Apparent Excess in $e^+e^- \to$ hadrons

A story in three parts...

Part I hep-ex/0401034

1. *motivation*
2. *the data*
3. *the method*
4. *Standard Model prediction*
5. *results*

Part II hep-ex/0403157

1. *incorrect data !!*
2. *consequences...*
3. *discussion*

Part III

the ending...

Michael Schmitt
Northwestern University

Part I: **The story starts out rosey. . .**

First explain the basic analysis as advertised. . .

1.Motivation

We are talking about the cross section for the **inclusive** process

$$e^+e^- \longrightarrow \text{hadrons.}$$

This has been measured many times over the decades – *PEP*, *PETRA*, *TRISTAN*, *LEP*, etc. The selection is typically very loose in order to avoid systematic uncertainties from the acceptance.

It is known that both the LEP 2 and the LEP 1 measurements fall above the SM prediction – but the significance is not high.

Why not check the lower energy data to see what they say?

2. The Data

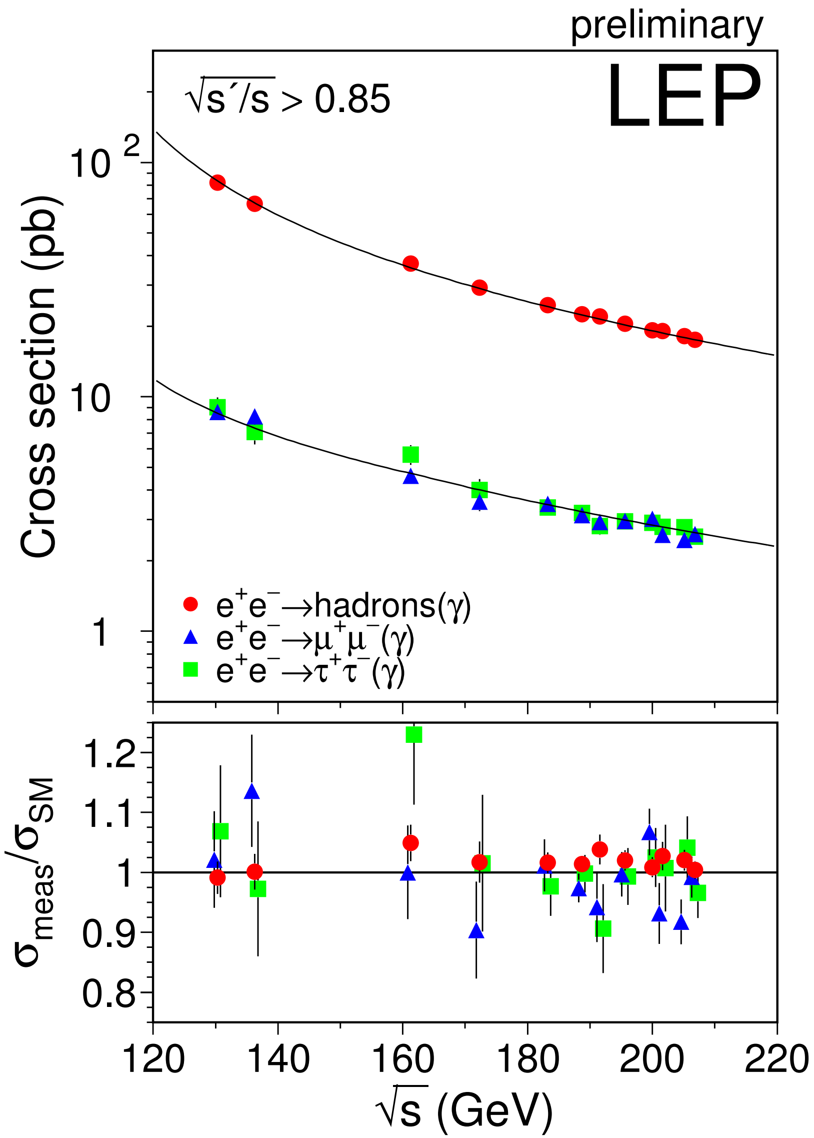
Starting from the top down,
we have first the LEP 2 Data:

$$130 \text{ GeV} < \sqrt{s} < 209 \text{ GeV},$$

where $\sqrt{s}$ is the center-of-mass energy,
$\sqrt{s} = E_{\rm cm} = 2 \times E_{\rm beam}$.

LEP 2 ran at twelve center-of-mass energies over several years.

These plots are from the LEPEWWG.

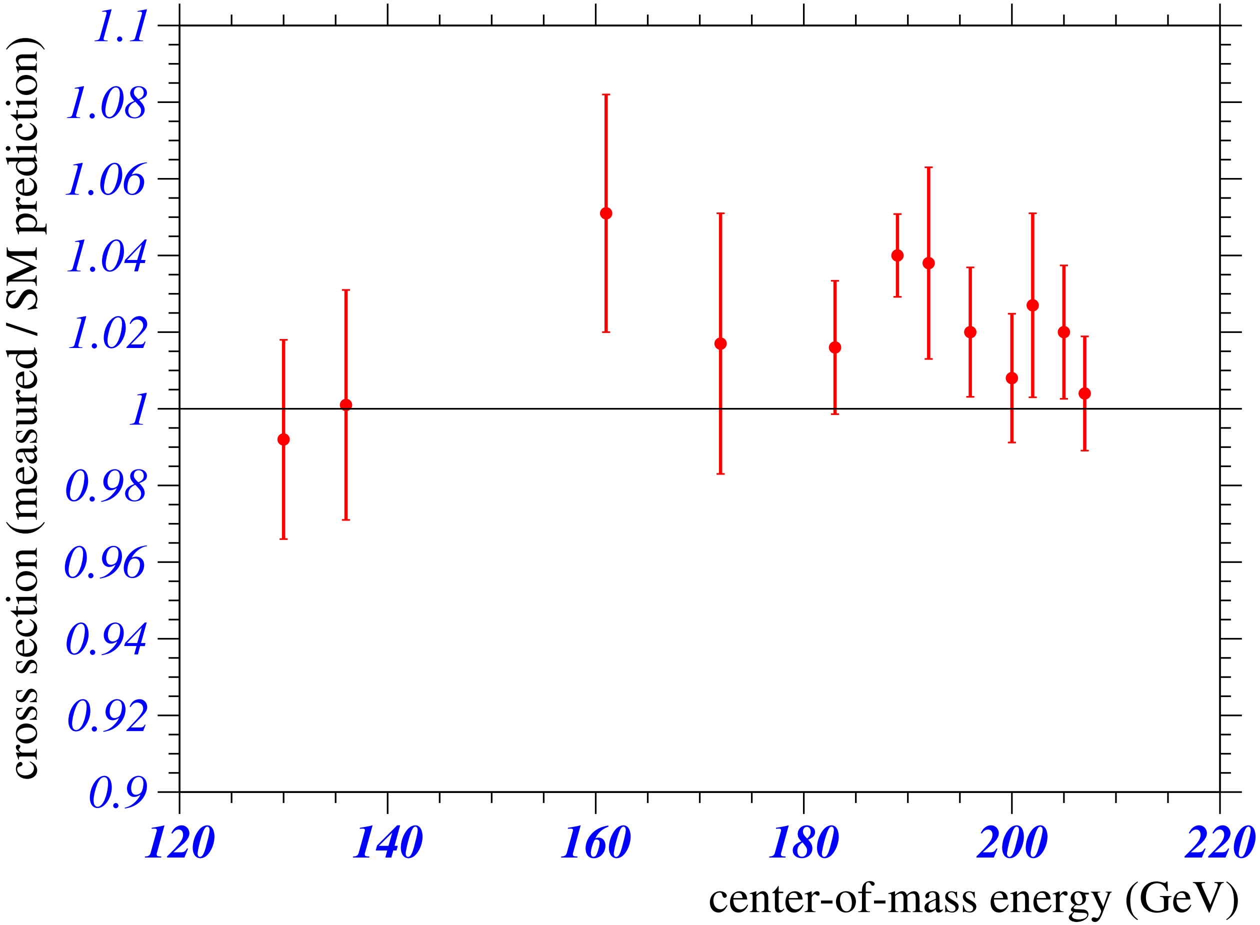


Here are the measurements and the SM predictions (in pb):

energy	combined measurement	SM prediction	difference	deviation
91.187	41540 ± 37	41478	62	1.68
130	82.1 ± 2.2	82.8	-0.7	-0.32
136	66.7 ± 2.0	66.6	0.1	0.05
161	37.0 ± 1.1	35.2	1.8	1.64
172	29.23 ± 0.99	28.74	0.49	0.49
183	24.59 ± 0.42	24.20	0.39	0.92
189	22.47 ± 0.24	22.16	0.31	1.29
192	22.05 ± 0.53	21.24	0.81	1.53
196	20.53 ± 0.34	20.13	0.40	1.18
200	19.25 ± 0.32	19.09	0.16	0.50
202	19.07 ± 0.44	18.57	0.50	1.13
205	18.17 ± 0.31	17.81	0.36	1.16
207	17.49 ± 0.26	17.42	0.07	0.27

Notice that nearly all points show a positive difference.

We can replot these data as the ratio: *(Measured/Predicted)*



It is clearly interesting to quantify the overall "excess."

One very simple way to compare the data to the prediction in an overall sense is to compute the **mean deviation** as follows:

- Let the measurements be y_i with uncertainties η_i.
  (We will take the measurements to be fullly independent, which means that the uncertainties are uncorrelated, for now. We return to correlations shortly...)
- Let the theoretical prediction be $y^{\rm SM}$, which is understood to be a function of $\sqrt{s}$.
- Then the mean deviation is simply

$$\bar{\Delta} \equiv \sum_{i=1}^{N} \left(\frac{y_i - y^{\rm SM}}{\eta_i^2} \right) \Big/ \sum_{i=1}^{N} \left(\frac{1}{\eta_i^2} \right) \qquad \text{and} \qquad \sigma_{\bar{\Delta}} \equiv \left[\sum_{i=1}^{N} \left(\frac{1}{\eta_i^2} \right) \right]^{-1/2} . \quad (1)$$

- The traditional "goodness" measure is χ^2 as follows:

$$\chi^2 = \sum_{i=1}^{N} \left(\frac{y_i - y^{\rm SM}}{\eta_i} \right)^2 \quad (2)$$

- For the LEP 2 data we have $\chi^2 = 12.2$, $\bar{\Delta} = 0.32$ pb and $\bar{\Delta}/\sigma_{\bar{\Delta}} = 2.8$ which means there is a 2.8 s.d. excess in this set alone!?!
  And, what about $\chi^2 = 12.2$ for 12 d.o.f.?

So why hasn't there been a lot of attention payed for this?

$\longrightarrow$ This treatment is too naive – correlations DO matter.

The correct expression for the χ^2 is the contraction of the inverse covariance matrix:

$$\chi^2 = \sum_{i,j=1}^{N} (y_i - y^{\rm SM})\,(\mathbf{C}^{-1})_{ij}\,(y_j - y^{\rm SM}) \qquad \mathbf{C} \equiv \begin{pmatrix} \sigma_1^2 & \rho_{12}\sigma_1\sigma_2 & \rho_{13}\sigma_1\sigma_3 \\ \rho_{12}\sigma_1\sigma_2 & \sigma_2^2 & \rho_{23}\sigma_2\sigma_3 \\ \rho_{13}\sigma_1\sigma_3 & \rho_{23}\sigma_2\sigma_3 & \sigma_3^2 \end{pmatrix}$$

- The LEPEWWG provide the complete correlation matrix as well as the errors (as listed in the table above) so it is straight forward to re-compute the χ^2.
- In general, $\rho_{ij} = 0.05$–0.2, so the correlations are not large.
- The upshot is that the signficance is 1.8σ instead of 2.8σ (which is also what the LEPEWWG report).

$\longrightarrow$ *One should look at other data sets...*

More Data

$\sigma(e^+e^- \to$ hadrons$)$
was also measured at LEP 1.

There are many measurements around the Z peak, spanning $88 < \sqrt{s} < 92$ GeV.

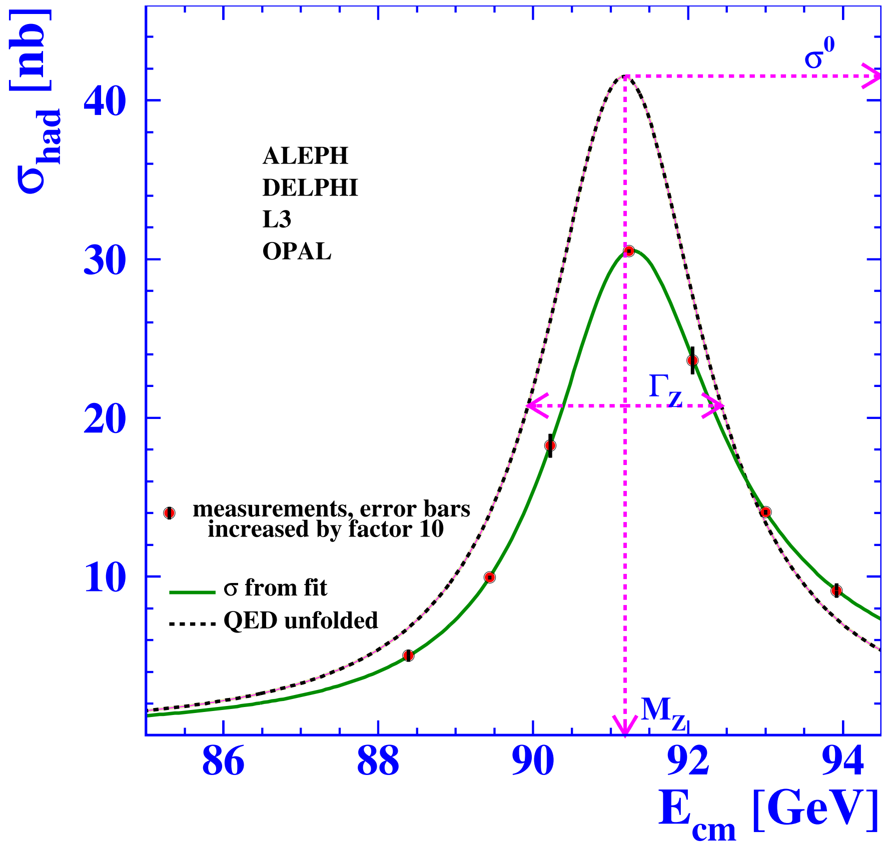
These have been combined into one number:

$$\sigma^0_{\rm had} = 41540 \pm 37 \text{ pb}$$

to be compared to

41478 pb in the SM.

From the LEPEWWG:



$\Delta = 62 \pm 37$ pb equiv. 1.7σ $\Longrightarrow$ also a small excess...

Any More Data?

Yes!

There are hundreds of cross section measurements made before the advent of LEP.

Do they show any excess?

Use a recent compilation by Zenin, *et al.*, which has QED corrections already applied.

Set a somewhat arbitrary cutoff $\sqrt{s} > 20$ GeV to avoid measurements with larger errors and problems with the theoretical prediction. Define 4 regions:

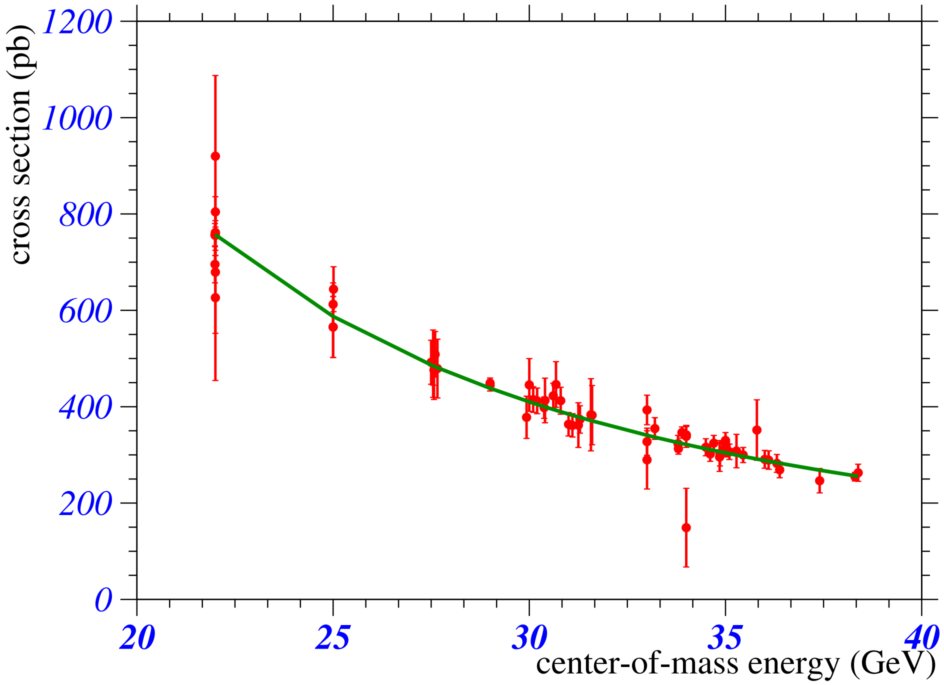
1	$20 < \sqrt{s} < 40$ GeV
2	$40 < \sqrt{s} < 70$ GeV
3	$88 < \sqrt{s} < 93$ GeV
4	$130 < \sqrt{s} < 210$ GeV

The first region is dominated by photon exchange, the second feels the influence of Z exchange, the third is dominated by the Z peak, and the fourth has roughly equal contributions from both γ^* and Z^*.

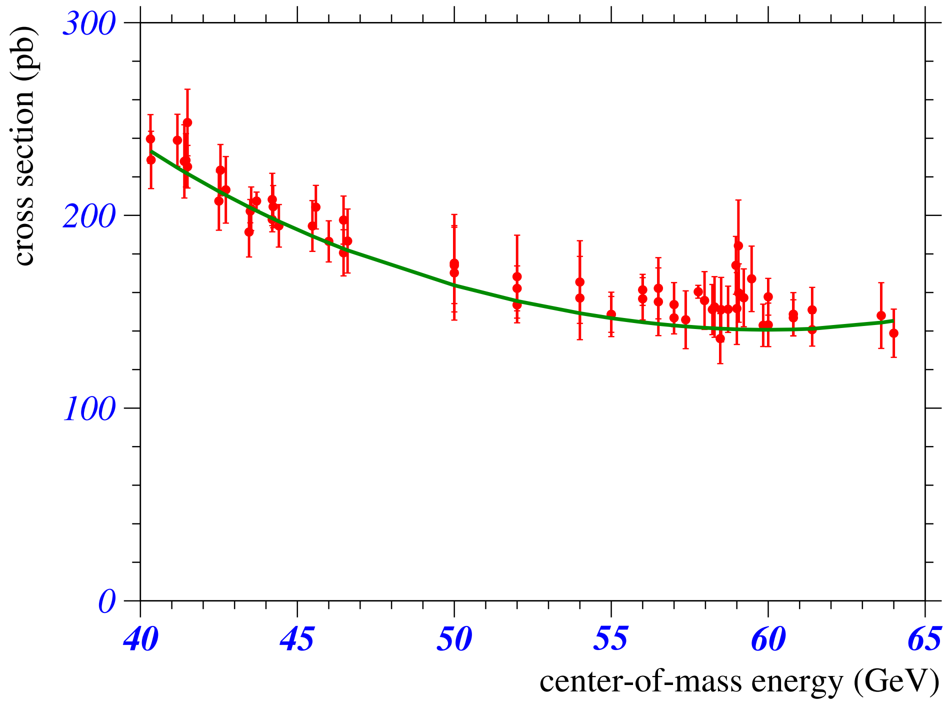
There are many measurements, experiments and calculations.

If there were an excess everywhere...

Region 1



Region 2



67 data points $\chi^2 = 50$

$\bar{\Delta} = 6.7$ pb, $\sigma_{\bar{\Delta}} = 2.5$ pb

$\Longrightarrow$ 2.9 s.d.

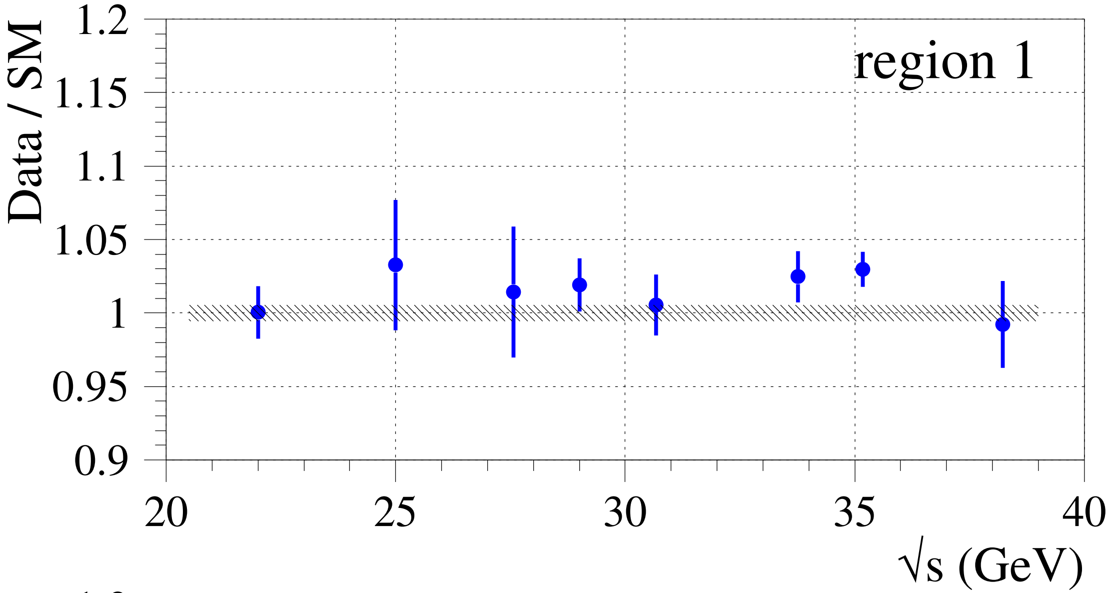
63 data points $\chi^2 = 55$

$\bar{\Delta} = 7.9$ pb, $\sigma_{\bar{\Delta}} = 1.4$ pb

$\Longrightarrow$ 5.6 s.d.

But it is hard to see the excess. . .

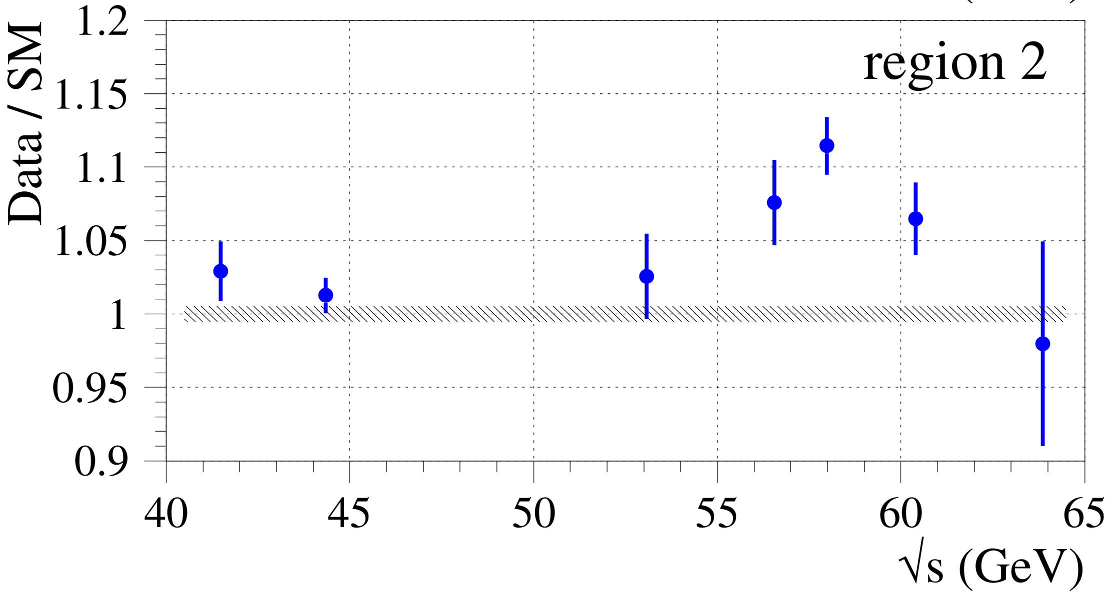
rebin



Combine data in ~ 1 GeV bins as a visual aid.

Plot the ratio

(Measured/Predicted):



3. Method

If we accept the statement that there is an overall excess everywhere, how to we combine the results from the four regions?

Use a likelihood method.

Instead of χ^2, which did not tell us anything, use:

$$\mathcal{P} \equiv \prod_{I=1}^{N} \frac{1}{\sqrt{2\pi}\,\eta_i} \exp\left(-\frac{1}{2\eta_i^2}(y_i - (y^{\mathrm{SM}} + \alpha y^{\mathrm{NP}}))^2\right). \tag{3}$$

(In fact it is more convenient to work with $\mathcal{F} \equiv -\ln \mathcal{P}$.)

The term $\alpha\, y^{\mathrm{NP}}$ represents a contribution from new physics:

$$y^{\mathrm{NP}}(s) \equiv (10\ \mathrm{pb}) \times \frac{(30\ \mathrm{GeV})^2}{s} \tag{4}$$

Compare $\mathcal{F}_{\min}$ to $\mathcal{F}$ for the SM alone ($\alpha = 0$). Improvement when a nonzero "new physics" contribution is included? This improvement can be described by

$$\mathrm{SD} = \sqrt{2\left[\mathcal{F}(0) - \mathcal{F}(\alpha_{\mathrm{best}})\right]} \tag{5}$$

The results are:

region	$\sqrt{s}$ range	α_{best}	$\mathcal{F}(\alpha_{\text{best}})$	$\mathcal{F}(0)$	S.D.
1	20–40	0.7	21.89	25.23	2.6
2	40–70	1.8	25.52	37.50	4.9
3	80–100	57	0	1.404	1.7
4	130–209	1.4	2.00	6.12	2.9
all	20–209	1.18	53.4	70.2	5.8

- Notice that S.D. is the same as $\bar{\Delta}/\sigma_{\bar{\Delta}}$ from before.
- Regions 1, 2 and 4 suggest $\alpha \sim 1$ but region 3 is much higher.
- The *net* significance is nearly 6σ!

NB: correlations & theoretical uncertainties not taken into account yet.

4. The Standard Model Prediction

This is an elementary calculation, at the Born level.

Improved Born approximation employes running coupling constants and masses, and a QCD correction for the final state.

In the 1990's it was crucial to make these SM calculations as accurate as humanly possible. One of the leading efforts is ZFITTER which has been cross checked with other high quality codes.

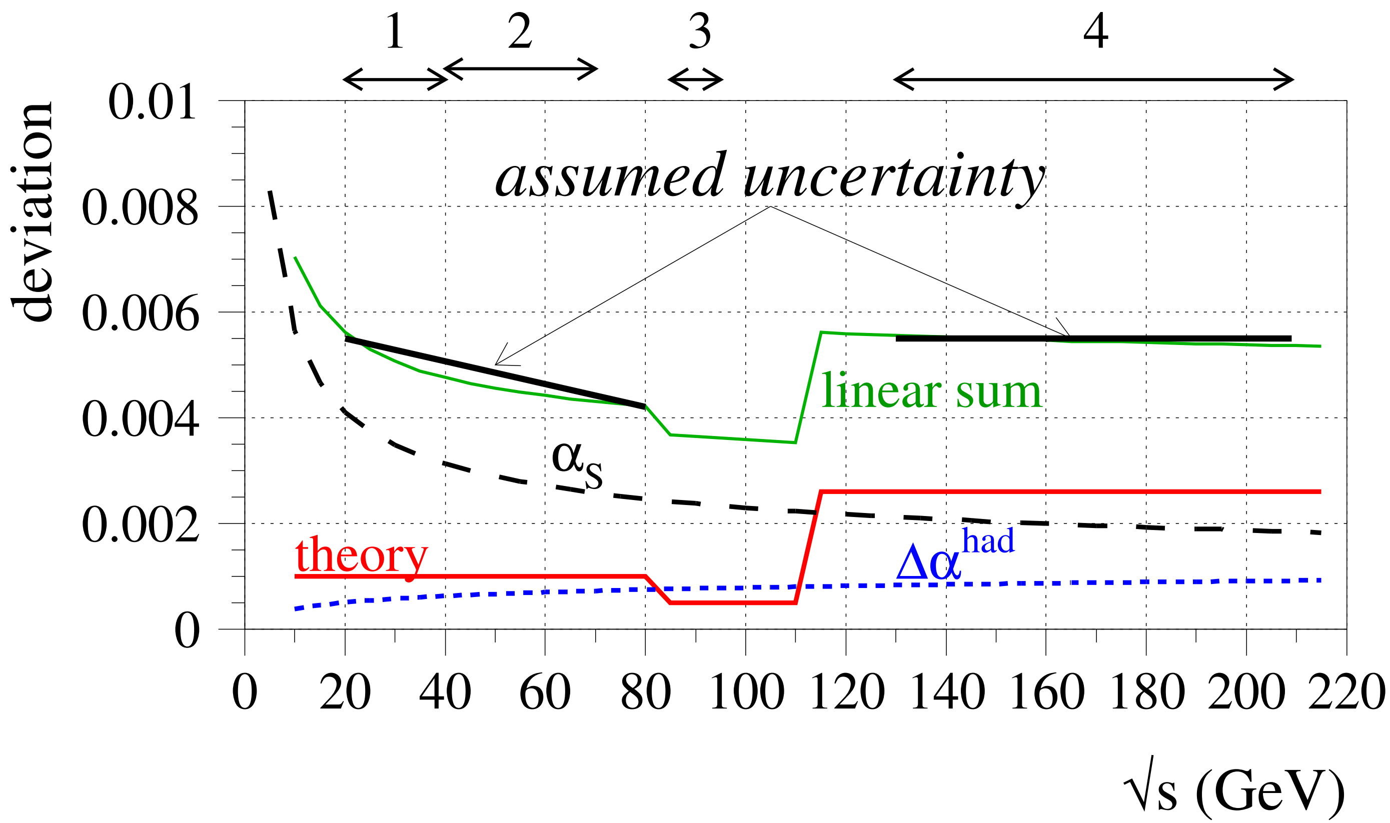
The LEPEWWG uses ZFITTER by default, and I use their numbers for regions 3 & 4.

For regions 1 & 2, I run ZFITTER myself. I also wrote my own code to compute $\sigma(e^+e^- \to \text{ hadrons})$ from scratch as a tool for understanding theoretical uncertainties.

Theoretical Uncertainties

- The numerical accuracy of ZFITTER is estimated to be 0.2% or better.
- The uncertainty from $\alpha_{\rm QED}(s)$ is estimated using the conservative uncertainty assigned by Jegerlehner on $\Delta\alpha^{(\rm had)}$.
- Take $\delta(\sigma)/\sigma \sim 2\,\delta(\alpha_{\rm QED})/\alpha_{\rm QED}$.
  This covers uncertainties in running $\sin^2\theta_W$.
- For running α_S, *double the usual uncertainty*, which should cover how α_S might change in presence of new physics. This dominates.
- Uncertainty from HO QCD corrections is negligible.
- Uncertainties from input parameters are negligible.

Take the *linear sum* of these already conservative uncertainties.



The heavy lines represent what we used in the calculation.

They are <u>not</u> negligible.

Incorporate this in the likelihood function.

One could argue that these are over-estimated by a factor of about 2.

Modify the likelihood function – include multiplier ρ for y^{SM}:

$$\mathcal{P} \equiv \left[\prod_{I=1}^{N} \exp\left(-\frac{1}{2\eta_i^2}(y_i - (\rho y^{\text{SM}} + \alpha y^{\text{NP}}))^2\right)\right] \times \exp\left(-\frac{1}{2\eta_\rho^2}(\rho - 1)^2\right)$$

$\mathcal{F}$ is now a function of α and ρ.

The optimal $\mathcal{F}$ will not change.

But when $\alpha = 0$ (which represents the case of the SM with no new NP), ρ imparts a degree of freedom which will allow $\mathcal{F}$ to decrease, so $\Delta\mathcal{F} = \mathcal{F}(\alpha = 0) - \mathcal{F}_{\min}$ will diminish.

Result: 6σ becomes 4σ.

We also need to take into account error correlations...

For region 4 (LEP 2) this was already discussed.

For regions 1 & 2, it is much trickier.

- the data appear in 21 publications
- assume correlations hold for the data from a given publication
- correlations should not be very large, because:
  - statistical errors usually are large
  - detector issues dominate the systematic error
  - different groups use different methods
- take one-half the systematic uncertainty to be correlated
- impact on $\Delta\mathcal{F}$ is small.

Result: net significance becomes 3.9σ.

Conclusion: the apparent excess survives the more careful treatment.

5. Results

The simple $1/s$ ansatz is special because it assumes the Z plays no role.

From the combined α value, we can infer that "new physics" is contributing

$$y^{\mathrm{SM}}(s) = (11.8 \pm 2.0\ \mathrm{pb}) \times \frac{(30\ \mathrm{GeV})^2}{s}$$

(This is 3% of the total hadronic cross section.)

At $\sqrt{s} = 30$ GeV, a muon has cross section of 110 pb, a charge -1/3 quark has 40 pb.

$\longrightarrow$ There must be a suppression by about a factor of 4.

One possibility is spin – a scalar quark has a cross section typically four times smaller than a quark, well above threshold.

There already are arguments why a light scalar bottom quark might exist.

Light Scalar Bottom Model

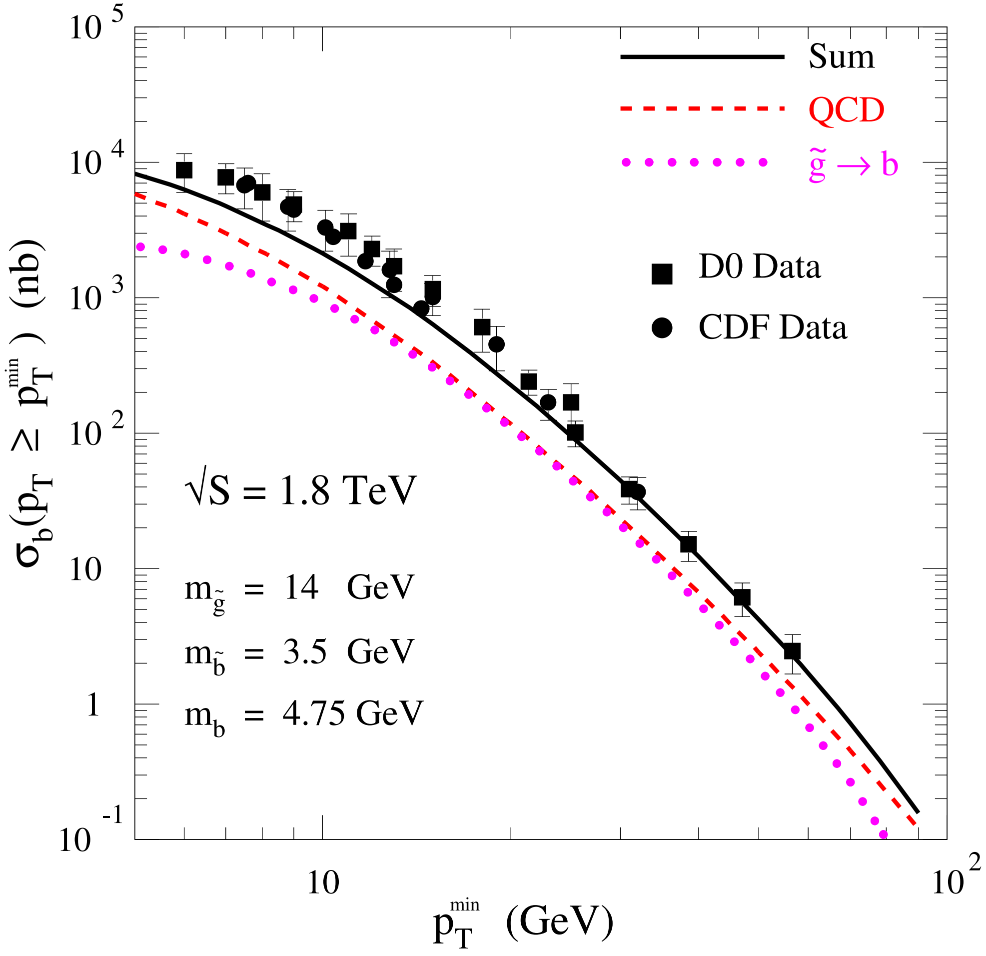
- mass range: 2 – 6 GeV
- decay: promptly via R-parity violation

$$\tilde{b}_1 \longrightarrow \bar{u}\bar{s} \quad \text{or} \quad \bar{c}\bar{s}$$

The anti-quarks hadronize and the event is hadronic.

- charge is -1/3 so coupling to the photon is fixed
- “Right” and “left” interaction eigenstates mix to form the mass eigenstates.
  The mixing parameter, $\cos\theta_{\tilde{b}_1}$, is not known – a free parameter.
  It is possible to have zero coupling to the Z.
  We will use the data to constrain the mixing $\cos\theta_{\tilde{b}_1}$.

- There is a light gluino in one model, but this plays no role in the present analysis.



Berger, Harris, Kaplan, Sullivan, Tait, Wagner

Berger *et al.* proposed this model to explain the excess in b-hadron production at the Tevatron.

They linked this mechanism to a number of other phenomena:

- Upsilon decays
- Higgs physics
- time-averaged B-mixing parameters, $\bar{\chi}$

Light sbottoms have been discussed by other theorists, too.

New preliminary CDF measurements show essentially no discrepancy with predictions.

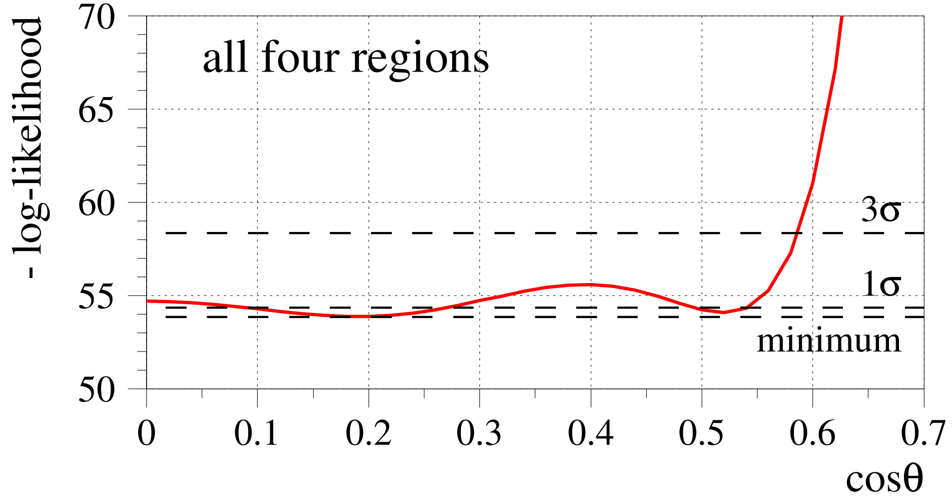
The negative log-likelihood function is now a function of two parameters related to the "new physics" part:

1. α turns $y^{\rm NP}$ on and off
2. $\cos\theta_{\tilde{b}_1}$ controls the contribution of the Z

First fix $\alpha = 1$ and vary $\cos\theta_{\tilde{b}_1}$.

Regions 1 & 2 have little impact.

Regions 3 & 4 give similar results which disallow $\cos\theta_{\tilde{b}_1} > 0.6$.



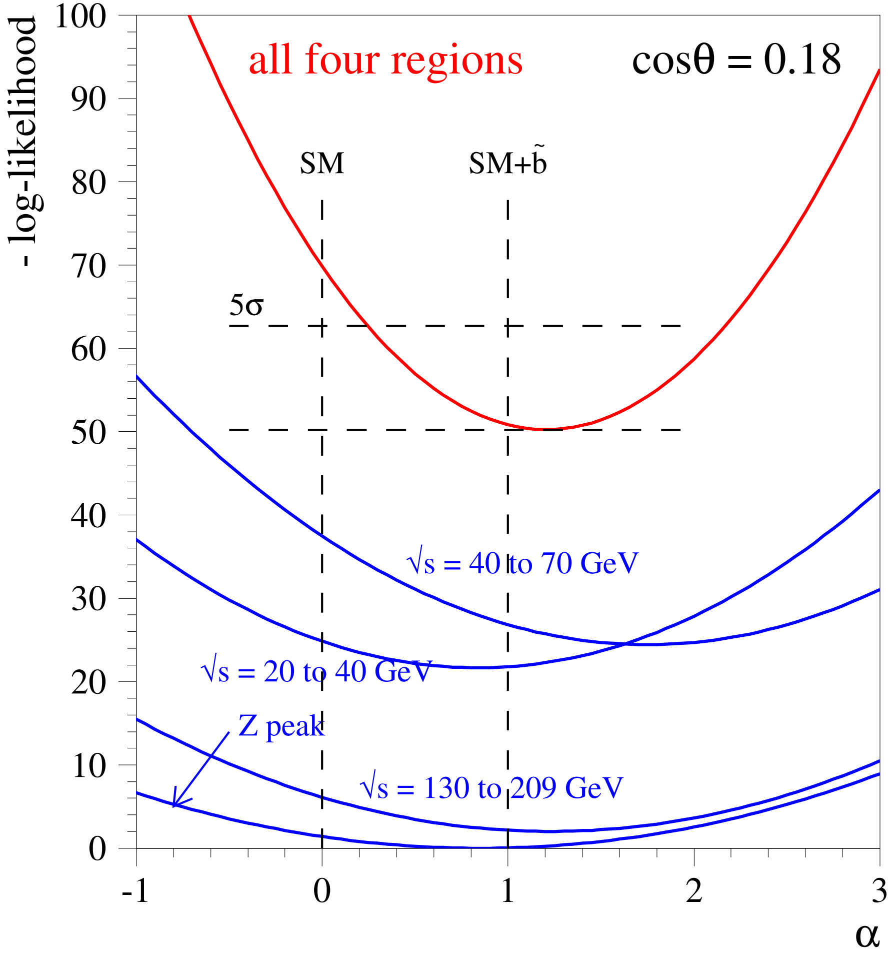
The data favor $\cos\theta_{\tilde{b}_1} = 0.18$, so we take that as fixed.

Vary α one region at a time – blue lines.

Vary α for all regions together – red.

Observations:

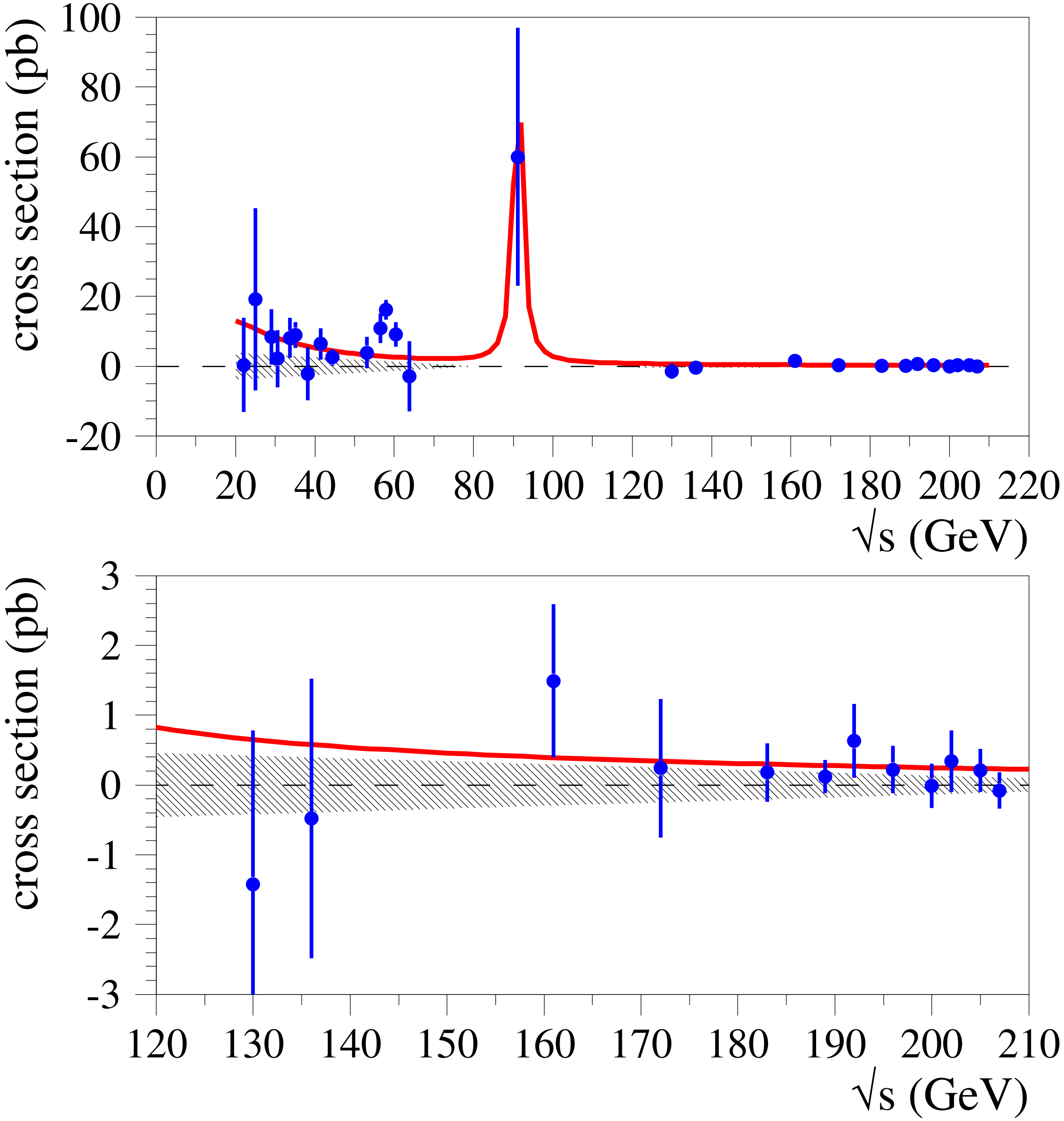
1. All four regions contribute to α_{best}.
2. α_{best} is consistent with the best α in each region.
3. α_{best} is consistent with 1.



After taking theoretical uncertainties and correlations into account,

the statistical significance is 4.3 s.d.

Here is a depiction of the sum SM + light $\tilde{b}_1$ compared to data:



Another Ansatz: high mass Z'

There would be no new final states, only new exchange matrix elements.

If we ignore interference terms, then $y^{\rm NP} \propto s$.

(It is probably not a good idea to ignore interference terms –
this needs to be revisited...)

Upshot: a linear rise in s is not supported by the data.
Regions 1 & 2 are contradicted by region 4.

6. Conclusions & Directions for Future Research

1. There is an apparent excess in $e^+e^- \to$ hadrons with a signficance of more than 4.3σ, conservatively estimated.

2. The "excess" fits well the expectation for a light $\tilde{b}_1$, coincidentally.

3. This is not proof that light sbottoms exist!

4. A heavy Z' does not fit the data when interference is ignored.

5. To do:
   (a) improve the Z' analysis
   (b) consider the "bump" at $\sqrt{s} \approx 57$ GeV
   (c) perform a full fit to the LEP 1 data
   (d) check impact of correlated systematics from below the Z in more detail

Part II: **The story takes an ominous turn…**

A colleague at CERN, Patrick Janot, investigates these results…

Findings

- He reproduces all of my results

BUT

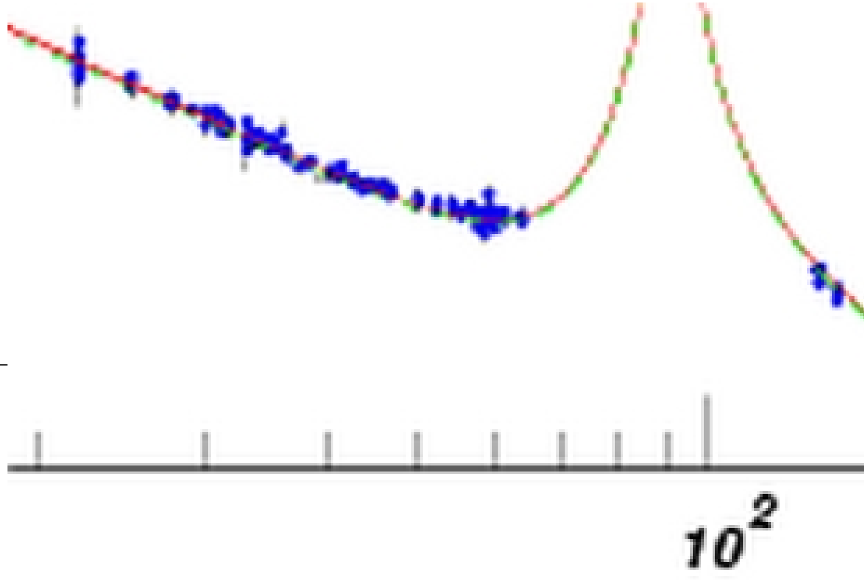
- There are major errors in the compilation from Zenin and the PDG!
  1. Several of TOPAZ measurements are shifted high by $\sim 11\%$!
  2. Many of the TASSO data are entered multiple times.
  3. An important set of VENUS data are missing.

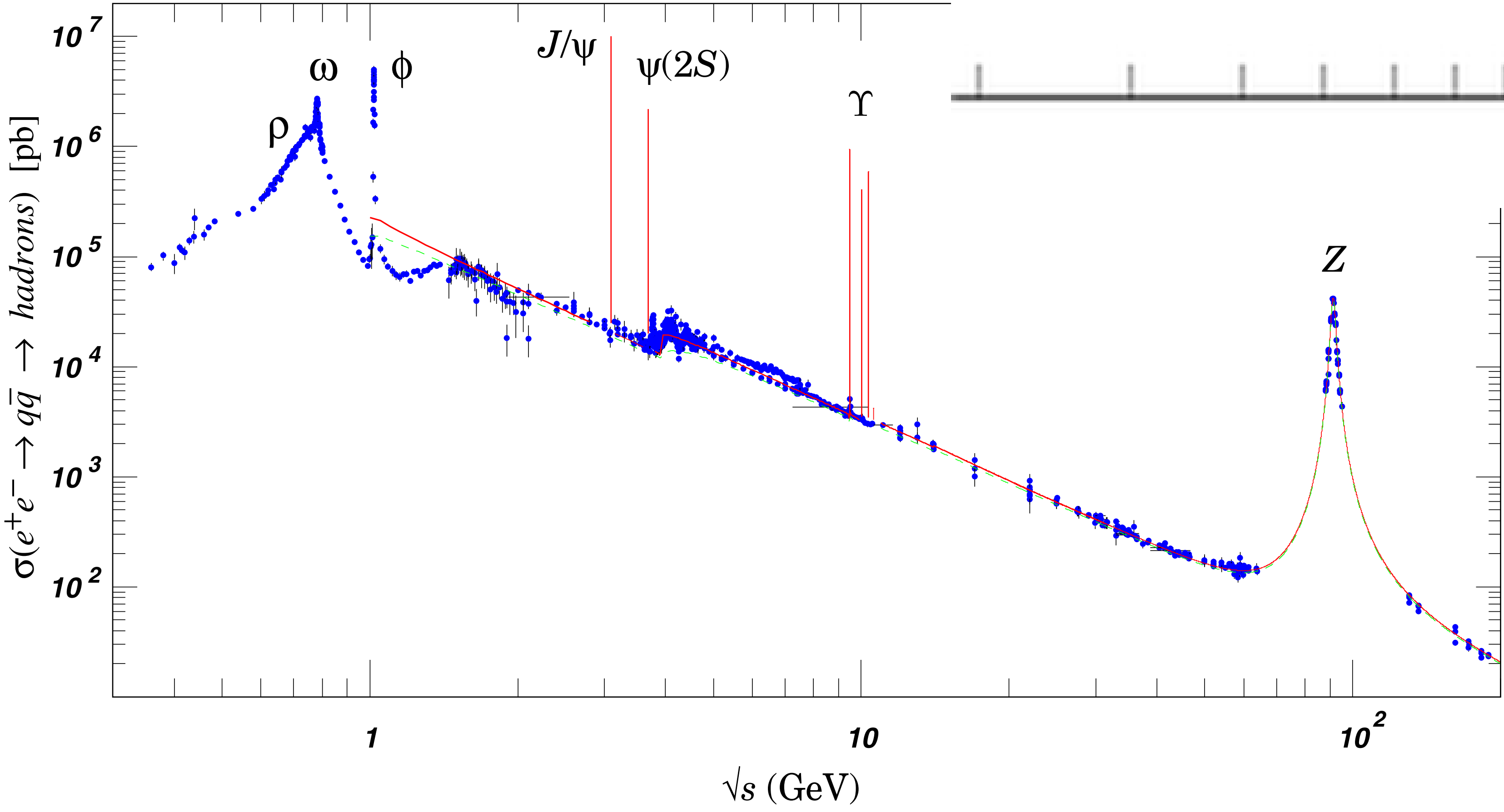
What will be the impact of correcting these errors?

- We are working together to obtain a correct and reliable set of measurements.
- He has concluded the light sbottoms are excluded!
- Here I will show my preliminary re-analysis of the corrected data, and summarize his published results.

The PDG did reference Zenin, *et al.*

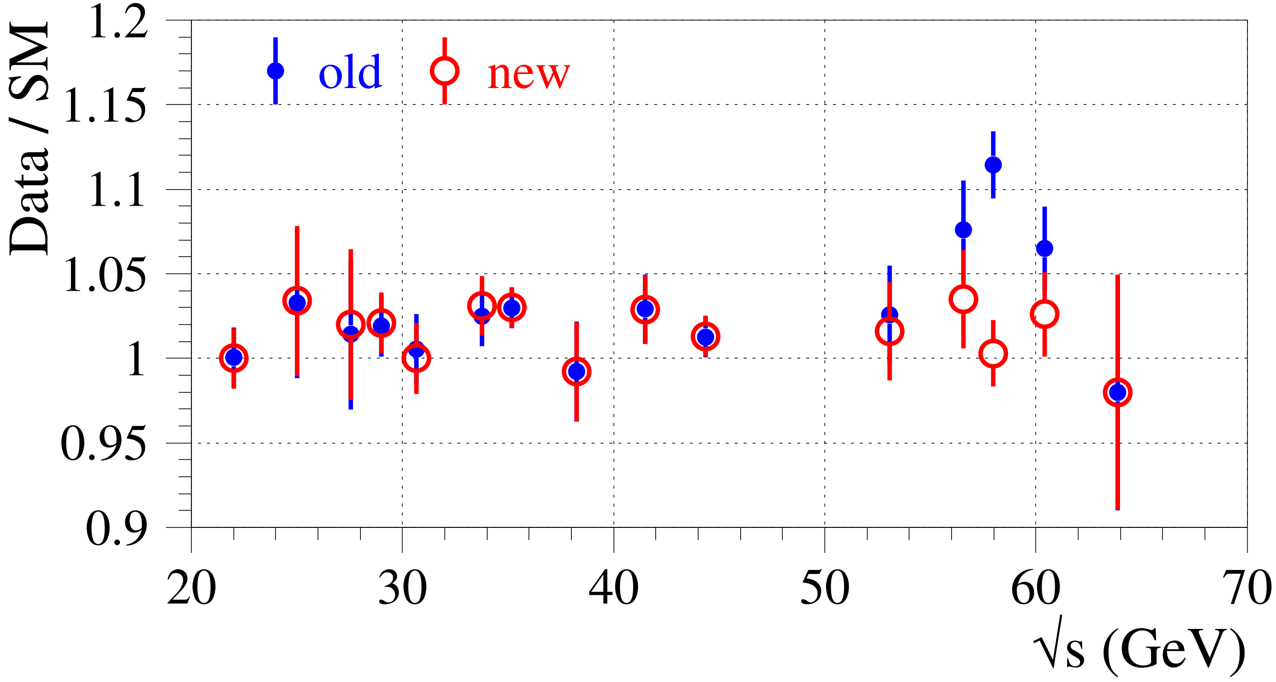
and their plot indicates an excess.





Re-evaluate the excess using corrected input data files.

region	$\sqrt{s}$ range	s.d. - old	s.d. - new
1	20–40	2.6	2.7
2	30–70	5.9	2.2



Re-evaluate the likelihood and check the α's

region	$\sqrt{s}$ range	α_{best} - old	α_{best} - new	S.D. - new
1	20–40	0.50	0.53	2.7
2	40–70	1.89	0.80	2.3
3	80–100	57	unch.	1.7
4	130–209	1.37	unch.	2.9
all	20–209	1.18	0.96	4.5

What is the change in the total significance?

Keep all aspects of the analysis unchanged aside from the input data:

- the total significance drops **4.3 s.d $\longrightarrow$ 3.1 s.d**

(This includes theory uncertainties and correlations.)

We are developing a more correct treatment of the LEP 1 data.

- impact of $\tilde{b}_1$'s on Γ_Z
- fit the measurements at their $\sqrt{s}$ rather than just one conglomerate value at $\sqrt{s} = M_Z$
- take interferences into account

$\longrightarrow$ conservatively *drop the LEP 1* data for now

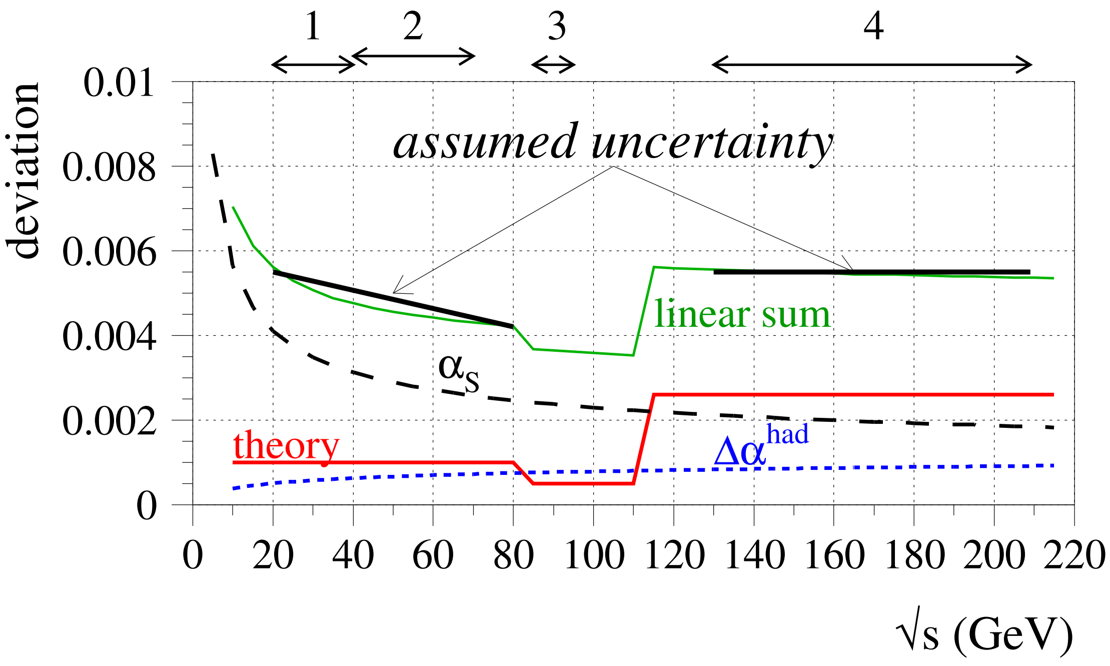
- the total significance drops **3.1 s.d** $\longrightarrow$ **2.7 s.d**

Basic Conclusion:

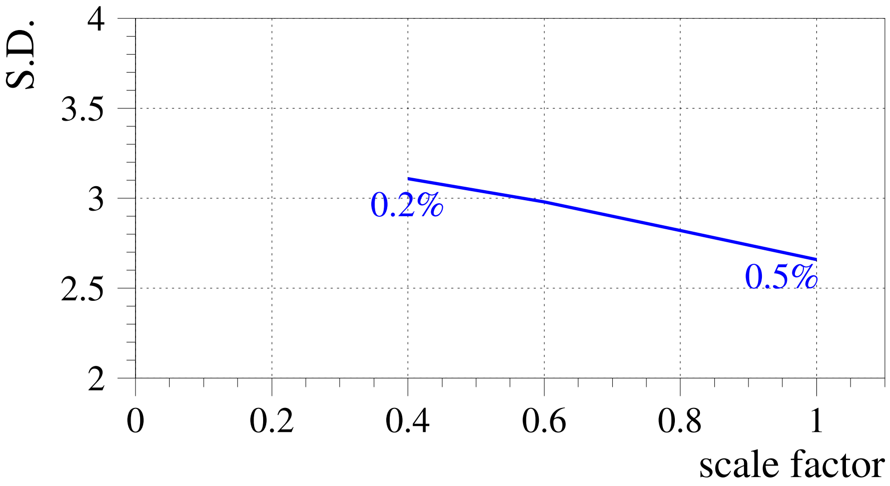
The "apparent" excess is weaker than originally thought.

But *is there really no excess?*

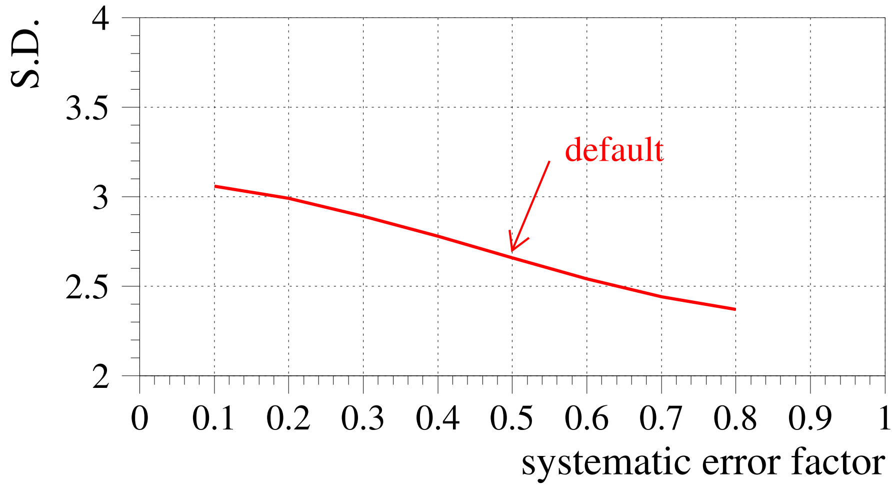
- Regions 1, 2, (3) and 4 do have more events than predicted.
- In the extreme case that one neglects theoretical errors and correlations, the significance is **4.5 S.D.** w/o LEP1 data
- Examine the dependence of the *significance* on the assumed *theoretical uncertainties* and *correlations* in regions 1&2.



scale the theory uncertainty:



vary the degree of correlation:



Try a 'mixed' variation:

reduce theoretical uncertainty by half, and take 1/3 of systematic uncertainty to be correlated

$\longrightarrow$ 3.3 standard deviations

The actual significance depends sensitively on the actual choices made (how aggressive or conservative), but nonetheless, it will not be 4 S.D.

What is needed here?

- We need to continue to scrutinize the published data and make sure they are correct. *In progress w/ P. Janot...*

  Note this might well further reduce the signficance of the lower-energy data.

- Existing data should be analyzed more carefully, if possible.
  - ⋆ Can CLEO make more precise measurements?
  - ⋆ Can LEP2 hadronic events be investigated in more detail?

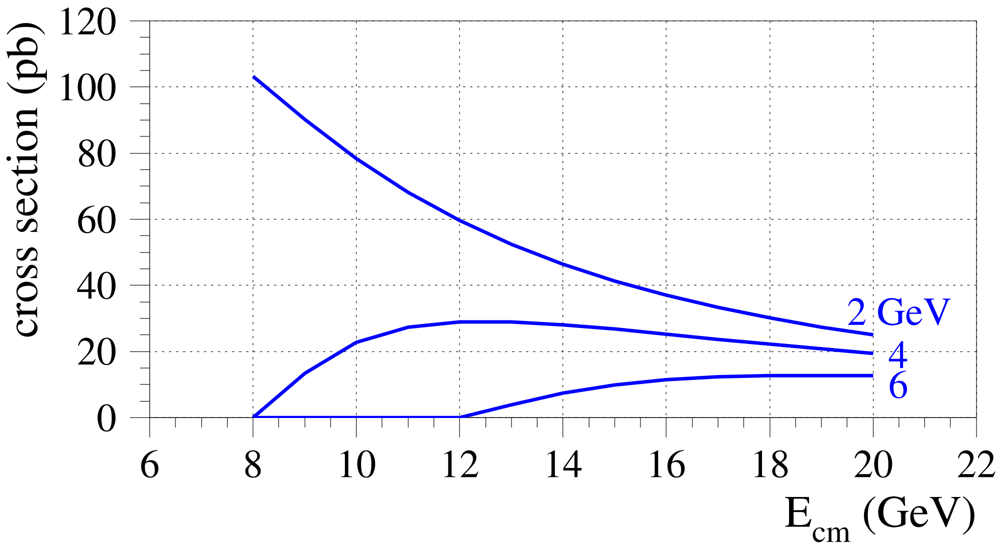
- It would be great to have new, high quality data.
  - ⋆ Run CESR or BELLE at higher $\sqrt{s}$? **
  - ⋆ Run an early stage of CLIC at these energies? †

** under discussion with Alexei Ershov and André de Gouvêa.

† suggested by Mayda Velasco.

A quick look at what CESR or Belle might do:

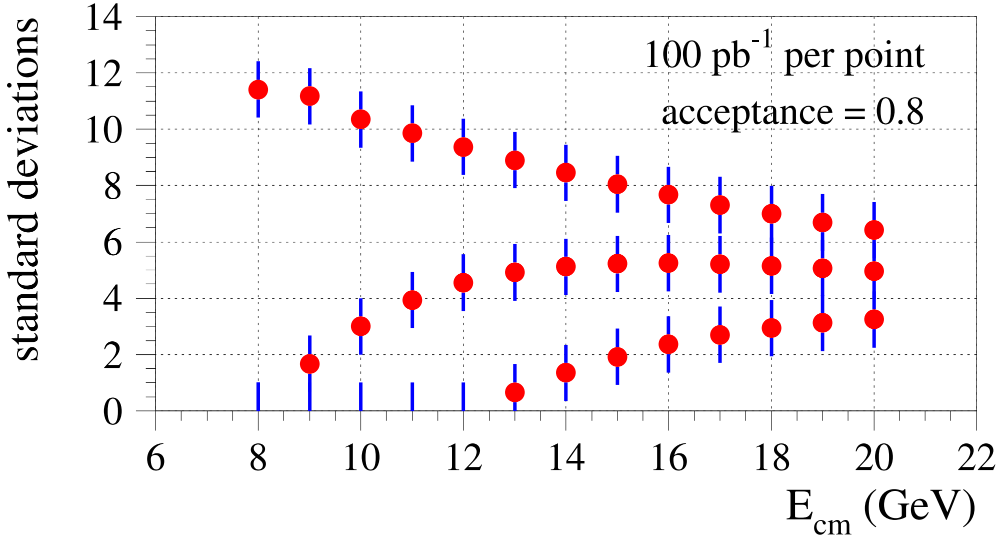
$\sigma(e^+e^- \to \tilde{b}_1\tilde{b}_1^*)$ depends strongly on $M_{\tilde{b}_1}$



This would be a virtue / opportunity:

- look for non-$(1/s)$ excess
- potentially estimate $M_{\tilde{b}_1}$

How much data to get a significant result?



⋆ These machines deliver amazing $\mathcal{L}$.

⋆ Expend $\mathcal{O}(1\ \mathrm{fb}^{-1})$ on a scan.

Observe either a very significant excess or an excess depending on $\sqrt{s}$.

What about the TEVATRON?

- Forgetting about the gluino, these are very hard to find because they decay to light quarks.

  $\longrightarrow$ no $\not{E}_T$, no leptons or isolated γ's

- If the lifetime is long enough, look for the bound state $(\tilde{b}_1\tilde{b}_1^*) \to \mu^+\mu^-$.

  $\times$ This state is not required – the $\tilde{b}_1$'s may decay too quickly.

- Within the model of Berger *et al.*, there are signatures coming from the light $\tilde{g}$ state.

  same-sign charged B-mesons

  inflated value for $\bar{\chi}$

Patrick Janot's Analysis

After finding the errors in the PDG data files*, Patrick carried out his own analysis with the conclusion

light sbottoms are totally excluded!

To understand how he reaches this conclusion, we must look at the details of his work.

- different analysis of Z-peak data
- less conservative theory uncertainty
- additional global uncertainty for QED corrections

(See `hep-ex/0403157` for his description.)

* since acknowledged by the PDG with plans to amend them.

(Re-)Analysis of the Z-peak Data

Consider the production of a new particle, such as $\tilde{b}_1$.
There are two modifications to $\sigma^0_{\rm had}$:

1. γ^* exchange leads to an increase in events
2. any increase in $\Gamma(Z \to$ hadrons) leads to a decrease in events

The second point follows from the tree-level relation

$$\sigma^0_{\rm had} = \frac{12\pi}{M_Z^2}\,\frac{\Gamma_{ee}\Gamma_{\rm had}}{\Gamma_Z^2}$$

Since $\Gamma_Z \approx 9\Gamma_{ee} + \Gamma_{\rm had}$, any increase in $\Gamma_{\rm had}$ from a non-zero $Z - \tilde{b}_1$ coupling will also increase Γ_Z, leading to a decrease in $\sigma^0_{\rm had}$.

$\longrightarrow$ it is difficult to achieve an excess with a nonzero Z coupling.

Patrick concludes

- $\sigma^{\rm NP}_{\rm had} = -24 \pm 36$ pb while the data give $\Delta\sigma = 62 \pm 37$ pb.
- $\sigma^{\rm NP}_{\rm had} < 56$ pb at 95% CL

The cross section for $e^+e^- \to \tilde{b}_1\tilde{b}_1^*$ at $\sqrt{s} = M_Z$
is about 74 pb for $M_{\tilde{b}_1} = 6$ GeV and $\cos\theta_{\tilde{b}_1} = 0.18$,
so Patrick concludes that the LEP 1 data alone rule out this case.

He sets $\cos\theta_{\tilde{b}_1} = 0.39$ which corresponds to zero coupling to the Z,
hence, $\Gamma_Z = \Gamma_Z^{\rm SM}$.

He takes the LEP 1 data to be constraining on the model according to the information that $\sigma_{\rm had}^{\rm NP} = -24 \pm 36$ pb.

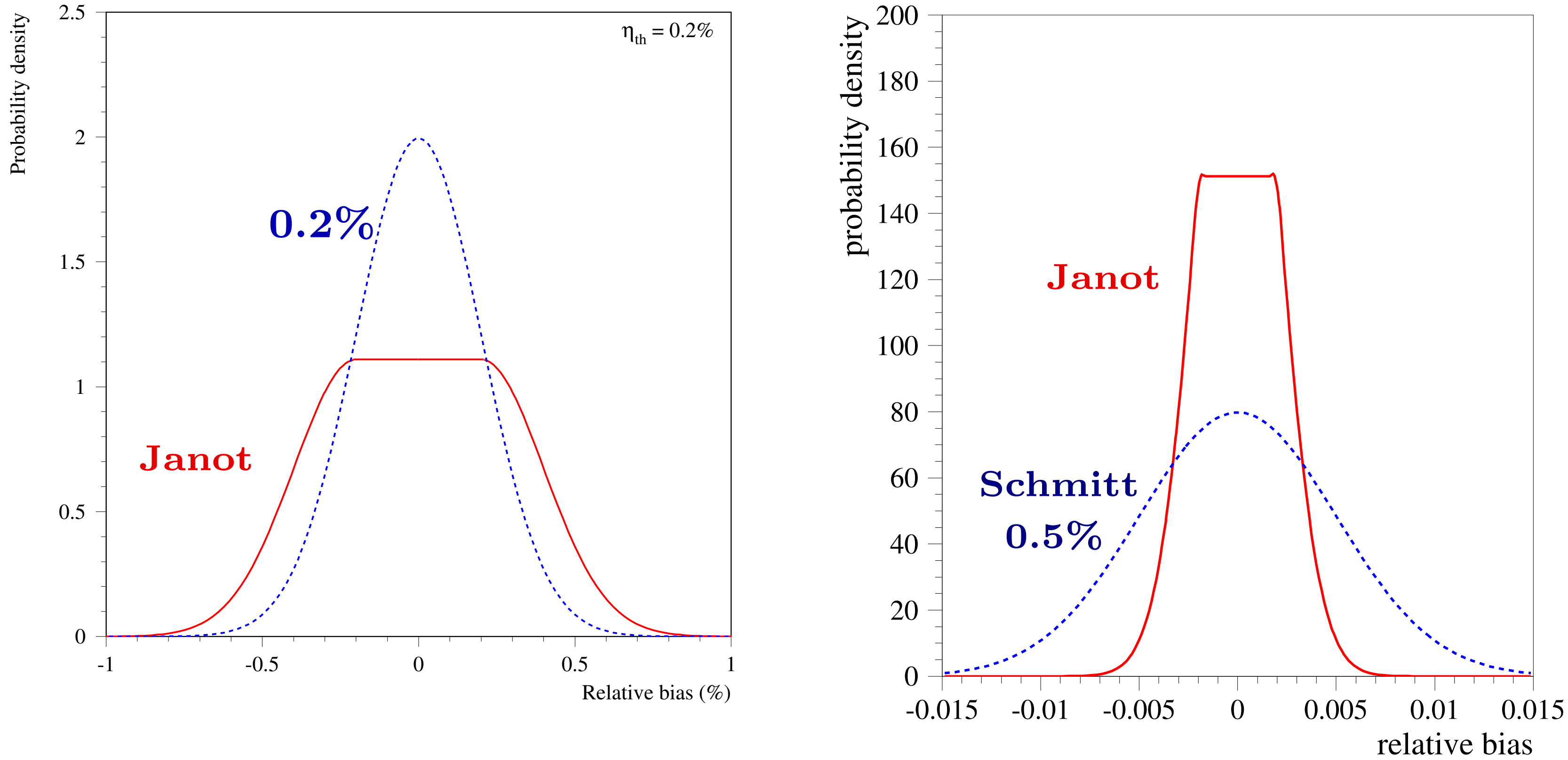
At the same time, he states that there is no constraint on α since the $\tilde{b}_1$ decouples from the Z.

Tighter Definition of Theoretic Error

Patrick argues for a smaller theory uncertainty of 0.2% (I used 0.5%).

He points out that the character of a theory uncertainty
is more flat than Gaussian, and proposes the form below.

This makes the theory more constraining.



Extra Correlated Uncertainty for QED Corrections

Patrick decided to re-correct the low energy data for QED because the theoretical advances embodied in `ZFITTER` were unavailable in the 1980's.

$$\sigma^0_{\mathrm{had}} \longrightarrow \sigma^0_{\mathrm{had}} \times \frac{\sigma^{(\mathrm{all})}_{\mathrm{ee}}}{\sigma^{(1)}_{\mathrm{ee}}} \frac{\sigma^{(1)}_{\mathrm{had}}}{\sigma^{(\mathrm{all})}_{\mathrm{had}}}$$

This was carefully done.

The corrections to the hadronic final state and to Bhabhas cancel to the level of 0.1%. He assigns an overall correlated uncertainty of 0.1% for regions 1&2.

Each experiment assigns typically 1–2% uncertainties for this.
As Patrick says, one could reduce these uncertainties after this work, but he left the systematics alone, to be conservative.

This has very little impact.

Patrick's result:

Uncertainties	α_{min}	α_{95}	α_1	α_2	α_4
Gaussian	0.43 ± 0.29	0.92	0.30 ± 0.38	0.15 ± 0.68	1.54 ± 0.90
Non-Gaussian	$0.36^{+0.46}_{-0.34}$	1.00	$0.16^{+0.67}_{-0.38}$	$0.02^{+0.93}_{-0.68}$	1.72 ± 0.90

He integrates α to define an upper limit.
α_{95} corresponds to the upper 5% of the α dist'n.

Since $\alpha_{95} < 1$, he concludes that
this model is *excluded* at 95% CL.

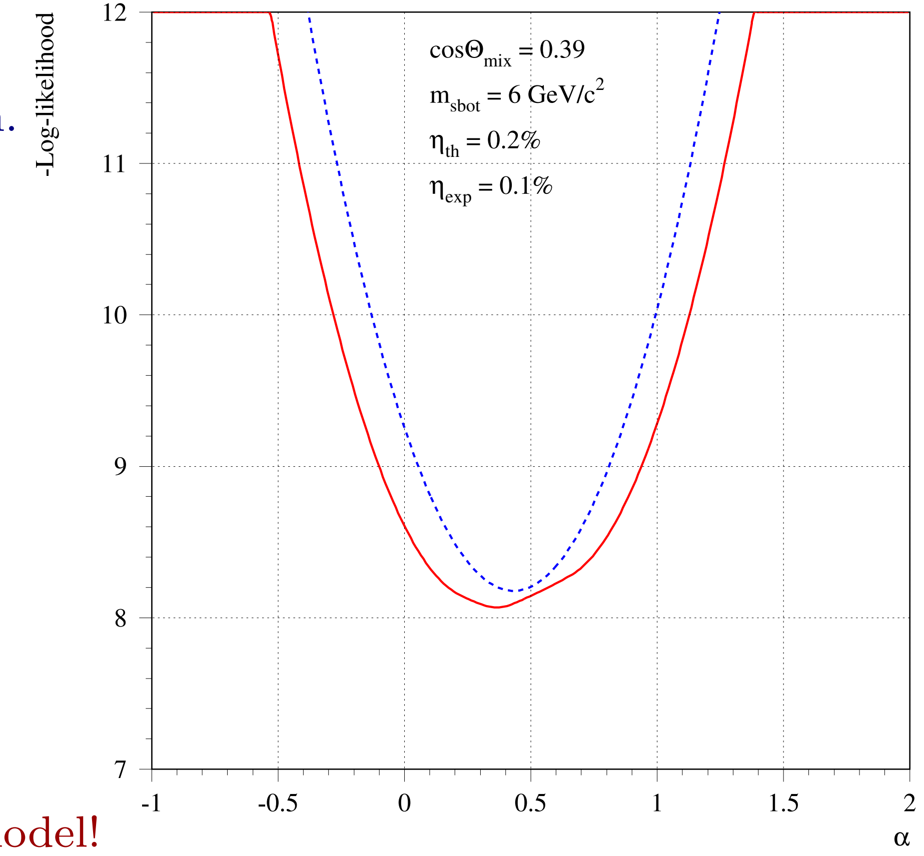
My viewpoint is different.

Remember that $\alpha = 0 \Longrightarrow$ SM
$\alpha = 1 \Longrightarrow$ this light $\tilde{b}_1$ model.

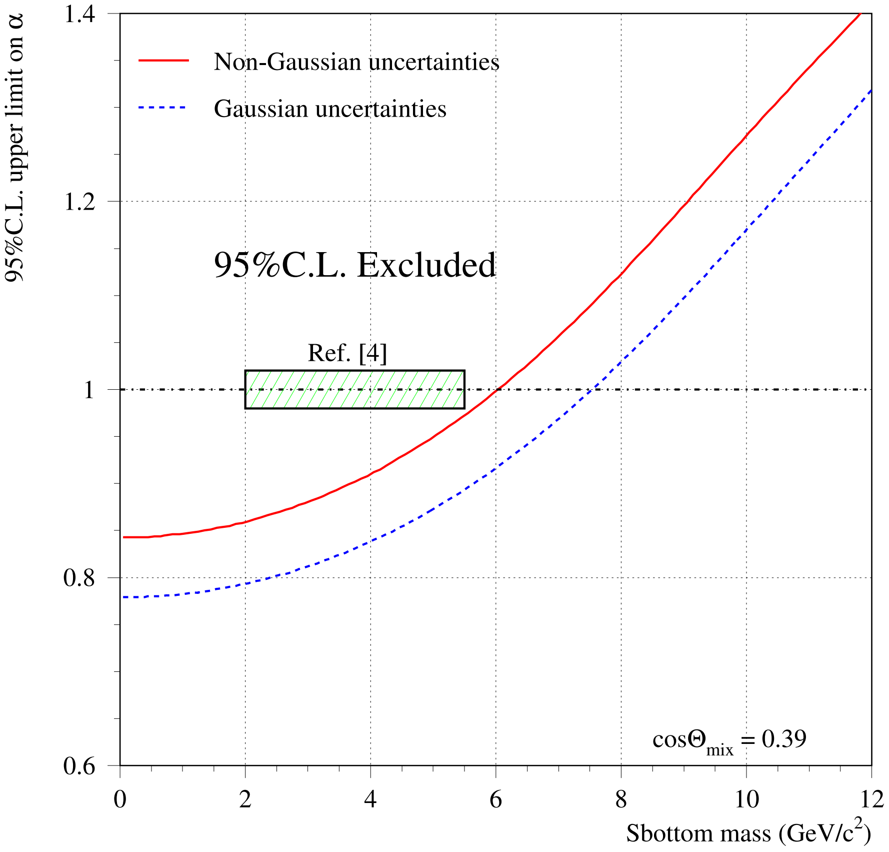
We are trying to *test 2 hypotheses*, so
what matters is comparing -LL at $\alpha = 0$ & 1.

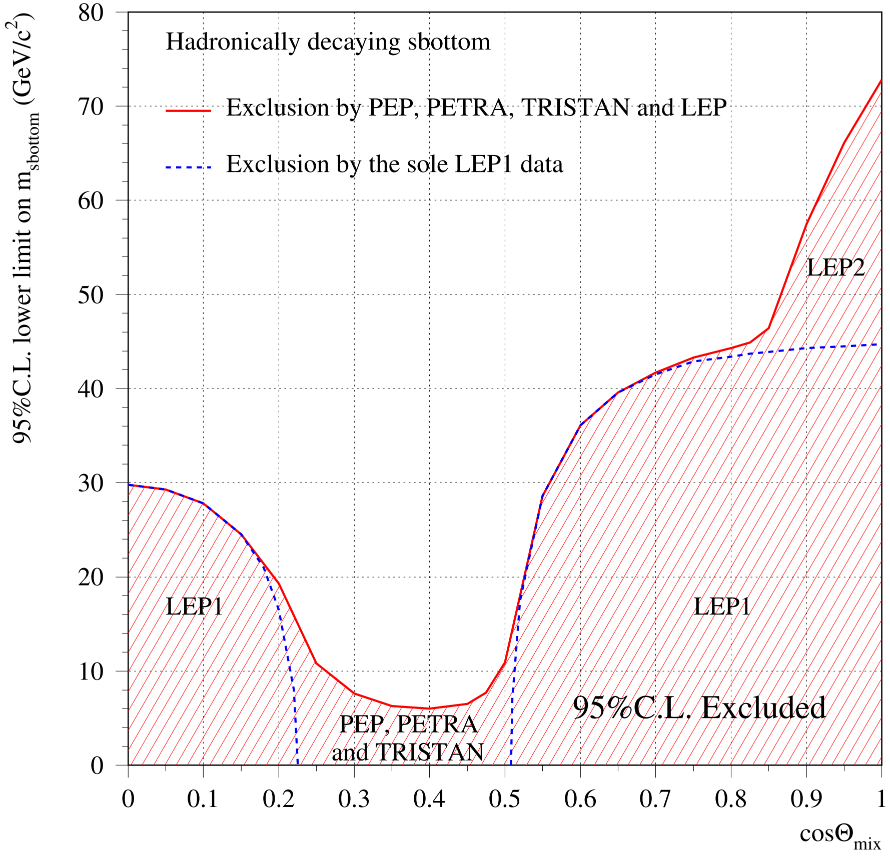
The minimum of -LL is not at $\alpha = 0$.

The SM is apparently nearly as bad as the $\tilde{b}_1$ model!



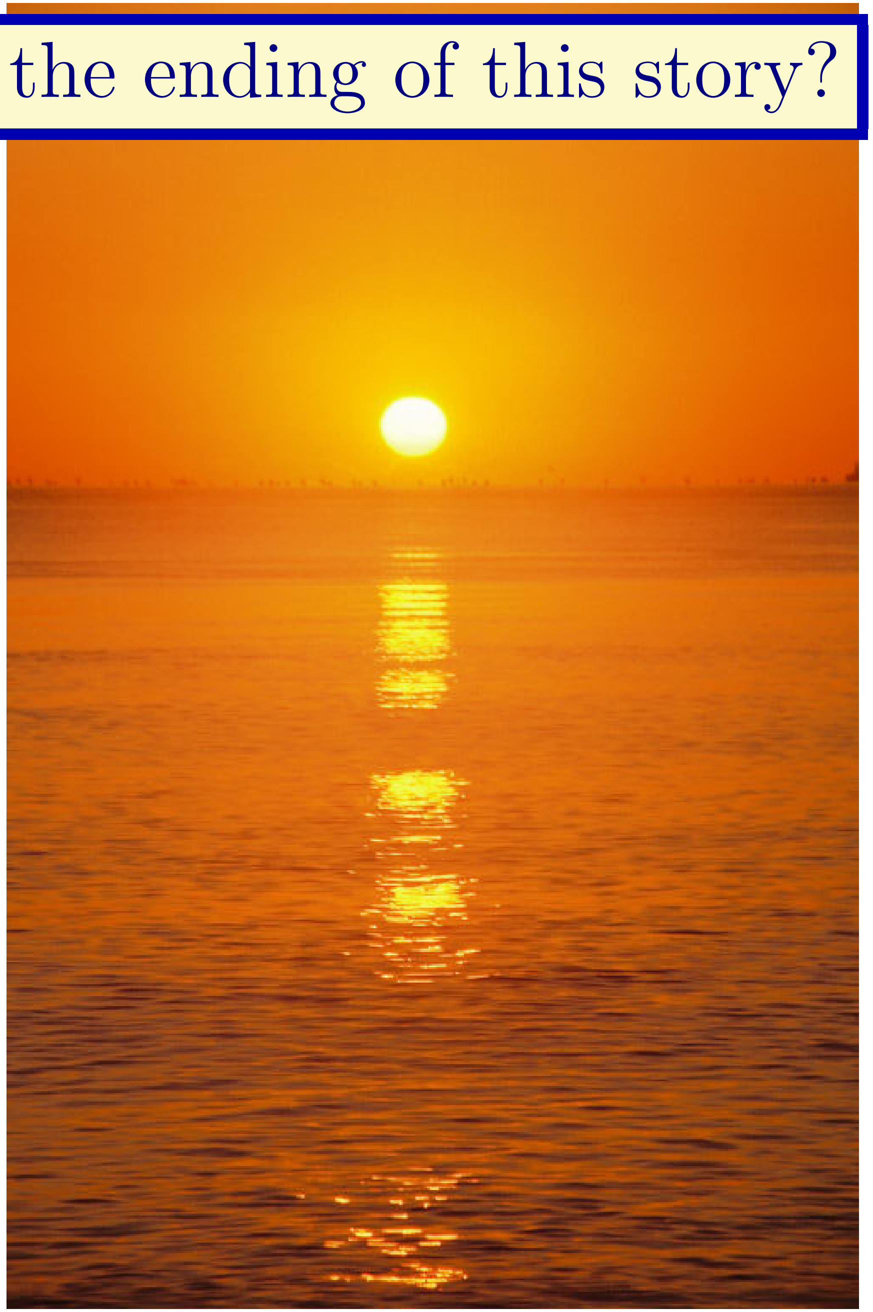
Patrick explores the dependence on the mass and mixing angle:





He is focussed on excluding the model of Berger *et al.*[4].

Part III: **What will be the ending of this story?**



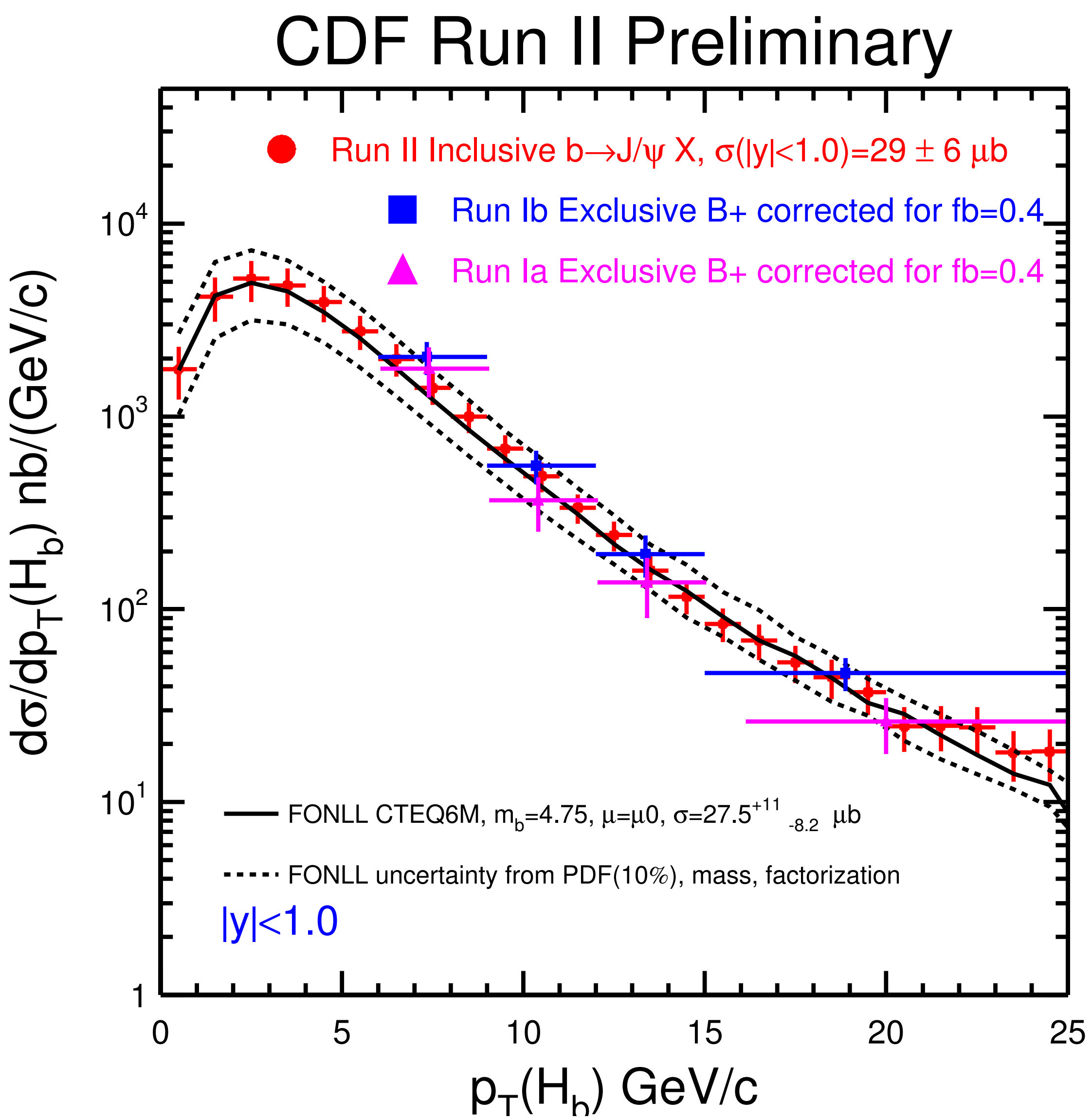
1 The excess rises again
and we are motivated to search
for whatever is responsible.

OR

2 The excess sinks away
and we look for something else.

Time will tell.

Backup Slides



Preliminary Run II result from CDF show no large discrepancy with theory.

(Presented by D. Litvintsev at Aspen last February.)