# Initial Conditions and Parton Distributions
# What We Know
# What We Don't Know
# What We Should Know


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**Outline**

1. Conclusions

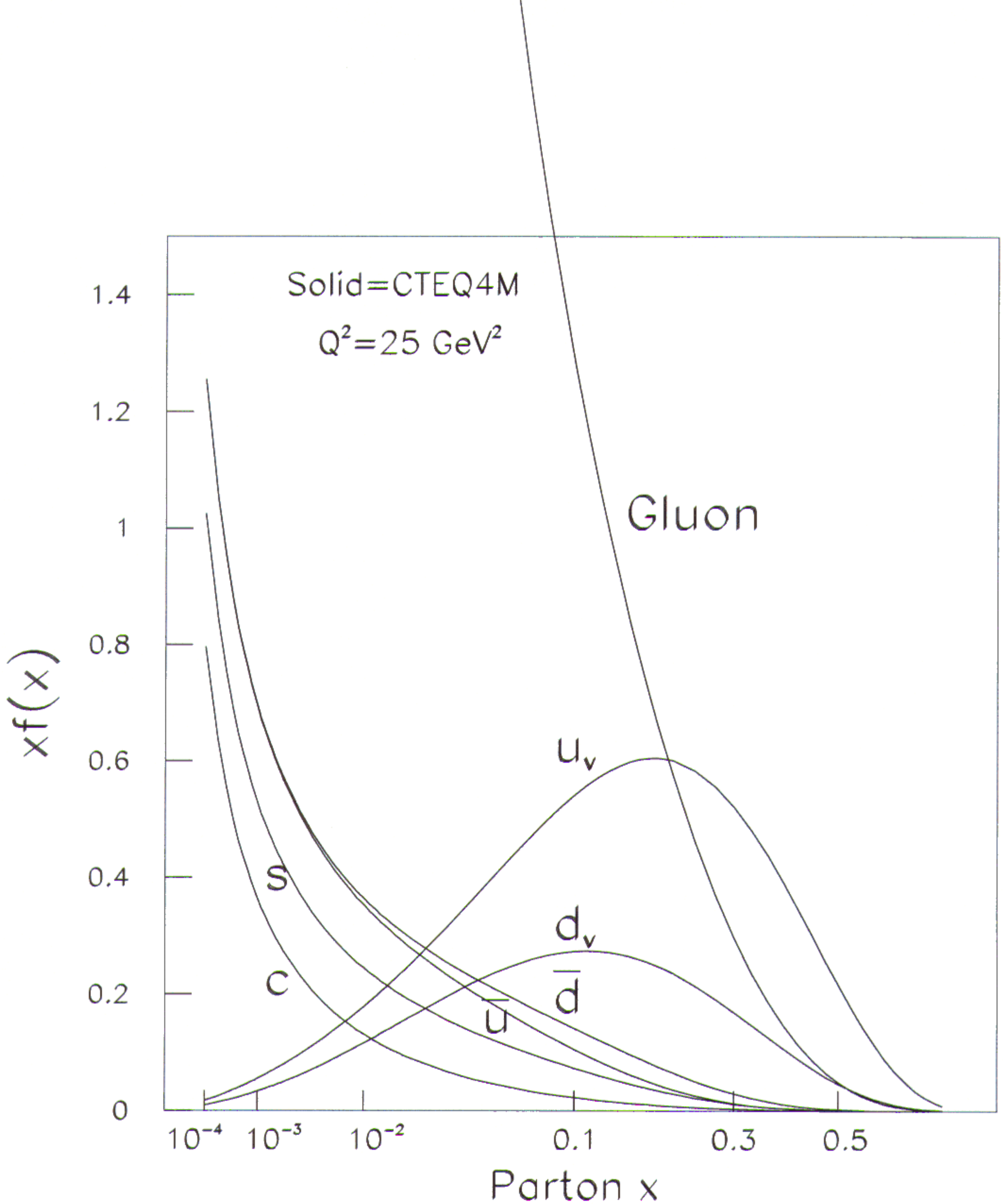
2. What goes into Global Fits

    Measurement by Measurement (I will skip some, mostly the ones that deal with flavor content)
    What are the chances it will get better?
    What are the implications for RHIC?
    Can RHIC improve on things?

3. What doesn't go into Global Fits

    Why not?
    What are the implications for RHIC?

4. Conclusions



Solid=CTEQ4M
Q²=25 GeV²
Gluon
$u_v$
$d_v$
$\bar{d}$
$\bar{u}$
s
c
xf(x)
Parton x
1.4
1.2
1
0.8
0.6
0.4
0.2
0
$10^{-4}$
$10^{-3}$
$10^{-2}$
0.1
0.3
0.5

**Conclusions**

1. Even in the proton, the gluon distribution is only known to ~15%. Quarks are known better.

2. The biggest open questions in PDF's deal with flavor issues, largely in the sea: $u$, $d$, $s$, and $c$ are all hot topics.

3. RHIC is less sensitive to the flavor issues than the overall quark and gluon content.

4. Predictions for $c$ and $(c\bar{c})$ production from first principles are at least questionable. The measurements can't be easily turned around to extract $G(x)$.

5. The available PDF's do not bracket the allowed range of variation.

Overall: any "discovery" at RHIC that depends entirely on a discrepancy with canned PDF's will probably be unconvincing to the larger physics community. We will need to show the offered interpretation is correct with data.

**Caveat**

◆ Parton Distribution Functions (PDF's) are necessary to understand hard processes

◆ Hard processes are rare.

◆ The luminosity of RHIC will be lower in Year One than later (we hope!)

◆ PDF's are less important in Year One than Year Two, Year Three, Year Four...

(That doesn't mean they are unimportant)

## Global Fits: The Basic Idea

Consider the reaction $A + B \to C + X$

with the partonic reaction: $a + b \to C$

The rate is determined by $\hat{\sigma}(a + b \to C)$, and the flux of partons $a$ and $b$ at a particular energy.

If we know the momentum of $A$, $B$ and $C$, and can calculate $\hat{\sigma}$, we can solve for the parton flux: we normally express this as a probability density that a particle of momentum $p$ carries a parton of momentum $xp$.

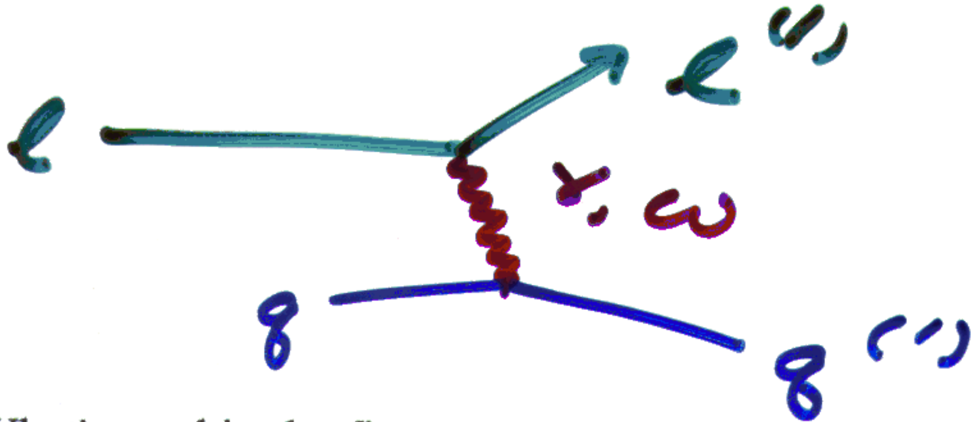
If these PDF's are physical things, we should get consistent results from different measurements.

Several groups take collections of data (selected for "quality") and fit common PDF's that explain all the data.

Two popular ones are MRS (Martin, Roberts and Stirling) and CTEQ (descendents of Duke-Owens and Morfin-Tung).

## Deep Inelastic Scattering

The granddaddy of PDF measurements.




What's used in the fit:

Collider *ep*: HERA (ZEUS, H1)
Fixed Target $\mu N$: NMC, FNAL E665, BCDMS
Fixed Target $\nu N$: CCFR (charged-current)

What's measured: 4 structure functions, which are related to:

$$u + \bar{u}$$
$$d + \bar{d}$$
$$\bar{u} + \bar{d}$$
$$s \text{ (assumed} = \bar{s})$$

Gluon constrained indirectly - "what's not the quarks must be the gluons"

A PLOT WITH LOTS OF DATA POINTS, ALL OF WHICH CAN BE FIT BY CTEQ-4M

CTEQ


**Figure 2** : Comparison of $F_2^p$ data from NMC, H1 and ZEUS to NLO QCD calculations based on CTEQ3M and CTEQ4M. The improvement in the small-$x$ region is evident.

**Nuclear Target Effects in DIS**

MRST apply an $x$-dependent heavy target correction (extrapolated from EMC data; larger than implied by shadowing at some $x$'s)

> "It is not clear whether the correction factor should be the same for neutral current and charged current DIS data, or be the same for $F_2$ and $xF_3$ neutrino data. For these reasons we do not include the CCFR heavy target data for $x < 0.1$..."
>
> MRST, hep-ph/9803445

These $x$'s correspond to minijets of 5-10 GeV at RHIC.

However, this is a 3% correction for Fe, and $q$+$g$ scattering isn't the dominant process at these $p_T$'s.

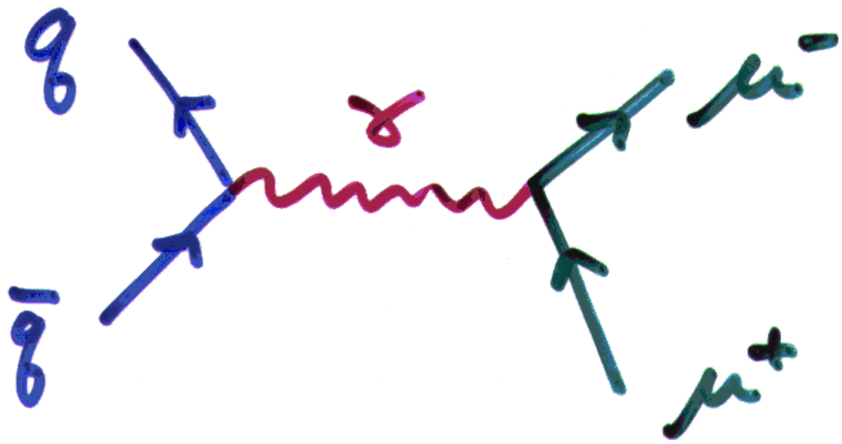
**What We Would Like**

The measurements that would completely settle this issue would be DIS at HERA off nuclear targets.

This has about the right $x$, $Q^2$ for RHIC.

This has been discussed by the HERA folks, but a strong consensus to do this hasn't developed: the feeling is that HERA would like to collect more $(e,p)$ data over the next few years.

**Drell-Yan**



What's used in the fit:

Fixed Target:  E605, E772, E866, NA51

What's measured: $\bar{q}$ in the proton,
$\bar{u} - \bar{d}$ (HERMES data also contributes)

**Nuclear Effects:**

For $x_F > 0$, nuclear effects are small or absent. There is no correction (that is, $A^\alpha$ has $\alpha=1$) for nuclear effects.

For $x_F < 0$, nuclear effects could be significant. (E-772)

However, E-772 is a small acceptance, forward spectrometer. The effect is largest where the acceptance is smallest.

It would be worthwhile to repeat the experiment at smaller (more negative) $x_F$:

More easily measure the effect at turn-on
See how the effect develops with $x_F$.

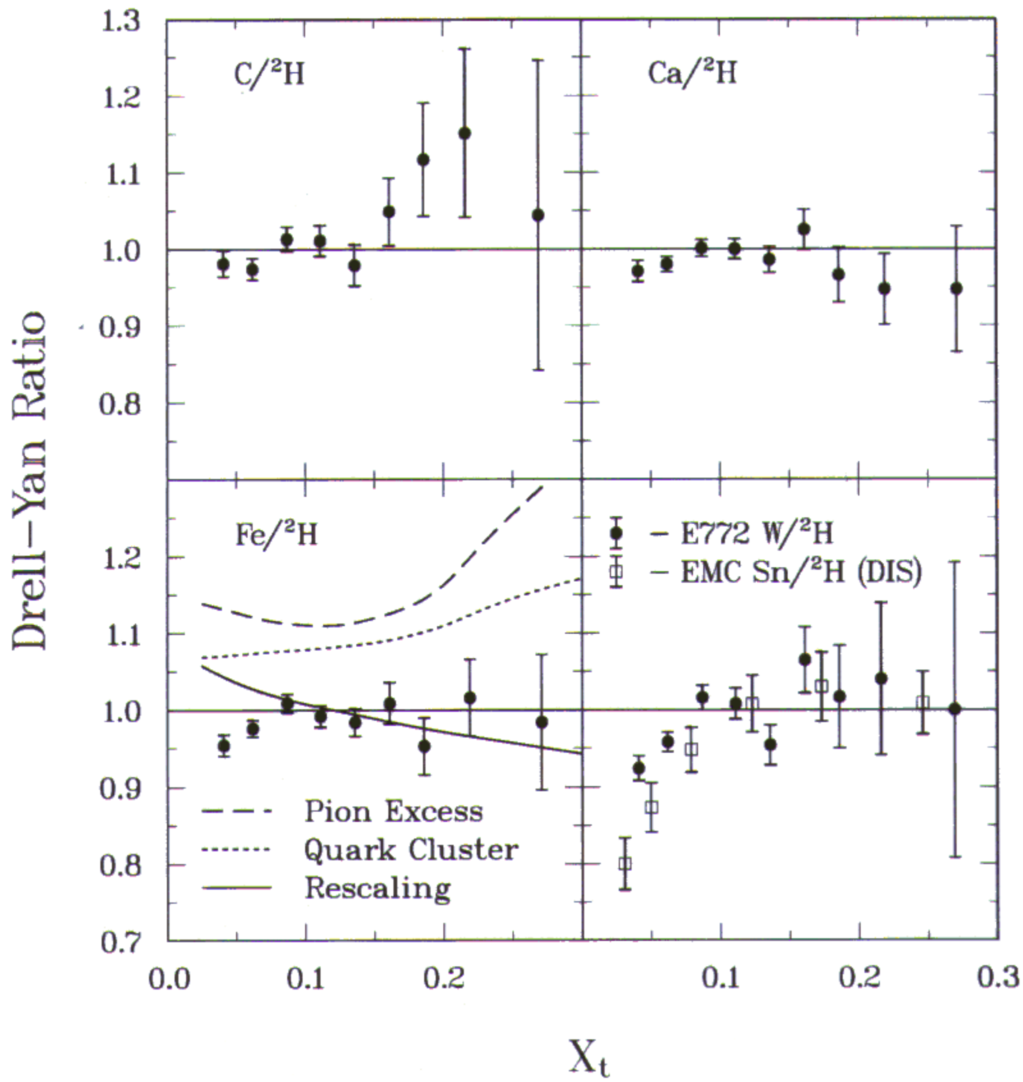
**What about the difference between *u* and *d* sea quarks?**

I think this is one of the most interesting physics results of the last few years.

I also can't see how it impacts RHIC physics, especially in the early years. (Lack of imagination on my part, surely)

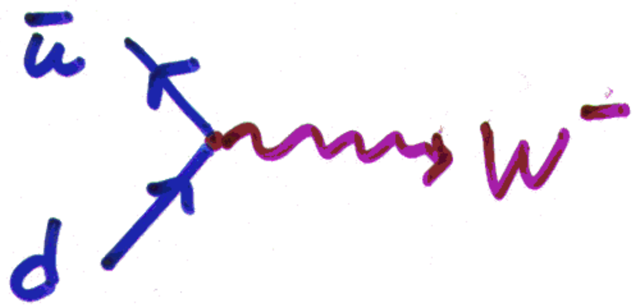
So I (regretfully) won't talk about it.

E-772



C/$^2$H
Ca/$^2$H
Fe/$^2$H
– E772 W/$^2$H
– EMC Sn/$^2$H (DIS)
Pion Excess
Quark Cluster
Rescaling
Drell–Yan Ratio
1.3
1.2
1.1
1.0
0.9
0.8
0.7
0.0
0.1
0.2
0.3
$X_t$

**$W$ Asymmetry**

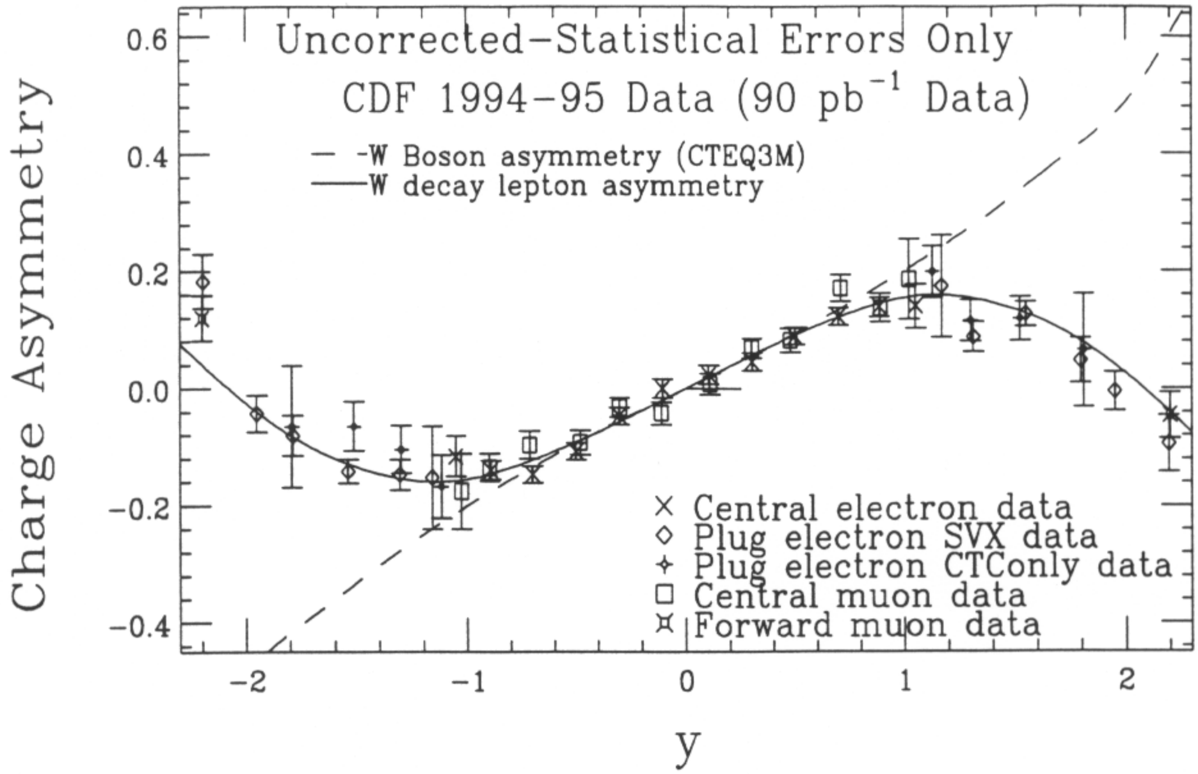


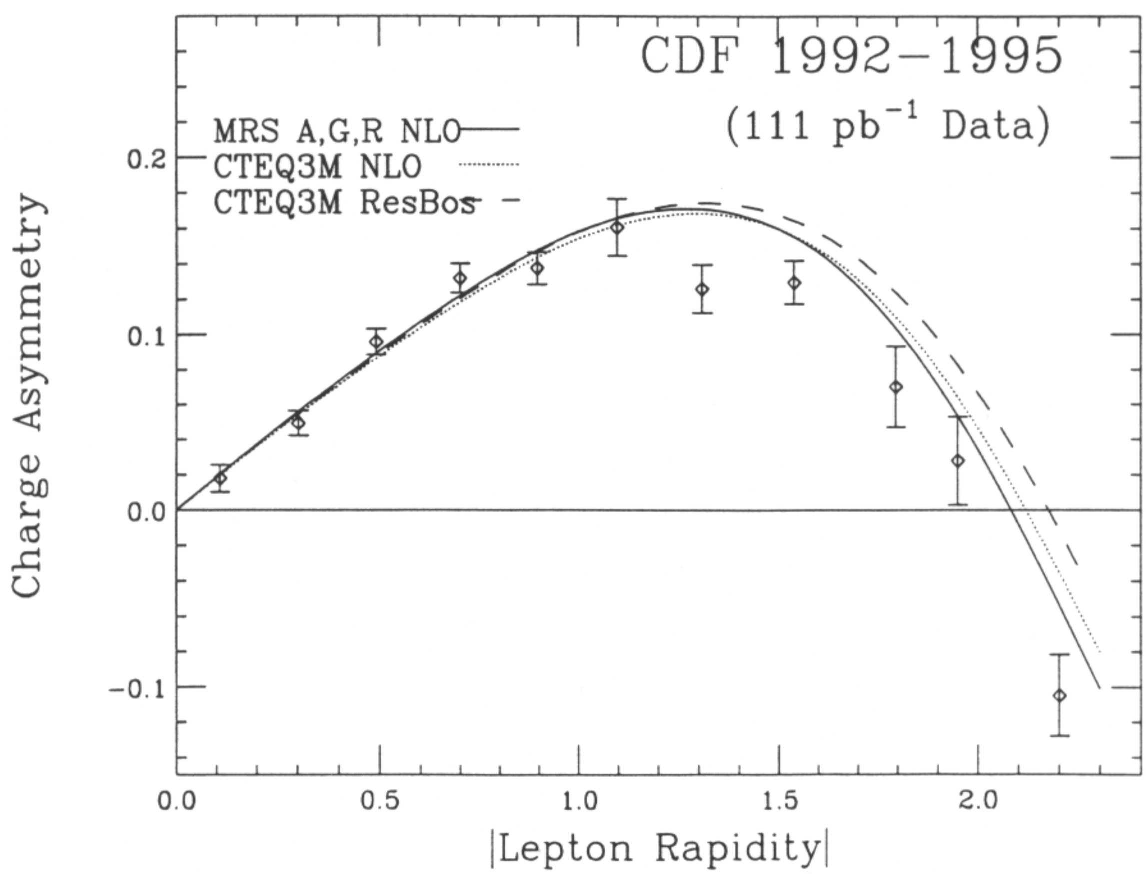

What's used in the fit:

Collider: CDF
What's measured: predominantly $u$-$d$ in the proton, some minimal sea information

The idea: the $p_Z$ of the $W^+$ is $p_{\text{beam}}(x_u - x_d)$. (Proton direction is forward) For a $W^-$, the signs flip because the participating quarks came out of the other particle.

Doing the math, $A_W(y) \cong \dfrac{u(x_1)d(x_2) - d(x_1)u(x_2)}{u(x_1)d(x_2) + d(x_1)u(x_2)}$


Uncorrected−Statistical Errors Only
CDF 1994−95 Data (90 pb$^{-1}$ Data)
−W Boson asymmetry (CTEQ3M)
W decay lepton asymmetry
Central electron data
Plug electron SVX data
Plug electron CTConly data
Central muon data
Forward muon data
Charge Asymmetry
y


CDF 1992−1995
(111 pb$^{-1}$ Data)
MRS A,G,R NLO
CTEQ3M NLO
CTEQ3M ResBos
Charge Asymmetry
|Lepton Rapidity|

MRS-T

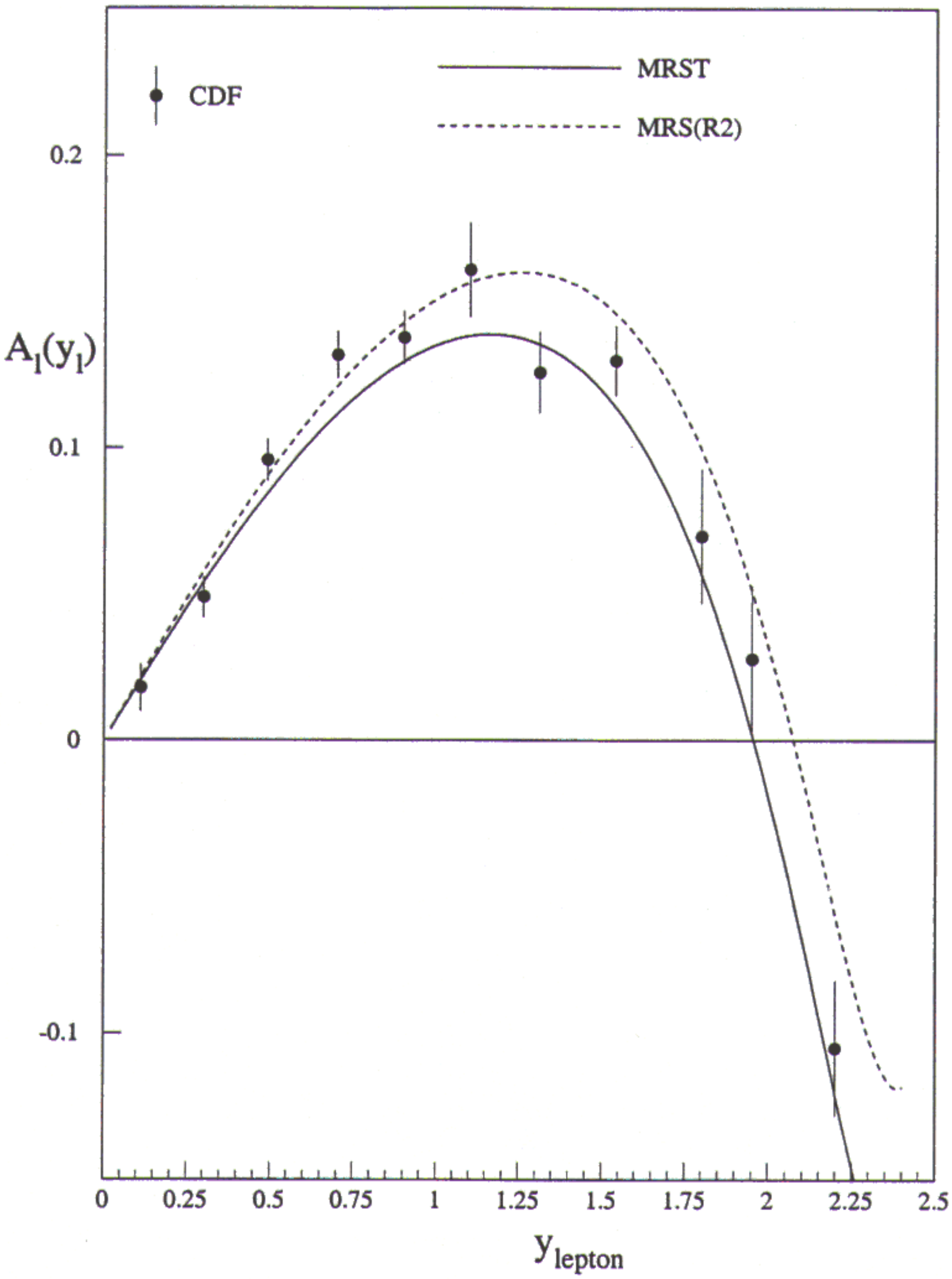



Figure 35: The description of the lepton asymmetry for $W^{\pm}$ production in $\bar{p}p$ collisions at $\sqrt{s}$ = 1.8 TeV. The data from CDF [12] are compared with the new MRST parton set and with the previous set MRS(R2).

**Nuclear Effects:**

Nobody has ever seen a $W$ produced off a nuclear target.

RHIC can do it, by running as an asymmetric collider. If one runs at $p(p)$=250 GeV and $p(A)$ = 100$A$ GeV, the center of mass energy is 316 GeV.

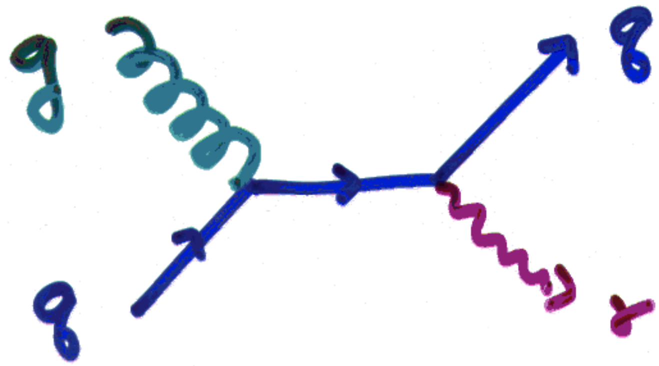
Going to $p(p)$=300 GeV and $p(A)$ = 120$A$ GeV gets us to 379 GeV.

This is good for hundreds to thousands of $W$'s.

Of course, the asymmetry has to be slightly redefined because there is no longer a participating antiproton. Also, the sea quarks are now involved in a way that they weren't before.

Also, to be honest (even though this is a pet idea of mine), the difference of $u(x)$ and $d(x)$ is less important than the sum. (The difference does contribute to direct photons, though, because a $u$ quark is 4x as likely to radiate a photon as a $d$)

**Direct Photons**




What's used in the fits:
Fixed Target: WA70, E706, sometimes UA6

What's measured: gluon content of the proton

Sociological Aside: there is one post-WA70 experiment used in these fits, Fermilab E-706.

Lots of experiments tried (and many succeeded) to measure direct photons, but aren't in the fits.

E-706 was the only experiment that set out exclusively to do direct photons: everyone else said, "Oh yeah, I guess we can do that too."

The downside of a lack of competition: it's been a decade since E-706 ran, and results are still dribbling out.

I'm sure there's a lesson for RHIC in there somewhere.

**Nuclear Effects:**

There isn't much evidence for any: $A^{\alpha}$ has $\alpha$=1

There is no published data from E-706 on shapes of distributions ($p_T$, $y$) vs. nuclei. (Be, Cu)

(Remember, E-706 has no real competition)

There might well be something:
We know the quark distribution in nuclei is different
Sum rules link the quarks and the gluons

***$k_T$* Smearing**

The biggest problem with extracting $G(x)$ is $k_T$ Smearing: the fact that the parton direction of motion is not the same as the particle direction of motion.

If one triggers on a photon of a given $p_T$, it is more likely that the true $p_T$ is smaller and has been boosted by intrinsic $k_T$ than the true $p_T$ is bigger, and the intrinsic $k_T$ points in the other direction.

i.e. an observed 10 GeV photon is more likely to be a "real" 9 GeV photon with an extra GeV of $k_T$ than a "real" 11 GeV: because there are more 9's than 11's.

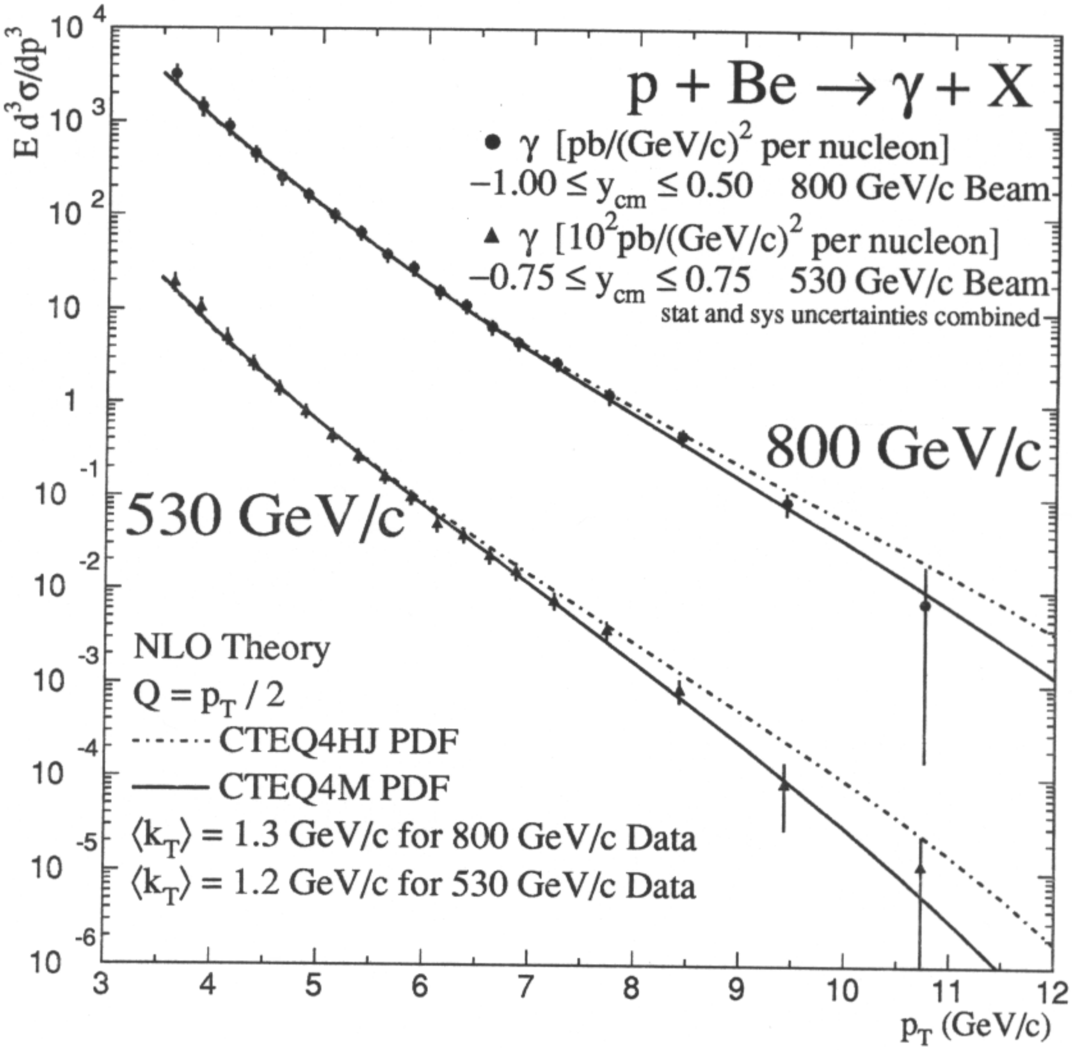
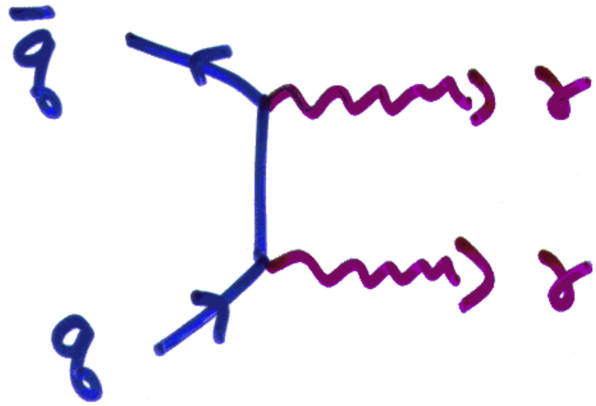



FIG. 6. Direct-photon inclusive cross sections as functions of $p_T$ for 530 and 800 GeV/$c$ proton-nucleon interactions compared to results of NLO PQCD calculations using CTEQ4HJ (dot-dashed curve) and CTEQ4M (solid curve) PDF. Factors for supplemental $\langle k_T \rangle$ are included. (Note that the units for the 530 and 800 GeV/$c$ results differ by a factor of 100.)

**Measuring $k_T$:**

Standard Trick Number One - Diphotons:




$p_T(\gamma_1)$ - $p_T(\gamma_2)$ and $\phi(\gamma_1)$-$\phi(\gamma_2)$ are both dependent on $k_T$.

The bad news: the diphoton cross-section is less than a percent of the total photon cross-section.

Standard Trick Number Two - Photon/Jet Balancing.

$p_T(\gamma)$ - $p_T$(jet) and $\phi(\gamma)$-$\phi$(jet) are both dependent on $k_T$.

The bad news: jet $p_T$ (and to a lesser extent $\phi$) resolution isn't so great. Also, this technique is usually used to *measure* the jet resolution. (i.e. $k_T$ needs to be an input, not an output)

The (partial) solution: use jets that fragment to leading $\pi^0$'s. Unfortunately, the cross-section is now not all that much better than for diphotons.

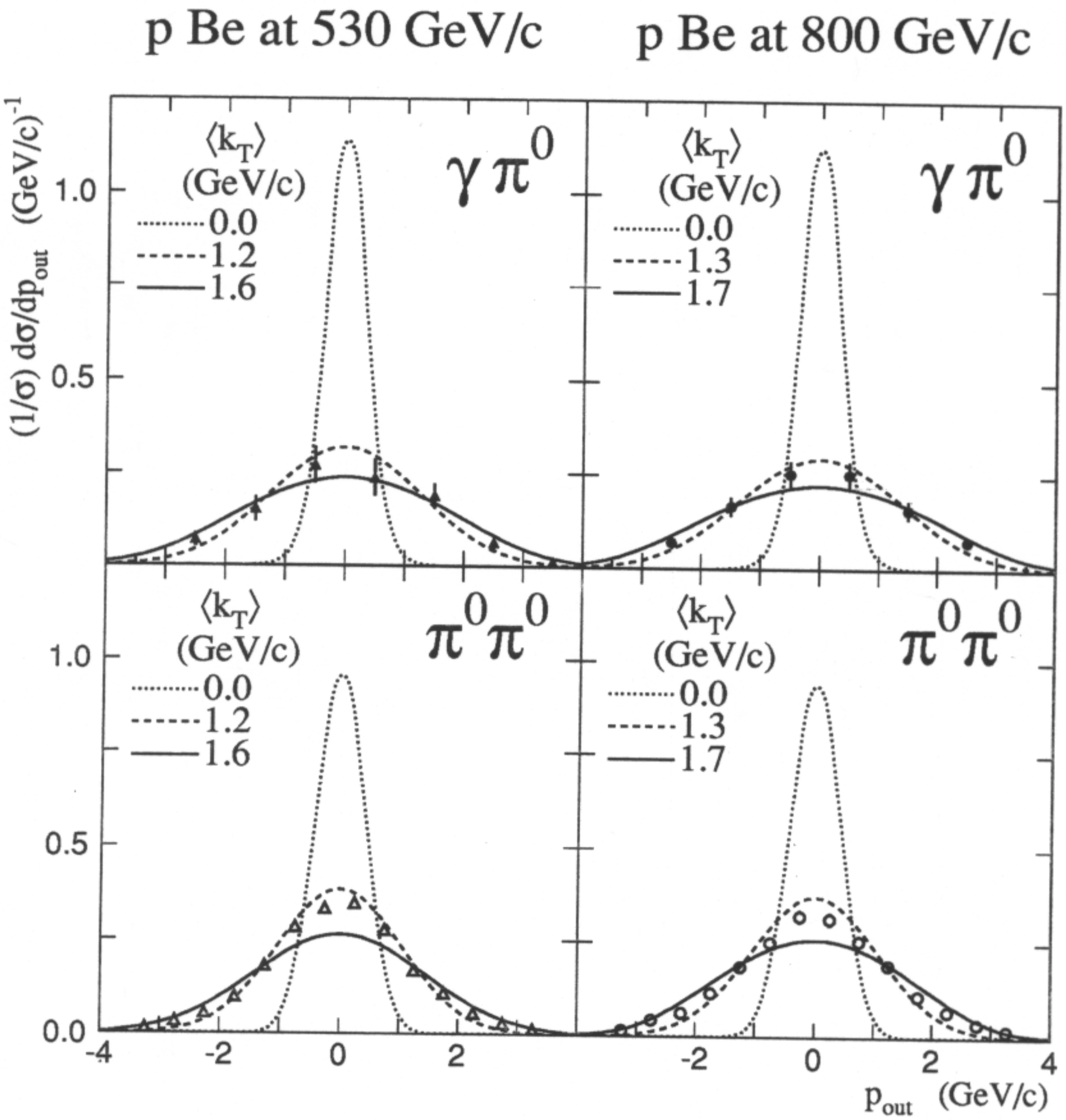
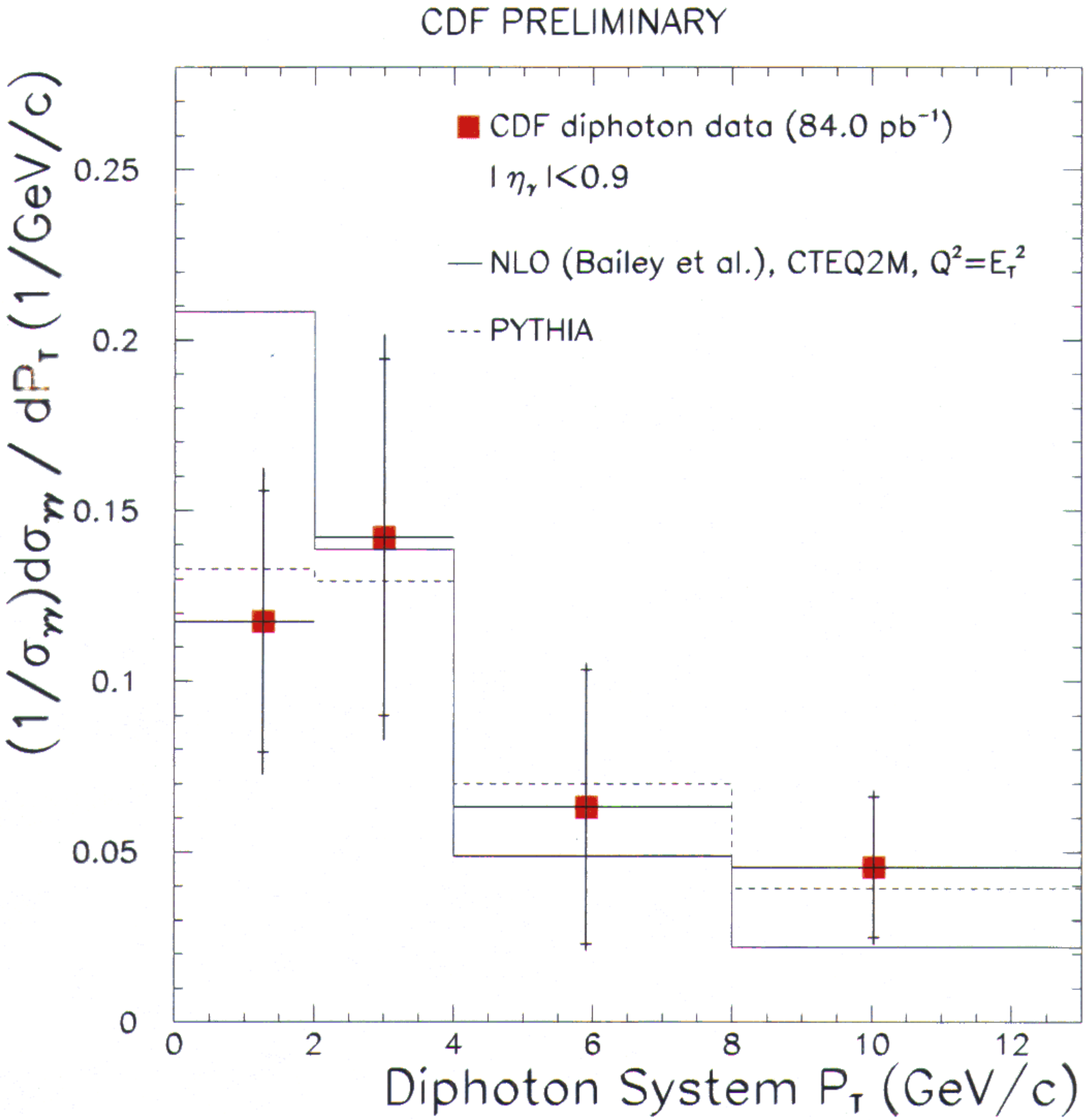
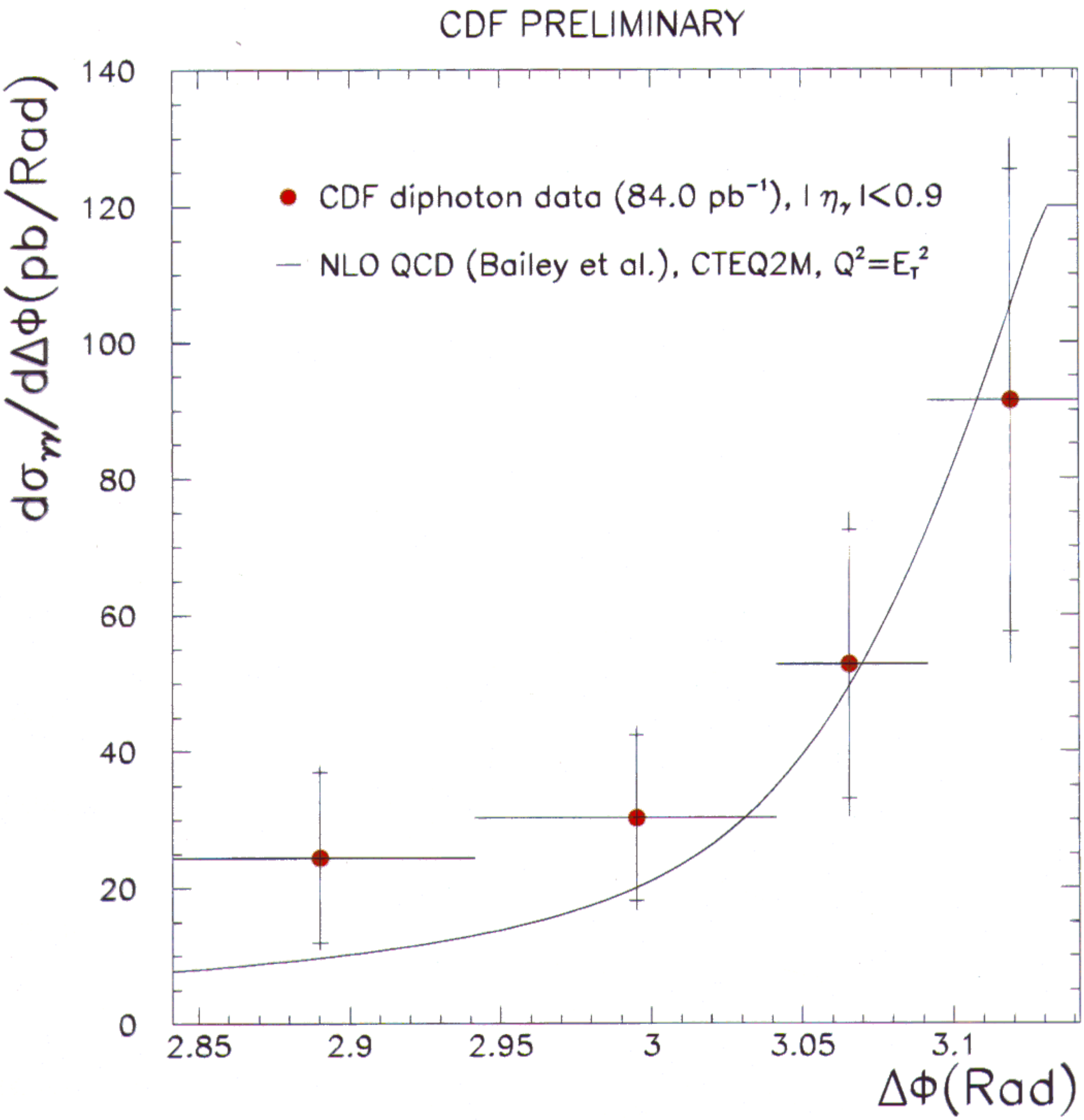



FIG. 5. The out-of-plane momentum distributions for high-mass pairs produced in proton-nucleon interactions at 530 and 800 GeV/$c$ compared to results of LO PQCD calculations (using CTEQ4L PDF) for several $\langle k_T \rangle$ values.



CDF PRELIMINARY
CDF diphoton data (84.0 pb⁻¹)
$|\eta_\gamma|<0.9$
NLO (Bailey et al.), CTEQ2M, $Q^2=E_T^2$
PYTHIA
$(1/\sigma_{\gamma\gamma})d\sigma_{\gamma\gamma}/dP_T$ (1/GeV/c)
Diphoton System $P_T$ (GeV/c)
0
0.05
0.1
0.15
0.2
0.25
0
2
4
6
8
10
12



CDF PRELIMINARY
$d\sigma_{\gamma\gamma}/d\Delta\Phi$(pb/Rad)
CDF diphoton data (84.0 $pb^{-1}$), $|\eta_\gamma|<0.9$
NLO QCD (Bailey et al.), CTEQ2M, $Q^2=E_T^2$
0
20
40
60
80
100
120
140
2.85
2.9
2.95
3
3.05
3.1
$\Delta\Phi$(Rad)

## $k_T$ and Energy

$k_T$ smearing is small in WA70 data. It's bigger at E-706, and bigger still at CDF.

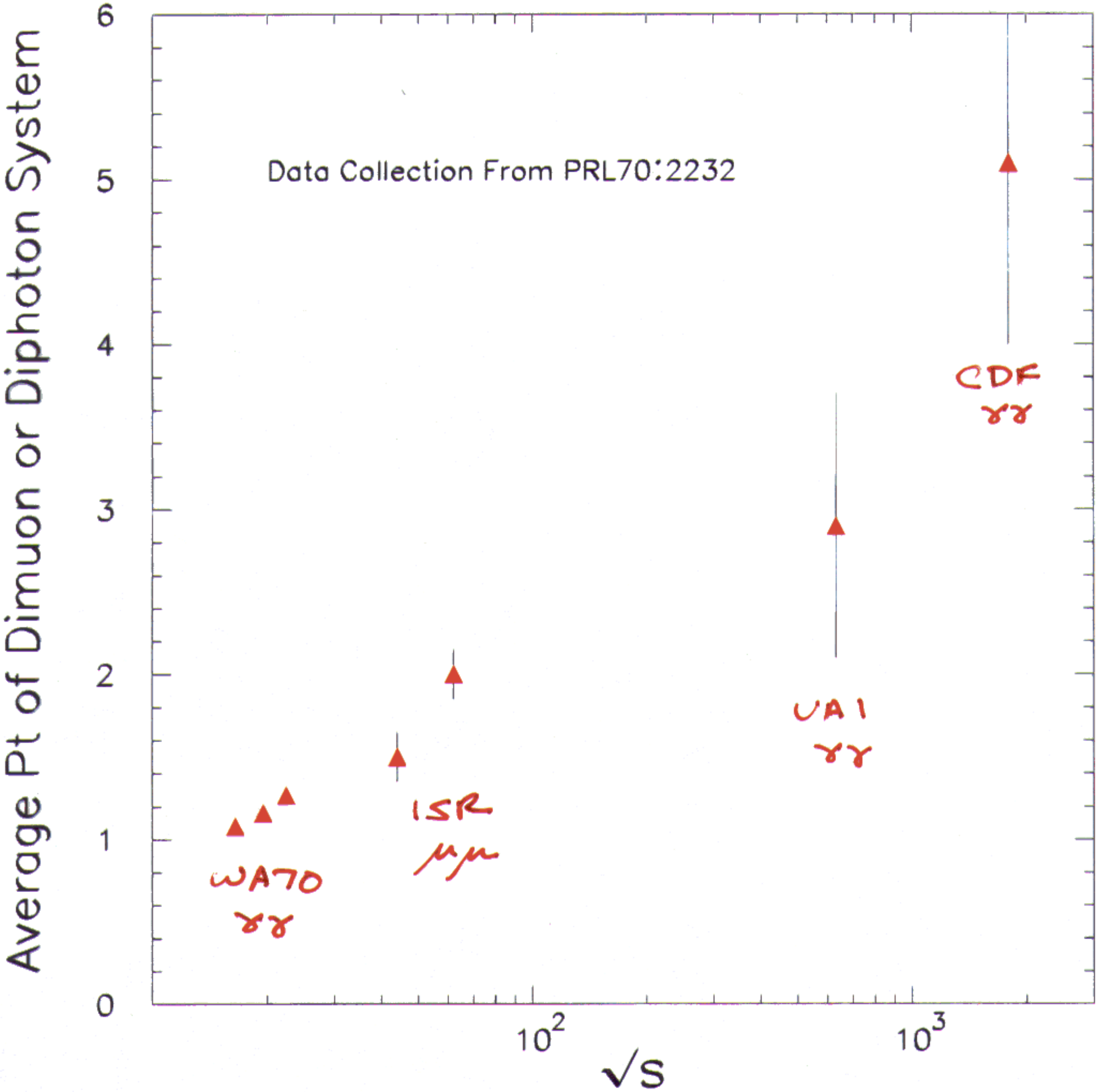
There's no data at 200 GeV. Do we interpolate between E-706 (40 GeV) and UA2 (620 GeV) or CDF (1800 GeV)?

Note that the UA2 and CDF photon data are not considered "high quality" by the fit groups - the data is not used in the fits.

## $k_T$ and Nuclei:

Do we have any reason to believe there is not (or is) a nuclear dependence to these $k_T$ effects?

E-706 uses a Be target, and CDF and UA2 use antiprotons. Is gold different? It's certainly heavier.


Data Collection From PRL70:2232
Average Pt of Dimuon or Diphoton System
√s
WA70
γγ
ISR
μμ
UA1
γγ
CDF
γγ
0
1
2
3
4
5
6
10^2
10^3

**The Long Term Solution**

The right way to solve this is to compare *pp* and *pA* collisions at 200 GeV. (Direct $\gamma$, $\gamma$-jet and $\gamma$-$\gamma$)

*pA* running seems to be in the future at RHIC. Year Three?

*AA* has (interrelated) difficulties:

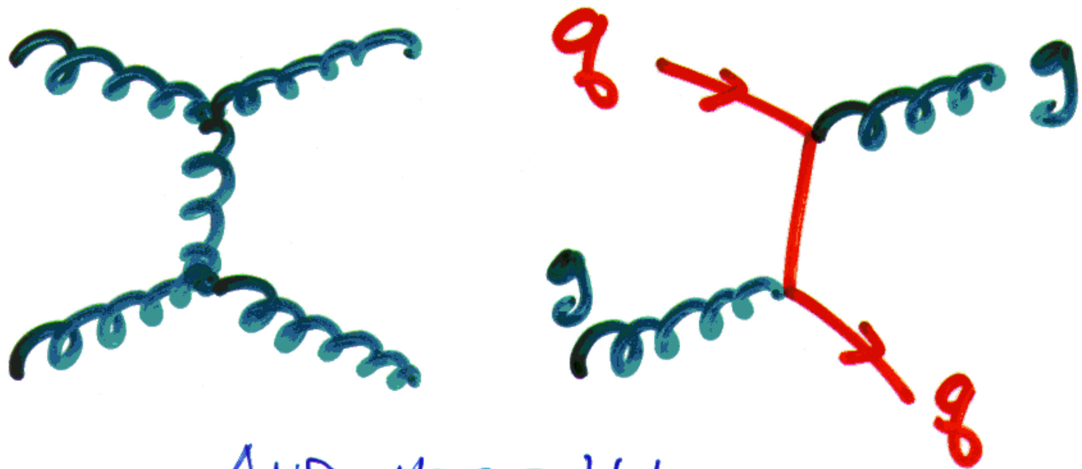
- Luminosity is low: pushes to low $p_T$. (Large $\sigma$)

- At low $p_T$, the problem of multiple parton interactions becomes more severe.

- The combinatorics for $\pi^0$ rejection is harder, so background subtraction is a bigger issue.

It's probably not impossible, but it's a lot of work. Then there's the question of whether a QGP will interfere with this - we lose some independence of controls this way.

**Jets**




AND MORE!!!

What's used in the fits: CDF, D0 inclusive cross-section

What's measured? $G(x)$ and $q(x)$ (sum over flavors)

Nuclear effects? Nobody knows.

$k_T$ effects? Probably, but are small compared to the jet resolution. (Lousy resolution is an advantage?!?)

The CDF measurement was about 40% higher than *any* set of PDF's available at the time predicted.

Lesson Learned: these PDF's do **not** represent the extremes of possible variation.

## What's Not in the Fits

### Heavy Flavor Hadroproduction

Not quite true: MRST uses the few dozen observed top quark events. It has all the impact that you expect a few dozen events to have.

CDF $b$ production (e.g.) is way larger than predicted by NLO QCD. It's likely that this is due to NNLO effects.

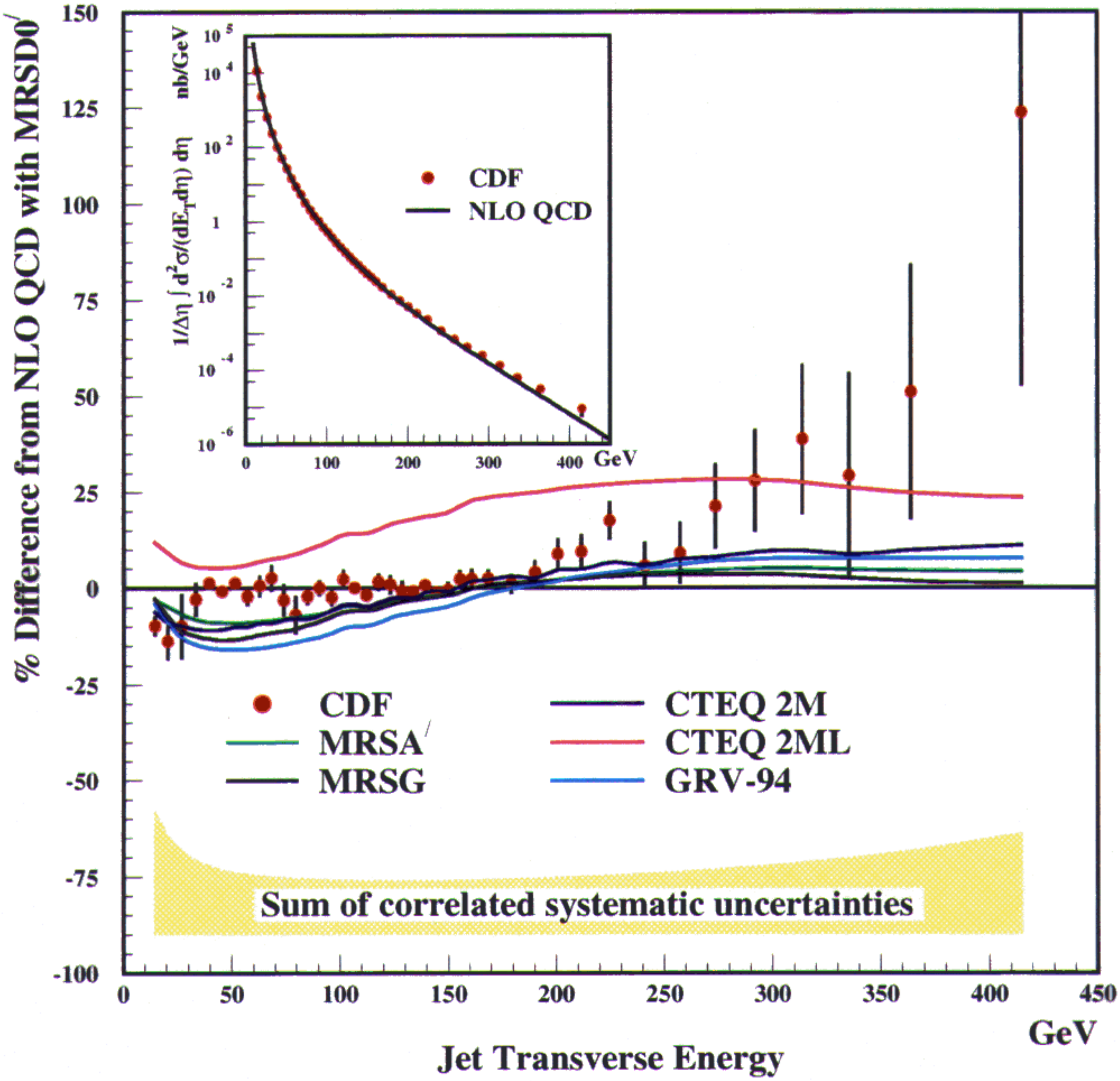
$c$ production is likely even less well understood.

### Quarkonium Hadroduction

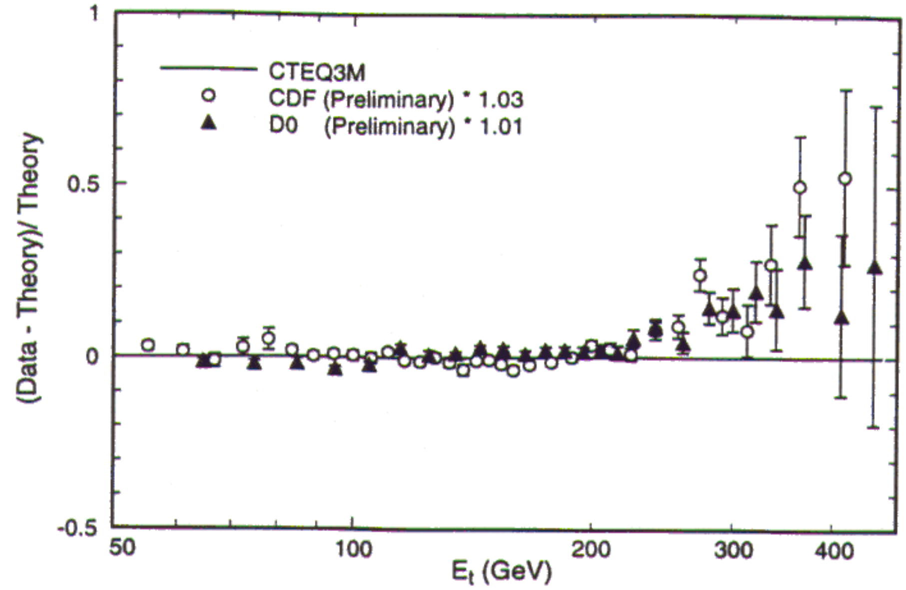
The most common suggestion is $gg \to \chi_2$ as a probe of $G(x)$.

If it tells us anything, the data we have on quarkonium production tells us that the story of quarkonium production is complex and filled with twists and turns.

Even if it were not so, the limitation of collider data to high $p_T$ $\chi$'s makes the extraction of $G(x)$ messy (or impossible).

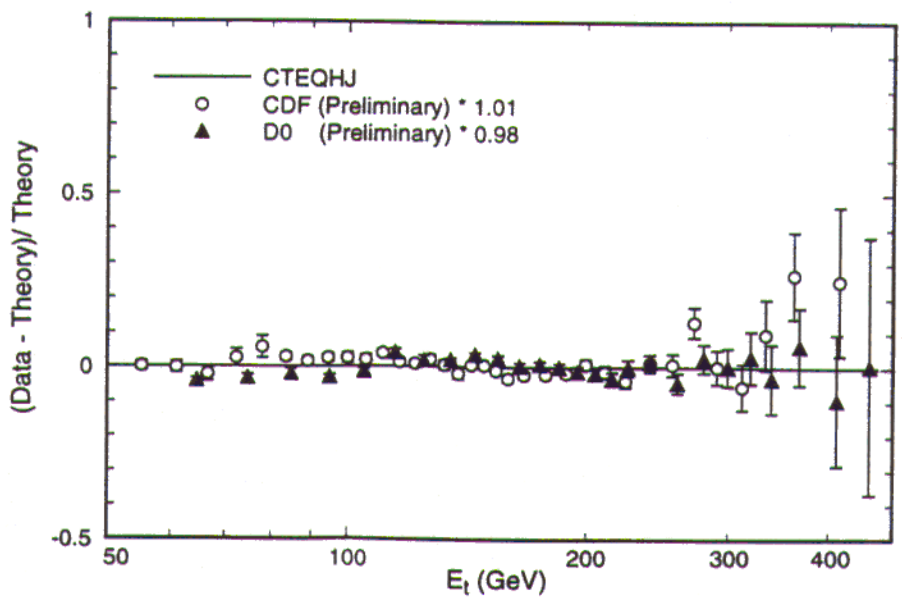


% Difference from NLO QCD with MRSD0'
150
125
100
75
50
25
0
-25
-50
-75
-100
0
50
100
150
200
250
300
350
400
450
GeV
Jet Transverse Energy
CDF
MRSA'
MRSG
CTEQ 2M
CTEQ 2ML
GRV-94
Sum of correlated systematic uncertainties
nb/GeV
$1/\Delta\eta \int d^2\sigma/(dE_T d\eta)\, d\eta$
$10^5$
$10^4$
$10^2$
1
$10^{-2}$
$10^{-4}$
$10^{-6}$
0
100
200
300
400
GeV
CDF
NLO QCD




← EXCESS

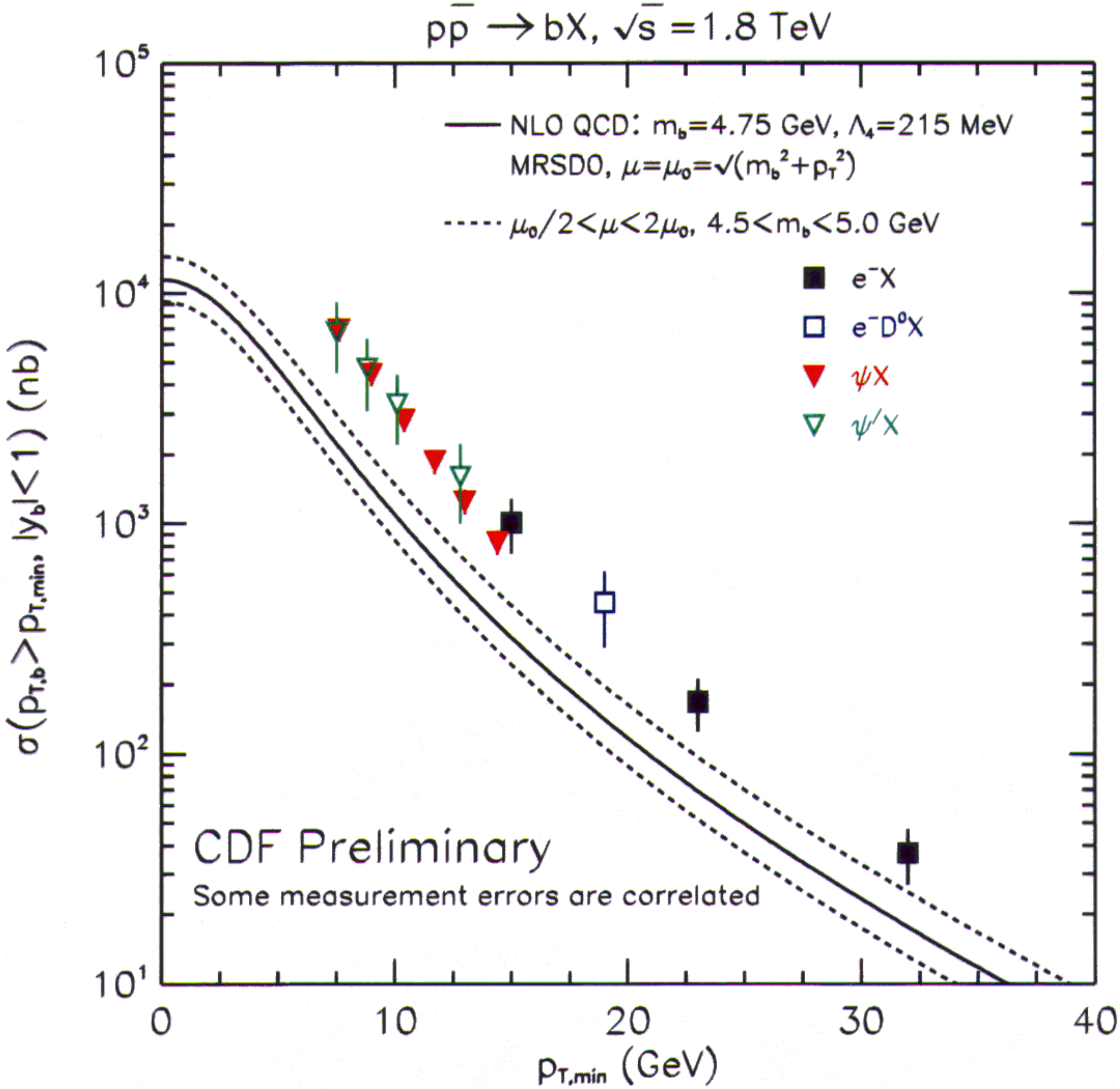
OLD GLUON




NO EXCESS

NEW GLUON

CTEQ



$p\bar{p} \rightarrow bX$, $\sqrt{s} = 1.8$ TeV
NLO QCD: $m_b$=4.75 GeV, $\Lambda_4$=215 MeV
MRSD0, $\mu=\mu_0=\sqrt{(m_b^2+p_T^2)}$
$\mu_0/2<\mu<2\mu_0$, $4.5<m_b<5.0$ GeV
$e^-X$
$e^-D^0X$
$\psi X$
$\psi' X$
CDF Preliminary
Some measurement errors are correlated
$\sigma(p_{T,b}>p_{T,min}, |y_b|<1)$ (nb)
$p_{T,min}$ (GeV)

## Conclusions

1. Even in the proton, the gluon distribution is only known to ~15%. Quarks are known better.

2. The biggest open questions in PDF's deal with flavor issues, largely in the sea: $u$, $d$, $s$, and $c$ are all hot topics.

3. RHIC is less sensitive to the flavor issues than the overall quark and gluon content.

4. Predictions for $c$ and $(c\bar{c})$ production from first principles are at least questionable. The measurements can't be easily turned around to extract $G(x)$.

5. The available PDF's do not bracket the allowed range of variation.

Overall: any "discovery" at RHIC that depends entirely on a discrepancy with canned PDF's will probably be unconvincing to the larger physics community. We will need to show the offered interpretation is correct with data.