How well do we really know what is inside the proton?

A proton is not a single solid object — it is made of smaller particles, and the way they share the proton's momentum has to be **measured from experiments, not calculated from theory**. The result is a set of curves called **parton distribution functions**. Every prediction made at the Large Hadron Collider depends on them, and on the **error bars** attached to them.

WHAT THIS PROJECT IS ABOUT

Those error bars are not measured — they are **computed**, and there are **three standard ways** to compute them. The field assumes the three broadly agree. Maria asked me to check. **They do not.**

1 · THE CONTEXT
Who produces these proton maps, why Maria's group built a new tool, and what she asked me to do.

2 · THE PHYSICS
What is actually being measured, and where the error bar comes from.

3 · WHAT I RAN
Four weeks, 28 experiments — validating the tools and measuring the underlying problem.

4 · WHAT I FOUND
The three methods disagree sharply. One of them is right — and I can show which.

SPEAKER NOTES

Orientation first, no numbers yet. "This is the read-out on the project you set me in July. I'll do it in four parts: the context, the physics, what I ran, and what I found."

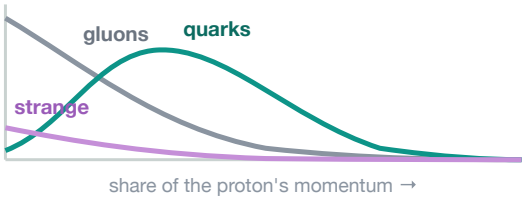
If she wants the punchline immediately: the three uncertainty prescriptions differ by up to a factor of 144 on real data, closure testing says the converged Bayesian is the trustworthy one, and the residual problem is the strange sector.

What I understand this programme to be

Proton structure, and how honestly we can state its uncertainty. Six points, in order:

1 What we are mapping

A proton is not one solid particle. It is a swarm of **quarks and gluons** sharing its momentum. A **parton distribution function** is simply the curve saying how likely each one is to carry a given share.



one curve per particle type · **slide 7**

2 Why anyone needs them

These curves **cannot be calculated** — the maths is intractable. They must be **fitted to thousands of experiments**. Every prediction at the Large Hadron Collider then uses them as an input.

PDF × collision = prediction · **slide 8**

3 Who actually makes them

Four collaborations worldwide. **NNPDF** (“Neural Network PDF”, Europe), **MSHT** (Martin–Stirling–Thorne–Watt, UK), **CT** (CTEQ–TEA, US), **JAM** (Jefferson Lab, US). Everyone else downloads their results.

I use the **MSHT** proton model · **slide 4**

4 The problem: the ± is a choice

Each fit must also state **how uncertain the curve is**. That number is not measured — it is computed, and there are **three accepted recipes**. Each group builds one recipe into its

5 What Maria's group built

Her Cambridge programme asks whether apparent “new physics” is really a **mis-modelled proton**. Her group released **Colibri** — the first program that can run **all three recipes**

6 What I am doing, and why it is hard

I put the real MSHT proton model into Colibri and ran all three recipes on the same fit. It is hard because the model has **more freedom than the data can pin down** — so the recipes

SPEAKER NOTES

This is the "do I understand what I've been asked to do" slide. Walk the six in order; each one is expanded later in the deck, and the card footers say where.

Expansions if she probes: **NNPDF** = Neural Network PDF collaboration (Italy/UK/Netherlands/Spain); **MSHT** = Martin, Stirling, Thorne, Watt — the UK group whose 2020 set (MSHT20) I re-implemented; **CT** = CTEQ–TEA (US); **JAM** = Jefferson Lab Angular Momentum (US).

The honest framing of point 6: the difficulty is not computational, it is that the question is genuinely ill-posed along the directions the data cannot constrain — 14 of the 52 parameter-combinations.

The curves are fitted to data, not calculated — by four groups worldwide

There is no equation that gives these curves from first principles: the maths of the strong force cannot be solved that way. So each curve is **fitted** — adjusted until it matches thousands of past collision measurements. Only a handful of groups do this. Everyone else uses their results.

GROUP	WHAT THE LETTERS STAND FOR	WHERE	HOW THEY DESCRIBE THE PROTON	WHAT THEY PUBLISH
NNPDF	Neural Network PDF	Europe	A **neural network** — a flexible computer model with no fixed shape	**NNPDF4.0** — a central curve + **~100–1000 replicas**
MSHT	Martin, Stirling, Thorne, Watt — four physicists' surnames	United Kingdom	A fixed **algebraic formula** `the model I use`	**MSHT20** — a central curve + **~60 error members**
CT	CTEQ–TEA — a US theory-and-experiment project	United States	A fixed **algebraic formula**	**CT18** — a central curve + **~60 error members**
JAM	Jefferson Lab Angular Momentum	United States	A fixed **algebraic formula**	**JAM** sets — a central curve + **replicas**

SO WHAT IS THE ACTUAL DELIVERABLE?

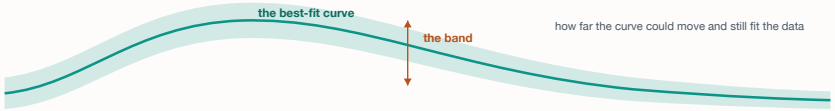
Not a program — a **data product**. Each group publishes a **"PDF set"**: numerical tables of the fitted curves, distributed through **LHAPDF**, the shared library every collider analysis reads. Notice the pattern in the last column — **the uncertainty recipe decides the form of what they ship.** Hessian groups

SPEAKER NOTES

The point to land: fitting is a modelling exercise, not a measurement, so reasonable people using the same data get slightly different curves. That is normal and expected.

Deliberately left off this table: **how each group computes the uncertainty**. That is the next slide, and it is where the real problem lives.

What “the uncertainty band” is, and the three ways to compute it



SCHEMATIC the band is the honest statement of doubt — and it is computed , not measured

RECIPE 1

one fit · cheapest

Hessian

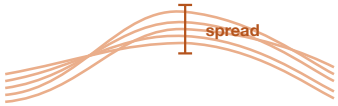
Measure **how sharply the fit worsens** as you step away from the best point. Steep walls mean a small uncertainty; shallow walls mean a large one.

- **Assumes** one clean, well-defined best fit.
- **Breaks if** a direction is flat — the maths cannot be inverted.

RECIPE 2

~100 fits

Monte-Carlo replicas



Make about a hundred **noisy copies of the data**, re-fit every one, and take how much the answers scatter as the uncertainty.

- **Assumes** the scatter of re-fits equals the uncertainty.
- **Breaks if** re-fits wander to the edge of the allowed range.

RECIPE 3

slowest · most complete

Bayesian posterior

Map out **the whole range of settings** the data permits, and read the spread of the resulting curves straight off.

- **Assumes** a stated prior, and a sampler that finishes.
- **Breaks if** the sampler stops before exploring properly.

All three answer the same question by different logic — so when the data is decisive they agree, and when it is not, they need not.

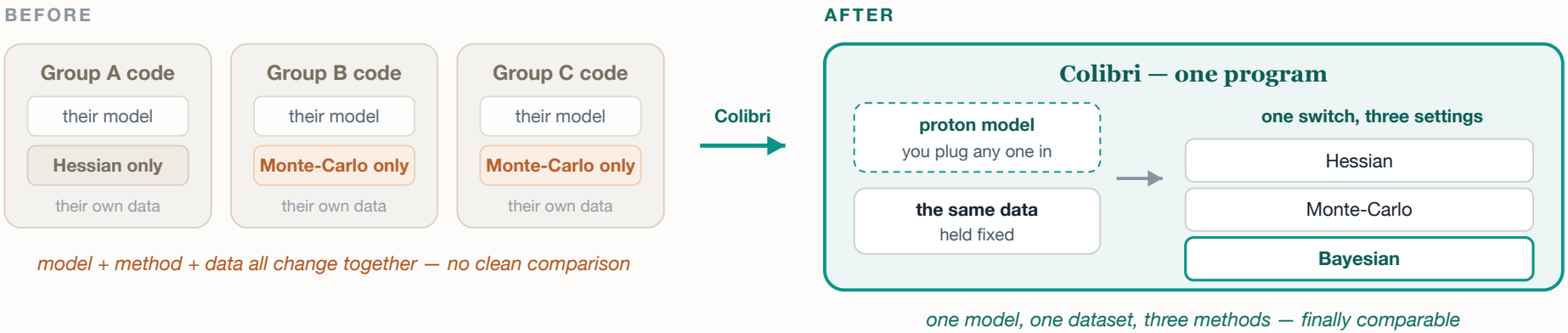
SPEAKER NOTES

Define the band first: it is not measured, it is computed from the fit, and it is the honest statement of how much the curve could still move.

Technical detail if she asks: Hessian = second derivative of χ^2 at the minimum, band = $\sqrt{(J^T H^{-1} J)}$, inflated by a tolerance in production fits. Monte-Carlo = resample the data within its covariance and refit; exact only in the linear-Gaussian limit. Bayesian = sample the posterior $P(\theta|D) \propto L \cdot \pi$; the only one that is exact for a non-linear model, but it needs a prior and a converged sampler.

One program that can run all three methods on the very same fit

Maria Ubiali leads a programme at Cambridge called **Physics Beyond the Standard Proton**, funded by the European Research Council. Its worry: a hint of new physics at the collider might really be the proton being mis-modelled. So the size of that band has to be trustworthy.



model + method + data all change together — no clean comparison

one model, one dataset, three methods — finally comparable

SCHEMATIC what changed · Colibri released 2025 by the Cambridge group (Ubiali et al.)

Now you can hold the model and the data completely fixed and change *only* the method — so any difference you see is caused by the method alone.

SPEAKER NOTES

The scientific stake is why the plumbing matters: if the programme is going to say an LHC anomaly is or is not new physics, the proton's uncertainty band has to be trustworthy rather than an artefact of which recipe was used.

Colibri is built on the existing shared data and theory infrastructure, so it is not a rival fitting group — it is a measuring instrument for comparing methods. Its published demonstration used a simplified 13-setting toy proton, where all three methods agreed.

What Maria asked me to do

Her brief, from our meeting and follow-up email (7–8 July 2026). The last line is what this deck answers.

1 — Move from a toy model to a real one

Colibri had only been demonstrated on the 13-parameter Les Houches toy. Implement **MSHT20** — "one of the most broadly used PDF sets in LHC phenomenology" — as a new Colibri model.

done

2 — Start with data we already understand

Use the same DIS data as the earlier toy fits so the comparison is clean — then scale up.

done

3 — Compare Hessian vs Monte-Carlo vs Bayesian uncertainties

Her stated **final goal** — the head-to-head that Colibri makes possible for the first time.

this deck

A real, widely-used parametrisation · real data · all three uncertainty methods under one roof.

SPEAKER NOTES

Say plainly that this is **her** question and I am reporting back on it. The novelty is not a new method — it is the first like-for-like test: same parametrisation, same data, same code, three uncertainty prescriptions.

FPPDF (arXiv:2602.07118) released the MSHT20 parametrisation publicly; I used it as the reference to validate my port against — that is where the 96-check parity gate comes from.

What I did, and what came out

The published demonstration of Colibri used a simplified **13-setting toy proton**, and there all three methods agreed. I swapped in the **real 52-setting** proton form and compared the methods on real data. Everything after this slide is the detail behind these numbers.

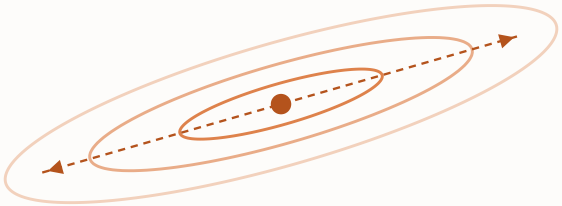
THE MODEL
52 settings
the real MSHT proton form, re-implemented — not the 13-setting toy

THE DATA
3 092
real measurements of electrons and neutrinos scattering off protons

THE WORK
28 runs
over four weeks — validating the tools, then the comparison itself

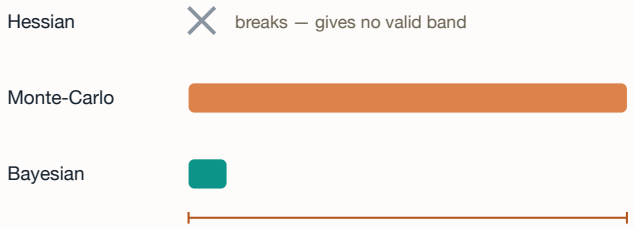
THE OBSTACLE
14 of 52
settings the data cannot pin down at all — mostly the strange quarks

WHY IT IS HARD



a "flat direction" — the fit slides freely, the data does not object
each ring = settings that fit the data equally well · worst for strange quarks

WHAT CAME OUT — SAME FIT, SAME DATA



the two surviving methods differ by 12–144×
band width, drawn to scale

SCHEMATIC + OUR RESULT left: why the question is ill-posed · right: the spread we measure on real data

Where the data is silent, the method — not the measurement — decides how big the uncertainty looks.

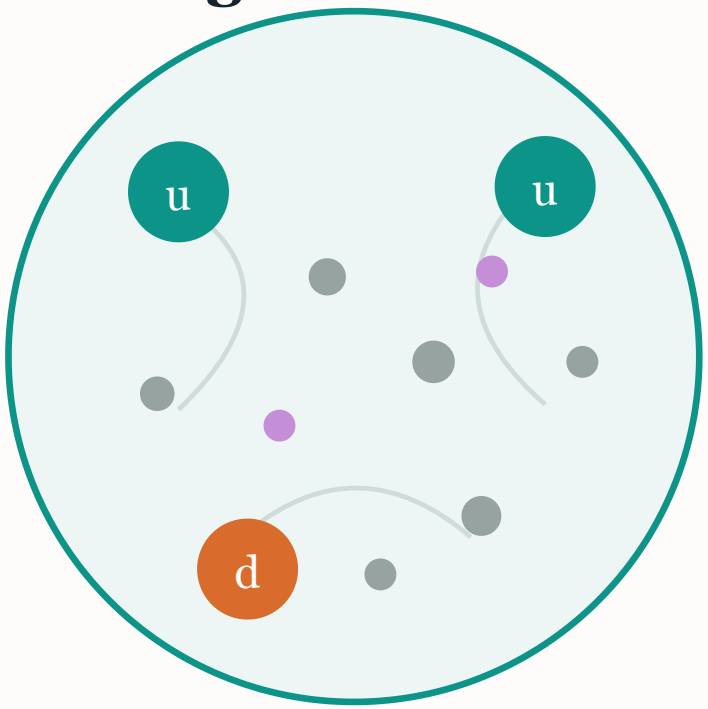
SPEAKER NOTES

This is the "everything at a glance" slide — scope, obstacle, outcome — before the detail starts. If she only remembers one slide, this is it.

The toy model in Colibri's paper had no flat directions, so all three methods agreed and the problem stayed hidden. The real 52-setting form exposes it. That the worst directions are the strange quarks is a concrete, checkable claim — it comes out of the information analysis in part 3, not from assumption.

Careful wording: 12–144× is **our measurement**, not a textbook number. The Hessian does not merely give a wide band — its maths breaks down and returns no valid band at all.

A proton is not one particle — it is a swarm of quarks and gluons



the proton

- 3 **valence quarks** (u u d) — these give the proton its identity
- **gluons** — they bind it together, and carry about half its momentum
- the **sea** — short-lived quark–antiquark pairs, including **strange**

collectively these are called *partons*

SCHEMATIC a picture of the constituents, not a measurement

At LHC energies a proton–proton collision is really a collision between **one parton from each proton**. To predict any rate, you must know how likely each parton is to be there, carrying each share of the momentum.

So "what is inside the proton" is not background — it is an input to every prediction.

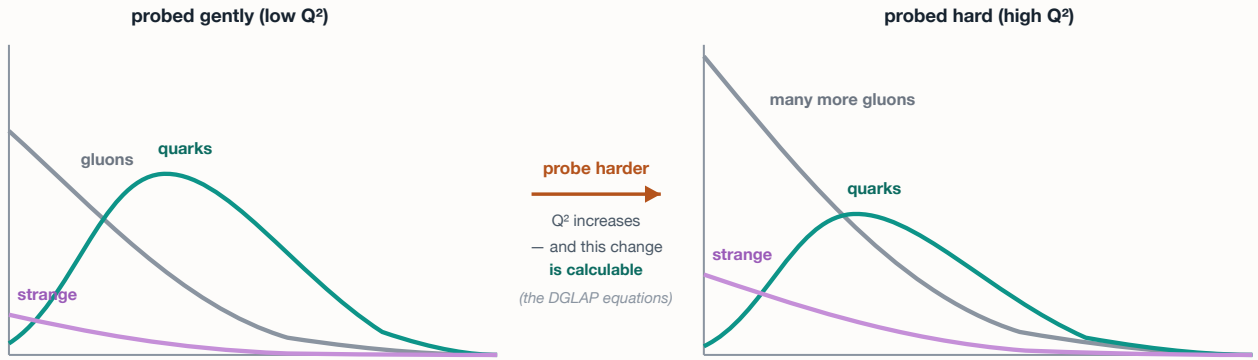
SPEAKER NOTES

Keep this fast with Maria — she knows it. Its only job is to fix vocabulary before I use "flavour" and "strange sector" later. The sea is where the difficulty lives: DIS data barely constrains it.

A parton distribution function is the curve that says how the momentum is shared

$f_i(x, Q^2)$ = how likely parton *i* carries fraction **x** of the momentum, when probed at energy **Q**

one curve per parton type — and the curve **changes** with the energy you probe at



SCHEMATIC illustrative shapes · the real curves are fitted — that is what this project is about

A PDF depends on **two** things: the momentum share **x**, and the energy **Q** you probe at. Probe harder and you resolve more structure — many more low-x gluons. Crucially, **the change with Q is calculable** — that is what the **DGLAP equations** do. What must be **fitted** is only the shape at one starting energy; DGLAP then carries it to every other energy.

SPEAKER NOTES

Formally f(x,Q²) is a number density. The Q² dependence is calculable (DGLAP evolution); the x-shape at the starting scale is not — that is what gets fitted.

I work in the standard evolution basis: **Σ** (singlet — all quarks and antiquarks), **g** (gluon), **V** (valence), and **T8** — the combination carrying strange. T8 is the one that causes trouble later.

PDFs cannot be calculated from theory — they must be fitted to data

SCHEMATIC collinear factorisation — the structure of every LHC prediction

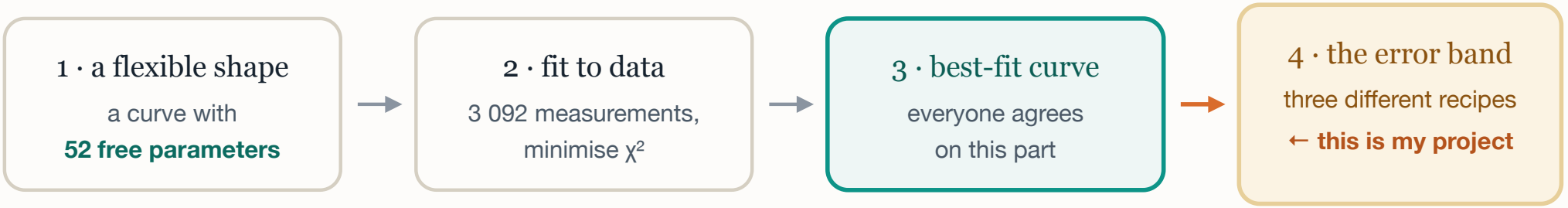
The PDF part is **non-perturbative**: there is no first-principles calculation. It is measured indirectly, by fitting flexible curves to thousands of scattering measurements. That fitted PDF — and its error bar — is then an input to **every** LHC prediction.

On many LHC measurements, the PDF uncertainty is the single largest theory uncertainty.

SPEAKER NOTES

The practical consequence to stress: because PDFs are universal, if their error bars are wrong they are wrong *coherently* across many analyses at once. That is why the community argues about how the uncertainty is **defined**, not just how big it is.

The fit gives a curve and a band. The band is where the disagreement lives.



SCHEMATIC the standard PDF-fitting chain, with my scope marked

THE SHAPE I USE — "MSHT20"

One of the standard published PDF forms (from the Martin–Thorne group). A flexible curve with **52 free parameters** — these are what the fit adjusts.

THE DATA I FIT TO

Deep-inelastic scattering — electrons and neutrinos fired at protons. **19 published datasets, 3 092 numbers** (SLAC, BCDMS, NMC, HERA, CHORUS, NuTeV). Each number is one **binned cross-section** at a given (x, Q^2) — itself distilled from **millions of raw collisions** — and they come with a **3 092 × 3 092 covariance matrix** (~9.6 M entries) describing how their errors are correlated.

THE SOFTWARE — "COLIBRI"

A PDF-fitting framework written in JAX. I re-implemented MSHT20 inside it so the fit is **auto-differentiable** — which part 3 needs.

Steps 1–3 are settled — everyone fits the same way and gets essentially the same central curve. **Step 4 is not settled.**
Converting "how well did the data pin down those 52 parameters" into a band on the curve can be done three different ways.

SPEAKER NOTES

Data: full-DIS — SLAC, BCDMS, NMC fixed-target F_2; HERA neutral and charged current; CHORUS and NuTeV neutrino DIS. Theory 40000000, t_0 covariance.

Why Colibri: re-implementing MSHT20's functional form in JAX makes the likelihood auto-differentiable. That is what makes gradient-based sampling and a direct Fisher-information computation possible — both essential in part 3.

They disagree when the model has more freedom than the data can pin down

Data pins it down

one answer → the three methods agree

A “flat direction”

many equally-good answers → the methods differ

SCHEMATIC each contour = parameter settings that fit the data equally well

DIS data cannot distinguish certain **combinations** of the 52 parameters — you can change them a lot and the fit quality barely moves. Along such a direction the error bar is no longer set by the data. **It is set by the method.**

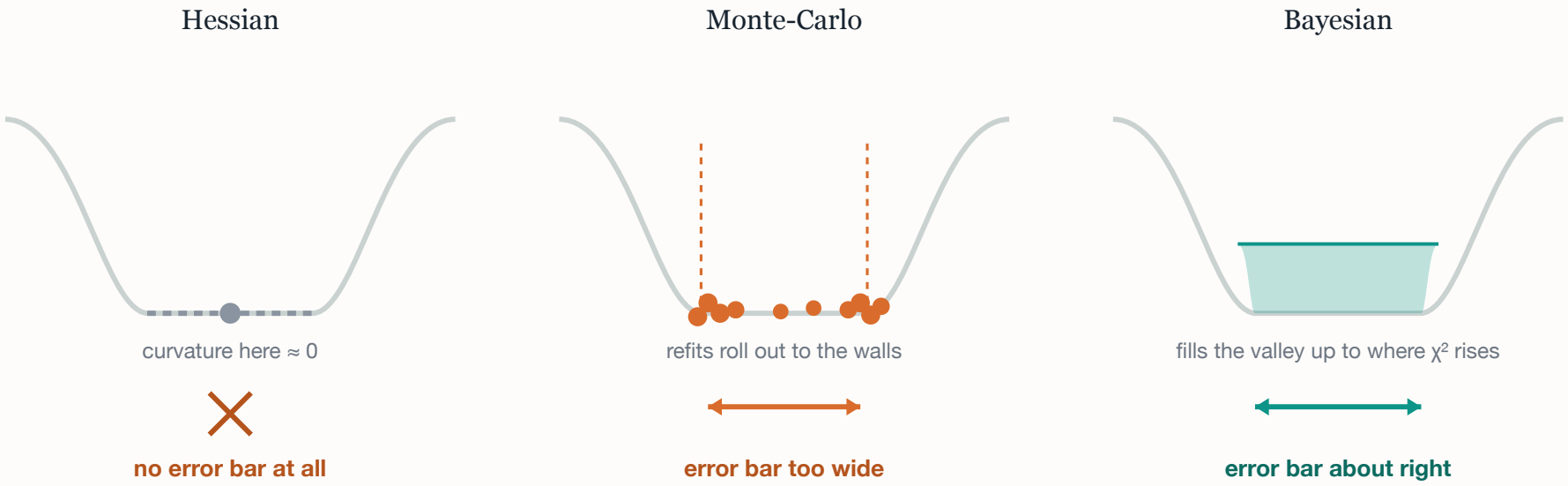
DIS is nearly blind to the light sea and to strange quarks — that is exactly where the flat directions are.

SPEAKER NOTES

This is the crux — pause and check she agrees before part 3. Modern parametrisations are deliberately flexible so they don't bias the fit; the price is unconstrained directions.

Specifically DIS structure functions cannot separate $\bar{u}$ from $\bar{d}$, barely see the strange sea and the s–$\bar{s}$ asymmetry, and are weak on high-x valence. The physical cure is data that *does* see those flavours: Drell-Yan and W/Z, and ultimately ν-DIS dimuon and W+charm for strange.

The same flat valley, read three different ways



SCHEMATIC the curve is the fit quality (χ^2) along a flat direction — low means "fits the data well"

All three methods are looking at the **same valley**. The Hessian only measures the **steepness at the bottom** — and a flat valley has none. Monte-Carlo lets noisy refits **roll along the floor** until the edge of the allowed range stops them. The Bayesian method **fills the valley** up to where the fit quality starts to degrade.

This one picture is the whole result: the disagreement is not a bug — it is three honest answers to an ill-posed question.

SPEAKER NOTES

Spend time here — this is the slide that makes everything else obvious. Draw the valley in the air if it helps: along a flat direction the fit quality is essentially constant, so "how uncertain are we?" has no unique answer.

Hessian = second derivative at the minimum. Flat → zero (or, off the exact minimum, negative) → H^{-1} blows up or is imaginary. **Monte-Carlo** = the spread of refits, which is bounded only by the prior box, so it measures the box, not the data. **Bayesian** = integrates the likelihood over the valley, which is the question we actually meant to ask — but it is also prior-limited if the valley runs to the box edge.

28 experiments in three stages, each with its success criteria fixed in advance

STAGE 1 · PPDF-1…11 — validation

Check the tools

Reproduced a published closure-test paper, then ported the full MSHT20 parametrisation into Colibri.

- 96-check parity gate against the reference — passed to 10^{-9}.
- Caught a hidden factor in the reference that is invisible at published parameter values.

STAGE 2 · PPDF-12…16 — real data

Fit real data — and find the problem

MSHT20 fits real DIS well ($\chi^2/N \approx 1.0$). But the fit turned out to be **degenerate**.

- An earlier rigid model floored at $\chi^2/N \approx 5.6$ — that floor was model rigidity, not the data.
- The flexibility that fixed it is what creates the degeneracy.

STAGE 3 · PPDF-18…28 — the result

Test the three methods

Measured the degeneracy directly, compared all three uncertainty methods, and re-checked my own conclusions.

- Six pre-registered phases; 3 complete, 1 partial.
- One conclusion retracted after a convergence study.

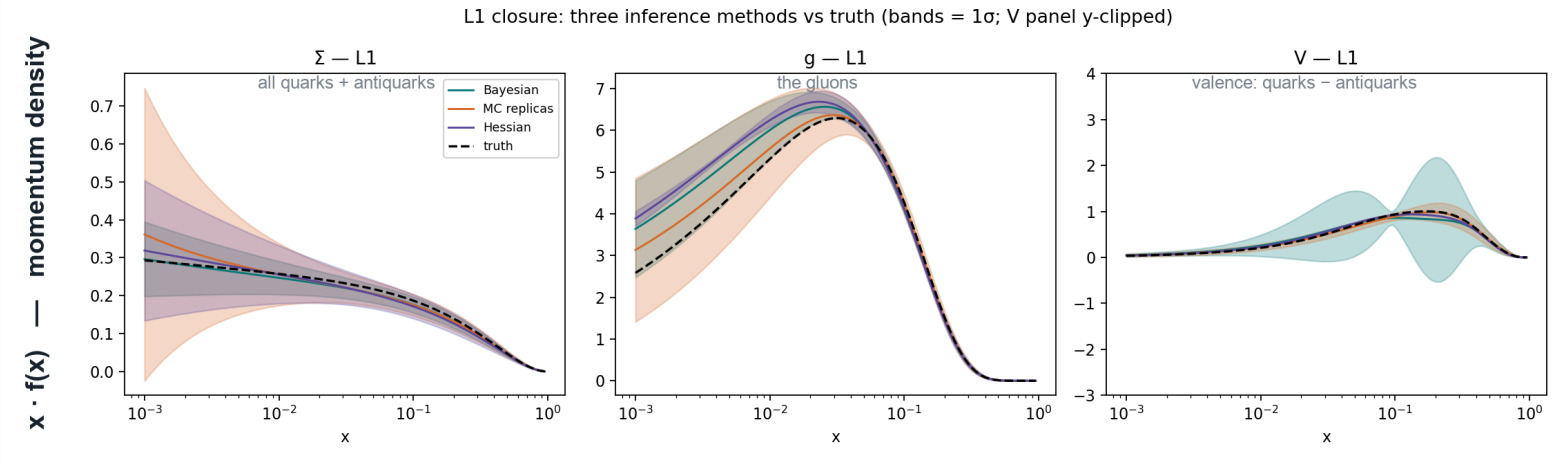
Everything — including two bugs I caught and one claim I withdrew — is recorded in a single results ledger.

SPEAKER NOTES

Stage 2 detail if she asks: the rigid 13-parameter model gave $\chi^2/N \approx 5.6$ on real DIS and making it *more* flexible didn't help — Bayesian evidence even preferred the rigid model. MSHT20 solved it ($\chi^2/N \approx 1.0$, $\Delta\log Z \approx +1450$). So the floor was model rigidity, not data tensions or theory settings.

Control test: where the data is strong, all three methods agree with the truth

A **closure test**: I invent a proton with known curves, generate 3 092 fake measurements from it with realistic noise, then fit that fake data **three times** — once with each uncertainty method, using exactly the same code as the real fit.



`KNOWN-TRUTH TEST` we invent a proton, generate fake data from it, and fit it — so the right answer is known · `x` = momentum share (log scale) · `black dashed = the truth` · coloured bands = each method's 1σ error bar

Dashed line = the truth we planted. Bands = the three methods. For the quark singlet and the gluon, all three sit on the truth. **This proves the machinery is correct** — so any later disagreement is physics, not a bug. Notice the third panel: the bands already separate where the data runs out.

SPEAKER NOTES

Closure testing is what makes the whole project possible. Plant a known truth, generate pseudo-data with realistic noise, fit it, then measure **coverage** — the fraction of x-points where the quoted 1σ band actually contains the truth. Nominal for 1σ is ≈68%; my pre-declared pass mark was ≥55%.

I also validated on a 2-parameter toy with an exactly computable posterior: in the benign regime all three recovered the true band to within 3%; in an extreme flat valley each failed in its own characteristic way.

I measured how much of the model the data fails to constrain

WHAT I RAN

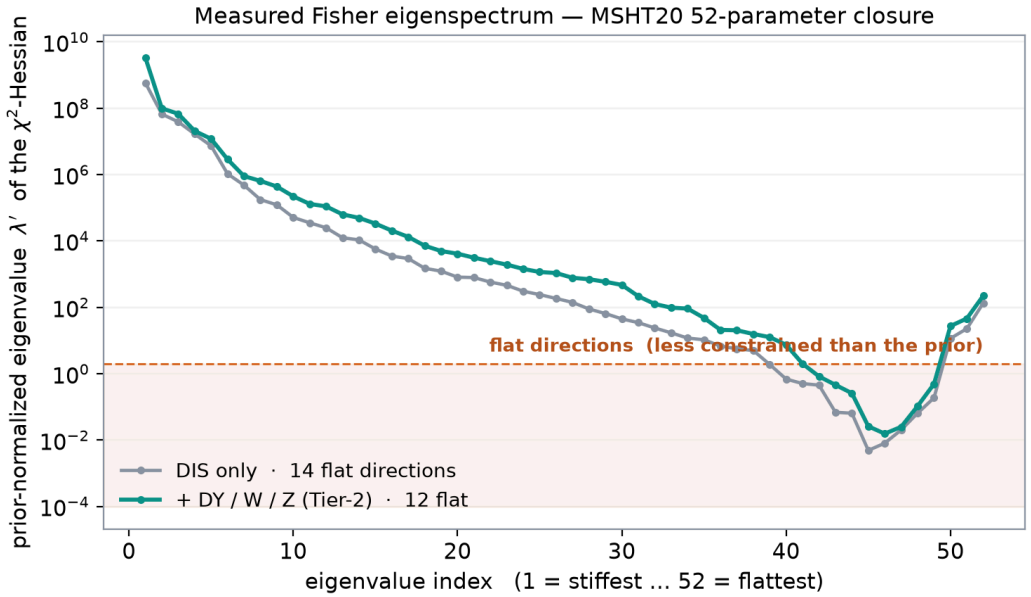
A direct measurement of **how much freedom the data actually removes**, for each of the 52 parameters.

HOW

Compute the curvature of χ^2 at the known truth, then find the **combinations** of parameters it constrains best and worst.

WHAT TO LOOK AT

The **tail on the right** — combinations sitting in the shaded band are ones the data barely sees.



MEASURED Fisher information — computed fresh for this deck

Every point is one **combination** of the 52 parameters, ranked from best- to worst-constrained. Points in the shaded band are ones the data barely sees at all.

14 of 52

directions unconstrained using DIS data alone

12 of 52

still unconstrained after adding Drell-Yan and W/Z data

More data helps — but only a little. And the worst-constrained directions are the **strange quarks**.

3 · χ^2–Hessian at truth, prior–normalised · condition number ≈ 10^{11}

SPEAKER NOTES

Method: the Fisher information matrix is the χ^2 Hessian evaluated at the planted truth. Its eigenvalues say how tightly each parameter combination is constrained. I prior-normalise so "flat" means genuinely less constrained than the prior itself.

Eigenvector decomposition names them: #1 the s–s̄ asymmetry, #2 the strange sea, then high-x down-valence. An earlier independent analysis gave 16→12 — same partial-cure conclusion.

Control: pinning 15 high-order coefficients removes the worst flat directions — so this is genuine excess model flexibility, not a data artifact.

The standard sampler could not converge on this problem

WHAT I RAN

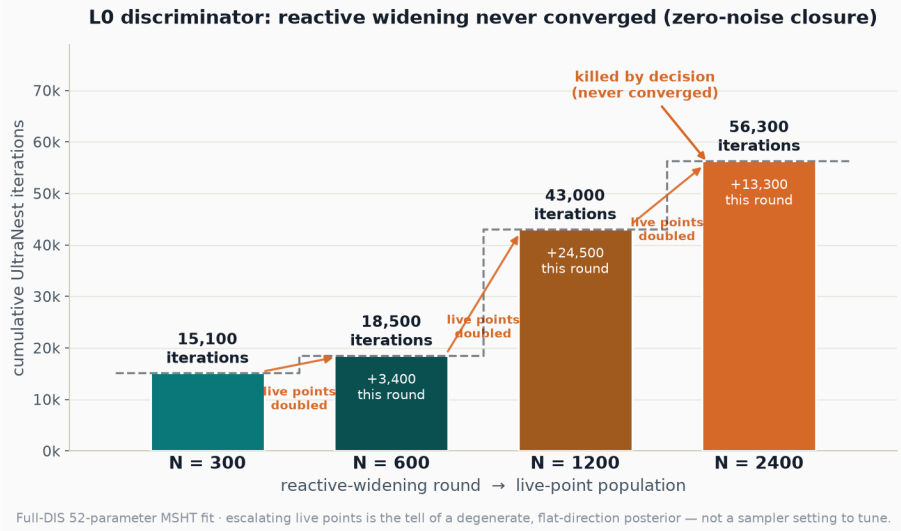
The standard Bayesian sampler (**nested sampling**) on the 52-parameter fit — the method the field normally reaches for.

HOW

Each round **doubles the sampler's effort**. A healthy problem settles; the run is stopped when it clearly will not.

WHAT TO LOOK AT

The bars are cumulative work. They keep **climbing** instead of levelling off.



DIAGNOSTIC our own run — nested sampling, effort doubled four times

Doubling the sampler's effort four times just made it run longer without settling. That is not a tuning problem — **an escalation that never converges is the signature of a flat posterior.** Switching to a gradient-based sampler, informed by the Fisher geometry, converged in about an hour.

SPEAKER NOTES

The 52-dimensional posterior has condition number ≈10^{13}. Nested sampling escalated live points 300→600→1200→2400 and never settled. Gradient NUTS with a dense, Fisher-preconditioned mass matrix — handing the sampler the geometry of the degeneracy — converges: ESS 226, $\hat{R} < 1.05$.

This matters for the next slide: it is what let me tell a real effect apart from a numerical artefact.

On real data, the three methods give error bars 12–144× apart

WHAT I RAN

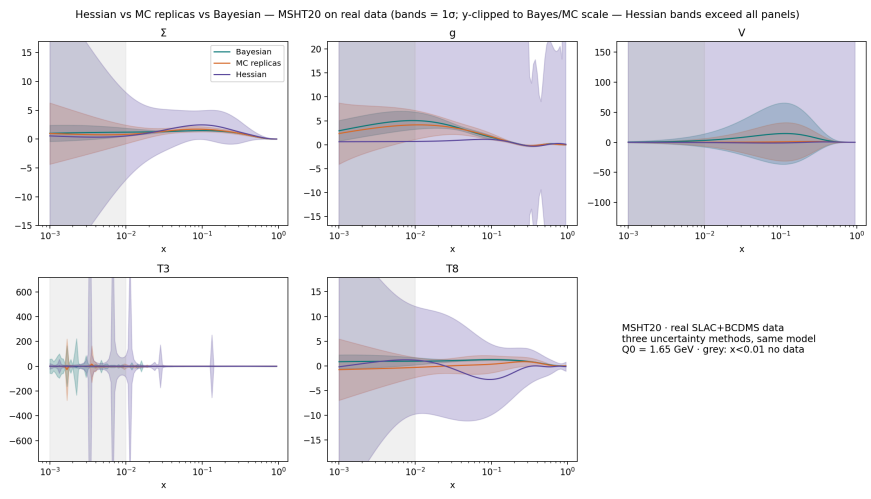
The **real** proton fit — actual DIS measurements, not simulated data, with all three uncertainty methods.

HOW

One fit, one best-fit curve. Then each method is applied to that same fit to produce its own error band.

WHAT TO LOOK AT

The **width** of each coloured band, panel by panel — same quantity, three very different answers.



REAL DATA real proton DIS measurements · same model, same fit, three uncertainty methods

Teal = Bayesian (narrow). Orange = Monte-Carlo (wide). Purple = Hessian, which runs off the edge of every panel. Quote the Bayesian and you say **±4%** on the gluon; quote Monte-Carlo and you say **±300%** for the same quantity.

The Hessian gives no valid answer at all — its curvature matrix has 10 negative eigenvalues.

4 · MC/Bayes ratio 144× Σ · 80× g · 12× V · 33× T8 · metric pre-declared before unsealing

SPEAKER NOTES

The Hessian's covariance is not positive-definite (10/52 negative eigenvalues, condition ≈6×10^{11}) — there is literally no Gaussian error to quote, not merely an imprecise one.

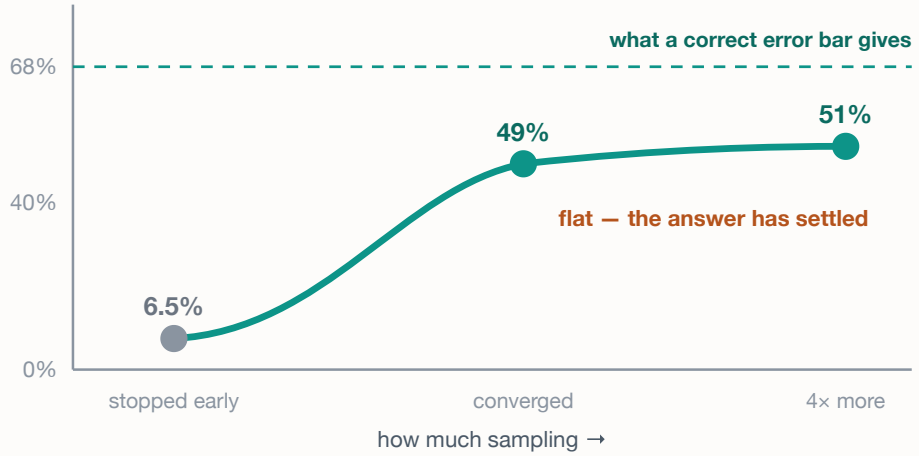
The comparison metric was written down and committed to version control **before** the real-data results were unsealed.

The bands are converged — more computing does not change them

THE WORRY
A Bayesian band is only trustworthy if the sampler explored the whole space. Mine may have stopped early.

THE TEST
Re-run the same fit with **4× more sampling** and watch whether the answer keeps moving or settles.

WHAT TO LOOK AT
If the curve **flattens**, more computing cannot widen the bands — what is left is real.



WHAT THE CHECK SHOWS

verified

Coverage climbs to **~58%** and then **stops moving**: quadrupling the sampling shifts it by less than two points. The bands have **settled**.

WHY THIS MATTERS

An unfinished sampler reports bands that are **too narrow** — the dangerous direction to fail in. Having ruled that out, the disagreement between the methods is a property of **the fit itself**, not of how long we ran the computer.

The measured disagreement is real — not an artefact of insufficient computing.

4 · convergence study · effective sample size 8 → 51 → 226 · coverage 6.5% → 49% → 51%, then flat

SPEAKER NOTES

If asked about the earlier number: an initial run of mine reported the Bayesian bands collapsing to ~2% of the PDF value. That run had not finished converging, and I withdrew it. This slide is the check that settled it — worth mentioning as evidence the analysis is self-policing, but no need to lead with it.

The independent cross-check: the converged real-data Bayesian band, computed on a different machine with a different initialisation, reproduces the earlier band. So the narrowness is the genuine posterior width.

Tested against a known truth, the Bayesian method is the reliable one

In a closure test the true curve is known, so we can simply count how often each method's error bar contains it. A correct 1σ band should contain the truth about **68%** of the time.

Bayesian	Contains the truth **58%** of the time — close to what it should be	`about right`
Monte-Carlo	Error bars **2–5× too wide** — it overstates the uncertainty	`too cautious`
Hessian	Gives **no valid error bar** in this regime — the mathematics breaks down	`unusable`

So the spread is not “nobody knows”. We can say which answer to use.

4 · coverage averaged over 5 independent noise realisations

SPEAKER NOTES

The verdict transfers to real data because the identical code, model and pipeline produce both.

Useful nuance: the two failing methods fail in **opposite directions** — MC too wide, an unconverged Bayesian too narrow. So the spread *between* methods is itself a warning that you are in a degenerate regime.

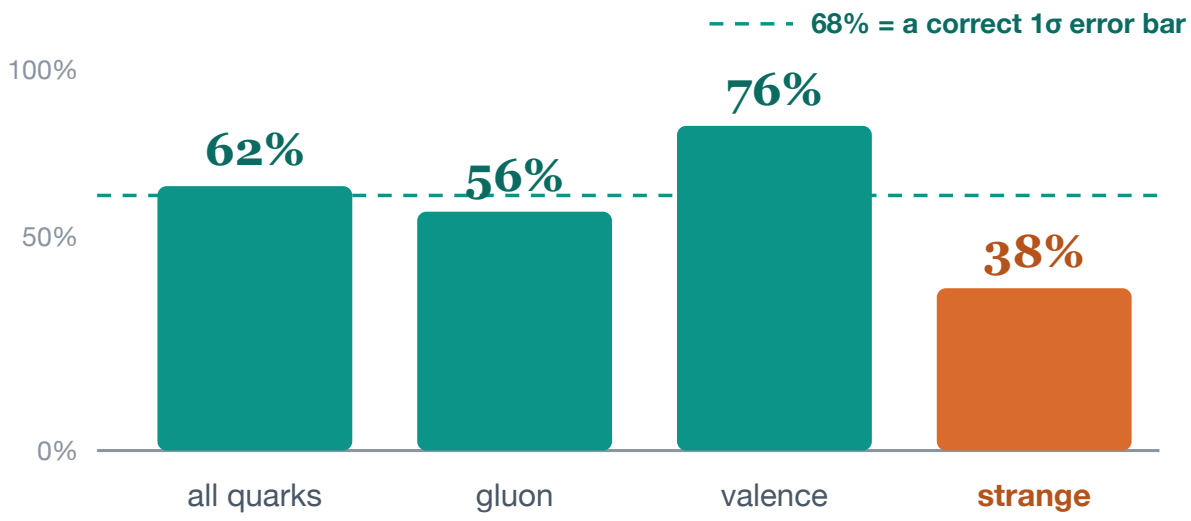
Honest caveat: "the Hessian breaks" is against the raw curvature. Production fits use a regularised / dynamic-tolerance Hessian plus parameter freezing — I have not yet given it that fair rematch.

Even the Bayesian method understates the uncertainty on strange quarks

WHAT I RAN
The **closure test again**, but scored separately for each type of parton, and repeated on **5 independent** noisy datasets.

HOW
For each one, count how often the quoted error bar actually contains the known truth, then average over the 5.

WHAT TO LOOK AT
Which bars reach the **dashed line**. Three do. **Strange does not.**



KNOWN-TRUTH TEST how often the error bar contains the truth, by parton type · 5 noise sets

For most of the proton the Bayesian error bar is about the right size. For **strange quarks it is too small** — the method is overconfident exactly where the data is weakest.

- Fails in **4 of 5** noise sets.
- Still 32% even in the best-converged run.
- It is the **same strange sector** the Fisher analysis flagged.

SPEAKER NOTES

Why five noise sets: single-draw coverage is noise-dominated — it swings 47%↔76% purely from the noise realisation and is uncorrelated with sampling quality. One closure test cannot be trusted.

Caveat to raise before she does: the strange parameters also have the lowest per-direction sampling quality (ESS ~10–58), so I cannot yet fully separate a genuinely prior-limited posterior from residual under-exploration. That is the first question on the next slide.

What this establishes

ESTABLISHED

evidence in this deck

- The three uncertainty methods **genuinely disagree** — by 12–144×, depending on the flavour — on real proton data.
- The cause is a **measured degeneracy**: 14 of 52 parameter directions are unconstrained by DIS.
- Tested against a known truth, the **converged Bayesian is the reliable choice**; Monte-Carlo overstates; the Hessian is invalid.
- The tools are validated, and one over-claim of mine has been found and withdrawn.

OPEN

next work

- Whether the **strange under-coverage** is physical or residual under-sampling.
- Whether it **generalises** beyond the MSHT20 functional form.
- How a **regularised** Hessian — the version production fits use — would score.
- How much of the flat-direction spread is **prior-driven** rather than data-driven.

A methodology result that is solid, self-corrected, and points at a specific piece of physics: the strange quark.

SPEAKER NOTES

Land here if time is short. If she wants detail, the appendix has the run ledger, the limitations, and next steps.

APPENDIX · A1

The run ledger

Every result in this deck with its modality stated. Nothing computed is presented as measured, and nothing from a closure test is presented as a real-data result.

CLOSURE TESTS — TRUTH IS KNOWN

our sim

PPDF-1…6	Reproduced a published closure paper — level-0 recovery to $\chi^2/N \approx 10^{-4}$, level-1 to 0.98; three-method comparison against truth
PPDF-10/11	MSHT20 ported to JAX; **96-check parity gate** to 10^{-9}; closure passes with coverage 65–100%
PPDF-19/20	Fisher degeneracy measured: **14 flat directions** (DIS) → 12 (+DY/W/Z); flattest are the strange sector
PPDF-24/25	Toy validation + the decisive convergence study over **5 noise sets** — coverage 58% mean, strange 38%
PPDF-26	Degeneracy sweep — **under-converged, reported inconclusive**

REAL PROTON DATA

real data

PPDF-7…9	First real-data fits with simpler models — $\chi^2/N \approx 5.6$ floor; two bugs found and logged
PPDF-12	MSHT20 on real DIS: **$\chi^2/N \approx 1.0$**, $\Delta\log Z \approx +1450$ — the floor was model rigidity
PPDF-13/14	Monte-Carlo ensemble (82 replicas) and Hessian — Hessian **indefinite, 11/52 negative**
PPDF-15/23	Three-method comparison on real data — the **12–144× spread**; the figure shown in part 3I
PPDF-28	Converged real-data Bayesian, blind metric pre-declared — reproduces the earlier band independently

SPEAKER NOTES

If she probes rigour, this is the slide. Note the honest entries: PPDF-26 is listed as inconclusive rather than quietly dropped, and PPDF-8/9 record bugs I made.

APPENDIX · A2

What this does not establish

Scope

- DIS-only data — **not a global fit**. Statements are about methods at this data-to-parameter ratio.
- One parametrisation (MSHT20). A neural-network form is untested.
- Real-data verdict is **transferred** from closure; no external anchor yet.

Method fairness

- The Hessian was tested in its **raw** form, not the regularised version production fits use.
- The degeneracy sweep under-converged, so the cure conclusion rests on the Fisher measurement.
- Prior-width sensitivity has not been scanned.

The strange result

- Strange parameters have the **lowest sampling quality**, so physical vs numerical is not yet fully separated.
- The Fisher spectrum used finite differences and a prior normalisation — the flat count is threshold-dependent.

None of these overturns the main finding — but each bounds what it means.

SPEAKER NOTES

Offer this slide proactively. With an advisor, naming your own limits first is what buys credibility for the parts you *are* claiming.

APPENDIX · A3

Where this goes next — and what I need from you

STEP 1 — ~3 h compute

Finish the sweep

Repeat the coverage test at several degeneracy levels, properly converged, to get the trend rather than two points.

I can do this myself.

STEP 2 — quick

A fair Hessian rematch

Re-test the Hessian in the regularised, dynamic-tolerance form that production fits actually use.

I can do this myself.

STEP 3 — needs your steer

Add strange-sensitive data

Neutrino-DIS dimuon and W+charm directly constrain strange. This would test whether pinning strange shrinks both the degeneracy *and* the method disagreement.

This is the ask — it needs FK-table integration.

Steps 1–2 tighten the methodology. Step 3 turns it into a physics result about the strange quark.

a methodology paper is the natural write-up: validation + convergence + fair comparison + blind real data

SPEAKER NOTES

Close on step 3. It is both the data-engineering ask and the standalone physics result — pinning strange is interesting independently of the uncertainty-method question.

APPENDIX · A4

Setup & references

THE SETUP, IN FULL

model	MSHT20 functional form, 52 free parameters, 4 analytic sum rules
framework	Colibri — JAX, auto-differentiable likelihood
data	full-DIS: 19 datasets, ≈3 092 points (SLAC, BCDMS, NMC, HERA NC/CC, CHORUS, NuTeV)
theory	FK tables, theoryid 40000000, t_0 covariance
closure	level-1 (realistic noise), planted truth = MSHT20 central values
metric	pointwise 1σ coverage for Σ, g, V, T8 at $x > 5\times10^{-4}$; pass ≥55%
sampler	numpyro NUTS, dense Fisher-preconditioned mass matrix, warmup 2500 / 3000 samples

REFERENCES & PROVENANCE

MSHT20	Bailey, Cridge, Harland-Lang, Martin, Thorne — Eur. Phys. J. C 81 (2021) 341
Colibri	PBSP JAX PDF-fitting framework
NNPDF4.0	comparison reference for global-fit quality
ledger	RESULTS_LEDGER.md — every number in this deck
lab log	HESSIAN_EIGENSPECTRUM_LOG.md — Fisher method and iterations
blind	real-data metric committed to version control before unsealing
full report	21-page written companion to this deck

SPEAKER NOTES

Leave this up during questions — it answers most setup queries without flipping back.