Thoughts on what we are doing here

Benjamin Grinstein
This is a workshop

Working groups?
Working groups!
more like

Brain droppings
(after George Carlin)
more like

Brain droppings

(after George Carlin)
Provocative, not inspiring
Fine tuning
Teide 

Caldera 

Teide at center of caldera:
Teide at center of caldera: coincidence
Teide

Caldera

Teide at center of caldera:

coincidence (aka fine tuning)?
Teide at center of caldera:

coincidence (aka fine tuning)?
or UV completion?
The caldera was formed as a result of a geological landslide taking 40Ky about 180Ky ago

There was no Teide then

“Con el paso del tiempo, la gran depresión se fue rellenando hasta formar el Teide, que se convirtió en el pico más alto de España por "un caprichoso azar de la naturaleza", una erupción que "se pudo producir en el siglo XIV", pronostica el geofísico.”

https://www.elmundo.es/elmundo/2012/04/12/ciencia/1334226413.html

(Actually, several eruptions, starting 120Ky ago, building up to present size)

let’s stop making up problems where there are none
The caldera was formed as a result of a geological landslide taking 40Ky about 180Ky ago

There was no Teide then

“Con el paso del tiempo, la gran depresión se fue rellenando hasta formar el Teide, que se convirtió en el pico más alto de España por "un caprichoso azar de la naturaleza", una erupción que "se pudo producir en el siglo XIV", pronostica el geofísico.”

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(Actually, several eruptions, starting 120Ky ago, building up to present size)

let’s stop making up problems where there are none
1. Teno Shield Volcano

2. Joins previous shield volcanoes together
   - Forms one large centre at heart of the island.
   - Las Cañadas Volcano

3. Slowly builds up current island configuration
   - Las Cañadas Volcano

4. Collapse of the Las Cañadas Centre
   - Las Cañadas Caldera

5. Growth of new Teide Volcano in Las Cañadas Caldera
   - Teide/Viejo Strato-Volcano
   - Las Cañadas Caldera
Flavor dishonesty

- Generic flavor speaker motivation slide
  - Explain origin of matter (ugh)
  - Why are there 3 generations
  - Why hierarchies of masses
  - Why texture of mixing matrices

Modeling flavor anomalies: shouldn’t they answer the flavor questions?? How do we propose to ever address the “motivation” Qs?
Flavor dishonesty

- Generic flavor speaker motivation slide

- Explain origin of matter (ugh)

- Why are there 3 generations

- Why hierarchies of masses

- Why texture of mixing matrices

Modeling flavor anomalies: shouldn't they answer the flavor questions?? How do we propose to ever address the “motivation” Qs?
Glad to see some are starting to think about this, cf, talks by

Marzia Bordone (using Froggatt-Nielsen power counting in SMEFT coefficients)

Gino Isidori (proposing different gauge group representations to distinguish generations and hierarchical symmetry breaking to produce accidental U(2))

(I, with G Ross and S Pokorsky also tried)
Warning: this is not a summary talk

Do not expect to see your name quoted

Chances are if I did, it would not be kind
What do you say to

“We badly need LHC to see something”

? 

This is same as this talk’s title:
What are we doing here?

HC2NP: Can we ever be sure?

Can you trust there is NP in a $5\sigma$ 
deviation in $(g-2)_\mu$. In $7\sigma$? $10\sigma$? 
How about in $\varepsilon'/\varepsilon$
What do you say to

“We badly need LHC to see something”

? 

This is same as this talk’s title: What are we doing here?

HC2NP: Can we ever be sure?

Can you trust there is NP in a $5\sigma$ deviation in $(g-2)_{\mu}$. In $7\sigma$? $10\sigma$?

How about in $\varepsilon'/\varepsilon$
I feel more and more irrelevant

Lattice will do it all

Homework: list all the problems in flavor physics that lattice will not do.

Grade: Look at it and ask: should I change fields?
I feel more and more irrelevant

Lattice will do it all

Homework: list all the problems in flavor physics that lattice will not do.

Grade: Look at it and ask: should I change fields?
Bare with me: longish intro to my next dropping
Peter Galison in *How Experiments End* asks: How does a scientist decide at what point an experiment is completed and the results can be communicated to the scientific community? Galison's thesis: sociological considerations matter in deciding when to stop looking for systematics.
Two interesting proposals he gathers from particle physics

- Personal bias (historical) on methodology (often tied to type of instrumentation)
  - Example: golden event vs statistics
    - Gargamelle vs HPWF
      - (“alternating neutral currents”)
  - Example: Even within Gargamelle,
    - “...several group members, particularly Perkins, found the single photograph of neutrino–electron scattering particularly important. For others, the difference in the spatial distribution between the observed neutral current candidates and the neutron background was decisive.”

Two interesting proposals he gathers from particle physics

- Prejudice on expected result, provided by previous experiments
- theoretical expectations

“A reasonable point at which to stop looking for [...] systematic effects is when the result agrees with previous measurements or when it agrees with existing theory.”

-Allan Frankin, idem
It is easy to find suggestions (evidence?) of prejudice due to previous measurements.

**PDG Historical Plots**

**VS**
Let’s be clear
Let's be clear

Nobody is cheating
Let’s be clear
Nobody is cheating
The point is...
Let's be clear

Nobody is cheating

The point is...

systematic errors are hard to determine
Let’s be clear

Nobody is cheating

The point is...

systematic errors are hard to determine

And it is difficult to insure all sources are included
Let’s be clear

Nobody is cheating

The point is...

systematic errors are hard to determine

And it is difficult to insure all sources are included

The prejudice comes in deciding when to stop trying
Prejudice from theoretical expectations

\[ g - 2 = -1 \]

"Their result for the ratio of the angular momentum of the sample to its magnetic moment (the authors called it) was very close (within 3%) to the expected value of \( 2m/e \). It was realized later that their result with the quoted uncertainty of 10% was not consistent with the correct value which is close to \( m/e \). Apparently, the authors underestimated the experimental uncertainties."

https://en.wikipedia.org/wiki/Einstein%E2%80%93de_Haas_effect

"Einstein and de Haas ended their search for systematic errors when their result for the gyromagnetic ratio, \( g = 1 \), agreed with their belief that magnetism was due to orbiting electrons."

-Allan Frankin, idem
Correlated?

\[ V_{ud} = \sqrt{\frac{4909s}{\tau(1 + 3g_A^2)}} \]

if you do this year by year with error bars, please send me a copy

by eye
Clearly experimentalists have learned to avoid this.

They have invented “blind analysis”:

Do not look at signal in data until confident all systematics are down to the best you can do (or below statistical uncertainties).

Only then “open the box”

Caveat: only, they do not do this if the result is unexpected (private communication, many experimenters that prefer to remain anonymous).
Theorists also allow prejudice to enter their “high precision” calculations.

(Do) We also need theory “blind analysis”(!)!
Theorists also allow prejudice to enter their “high precision” calculations.

(Do) We also need theory “blind analysis”(??)!
Example

NB, examples abound, just pick one for fun
Superallowed what?

- $V_{ud}$ is cleanly determined from super allowed beta decay.

- CVC guarantees the exact normalization of the matrix element:
  $$\langle 0^+ | V^0 | 0^+ \rangle = 1$$
  (ok, not=1, but the isospin charge of state)

- Hence PDG: $|V_{ud}| = 0.97420 \pm 0.00021$
  (in case you missed this, that's 0.02% accuracy)

- Hadronic complications averted!! No place in HC2NP program.
uh?

Exact isospin?

$m_u = m_d$ ?  $e = 0$ ?

mmmhhhhh...

Next few slides stolen from talk by Dan Melconian
The Corrected $\mathcal{F}t$ value

We must account for the fact that the decay occurs within the nuclear medium

\[
\mathcal{F}t \equiv ft \left(1 + \delta'_R\right) \left(1 + (\delta_{NS} - \delta_C)\right) = 2915.6 \pm 1.1 \, \text{s} \over |V_{ud}|^2
\]

(really should be constant)

- $\delta'_R = E_{e}^{\text{max}}$ and $Z$ dependent radiative correction
- $\delta_{NS} =$ nuclear structure dependent radiative correction
- $\delta_C =$ isospin symmetry-breaking correction
- $\Delta^V_R =$ transition independent radiative correction
Testing calculations of ISB effects

How well does a model’s calculated $\delta_C$ align the $ft$ values?

Define:

$$\delta_{C}^{\text{"exp"}} \equiv 1 + \delta_{NS} - \frac{A}{ft(1 + \delta'_R)}$$

and fit to the model-calculated $\delta_C$’s, with $A$ a free parameter of the fit

($A$ corresponds to $\langle Ft \rangle$... if CVC holds!)
As you know

Vud is free from hadronic systematics

no nuclear modeling needed

all thanks to CVC ...
Testing calculations of ISB effects

How well does a model's calculated $\delta_C$ align the $ft$ values?

Shell model with Woods-Saxon radial wavefunctions (TH):

best-fit $\langle F t \rangle = 3072.0 \pm 0.6$ s

$\chi^2/13 = 0.4$, $\text{CL} = 96\%$
Excellent
Excellent

Too excellent
Excellent

Too excellent

CL = 96%
Excellent

Too excellent

CL = 96%

What do you think of elections
Excellent

Too excellent

CL = 96%

What do you think of elections won by 96% majority?
Testing calculations of ISB effects

How well does a model’s calculated $\delta_C$ align the $ft$ values?

Shell model with Hartree-Fock radial wavefunctions (TH):

(would get $\langle Ft \rangle = 3071.3 \pm 0.8$ s)
Testing calculations of ISB effects

How well does a model’s calculated $\delta_C$ align the $ft$ values?

Isovector monopole resonance (Auerbach):

(would get $\langle \mathcal{F}t \rangle = 3087.7 \pm 1.9$ s)

\[ \chi^2/13 = 11.2, \; \text{CL} = 0\% \]
Testing calculations of ISB effects

How well does a model’s calculated $\delta_C$ align the $ft$ values?

Relativistic RPA with PK01 eff. interaction (Liang):

(would get $\langle Ft \rangle = 3078.9 \pm 1.0$ s)

\[
\delta^\text{"exp"}_C - \delta^\text{calc}_C \quad [\%]
\]

$\chi^2/8 = 3.1$  CL = 0%

$Z$ of daughter
Testing calculations of ISB effects

How well does a model’s calculated $\delta_C$ align the $ft$ values?

Relativistic RPA with DD-ME2 eff. interaction (Liang):

(would get $\langle F_t \rangle = 3081.4 \pm 1.1$ s)

\[ \chi^2/8 = 2.4, \quad \text{CL} = 2\% \]
Testing calculations of ISB effects

How well does a model’s calculated $\delta_C$ align the $ft$ values?

Density functional theory (Satuła):

(would get $\langle Ft \rangle = 3070.0 \pm 1.4$ s)

\[
\delta_c^{\text{exp}} - \delta_c^{\text{calc}} \left[ \% \right]
\]

\[\chi^2/12 = 5.2, \quad \text{CL} = 0\%\]
and so on, ... 

you get the idea

which brings me to ...
When being correct does not mean being correct

Distinguish models from models
1st kind: A mathematical model of reality
   SM
2nd kind: A mathematical model of what a mathematical model of reality predicts
   hadronic models; examples abound:
   ISGW
   QCD sum rules
   shell model with Woods–Saxon potential
2nd kind: it is what nonHE physics is all about
We do not do

- Nuclear physics from QCD
- Condensed matter from QED
- Biophysics from atomic physics

and so on

But the “NP” in “HC2NP” demands attention to the 1st kind
My favorite model (of the 2nd kind) correctly describes areas of reality

(Translation: it agrees with experiment when compared against a specific set of observations for which it was designed, and tuned over time)
I would hope you know why getting the correct pre/post-diction is not the same as getting the correct explanation.

If I have to explain, I do it by analogy:
I would hope you know why getting the correct pre/post-diction is not the same as getting the correct explanation

If I have to explain, I do it by analogy:

\[
\frac{16}{64}
\]
I would hope you know why getting the correct pre/post-diction is not the same as getting the correct explanation

If I have to explain, I do it by analogy:

\[
\frac{1\times}{6} \div \frac{64}{64}
\]
I would hope you know why getting the correct pre/post-diction is not the same as getting the correct explanation

If I have to explain, I do it by analogy:

\[
\frac{1\emptyset}{\emptyset 4} = \frac{1}{4}
\]
I would hope you know why getting the correct pre/post-diction is not the same as getting the correct explanation.

If I have to explain, I do it by analogy:

\[
\frac{1\emptyset}{\emptyset 4} = \frac{1}{4}
\]

Wrong method, right answer
- close encounters of the 2nd kind
- can it be used elsewhere?
- do you learn anything from it?
In HC2NP precision is key

Hadronic models physically illuminating (sometimes)

Not systematic expansions

No reliable way to estimate systematic errors
Being popular does not mean being correct

- Not quite the same as previous
- But related. Citations:
  - ISGW: 1361
  - QCD sum rules: 5175
The incredible shrinking error

My model of time dependence of theoretical error:

\[
\frac{d\Delta}{dt} = - \sum_i \epsilon_i \delta(t - t_i)
\]

constrained by \(\sum \epsilon_i < \Delta_0 \equiv \Delta(t \to -\infty)\)

Empirically, often

\[
\frac{d\Delta}{dt} = -\gamma \Delta - \sum_i \epsilon_i \delta(t - t_i)
\]
The incredible shrinking error

My model of time dependence of theoretical error:

\[ \frac{d\Delta}{dt} = - \sum_{i} \epsilon_i \delta(t - t_i) \]

constrained by \( \sum \epsilon_i < \Delta_0 \equiv \Delta(t \rightarrow -\infty) \)

Empirically, often

\[ \frac{d\Delta}{dt} = -\gamma \Delta - \sum_{i} \epsilon_i \delta(t - t_i) \]
WE (meaning the HEP community, both HEPEXers and HEPTHers) like small errors.

We will use, cite, fund, invite to talk, invite to review, revere (ok, that’s going too far) those who publish the smallest error.

Could this create an incentive to underestimate errors? Or an incentive to oversell methods and results?

COULD ERRORS SHRINK SANS JUSTIFICATION?
An example,

but again, they abound
% error Vub : inclusive (B-> Xu l nu)
"Enhanced awareness of the theoretical uncertainties and the difference between the results obtained from inclusive and exclusive analyses leads us to be even more conservative in setting the error bar than in previous reviews"
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"A theoretical uncertainty within the 10%–15% window seems likely."
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"A theoretical uncertainty within the 10%-15% window seems likely."

"For a total theory error of 15% and total precision of 18%."
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"Enhanced awareness of the theoretical uncertainties and the difference between the results obtained from inclusive and exclusive analyses leads us to be even more conservative in setting the error bar than in previous reviews."

"A theoretical uncertainty within the 10%-15% window seems likely."

"For a total theory error of 15% and total precision of 18%."
and PDG conveniently ignores ADFR

| QCD Calculation | Phase Space Region | $\Delta \Gamma_{\text{theory}}$ (ps$^{-1}$) | $|V_{ub}|$ (10$^{-3}$) |
|-----------------|--------------------|------------------------------------------|------------------|
|                 | $M_X \leq 1.55$ GeV| 39.3$_{-0.8}^{+1.4}$ | 4.17 $\pm$ 0.15 $\pm$ 0.12$_{+0.24}^{-0.24}$ |
|                 | $M_X \leq 1.70$ GeV| 46.1$_{-0.9}^{+0.9}$ | 3.97 $\pm$ 0.17 $\pm$ 0.14$_{+0.20}^{-0.20}$ |
|                 | $P_{+} \leq 0.66$ GeV | 38.3$_{-0.8}^{+1.3}$ | 4.02 $\pm$ 0.18 $\pm$ 0.16$_{+0.23}^{-0.23}$ |
| BLNP            | $M_X \leq 1.70$ GeV, $q^2 \geq 8$ GeV$^2$ | 23.8$_{-0.4}^{+0.4}$ | 4.25 $\pm$ 0.19 $\pm$ 0.13$_{+0.17}^{-0.17}$ |
|                 | $M_X - q^2, p_T > 1.0$ GeV | 62.0$_{-0.9}^{+0.9}$ | 4.28 $\pm$ 0.15 $\pm$ 0.18$_{+0.20}^{-0.20}$ |
|                 | $p_T > 1.0$ GeV | 62.0$_{-0.9}^{+0.9}$ | 4.30 $\pm$ 0.18 $\pm$ 0.21$_{+0.23}^{-0.23}$ |
|                 | $p_T > 1.3$ GeV | 52.8$_{-0.3}^{+0.3}$ | 4.29 $\pm$ 0.18 $\pm$ 0.20$_{+0.20}^{-0.20}$ |
|                 | $M_X \leq 1.55$ GeV | 35.3$_{-0.3}^{+0.3}$ | 4.40 $\pm$ 0.16 $\pm$ 0.12$_{+0.24}^{-0.24}$ |
|                 | $M_X \leq 1.70$ GeV | 42.0$_{-0.8}^{+0.8}$ | 4.16 $\pm$ 0.18 $\pm$ 0.14$_{+0.26}^{-0.26}$ |
|                 | $P_{+} \leq 0.66$ GeV | 36.9$_{-0.8}^{+0.8}$ | 4.10 $\pm$ 0.19 $\pm$ 0.17$_{+0.34}^{-0.34}$ |
| DGE             | $M_X \leq 1.70$ GeV, $q^2 \geq 8$ GeV$^2$ | 24.4$_{-0.2}^{+0.2}$ | 4.19 $\pm$ 0.19 $\pm$ 0.12$_{+0.19}^{-0.19}$ |
|                 | $M_X - q^2, p_T > 1.0$ GeV | 58.7$_{-0.3}^{+0.3}$ | 4.40 $\pm$ 0.16 $\pm$ 0.18$_{+0.13}^{-0.13}$ |
|                 | $p_T > 1.0$ GeV | 58.7$_{-0.3}^{+0.3}$ | 4.42 $\pm$ 0.19 $\pm$ 0.23$_{+0.13}^{-0.13}$ |
|                 | $p_T > 1.3$ GeV | 50.4$_{-0.3}^{+0.3}$ | 4.39 $\pm$ 0.19 $\pm$ 0.20$_{+0.15}^{-0.15}$ |
|                 | $M_X \leq 1.55$ GeV | 41.0$_{-0.4}^{+0.4}$ | 4.08 $\pm$ 0.15 $\pm$ 0.11$_{+0.20}^{-0.20}$ |
|                 | $M_X \leq 1.70$ GeV | 46.8$_{-0.4}^{+0.4}$ | 3.94 $\pm$ 0.17 $\pm$ 0.14$_{+0.17}^{-0.17}$ |
|                 | $P_{+} \leq 0.66$ GeV | 44.0$_{-0.3}^{+0.3}$ | 3.75 $\pm$ 0.17 $\pm$ 0.15$_{+0.30}^{-0.30}$ |
| GGOU            | $M_X \leq 1.70$ GeV, $q^2 \geq 8$ GeV$^2$ | 24.7$_{-0.3}^{+0.3}$ | 4.17 $\pm$ 0.18 $\pm$ 0.12$_{+0.25}^{-0.25}$ |
|                 | $M_X - q^2, p_T > 1.0$ GeV | 60.2$_{-0.3}^{+0.3}$ | 4.35 $\pm$ 0.16 $\pm$ 0.18$_{+0.19}^{-0.19}$ |
|                 | $p_T > 1.0$ GeV | 60.2$_{-0.3}^{+0.3}$ | 4.36 $\pm$ 0.19 $\pm$ 0.23$_{+0.19}^{-0.19}$ |
|                 | $p_T > 1.3$ GeV | 51.8$_{-0.3}^{+0.3}$ | 4.33 $\pm$ 0.18 $\pm$ 0.20$_{+0.11}^{-0.11}$ |
|                 | $M_X \leq 1.55$ GeV | 47.1$_{-0.3}^{+0.3}$ | 3.81 $\pm$ 0.14 $\pm$ 0.11$_{+0.19}^{-0.19}$ |
|                 | $M_X \leq 1.70$ GeV | 52.3$_{-0.3}^{+0.3}$ | 3.73 $\pm$ 0.16 $\pm$ 0.13$_{+0.17}^{-0.17}$ |
|                 | $P_{+} \leq 0.66$ GeV | 48.9$_{-0.3}^{+0.3}$ | 3.56 $\pm$ 0.16 $\pm$ 0.15$_{+0.18}^{-0.18}$ |
| ADFR            | $M_X \leq 1.70$ GeV, $q^2 \geq 8$ GeV$^2$ | 30.9$_{-0.3}^{+0.3}$ | 3.74 $\pm$ 0.16 $\pm$ 0.11$_{+0.18}^{-0.18}$ |
|                 | $M_X - q^2, p_T > 1.0$ GeV | 62.0$_{-0.3}^{+0.3}$ | 4.29 $\pm$ 0.15 $\pm$ 0.18$_{+0.18}^{-0.18}$ |
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Is there NP in Inclusive vs Exclusive non-charm semileptonic B decays?

Gambino@Beauty 2016

Again:

My guess: Systematics in $|V_{ub}|$ from inclusive determination badly underestimated.

I already explained why

$V_{cb}$ from $D^*$ have recent tortuous story

It changed between versions for Belle (2017 $\rightarrow$ 2018)

Then it changed between versions for BaBar (2018 $\rightarrow$ 2019)

and hence I do not bother citing them
Concluding remarks
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Concluding remarks

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But perhaps you found it thought provoking (and then I did not waste large $T$ in preparing this).

Even better, perhaps you are annoyed because you feel I have criticized your work: then I have done my work.
I leave you with two RZ quotes:

“There is also an instanton model but I do not know if it can be trusted”

“I don’t understand how these authors get 10% errors by combining two results, both with greater than 10% errors”

–priceless
Please join me in thanking the organizers.