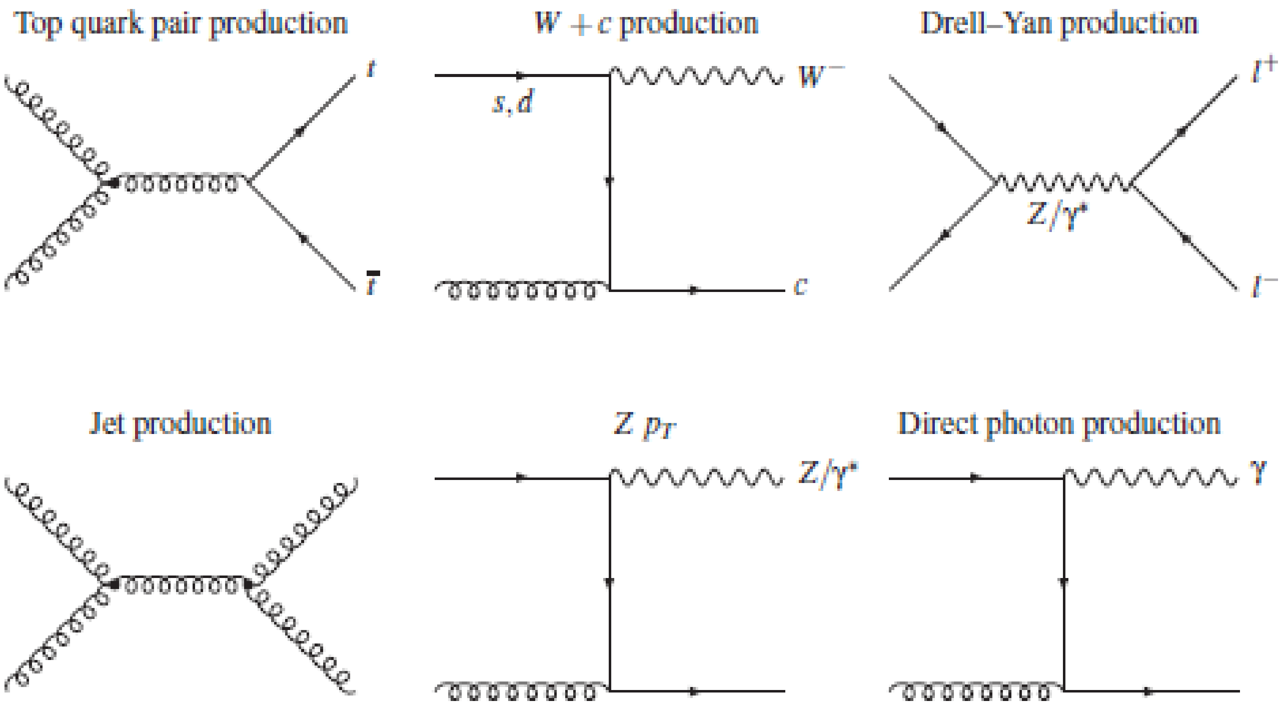
The PDF4LHC group makes combinations of the PDFs from the three main fitting groups NNPDF, CT and MSHT

The PDF4LHC15 combination has just been superseded by the PDF4LHC21 combination

There IS an improvement in uncertainty BUT this is not enough –for example—

to reduce the PDF uncertainty on on LHC measurement of m_W sufficiently -10 MeV could decrease to ~8MeV – we need more than this …

A recent study of potential improvements has been made using processes for which are still statistics limited, where the High-Luminosity LHC (HL-LHC) should help
arXIV:1810.03639



Pseudo-data is generated for these processes assuming luminosity of 3 ab^{-1} for CMS and ATLAS and 0.3 ab^{-1} for LHCb

Pseudodata is generated from the predictions of PDF4LHC 15 by fluctuating them point by point for their estimated experimental uncertainties and overall for their luminosity uncertainty

$$\sigma_i^{\rm exp} = \sigma_i^{\rm th} \times \left(1 + r_i \cdot \delta_{{\rm tot},i}^{\rm exp} + \lambda \cdot \delta_{\mathcal{L}}^{\rm exp}\right)$$

The experimental uncertainty is got from the statistical uncertainty expected for the high luminosity (modulo acceptance factors) and systematic uncertainty added in quadrature

$$\delta_{{\rm tot},i}^{\rm exp} \equiv \left(\left(\delta_{{\rm stat},i}^{\rm exp}\right)^2 + \left(f_{\rm corr} \times f_{\rm red} \times \delta_{{\rm sys},i}^{\rm exp}\right)^2\right)^{1/2} . \qquad \delta_{{\rm stat},i}^{\rm exp} = \left(f_{\rm acc} \times N_{{\rm ev},i}\right)^{-1/2}$$

The systematic uncertainty is taken from known uncertainties in present day measurements * two factors to account for possible improvement

Pessimistic and Optimistic assumptions are made

Both about the effect of correlations – typically, f_{corr} = 1, 0.25

And about possible reduction in uncertainty typically, f_{red} = 1, 0.4 with more data.

This is about as good as you can do with pseudo-data but let’s not forget that this is a somewhat ideal situation

Process	Kinematics	$N_{\rm dat}$	$f_{\rm corr}$	$f_{\rm red}$	Baseline
Z p_T	$20\,\text{GeV} \le p_T^{ll} \le 3.5$ TeV $12\,\text{GeV} \le m_{ll} \le 150$ GeV $\|y_{ll}\| \le 2.4$	338	0.5	$(0.4, 1)$	[52] (8 TeV)
high-mass Drell-Yan	$p_T^{l1(2)} \ge 40(30)$ GeV $\|\eta^l\| \le 2.5$, $m_{ll} \ge 116$ GeV	32	0.5	$(0.4, 1)$	[47] (8 TeV)
top quark pair	$m_{t\bar{t}} \simeq 5$ TeV, $\|y_t\| \le 2.5$	110	0.5	$(0.4, 1)$	[50] (8 TeV)
W+charm (central)	$p_T^\mu \ge 26$ GeV, $p_T^c \ge 5$ GeV $\|\eta^\mu\| \le 2.4$	12	0.5	$(0.2, 0.5)$	[24] (13 TeV)
W+charm (forward)	$p_T^\mu \ge 20$ GeV, $p_T^c \ge 20$ GeV $p_T^{\mu+c} \ge 20$ GeV $2 \le \eta^\mu \le 4.5$, $2.2 \le \eta^c \le 4.2$	10	0.5	$(0.4, 1)$	LHCb projection
Direct photon	$E_T^\gamma \lesssim 3$ TeV, $\|\eta_\gamma\| \le 2.5$	118	0.5	$(0.2, 0.5)$	[55] (13 TeV)
Forward W, Z	$p_T^l \ge 20$ GeV, $2.0 \le \eta^l \le 4.5$ $60\,\text{GeV} \le m_{ll} \le 120$ GeV	90	0.5	$(0.4, 1)$	[49] (8 TeV)
Inclusive jets	$\|y\| \le 3$, $R = 0.4$	58	0.5	$(0.2, 0.5)$	[61] (13 TeV)
Total		768			

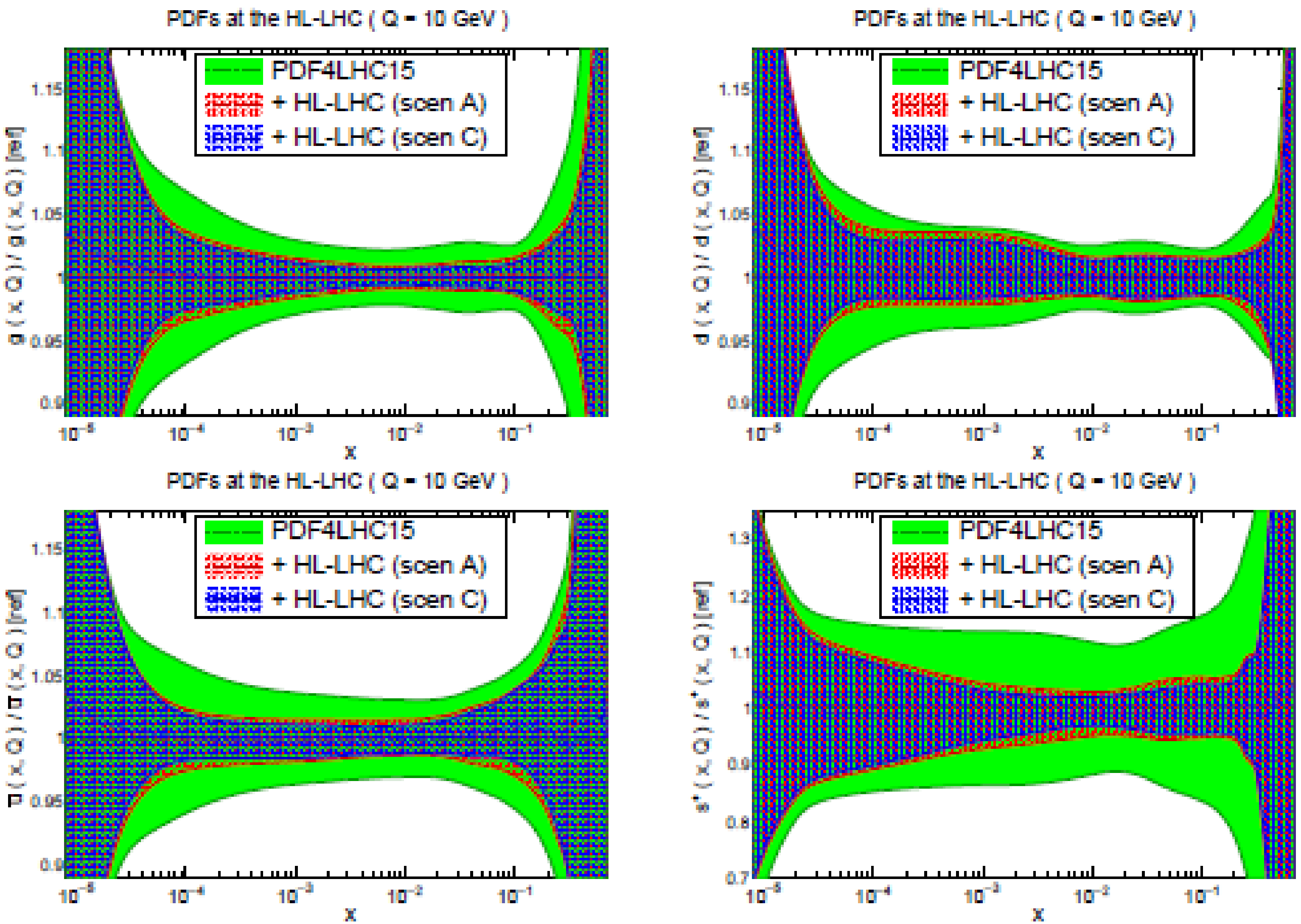
Table 2.1. Summary of the features of the HL–LHC pseudo–data generated for the present study. For each process we indicate the kinematic coverage, the number of pseudo–data points used across all detectors $N_{\rm dat}$, the values of the correction factors $f_{\rm corr}$ and $f_{\rm red}$; and finally the reference from the 8 TeV or 13 TeV measurement used as baseline to define the binning and the systematic uncertainties of the HL–LHC pseudo–data, as discussed in the text.

$$\chi^2(\beta_{\rm exp},\beta_{\rm th}) = \sum_{i=1}^{N_{\rm dat}} \frac{1}{\left(\delta^{\rm exp}_{{\rm tot},i}\sigma^{\rm th}_i\right)^2}\left(\sigma^{\rm exp}_i + \sum_j \Gamma^{\rm exp}_{ij}\beta_{j,{\rm exp}} - \sigma^{\rm th}_i + \sum_k \Gamma^{\rm th}_{ik}\beta_{k,{\rm th}}\right)^2 + \sum_j \beta^2_{j,{\rm exp}} + T^2\sum_k \beta^2_{k,{\rm th}} ,$$

Effect of the pseudo-data is assessed by Hessian profiling of the PDF4LHC15 PDF Taking into account correlated systematics of the pseudo-data (exp) and of the theory predictions (th) due to the eigenvectors of the PDF fit.

We minimise wrt both the experimental and theoretical nuisance parameters β
The new $\beta_{k,th}$ parameters can be interpreted as leading to PDFs that have been optimised to the new measurements.
The Hessian matrix in $\beta_{k,th}$ parameters space at the minimum can be diagonalized to construct new eigenvector directions. PDF uncertainties are then determined from $\Delta\chi^2 = T^2$, the study used T=3 a typical tolerance for CT and MSHT fits which accounts for tensions between data sets.

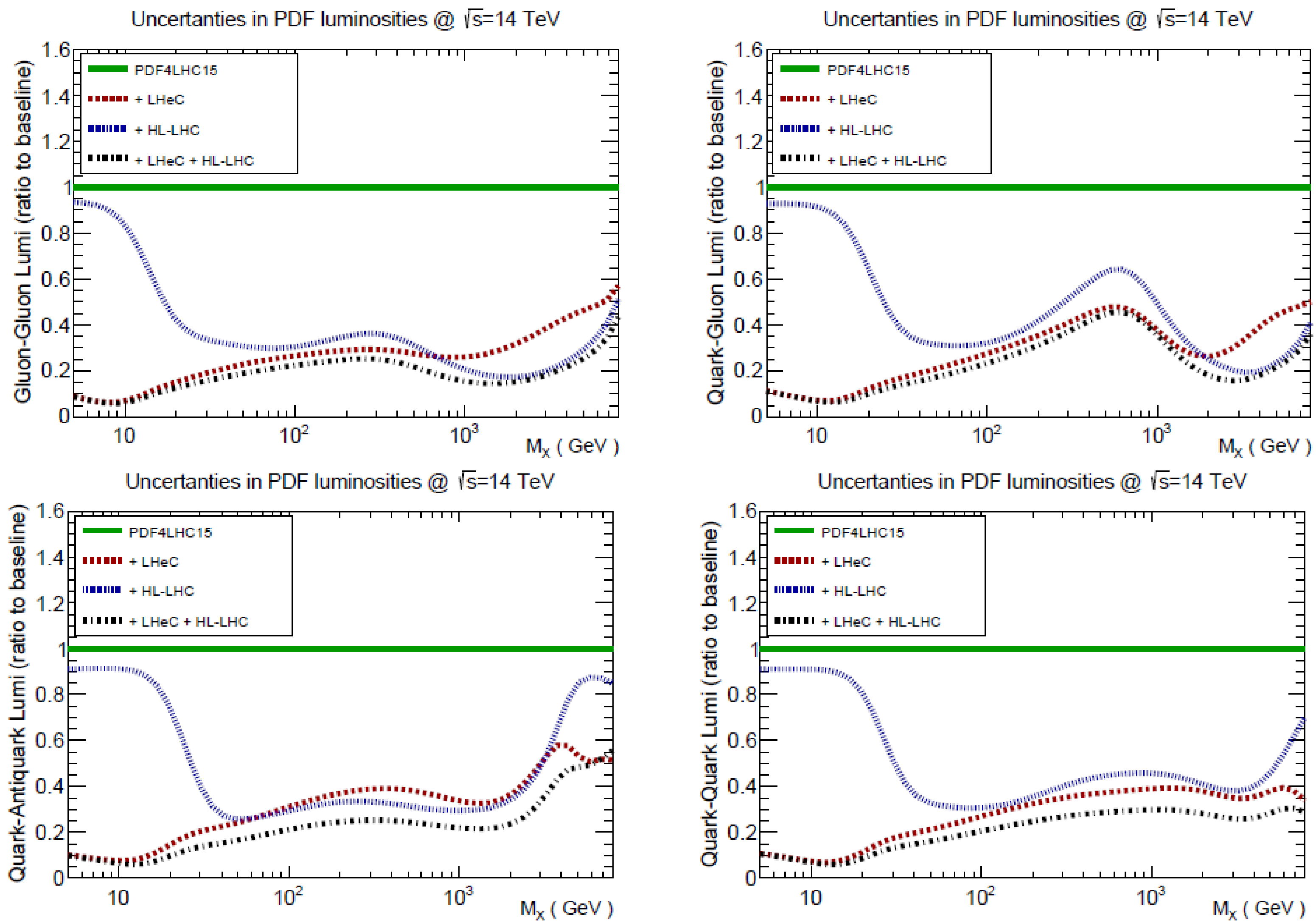
We see potential improvements in the PDFs



Where scenario A is pessimistic and scenario C is optimistic

- Such improvements could give up to a factor 2 improvement in the PDF uncertainty on something like m_W

arXIV:1810.03639, 1906.10127
Illustrating the improvement in a slightly different way
And comparing to LHeC expectations



But I think we are being too optimistic, correlated systematic errors are not so well behaved that we can scale them from what we already have.
How do we actually determine PDFs?

$$\chi^2 = \sum_{ik} \left(D_i - T_i(1 - \sum_i \gamma_{ij} b_j) \right) C^{-1}_{\text{stat},ik}(D_i, D_k) \left(D_k - T_k(1 - \sum_i \gamma_{kj} b_j) \right) + \sum b_j^2$$

We fit data D, to predictions of NNLO QCD, T, (these predictions rely on the PDFs, which are usually parametrised at an input scale), taking into account the uncorrelated and correlated uncertainties of the data.

Uncorrelated is easy, there are statistical and uncorrelated systematics and the statistical component may be bin to bin correlated, hence the Matrix C

Correlated uncertainties are supplied as fractional, γ, and can be applied as fractions of either data D or theory T (T to avoid d'Agostini bias), by using nuisance parameters b, which are ideally zero but can vary ~± 1 for 1σ variations. These parameters are fitted along with the parameters which describe the PDFs that are input to the predictions.

Experimentalists spend YEARS determining the systematic uncertainties of their data. They do the best they can.
But the formalism above assumes systematic uncertainties are well behaved Gaussian errors **They aren't**

In particular, some systematic uncertainties are what are referred to as
'2-point systematics'
This means they are determined by running one Monte-Carlo data simulator, say PYTHIA, and another, say HERWIG, and taking the difference as the systematic uncertainty. This is a reasonable estimate, it is not a gaussian error.
Unfortunately, such uncertainties are often the largest systematics--- a few percent- especially on jet related systematics.

THEN the formalism also assumes correlated systematic uncertainties are 100% correlated point to point throughout the data set to which they apply.
100% may not be realistic.
AND it has become common practice to assign more and more systematics.
AT HERA we had 169 for ~1200 data points
AT LHC we often have >~300 for <~300 data points (in some cases MUCH less data)
So we had better be treating them right.
Furthermore we have many types of data set analysed by different groups and with differing procedures for the evaluation of systematic uncertainties, which makes cross-correlating them difficult. Such correlations are not usually known/applied

SOME examples of what can 'go wrong':

ATLAS data on t-tbar differential distributions
ATLAS data on inclusive jet production (I will put this in back-up)

ATLAS 8 TeV t-tbar, four differential spectra, first fitted separately

		lepton+jets spectrum			
		m_{tt}	p_T^t	y_{tt}	y_t
Partial χ^2/NDP	ATLAS $t\bar{t}$	3.4 / 7	7.9 / 8	19.7 / 5	18.3 / 5

This is from ATLAS' own fit (ATL-PHYS-PUB-2018-017) but is very similar for MSHT and CT18.

THEN we can use the statistical correlations AND the systematic correlations BETWEEN these spectra to fit them together. Take two that are well fitted (p^t_T and m_{tt}) and one that is not well fitted y_t with one that IS well fitted p^t_T (p^t_T and y_t)

		lepton+jets spectra			
		p_T^t and y_t with statistical correlations	p_T^t and y_t without statistical correlations	p_T^t and m_{tt} with statistical correlations	p_T^t and m_{tt} without statistical correlations
Partial χ^2/NDP	ATLAS $t\bar{t}$	33 / 13	30 / 13	45 / 15	42 / 15

This is done both with and without the statistical correlations, to show that the application of the statistical correlations does not change much.
However the role of the systematic correlations is dramatic as we shall see…

For (p^t_T and y_t) the joint χ2/NDP ~30/13 is more or less the addition of the separate fits (7.9+18.3 = 26.2) / 13 ------FINE, but
For (p^t_T and m_{tt}) the joint χ2/NDP ~45/13 is way more than the addition of the separate fits (3.4+7.9 = 11.3)/ 15--- NOT FINE AT ALL

WHY?

For each systematic uncertainty source there is a fitted value of the nuisance parameter b. Let us look at these values of b for the separate fits to the spectra. For SOME of the '2-point systematics'

Systematic uncertainty source	lepton+jets spectrum			
	p_T^t	y_t	y_{tt}	m_{tt}
Hard scattering model	+0.74± 0.31	+0.48± 0.22	+0.92± 0.37	-0.43± 0.20
Parton shower model	-1.32± 0.43	-0.79± 0.26	-0.51± 0.17	+0.39±0.13
ISR/FSR model	-0.47± 0.18	-0.87±0.30	-1.27± 0.38	+0.33± 0.10

The b values for p^t_T and y_t are broadly compatible

The b values for p^t_T and m_{tt} are not

BUT when we fit the spectra together we are assuming 100% correlation of the same systematic so that the same value of b should describe both spectra.

This does not suit either p^t_T or m_{tt}.

If we keep all other systematic correlations and investigate the effect of breaking the correlation of just these three '2-point systematics'

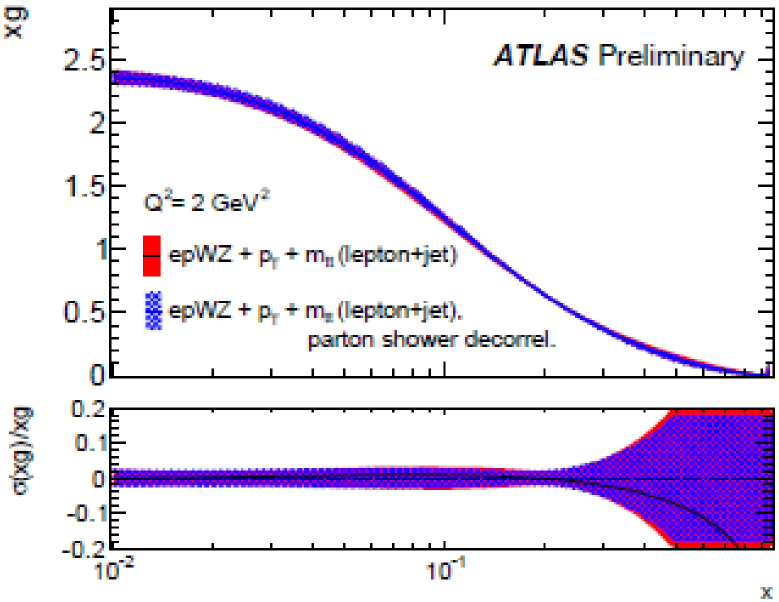
		lepton+jets spectra		
		p_T^t and y_t decorrelate 2-point uncertainties	p_T^t and m_{tt} decorrelate 2-point uncertainties	p_T^t and m_{tt} decorrelate parton-shower model uncertainty
Partial χ^2/NDP	ATLAS $t\bar{t}$	27.8 / 13	11.5 / 15	14.1 / 15

Breaking the inter-spectra correlations for all three systematics gives χ2/NDP of the same size as the separate fits

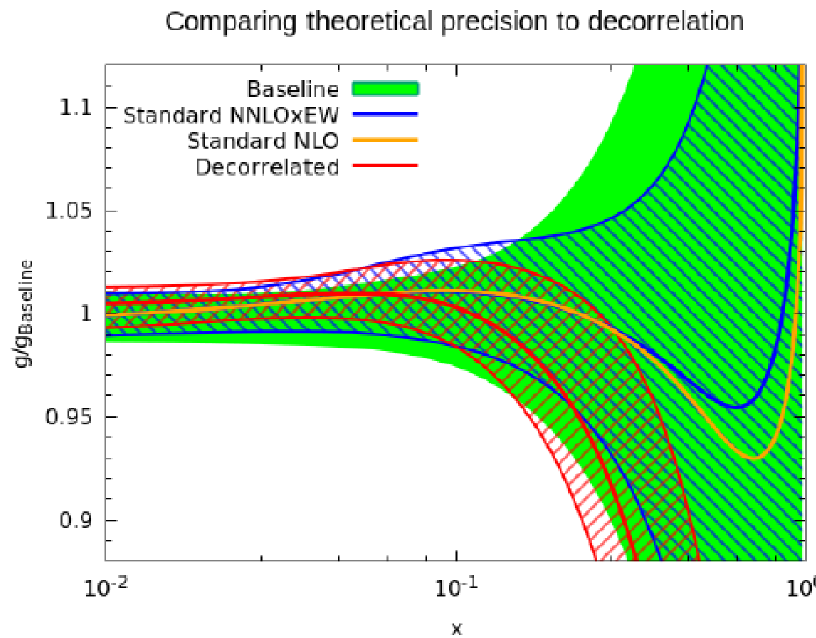
Breaking JUST the parton-shower model correlation does most of the work

BUT YOU HAVE TO KNOW WHAT the experimentalists will tolerate….THIS was ATLAS approved

The question then is does the decorrelation affect the resulting PDFs?



In the simple ATLAS study decorrelating ONLY parton shower between p^t_T and m_{tt}. the effect of the decorrelation on the PDFs is not very significant.



Those paying close attention will have noticed that the rapidity distributions are still not well fitted.
In arxiv:1909.10541 members of MSHT went further and considered further decorrelation within the rapidity spectra to achieve better fits.

But for the arxiv:1909.10541 study decorrelating all 4 spectra and using decorrelation within the rapidity spectra we see that the effect can be larger than the NLO to NNLO difference.

None of this could have been anticipated in a pseudo-data study

We have been talking about breaking some of the assumptions of 100% correlation of systematics.
But what about the correlations that probably should be there, but which we do not account for?

One of the issues with LHC data is that realistically it involves the combination of many data sets analysed by different groups and with differing procedures for the evaluation of systematic uncertainties, which makes cross-correlating them difficult. Such correlations are not usually known/applied

Some nice work on cross data set correlations was done for ttbar/Z ratios in 1612.03636

Here we see the size of the systematic

Systematic [%] / $\sqrt{s}$ [TeV]	$\delta\,\sigma_Z^{\rm fid}$ 13	8	7	$\delta\,\sigma_{t\bar{t}}^{\rm tot}$ 13	8	7
Luminosity	2.1	1.9	1.8	2.3	2.1	2.0
Beam energy	0.7	0.6	0.6	1.5	1.7	1.8
Muon (lepton) trigger	0.1	0.6	0.1	0.1	0.2	0.2
Muon reconstruction/ID	0.7	0.5	0.3	0.4	0.4	0.3
Muon isolation	0.4	0.0	0.2	0.3	0.2	0.4
Muon momentum scale	0.1	0.0	0.0	0.0	0.0	0.1
Electron trigger	0.0	0.2	0.0	0.1	—	—
Electron reconstruction/ID	0.4	0.8	0.3	0.3	0.4	0.1
Electron isolation	0.1	0.0	—	0.4	0.3	0.6
Electron energy scale	0.3	0.1	0.1	0.2	0.5	0.2
Jet energy scale	—	—	—	0.4	0.7	0.4
b-tagging	—	—	—	0.5	0.4	0.5
Background	0.1	0.2	0.1	1.1	1.0	1.0
Signal modelling (incl. PDF)	0.1	0.1	0.3	3.0	1.7	1.8

Here the letters tell us what is correlated to what

Source / $\sqrt{s}$ [TeV]	$\delta\,\sigma_Z^{\rm fid}$ 13	8	7	$\delta\,\sigma_{t\bar{t}}^{\rm tot}$ 13	8	7
Luminosity	A	B	C	A	B	C
Beam energy	A	A	A	A	A	A
Muon (lepton) trigger	A	A*	A	A	B	B
Muon reconstruction/ID	A	B	C	A	D	D
Muon isolation	A	A	A	B	C	D
Muon momentum scale	A	A	A	A	A	A
Electron trigger	A	A	A	A	—	—
Electron reconstruction/ID	A	B	C	A	D	D
Electron isolation	A	A	—	B	C	D
Electron energy scale	A	A	A	A	A	A
Jet energy scale	—	—	—	A	B	B
b-tagging	—	—	—	A	B	B
Background	A	A	A	B	B	B
Signal modelling (incl. PDF)	A	A	A	B*	B	B

But this was for the total cross sections and these lepton systematics are not the largest that we have to deal with…

Further work on differential distributions was done for arXIVs: 2101.05095, 2112.11266
These were PDF fits using various ATLAS data sets
E.G.Correlations between data sets with V+jets, t-tbar in lepton+jets, inclusive jets

Systematics	8 TeV W + jets	8 TeV Z + jets	8 TeV $t\bar{t}$ lepton + jets	13 TeV $t\bar{t}$ lepton + jets	8 TeV inclusive jets
Jet flavour response	JetScaleFlav2	Flavor Response	flavres-jes	JET29NP JET Flavour Response	syst JES Flavour Response*
Jet flavour composition	JetScaleFlav1Known	Flavor Comp	flavcomp-jes	JET29NP JET Flavour Composition	syst JES Flavour Comp
Jet punchthrough	JetScalepunchT	Punch Through	punch-jes	-	syst JES PunchThrough MC15
Jet scale	JetScalePileup2	PU OffsetMu	pileoffmu-jes	-	sys JES Pileup MuOffset
	-	PU Rho	pileoffrho-jes	JET29NP JET Pileup RhoTopology	sys JES Pileup Rho topology*
	JetScalePileup1	PU OffsetNPV	pileoffnpv-jes	JET29NP JET Pileup OffsetNPV	syst JES Pileup NPVOffset
	-	PU PtTerm	pileoffpt-jes	JET29NP JET Pileup PtTerm	syst JES Pileup Pt term
Jet JVF selection	JetJVFcut	JVF	jetvxfrac	-	syst JES Zjets JVF
B-tagged jet scale	-	btag-jes	JET29NP JET BJES Response	-	-
Jet resolution	-	jeten-res	JET JER SINGLE NP	-	-
Muon scale	-	-	mup-scale	MUON SCALE	-
Muon resolution	-	-	muonms-res	MUON MS	-
Muon identification	-	-	muid-res	MUON ID	-
Diboson cross-section	-	-	dibos-xsec	Diboson xsec	-
Z + jets cross section	-	-	zjet-xsec	Zjets xsec	-
Single-t cross section	-	-	singletop-xsec	st xsec	-

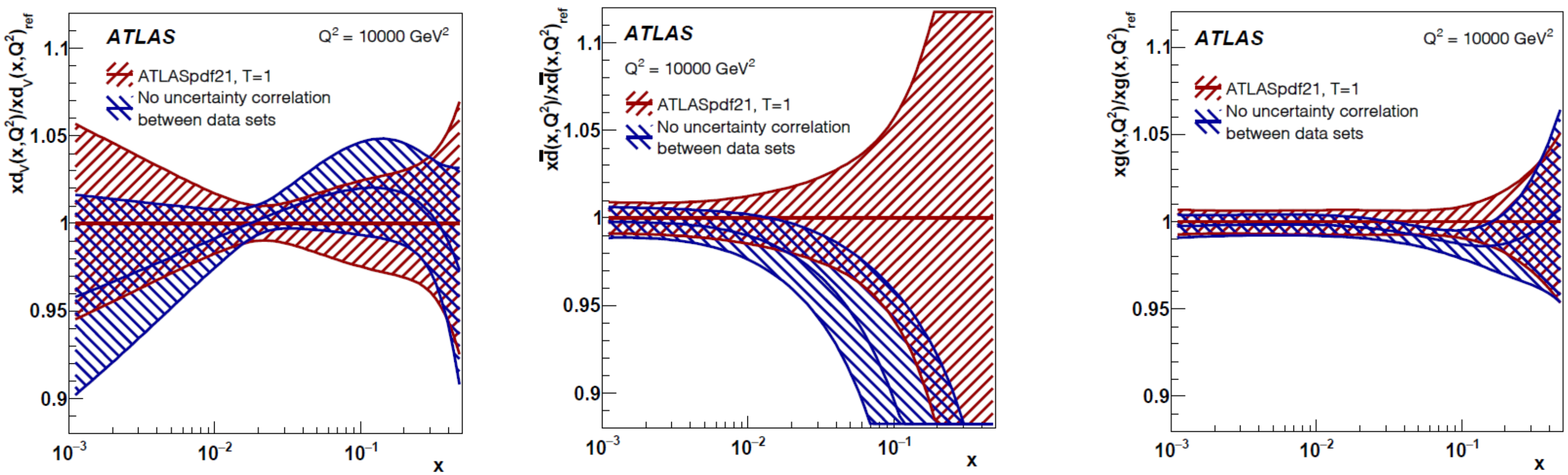
Would you have known that these contributions to the JES are the same from looking at their names on HEPDATA?
You have to ask the experimentalists.. OR RATHER perhaps we have to be more helpful !

Entries in the same row are considered 100% correlated for the central fit
Cross checks are made of alternative degrees of correlation between jets with R=0.6 rather than R=0.4.

Note these are mostly the JES correlations, which are the largest, lepton correlations are much smaller

So what difference does accounting for these inter-data set correlations make?

Lets look at a scale relevant for LHC physics and focus on the middling x range where W,Z and Higgs are produced



The difference between accounting for the correlations or not doing so is the shift from red to blue—shown in ratio here

The χ2 of the fit is 30 units better when correlations are included

The difference in PDFs is small for the gluon

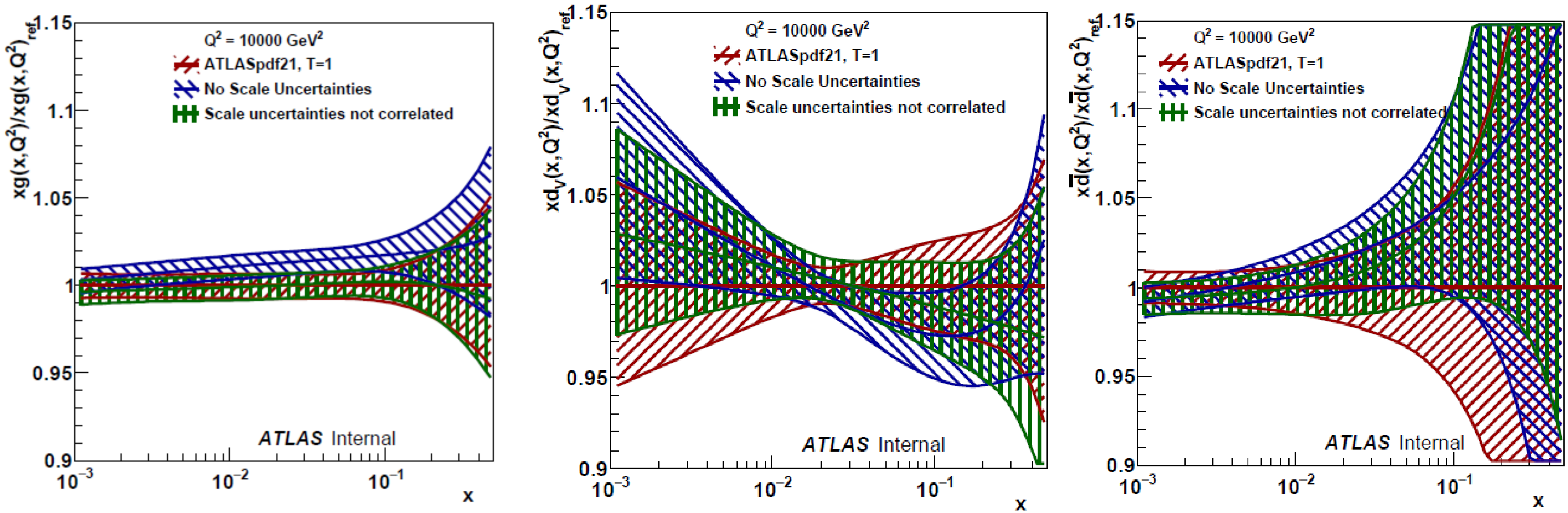
But larger in the d-quark sector

Remember the goal for PDF precision is ~1% for M_W and $\sin^2\theta_W$ if BSM effects are to be seen by the deviations of these parameters from their SM values

Correlations can be important

There are further effects we might consider: Impact of scale uncertainties

In the ATLASpdf21 fit (arXIV:2112.11266) for the inclusive W,Z production at 7 and 8 TeV the scale uncertainties are comparable to the experimental uncertainties and are thus included as theoretical uncertainties in the fit. They are correlated between the W and Z data and between the 7 and 8 TeV data

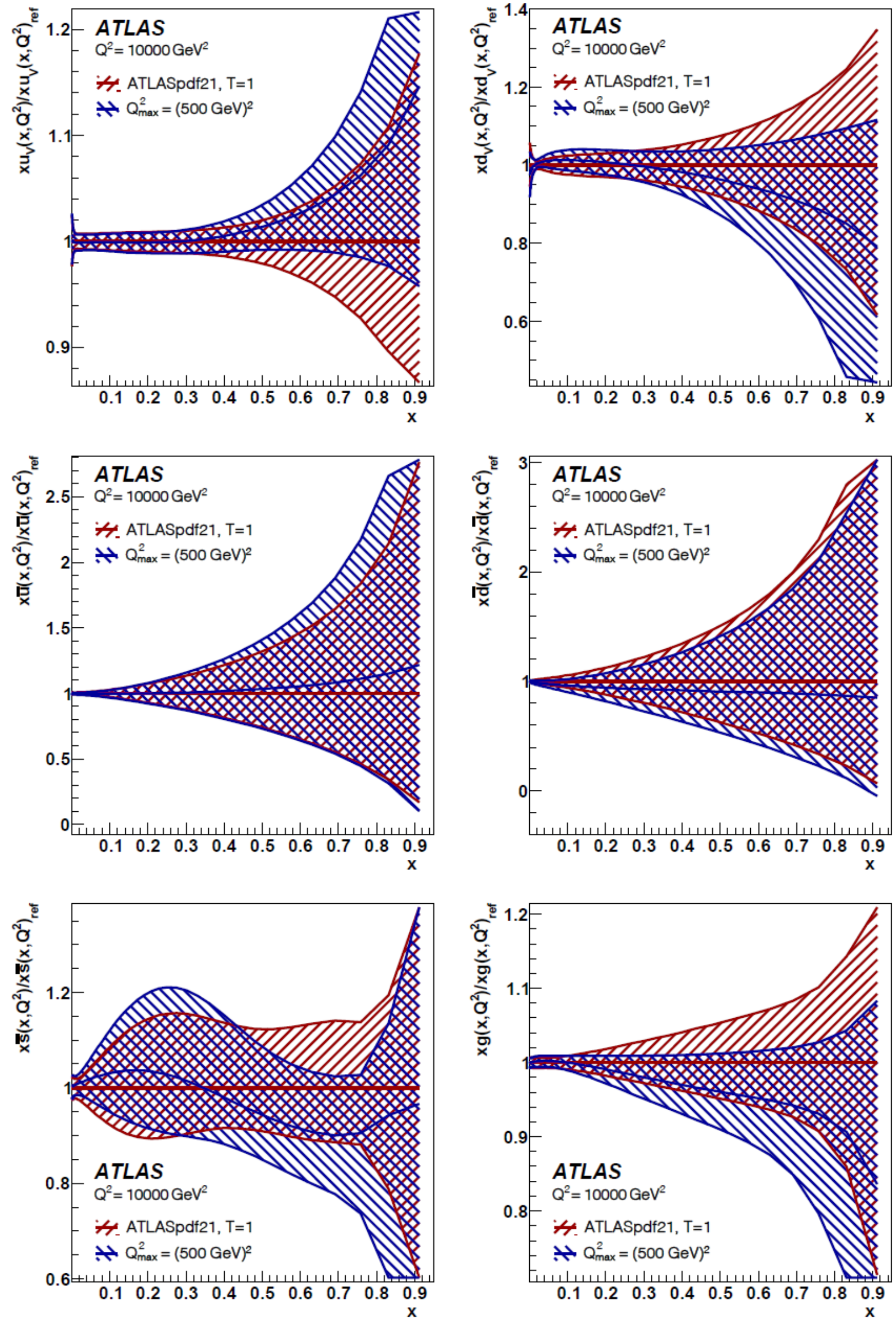


Here we show the ratios of the gluon, d-valence and dbar PDFs with (red) and without (blue) these scale uncertainties included
In green we show the effect of including scale uncertainties but not correlating them between 7 and 8 TeV data

Clearly scale uncertainties can be important if 1% precision is sought

Effect of Q^2_{max}=(500GeV)^2 cut
ie are we fitting data which may already have new physics effects in it

Check by applying a maximum Q^2 cut
Standard fit in red
Fit with Q2 cut in blue
This is not a big effect at the moment.



Lessons for the EIC

One should not underestimate the trouble that correlated systematic uncertainties can cause.

A big advantage is that inclusive DIS data will be analysed as one (or is it two) data set across a broad kinematic range
But there are always new runs and new procedures

We experimentalists continually refine our procedures for evaluating systematics
Remember to keep track of what is correlated to what between runs
Try to have a common obvious naming convention
(Even between experiments)
It took us years of work from the end of HERA running to understand correlations within H1 and ZEUS data sets and then between H1 and ZEUS
Do better by thinking of this from the beginning

The same applies when combining say heavy flavour data sets and jet data sets with the inclusive data.

Back-up

Second example ATLAS 8 TeV inclusive jets

These data come in the form of jet p_T spectra in bins of jet rapidity
It is well known that individual rapidity bins can be fitted well but that when the rapidity bins are all combined the fits deteriorates.
This is true for ATLAS jets at 7, 8 and 13 TeV

Rapidity ranges	P_{obs} CT14	MMHT2014	NNPDF3.0	HERAPDF2.0
Anti-k_t jets $R = 0.4$				
$\|y\| < 0.5$	44%	28%	25%	16%
$0.5 \leq \|y\| < 1.0$	43%	29%	18%	18%
$1.0 \leq \|y\| < 1.5$	44%	47%	46%	69%
$1.5 \leq \|y\| < 2.0$	3.7%	4.6%	7.7%	7.0%
$2.0 \leq \|y\| < 2.5$	92%	89%	89%	35%
$2.5 \leq \|y\| < 3.0$	4.5%	6.2%	16%	9.6%
Anti-k_t jets $R = 0.6$				
$\|y\| < 0.5$	6.7%	4.9%	4.6%	1.1%
$0.5 \leq \|y\| < 1.0$	1.3%	0.7%	0.4%	0.2%
$1.0 \leq \|y\| < 1.5$	30%	33%	47%	67%
$1.5 \leq \|y\| < 2.0$	12%	16%	15%	3.1%
$2.0 \leq \|y\| < 2.5$	94%	94%	91%	38%
$2.5 \leq \|y\| < 3.0$	13%	15%	20%	8.6%

Separate rapidity bin fits

χ^2/ndf	$p_T^{jet,max}$ $R = 0.4$	$R = 0.6$	p_T^{jet} $R = 0.4$	$R = 0.6$
$p_T > 70$ GeV				
CT14	349/171	398/171	340/171	392/171
HERAPDF2.0	415/171	424/171	405/171	418/171
NNPDF3.0	351/171	393/171	350/171	393/171
MMHT2014	356/171	400/171	354/171	399/171

All rapidity bins together
Same problem whatever PDF, whatever radius, whatever jet scale
Same problem in new PDF fits
This is from arxiv:1706.03192 the data paper. This was done at NLO
BUT IT IS NOT BETTER AT NNLO

So should we consider breaking correlations between rapidity bins
Just for some of the larger ‘2-point systematics’??

What could we decorrelate? ATLAS has considered

JES Flavour Response — JES=Jet Energy Scale

JES MJB Fragmentation — MJB = Multi-Jet background

JES Pile-up Rho Topology……….best not to ask

Non perturbative corrections—Hadronisation, underlying event

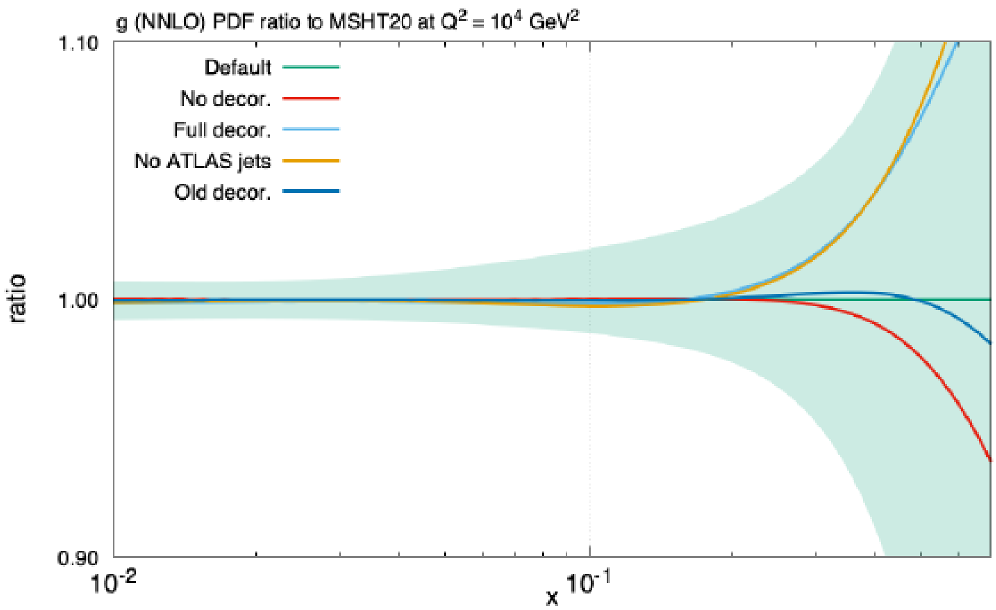
There are various options for the decorrelations. The decorrelations are not done abruptly between rapidity bins, but as smooth functions of rapidity and p_T.
Here are just of few of the possibilities considered, where the function
L (x, min, max)= (x-min)/(max-min) for min< x < max, =0 for x<min, =1 for x> max

14	$L(\ln(p_T[\mathrm{TeV}]), \ln(0.1), \ln(2.5)) \cdot \sqrt{1 - L(\lvert y\rvert, 0, 1)^2} \cdot$ uncertainty $L(\ln(p_T[\mathrm{TeV}]), \ln(0.1), \ln(2.5)) \cdot L(\lvert y\rvert, 1, 3) \cdot$ uncertainty
15	$L(\ln(p_T[\mathrm{TeV}]), \ln(0.1), \ln(2.5)) \cdot \sqrt{1 - L(\lvert y\rvert, 0, 2)^2} \cdot$ uncertainty $L(\ln(p_T[\mathrm{TeV}]), \ln(0.1), \ln(2.5)) \cdot L(\lvert y\rvert, 2, 3) \cdot$ uncertainty
16	$\sqrt{1 - L(\ln(p_T[\mathrm{TeV}]), \ln(0.1), \ln(2.5))^2} \cdot \sqrt{1 - L(\lvert y\rvert, 0, 1.5)^2} \cdot$ uncertainty $\sqrt{1 - L(\ln(p_T[\mathrm{TeV}]), \ln(0.1), \ln(2.5))^2} \cdot L(\lvert y\rvert, 1.5, 3) \cdot$ uncertainty
17	$\sqrt{1 - L(\ln(p_T[\mathrm{TeV}]), \ln(0.1), \ln(2.5))^2} \cdot \sqrt{1 - L(\lvert y\rvert, 0, 1)^2} \cdot$ uncertainty $\sqrt{1 - L(\ln(p_T[\mathrm{TeV}]), \ln(0.1), \ln(2.5))^2} \cdot L(\lvert y\rvert, 1, 3) \cdot$ uncertainty
18	$\sqrt{1 - L(\ln(p_T[\mathrm{TeV}]), \ln(0.1), \ln(2.5))^2} \cdot \sqrt{1 - L(\lvert y\rvert, 0, 2)^2} \cdot$ uncertainty $\sqrt{1 - L(\ln(p_T[\mathrm{TeV}]), \ln(0.1), \ln(2.5))^2} \cdot L(\lvert y\rvert, 2, 3) \cdot$ uncertainty

In each case the original uncertainty is split in 3, two as above and the 3rd to make up te size of the orginl uncertainty when added in quadrature

Under such treatment the χ2/NDP that were ~ 350/159, reduce to ~250/159

And does this sort of thing make a difference to the PDFs.
MSHT (arXiv: 2012.04684) have studied this on the 7 TeV jets with various decorrelation scenarios (similar to but not the same as those recommended by ATLAS)



AGAIN: None of this could have been anticipated in a pseudo-data study

Further work on differential distributions was done for 2101.05095
A PDF fit using ATLAS W,Z and W,Z+jets data
HERE's a complete list of what is correlated to what

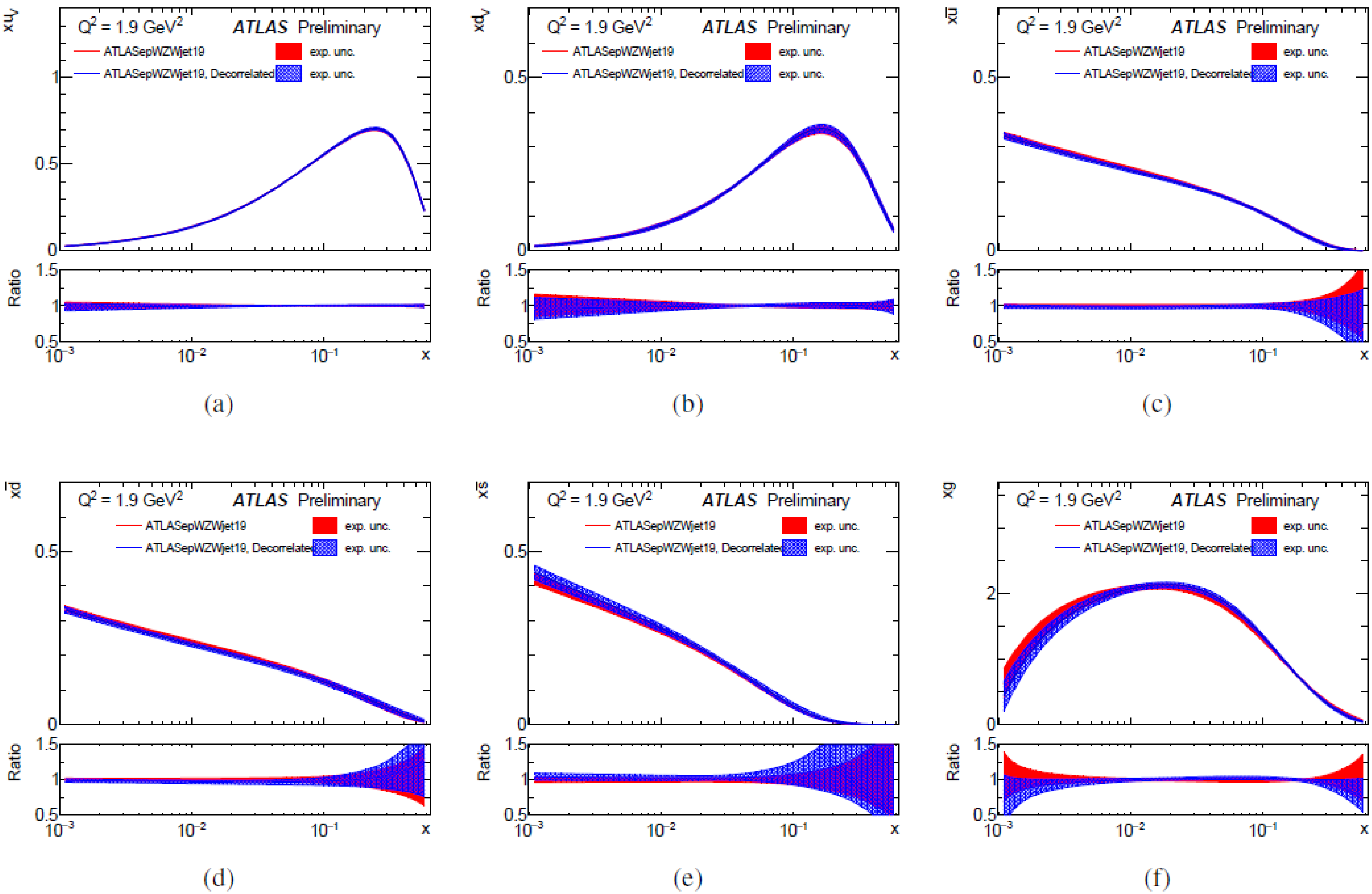
Systematic Uncertainty	7 TeV inclusive W, Z	8 TeV W + jets	8 TeV Z + jets
Jet energy scale [54]	*	JetScaleEff1	ATL_JESP1
		JetScaleEff2	ATL_JESP2
		JetScaleEff3	ATL_JESP3
		JetScaleEff4	ATL_JESP4
		JetScaleEff5	ATL_JESP5
		JetScaleEff6	ATL_JESP6
		JetScaleEta1	ATL_JESP7
		JetScaleEta2	ATL_JESP8
		JetScaleHighPt	ATL_JESP9
		JetScaleMC	ATL_JESP10
	JetScaleNPV	JetScalePileup1	ATL_PU_OffsetNPV
	JetScaleMu	JetScalePileup2	ATL_PU_OffsetMu
Jet punchthrough [54]	-	JetScalepunchT	ATL_PunchThrough
Jet resolution [54]	JetRes	JetResolution10	ATL_JER
Jet flavour composition [54]	-	JetScaleFlav1Known	ATL_Flavor_Comp
Jet flavour response [54]	-	JetScaleFlav2	ATL_Flavor_Response
Pile-up jet rejection (JVF) [55]	-	JetJVFcut	ATL_JVF
E_T^{miss} scale [56]	MetScaleWen	METScale	-
E_T^{miss} resolution [56]	MetRes	METResLong	-
		METResTrans	-
Electron energy scale [57]	*	ElScaleZee	ATL_ElecEnZee
Electron trigger efficiency [58]	*	ElSFTrigger	ATL_Trig
Electron reconstruction efficiency [59, 60]	*	ElSFReco	ATL_RecEff
Electron identification efficiency [59, 60]	*	ElSFId	ATL_IDEff
Luminosity [61, 62]	*	LumiUncert	ATL_lumi_2012_8TeV
WW background cross section [63]	*	XsecDibos	ATL_WW_xs
Top background cross section [64]	*	XsecTop	ATL_ttbar_xs

Would you have known that these contributions to the JES are the same from looking at their names on HEPDATA?

OR any of these below?

You have to ask the experimentalists..
OR RATHER perhaps we have to be more helpful !

And as usual we ask DOES IT MATTER?
Here is an example of PDFs a fit to HERA+ATLAS W,Z and W+jets data
With and without the correlations ATL-PHYS-PU-2019-106



It is not a big effect BUT it is visible– and worrying if we want 1% precision

Everyone knows that PDF uncertainties at high-x limit discovery physics- for 'obvious' discoveries of new particles at high energy

But PDF uncertainties are now also limiting discoveries based on deviations of SM parameters from SM values e.g. M_W, $\sin^2\theta_W$
This means we need to know PDFs better in kinematic regions where we already know them best x~ 0.01-0.1
We want ~1% precision…

BUT the path the precision lies through the data……

ISSUES at low-x

One seems to be able to use DGLAP by absorbing unconventional behaviour in the boundary conditions i.e. the unknown shapes of the non-perturbative parton distributions at Q_0^2

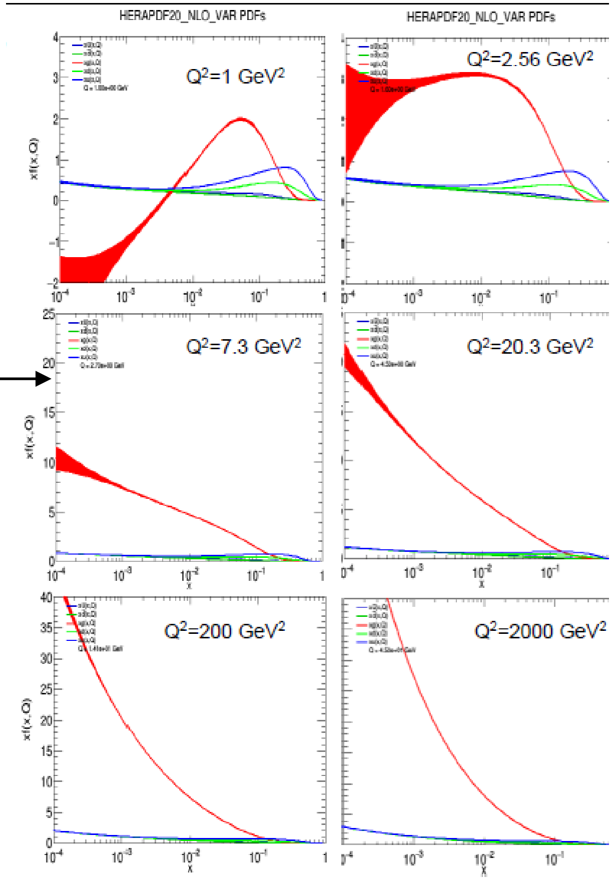
We measure, $F_2 \sim x\,q$

$$\frac{dF_2}{d\ln Q^2} \sim P_{qg} \cdot x\,g$$

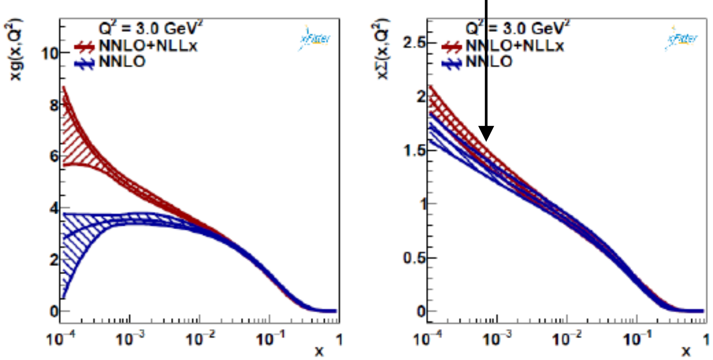
we can explain unusually steep $\frac{dF_2}{d\ln Q^2}$ by:

unusual $P_{qg} \rightarrow$ eg $ln(^1/_x)$, BFKL
OR unusual $x\, g(x,Q_0^2) \rightarrow$ “valence-like” gluon ..
And indeed the gluon is weird if you push this to low Q2, and this is worse, not better at NNLO

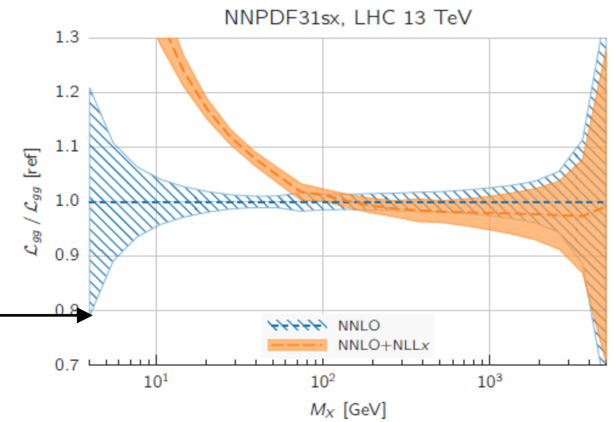
Conventional DGLAP



Conventional DGLAP extended with ln(1/x) resummation ‘BFKL’



And its consequences for the gluon-gluon luminosity at the LHC



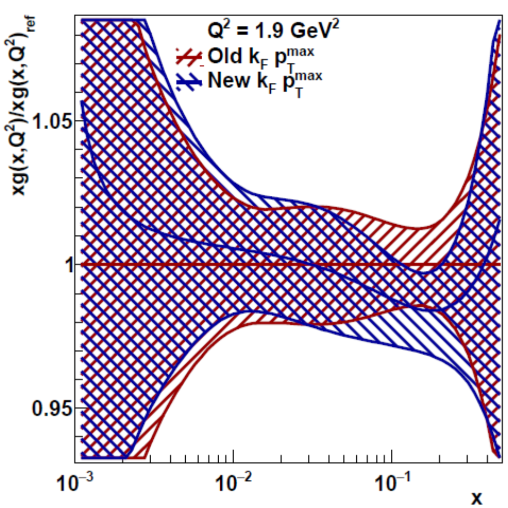
And there are further issues concerning predictions

1. For most processes we still use k-factors for NNLO predictions

Predictions must be altered many times as a PDF fit iterates towards the PDF parameters for the best χ2. Even NLO predictions are too time consuming to re-do at each iteration, Instead fast interpolation grids are used from **Applgrid or FastNLO**.

ONLY for the DIS HERA data and the LHC t-tbar data are these available at NNLO. The inclusive W,Z, W,Z+jets, direct photon production and inclusive jets processes are all predicted by grids at NLO and then k-factors of the NNLO/NLO prediction are applied to each bin (the **NNLOJet/ApplFast** project is a heroic effort to improve this– and it has taken years—that's why I say N3LO will not be so fast)

The k-factors are calculated with a fixed PDF—they could be iterated using the output PDF but this rarely happens—**DOES IT MATTER?**



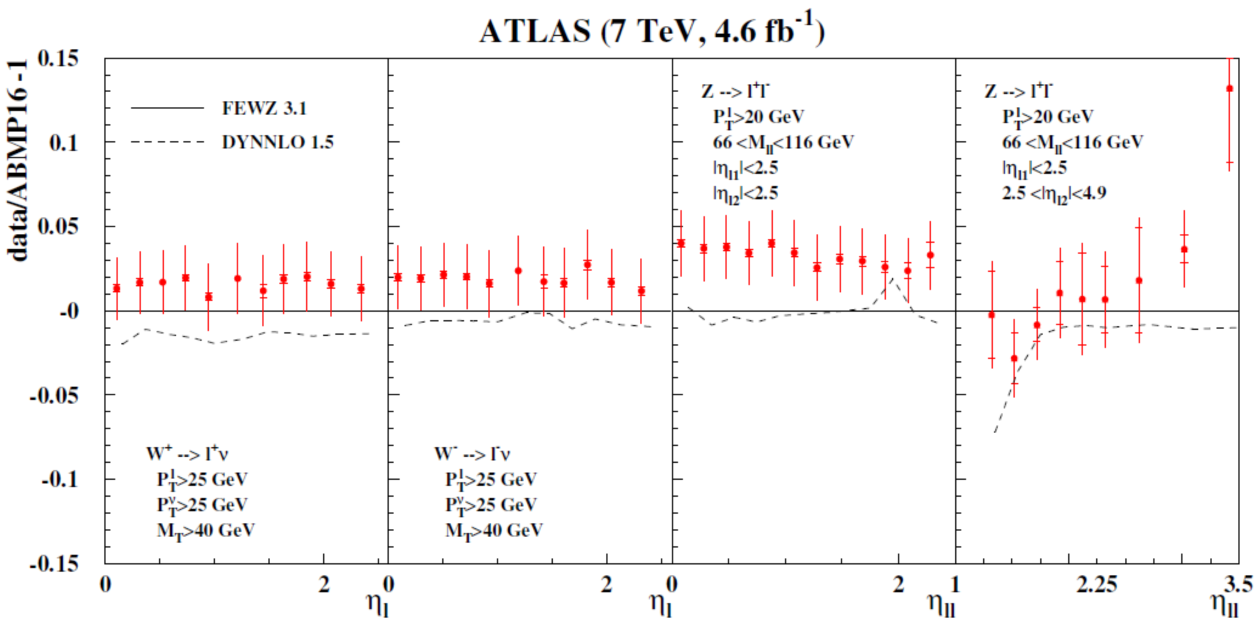
IF you want 1% accuracy then **YES IT DOES**
An illustration using k-factors for inclusive jet predictions with two different PDFs
Old used MMHT14
New used NNPDF3.1
BOTH use NNLOjet code
Ratio of resulting gluon PDFs is illustrated

Thanks to Francesco Giuli

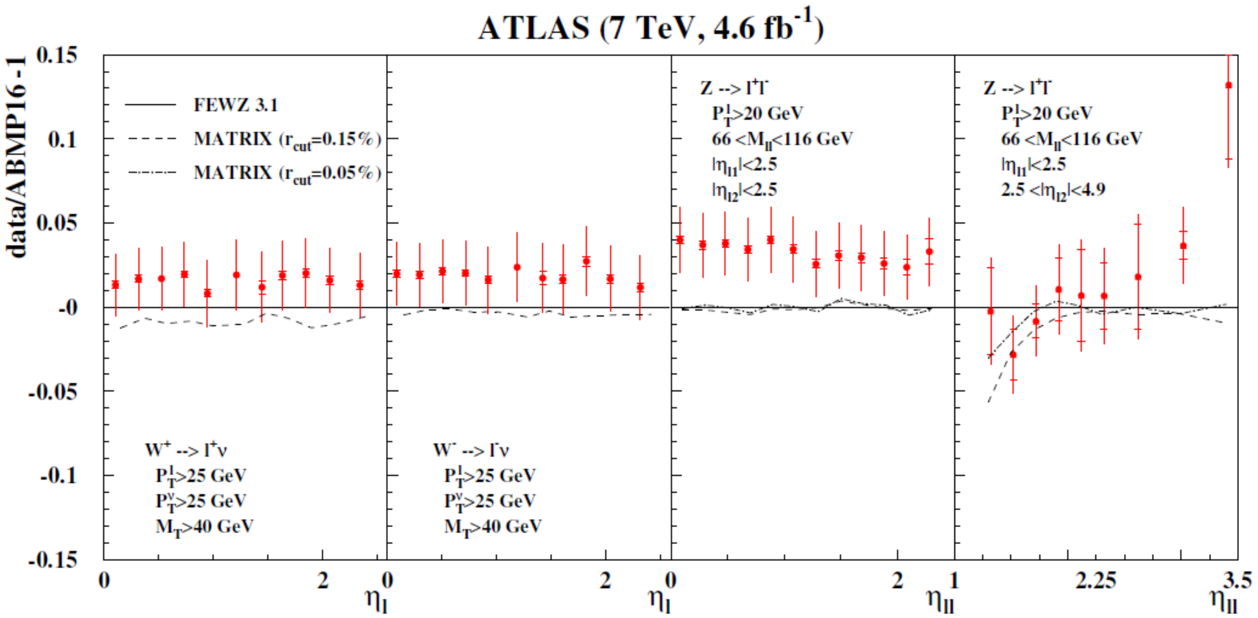
2. **All fitting groups do not use the SAME NLO grids and k-factors**

Some groups produce their own **grids**, some borrow from others, or use **Applgrid/FastNLO**
Everybody using the same validated source would help
There is progress—the **Ploughshare project (ploughshare.web.cern.ch)**

And then for **k-factors** groups use different codes
There has been a very recent warning about the use of different NNLO codes for Drell-Yan production arXiV: 2104.02400. There is particular sensitivity when experiments use non-optimal fiducial cuts



FEWZ and DYNNLO are particularly discrepant



But MATRIX and FEWZ are not in perfect agreement if 1% precision is required.

We need something like a Les Houches Accord on which grids to use and which calculations to use for which processes. (Unless there are good reasons to differ).

DOES IT MATTER?
Well that is part of the business of PDF benchmarking