LHC as a Photon Collider results from ATLAS (& CMS)

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LPC Seminar

November 29, 2016

result now in PRD

Took so long I had to petition to be an author.   Here is the rest of the group

Chav Chiv Chau
(U Toronto)
aQGC thesis

Last Feremenga
(U T Arlington)
Diff. H thesis

"and a cast of thousands"

Two photon physics

Started at e^+e^- colliders, became quite an industry

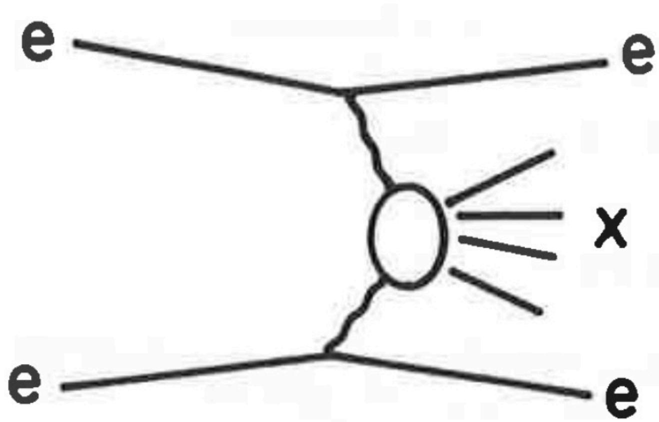
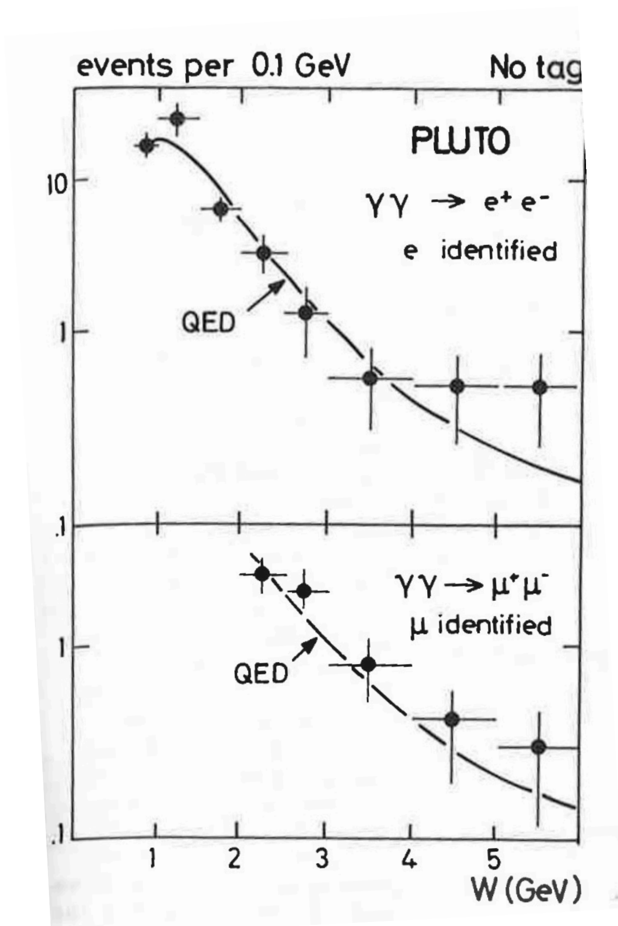


Fig. 1 basic diagram for the reaction ee → eex

$\gamma\gamma \rightarrow X$ X can be ee, $\mu\mu$, hadrons

Need very high E_{CM} to make massive X

QED and hadron spectroscopy



1980 DORIS result

Also studied at SPEAR

Not enough juice to get seriously interesting

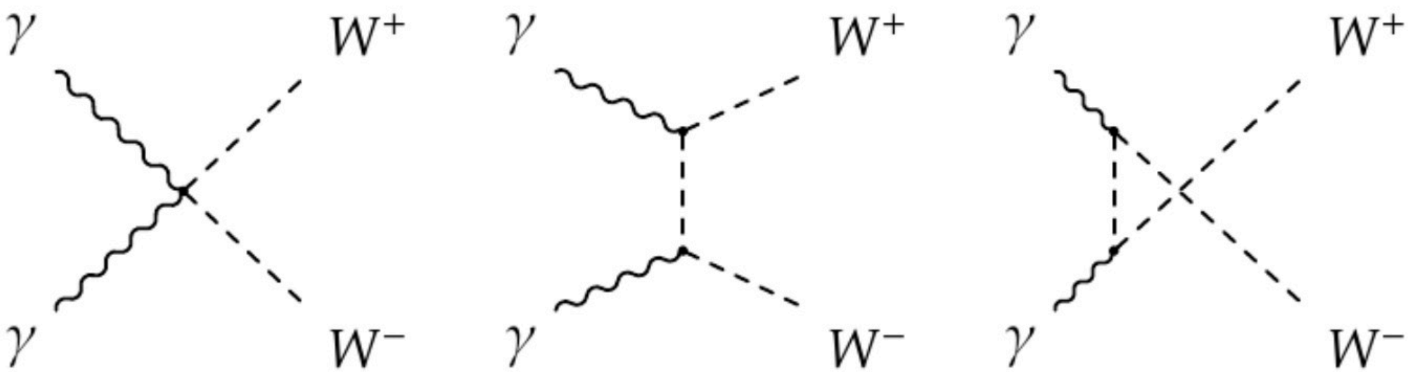
LEP just barely gets interesting

Off beaten path of boring charm, taus, WW, etc.

What, interesting?

I got interested from TL recounting CMS 7 TeV result

The Electroweak interaction is a complete self consistent theory (QED now SM)



SM got its start pondering unitarity in W pair production (νν➔WW!)

It might be, but really, is that all there is? Look for modifications in a generalized way – anomalous Triple and Quartic Gauge Couplings
In particular the guy on the left could have aQGC contributions!

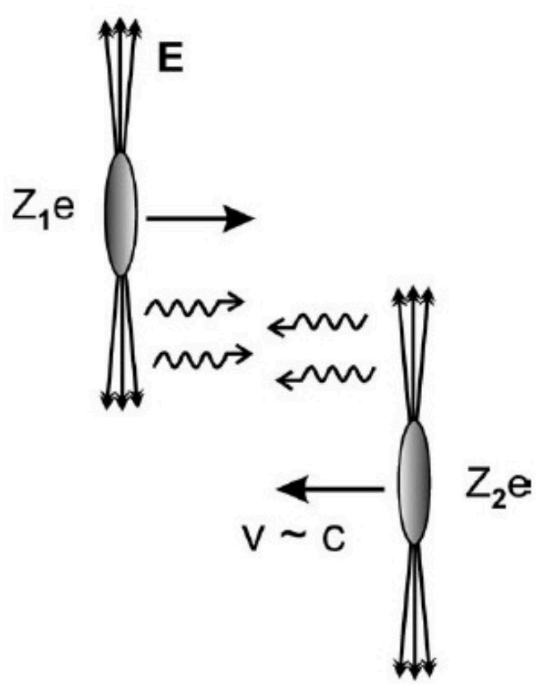
See http://arxiv.org/pdf/1309.7890v1.pdf
No theory talk here, I just use parameters in generators to quantify how much one measurement does not see to another!

Note: our channel is not competitive for aTGCs so we set them to 0 and look aQGC

Protons have charge!

Can use hadron collider, Q^2 limits how energetic photons can be without seriously trashing the proton(s) but with many TeV CM can make W pairs!

Pioneering work at Tevatron along with diffraction program: μμ, ψ, ψ`



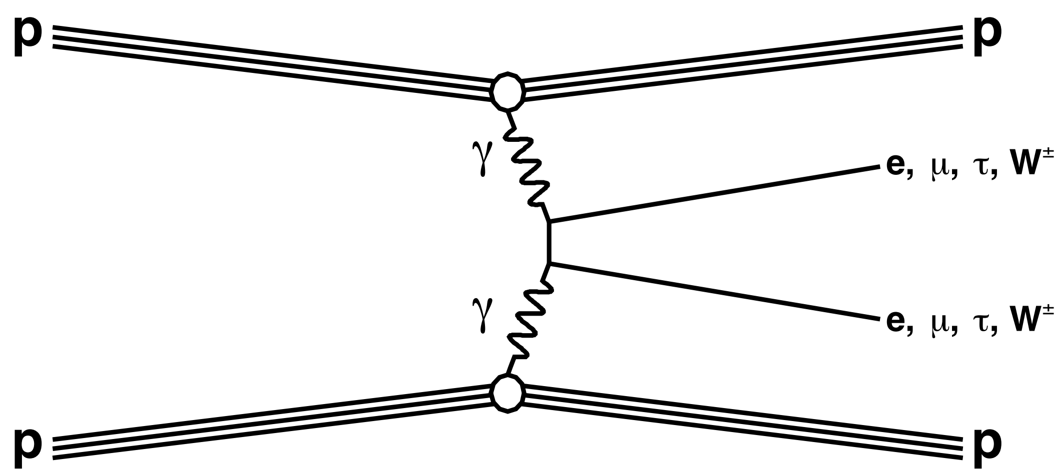
"Equivalent Photon Approximation"

Available in many MC generators

Proposed as LHC luminosity monitor
And diffractive H – clean study

Heavy ions even more but low rate

Photon fusion at LHC

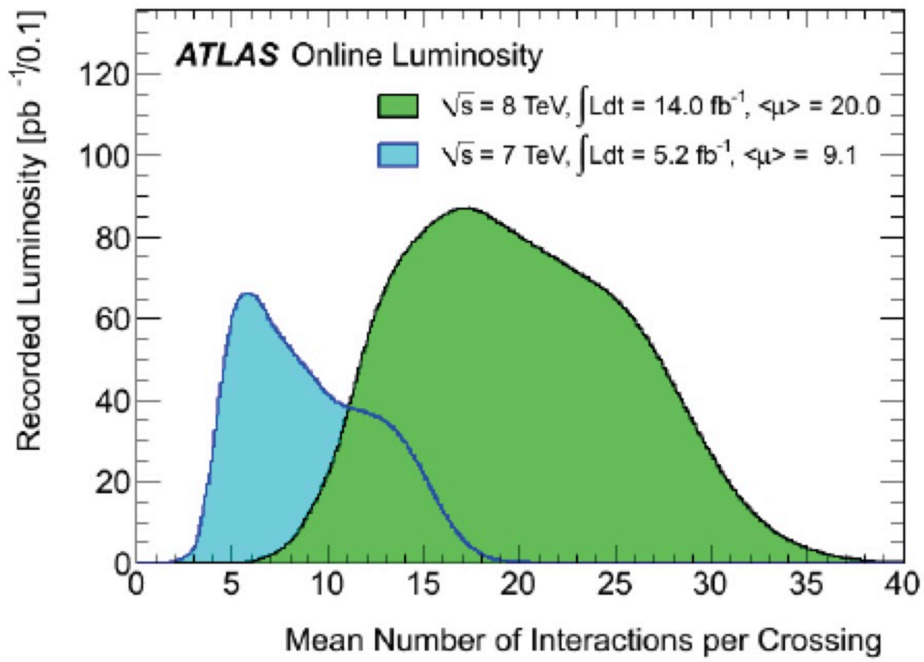


(WW 4 pt too)

X = ee, μμ, WW, …   m(X) can get up to a TeV

Looks just like diffraction – "rapidity gap" – oops pileup – track isolation
Also get coherent "elastic", single dissociation (SD), double dissociation (DD)

Pileup



So far CMS and older ATLAS results on 7 TeV data – both use ==2 track vertex, nothing else within 3 mm along beam line “EXCLUSIVITY”
This gets a bit inefficient at 8 TeV in ATLAS

Grand Strategy

Study dilepton QED, see if you can make sense of data
(will show old and new ATLAS results)
Demonstrate LHC as photon collider (well in 8 TeV ATLAS data)

Look at WW to see if anything funny is happening there
(will show ATLAS (and CMS) results)
aQGC studies started at LEP, then D0, both outgunned now

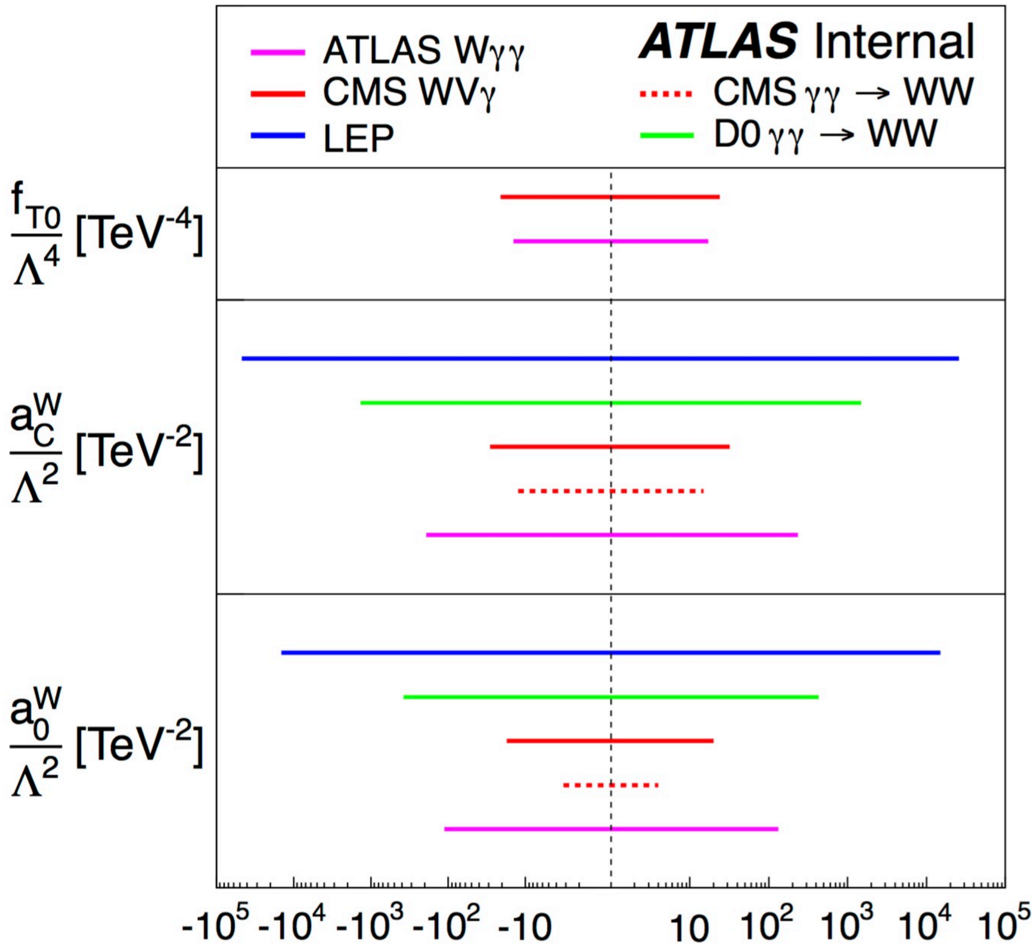
Discuss diffractive Higgs production

Mention light-by-light

Speculate about 13-14 TeV data

Random thoughts

When we started



From ATLAS Wγγ

I noticed CMS 7 WW

Tom LeCompte mentioned in conference report

Decided to try ATLAS 8 WW!

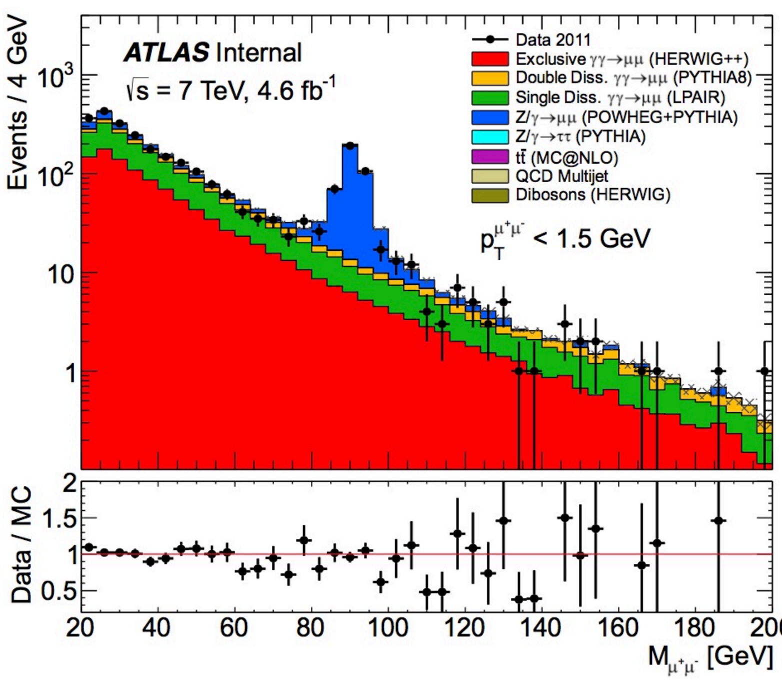
ATLAS 7 TeV ee μμ

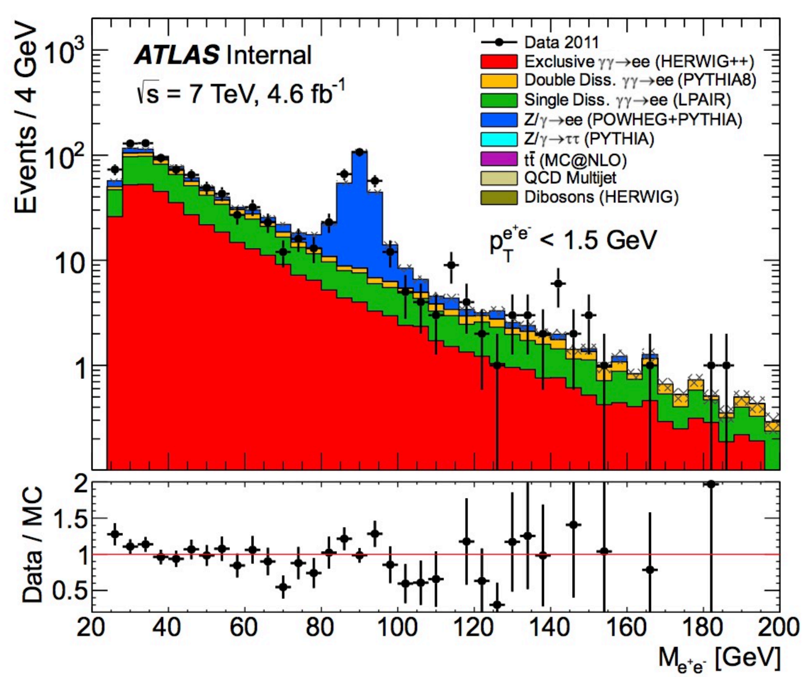
arXiv:1506.07098 PLB 749 66 (2015)

CMS did them first but ATLAS published the full 7 TeV dataset

Dilepton triggers μ: pT>10 |η|<2.4 m(μμ) >20 no Z
e: pT>12 |η|<2.4 m(ee) >24 no Z

Exclusivity (3 mm), pT(ll) < 1.5 (enhance QED!) 2124 μμ, 869 ee





Note on DY simulation

Simulations do not reproduce low multiplicity DY, reweighting needed

Z sample in data is used for tuning that and pT(ll), matrix inversion used (7 TeV)

Lots of official tools for reweighting for scales, resolutions, triggers, pileup etc.

For 8 TeV data we decided to avoid theory, use data driven efficiency, with WW in mind a 10% efficiency uncertainty is fine

Just scale MC by ntrack bin (8 TeV)

(Pardon the digression)

Fit acoplanarity

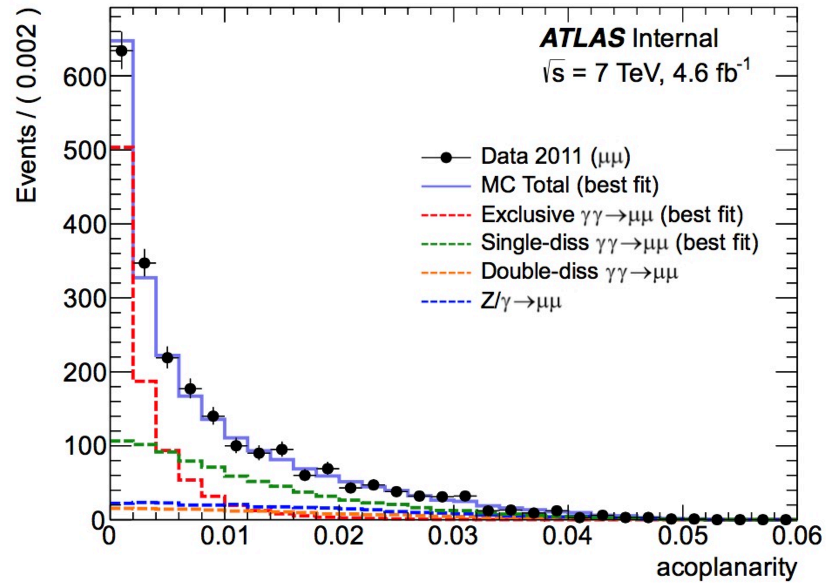
Dimuons
Constrain DY and DD, fit for coherent and SD parts, f parameters are data/MC fraction
MC=EPA or LPAIR SD

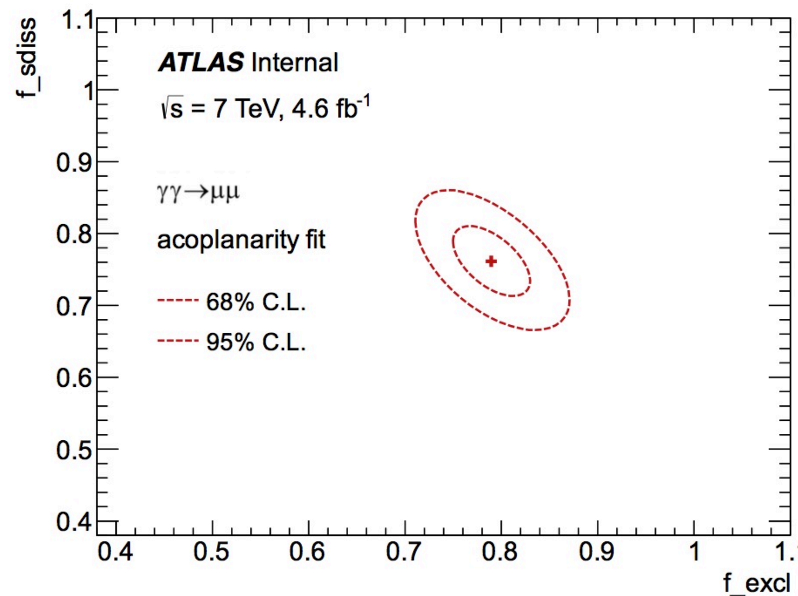
f_elastic = 0.791 +0.041 -0.040
f_SD = 0.762 +0.49 – 0.048

Similarly for electrons

f_elastic = 0.863 +0.070 -0.069
f_SD = 0.759 +0.080 -0.078

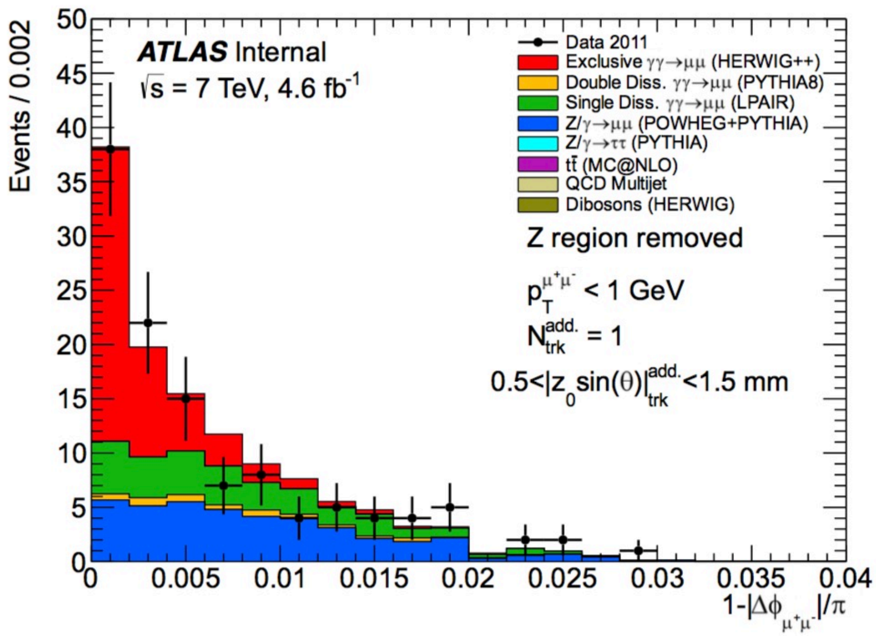
Study systematics by changing fit techniques, varying background …

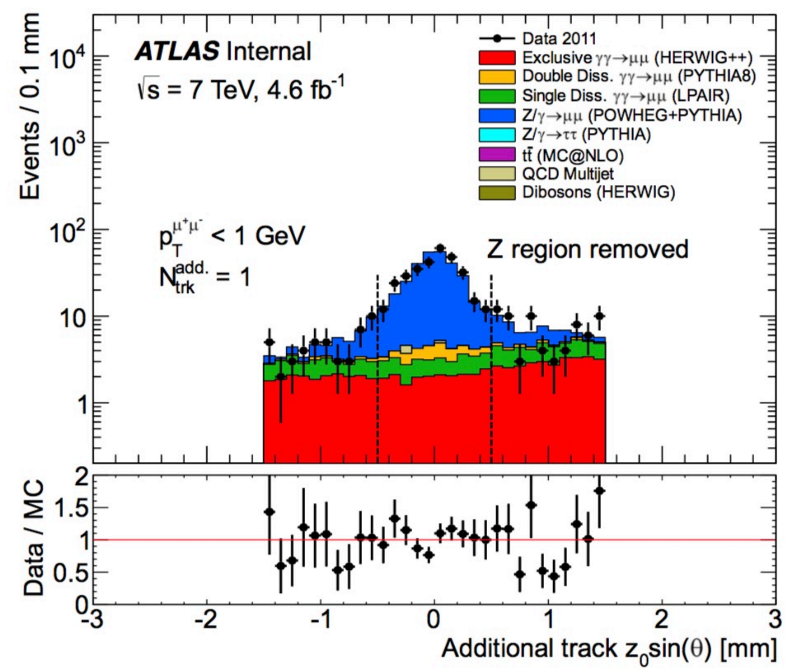




Check of pilup

Require exactly 1 extra track, |dz| 0.5-1.5 mm, pT(ll) < 1



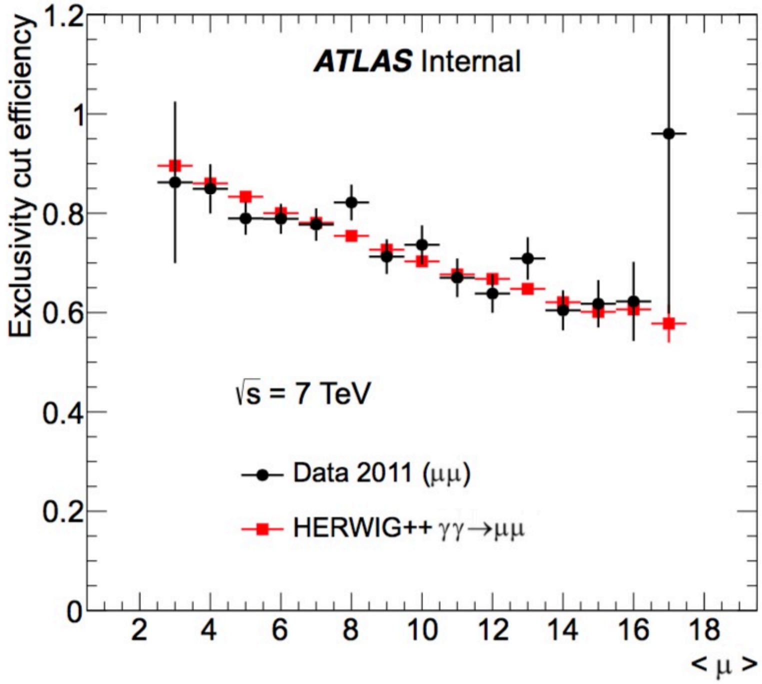


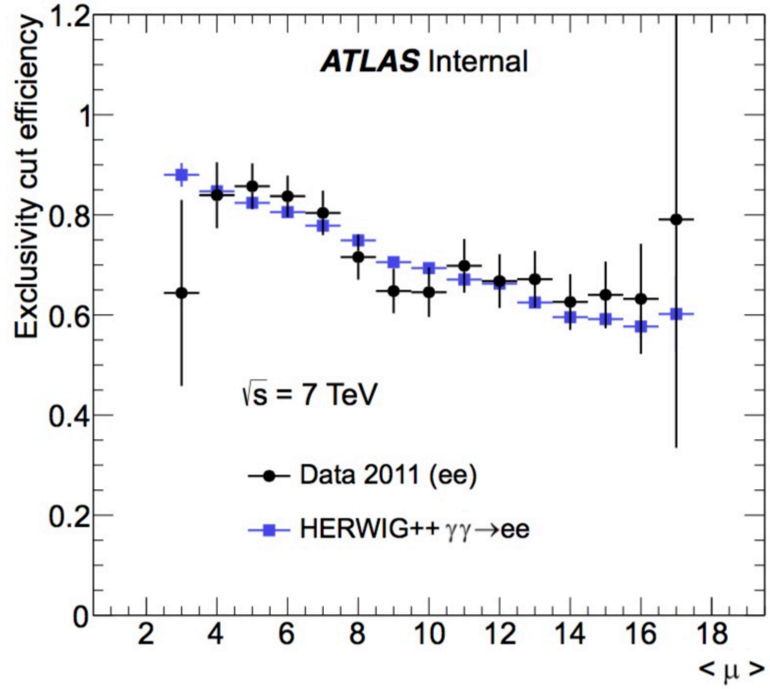
Signal yields agree with expectation, pileup well described

This was a response to a comment on the 7 TeV paper, we made it central for 8.

Efficiency vs μ

(average number of interactions per crossing)





systematics

Source	Variation from nominal yield	
	Muon channel	Electron channel
Muon reconstruction efficiency	0.2%	-
Muon momentum scale and resolution	0.5%	-
Muon trigger efficiency	0.6%	-
Electron reconstruction and ID efficiency	-	1.9%
Electron energy scale and smearing	-	1.4%
Electron trigger efficiency	-	0.7%
Exclusive veto efficiency and pile-up description	1.4%	1.4%
Backgrounds	2.0%	2.3%
Lepton angular resolution	0.2%	0.3%
Beam crossing angle	0.3%	0.3%
Fit stability	0.9%	0.9%
Luminosity	1.8%	1.8%
Total systematic uncertainty	3.3%	4.2%
Statistical uncertainty	4.8%	8.4%

Interpretation*

M. Dyndal and L. Shoeffel, PLB 741 (2015). "The role of finite-size effect on the spectrum of equivalent photons on photon-photon collisions at the LHC"

For the kinematics of interest here, expect proton survival factor of $S^2\gamma\gamma$ ~0.8.
Can build this into the prediction. "Starlight"

Needless to say Koze et al. (Durham) have a different interpretation to get a similar number. "Superchic"

Also studies with photon PDF formulations

* Definitely not mine! I happily depend on the kindness of strangers. And attempt to avoid religious conflicts.

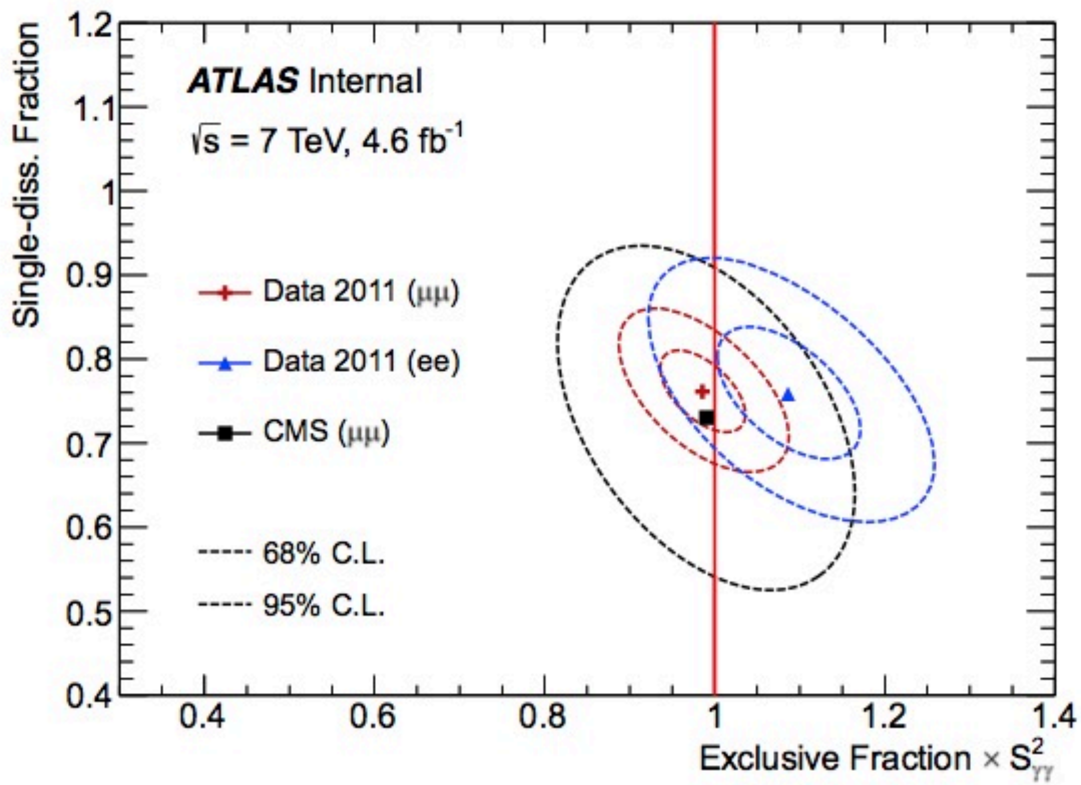
Dilepton bottom line @7

Exclusive μμ cross section
0.628 ±0.031 ±0,021 pb

Theory with survival
0.636 pb

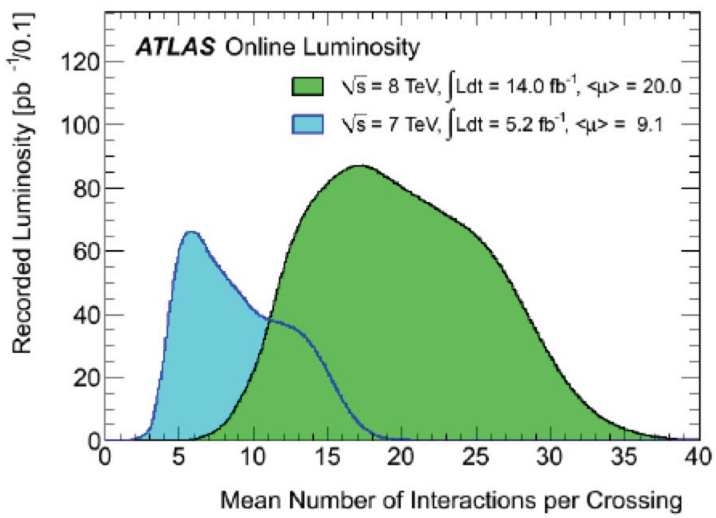
Exclusive ee
0.428 ±0.34 ±0.19 pb

Theory with survival
0.397 pb

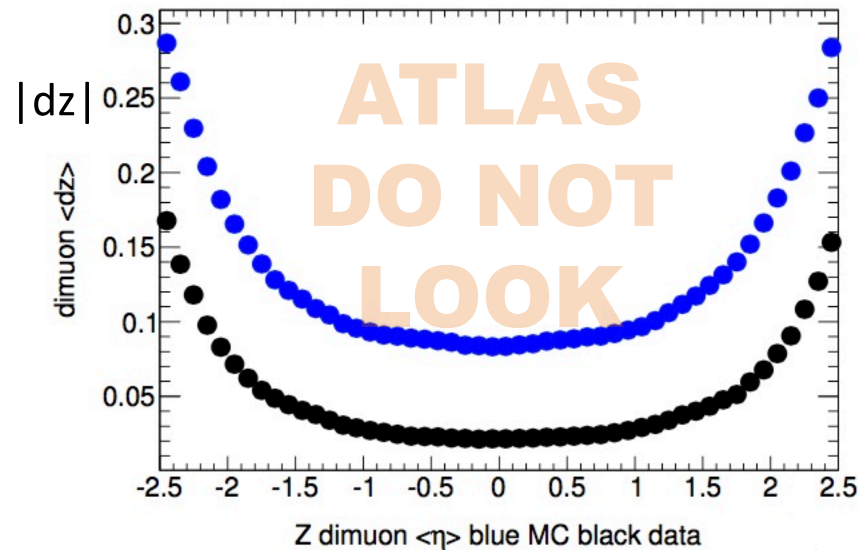


Presumbly SD has some factor as well but …

So far old published results now recent publication 7->8



Pileup goes from blue to green

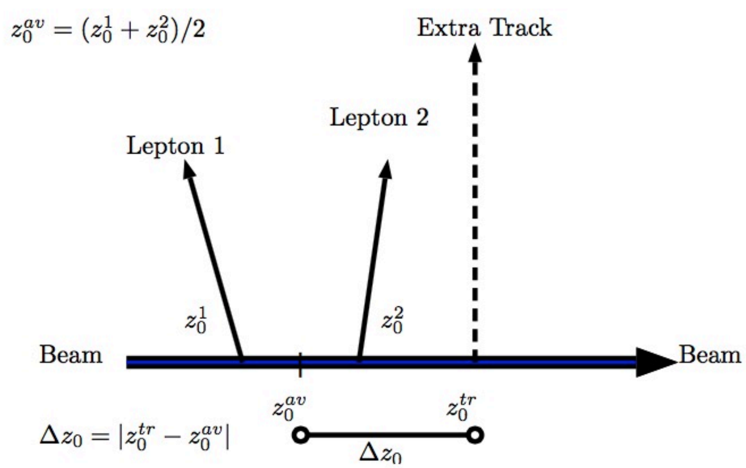


eta

Approved plots, showing how bad the simulation is, are not allowed!

Simulation of tracker z view accuracy is hopelessly pessimistic in 8 TeV data: need to avoid official vertexing, "hang loose"

➔Roll your own vertex

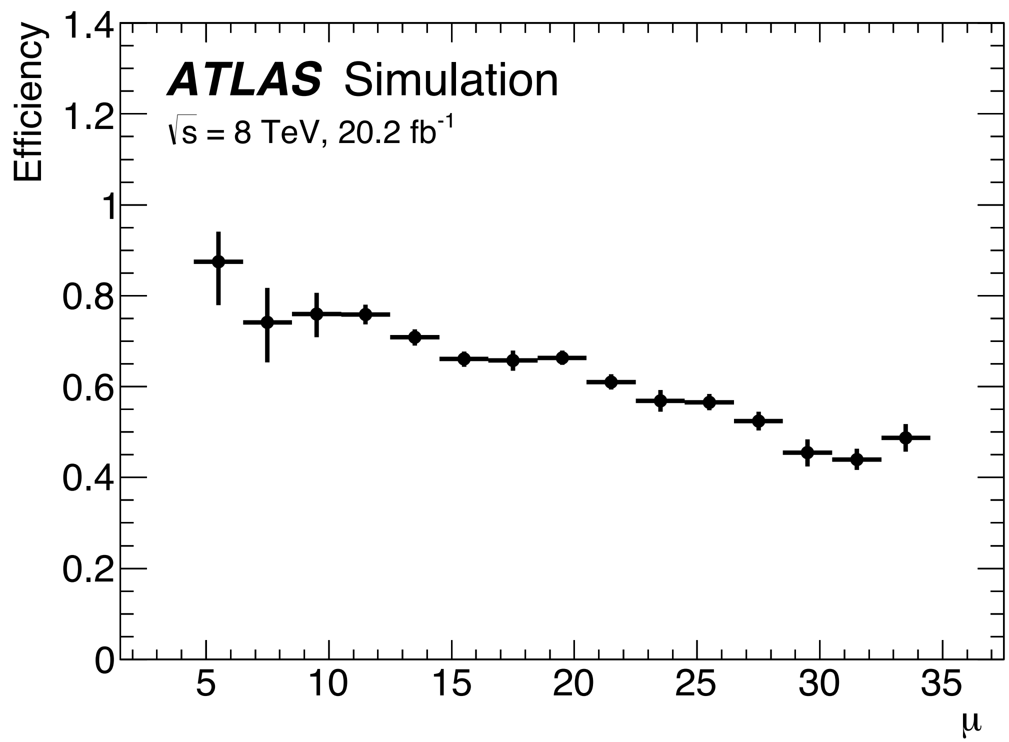


Project track to beamline – z at closest approach
2 leptons (with good tracks) are within 1 mm of each other
(100% even in simulation)

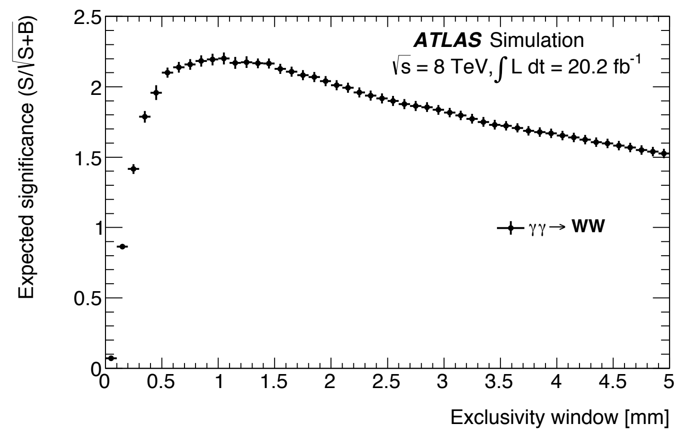
Count unmatched "extra" tracks in window
(z1+z2)/2 ±1 mm   tracks with enough hits to be reliable

Recovers loss from overenthusiastic vertex code

Signal "Vertex" efficiency

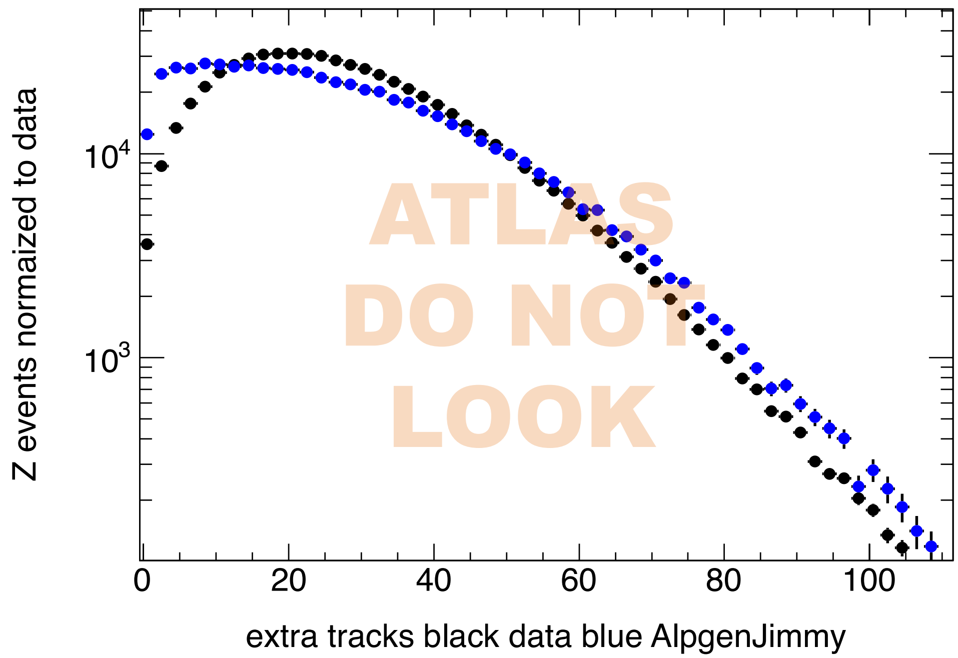


1 mm window keeps 58 ±6% of signal
Could not go tighter cause simulation is pessimistic, would be significant



Window too narrow, background gets in, window too wide, random vetoes
Cut 1 mm!

Underlying event simulation



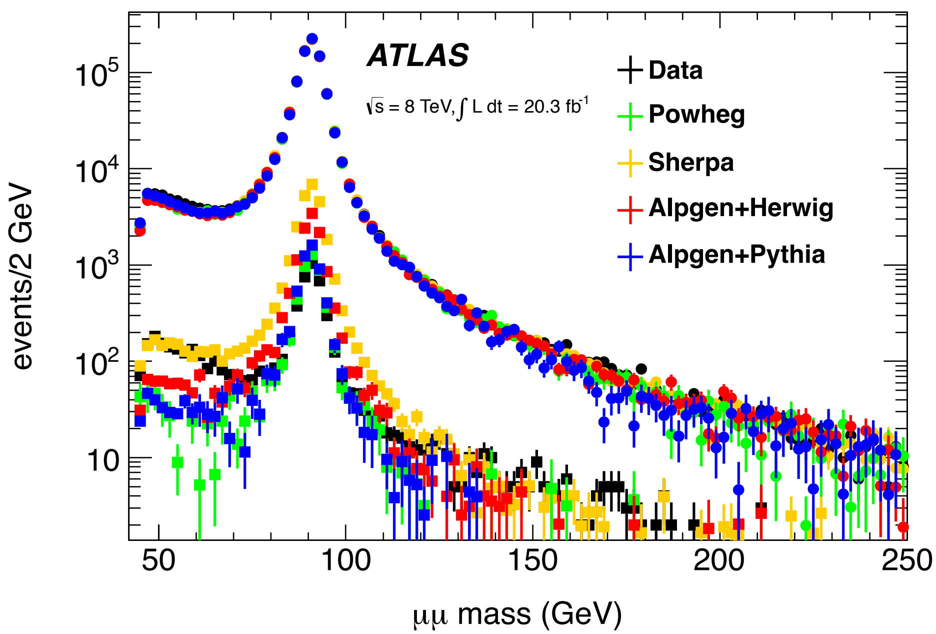
This and Sherpa are particularly bad, we try to use ones within x2 on zeroes!

Background event multiplicity tuning goes from poor to ridiculous, not even smooth variation

BKG reject: Calibrate with Zs

Note original notation
AlpJimmy->paper Alp+H

Sideband subtract to isolate physics (Z), measure Data/MC



"Exclusivity" (n_{trk} < dz)	Powheg	Sherpa	AlpgenJimmy	AlpgenPythia
0 < 1.0 mm	0.581	0.128	0.206	0.692
0 < 1.25 mm	0.549	0.113	0.194	0.679
0 < 1.5 mm	0.537	0.103	0.189	0.663
0 < 2.5 mm	0.494	0.084	0.176	0.613
0 < 4.0 mm	0.308	0.074	0.170	0.579
1-4 < 1.5 mm	0.837	0.518	0.355	0.819
1 < 1.5 mm	0.681	0.324	0.247	0.736

Close to 1 would be nice

Normalized ratios to Sherpa

Mass [GeV]	ALPGEN+HERWIG	ALPGEN+PYTHIA6	POWHEG+PYTHIA8
44–60	0.81 ± 0.02	0.84 ± 0.03	0.99 ± 0.09
60–90	1.04 ± 0.02	0.98 ± 0.03	1.01 ± 0.02
90–116	1.00 ± 0.01	1.02 ± 0.02	1.00 ± 0.02
116–200	0.89 ± 0.10	1.04 ± 0.19	0.76 ± 0.10

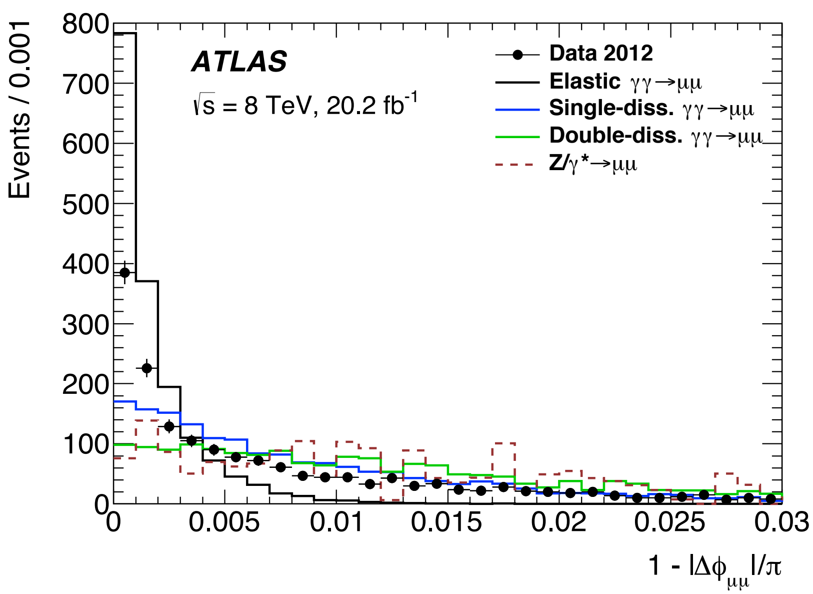
Need to extrapolate from m(Z)
MC line shapes after exclusive
±20 %

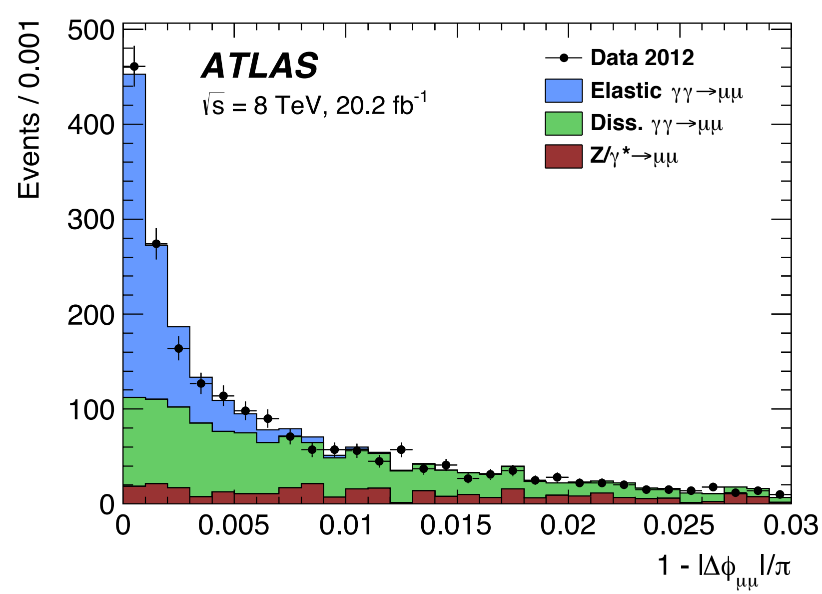
All the accepted MC Z events are from the 0 jet bin

Dilepton sanity check

20/20 threshold – start mass at 45 (7 TeV started 20)
Did not expect competitive measurement
Higher P_T means looser $P_T(ll)$ 1.5 becomes 3 or sometimes even 5 GeV

↓ Cut later



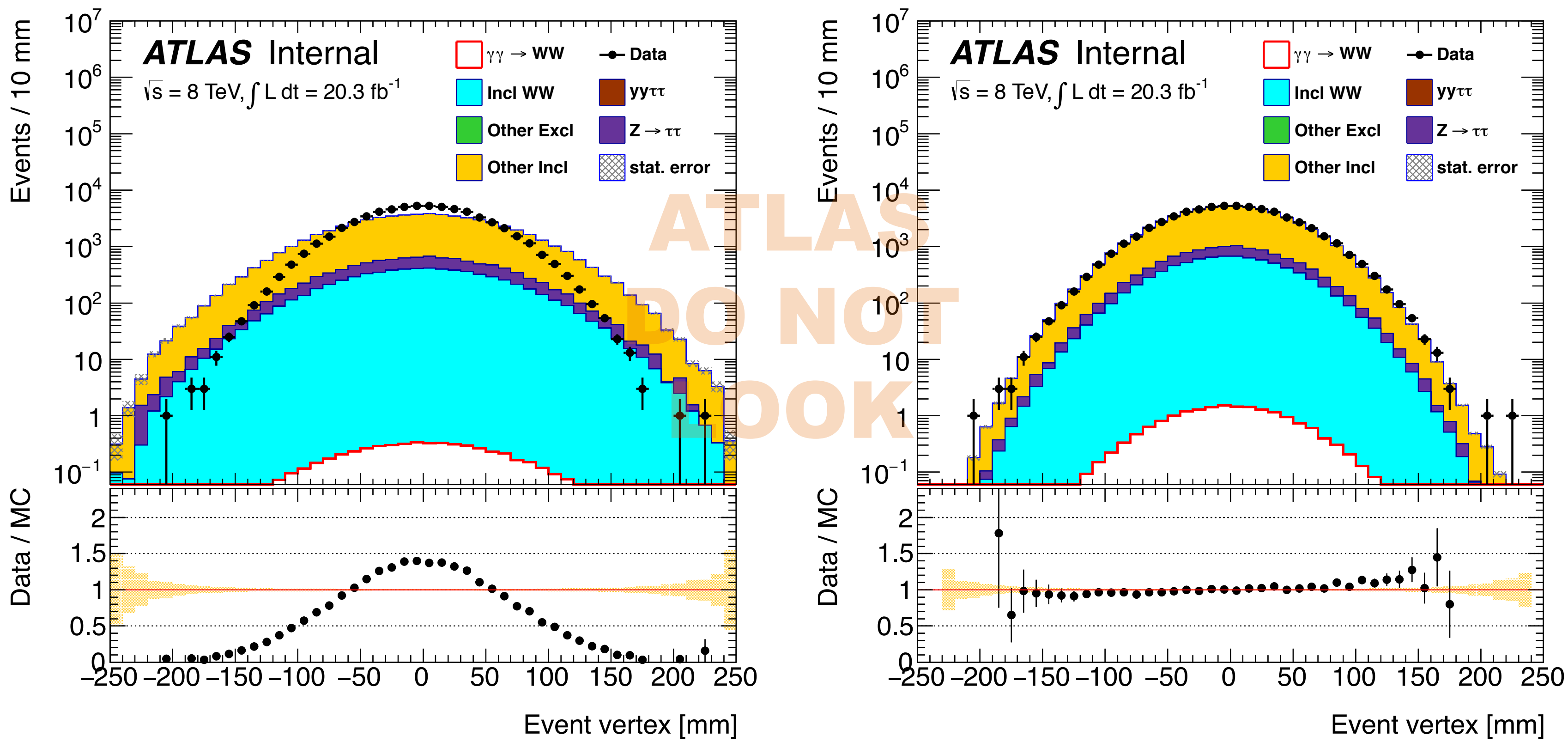


The shapes: coherent, SD, DD, background
(DD and background are similar)

2 shape fit: coherent, SD+DD,
background given (±20%)

Alternate strategy, use elastic, SD, and (DD+background)

Oops forgot something



Long laundry list of correction weighting but forgot shape of vertex distribution, more pileup in the middle ~ 10% worse exclusivity!

Dilepton numbers

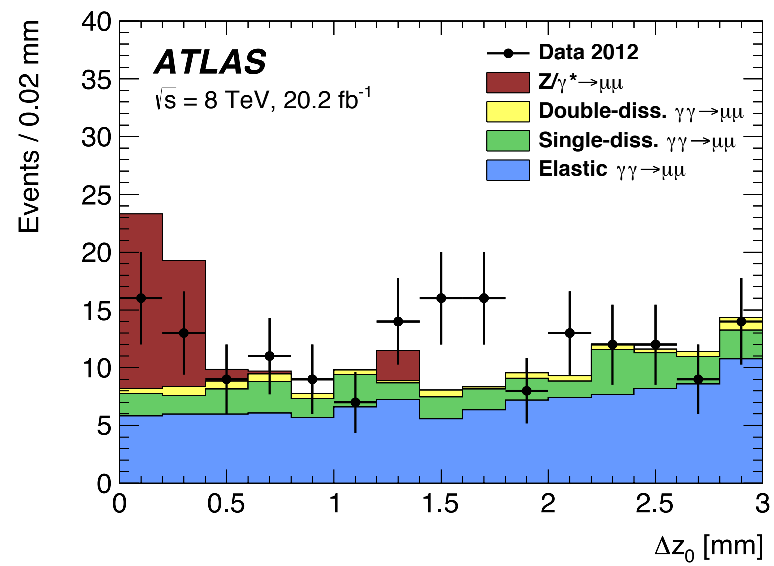
Boatload of fits, use spread to cover shape systematics ±0.07 dwarfs all other
Data/elastic Herwig++ = 0.76 ±0.04 ±0.07 expect 0.73-0.75 “proton size”

Cut and count **check pileup efficiency modeling**
1.0 mm window, $P_T(ll)<3$, acoplanarity < 0.0015 (as marked, mostly signal!)
No extra track
Data 607
QED 568 + 177 SD +33 DD = 778
DY Powheg scaled 41
(Data-DY)/ΣQED = 0.73 ±0.03 ±0.01

Demand one extra track within 3 mm
(Data-DY)/ΣQED = 0.70 ±0.07 ±0.06
511 data within 3, 217 within 1 mm

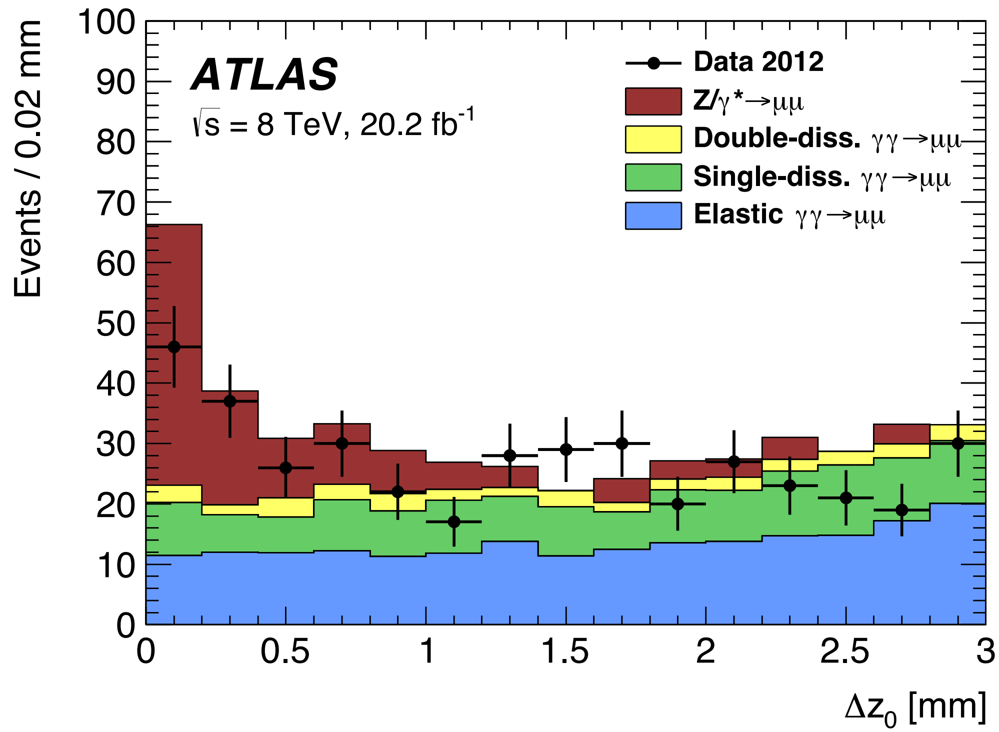
Not sure what it means but 0 and 1 agree!
Check ee: consistent

Theory free pileup efficiency to 10%



Dz signal extra track us flat (pileup!)

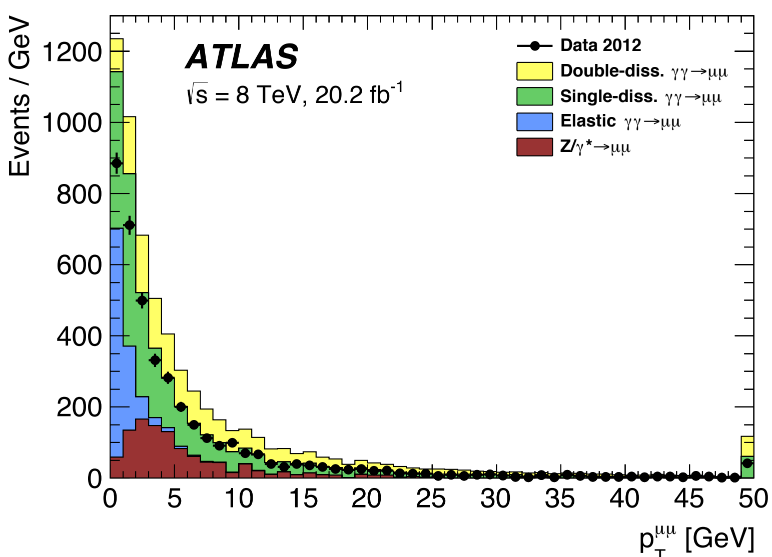
Prettier pic



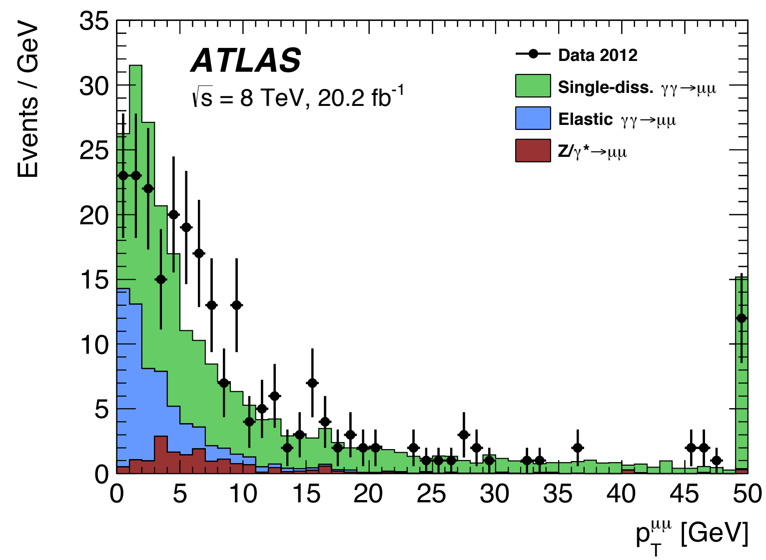
Loosen up acoplanarity cut to >0.006, let's see some DY!

WW has no acoplanarity!

Use eμ (+νν..) for WW, no way to distinguish coherent elastic vs SD vs DD
Generators calculate elastic expected
Use μμ m>160 to estimate total/elastic EPA expectation



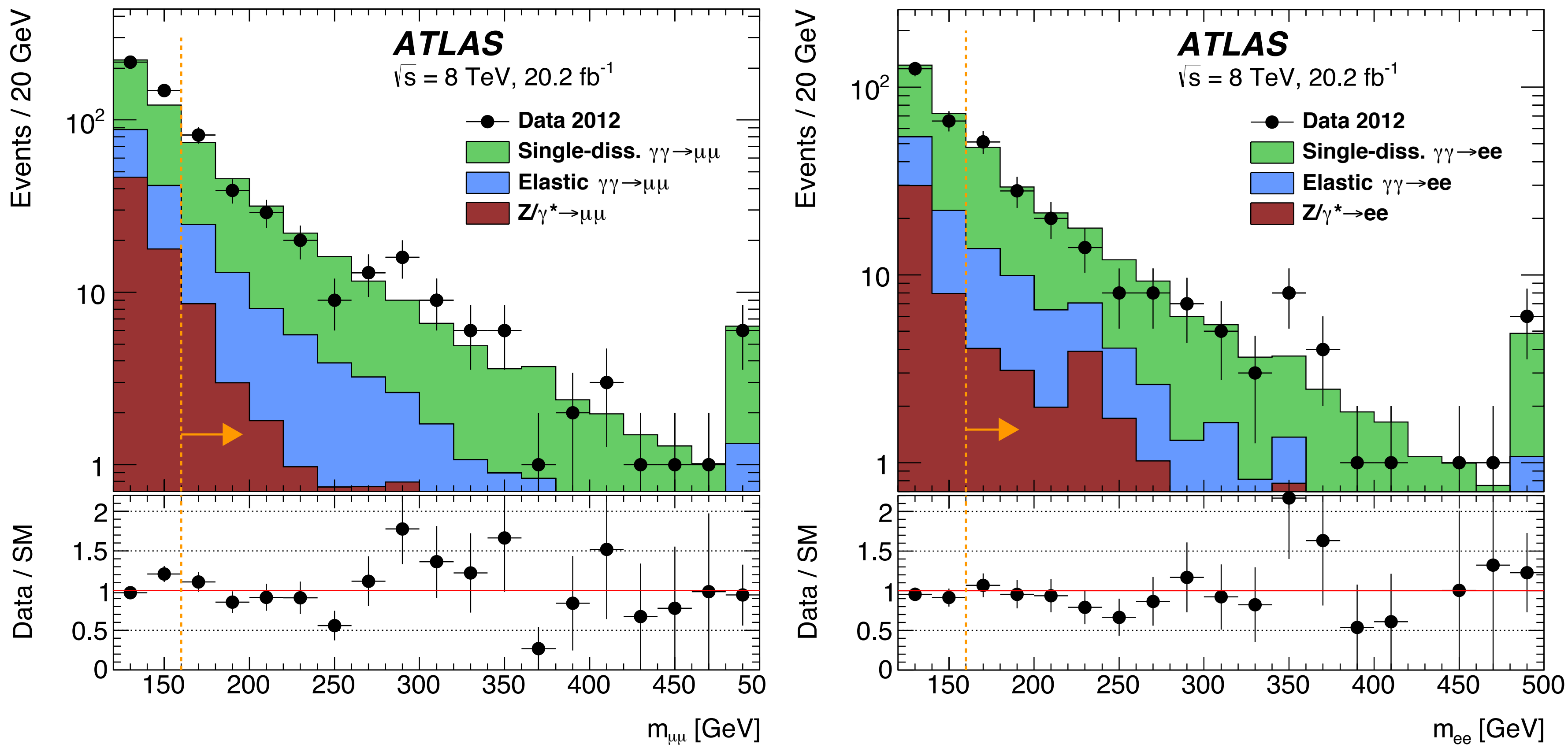
Low mass: Cut <3-5 to lose DY
Can isolate elastics
m > 45 no Z



No cut eμ, so no cut μμ, (m >160 =2 mW)
Sorry, got elastic +SD +DD, not much DY!

μμ P_T distributions low and high mass

Define ratio of m>160 observed to EPA prediction (to account Diss.)

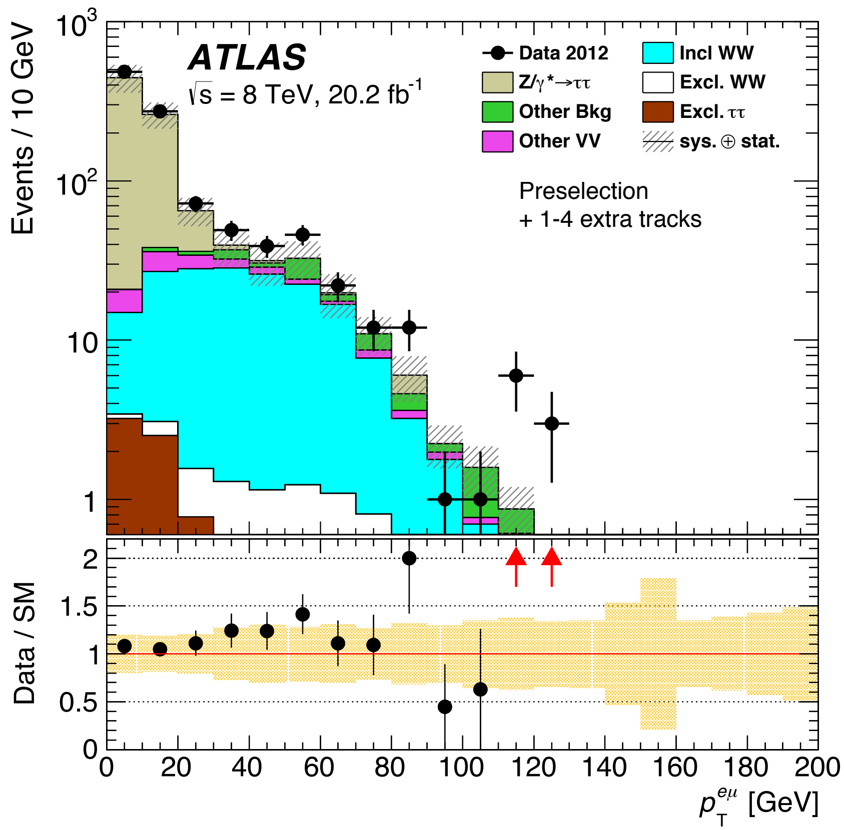


Normalize to muons, check electrons - DY & elastic given, SD scaled for XN

Background subtracted muons/elastic prediction = 3.30 ±0.22 ±0.06

Koze et al. actually predict this! 1601.03772 CMS 8 TeV 4.13 ±0.43

Background: Control regions



Processes	$Z \to \tau\tau$	Inclusive WW
$\gamma\gamma \to WW$	0.29 ± 0.024	5.23 ± 0.37
$\gamma\gamma \to \tau\tau$	4.55 ± 0.72	1.45 ± 0.24
Inclusive WW	13.2 ± 0.63	66.4 ± 1.5
Other diboson	2.18 ± 0.49	4.01 ± 0.54
$Z(\tau\tau)$+jets	573 ± 28	4.3 ± 2
$Z(ee/\mu\mu)$+jets	0.011 ± 0.011	0 ± 0
Top	0.44 ± 0.44	5.6 ± 1.4
Data	526	132

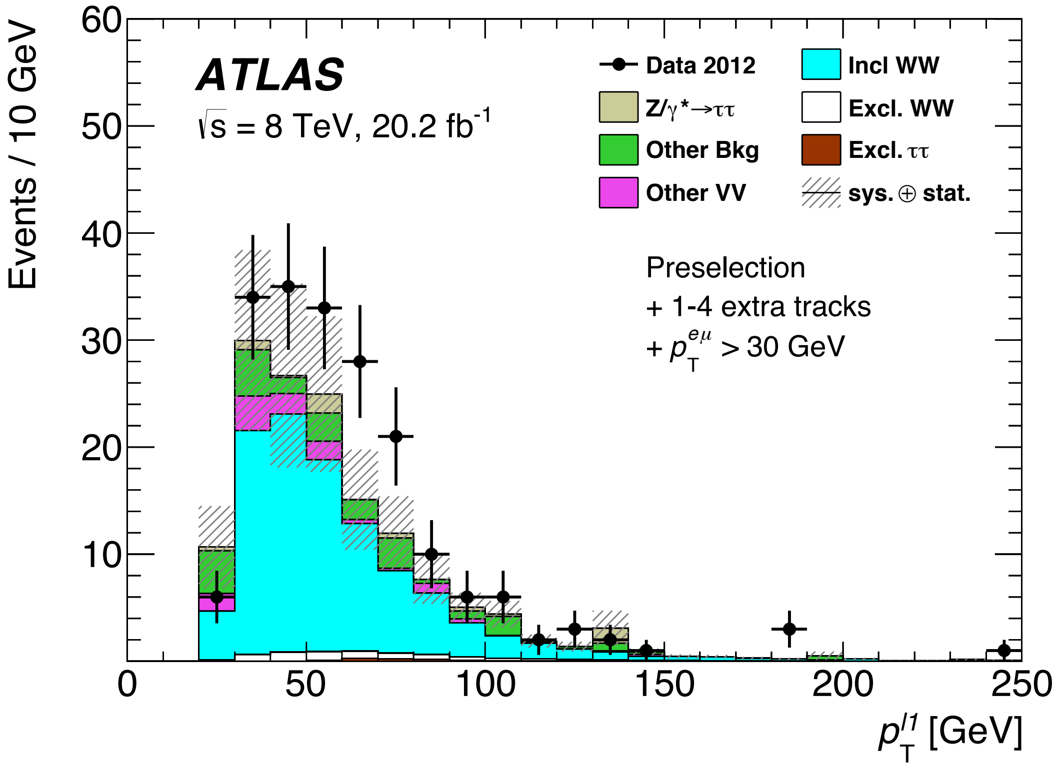
X1.2

Taus in good shape, agrees w Z calibration
WW missing stuff: Wjets, diffractive WW
Can take big error to cover

Low P_T(ll) & kinematics tau tau
High P_T(ll) WW

1-4 extra tracks instead of 0

What is the WW control region excess?



Background

WW control	6.6 ±2.5
Tau tau	1.4 ±0.3
Other-VV	0.3 ±0.2

Includes 20% twice and difference/2, "can live with it"

We have an extrapolation from Zs to understand rejecting inclusive WW, but if you allow a few extra tracks all kinds of stuff like top and fakes can get in there (not on our list!) and they should be much easier to reject. So bracket background: extrapolate the whole wad like it is all WW (high end) or assume the excess is completely rejected (low end). Split the difference and toss diff/2 into the uncertainty.

SM WW

	Expected Signal	Data	Total Bkg	Incl W^+W^-	Excl. $\tau\tau$	Other-VV	Other Bkg	SM/Data	ϵA (Signal)
Preselection	22.6 ± 1.9	99424	97877	11443	21.4	1385	85029	0.98	0.254
$p_T^{\ell\ell} > 30$ GeV	17.6 ± 1.5	63329	63023	8072	4.30	896.3	54051	1.00	0.198
Δz_0^{iso} requirement	9.3 ± 1.2	23	8.3 ± 2.6	6.6 ± 2.5	1.4 ± 0.3	0.3 ± 0.2	–	0.77	0.105 ± 0.012
aQGC signal region									
$p_T^{\ell\ell} > 120$ GeV	0.37 ± 0.04	1	0.37 ± 0.13	0.32 ± 0.12	0.05 ± 0.03	0	–	0.74	0.0042 ± 0.0005

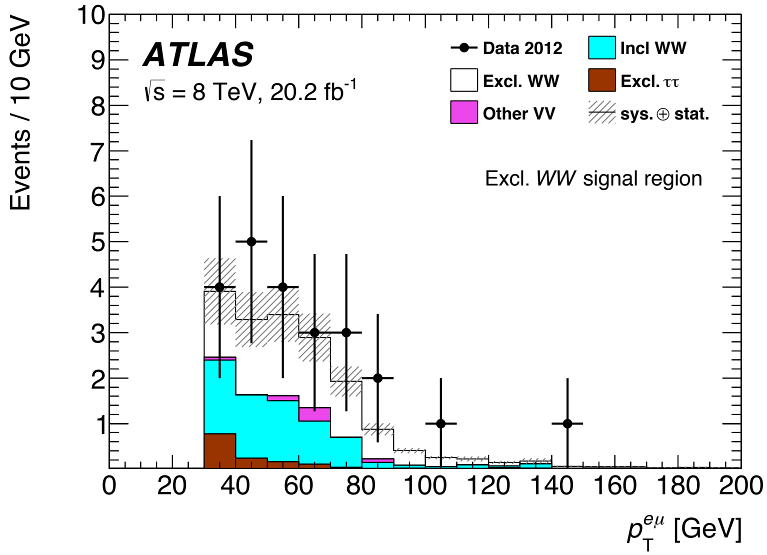
Expect 9 signal + 8 bkg = 17, see 23 P(background only) gives 3 sigma evidence for SM ϒϒ➔WW. CMS 7+8 get 3.4 sigma "seen?"

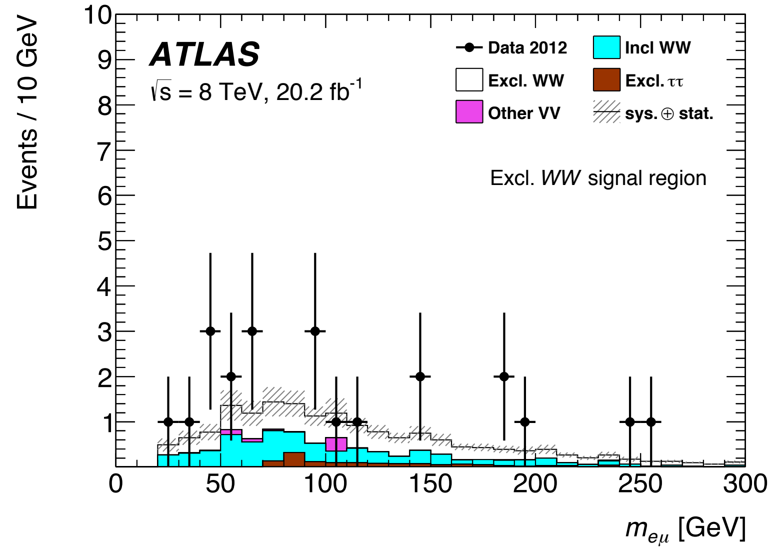
So without using any theory normalization efficiencies, correcting EPA to include SD & DD, we predict σB eμ of 4.4 ±0.3 fb and observe 6.9 ±2.2 ±1.4 fb (includes intermediate taus)

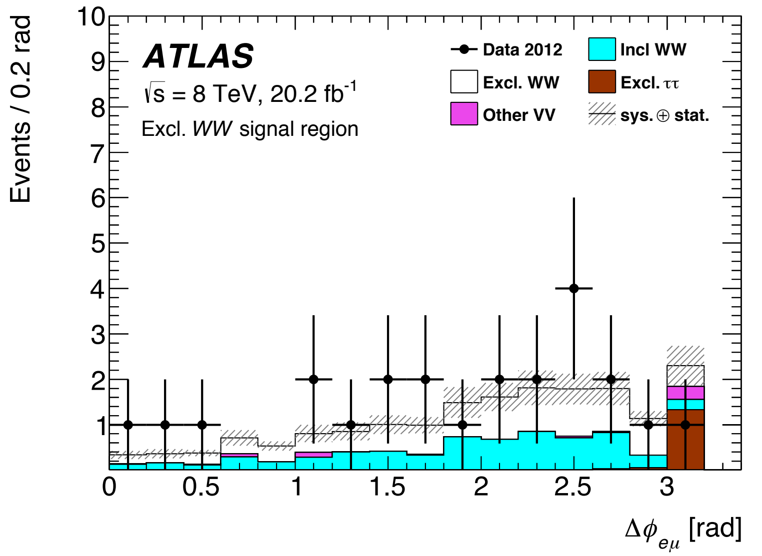
CMS on the other hand, normalizes efficiency to EPA and predicts 6.9 ±0.6 fb and observes 11.1 +5.6 -4.5 fb.

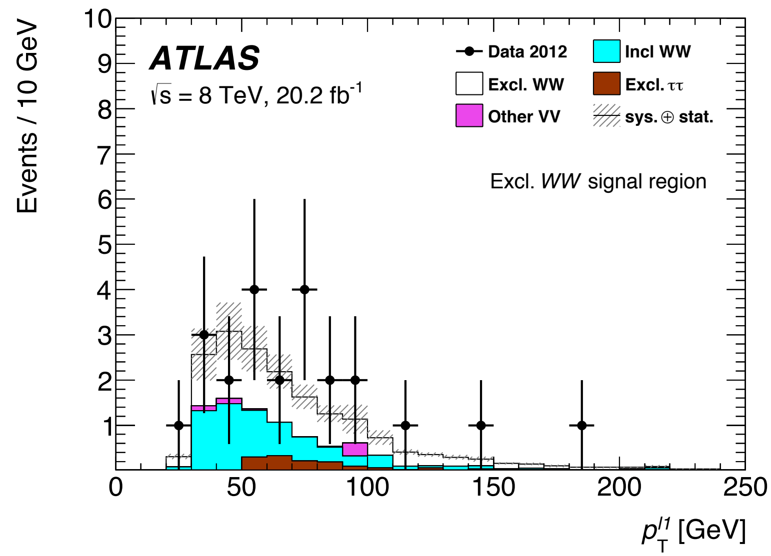
Apples to apples, the numbers agree well

Looks like a duck

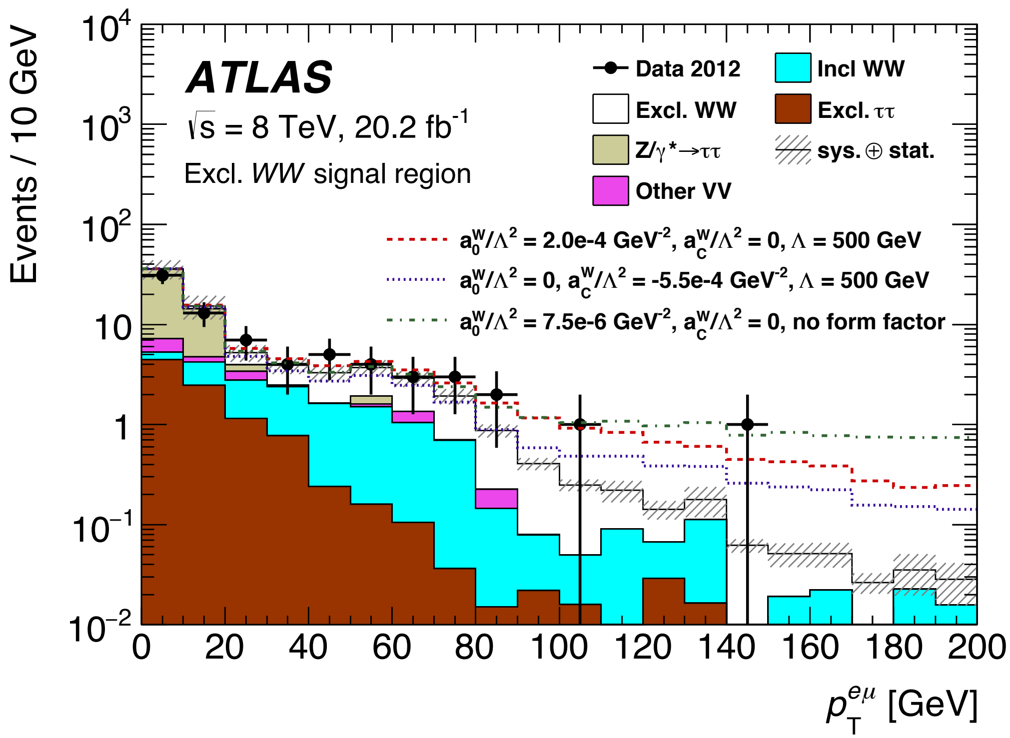








Not finding … aQGCs



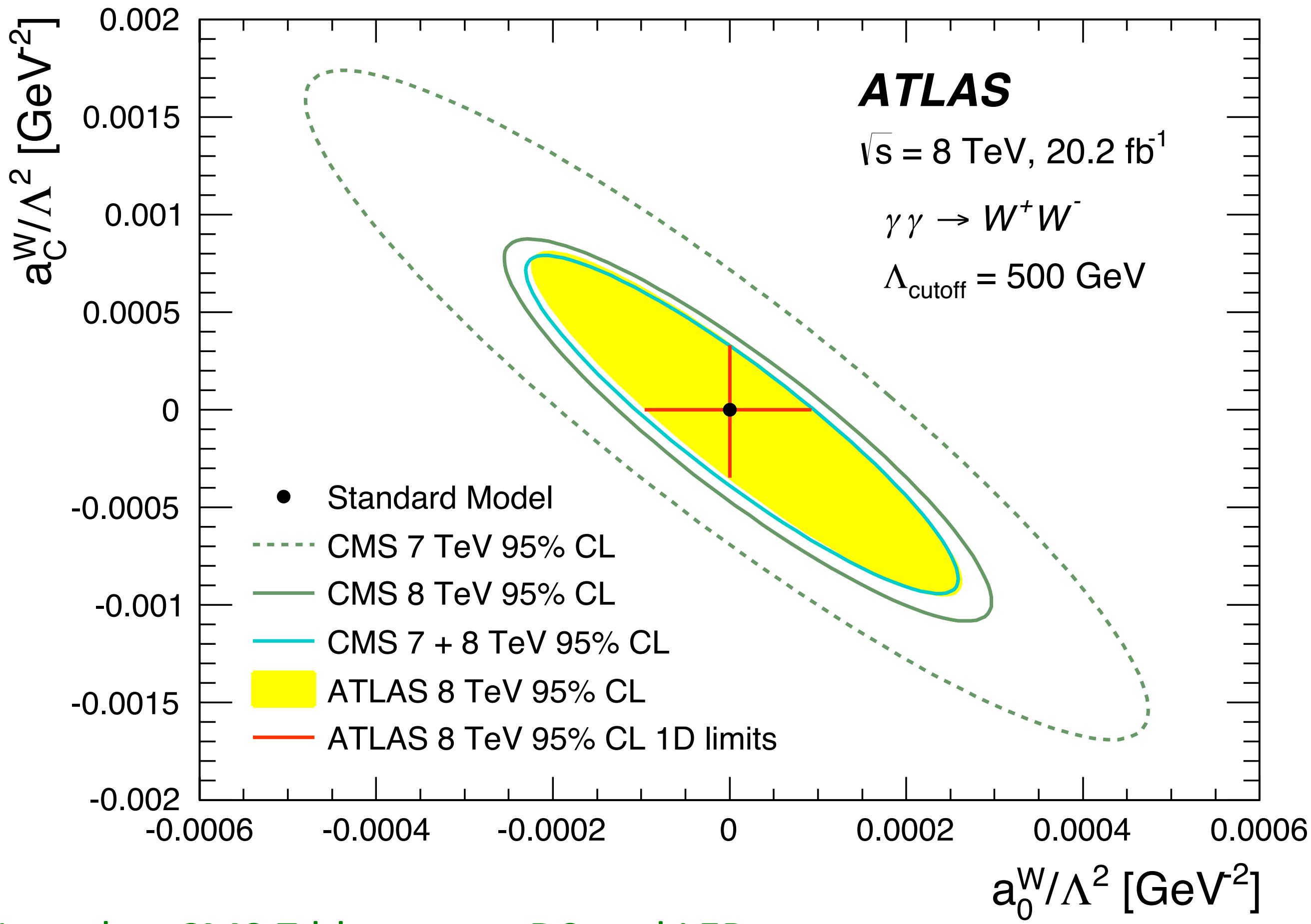
aQGCs give high pT
We have 1 >120
Expect 0.3+0.3
Dimension 6?
Dimension 8?
Dipole cutoff?
1D, 2D?

Whatever

As long as we can compare

Hey, this is an LHC talk, you expected something funny?

Compare to CMS (CalcHEP vs ?)



Note that CMS 7 blew away D0 and LEP

ATLAS 8 ~ CMS 7+8, we can not find things ok

Dim 6, cutoff 500 GeV

Diffractive Higgs Production

In principle, Higgs Bosons could be produced diffractively.

If you could observe them, they would be "clean" ~0 p_T – good for systematic studies

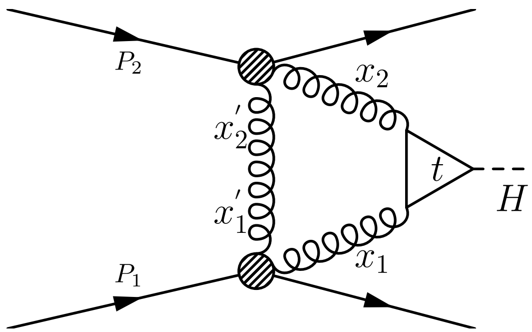
Pileup makes it harder but not impossible

Turns out low rate makes it not so useful, interesting stuff has low BR

Mike Albrow has been pushing this

Can't hurt to look! Perhaps find out possibility

Any diffractive H in there?



H ➔ WW* ➔μe not too bad

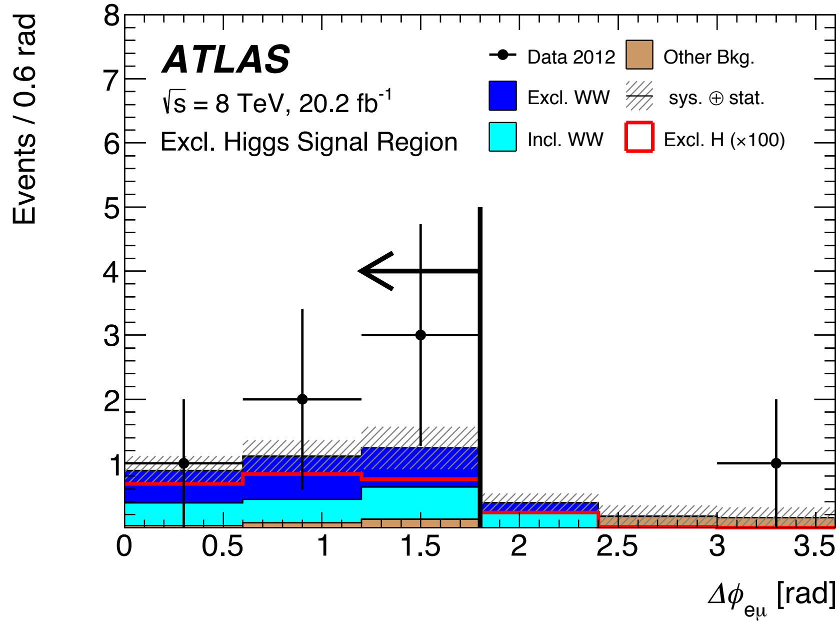
Lepton from W* too soft
Lower second lepton threshold 20 ➔15
More background
Oh well

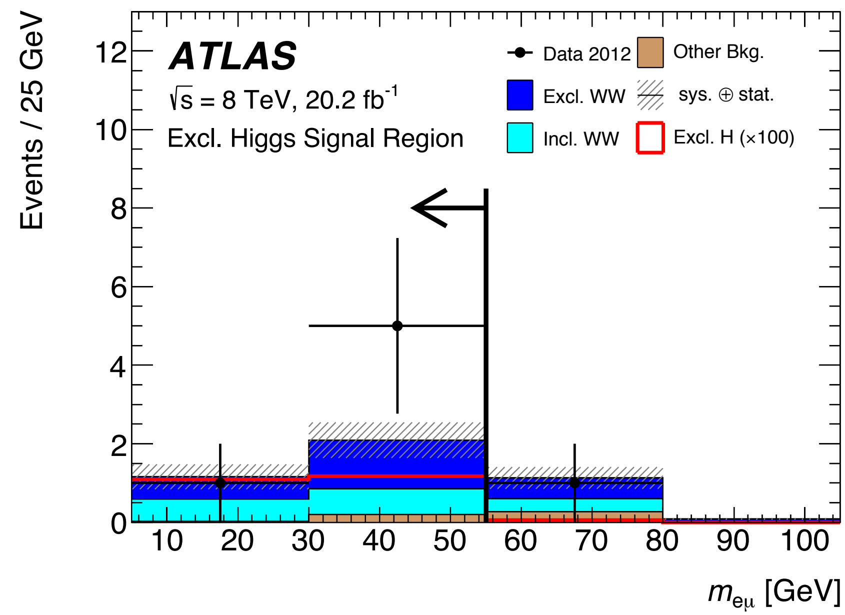
Note: no pomerons allowed!

	Excl. H Signal	Data	Total Bkg	Incl. W^+W^-	Excl. W^+W^-	Other Bkg
Preselection	0.065 ± 0.005	129018	120090	12844	43	107200
$p_T^{e\mu}$ >30 GeV, $m_{e\mu} < 55$ GeV, $\Delta\phi_{e\mu} < 1.8$	0.043 ± 0.004	18568	17060	2026	5.7	15030
Δz_0^{iso} requirement	0.023 ± 0.003	8	4.7 ± 1.3	1.4 ± 0.5	3.1 ± 1.3	0.2 ± 0.1
$m_T < 140$ GeV [Signal Region]	0.023 ± 0.003	6	3.0 ± 0.8	1.0 ± 0.4	1.8 ± 0.8	0.2 ± 0.1

Note that signal is from KMR (Durham) elastic,
probably low by x 10 (0.2 would be less unreal)
Expect 3, see 6: limit σB 1.2 pb, expected limit 0.7 pb Guess not.

No claim to see anything





Higgs specific selections

Some possible confusions

Exclusive and inclusive WW are serious backgrounds for H, but H is a negligible background for WW

Diffractive WW is found to be negligible, but with not so reliable generators.

The ATLAS Higgs group "HSG3" (WW) threw us out -> SM EWK

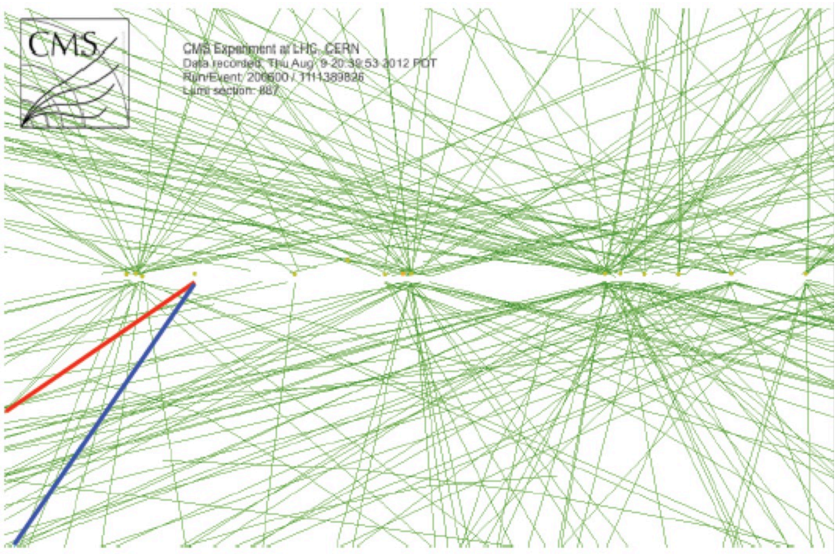
The EWK group had us report to soft QCD, the diffraction and exclusive dilepton types are there

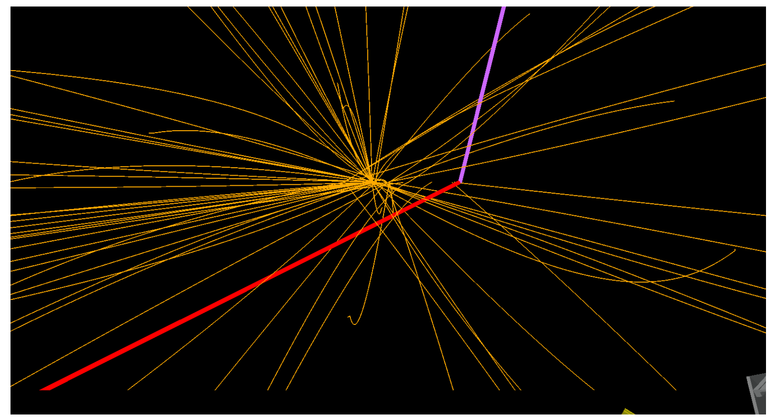
We used a package FPMC which includes CalcHEP for aQGCs, KMR for diffractive H, and had a version of HERWIG so old that we had to update it to do W polarization, important for H vs WW angular effects. Also no width Ws. Fortunately you can do Herwig++ that way and things agree. Apparently CMS 7+8 did not use the CalcHEP formulation …*

(The ATLAS analysis was cribbed from 7 TeV CMS anyway)

*M. Herndon at Gino Memorial

SM wins again





Not as pretty but this is the
ATLAS guy at 140

This is the deal!

And while we are at it

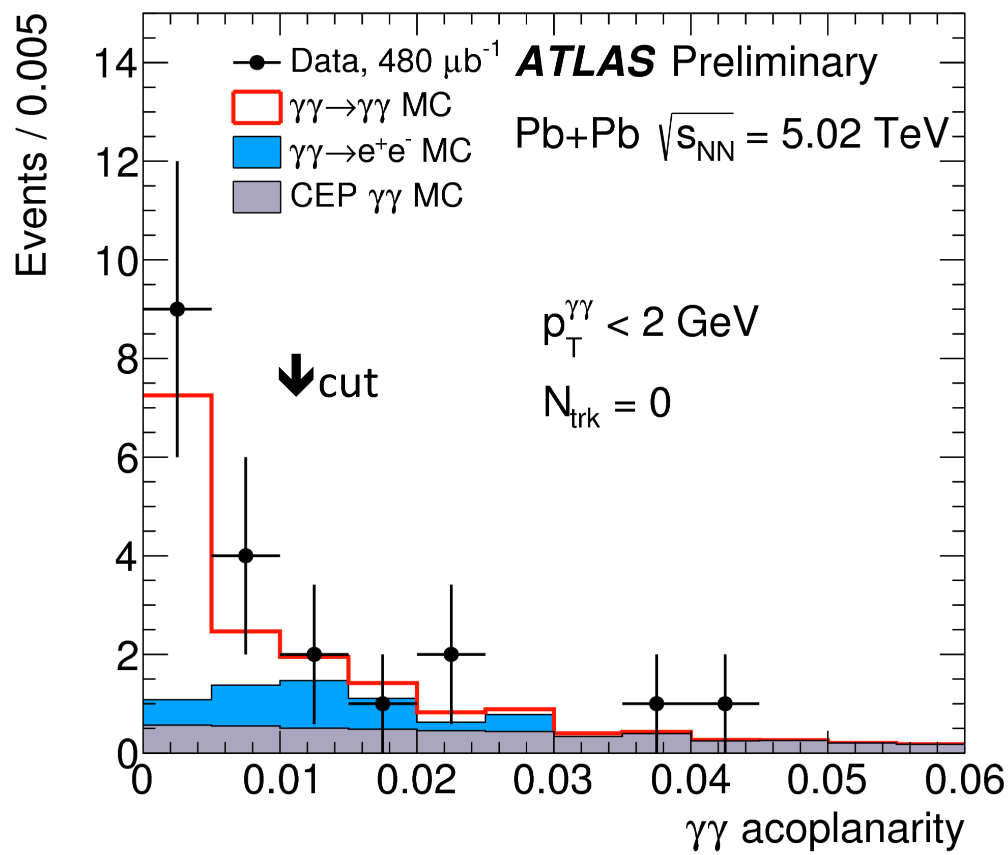
Look for light by light in PbPb

3 GeV photon thresholds
ATLAS, like CMS trouble e vs γ

See 13 events
2.6 ±0.7 background ee final and gg initial states
4.4 σ (3.8 expected)
7.3 events predicted QED EPA

Cute! (no pileup, 1 & 2 track control regions) (more in backup)

Not gonna help g-2

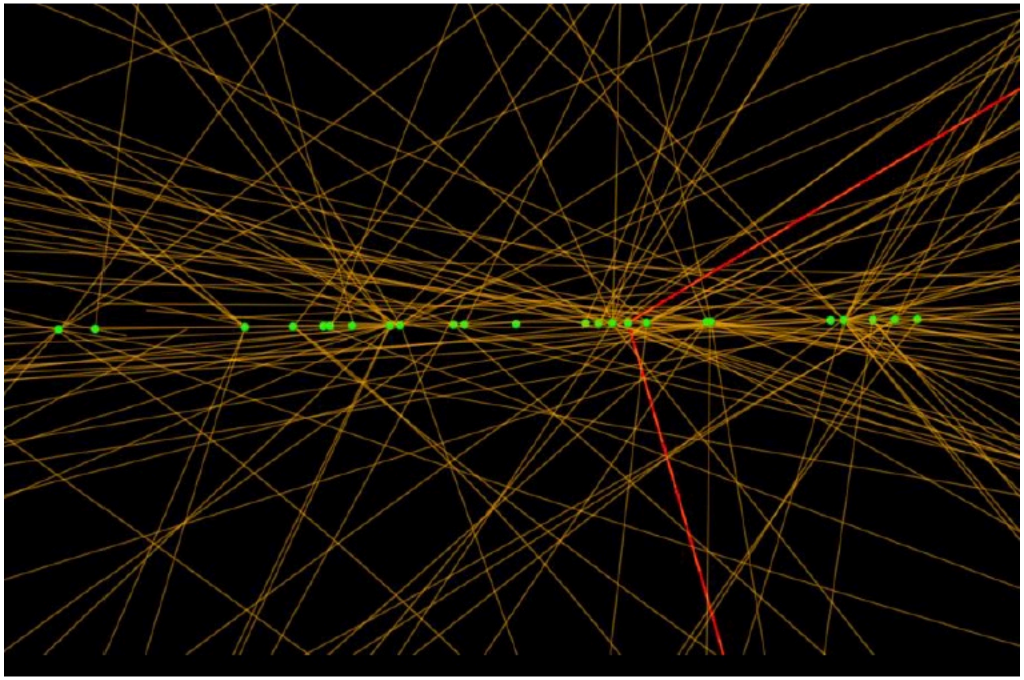


Looks like a duck plots, see
https://atlas.web.cern.ch/Atlas/GROUPS/PHYSICS/CONFNOTES/ATLAS-CONF-2016-111/

13-14 TeV?

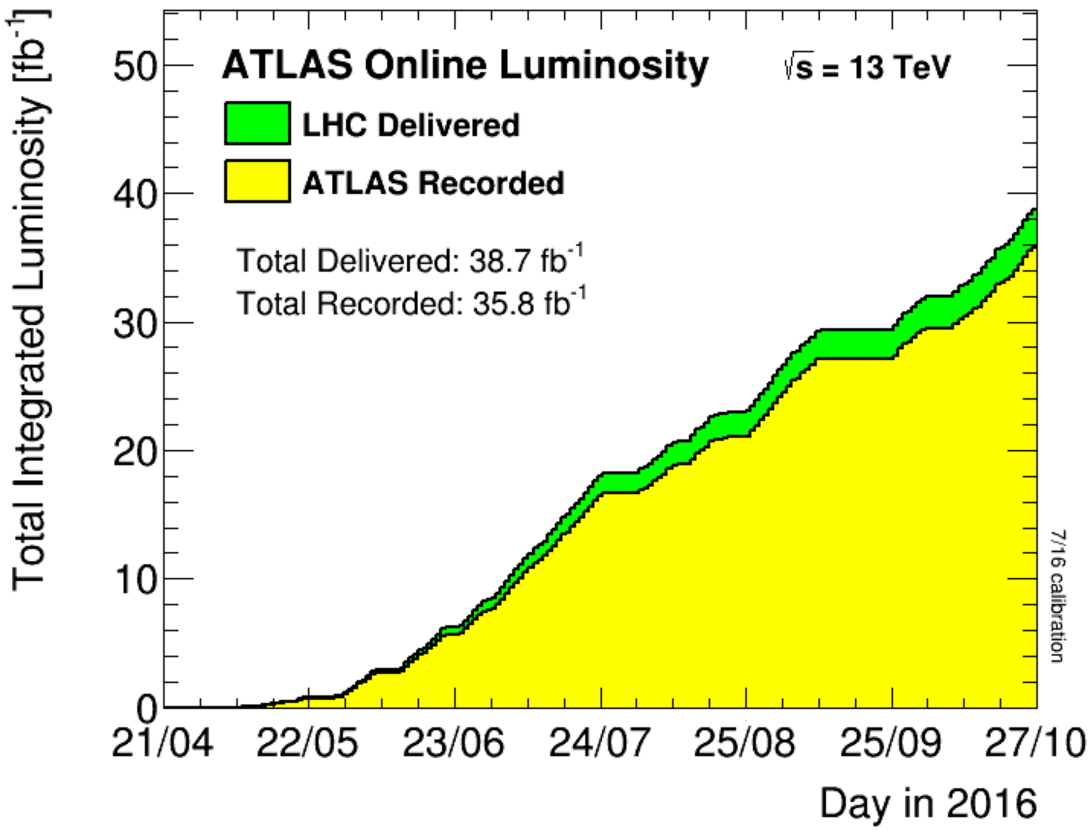
The group that did exclusive dileptons at 7 is working on 13 (2015 data). They find exclusivity as we defined it still works. They are in the soft QCD group and worry about "superchic" (KMR) vs finite size effect corrections "starlight." So far 1 mm ok.

2015 ~3 fb^{-1}



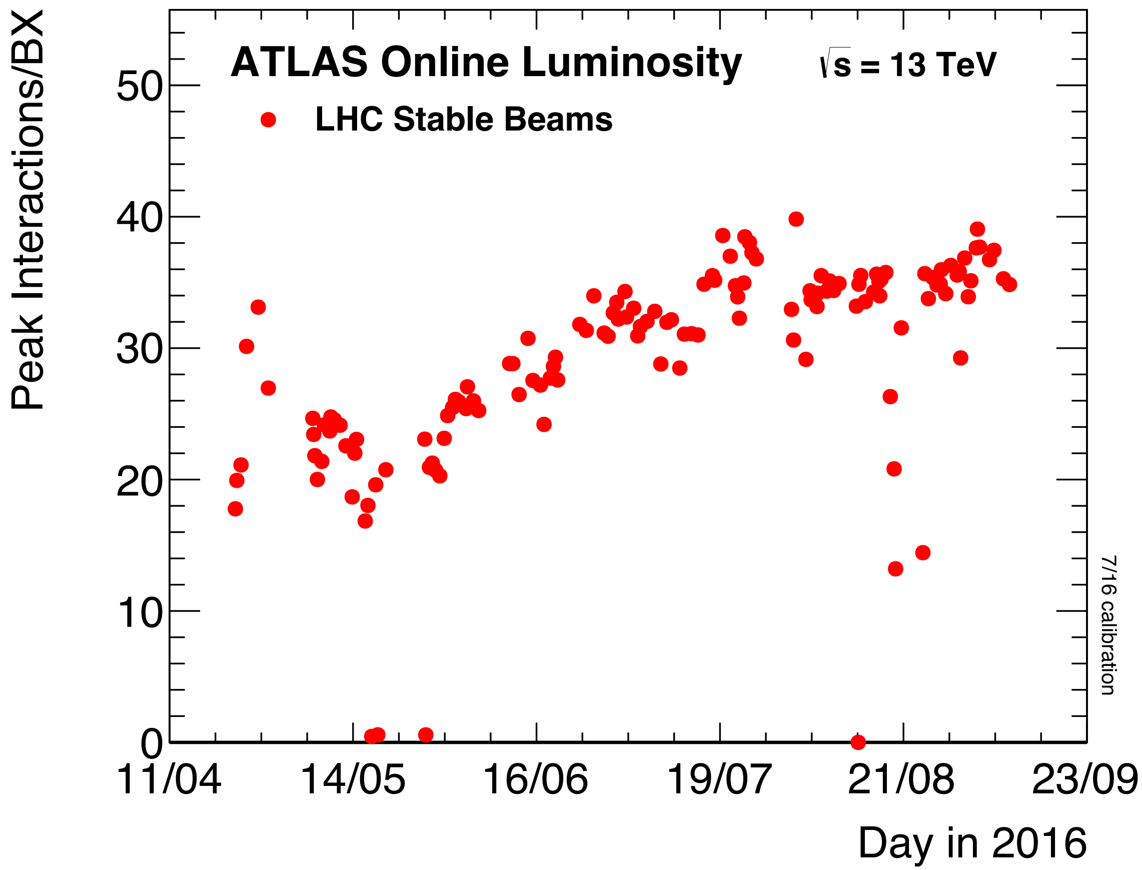
13 TeV exclusive dimuon

2016 data



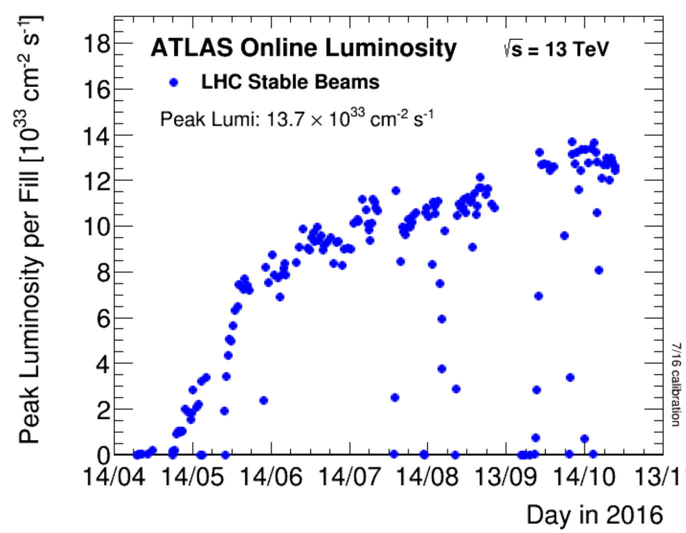
↑ 3 fb^{-1} in 5 days while I was tile DQL

Pileup ok so far



←Highest L still
"on the plot"

A bit worse↓



Not evil in 2016

outlook

Both CMS and ATLAS find evidence for SM γγ → WW with 7/8 TeV data

For dileptons, corrections to EPA work well, more than one path? More than 2? Different form factor formulations, photon PDFs …

Naively, one would think such corrections would suppress WW as well but no sign of that yet, could this be interesting? Not "interesting" yet.

Improved tools like aQGC and diffractive generators are needed and may actually happen!

Scheme still works for 8 TeV data, may need tuneup at highest L, ~ hundred fb^{-1} at 13-14 TeV should make an nice improvement, higher <n> 2017?

Probably impossible at HL-LHC unless you can get <<1 mm - current algorithm apparently only gets down to ~1 mm, so far μ ok …

My comments

My first CDF paper had ~21 events, my first (and last) ATLAS paper has 23
Small numbers are fun!

As I have personally failed to find SUSY on both CDF and ATLAS, it is nice to not find something else (Sunday w.o. nuts)

Working with not so reliable predictions is ok, well defined anyway, 20% problems not too terrible

It is nice to have systematic uncertainties that dwarf "the usual suspects"

"Let's keep dancing" - P. Lee

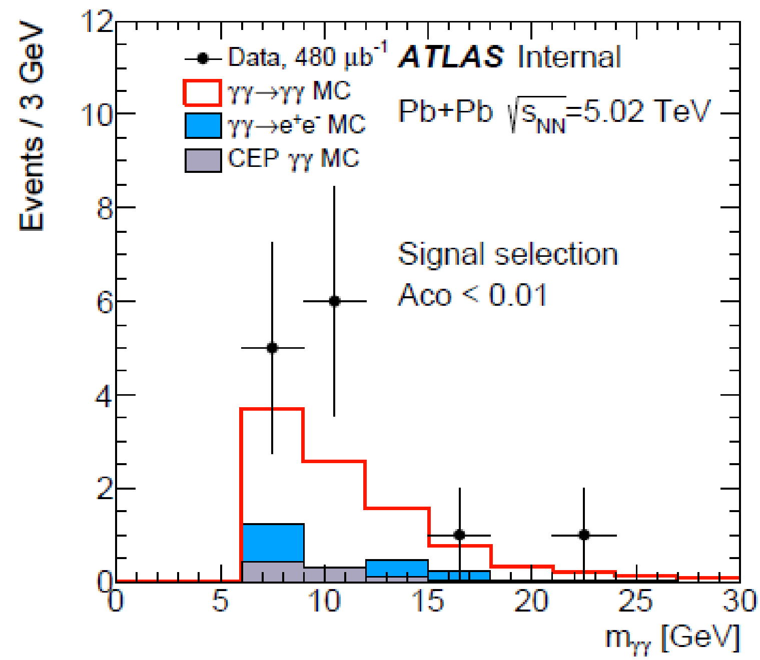
Thanks to CMS for the idea (and the audience)
Thanks Chav and Last! Both recently defended.
Thanks Sergei, Bob, William, Jae, …
Thanks Tokyo comments!
The process of getting a paper out of ATLAS is not good for the soul.

backup

Diphoton Selection

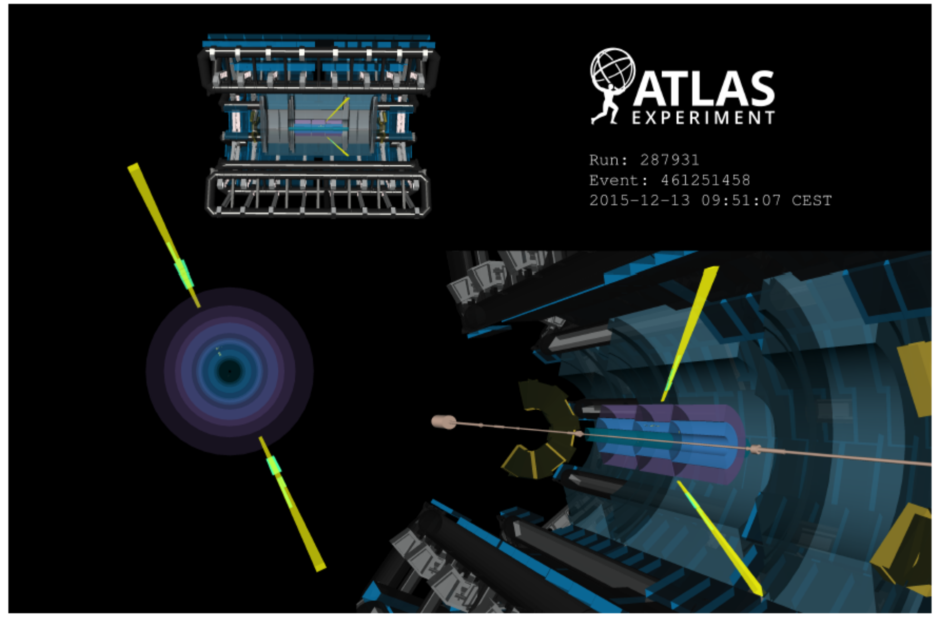
- Two photons (obviously)
- $E_T(\gamma) > 3$ GeV
- $m(\gamma\gamma) > 6$ GeV
- $p_T(\gamma\gamma) < 2$ GeV
- Acoplanarity $(1 - \Delta\phi(\gamma\gamma)/\pi) < 0.01$
  - Identical to $\Delta\phi < 1.8$ degrees from 180

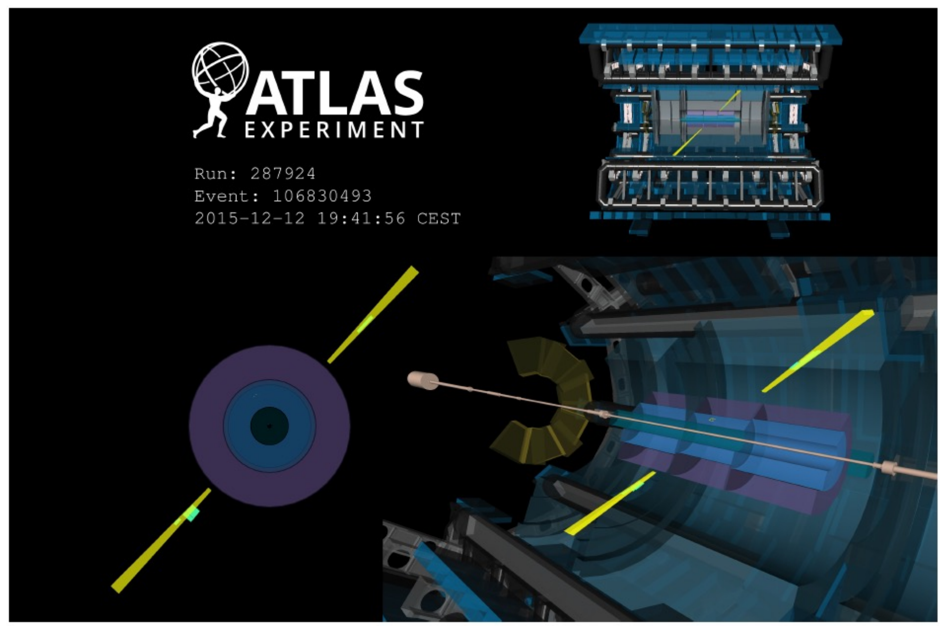
- Monte Carlos used
  - $\gamma\gamma \rightarrow \gamma\gamma$ STARLIGHT 1.1
  - $\gamma\gamma \rightarrow e^+e^-$ STARLIGHT 1.1
  - CEP ($gg \rightarrow \gamma\gamma$) SUPERCHIC 2.03
- Everything is simulated with Geant 4 and reconstructed with standard ATLAS software



Event Displays

- The other 11 events look pretty much like these





Yields

Selection	$\gamma\gamma \to e^+e^-$	CEP $gg \to \gamma\gamma$	Hadronic fakes	Other fakes	Signal	Total expected	Data
Preselection	74	4.7	6	19	9.1	113	105
$N_{\text{trk}} = 0$	4.0	4.5	6	19	8.7	42	39
$p_T^{\gamma\gamma} < 2$ GeV	3.5	4.4	3	1.3	8.5	21	21
Aco < 0.01	1.3	0.9	0.3	0.1	7.3	9.9	13
Uncertainty	0.3	0.5	0.3	0.1	1.5		

- ATLAS sees 13 events over a background of 2.6 ± 0.6

Significance is 4.4σ

- Backgrounds
  - Electron pairs from MC
    - We have control over this by looking at events with 1 and 2 tracks
  - Central exclusive production from MC
    - Constrained by the high Acoplanarity region
  - Hadronic fakes taken from data using other triggers
  - "Other fakes" are mostly cosmic rays
    - 13 of these events have an identified muon in them

Getting to a Cross-Section

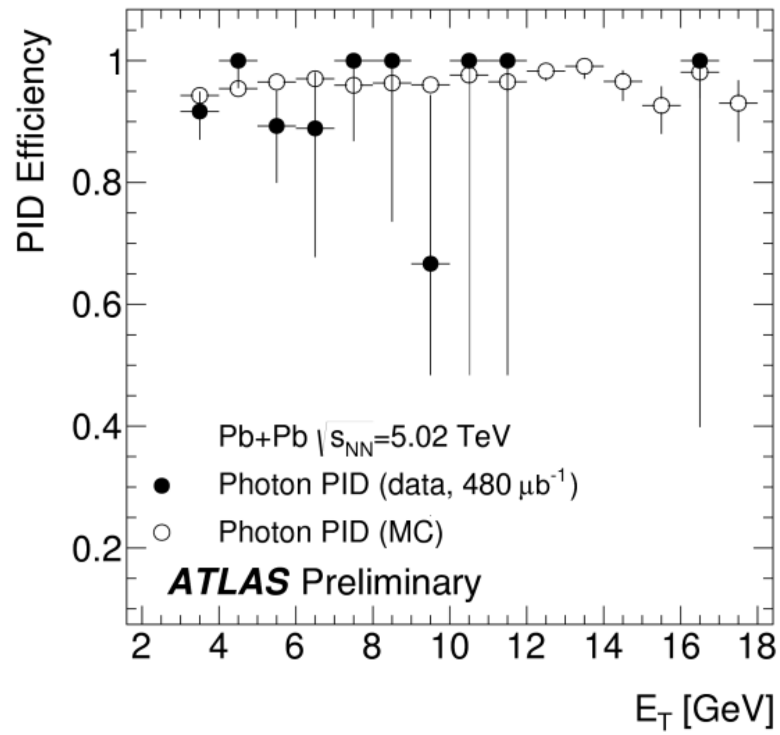
Source	Uncertainty
Trigger efficiency	5%
Photon reconstruction efficiency	12%
Photon PID efficiency	16%
Photon energy scale	7%
Photon energy resolution	11%
Total	**24%**

- We know the luminosity, the event yield, and the expected background. With the acceptance (from MC) and the systematic uncertainties (above) we can calculate a cross-section.

- In this fiducial region σ = 70 ± 24 ± 17 nb
- Theoretical predictions give σ = 45 ± 9 nb (d'Enterria et al.) and 49 ± 10 nb (Klusek-Gawenda et al.)

Photon Identification: How Well Does It Do?

- Pretty well
  - About a 95% efficiency
  - Reasonable Data/MC agreeement
- This would not be nearly as high if the environment were not as clean.



Measured from photons in $\gamma\gamma \rightarrow l^+l^-\gamma$ events. This provides a sample of real photons in an environment that is as similar as we can make it.