Mass and Spin Measurement Techniques (for the Large Hadron Collider) : Lectures given at TASI 2011, Boulder, Colorado.

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For TASI 2011, I was asked to give a series of lectures on "Mass and Spin Measurement Techniques" with relevance to the Large Hadron Collider. This document provides a written record of those lectures – or more precisely of what I said while giving the lectures – warts and all. It is provided as my contribution to the proceedings primarily for the benefit of those who heard the lectures first hand and may wish to refer back to them. What it is **not** is a scientific paper or a teaching resource. Though lecture slides may be prepared in advance, what is actually said in a lecture is usually extemporaneous, may be partial, can be influenced by audience reaction, and may not even make sense without a visual record of the concomitant gesticulations of the lecturer. More worryingly, some of the statements made may be down-right false, if the lecturer's tongue is in a twist. Accordingly, these proceedings are provided without warranty of any kind – not least in respect of accuracy or impartiality. The lectures were intended to engage the audience and get them thinking about a number of topics that they had not seen before. They were not expected to be the sort of sombre or well-balanced overview of the field that one might hope to achive in a review. These proceedings are provided to jog the memory of those who saw the lectures first hand, and for little other purpose. Footnotes, where they appear, indicate text/thoughts I have added during the editing process that were not voiced during the lectures themselves. Copies of the lecture slides are inserted at approximately the locations they would have become visible in the lectures.

1. LECTURE 1

So you're mostly theorists, I'm told, in the audience; theory students or people doing that kind of thing. And I'm really an experimental physicist.[1] I work on ATLAS, and intermittently try and help ATLAS come up with better ways of doing what it's trying to do: constrain super-symmetry or New Physics and so on. So I don't know whether this thing[2] will fly very well. You might end up at the end of this whole thing thinking, "Is that all they[3] do? Kinematics? Is it just momentum conservation and really that is all we do because us poor experimental people

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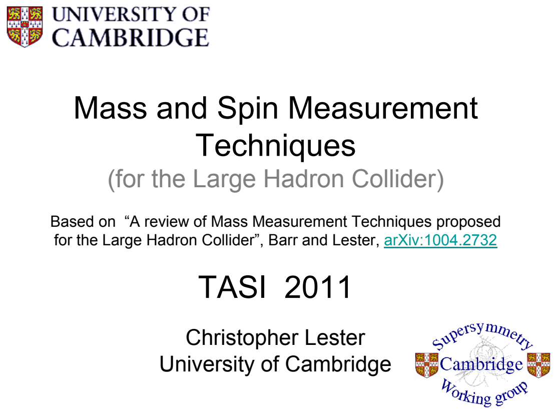
[1] Defence mechanism: I tell theorists I'm an experimental physicist, but tell experimental physicists that I'm a theorist. You get cut more slack.

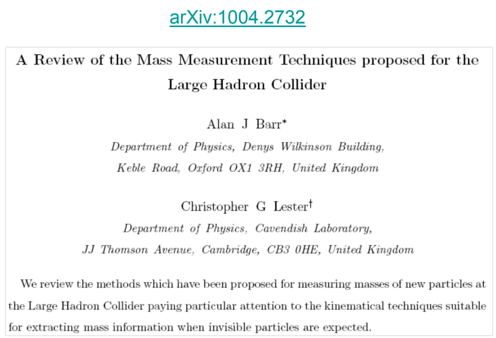
[2] [series of lectures]

[3] [experimental physicists searching for BSM]

don't know much else beyond that really." [Will you end up thinking that] we struggle when we get to the lagrangians and things like that?



Something else: in Cambridge – because the candle[4] has burnt down before you get to the end of fifty minutes – all our lectures are fifty minutes long. And this idea that one could possibly speak to someone for an hour and fifteen minutes[5] is beyond our understanding. I myself get bored in my own lectures at forty minutes, so I don't know what we're going to do but we'll see how this goes.



A Review of the Mass Measurement Techniques proposed for the Large Hadron Collider

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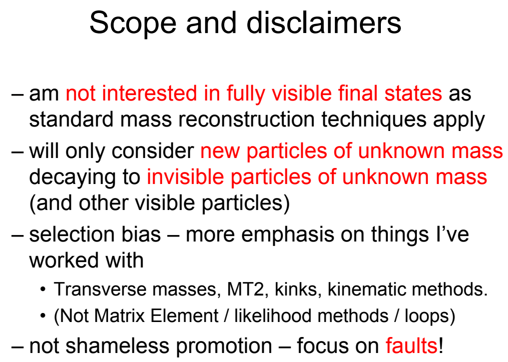
We review the methods which have been proposed for measuring masses of new particles at the Large Hadron Collider paying particular attention to the kinematical techniques suitable for extracting mass information when invisible particles are expected.

[4] Konstantin Matchev introduced me to the audience with some remark about The University of Cambridge being somewhat behind the times. The remark mentioned use of candles rather than electric light. This sentence refers back to that remark, while I attempt to explain how astounded I am that they want 75 minute lectures in Boulder!

[5] This is the length of lecture requested by the TASI organisers.

A lot of what I'm saying is a summary of a paper, a review that a colleague and I wrote up [1] where we tried to write down what people had been doing to say how you should measure masses and things like that, in the large Hadron Collider a couple of years back. We wrote it basically because we kept forgetting what people had done. That was the problem. At this point there was something like 150 papers with people proposing different methods and we could no longer remember what it was that people were doing, so we wrote this for our own benefit really, and stuck it on the archive.

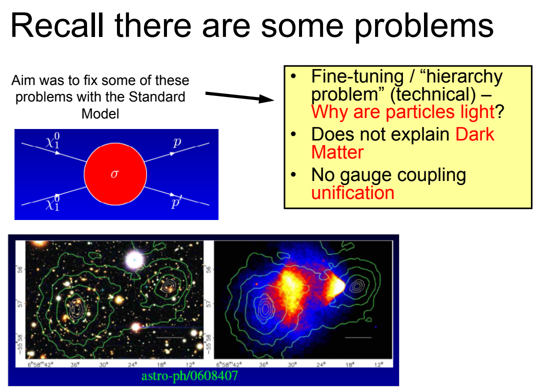


So let's go to the disclaimer. So there are all those methods out there. Things that we're not really going to talk about much here in these lectures are those that involve completely visible final states. So some particle decays, decays, decays, and makes a lot of stuff that you can see, because basically the way of reconstructing masses of particles in that sort of situation is generally looking at the invariant mass of some subset of the final state momenta, and there's not an awful lot you can talk about there.

So mainly we will be concerned here with those parts where you've got the large Hadron Collider, or a Hadron Collider of any type, producing invisible particles in the final state, particularly invisible particles whose masses you don't know. And there is some selection bias here. I'm mainly talking... I'm going to give more time to the things that I've worked on, not because I think that they're better or something like that. It's mainly because that's where I feel more confident.

There are loads of other ideas. Lots of them are quite good, and I would encourage you to use this course as mainly something like to maybe get you interested in the area if you were not interested in it at the moment, and then if your appetite inclines you to look for more, then go back and look at all the other things that I'm not talking about because you will find lots of gems in there. Right. So hopefully I'm going to be honest and show you the things I didn't like about our current ideas.

Now what are you going to learn at the end of this? Well maybe one thing that you might appreciate is that you should think, 'Crikey experimenters really don't know what they're doing'. They tend to do the wrong thing. They do it in an honest way. It's because of the pressures. They're always trying to build the next bit of the machine and then there's less time to actually spend thinking about how to actually analyse the data, and the software is always a horrible piece of equipment that no-one can figure out how to use. It takes them six months to learn just how to launch a job, and so in that time you are facing so many bugs that there's a huge pressure to just reproduce the analysis that was done by the previous graduate student. And at the end of it you're so pleased that that bit works that that's the analysis you use. You never change your analysis. You never try and make it better because it's just too scary to try and make it better. And maybe we'll see some examples of places where experimental people don't describe what they're doing very well.



Okay. So you all know far more than me about the problems that we face in particle physics, things that we don't know about. And one of the things that we expect to have to have in whatever solution for all these things is going to be extra particles are often produced in pairs, right, because otherwise, they would tend to still be around up in the universe now, or wouldn't have decayed or whatever.

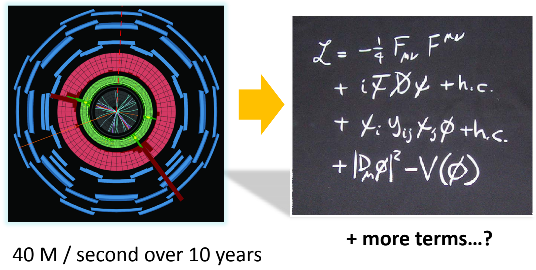
So we're expecting these dark matter particles perhaps. Our models have got to be able to look for them, find them, and if we're able to make them they will probably be produced in pairs and we'll have this real annoying complication that when they come out, not only will we not see them, but there will be two things we don't see. And that's perhaps one of the main things that's driving the techniques that we're going to be talking about.

What are common features of "solutions" to these problems?

- Big increase in particle content
- Longish decay chains
- Missing massive particles
- Large jet/lepton/photon multiplicity

Also, we are typically having large particle content. And that means the longest decay chains. Maybe as a dark matter particle it's not going to be strongly interacting otherwise it wouldn't be dark, but if you want to make it at the LHC you had better have things that are strongly interacting to start off with. And so then you've got to get from those strongly interacting particles down to the weak ones, and most of the models that you have come up with and your supervisors have come up with, have forced upon us some horrible glut, at least a doubling if not a trebling of the particle content because you're so obsessed with symmetries that you don't want to have just an extra particle here or an extra particle there. 'Give me them all!' you say. So we're facing this problem that you're going to be making these strong things and then decay down and you've got a whole lot of faff and crud and then maybe some of the things you're interested in at the end.

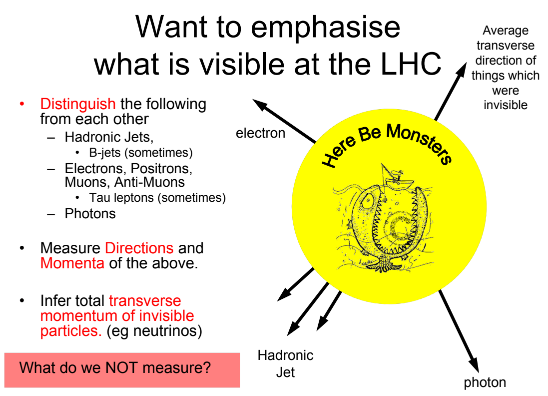
The game…



So our game, over in ATLAS, is to sort of make forty million events per second over about ten years and eventually help you find out what it's in your other lagrangians. And at some point five thousand people, or whatever it's... I think it's actually now sort of 6.4 killer professors, I think, is the author list on ATLAS. By the way, we're not professors in the UK, not unless we're really, really senior. I'm just a doctor, so I'm not even one of the killer professors.

At some point, 5000 people will shout:

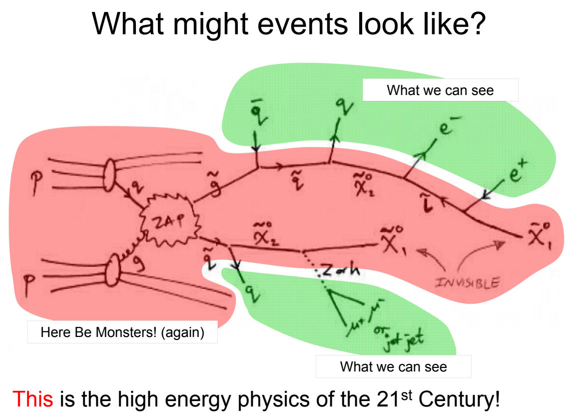
At some point five thousand people will shout, 'We've found a... something.' Spotting a deviation from the standard model is one way to go. But then people want to know, well actually what is that something? And how hard is it to identify that thing that we have found, from an experimental perspective?



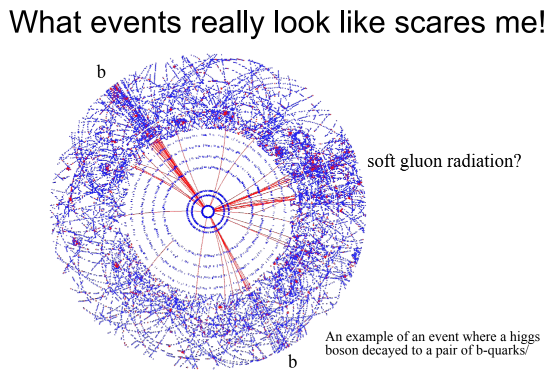
I want to emphasise the kind of thing that are visible to us at the LHC, because there's this huge fog of obscurity, which I've labeled as, 'Here be monsters!' Some kind of terra incognito at the end of the world. Does the top of Canada exist? Or whatever... it's hidden behind this fish that's going to eat that thing. Snoopy. And all the interesting stuff is in there.

What can we see? Well, we might be able to see the odd lepton. Yes. We might be able to see the average transverse direction of the things which were invisible. We might be able to see some things, but can we really see a photon and electron? Answer: disappointingly, no. The photons don't come out labeled 'I am a photon', and the electrons, 'I am an electron.' There's whole sub-teams within ATLAS: the e-gamma group, who are desperately trying to figure out how to make sure that the things they call electrons are electrons, and the things we call photons are photons, and we don't get mixed up. They've got their own Stalinist police that goes around and ensures that all your papers that you write within the collaboration definitely subscribe to their latest edict on exactly how you separate those two things.

And we certainly don't get to see whether we had quarks or gluons in there. We just get hadronic jets things like that spewing out. And even then we can't figure out, 'Was it a gluon? Was it a quark? Had it fragmented before?' There are loads of things which we don't measure and we don't see and we are faced with this.

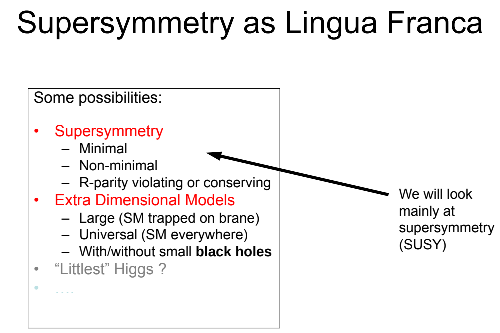


So our paradigm is that we have got our protons coming in, and we extract a bit of one and a bit of the other it could be a quark or a gluon. We make that big zap and then some spew comes out, with that long train. And the things that we can see are over there are the periphery: the Hadronic jets, perhaps the electrons if we're lucky. But all the things that we can't see are buried right in the middle where the monsters are, and we are really, in a sense, having to try and find out what was in the fireplace the morning after by inspecting the ash that is sitting in there. And we really are a long distance away from what we would really like to be looking at.

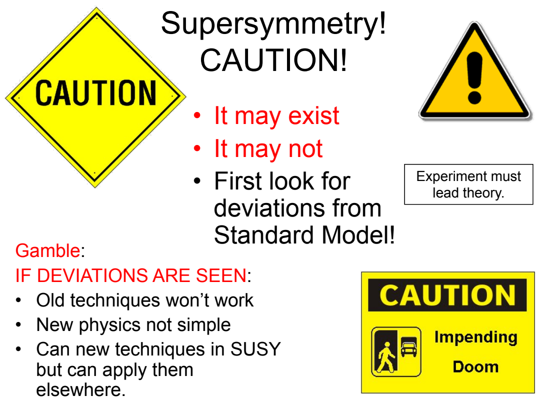


It doesn't help that the actual events really scare me. This is supposedly an example of a Higgs to BB Bar event that ATLAS have simulated, and you see all these hits here in the semi-conductive tracker? And then in the pixel detector? The pixel detector is not showing bam bam, two hits here and here because this particle came through. Every one of those little blue dots is a hit; probably noise or other things, some kind of looping particle in the event. And some algorithm, probably a competing algorithm, if it's about the muons, because the muons can never agree how to reconstruct muons in ATLAS. We've got two competing groups; people who never talk to each other.

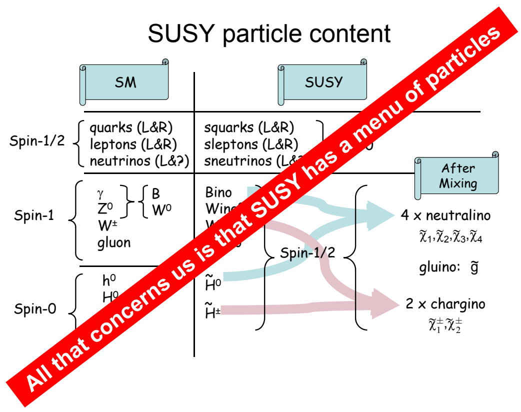
Anyway, they write pieces of code and try and look at all these tiny little dots and try and join the dots and say, 'Yeah, there was a particle here but not there or not there.' I don't know. I don't sleep happy - with these anyway.



Now, in this set of talks, I'm going to use super-symmetry as our sort of lingua franca. Okay? Now that's basically not because I think that it's there.

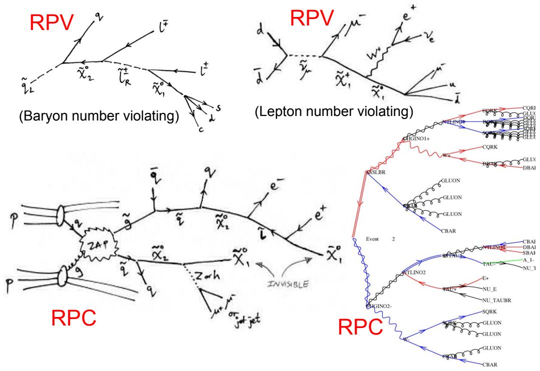


In fact the LHC is doing a good job of trying to persuade us that it might not be there, but that doesn't in the least bit interest us from this point of view because what super-symmetry has got is a nice big menu of particles.



Lots of the other models that people are interested in looking for or spotting or ruling out, they have got a big menu of particles as well. So we can map most of the techniques that are here, presented in terms of super-symmetry, onto ones with Kaluza-Klein exhortations or what-have-you. So no real detail here sort of needed.

Even in SUSY many possibilities



Even so, even within super-symmetry bear in mind you've got lots of different types here, the types that I talked about just now where you have got invisible things in the final state. Of course we can have entirely visible final states from super-symmetry, things where that neutralino at the end can decay maybe baryon number violatingly or lepton number violatingly and so on. So lots and lots of choice here, even though we restrict ourselves to SUSY.

Do we care about masses?
- Common **Parameter** in the Lagrangian
- Expedites **discovery** – optimal **selection**
- **Interpretation**
  (SUSY breaking mechanism,
  Geometry of Extra Dimensions)
- **Prediction** of new things
  Mass of W,Z → indirect top quark mass "measurement"

Do we care about these masses and so on? Perhaps one of the things that least excites me about them is that there are some parameters in one of those lagrangians that you're interested in, mainly because I can't work the lagrangians. Mainly I'm interested in them I suppose because they are... the spectrum of particles for example in lots of typical SUSY models is different than the sort of spectrum of particles that you might expect in a model with extra dimensions say. There might be degeneracy's or bigger gaps between particles; the strong particles might be in different places. So you've got lots of things. So one thing you're doing if you're tracking down the particles is you're actually helping to figure out what is the underlying physics going on there.

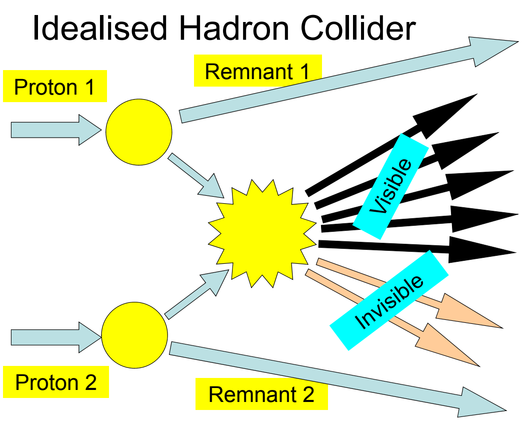
"mass measurement methods"

... short for ...

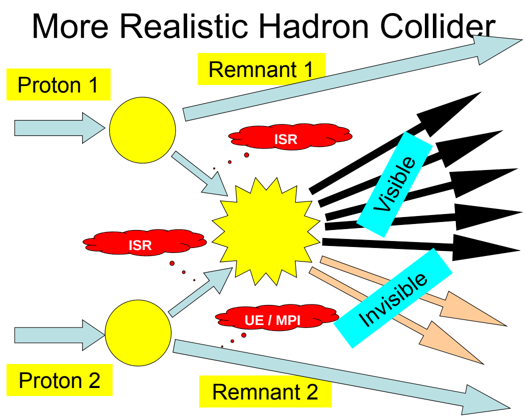
"parameter estimation and discovery techniques"

Also, although we have talked about mass measurement methods, it's not really just about mass measurement methods. You could really call it sort of optimal parameter selectional... how to select events. If you're trying to select beyond the standard model events, events we don't yet know about, in a data sample, what does it mean to be looking for new things? Well basically one of the big things that you're supposed to have in New Physics very often is heavier particles, right? And the light particles are the ones we already know about. And so if you're able to come up with some variable that should map super-symmetry, or another model, into some high value... by virtue of the fact that... some high parameter because you're able to say something about it's mass scale, then you are able to start selecting the... separating the super-symmetric events from the non-super symmetric events, or whatever.

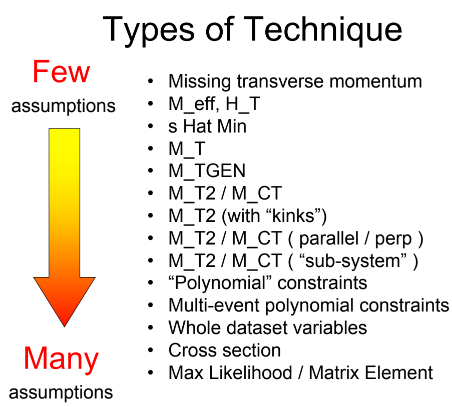
So actually looking for the stuff, it's very hard to look for New Physics without having some means of being able to say where you want the New Physics to show up, and wanting the New Physics to show up in certain extended mass regions is one way of trying to do it. Okay. So, in other words, it's called mass measurement methods but parameter estimation, discovery techniques are equally well things that you could apply to this. It doesn't have to be just mass.



So our idealized Hadron Collider. So there again we have our crunch and we get bits of our things and we produce them. Generically... the most general statement that we can make is that we'll produce some visible stuff and some invisible stuff, maybe none of one or none of the other. If it's really the only visible stuff from New Physics we're in big trouble.

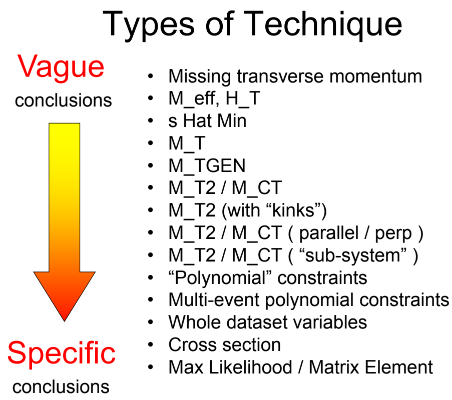


We should bear in mind that, to be realistic, Hadron Colliders aren't quite like that. You have ISR, FSR, Initial State Radiation all sorts of other things sort of leaking off, contaminating and... most of the time you really don't want to be looking for things that are sensitive to where these remnants are going and most of the time they're going down the beam pipe and we're not seeing them, but things can leak off from those and so when we start constructing variables you have got to be quite careful that those... that you're not sensitive to effects coming from those bits that are harder to model.

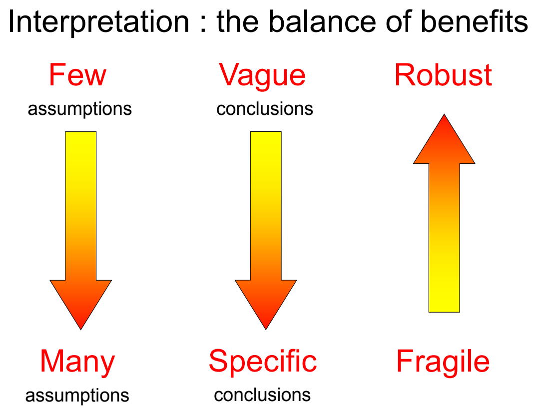


And, as I said, there are hundreds of different techniques that have been proposed. And I think you could, roughly speaking, rank them into those that make few assumptions and those that make progressively more and eventually many, many assumptions. This is not an exclusive list. This is just a few here and we'll look at some of those later. Don't worry about reading them now, ranking those. Have people got the handout yet? No. Okay. So at some point this morning... Susan was photocopying all these things. I've got one copy of it here but evidently you've not got it yet, but you will.

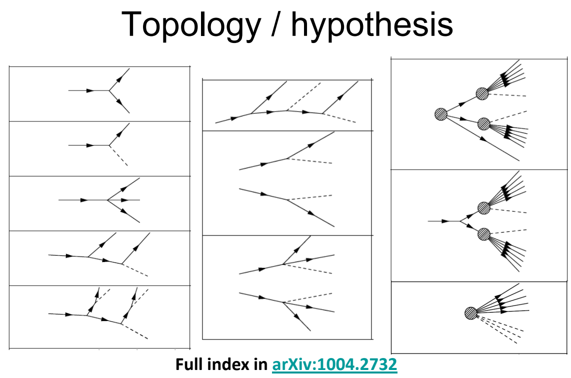
So... and those few assumptions might be starting... just sort of stab in the dark; wet you finger, stick it in the wind. Is it cold on one side? Yes. Okay. The wind is blowing. Maybe there's a weak wind. Down to things where you make many, many assumptions, where you claim to understand the whole of some highly specific model with all of its parameters "inaudible" and the detector response and everything and you take your events, at the end of some big run at the LHC, and you say, 'How likely is my data for this set of model parameters? How likely is it given that set of model parameters?' And then choose the set of model parameters that gives you the best likelihood - and that's the most complicated where you're having to assume everything.



Now as well as making sort of assumptions, we should bear in mind that correspondingly that means that you tend to have vague conclusions, or more vague conclusions with the things that you start with that make fewer assumptions, because you don't really know what you're measuring. On the other hand, here - well you really do know what you're measuring. You have very specific conclusions, but of course those very specific conclusions will be very wrong if the assumptions that you have made aren't right, and if you have assumed this whole detailed model and you've chosen the parameters that maximise it, well that's right if that's the right model, but it's almost certainly not the right model and so this is a meaningless set of conclusions. So on the other hand you get these sort of robust conclusions when you make fewer assumptions; robust but weaker. Yes? Robust but weaker. Yeah? Robust but weaker.



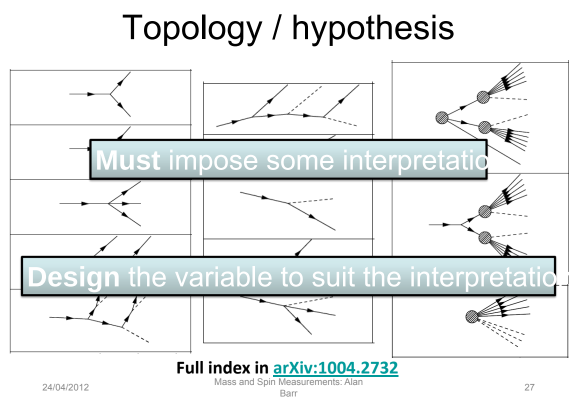
So I don't want you to think that any one of these places in this list is better or worse than any others. There are cases where you want to make these strong conclusions but you're willing to take the hit that they may be meaningless, or you may be prepared to make vague statements. And there's a balance okay? So really you look at things throughout the whole of this spectrum, and so if you wanted to rank complexity, one of the ways you could rank complexity I mean it's a vague concept would be to say, what sort of topologies or hypotheses are you making about the nature of the event that you're looking at?



[6]

So you see an event bff! It just looks like rubbish in the detector, and then with no real right or feeling that this is exactly what you are prepared to do you say, "Oh, I believe it's got events like this in. A pair of particles, [each] decaying to two visible particles and one invisible."[7] Dots tend to be invisible particles but not always. Or you might think, 'N". I just think I've got some big blobs I don't understand and it's making visibles and some invisibles."[8]

So that's what I mean by topologies and hypotheses. Roughly speaking I don't know if you would agree with me that looks kind of simpler, this looks harder, and that looks remarkably specific this one. And you have to sort of... these are the ways you impose your interpretation on events, and you could argue that once you have decided, 'I'm viewing my event through these glasses. I'm picturing that this is what's happening in it', then there are better and worse things that you can do to try and extract parameters about this particle, this parent particle here. You should in some sense design your variable or your technique to match the interpretation that you are considering.

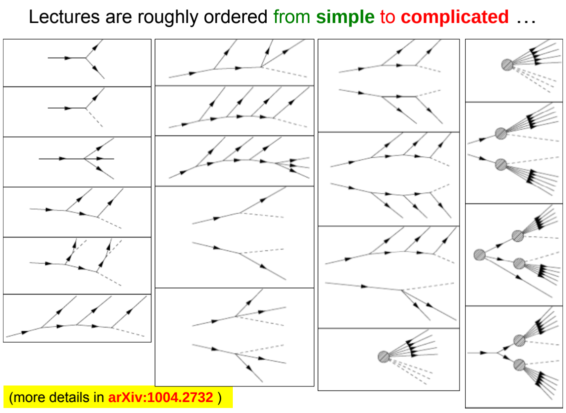


[6] Extracted from the full index found in [1].

[7] pointing to topology sketched at bottom centre of slide

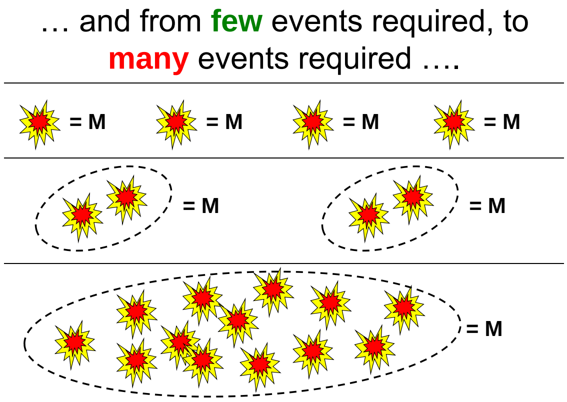
[8] pointing to topology sketched at bottom right of slide

The review that I talked about, basically the most useful part of it is an index, the front which is a pictorial index. It's got pictures like this. Disappointingly I looked last night and realised that we didn't have an index. There! So even though it extends a full page, there's plenty of things that you can miss out. There's lots of interpretations that you can layer on top of these things. But people don't always do this. They don't always construct a variable to match their interpretation. Experimental collaborations have a tendency to instead do the first thing that springs to their mind, and then see if it works; and if it does, as I said, it gets set in concrete.



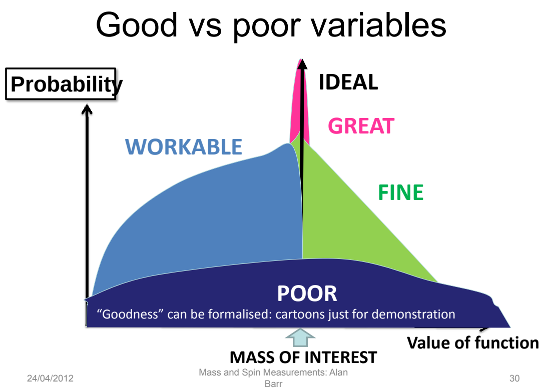
[9]

So that's, roughly speaking, the order we'll go through these things. We won't cover all of these by any means barely a third of them I suppose in this set of lectures.



Also you can rank things, not by the complexity of the typology of the decade inside, but by whether or not you think you're going to be able to get a conclusion from a single event. When an event is there, if you make your hypothesis, your picture in your mind about what it's contained in, can you extract a mass measurement or a bound on some mass or make some statement from that single event? Sometimes the answer is yes. Other times, the answer is, no, it's just too under-constrained. But perhaps if you saw a pair of events, or two bits of one... or two events happening simultaneously with some constraint that relates them, then maybe from a pair of events you can draw a conclusion. Or perhaps you have to just get thousands and thousands of events. You have to plot a distribution and look at something over a long period of time, and... so again, there's sort of a slight structure to this that we're sort of going down, making increasingly more specific assumptions about how many events we need to look at to get there.

[9] Extracted from the full index found in [1].



[10]

What can we say - sorry if this is this is broad at the moment. We will get bear with me - down to some specifics. What do we want in the variables that we are trying to find out? Suppose we are trying to find some mass, or some other parameter of interest that is supposed to be here.[11] In an ideal world we'd like something that just gives us a data function that maps every event to this value, and gives us a distribution which, over time, looks like this.[12] Okay? That's ideal. In practice, of course, that's just great.[13] If we can get something that looks like this, we're really still very happy.[14] Sometimes we can, right? Particularly we will hopefully recognize... that looks a bit like z mass plots. We can't always get such good things. We can get things like this a lot of the time, [15] and I'm not going to talk about many of the techniques that give these things, things with so-called kinematic cusps, although I recommend you read the literature and have a look at those parts yourself if you're interested. That's fine too.

Quite a common thing that we get are distributions in the variable of interest that we're going to try and get that look like this.[16] Okay? That have events in some position and then eventually stop at a certain point, and that's pretty good too because okay, we get this distribution and we see, after what point do we no longer see any events? Well strictly, we would never want to do that because there's always going to be background events. There's always events extending up here, but you do some fit to the shape of what you think the distribution is supposed to be, extract the parameter that is that endpoint that's a very good way of doing things. And the kind of things we don't want things that look like that.[17] Okay? Which is usually what the experiments use.

[Audience laughs]

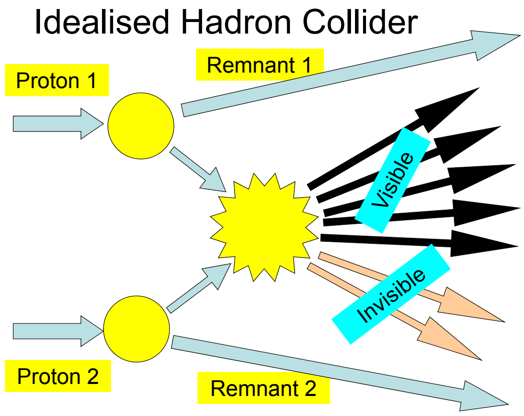
Few assumptions,
Vague Conclusions.

Anything with sensitivity
to mass scales.

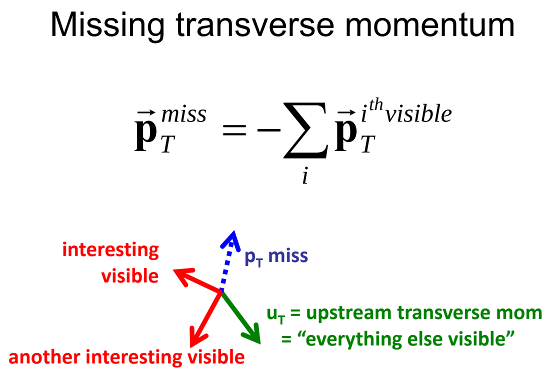
Okay. So let's start at the beginning with the sort of variables that make the fewest assump-

[10] This slide copyright Alan Barr, Meton College Oxford. Reproduced with permission.
[11] [pointing at "MASS OF INTEREST"]
[12] [pointing at "IDEAL"]
[13] [pointing at "GREAT"]
[14] [pointing at "FINE"]
[15] [pointing at "FINE"]
[16] [pointing at "WORKABLE"]
[17] [pointing at "POOR"]

tions. So here we're looking for just anything with some kind of sensitivity, so some kind of mass scale. Back to our picture again:



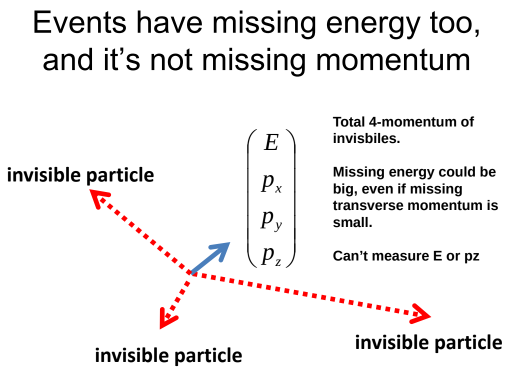
What could we do? Well, we could look at the missing transverse momentum.



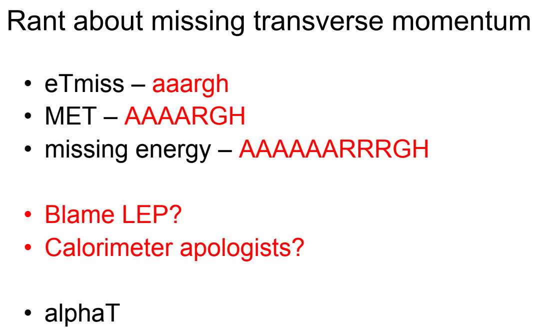
Okay. What is the missing transverse momentum? Now you will see why in a moment... I might faint in a second. I want to tell you something about this. So missing transverse momentum: presumably you've been told about this before and you know that it's the total transverse momentum which the invisible things must have had, which you figure out by looking at where everything else went and saying, 'Well we like momentum conservation so there must have been some stuff there'. So on the one hand, yes. It's the sum of minus the vector sum of all the visible things, all the transverse momenta are the visible things you saw.

In terms of notation: I want to point out at this stage that very often in these variables, it's useful to break down this sum into different parts. Okay? So very often the visible things, which are going to be these blue, and these two red things over here, can be grouped into categories. Those visible things, which, for some reason, are interesting to you - may be because you think they come from the particle whose mass we are trying to measure, and you're particularly interested in that stuff and then there'll be the dross, the rubbish, the everything else. This other stuff, often called upstream transverse momentum, or ut and things like this it doesn't necessarily even have to be upstream but it has retained that name is relevant because if you think that all the things that you're trying to track down, your hypothesis, all led to the missing momentum and the visible bits you're interested in, then this provides the recoil against which your interesting system is recoiling. So you've got Hadron Collider, makes an event, rubbish goes out this way, interesting system goes over here, and it falls apart into visible and invisible things. Okay?

So these things are clearly related. There's the upstream transverse momentum vector plus pt miss vector, plus the visible things you're interested in, they all add up to nothing. So there's a redundancy here. Sometimes people will talk about PT Miss. Other times they'll talk about this because... you can always extract one of these from the other three.



Okay. Well why might I faint? Because events have missing energy too, and missing transverse momentum is not missing energy. Or at least it depends on whose parlance and technology you use. What do I mean? So if there was an invisible particle heading off this way and one heading off this way and one heading off this way, there is some total invisible full momentum, and we don't know what it is because we can't measure these things, but it exists, and it's got some x components and y components and z components and an energy component. And these two we've got a real handle on, because of the momentum conservation. But we don't know how big this is. In fact the missing transverse momentum, the momentum components, could be 0 if these things are in some kind of Mercedes Star or back to back. Yes. There could be a huge amount of missing energy. And furthermore, the missing energy will depend on the masses of these guys. So if I had the same amount of momentum here but I increase the mass of these visible things, then that is going to go up too.



Now, this is a problem because most of the experimental collaborations and probably a lot of the theorists too, always refer to missing transverse momentum as missing energy. Now this might not matter if it was just a matter of terminology, because we could perhaps all agree that whenever someone says "ET miss", what they mean is missing transverse momentum. But the problem is it really does matter when you have got more than one invisible particle. When you only have one invisible particle, when there's only one little red thing here, and if you don't even know what its mass is and you're assuming it's got 0 mass, then E is $\sqrt{p_x^2 + p_y^2}$. So it really doesn't matter.

But, as soon as you've got multiple invisible particles, which you do if you're trying to constrain super-symmetry, then you've got to draw this distinction. And what do I hate? The name of the "missing energy" group! That's what it's called in ATLAS. We can't stop people calling it this .. but at least we have agreed in ATLAS a kind of a compromise solution, that the SUSY group, which has to draw this distinction,[18] must use the term 'missing transverse momentum' in text, but it has to denote it with the symbol $\not{E}_T$. Okay? So at least we've got half of it there which is a kind of compromise. "Missing energy"[19] is even worse.

Who can we blame here? Should we blame ... this is actually charitable blame.[20] I mean here I'm trying to say, can we say, 'Look. It's okay. I'm a reasonable person. You're reasonable. Can we just get together. What...?' There really *was* missing energy at LEP because it was not a Hadron Collider. Okay? Things [21] came together. There was no PDF, no particle distribution function, and so if you added up the energy of the things you saw, the rest was your missing energy and you knew how much it should be and they even used that. It was a perfectly valid terminology. I think that's where we got the terminology. That's where it came over. It was a hangover from the LEP days.

What about the calorimeter apologists? These are the people who say, 'Look, we measured this missing energy in the colorimeter. That measures energies.' It might. I mean there's other people who would say it actually measures photon counts, from the photosensitive things that are going to... let's not get into that. Moreover, you've got to use that energy if you think it's there, and a hypothesis about zero mass to get a momentum so that you can use momentum conservation to get missing transverse momentum. I won't even talk about AlphaT of CMS. There, the problem is not denoting different ways of missing transverse momentum but just defining "transverse energy" unambiguously. If you've got your (E, p_x, p_y, p_z) ...

[Student poses question that is not audible on the recording .. possibly asking what "Transverse Energy" actually is, since this is where the discussion now seems to head]

Now, very often, because it's a Hadron Collider, you don't know what's going down the beam pipe. The Z momenta are awkward and you end up working in sort of a transverse projected space sometimes, so you end up working not in a Lorentz four space with sort of 1+3 signature, but a Lorentz 1+2 space, where you want to have some kind of energy-like quantity let's call it ET, or sometimes called 'Little E' to distinguish it from 'Big E' and then a x and a y component, but we have somehow dispensed with the z components.

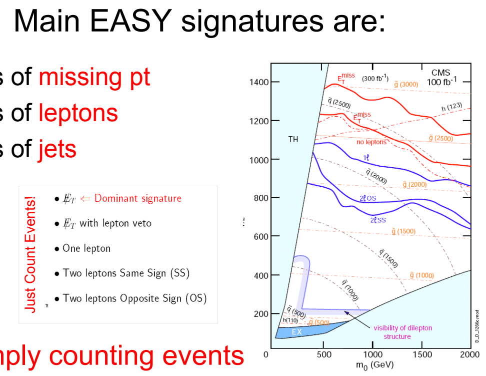
But in the way you might construct this, if you want to retain information about masses when you are working in this reduced space, then you don't want to throw away mass information, and so the kind of term that works best in the top is typically a thing where it's the mass of

[18] [i.e. needs to deal with massive invisible particles]
[19] i.e. without the "transverse"
[20] Very little of this paragraph seems to make any sense when the transcript is read back in isolation. All I can say in my defence is that this is probably because the whole issue of mis-naming missing transverse momentum makes me very upset. Incomprehensible on the page or not, I was gratified to find the message well received by those present in the audience.
[21] [i.e. electrons and protons]

your particle squared plus this: px + py. That's the ET of the transverse energy fits you'll be using if you don't want to start throwing away mass information. I won't go into the others that you would have. I mean alternatives, Big E signed theta, where theta is the "inaudible". That's another one, and there are others.

Now alpha T is defined in terms of ETs, but lots of people don't tell you what their ET is. They won't tell you whether it's the one that I've just written, or whether it's just $\sqrt{p_x^2 + p_y^2}$, or whether it's $p \sin\theta$, and my poor old graduate student was trying to figure out whether or not alphaT was worth us using in ATLAS. And we found... CMS had defined alpha T in slightly different ways in each of the three papers where they were saying what they were going to do, and each one of them used a totally different notation and didn't define the notation. And we thought we've got it covered. It's okay - because we had a friend in CMS who gave us their code, or some answers for alpha T values, and we tracked down... We thought, 'Right. There's basically five squared. There's 25 ways they could have confused us with a notation'. So we produced 25 different alpha T calculators and ran them and compared them to see how many decimal places agreement we got, and some of these values were orders of magnitude different. Others were differing only in the fifth decimal place and so on, and we worked our way through it.

And eventually we thought, 'This is it. It's this one. It doesn't agree to all decimal places but it's pretty good. It would be very hard for them to have thought of a different way of doing it.' And eventually we found that we couldn't reproduce their values at all. We had missed one. There was a 26th ambiguity that we hadn't thought of because surely no-one would make that mistake! **And eventually we could reproduce their results. So please, if you care about our sanity, you have a duty to actually talk about these things correctly.**[22]



Anyway... so, what are main easy signatures? $\not{E}_T$, that's just ATLAS for you. So the main signatures therefore: you could just look for the missing energy and say how much missing transverse momentum have I got? And back in the old days that was thought... this is some kind of space, and which parts of this space can we rule out by just looking for events that have got missing transverse momentum, bigger than we expect? And each curve here is a different search technique: looking for leptons, looking for one lepton. And basically the highest... the thing that was going to rule out most is the missing

[22] i.e. you must define your notation unambiguously

transverse momentum. Just ask for a lot of it, and count your events and see if you've got a lot, and that's a pretty good thing and it's still hard to beat that today. And perhaps that's it. We should just end the thing here, say, 'Look for missing energy if you're looking for our parity conserving super-symmetry. The End!'

Can attempt to spot susy by counting "strange" events …

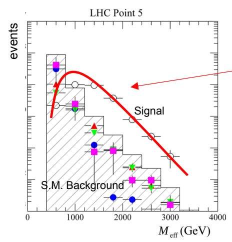
… but can we say anything concrete about a mass scale?

Next example still low-tech ….

Now in practice, people want to do more than that. They don't just want to spot a difference from the standard models by counting events that don't fit in with the standard model. They want to say something concrete about these events, so actually the ATLAS searches for ages and ages and ages used the next kind of lowest tech, making not really many detailed hypotheses about the event.

Effective mass

What you histogram: $$M_{\text{eff}} = \mathbf{p}_{\text{T}}^{\text{missing}} + \sum_i \left|\mathbf{p}_{\text{T}}^{\text{jet}_i}\right|$$



You look for position of this peak and call it MeffPeak

Call it Meff and Mest too (just to confuse people!)

The next lowest tech thing - something which ATLAS has been calling, since long before 1999, 'the effective mass.' You'll see that in the intervening time Tevatron started calling it something different and now it's started confusion. No-one knows what it's called any more, but I'm going to use this terminology. And that basically says, 'Let's take the missing transverse momentum' it should have the modulus around there, the modulus of the missing transverse momentum. 'And add it to the sum of the transverse momenta of the few salient things in the event.' That could be the first four jets, or some leptons you're particularly interested in.

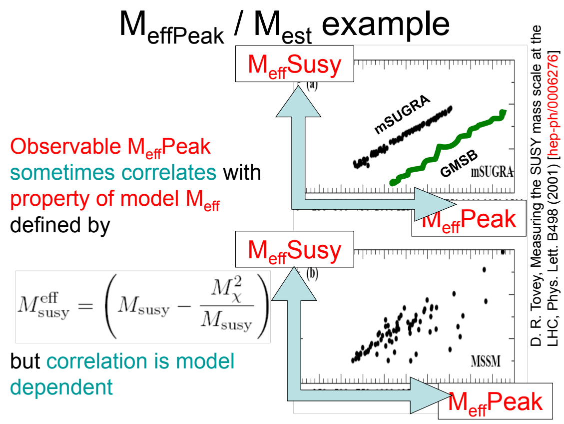
It's a bit of a vague definition. You change the definition depending upon the time of day or what you feel that you are looking for or whether your hypothesis is supposed to have lots of jets in and whether it's not supposed to. And the idea is that you... so for any event that comes along, calculate this number, this sum of stuff and then you put it in the histogram. The next event comes along, and so on. And you might hope, if you're lucky, that the standard model should have low values of this because low scales are involved and you get some kind of smearing up to high values by the detector resolution effect, or mismeasurement, so you will have some kind of exponentially falling background distribution shape there, for the standard model, and then, if you're lucky, some SUSY signal might stick out with a lump. And, what are you supposed to do with this lump? Well, one thing you could do, to see if you could draw a conclusion about the mass scale from it, is you could fit that peak or something, or identify if this peak is there at all, and say, 'What x value across here? Where is my peak position?' And you could call that peak position an effective peak position. Unfortunately that's not the only thing people call it. They call it an effective 2 and even MS. That's just to confuse people. I call it a peak position.

What might Meff peak position correlate with?

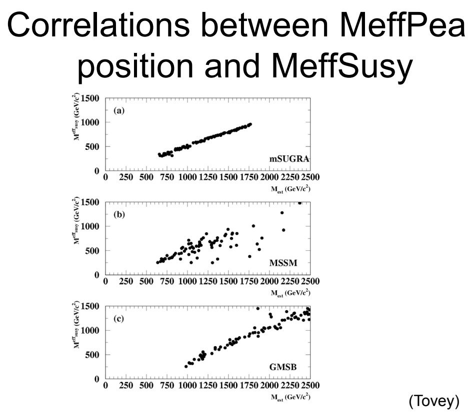
Define SUSY scale:

$$M^{\rm eff}_{\rm susy} = \left(M_{\rm susy} - \frac{M^2_\chi}{M_{\rm susy}} \right), \text{ with } M_{\rm SUSY} \equiv \frac{\Sigma_i M_i \sigma_i}{\Sigma_i \sigma_i}$$

And what might this peak position correlate with? This is really coming from the point of view of someone just creates a variable and they see, does it correlate with anything, and cross their fingers. Well it turns out the thing that it correlates quite well with, or under some circumstances, is... is a kind of effective SUSY scale. What do I mean by that? Well if you define a SUSY scale there's lots of ways you could do this, as being say the average mass of the particles that you pair produce. And when I say 'average' I mean weighted by cross section. So if there's something which you only produce squarks, then it will be the average squark mass. If you're sometimes producing squarks, sometimes something else, then you sort of weight each of those masses by how frequently you're producing them and normalize it correctly, and we'll call that the SUSY mass scale. And if you take that SUSY mass scale and then take away from it the mass of your invisible particle squared over the SUSY mass scale then someone spotted[2] that you get a nice correlation, in models like Msugra, between this peak position and that SUSY scale. So you could say that you are measuring a mass scale somehow, an average mass scale. Not completely the mass scale. There is a mass difference there. Perhaps it would be better to write this as $M_{SUSY} - M^2_\chi/M_{SUSY}$, if you're sensitive to a mass square difference, the difference in mass between the things you are making and the invisible things you can't see.

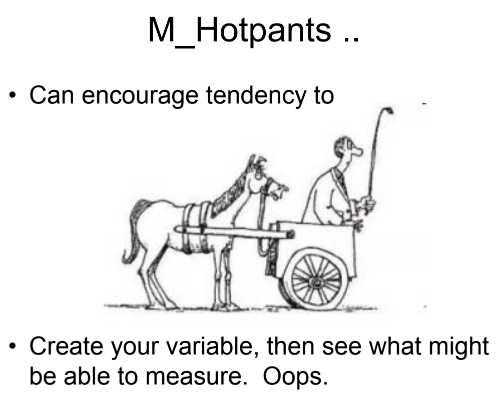


But is this a reliable conclusion? How reliable is this? Well if you move outside of a model that is as constrained as is MSugra where there are only a tiny number of free parameters, then if you move to a wider set of models you get a much bigger scatter. So you can't that's what I mean about these vague conclusions you can't interpret this peak position as a mass scale unless you are willing to hypothesise additional things. If you take a model like GMSB, gauge mediated supersymmetry breaking, then you get a lovely correlation but with a completely different intercept between these two things. So depending on what you know, whether you know your energy MSP scenario or SUSY or something like that, you would conclude completely different things from this.

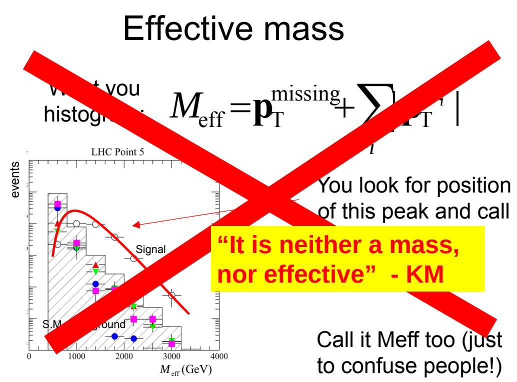


[23]

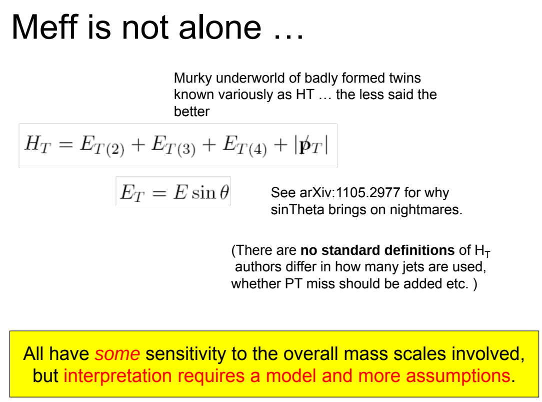
So here's the GMSB plot there okay? A different intercept than the MSugra plot.



So I sort of call this MHotpants, at least I, at some point, promised Konstantin that I would put a paragraph about "Hotpants variables" in something that we were writing, and I never really did because it was always kind of very hard and vague to define what it meant. But what it means is kind of really putting the cart before the horse, defining a variable because you think you can measure it, and then trying to see what it might be able to measure. Perhaps you should do it the other way around, it might be better, but it perhaps led Konstantin to say that the effective mass is, 'neither a mass nor effective'.[24]



We had to tone that statement. In papers we have to tone down many such statements he is trying to make. But whether we like it or not... just as an aside, you will probably see things like HT being mis-described by various people. Here's one I just pulled out of a paper:



HT is the ET of particles 2, 3, and 4 plus the PT', but look at another paper, you'll see

[23] Figure from [2]

[24] Compare this frivolous statement with later remarks in which I make clear that it Meff most definitely *is* used to good effect by ATLAS in most of its hadronic susy searches.

different things being added up. They will be defined in terms of PT sometimes. If defined in terms of ET you will almost never be told how that ET is defined.[25] So this is a related variable, sometimes using sort of other things.

Why are we adding transverse momenta?

- Why not multiply? (or add logs)? $$M_{happy} = \left(\prod_{i=1}^{n} \mathbf{p}_T^i \right)^{\frac{1}{n}}$$
- Serious proposal to use Meff^2-$(u_T)^2$ in arXiv:1105.2977
- Why are the signs the same? Why equal weights? Silly?
- How many years would it take ATLAS/CMS to discover the invariant mass for Z -> a b ?

$$M^2 = \left(\sqrt{m_a^2 + a_x^2 + a_y^2 + a_z^2} + \sqrt{m_b^2 + b_x^2 + b_y^2 + b_z^2} \right)^2 - (a_x + b_x)^2 - (a_y + b_y)^2 - (a_z + b_z)^2$$

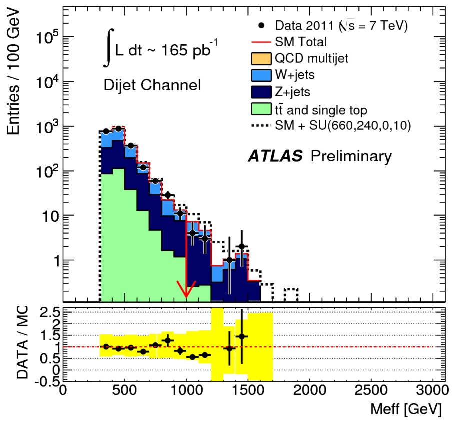
So you should perhaps ask yourself the question, since we just invented this off the top of our head, we just add up these things because we can and we'll see some correlation, why are we doing that? Why do we accept the fact that people tell us that HT or M-Effective is a good thing to do? Why add them? Why not multiply them together? You could invent MHappy, the Happy Mass, which could be the product of the PTs of the first few momenta, okay? And then to get the units right you could raise it to the power of one on the number of particles you've put in there. Maybe that's as good. Nobody knows. No-one has tried it. Konstantin and I have made a serious proposal in the paper with this flying squirrel on it, after looking at things and I won't talk about why this is, but what you really should do is perhaps, if you're going to use something like MEffective, is really use MEffective squared minus this upstream transverse momentum squared, because that has better properties, at least at the truth level. I'm not going to tell you why. You can read the paper if you're interested.

But, it just goes... what I'm trying to emphasise is that this is just sort of a made up variable and there's lots of scope for trying to... for trying to think, are we doing the right things. If we are going to add them up, these momenta, why are we even adding them up with the same weights? Why not... maybe the lighter ones should count for less or something. Interestingly this is effectively saying, take off the momenta of those particles you're not going to stick into MEffective, so you're... I won't talk about that.

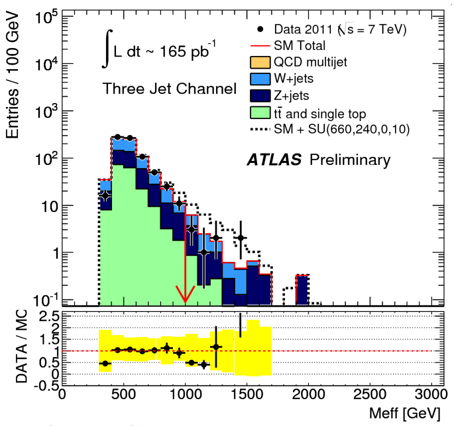
Anyway, so let's effectively say to ourselves, 'It's a good job that you theorists have been telling us, the experimental people, for such a long time, that there is this Minkowski metric on space; because if you hadn't told us that the invariant mass squared was of two particles, of something going to two particles, was the energy of the one plus the energy of the other squared minus the sums of the x and y and z component squared, we would probably have never found this, because we would have... our detector would have been telling us momentum x, momentum y and momentum z for particle 1 and momentum x, momentum y and momentum z for particle 2, and what would we have done? We would have added them all together, and then we

[25] See [3] for why defining ET in terms of $\sin\theta$ should give you nightmares in BSM searches.

would have had something that had sensitivity to the mass scale, yes. And we wouldn't have known that there was this fancy way of combining them that would in fact give us a nice Delta function. I estimate it would take us at least 15 years to come up with something like this and we would probably only get it to first order.

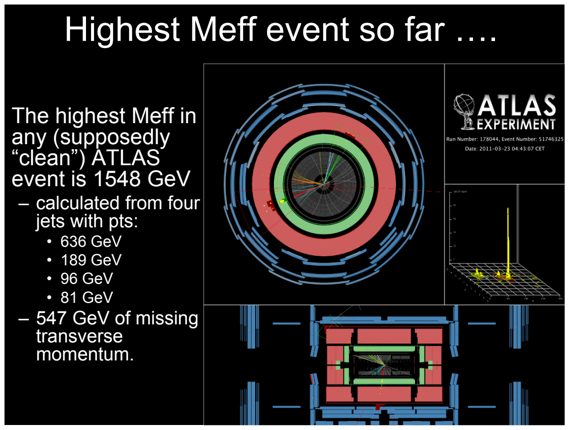


Latest ATLAS 0-lepton, jets, missing transverse momentum data.

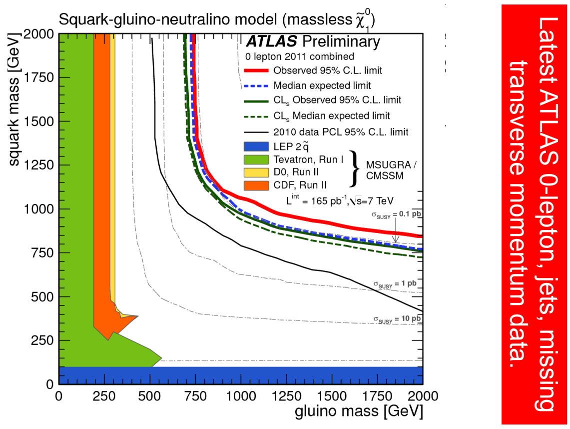


Latest ATLAS 0-lepton, jets, missing transverse momentum data.

Okay. So whether we like it or not now this is the slide I thought was coming up whether we like it or not, this is and effective is still the primary tool that ATLAS has for ruling out strongly produced SUSY, with invisible particles... R Paradis conserving SUSY. Here's the latest... well there are two latest plots with 165msb the so-called speaker bounds. The so called "inaudible" channel thing. This is where MEffective has only had the... This is the distribution of MEffective where you only add in the first two jet momenta, the two hardest jet momenta. You can do the same thing if you add in three... the first three hardest jet momenta and you get this distribution. All the coloured-in stuff, the blues, the greens and so on, have some kind of slope, and at the moment all the data points are landing on those slopes. Some putative signal, if it were there from RPC would come out as a bump - the type of thing I've been talking about earlier. That's just an example bump from a made-up... some particular model, inconsequential. And the point is, by looking for agreement between these standard model effective distributions and what we see, we have been excluding bumps. We've been excluding SUSY production.



Here, if you're interested, here is the... it doesn't show up very well does it? This is the highest MEffective event so far. If you want to go and jot down these numbers or read them off the handout when it appears, then perhaps you can come up with a fancy theory that's going to produce just these PTs.



So what I've done with this is basically said, if you had for the sake of argument a model... it's not a realistic model, just a straw model to present data in terms of, 'If you had a model that just produced, that could only produce, squark squark, or squark gluino, or gluino gluino, and nothing else. There was no other fancy particles in this decay process, and they can decay, only by... if it is a squark by radiating one jet to a missing neutralino, or if it's the gluino, by radiating two jets and going down to a neutralino, then in that framework, a very minimal heuristic framework, sort of big... the kinds of things that are still admissible are things where you've got big gluino and big squark masses.

But if you have either of them is low, where low means up to about 750GeV or a TeV scale, then that seems to be out. Now don't worry. That doesn't mean you have to stop all work on super-symmetry because SUSY is no longer below the TV scale; because this essentially says, 'That is the best possible, most optimistic ruling out you could do if the only things you are producing are the very things that you're sensitive to in the measurement.' If there really was a super-symmetry out there, only some fraction of the events would be producing the things with the signal here, sort of the signature that we're looking for here. There would also be thing with leptons in. They'd get vetoed. They wouldn't affect this plot. There might be branching ratios to more than two jets or three jets or things that would smear out the MEffective distribution. So don't worry.

[Student]

But when I'm looking at a plot like this, if I were to say, 'Okay, maybe only one mattered and it does this, how much "inaudible" provide?[26]

[Lecturer]

I suppose roughly speaking you could follow the contours of the physi-cross-section down here so that's... the cross-section across this space is going down by a factor of 10 each time you get to one of these dotted lines, so if you had about 100 times less, this vision would be, roughly speaking, following this dotted line.

[Student] "inaudible"

[Lecturer]

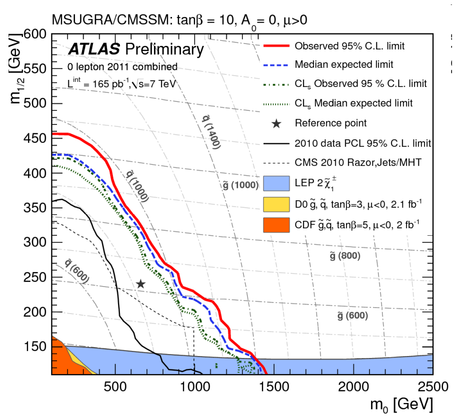
Yes. So the questioner says, in these models the neutralino is taken to be mass-less. That's correct, and that is also optimistic, because the smaller is the mass of the neutralino, the more energy is available to pop out from the decay

[26] This question is not audible on the recording. I suspect the questionner was asking whether it is possible to re-interpret the limits on the exclusion plot if you assume a smaller branching ratio from squarks and gluinos to the expected final state – eg if the squarks and gluinos mostly decayed to things that would not make large meff.

of the heavy to a light object. Why does ATLAS therefore produce in that model? Well, because... for one reason, we don't know what neutralino mass to assume yet, and there are bounds across but all those bounds are very dependent upon choices of model, so the bounds might come assuming MSugra or something like this, assuming some type of production. And so the bounds that there will be will change all the time, so for one reason... the basic reason that we have just a zero neutralino here, a zero mass neutralino, is because we can keep using this prop for a long while until, or unless, we start to see signals that upset us. What's the consequence of changing that assumption? The answer is, these sort of exclusions up here, at the order of a TEV, because the kinds of things you're sensitive to are mass squared differences, if you had a 300 or 200 DEV neutralino the kind of changes to the mass of your stuff would be of the order of 1000 squared minus 200 squared, as a fraction of 1000 squared.

So basically this end up here doesn't really move at all. I mean within the experiment, the experimental errors or where this thing could have moved if we have got our jet energy scale wrong or our luminosity, is more than absorbed by. Where it does make a difference is down at these bottom edges okay? So the particular analysis that we've got here is, in some sense, when you're very low down although the cross-section is quite high in this region, you are fighting with whether or not things are visible or not because... suppose we had a 200 GEV squark and gluino, and a 198 GEV neutralino, then the jets are coming out very weak and we wouldn't see them. We wouldn't trigger on them or we wouldn't see them.

So the bottom end of the exclusion, which is cleverly disguised and not on this plot but is available in the supplementary material at the backs of the available... the bottom edge could move quite a bit. But the very first paper that ATLAS produced, although there is some sort of leeway at the bottom, some sensitivity to that neutralino mass assumption, it's covered up to some extent by the fact that there are already exclusions down there. But I don't think ATLAS should rely on that cover up because the exclusions that already exist come making different assumptions themselves, which is why we insisted that the full contour is available. So I think that ATLAS is still... one of the things it wants to do once it has got bored with high luminosity is really start pushing the exclusion carefully down in this region and actively exploring these different neutralino masses.



Latest ATLAS 0-lepton, jets, missing transverse momentum data.

We were sort of forced, had our hands

twisted, and we had to produce the same plots, or the same exclusions in Sugra space, but I would encourage you to disregard most of these. Not because they are wrong but because Sugra is a very, very narrow model. It doesn't, for example, doesn't have a great deal of variation between what sort of achievable squark and neutralino masses you can have are. So although this is a perfectly valid exclusion, it's not really testing lots of different regimes that were used to generate these plots. There were actually many different variables. There were four types... three types of MEffective and an NT2 variable were used to generate the first version of this plot, and each was able to work well in certain regions. NT2 works well down here, when you've got lower luminosity and less pile up, and MEffective for the four jets works well up here, and three jets and two jets up here; because NT2 is a variable that we haven't yet come to and you'll see a bit more of later on in here, as a variable that is interested in pair production of light objects and "inaudible" two objects.

Anyway, let's get off that subject. All I'm trying to say is that if you mapped Sugra into this space it would only cover some small blob here. It might be over here or over here. Here I think. And so we are presenting this stuff in this format because it shows us what we are sensitive to and allows you to figure out, 'Does your model meet these expectations or not, or is it similar but with a reduced cross-section and so on.' But don't take this as a "inaudible" saying, 'Suzy is excluded up to these limits', because it certainly isn't. That's only if nothing else was being produced other than the stuff we were looking for. Okay. So that's an example of a Hotpants variable which you can still do a lot of stuff with and is only a minor change to missing transverse momentum.

Don't confuse simplicity with complexity … can layer add many layers of interpretation

Don't confuse simplicity with complexity, because, to give you an example now of something which, on the face of it, looks simple, but I would regard as being at the opposite end of the spectrum, being highly complex in some ways. I'm not saying it's a bad thing to do. I just want you to see a different picture.

Measure top quark mass from mean lepton PT only!

CDF note 8959

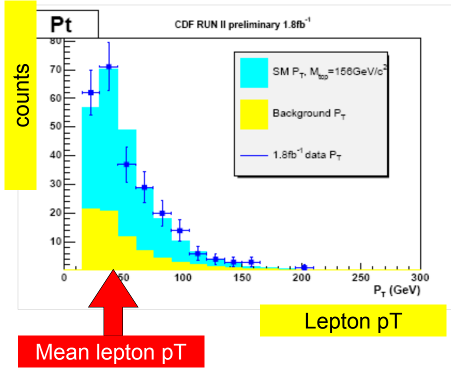
Measurement of the top quark mass from the lepton transverse momentum in the $t\bar{t} \rightarrow$ dilepton channel at the Tevatron

A new measurement of the top quark mass at 1.8 fb^{-1} integrated luminosity, using leptons' P_T in the dilepton channel is presented. A top quark mass of $m_{top}=156\pm20_{(stat)}\pm4.6_{(syst)}$GeV/c^2 is obtained with the Likelihood method and of $149\pm21_{(stat)}\pm5$(syst)GeV/c^2 is obtained with the Straight Line method.

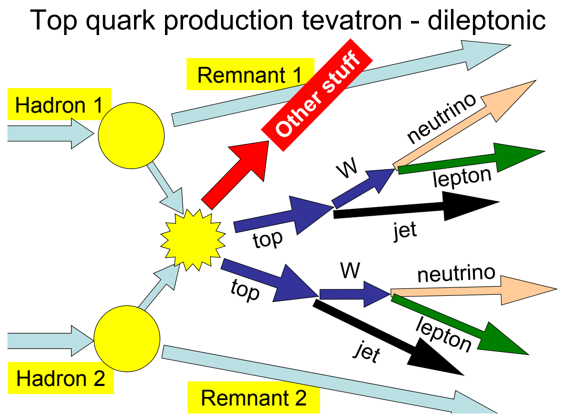
So take an example of this nice paper from CDF[27], or a note or whatever, where they said

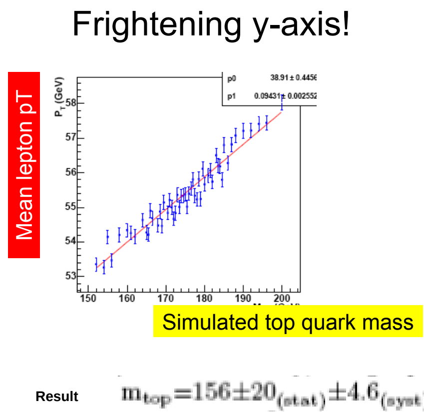
[27] CDF note 8959, found at `http://www-cdf.fnal.gov/physics/new/top/confNotes/cdf8959_DILMass_with_`

they had got lots of different ways of measuring top mass. They are interested in all of them. One way they have is to measure the top mass from Di-top events by looking only at the mean lepton PT.



It's like Meffective. It's a very simple thing to do. You've got your mean lepton PT distribution, and you have some background shape, and the signal sits on top somewhere, and we can read off the... and so there's many different lepton PTs you see, but the lep distribution has a mean and it's about here, maybe 45-ish GEV on that plot.





So what am I talking about here? So you've got your zap zap at the tevatron. It might make two tops and some other stuff, that upstream transverse momentum, whatever that might be. They're not interested in it in this context. They've got their two tops, okay? They go to WW and they're interested in deleptonic situation, so you've got lepton, neutrino, lepton, neutrino. And once they have selected these events they say, 'Well what lepton PT did we see?' I don't know if they add up, the two lepton PTs or put them both in the diagram. But anyway, they get a histogram of the mean lepton PT, nothing more than that really.

What do they do with it? They say, 'Well, we know, when the top quark is say 150 or 160 or 170', or, 'We've Monte-carlo-ed and we figure out that if the top quark has some particular mass, after lots and lots of Monte Carlo, we think that the mean lepton PT should be 54 or 55 or 56 GEV.' And that's... sort of a frightening access in a way. I mean look at the data that you might be getting and think where 51, 52, 53, 54 is and think how much you are reliant

Lep_Pt.pdf

on pretty much every single tool in your Monte Carlo and your detector simulation to tell you what's actually happening here. This is a tiny, tiny range and if you believe every part of your simulation then you can kind of extract from the mean that you see a top quark mass, by reading across and down. But they are honest, sensible people. They don't conclude more than they should from this. It's a good paper. They get quite big systematic errors, and the mass is also quite low, compared within sensible...

So the cost that is paid, the price you have to pay for doing this, is that you really have to trust all this stuff and so you have to throw in all the systematic errors associated with your simulation and production processes and that degrades your ability to get a sensible answer out. So that's an example of doing something that is simple, on the face of it, but actually requires you to believe loads and loads of stuff.

Moral

- You can monte-carlo anything.
  - example h->tau tau
- But do you trust it? Is it the best you can do?

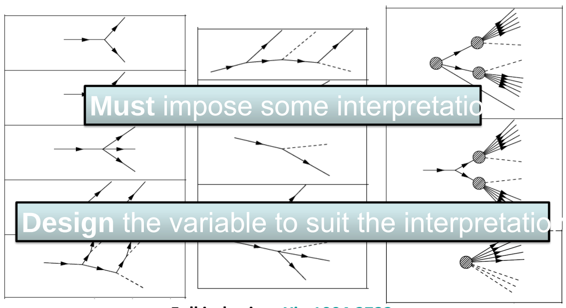
So what's the moral of this sort of story? Bear in mind, you can really Monte Carlo anything, but you should ask yourself do you trust it? Is that the best thing you could do or is there some other variable you should be using, or could use that would help you out a bit better, and perhaps make you less sensitive to effects you don't want to have to monitor.

More assumptions
Less Vague Conclusions

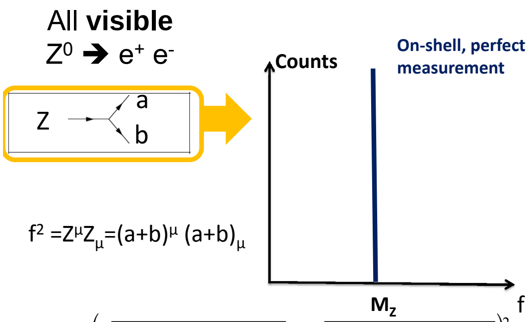
non-hotpants

So now let's go to an example of a variable where we start making more assumptions, not very many more, perhaps one more, and as a result we will get some kind of slightly less vague conclusion. So if you like this is a way... a slight move away from these. And it goes back to actually imposing a typology or a hypothesis and trying to make that variable fit that hypothesis.

Topology / hypothesis

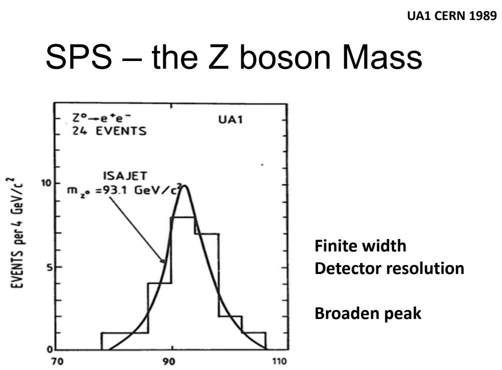


Full index in arXiv:1004.2732



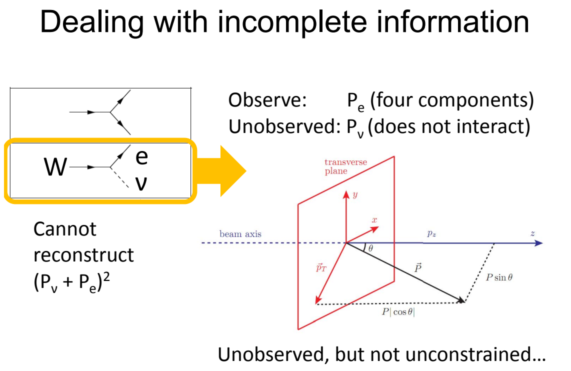
$$M^2 = \left(\sqrt{m_a^2 + a_x^2 + a_y^2 + a_z^2} + \sqrt{m_b^2 + b_x^2 + b_y^2 + b_z^2}\right)^2 - (a_x + b_x)^2 - (a_y + b_y)^2 - (a_z + b_z)^2$$

So the ones that... forgive me, but for the sake of talking about it in this way it's helpful for me to show this: so the example being, if you believed, if your hypothesis was, you had a single particle that became two visible ones A and B, such as Z goes Eplus Eminus, then we know that the former momentum of A plus B it's modular squared should be modulo width of the Z, modulo resolution effects, the mass of the Z well mass of Z squared, you can square root it, and get your Z boson mass peak.



28

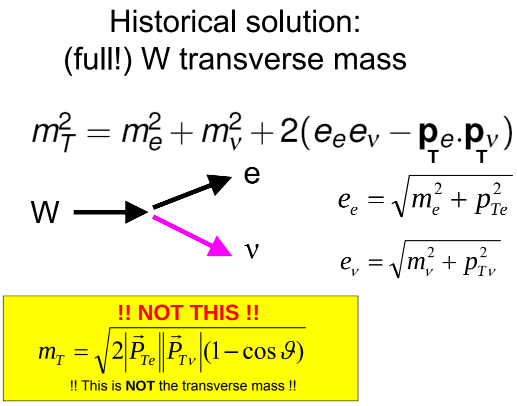
That's from UA1 I think, when they were discovering the Z boson [4]. A nice example of something we can do when everything is under control.



I won't dwell very long there because the kinds of challenges that the LHC faces for spotting new... for measuring the masses of these new particles, as I've said, is that there will be invisible particles radiated. So the W, the case of the W going for electron neutrino is the simplest standard model example that actually has almost all of the technology behind it to describe what we actually do in SUSY, even though the difference here is that here we have a single particle. Ws don't have to be produced in pairs. They don't have to have two of these neutrinos in the way that you might in an R parity conserving super-symmetric model expect to have two chains of particles decaying with two visible guys at the end. And the situation, the problem we face here, as we've mentioned earlier, is that this time we can't do the same thing. We can't take the former momentum of E, add it to the former momentum of the neutrino and take the mod-squared of that thing, because we don't know the momentum of the neutrino. We don't know it but it's not free. It's not totally unconstrained remember. We've got the transverse components of it from the missing momen-

[28] Plot from [4]

tum at least if we're assuming that there is only one invisible particle. And what do people do? What have people done?



Historically what people have done I really mean sort of historically, this is going back to the 60s/70s they said what you should do is construct the transverse mass. If you write it out in full, the transverse mass of a system where you've got two particles being produced is the mass of the particle that you saw squared plus the mass of the particle you didn't see squared, or if you don't know it you'd have to make some kind of guess, and for the moment here I'm going to assume that we know the mass of this invisible guy. Later on in the lectures we will start removing that assumption, but for the moment, we're just going to say, 'Oh, God has told us that it was a neutrino and a neutrino mass is blah.' Right?

So you've got to add those two things together and you've got to add on twice the product of these little energies, that's these things over here, these so-called transverse energies. The transverse energies are what you might... they are to ordinary energies what you might think if you had just magically thrown away the Z component; but I hope you will see later that they are not really obtained in that vague way of just throwing away Z components. There is a very good reason to use them. We'll see that in a bit. The product of those transverse energies, and this is something that we can compute if we know the mass of the neutrino and we do know its momentum, its transverse momentum squared. These are just the x and y components. We know these things and we have to take away the dot product of the transverse momenta.

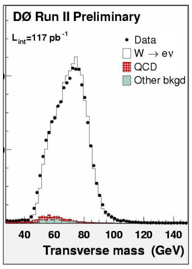
A lot of papers, including those early papers, in fact write down the transverse mass in this form.[29] They say it's the square root of twice the modulus of these transverse momenta times one minus Cosh Theta. That's because this formula reduces to that if you set the mass of the electron to zero and the mass of the neutrino to zero, because then these tranverse energies would lose their mass and they would just become moduli. So remember that that is not the transverse mass. It's valid to do that if you're working with Ws but you'll get nowhere in SUSY or beyond the standard model mass measurements if you start using this thing. You've got to use that thing at the top.

[29] [Here I am pointing to the yellow box on the slide labelled "!! NOT THIS !!".]

W transverse mass: nice properties

- In every event $m_T < m_W$ if the W is on shell
- There are events in which m_T can saturate the bound on m_W.

motivate m_T in W discovery and mass measurements.



But where did these properties come from?

And I gather that Lian-Tau Wang may have given you one lecture or some lectures that have talked about this a bit. I'm not quite sure what he said, but I would emphasise that one of the things that emerges from the transverse mass is that for every event it should be less than W and you can get events where the transverse mass can reach up to NW, so you should get some kind of distribution that will be smeared a bit by the W width and by resolutions but basically it will be one of those things that gums up and stops at the mass of the W, and that motivates its use in measurements of the W. Where did these properties come from? Let's look a bit more closely at where they came from.

Re-examine invariant mass: M→a b

$$
\begin{aligned}
M^2 &= \left(\sqrt{m_a^2+a_x^2+a_y^2+a_z^2}+\sqrt{m_b^2+b_x^2+b_y^2+b_z^2}\right)^2 \\
&\quad -(a_x+b_x)^2-(a_y+b_y)^2-(a_z+b_z)^2 \\
&= (E_a+E_b)^2-(a_x+b_x)^2-(a_y+b_y)^2-(a_z+b_z)^2 \\
&= m_a^2+m_b^2+2(E_aE_b-a_xb_x-a_yb_y-a_zb_z)
\end{aligned}
$$

$$
= m_a^2+m_b^2+2(e_ae_b\cosh(\Delta\eta)-a_xb_x-a_yb_y)
$$

where $e_a=\sqrt{m_a^2+a_x^2+a_y^2}$, $e_b=\sqrt{m_b^2+a_b^2+a_b^2}$ and $\eta_a=\frac{1}{2}\ln((E_a+a_z)/(E_a-a_z))$, $\eta_b=\frac{1}{2}\ln((E_b+b_z)/(E_b-b_z))$, $\Delta\eta=\eta_a-\eta_b$

Go back to the ordinary invariant mass, not the transverse mass. The invariant mass squared you can write down and this is an exercise if you haven't done this that I would recommend that you have a go and try and do you could rewrite the common-or-garden, honest-to-goodness, invariant mass of two particles, A and B, as the sum of their invariant mass squared, plus twice the... well, this part here is almost exactly what we had in the transverse mass except that there is a cosh of the rapidity difference between those two particles in there. So let me put those side by side.

Comparing invariant and transverse masses:

$$
M^2 = m_a^2+m_b^2+2(e_ae_b\cosh(\Delta\eta)-a_xb_x-a_yb_y)
$$
$$
M_T^2 = m_a^2+m_b^2+2(e_ae_b-a_xb_x-a_yb_y)
$$

Since $\cosh(\Delta\eta)\geq 1$ have $M_T\leq M$ with equality when $\Delta\eta=0$.

(Not same as throwing away z information!)

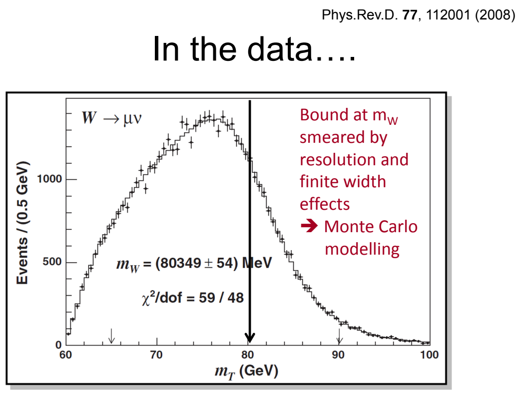
But have bound, and bound can be saturated.

Note that at this point we are assuming we know m_b.

The actual invariant mass is this thing at the top. Transverse mass is the same, but without the Cosh Delta. And what do we know about COSHs? Hyperbolic cosines? We know that they are always bigger than one, at least if you've got real rapidity differences and I hope we certainly do.

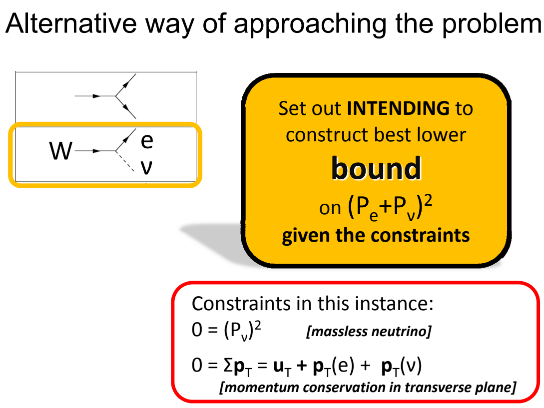
So because this is bigger than one, that means this thing is definitely going to be bigger than or equal to this thing, and you'll get equality when the rapidities are equal to zero. And I want to emphasise that. It might seem a silly thing to emphasise but I really want you to realise somehow that it is not... the way one goes from the mass to the transverse mass is not at all by just throwing away dead components. It's not about lack of knowledge of Z components. The transverse mass is really about lack of knowledge of... which is two things. There are two dead components. It's about throwing away one thing. It's about throwing one thing away; the lack of knowledge of the rapidity difference between those two particles.

If you mistakenly think, 'Ah, well transverse masses are just about constructing things by throwing away Z momenta', then you get into a lot of trouble when you have multi-particle systems because the invariant masses, if you've got something decaying into a lot of particles over here and a few invisible particles, then a lot of the mass bounds that you can get come from the invariant masses of this compound system of many particles and that needs to know about relative Z momenta inside it. But ultimately, the transverse mass will depend on the rapidity difference between net collection of all of these objects with all those Z momenta in, and the net momentum of the invisibles over on the other side. So we'll see perhaps why this matters a bit later. So we're not throwing away that information. But nonetheless, we do see that this thing is going to bound, the transition is going to be bounded by MW, and so, as we've said, if we plot events, they will appear. We will look for an endpoint in this distribution, and we will be seemingly measuring the data we, of course, even to this day get the best measurement of the W mass and width from plotting this distribution at the tevatron.[30]



[31]

Now I want to turn that around. There we looked at the thing. We said this variable is bounded above by MW. Why don't we ask a different question, although it's very, very similar, which is: given what we see in an event, given the missing momentum that we see and some visible particle, the electron, can we ask ourselves, what is the greatest possible lower bound we could make on the mass of our parent particle that led to those things existing, the best possible bound that we can construct, subject, needless to say, to the constraints we wish to impose?



It makes sense that it fits our hypothesis and

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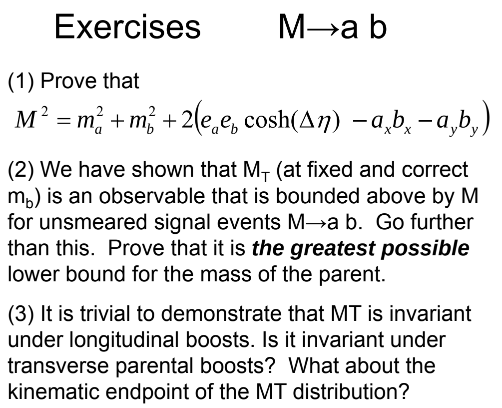
[30] It was pointed out to me that this statement is debatable – depending on what one means by "measurement" – direct or indirect.

[31] Plot from [5]

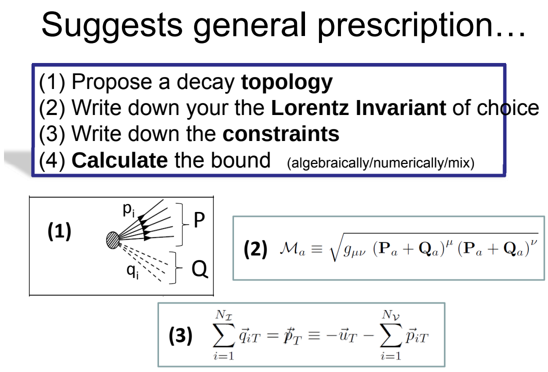
our hypothesis might be that neutrinos should be mass-less, and the hypothesis might include the fact that there should be no other visible particles so that the momentum of the neutrino added to the momentum of the other junk and electrons should add up to nothing.

And it's not guaranteed a priori that you're going to get the transverse mass out of that. Okay? Because this is asking a different question. The transverse mass that we worked out we know is below MW, because COSH delta eta is bigger than one. But we don't know if there is some fancy better variable that you could use that is able to sneak a bit higher in events where there is a non-zero rapidity difference between objects. Now, you in fact do find that the transverse mass is what you obtain from this. If you answer this question, what is the best lower bounds you can place on W mass, you get a transverse mass. And I would recommend that you have a play with that.



One of the exercises I've suggested is you should first, as I've said earlier, check that this formula agrees, and actually go away and see if you can prove it's not too big a proof but it is a thing that you've got to do and think about to demonstrate that this is a best bound.

If you want to play around with other things to test what's going on you can trivially see that the transverse mass must be invariant under longitudinal boosts, because it hasn't got any Z components in it. So you can boost the thing down with Z access, nothing will change. But the transverse mass is not invariant under transverse boosts, sideways boosts, and you might want to go away and prove that, but remember that the end point is supposed to be MW. We have said it is, and that was for arbitrary other junk, arbitrary upstream transverse momentum. So, for some reason, the endpoint of this distribution, the highest bit, must be invariant even though the rest of the distribution is around when you start throwing these events sideways, so you might like to prove that as well, see where that comes from.



But anyway, what this does is, this suggests for us or motivates sort of a general prescription. It says that maybe one of the things it's not the only thing you could do and it's not always the best thing you could do but one of the things you could do, when you're trying to track down New Physics events, is make your hypothesis about what you think is going on, and then set yourself the challenge of constructing... of deciding what it is you want to measure. Is it the mass of some initial particle? Some Lorentzian invariant of choice? It could be, in the case of the W it's the W mass, and then set yourself the task of calculating the best possible bound you could make on... lower bound on that mass you're after, subject to all of the constraints that you're willing to hypothesize as part of your mindset, your paradigm, how you view the event; because we know that gives us the transverse mass for W and we know that that's very good so perhaps it will give us good things in other cases.

Why might we suppose... why might this give us good things? And it comes down to this business of the bounds. If I'm trying to look for a particle Higgs goes to Tau Tau to some visible bits, so it could be lnu... Sorry it is not lnu... some visible things and some invisible things, I would like to be able to select events that have got this Higgs to Tau Tau to stuff invisible stuff and visible in. What's my biggest background? My biggest background might be z to Tau Tau to invisible stuff, invisible stuff.

Now if one has got a magic variable that tells you, 'I'm looking at this event and this event could not possibly have come from a particle that was heavier than something', okay? It gives you back a number, then you know that all of your background events for the dominantly Z to Tau Tau, must come back below MZ. Yes? They have to be below MZ, subject to widths and detector resolutions and smearing. On the other hand, the Higgs, if it's heavier than MZ, well it could be... the bound you get back, when this event appears, well say that event is only consistent... the particle that produced this visible final state cannot have been heavier than... and the answer could go all the way up to MHiggs, the Higgs mass. So what's that?

So in other words this bounding thing: well, background distribution might well fall down and stop at MZ, whereas your signal distribution, Higgs that you're trying to look for will have some analogous thing but will go to MHiggs, and so if you're able to place a cut somewhere here, this exact position would be motivated by your smearing, your ability to detect how much this should leak up. Then you might, in an absolutely ideal world, get a completely background-free region. And that's one of the reasons why bounds are good. Bounds aren't the only thing. They are a thing that I've looked at a lot of, which is why I'm talking about them, but you shouldn't assume that I think that this is the only thing that is interesting or worth considering. But it certainly motivates why it worked well for the MZ, for the Mass of the W,. and it's why, a lot of the time, it's been hypothesized to work quite well for the other things.

Let's see. Maybe this is a good time to stop. I'll stop here because I don't want to break the next topic. Okay. Thank you.

[applause]

1.1. Lecture ends, and discussion begins

[Member of the audience]

So most of your talk is, you've got to be wary of experimentalists, and then you have this break in the middle where you're showing us ATLAS data with things that people care about because they need their PhDs depend on it. So I have this tremendous problem with dissonance because most of your talk is about how we can't trust anything and then drawing us pictures, so what am I supposed to take away?

[Lecturer]

I think the thing that I want you to believe is that you shouldn't trust the experimental people are doing the *best* thing. Okay? I have not said, and I wouldn't present to you any evidence that suggests that you shouldn't trust that what they are doing is not *valid* within the framework that they are working in. So in other words the limits that the experimental collaboration has produced are, in my view, likely to... you can believe them, in the context in which they present them, but what you should bear in mind is that just around the corner may be some experimental person who has finally managed to get his code to compile after six months, and is emboldened to go and actually try and do something better and all of a sudden they may rule out far more of the stuff that your students are working on. So you [theorists] absolutely have a vested interest in keeping us [experimentalists] from making improvements [to our analyses and techniques so as] to keep as many of your crazy models viable, unless, of course, you take the view that it's good to rule out the competitor's model and focus on yours.

But yeah, I don't think that they[32] are doing things that are wrong. I just think that they're not always doing the best thing.

[Student]

Why is it that every collaborative effort shows observed limits that are better than the expected?

[Lecturer]

Well, it should be half the time! I mean there's... I don't want to get drawn on that, but for example in the paper that we produced in February, the earlier version, like this for example, had the MT2 and the MEffective in, there the bounds came entirely from the MT2 and the MEffective search reaches. MT2 was there to look for disquark production, the single jet on each side, the leptons, and the MEffectives were there for the ones with more jets on each side from the gluon production processes. Now the MEffective distributions are all correlated with each other. The three that there were, or had in the background, were a two jet MEffective with one threshold, a two jet MEffective with a different threshold and a three jet MEffective. But basically they're all MEffective. So if

[32] [experimental collaborations]

one of those MEffective distributions goes high, then the other two almost certainly do as well. They're very, very closely related.

On the other hand, the MT2 distribution is very unrelated. You're looking at completely different events in a completely different way. So basically although we had four things, and you would see four plots there, basically there were only really two, in some sense, two very independent ones. In the MEffective we had a deficit, which leads to an excessive events in the data over what we predicted. Sorry, the other way around. The one that pushes the actually observed reach beyond the expected reach. On the other hand, in the MT2 plot the reverse was true. We saw the opposite thing: whatever it is, deficit, ra ra ra, excess, that pulled the thing the other way. So the final boundary, when you... the analogue of... am I going to be able to bring it up. Yes.[33] So that if you look at the February results, the observed dips down below the expected just in the region of this end of the plot where MT2 was providing the expectation. We didn't get the expected reach because we got an undershot of events.

Okay. So I mean... we've now got more data. This data doesn't MT2 in it, because the luminosities we've got at the moment, we haven't figured out how to coster the two jet-fragmented systems up together into CO jets in a way that we're happy with yet. So we've only got MEffective in these plots which means there's only really one distribution again, even though there's four in there. And so the fact that it happens to have gone a bit high, well again, it can happen. CMSs have gone low. That can happen too, and I don't feel that they should be... that doesn't show that they're doing something wrong. Eventually, after lots and lots of data, we might one day find that we've underestimated our background and they have underestimated theirs, or... things like this, but we're not at the stage that we can tell that yet, because at present the excesses are sort of only one sigma type excesses or deficits at the level of our own experimental uncertainties.

So I think it's... these sort of things aren't things that people, from CMW or anywhere else, should really worry about too much at this stage.

2. LECTURE 2

Yesterday, after the lecture, one of the organisers took me aside, and offered me some advice. He said, "Look, this is Boulder, not Cambridge. It's [insert hot temperature] degrees. Lose the tie." This got me thinking. In Cambridge, if you're giving a lecture course, you have to show the students that you respect their attendance. It's the quid-pro-quo. If they are able to turn up promptly, the least you can do for them is dress properly. They expect nothing less. Were I not to wear this tie, I would assume you would inder that I don't care about you! But then I thought:

[33] here I re-show the ATLAS 0-lep exclusion slide

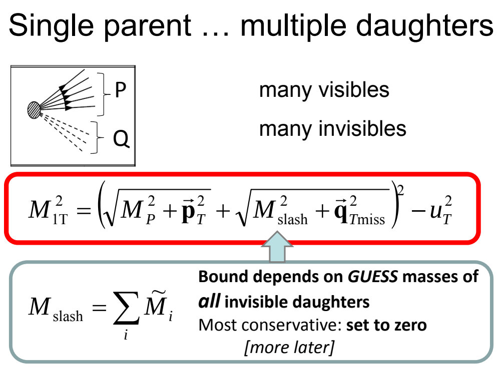
"Am I making a cultural error?" You *have* come here 9 o'clock in the morning. Yes and to be honest, I concluded he was right. I think I *am* kind of riding rough shod over these cultural differences, sort of 300 years of post-colonial stuff here. Who am I to do all that?. If I am going to get respect from you and show you that I appreciate the fact you have come, I *should* take off that tie!

[Applause and cheers]

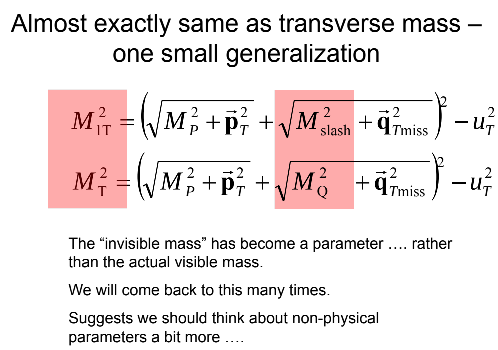
[Alas, at this point, the intended dramatic and flambuyant guesture of ripping off my tie, turned out to break the microphone into two pieces.]

[Applause and laughter as audience watches failed attempts to repair microphone.]

Except it has broken the microphone. How about if I shout louder, then I guess the recording thing pick this up. [resetting microphone].



Last lecture we were just at the point of trying to figure out: How do you bound the mass of an object that decays to some number of visible and some number of invisible particles? We have done this when there was just one invisible particle down here, and the answer was the transverse mass. The only difference is now we have added in more than invisible particle. If you go through the same procedure as before - the sort of four steps, saying: I want to bound this mass, my constraints are that it should match this picture. I don't know where these things went. What is the maximum upper bound I can get? Then you get this formula here: I mean people have written in different ways, there is more than one way of writing it, but this is one way of writing it. It is basically almost the same as the transverse mass but with one difference, it has this parameter in here mSlash. I would have done that with a sort of slash through with PowerPoint knew how to do that. What that is, is that some of the mass of the invisibles are here. When we were doing this for the transverse mass - What did I say we had to put in?



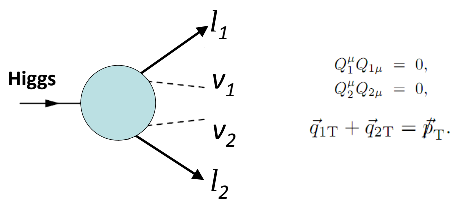
The formula transverse mass and the other bits that you have seen there, but you had to put the mass of the invisible particle in that part, whereas here... Analogously you might think that we put the invariant mass of the three invisible particles, that are heading out. But we don't know what their invariant mass is because we can't see where they are individually going, and it turns out that when you try and construct this bound the right thing to put in there to get the bound is just the sum of the mass of the invisible particles. Because effectively the bound occurs when they are collinear.

So not much difference. Of course we still don't know whether we know this, at the moment I am assuming that we know the mass of the invisible particle, it is part of our hypothesis, and now for this M1T... One for 1 parent, T it is a transverse mass erm... It may well be the case that we simply don't know what the mass of those invisible particles are so we might have to set this to 0 if we want the most conservative bound. Okay. We are trying to measure the mass... bound the mass of this object here. If you really don't know the mass of these things here, the most conservative bound you would get, would be by setting this MSlash parameter to be zero.

Applications of M_{1T}?

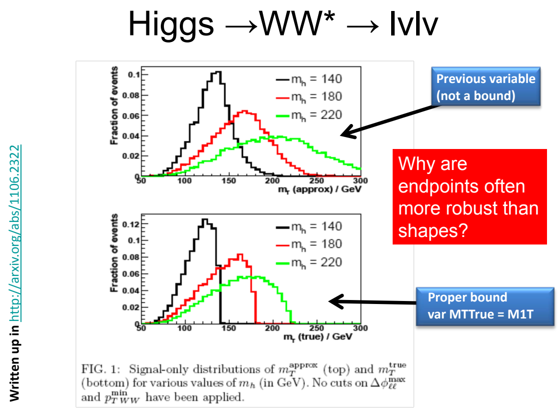
We will come back to that later, and whether you still... Why you might find that relevant. So let us have a look then. So what are the applications of this slight generalisation of the transverse mass?

Higgs →WW* → lvlv



Written up in http://arxiv.org/abs/1106.2322

One example would be something like the Higgs, to ww start at lnulu. So you have got Higgs coming in, it is going through a pair of W's and it has decayed to a final state that has got more than one invisible particle and more than one visible particle. The W's... Well if Higgs is light enough, the W's can't both be on shelves, so you kind of... it might be that the first assumptions you would make is if you just assume that is some kind of blob that you cannot pick apart inside there. And so we can apply m1t to this. In particular we know that neutrino is a mass less, or nearly mass less, so we may as well accept m/ to be 0 and work with this. If you do that... Here is an example. This is just Monte Carlo truth so you don't have to worry about smearings and things like that, to see what is going on.



[34]

On this axis here [35] [6] we have got this M1T, or here it is called mT(true), but it is the same variable. If the Higgs was a 140 GeV Higgs say and then you have this distribution [36] on that variable that has an endpoint at 140, as we have said that it should. That is the whole name of the game. On the other hand if you had a heavier Higgs your distribution would go up higher and stop.[37] If it was the 220gv Higgs and so on it would go higher and stop. [38]

Is this useful? Even though that is such a trivial thing to want to do, about two years ago, at least one of the LHC collaborations ATLAS, possibly others as well, I haven't looked quite closely, was in fact using this other variable [39] to... for the Higgs to WW events, the idea was to plot a variable which - what do they call it? - we called it mt approx when we were sort of trying to persuade them that they shouldn't do it. What was the mt approx?, I forget the exact details, but what it basically was... by chance it was... it was this m1t variable but with mSlash not set to be 0 (the sum of the mass to neutrinos) but set equal to... I think equal to this, the invariant mass of the two leptons! I didn't come up with that choice. I don't want to make wrong statements about how it came up, but I think probably what was going through the mind of someone was, they probably came up with... They thought a transverse mass is good to use, but I don't know the invariant mass of my invisible particles, I should put something in there. Mm. What can I put? I don't know leptons are quite light, neutrinos are quite light. Probably there is some correlation between the neutrino invariant mass and the visible system invariant mass, so let's just stick in something. We will put invariant mass for the visible things we can see. This got mT(approx).

So if you do that, okay then you get this variable here, [40] which is no longer a bound, because you haven't stuck to the rules, not that they were trying to stick to those rules at the time. The point is that if you are trying to search in data, where there is also background distributions here, of lighter values from background sources. You are much better off if you are trying to search in a variable which has a sharp cut. Why? Because if you imagine, across this space here some... This is truth level, but at detector level cuts would be applied to have

[34] Plots from [6]
[35] [poinring at the lower of the two sets of histograms]
[36] [pointing to the lower black histogram]
[37] [pointing to the lower red histogram]
[38] [pointing to the lower green histogram]
[39] [points now to the **upper** set of histograms]

[40] [points again to upper set of hitograms of mT(approx)]

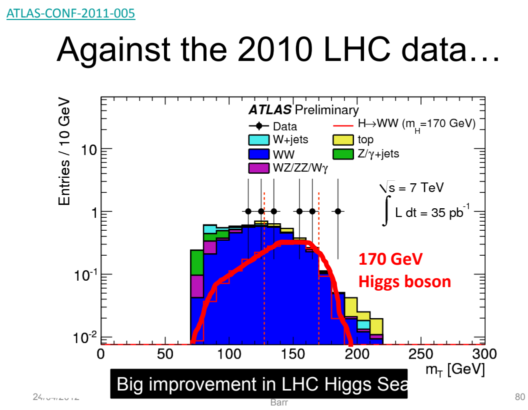
kept to these plots. So these shapes would be modified a bit. It might be harder to see bits down here [41]. It might be easier to see bits over here, so effectively these distributions after cuts and acceptance and corrections and things like that, have their shapes changed a bit? Each bin is sort of multiplied out by a bit or down by a bit. But when you multiply 0 by 0 you get 0. When you multiply a positive thing by a positive thing you get something positive.

So actually a Heaviside step-function is easy to see, or easier to see even if you have some kind of modulating acceptance over it, that is something which is smoothly falling. Over here you... So er... So basically the fix [42] is quite simple, just comment out one line of code [43]

[A student ask a question about whether the spin an helicities of the higgs, W bosons and leptons lead to the leptons tending to be mroe collinear than phase space alone would suggest.]

I don't know. I remember when... I have forgotten the answer to your question, I do not know the answer off hand. But I remember the Higgs... the spin of the system... the spin to the particles involved in this system were such that if you were do this with scalars everywhere. If you just looked at the face based distribution of the same quantity for a scale of Higgs going to 2 scale W's, then you don't get as much stuff near the endpoint as you do when you use the right spins. So the fact that it is peaking quite nicely near the endpoint, despite the fact there is missing information, is certainly helped a spin correlation between parts of decay, pushing things into a particular configuration. I don't remember what that configuration is for sure. It is almost certainly with the two leptons going in the same direction.[44]

I think that this is suppose to be a positive story because we fired this research on M1T into the ether and, to our surprise (because we haven't done any sort of test simulation on this kind of thing to see if this is going to break, if you put it through a realistic detector) ATLAS ended up using it for the Higgs to WW to lnulnu analysis!



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And ATLAS still uses it [46] What they call mt here is the variable I have just described [47]. So at the moment we still can't see a signal from Higgs to WW, it is very faint, I think I can make

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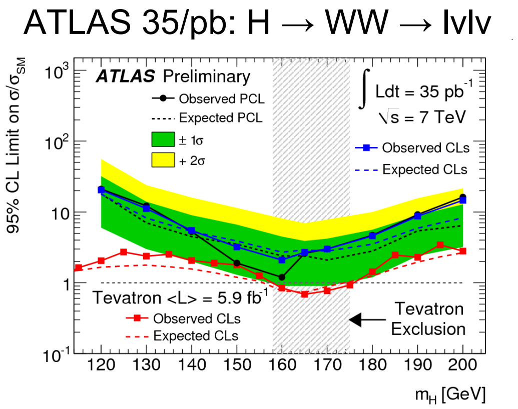
[41] [poointing to the tails]
[42] [for mT(approx)]
[43] [the part setting mSlash to something other than zero, and you turn it into M1T, which has a well defined bound and a sharp end-point.]
[44] Indeed this is the case.
[45] Figure from ATLAS-CONF-2011-005
[46] [M1T]
[47] [as M1T]

it a bit bolder. That sort of standard model type Higg signal there sitting on the background distributions. And the fact that this has some kind of steppish edge, buys you some time. It means you can... You know you can see this Higgs in this channel some number of months earlier than you would, with less data than you would need, if you were trying to look for something that was more splayed out.



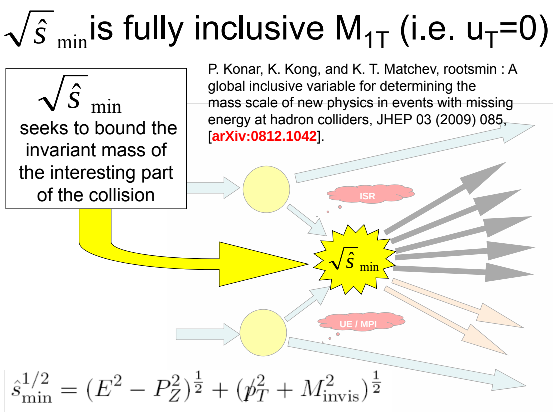
[48]

I think the current ATLAS sort of exclusion plot. So this is a... You pick a Higgs value, a Higgs mass, do you think a Higgs is a 150gb, if you do this is the current... the slice up through this plot shows you at present that ATLAS at 95% confident, sort of ruling out Higg's that have got about twice the production rate you would expect from a standard model. But this little black line comes down here, this is ATLAS's preferred thing, not because it is deeper, but because it is the one that they wanted to use until other people said "You have got to put on this blue, different type of calculation as well". Now it is nearly approaching the standard model cross-section there. I think when you get... We have got a lot more data than this now, so when eventually the Higgs to WW note comes out, er the next conference note that replaces this, you might expect - looking at this plot - that if we are lucky, some bits of Higgs masses are going to be excluded by ATLAS. Maybe not. It depends on how the fistula fluctuations go and whether there is a Higgs.

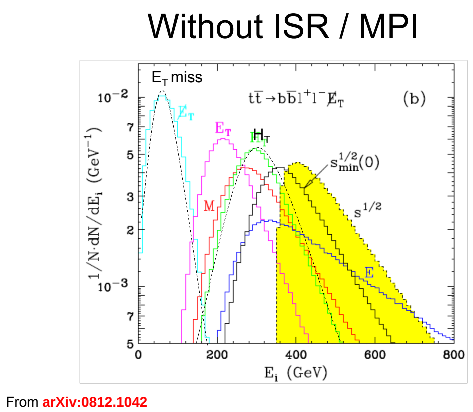
Other applications of M_{1T}?

What else? Applications of this just plotting the transverse mass median. So Konstantin went back... really one of the first people to actually say "Why don't we start using this variable for things?" was interested in getting hold of a mass scale by saying "When you have got your hydron collider it has got some collisions, you make an initial state that then might go into your squark, anti-squark. Why don't we try and bound, not a mass, but bound route s hap, the centre of mass energy of the interesting part of the collision that went onto to reduce the stuff that you saw in your detector.

[48] Figure from ATLAS-CONF-2011-005

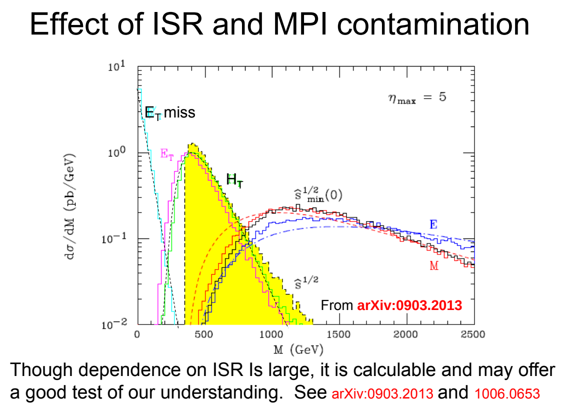


At the time, the first situation is this, the idea was: "Well why not just put in absolutely everything in? Effectively fit all the visible particles and all of the invisible energy, and all of the missing momentum that you see into an event." Which is effectively the same thing as saying there is no other stuff. You choose the upstream transverse momentum, the other junk is non existent. In which case you get this formula here, which is the same as you had before for m1t but without the -c. So you could call that fully inclusive measurements of those things and er... you get some nice plots.



49

So the real root-S-hat [7] distribution for... If you are peaking to truth would be the sort of yellow curve, which was this... This was a tt bar production, so starting at a threshold of twice the top mass, it can be higher, and the black curve which is at a reduced position is this root-s-hat min bound. The bound you will get on an event by event basis asking what sort of energy was there in that centre of collision. So you could use that as... if you wanted to try and get a handle on the scale of what is going on. There is some distribution sitting here around twice the mass at the top, so you could divide it by 2 you would have a means of testing, of getting a handle on the mass scale, with very few assumptions. And that is good. Fewer assumptions means robust conclusions, but maybe not very specific conclusions. On the other hand you should always be careful, and I am not picking on this particular variable, it is a good illustration of how you must make... bear in mind, that the best made plans can sometimes be affected by you feeding in the wrong stuff. [50]



51

So what happens if we feed in wrong stuff?

49 Figure from [7]

50 See also [8] and [9].

51 Figure from [8]

What do I mean by wrong stuff? The point of this root s exercise it is trying to tell us what we think of its collision. We don't want it tell us about the energy that was in the outgoing protons or MISR or for FSR, for multi-particle interactions and things like that, outside here. We don't want it to tell us about that.

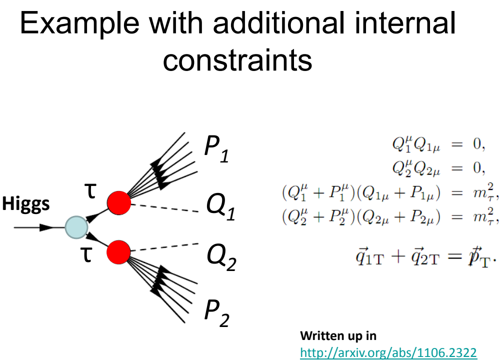
Moral

- Remember our variables are always limited by what we feed them
  - (garbage in garbage out)
- May need alter variable in light of pathologies
  - Try to locate the subsystem that lacks ISR/FSR, e.g. by using reconstructed objects with pt thresholds
  - This takes away u_T=0 requirement, and gets us back to M_{1T} (a.k.a. "subsystem root s hat min")

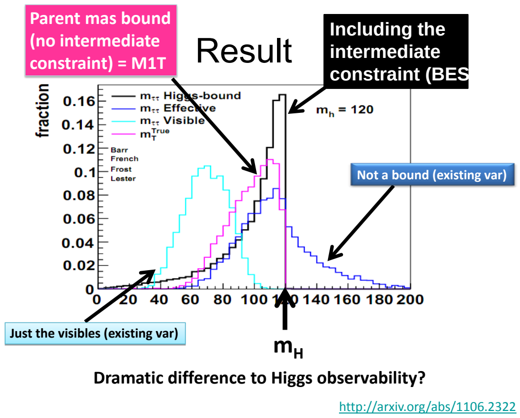
So if we do put absolutely everything in, as is happening, and have lots of initial stage radiation of particle interactions then our root-s-hat distributions might be become huge. So we have lost that bounding property. But that is okay because we have just put the wrong stuff in. So you could go back and say 'Okay if we put garbage in we get garbage out' but let's just alter what we are doing. You could say 'Well I will just choose to isolate the bits that matter.' Maybe not put in all the of the visible matter that I see, but just put in the reconstructed particles or the ones in some repetitive region, or those above a certain threshold or something. Trying to get a handle on just those that you think really came from the centre of the collision. Effectively that is removing the $u_T = 0$ requirement, you are putting other stuff in and then we get back to m1t. Yes.

[Student asks a question that is not audible on the record, and the replies and further discussion with the students makes no sense one-sided. For this reason, the extended discussion has sadly been deleted here.]

My point here is just an illustration of when things work very well... When things meet your hypothesis, and if they don't then they will work less well, but you can adapt to make sure that them things meet that hypothesis.



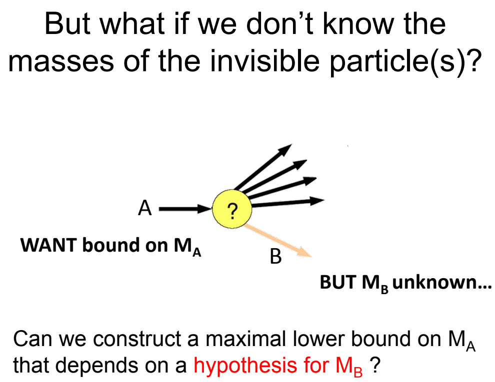
Other things you can do. Up to now we have had just this stuff where we had a blob in the middle, we didn't care how things worked inside that blob. When Higgs went to WW, we didn't ask what was going on inside. We just said we have got some visible final statements, and some invisible final state particles and we have solved ptmass. No reason why you couldn't add internal constraints, if you had something like a Higgs to tau tau [10]. Those tau's should be on show and you have got some invisible and some visible particles there. So this time you won't end up with a variable m1t, you will get a different variable because m1t has not imposed in the bound construction any of these internal constraints. If you do, you will get something new. And because you have got more knowledge about what is going on... You have said I really know, or at least I am hypothesising. I am hypothesising that this thing has got these mass-shell constraints inside. You are effectively asserting you know more about the event, and so your distribution of the variable - here in black -



[52]

that you would get from imposing that constraint. Certainly at truth level it would be much more peaked than the kind of ones we have been seing before. But, of course, this is going to be far more fragile than the sort of lower tech variables which are the sort of fainter ones that you can see, which are the ones that ATLAS use at present to do the... ATLAS use two variables at present to look for Higgs to tau tau. This blue one - which is not a bound but it has got sensitivity clearly to the Higgs mass. It is sort of peaking at the Higgs mass. Then there is a faint one you won't be able to see here in cyan, which is just a mass of the visible... invariant mass of the visible particles. Again they are like sort of hotpants variables. They have got sensitivity, and in fact they may turn out to be the best things this time, because they won't be so fragile as this thing,[53] when you impose the internal constraints. I suspect this nice peak here will become horrible by the time we add in a momentum detector resolutions.

Enough of mt for a bit. You must be quite sick of the transverse mass, but your pain is not over because there is even more transverse mass stuff to come. Nonetheless a change of topic.



We have been talking about measuring the masses of this particle [54] that was produced here from its decays, it had a visible object in it. What if it were actually the masses of this in-

[52] Figure from [10]

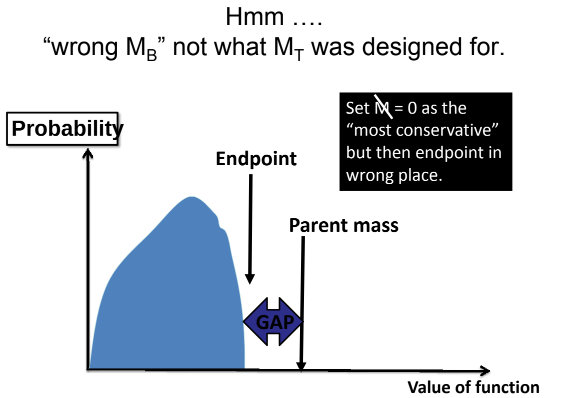
[53] [pointing once more at the black curve]

[54] [pointing at particle A]

visible particle [55] that we were trying to find. Because up till now I have just said we know the mass of B, or we are pretending we know it. It was a neutrino or something like that. What if we actually want to measure this mass of this invisible particle? After all that is one of the whole purposes of the hadron collider, we are supposed to be looking amongst other things for dark matter. We want to measure the masses of the invisible particles. Have we got techniques that we can use to find them?

So that is what this topic here is about. Now you can talk about that in various ways. If you will take my word for it, for the moment, that it is fairly easy to get mass differences out. The mass difference between this and this is readily extractable by a variety of different methods. The search for the mass of this could be called 'The Search for the Mass of the Invisible Particle' or it could be called 'The Search for the Absolute Mass of this'... not just the mass difference, but its actual mass, or to absolute mass scale.

A lot of the time here I am going to talk about finding the mass. This is us wanting to place an absolute bound on the mass of this, when that is unknown. So perhaps our first thought that might occur to us, is: Can we try and place bounds on this for different hypothesis for this? Up until the present we have always just had the right value for that mass, for that b. What if we put in different masses, masses that aren't necessarily right, or we don't know whether they are right or wrong for the mass of the invisible. Well one thing we might expect straightaway to happen is if we put in a mass that is too small, our nice bounding variable is no longer going to reach the endpoint that it is supposed to. It will be shorter. There will be some kind of gap.



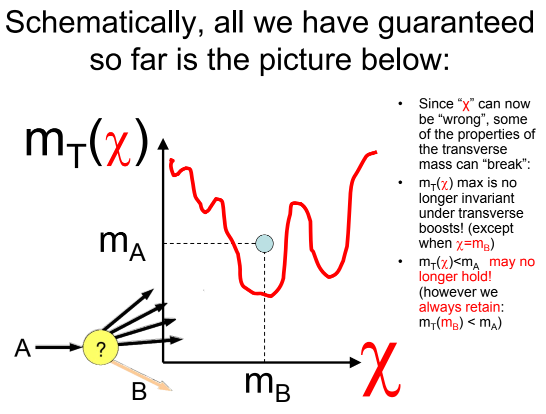
So the endpoint will no longer be measuring the parent mass. Nonetheless we might be able to get a mass difference out of it. If we really don't know the mass we are sticking in of the invisibles, we might put the most conservative estimate we can put in, 0 say.

Let's go back to the (full) transverse mass again for a closer look!

But let's actually see what happens. Closely, what actually does happen when you put in the wrong masses. So to distinguish the mass of the invisible mb from a guess for it, I am going to de-

[55] [pointing at particle B]

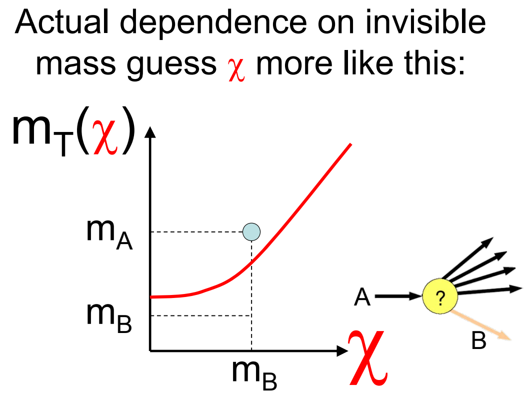
note a guess for mass of B by the symbol χ. This is just some parameter, mind! It is not a neutralino, though I suppose there is some heritage, maybe that is why I originally chose this greek letter. But χ is a guess for the mass of the b. Let us see what happens if we put that χ in place of all the mb's that we have previously been using in our transverse mass formulae. Schematically if we do that what we... Well we have turned the transverse mass into a function. Previously we had the transverse mass for an event. Now we have got the transverse mass as a function of the guess. Okay we can change our mind about this guess. We can plot mt as a function of the guess. One value is actually correct. When we choose χ equal to mb we have really got the true value.



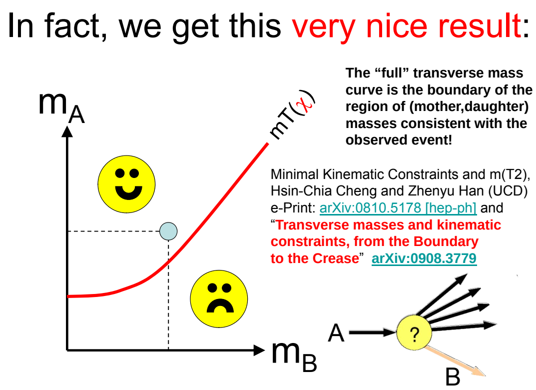
All we know we have guaranteed from the construction processes so far that at that point the transverse mass must be below the actual mass of the parent, because that is what we have spent a lot of time proving.

So we have guaranteed that the transverse mass as a function of χ curve will go underneath the spot that represents the true mass of the a.

Well we haven't really ever thought, up till now, about what happens on either side. If you look at the formulae you find that actually what happens either side is like this.



It is a function that has got some χ and some chi^2 terms in it. And it sort of looks like that. The point is it has to go underneath this dot. Okay. The curve will not ever be over the dot. In fact it turns out that there is quite a nice result. Not only is this a curve you can draw that goes under the dot, but it is actually... it has a proper physical interpretation for values of χ that aren't equal to the true mass. And the interpretation is this:



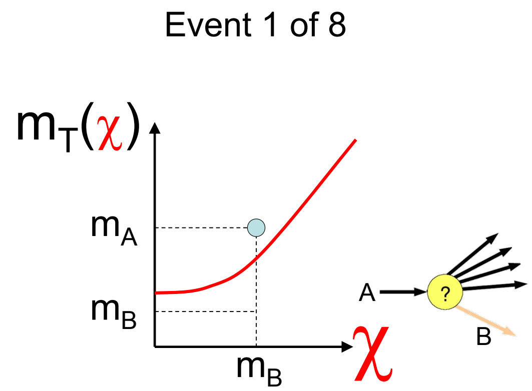
The mt of χ curve is the boundary between those parts of mb ma space that are consistent with what you saw and those parts that aren't. In other words one event comes along, if you were the plot the curve mt of χ for that event in this space, then you are basically saying 'I rule out all mb ma combinations that are underneath this line, and I am still allowing all the ones that are above this line. Modulo resolution effects. The detector effects, and the fact this might be a background.

This seems to be something that wasn't widely known - I didn't know about this until about 2008 - and it was odd, this result was proved in a much more complicated case first [11] and went backwards [12]. I don't know if that [result] is widely known to people who were doing lots of meson physics in the 60s, I don't know. But it was news to me in 2008. So as an exercise you might want to prove, it is not too tricky to prove, you might want to prove that happy/sad face statement that I have just made, as an exercise.

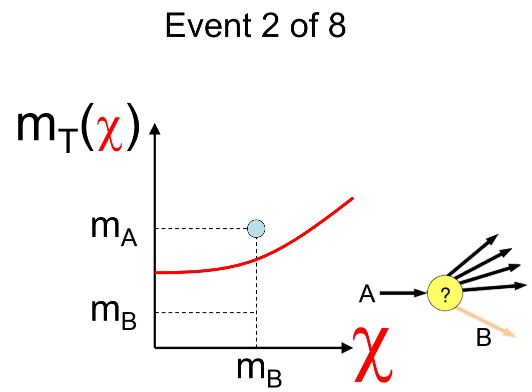
Exercise

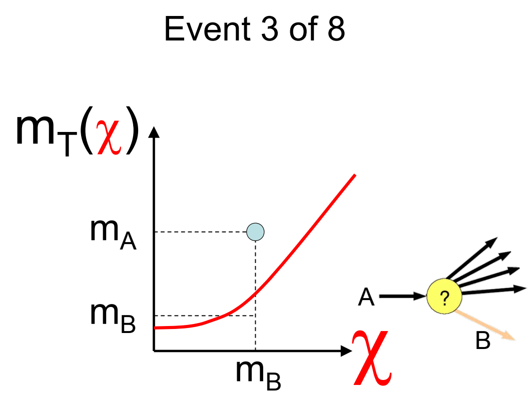
- (4) Prove the happy-face/sad-face statement made on the previous slide.
- [Note: not same as exercise (2). There mass of invisible was fixed at true value. Here it is not.]

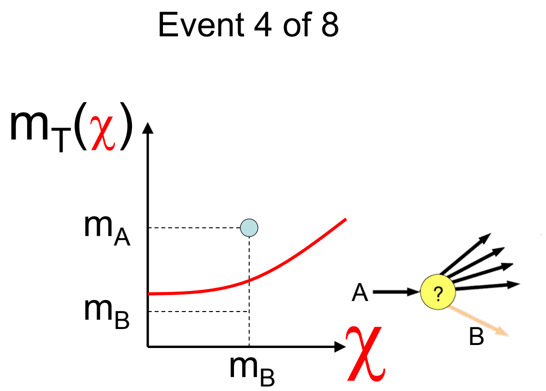
So what would happen? We turn on our Collider okay, let us just suppose we have got signal, we have got events of this type. Event 1 pops out of the collider it will have a mt of χ curve and it will go underneath this point here.

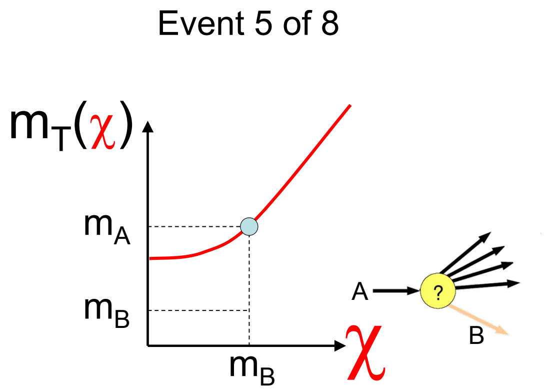


There will a second event come along you will have a different mt of χ curve. Third event, fourth event, fifth event, they can go to different places.

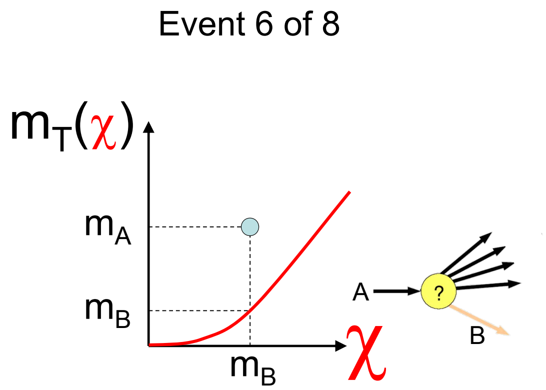




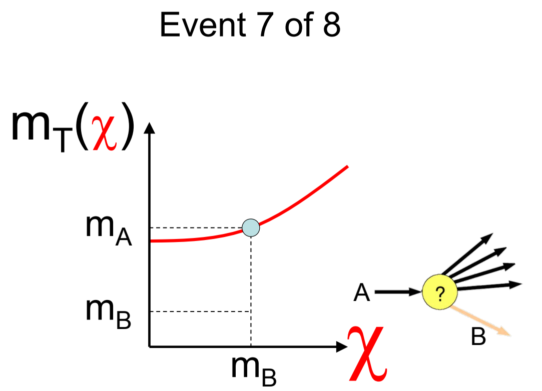


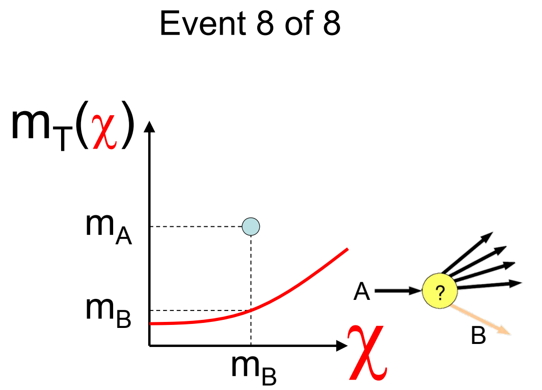


That last one [56] actually was a sort of a maximal event, it was an event that was sitting right at the endpoint of the mt distribution. Others will appear.

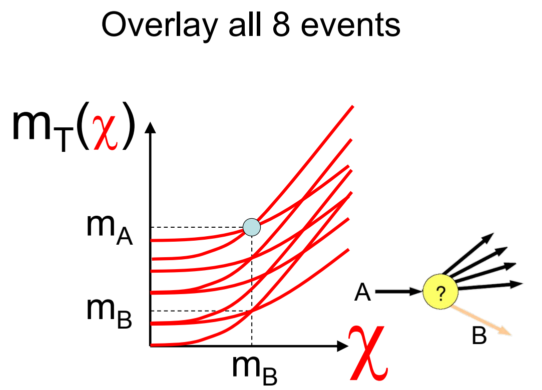


56 [Event 5 of 8]

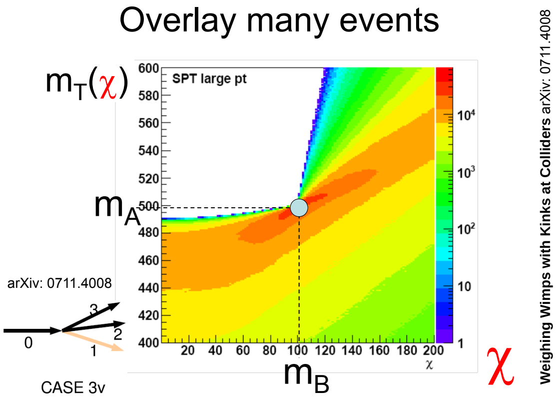




Let's put them all on the same plot and see what we get.



Okay, that's 8 events overlaid, what if we put many many events overlaid, what do we get? We get some kind of thing like this ...



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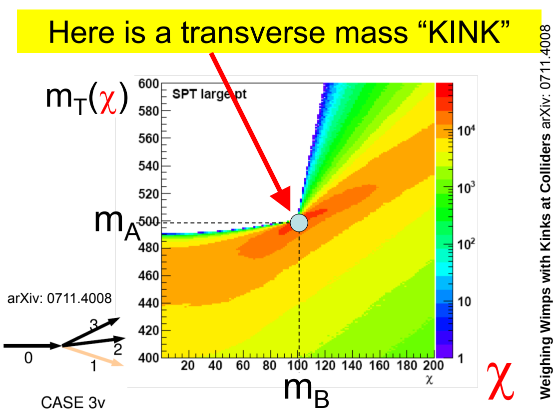
... where the colour scheme here indicates the density of the number of lines crossing at a pivotal point. So lots of lines cross at this point, not many crossed out here. We are interested in where is this dot? Can we find where the dot is? Because if we can find where the dot is, then we have got mb and ma, we have got the absolute mass scale. Well it is sitting in that corner. So in principle if you plot all this stuff up, you do all these things, then that thing there, in the corner that you would see, or could perhaps see - I will emphasise the 'could', 'perhaps' - is what is called 'Transverse Mass kink'.



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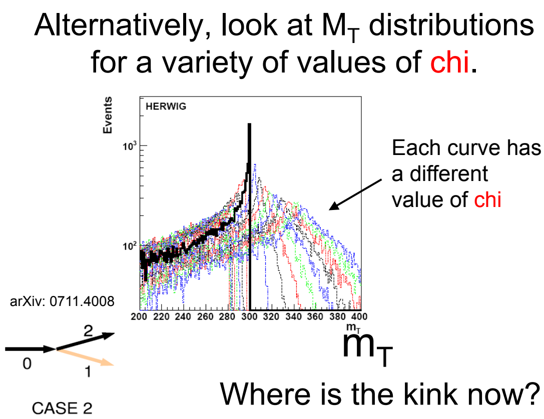
I mention it because people have sometimes heard about these things, and I would like to attach health warnings to these kinks, but anyway. Let's run with it at the moment.

So let's think about this distribution again. What is it saying? Slice up this plot, particularly where χ is, like drawing a mt distribution. When you put in the right mass of b the slice up here, so it is a sort of rising function and it goes up to there, and then it has an endpoint and no events beyond it. That's your mt curve that we have been plotting before. When you choose a bigger value of χ up here, your mt distribution extends up to higher values.

So you would have a mt curve that sort of extends beyond, or if you put lower values it is saying the mt curve is sort of squashed up. In a sense what is happening is that as you raise that value of χ from small values through the actual value to big values the edge of this distribution is moving up slowly, until it gets to the right value and then fast. And you can see that in this kind of plot. Note however: Warning: Log scale.



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57 Figure from [13]
58 Figure from [13]

59 Figure from [13]

Let's still ride with this. So what causes this kink? It helps to know where these things come from because although I think a lot of the time you shouldn't be trying to use them, there could be times where they would be very good to use, so we should know where this kink comes from. What causes this kink? It turns out that there are two entirely different things that could cause this kink and you might see a kink from either, or from both. What are those causes?

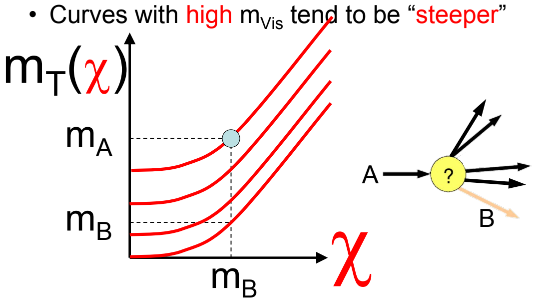
What causes the kink?

- Two entirely independent things can cause the kink:
  - (1) Variability in the "visible mass"
  - (2) Recoil of the "interesting things" against Upstream Transverse Momentum
- Which is the dominant cause depends on the particular situation … let us look at each separately:

So one cause is variability in the visible mass. If we have got a particle decaying to say four visible particles and one invisible particle like this,[60] these four visible particles here could come out in some kind of big invariant mass combination:

Kink cause 1: Variability in visible mass

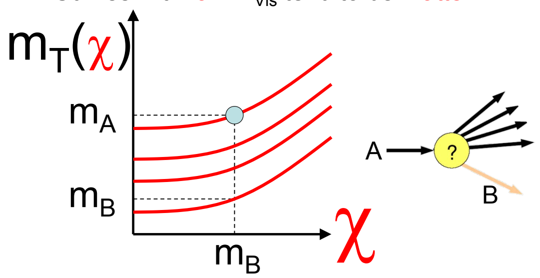
- m_{Vis} can change from event to event
- Gradient of $m_T(\chi)$ curve depends on m_{Vis}
- Curves with high m_{Vis} tend to be "steeper"



or some kind of small invariant mass combination, like that:

Kink cause 1: Variability in visible mass

- m_{Vis} can change from event to event
- Gradient of $m_T(\chi)$ curve depends on m_{Vis}
- Curves with low m_{Vis} tend to be "flatter"



You see they momenta have opened up, big invariant mass, and closed down again, small invariant mass.[61] I mean it turns out that these mt of χ curves, these bounds tend to be steeper in one case than they are in the other. So if your particle can decay in terms of two or more things that you can see, then there is possibility for variability in visible mass, and so these curves were overlaid to make a kink. You might like to prove that.

Exercise: $M \to (a_1 a_2) b$

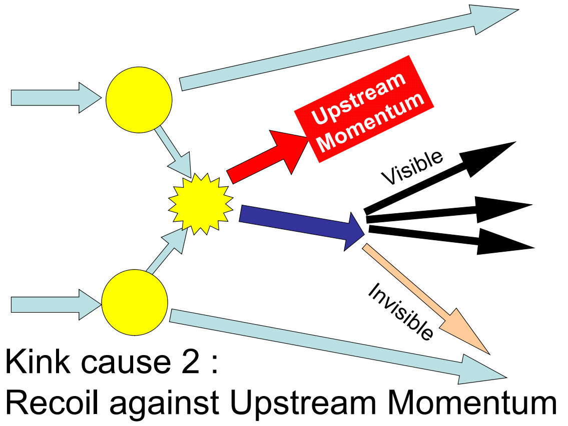
For the three body decay $M \to (a_1 a_2) b$ where a_1 and a_2 are visibles of known masses, while the b is invisible.

- (5) Satisfy yourself that, at the true value of the invisible mass, events can have M_T values that saturate the bound (i.e. have $M = M_T$) regardless of the invariant mass "m_{vis}" of the $a_1 a_2$ system.
- (6) Sketch a proof of the statements made in the last two slides – in some limit if necessary.

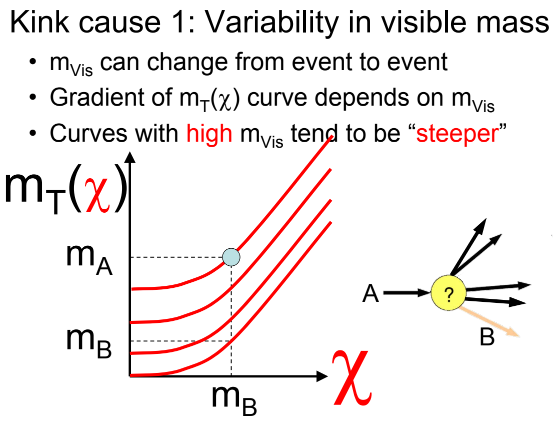
The other source of these things is this old upstream trans-momentum that I have been talking about.

[60] [draws on whiteboard]

[61] [Here I fliked back and forth between the last two slides emphasising the relationship between the visible invariant mass and the steepness of the mT curves.]

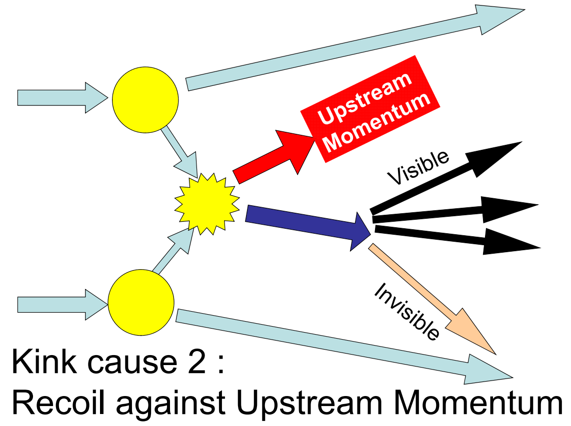


- Oh hang on before I go on, I want to come back and say one thing -

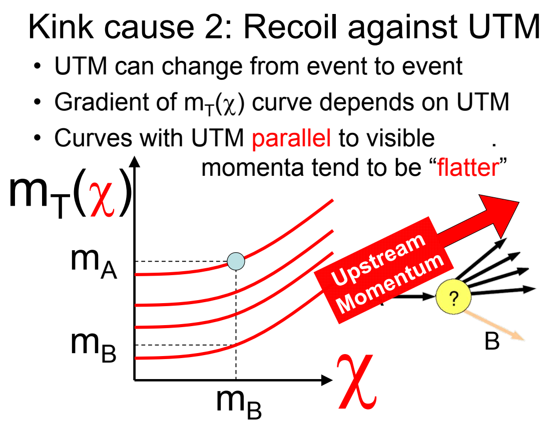


So in other words, if this particle here "A" can only go to a single visible particle (an electron) and a single invisible particle (eg a neutrino), that is to say if you imagine "A" to be a W boson, it goes to electron and neutrino then you cannot get a kink at all, from this process. Because that electron will have one, and only one mass, there will be no variability in the mass of the electrons event to event. You only see a kink [62] if you have got *multiple* particles that are visible being produced by each parent.

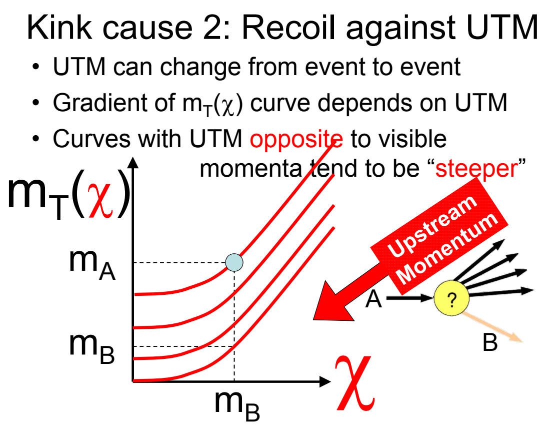
[62] [from variability in visible mass]



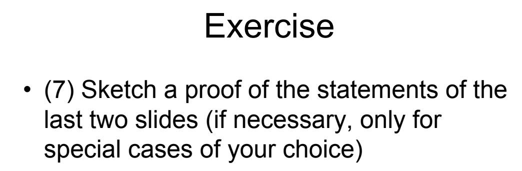
What else can happen? So the particle whose mass we are trying to bound - here this blue one - decay into some visible stuff and some invisible stuff, is recoiling generically against some other stuff, upstream transverse momentum. That doesn't have to be a single object, it could be that there is pair production of this thing. Or there are a whole lot of SUSY chains and stuff going on. This doesn't just have to be just isotopic. It could be isotopic in which it is very weak, but it could be really strong stuff. It turns out that when that upstream transverse momentum is aligned with the direction of visible particles.



Then these mt curves tend to be flatter, whereas when it is going the other way. So your visible system is thrown in the direction of your... the visible particles are being thrown against some upstream transverse momentum stuff, recoiling in the other direction, then you get steeper curves. So they are another source of this kink.

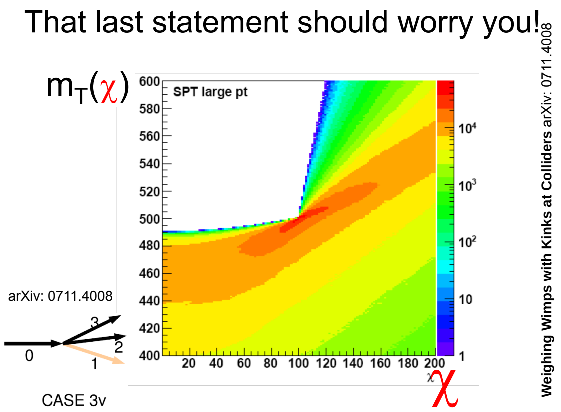


This is the only way you are going to measure the mass of the "A particle" if your "A" is something like a slepton that goes to lepton neutralino, so you only have a single particle that can come out - from slepton to lepton neutralino. And if for some reason you are unlucky enough that you have only got dislepton production because all the squarks and neutralinos are too heavy or something like that then you would perhaps be... stuck... Only able to get an absolute mass scale out of those events, if you could wait for a big enough sort of transverse momentum boosts of these sleptons that you are looking for. So you might like to prove that as well. I mean they are not terribly tricky proofs...



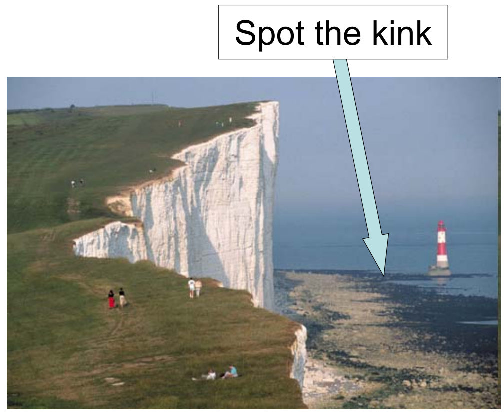
Any way: Health warning.

If you are interested in dark matter mass measurements, I think you should be very worried that these mt kinks provide, at present, they are the only known... kinematic - I will emphasise that kinematic method which in principle allows determination of this mass scale. There are other methods that you might call more dynamical ones, or where you make more assumptions when you are interested in the shapes of distributions, not just their endpoints. I would call them dynamical techniques. There are things, like likeihood methods, that you might be able to use for mass determinations of invisible particles in short decay chains at hadron colliders... [14, 15] if you suppose an entire model, a bit like when we measured the top mass by just looking at the average lepton pt well you could try and do something like that here. Try and measure the slepton, that is just by looking at an average lepton pt if you are prepared to assume a lot about the model.



[63]

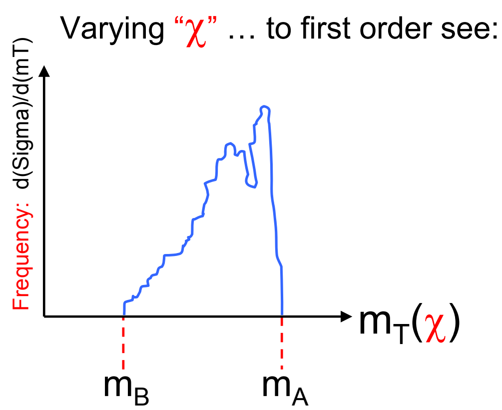
But you should be very worried because this kink is very hard to see a lot of the time. Why? Because look at the colour scale here, right! There's five orders meshed in this particular one, it is drawn like this. So if were plotting this as a kind of... If this was a map of the world this would be bolder with a high altitude here, and then we are moving down into the plains and whatever that is here, Kansas or something. Whereas most of the fall from the top of the cliff down to the bottom - maybe to where Denver is - where has that happened. Basically along maybe this orange boundary is where you have got 10% of your drop. There is not much of a kink there. In fact this is an optimistic one, because this is plotted in a situation where you have got a particle decaying... two visible that can gain a kink from variability of mass, as well as from "inaudible" boost. And there was a lot of boost. There was a large pt boost given to the upstream transverse momentum here. So that is an optimistic one. What you are really trying to do effectively is... Here is a picture of the white cliffs of Dover, near where I come from.



If I said 'Spot the kink', you might think the kink was here, at the bottom of the cliff. But no the kink that you are trying to look for is just where... you see this water comes in here, just beside the... This is not Dover, it is Beachy Head. It is nearer Southampton. Yes this is the Beachy Head lighthouse. You are looking for this tiny little bit of water that has come in there. That is where a lot of your... what looks like you are staring at the obvious kink is coming from.

So a lot of things have got to in your favour for you to use this kink to measure the neutrolone mass from a kinematic method.

[63] Figure from [13]



To first order... If you have plotted your mt distribution. Here it is plotted for the correct value of χ so it will start at... mass to the lighter particle, and have an endpoint at the mass of the parent. If you vary k, to first order your transverse mass distribution will just sort of slide up. Technically I should be drawing a little bit of a foot appearing here as it slides up. As it slides up, a little sort of nose should appear here and it should translate a bit faster. It is the velocity of this little tiny nose that is appearing there, that you are trying to measure, and I think that should do it. Any background here? Are you going to see it?

Take home messages for MT

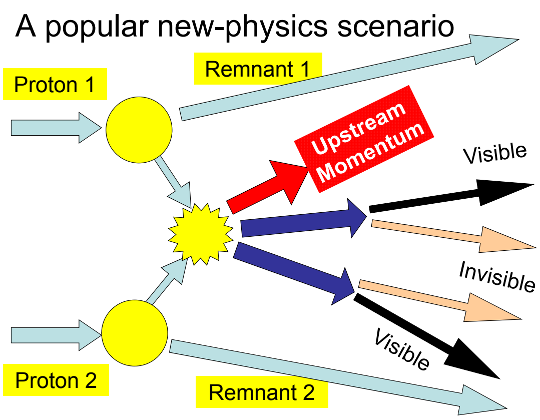
- **EASY to get MASS DIFFERENCE**
- We have two **independent kinematical** opportunities to measure **invisible daughter mass** in single particle decays:
  - "Upstream boost induced" MT kink
    - from ISR alone, useless, from real UTM, possible
  - "Variable visible mass induced" MT kink
    - impossible in 2-body decay, otherwise possible
  - HARD to set absolute mass scale
- We used pT-miss information – so only works with one invisible (so far …)

So take home messages for the transverse mass trying to get absolute mass scales. Easy to get a mass difference. Okay, this plot here... the width of that plot is pretty thick so that tells you the mass difference between ma and mb. So easy to get mass differences but we have two independent kinematical opportunities to try and get the handle on the daughter mass for single parent decays. One based on upstream transverse momentum boost induced kinks, and one based on variability visible mass, but they are both hard.

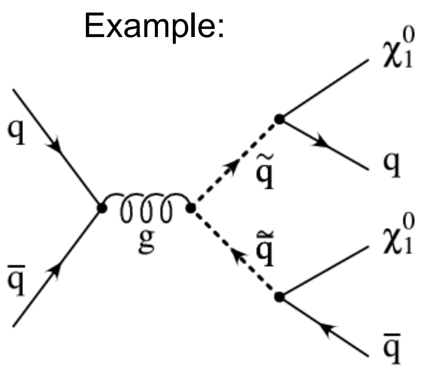
Change of topic:

How do we measure masses when there is Pair Production ?

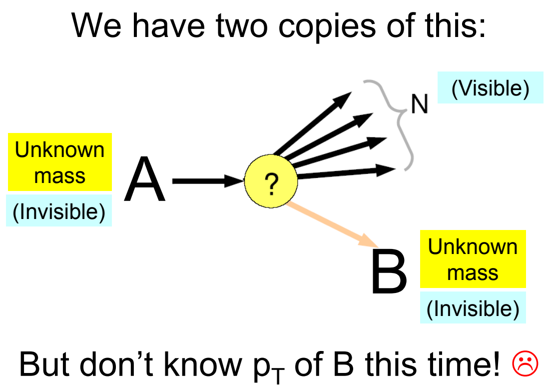
So that was just the single production. So now a change of topic again. I want to get finally... I think we are almost completely done with mt now, transverse mass. Because we want to get perhaps the case that if you are interested in a lot of SUSY models where there's all parent conserving super symmetry and so there are two invisible particles in each event protected by that symmetry, so these things can hang around. Which is good for the universe. We are looking at this case.



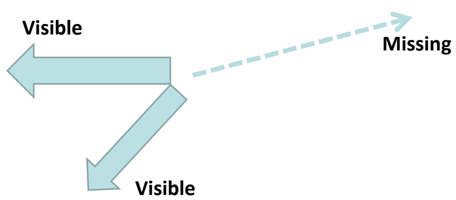
We might have pair production of, I don't know squarks to quark neutrino, quark neutrino. Pair production of sleptons to lepton neutrino, and lepton neutrino. Something like this. Here is the Squark case.



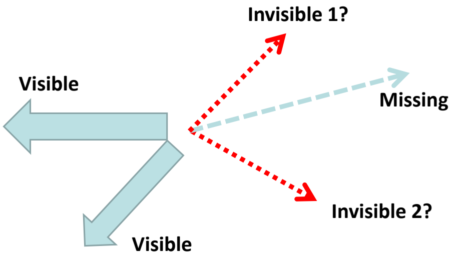
Can we get a handle on the masses of these parents? If we are unlucky enough that there's only these short chains involved. Later on we are going to come to long chains. It turns out things are a lot easier if you have got long cascade decays. But this is the hard part. The hard part is if you have only got very small events, there is very little happening in them. Very few observables. Very few things you have got a handle on. Can we measure the mass of these squarks, of these sleptons or whatever they were, and particularly their absolute mass scale. The answer is well now it is almost completely trivial. We have effectively constructed everything we need. All the technology is there, just in the ordinary transverse mass, the rest is just a sort of a slight corollary. Remember we have two copies of this thing going on.



The problem is that we don't know what the transverse momentum of this guy was anymore. When there was only a single parent there decaying we knew where this thing was going, even if we didn't know its mass from the missing energy constraint. But now there are two invisible particles and they add up to give the total transverse... missing transverse momentum, but we don't know how they conspired.

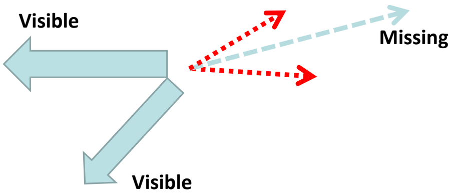


One possible splitting could have been like this.



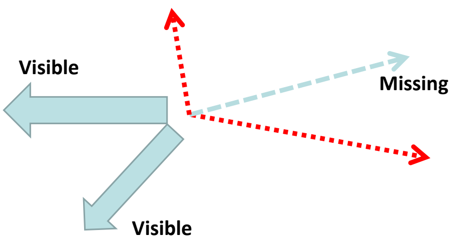
a possible “splitting”

Invisible one could have been going along there. Invisible Two could have been going on here, and that could have added up to give what you saw. Or they could have been more closer together,



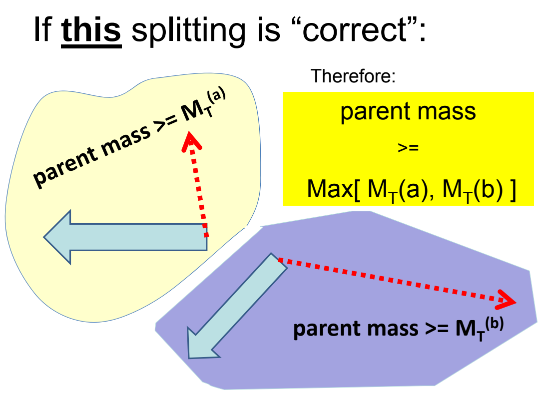
another possible “splitting”

or they could have been further apart.

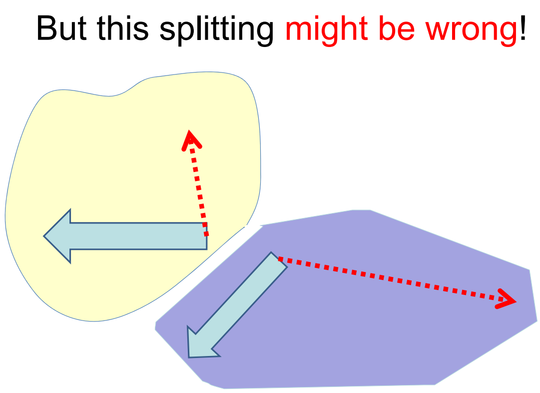


another possible “splitting”

Now if we knew that this particular splitting were correct ...



... i.e. if that was the one that was really true, then our mt transverse mass technology would tell us that the parent that led to these must weigh at least the mt for this system. The parent that led to these two must weigh at least the mt for this sytem, because that is what the transverse mass tells you. So those two different transverse masses are not necessarily the same, so we will even be able to say that the parent mass, since it is bigger than this, and bigger than this, must be bigger than whichever of them is biggest. The problem is, we don’t know if that is the correct splitting.



We don’t know if that is the correct assignment of the momentum. It could be wrong. But of course what we can say is this:

But can say that:

parent mass $\geq$ **Min**{ Max[M_T(a), M_T(b)] }
over all splittings of ptmiss

We can say that if we all try all possible splittings and we take the smallest such bound we have got, then that minimal/maximum overall splittings, the parent mass will definitely be bigger than that. Well that is what mt2 is.

Lester and Summers (hep-ph/9906349)

This is m_{T2}
the "Stransverse Mass"

$$m_{T2}(v_1, v_2, \not{p}_T, m_i^{(1)}, m_i^{(2)}) \equiv \min_{\sum \mathbf{q}_T = \not{\mathbf{p}}_T} \left\{ \max\left(m_T^{(1)}, m_T^{(2)} \right) \right\}$$

The most conservative partition consistent with the constraint

Take the better of the two lower bounds

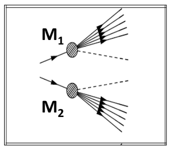
It is the <u>generalisation of transverse mass to pair production</u>. Clear how to generalise it to any other types of production.

[Received six comments about "mis-spelling" of transverse in ATLAS editorial board!]

If you have heard of mt2, that is the transverse mass [16]. It is just a rather dull and boring generalisation of the transverse mass to parental, to pair production of things.

The way we have constructed it here, it is not immediately obvious, but if you look through the steps you can prove to yourself that almost by construction, because you have built it out of transverse masses and you have minimised all... overall splittings, it is the best possible bound you could get on the mass of your parent particle, under that set of assumptions.

Note MT2 def is part of the four-step procedure:
[(1) select topology, (2) parent mass, (3) constraints, (4) find maximal lower bound]
described earlier.



Note, other approaches: MCT, Rogan, etc.

CONSTRAINTS

$M_1 = M_2$ + $\sum_{i=1}^{N_I} \vec{q}_{iT} = \not{\vec{p}}_T \equiv -\vec{u}_T - \sum_{i=1}^{N_V} \vec{p}_{iT}$

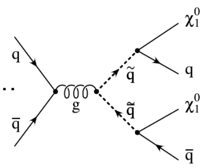
Momentum conservation in transverse plane

So in other words this fits into the scheme of "tailor your variable in the best possible way to the situation that you are willing to hypothesise, if that is what you want to hypothesise."

The only differece to what we had before is your constraints had multiple copies. So let me just kind of back track... If you want to forget about the definition. What will I learn if I have got a single event? I see it in my detector, but of course I don't know where the invisible guys are going. What does mt2 tell me? Well if you calculate mt2 for that event and you find it is 350 GeV, what it has told you is the squarks that you didn't see are at least 350 GeV big.

In other words:

- If your event is signal ...



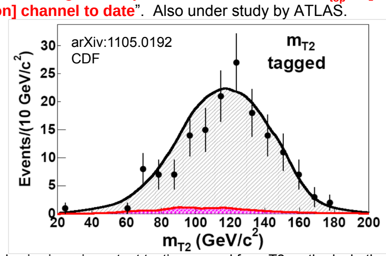
and if MT2 is "350 GeV" ...
then the squark mass is >= 350 GeV.

Indeed, can show MT2 is, by construction, the best possible lower bound on the squark mass.

That's it. That's all it tells me. Assuming here you have put in the right mass of the neutralino, and in the guest the mt has got this. You have got to feed it, I guess for the mass of your invisible things. That 350 gev that bound is going to be the best possible bound you can get. Examples: Is it actually useful? Is it used in real data?

MT2 example in real data …..

- "Top Quark Mass Measurement using mT2 in the Dilepton Channel at CDF" (arXiv:0911.2956 and arXiv:1105.0192) reports that they "achieve **the single most precise measurement of m_{top} in [the dilepton] channel to date**". Also under study by ATLAS.



Top-quark physics is an important testing ground for mT2 methods, both at the LHC and at the Tevatron. If it can't work there, its not going to work elsewhere.

The first people who bothered to try and use this was CDF, who have now used it twice, 2009 [17] and recently in 2011 [18], basically to try and measure the mass of the top quark in the dilepton channel. So they have got their t and their t bar, then w lubbwlnu. So they have got two parents, they are identical in mass, and they each lead to some visible things and some invisible stuff. So if they construct mt2 with the sort of bl combinations as their... their b lepton combinations as the visible things, they can construct a distribution that should stop at the top mass. And of course it looks worse because they have got proper detector effects and so on. They claim... I don't know, I am not really able to judge this, but they claim this gives them, what they say is the single most precise measurement then top in the dilepton channel. There are much better ways of measuring the top mass than this, because there's lots of missing information here. You are making it hard for yourself, because you don't know where most of these things went. But then they are interested in measuring it this way because they want to see... If they do try, nonetheless use this, do they get the same result? Or is there something really fishy going on... Is the top behaving properly? I think this is quite valuable, because if they can't use it in this kind of context, we certainly can't use it in the lhc. If we want to use this to measure particles properly we have got to use things like tevatron as a testing ground.

A digression

(Salutary Tale – how not to generalise to dissimilar parent and daughter masses)

So a digression, because I think you need a break from some of these mt things. I have said that sort of by construction this is fairly easy to see how you would have generalised mt2, if you had two particles. So you can see that it is fairly straightforward if you wanted to generalise it to more particles, or particles of different masses you would just put the mt ingredients together in different ways. You can be carried away with this and think oh this is all great fun, let's write up some of these. Let's try and tell people how to measure dissimilar production, so I suppose you have got chargino-neutralino production and you want to measure them, then you would generalise it in a different way.

Now the problem can occur that [while you are writing all that up] [64] you can be carried away with yourself, you can get very distracted by what is happening on the television. What was on the TV when we were writing this up? Oh it was the Ashes. Okay. What are the Ashes?

Cricket

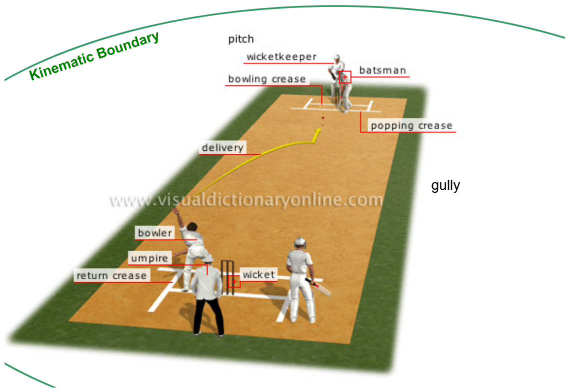
Cricket is a sport that we play in the UK, it is not at all like American football, and every few years England compete against Australia and attempt to reclaim the Ashes.

The Ashes

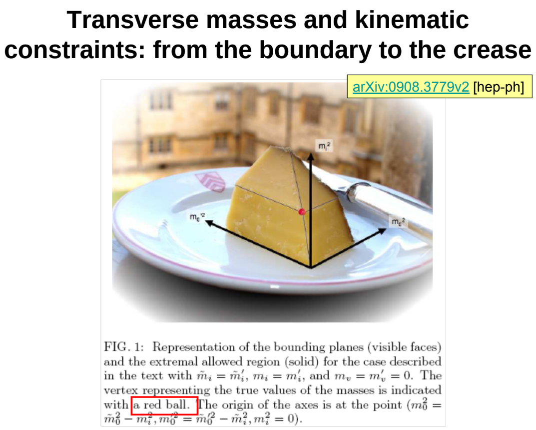
This is one of the smallest trophies in any sporting event in the world. That is the trophy you get. And it is a funerary urn, that contains the ashes of cricket. Because the first time that England was beaten by Australia, people at the Marylebone Cricket Club said this is disastrous. England has lost cricket. Cricket is dead. And they burnt the stumps, which are these things here

and the ashes were put in the funeral urn. The problem is that the Aussies are much sportier than us and they have got a nice climate and it is kind of warm in there, I think they practice a lot and try hard, and all those things that you shouldn't really do in a gentlemanly game. So they have a habit of beating us and most of the time they beat us in this game. But just as we were writing this paper, England was on the verge of winning, in the fact they did go on to win, and we were so excited about this that, because the kink structures we had found for this extra generalisation, they became three dimensional. You had not 2D spaces with a kink in, but 3D spaces with some kind of folding or crease in fact. Not really a kink, but a crease because it was two-dimensional. There's a lot of cricketing terminology and there is a place called the popping crease, you see in here.

[64] [It ended up as [12]]

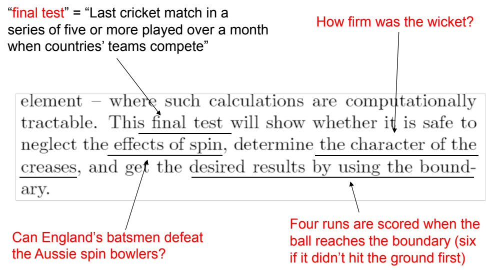


Then there is kinematic boundary that goes around the oval shape of the pitch. If the ball goes over the boundary you get lots of points, you get six points if it hasn't touched the ground and four if it has. So you can get sort of carried away with this, and you think ah yes, our diagram how are we going to label the point, the junction of these creases. We will label it with a cricket ball. The red ball, labels the point.



65

Then what else do you do? You think wouldn't it be great if we could write our conclusions with this huge double meaning, the conclusions will both be completely true physically, and also it will be a report on the state of Test Match:



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"This final test". You know the last cricket match being played between England... "We will show whether it seems to neglect the effects of spin", because we weren't sure about it, and who knows whether the bowlers are going to... you know the Australian spin bowlers. And will "the character of the crease get in the way of the desired result using the boundary" ?

You see you feel very happy with yourself. But, the real problem is all this causes is the following. You should always call your paper what it does. You should not call a paper "Transverse mass and kinetic constraints from the boundary to the crease", just because you are excited by the prospect of England winning the Ashes. Unless not, unless you are going to choose a sport that more people play.

[Laughter]

There are a couple of people from commonwealth countries who said 'Oh yes. Very nice paper.' Good result we got there. And I think everyone else didn't understand what the paper

[65] Figure from [12]

[66] Text from [12]

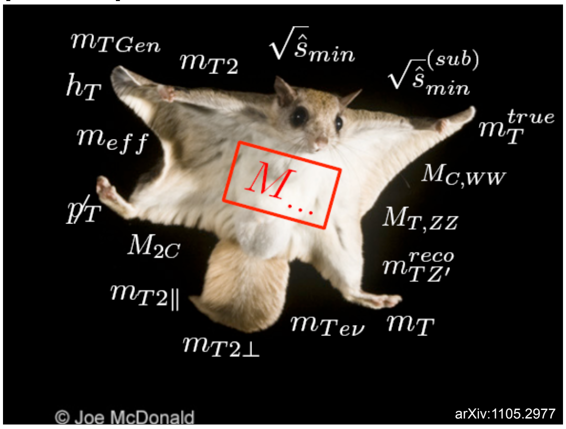
was on about, and I think Konstantin wrote a paper shortly afterwards with the same idea.

Moral

- Call the paper what it does
- or choose a sport that more people play
- or try for furry animals?

Alternative is perhaps you should try things, with furry animals, because everyone likes furry animals, and I think this is working much better as you have already seen, that this is getting around. And people will cite our papers [3] because we have nice monkeys... not monkeys...

[laughter]



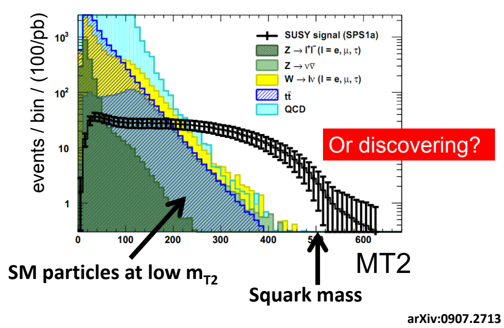
[67]

[Audence member points out what sort of animal it is:]

Flying squirrel.

[Lecturer]

Back to work. This is the point... Because you [addressing the students in the audience directly] have got to learn to write papers and you [addressing the students in the audience directly] shouldn't make the same mistakes that we make. So you might be able to measure the top mass in the dilepton channel. Okay that is good. But is it [68] useful for the LHC? When you sort of figure out... Say you are trying to measure the squark mass this time... Okay back to the squark to jets neutralino, jets neutralino. So we are trying to measure the squark mass. So you would construct mt2 using the two... Somehow you would have to select two jets in your events. You would have to make some choices about how you thought that these jets would show themselves. Would they be the two hardest jets, that's probably the simplest thing... You know the two highest to pt that is probably the simplest thing you could try, and you would calculate m2 of them, and the missing momentum, and you would get a distribution.



Here is an example with a reasonably good detector simulation but not full ATLAS detector simulation, showing that... a 500 GeV squark

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[67] Figure from [3]. Note the underlying picture of the animal is copyright Joe McDonald, who graciously took time out from a Tiger Safari in India to gave us permission to use his photo in our paper.

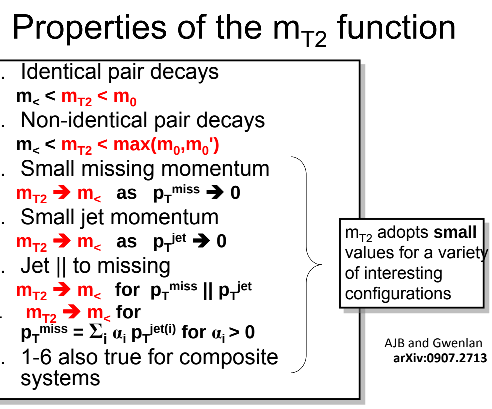
[68] [mt2]

signal would produce something with its endpoint. And then you have got standard backgrounds that are lower values. And principle... If there was perfect resolution in your detector all the standard model stuff should in some ways, largely speaking below m top. Completely below m top, and in many cases actually at 0, just because the mass scales of the things that you are measuring are so small. The fact that it can leak up is just because there is such a huge amount of QCD, such a huge amount of tt... things like this that mis-measurement errors can lead to some smearing, etc, but nonetheless you have bought quite a lot by having a distribution that has bunched itself up near the endpoint.

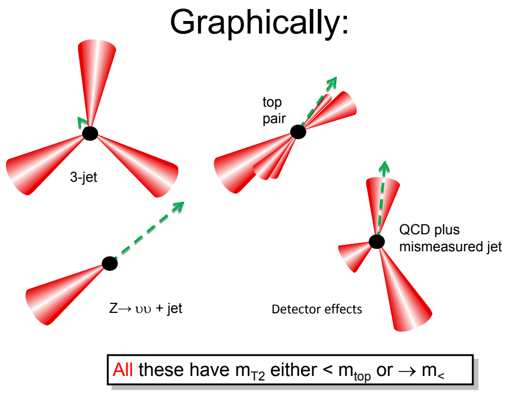
So in fact because we were mainly interested in finding super-symmetry and things like that at ATLAS, and indeed ruling out discovering it. There is much more importance of... find out that it is there first and then measuring masses will come later. So really the way that things like this stick out mean that people are mainly interested, actually if they are interested, not everyone is, but if you are interested then you might be well interested in it as a means of discovering stuff. Using it as a cut variable to sift your light standard model stuff down to low values, and push your heavy scale objects up to big masses, if they fit this structure. If they don't fit this structure it is going to be no good, because if... it is actually glue, glue... Oh you can't see black can you. If it is glue, you know glue, no production, then you will have extra jets here. And if you are only put two of them into mt2 you are not matching onto the right hypothesis, and so you won't get the kind of balance you want, and this thing will start to slump down and it won't be any good anymore.

[A member of the audience asks a question that is not audible on the record.]

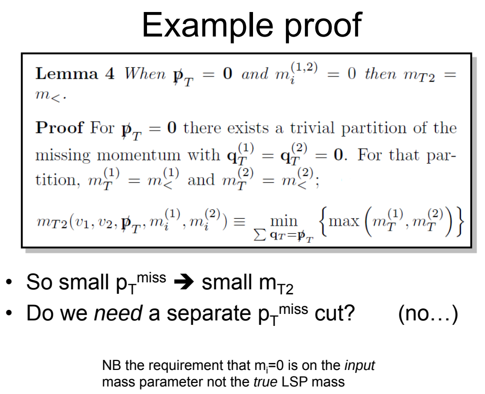
Well I am going to have some slides effectively talking about that in a minute. Come back to me if I don't emphasise it, it is not already in plots, but I am going to try and talk about it. So Alan Barr and Claire-Gwenlan went off [19] and tried to figure out why do these plots look... Why do these backgrounds get suppressed to such low values? Because initially it was a bit of a surprise that they should be. You would think that lots of this standard model stuff isn't pair production to invisible and invisibles. It might be pair production like di-jets, but it doesn't really fit this paradigm. In principle you could have perhaps have had mt2 values for the backgrounds anywhere. Why is all squashed down to low values? Quite a cute thing that seemed to surprise me, is they were sort of able to play around and find that er...



That is too busy isn't it, nobody is going to read that slide, let me just do the pictorial one.



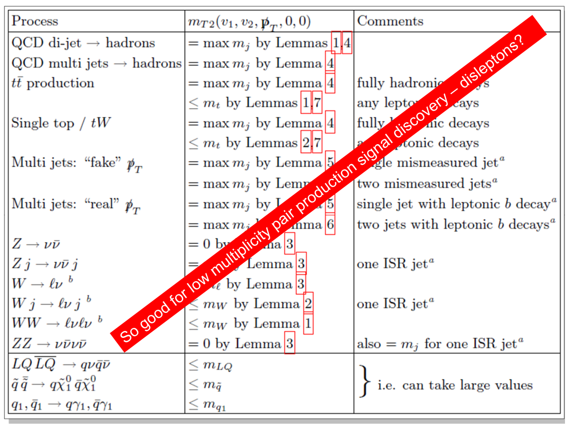
Basically you can probably prove with... he had never written a paper with the word 'lemma' in force. It was just funny they put lemmas and things like that in.



You can prove that mt2 should be 0, identically 0, or at least as smallest a mass of the visible things you are putting in, which if they are jets they would be... Usually jets have very small masses. The mass of the smallest visible thing in your system has been labelled to m less than in very small. So mt2 has to always be at least as big as m small, because if you are saying that I have got a particle here that decayed to that, then this parent must be at least as heavy as the visible thing that you made. So every mt2 distribution starts at m small and goes up to m parent if you get the neutrino mass right.

For most of the things you would get from qcd such as back to back jets, or things where there are 3 jet systems and no missing energy. Or most importantly for us mis-measured QCD. That is to say if you have a QCD event mainly 2 jets, perhaps a bit of gluon radiation. But this jet gets mis-measured, which happens all the time. It is one of our biggest backgrounds from tgd is that we under-estimate the energy of this jet. So you get some missing momentum that is largely colinear with this jet. In those kind of kinematical configurations, you can prove that basically mt2 should go to the small mass, to m small, it goes down to this low value. Which is all just by chance really, it is not intended, it is just fortunate.

So if you want to look up the proof, you can get it out of their paper, which is sadly not cited properly.[69]



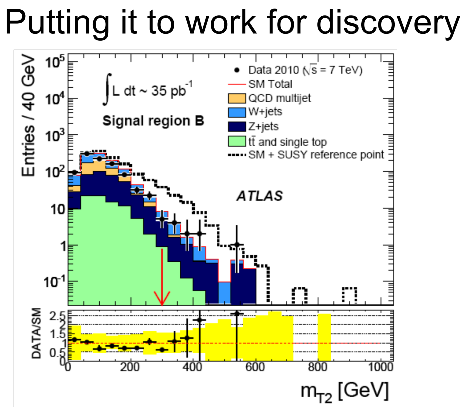
Process	$m_{T2}(v_1, v_2, \not{p}_T, 0, 0)$	Comments
QCD di-jet → hadrons	$= \max m_j$ by Lemmas 1,4	
QCD multi jets → hadrons	$= \max m_j$ by Lemma 4	
$t\bar{t}$ production	$= \max m_j$ by Lemma 4	fully hadroni[illegible]
	$\leq m_t$ by Lemmas 1,7	any lepto[illegible]cays
Single top / tW	$= \max m_j$ by Lemma 4	fully [illegible]nic decays
	$\leq m_t$ by Lemmas 2,7	[illegible]tonic decays
Multi jets: "fake" $\not{p}_T$	$= \max m_j$ by Lemma 5	[illegible]gle mismeasured jet[a]
	$= \max m_j$ by Lem[illegible]	two mismeasured jets[a]
Multi jets: "real" $\not{p}_T$	$= \max m_j$ by [illegible] 5	single jet with leptonic b decay[a]
	$= \max m_j$ [illegible]mma 6	two jets with leptonic b decays[a]
$Z \to \nu\bar{\nu}$	$= 0$ by[illegible]ma 3	
$Z\,j \to \nu\bar{\nu}\,j$	= [illegible] Lemma 3	one ISR jet[a]
$W \to \ell\nu$ [b]	[illegible] by Lemma 3	
$W\,j \to \ell\nu\,j$ [b]	$\leq m_W$ by Lemma 2	one ISR jet[a]
$WW \to \ell\nu\ell\nu$ [b]	$\leq m_W$ by Lemma 1	
$ZZ \to \nu\bar{\nu}\nu\bar{\nu}$	$= 0$ by Lemma 3	also $= m_j$ for one ISR jet[a]
$LQ\,\overline{LQ} \to q\nu\bar{q}\bar{\nu}$	$\leq m_{LQ}$	} i.e. can take large values
$\tilde{q}\,\bar{\tilde{q}} \to q\tilde{\chi}_1^0\,\bar{q}\tilde{\chi}_1^0$	$\leq m_{\tilde{q}}$	
$q_1, \bar{q}_1 \to q\gamma_1, \bar{q}\gamma_1$	$\leq m_{q_1}$	

70

Which reminds me. The people who are photocopying my handouts [for you] said they

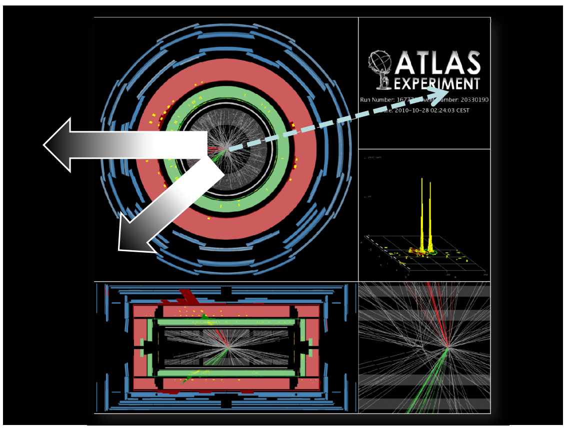
[69] Of course it is cited properly here [19] !
[70] Table from [19]

couldn't. There were too many slides. They produced a few copied, and said it was just not going to be done in time, because there were too many slides. If you want to get the pdf for these things, I have put them on my own web page, right at the very top. So if you kind of Google me, Chris Lester, and there is a little link which I will delete after a few days, sitting at the top with the slides on it. It you want to read them later on.[71]



72

In the lower luminosity ATLAS running that we had at the beginning of this year, end of last year and the beginning of this year, we were using this thing actively to try and look for any evidence of SUSY revealing itself in the squark squark to 1 jet 1 jet neutralino neutralino scenarios [20], and as I said to Konstantin. Why is that not in the question at the last lecture, why is the ATLAS result always er... Why did our observed limit exceed the expected limit? The answer is: No it didn't really for this one, it went under. This basically rejected a lot of... there were fewer results in the tail that showed up. We were not really able to use this very successfully for the high luminosity running that we are doing at the moment, because there's a lot of other stuff going on in these events. Lots of other jets being produced and sometimes we put the wrong ones in. Our hypothesis start to break. Also at the boundary where we are now trying to rule things out, these things are sufficiently heavy, that these things may themselves be fragmenting and we may not be putting all the right energy in. So it is an interesting open topic of research that we are sort of doing now, trying to figure out how we should choose and cluster our jet ingredients to go into those things, to still get the best reach we can in the di-squark channel. At the moment we now revert just to using an effective the biggest mt2 event. Here was the biggest mT2 event seen so far, probably just "z to jets" and z against to invisibles" :



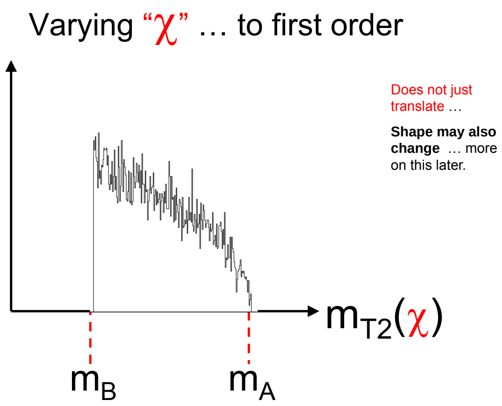
So we have dodged the question:

[71] They are now available at `http://www.hep.phy.cam.ac.uk/~lester/tasi/`

[72] Figure from [20]

Have dodged question of mass of invisible daughters.

What if we don't know their masses?

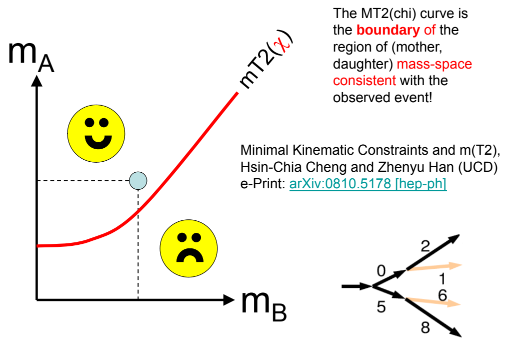


But... So to first order, remember though what had happened to the mt distributions, when we changed χ an mt distribution just slid up really. It retained knowledge of its endpoint. The odd behaviours was coming as a kind of a nose appearing at the end. Now when it is a mt2 distribution you are plotting and you are changing your k, it looks like this. It is not as peaky. The mt was like... It came up and came down with a nice peak there, because in an event with a single parent you have got more control over what's going on. You know your thing better. But in one of these parent production events there's lots more things you don't know about, so your endpoint structure is not as good. So although those tails, those noses will be appearing as you move these things up and down, they are much harder to see this time because there are tails appearing from what is already a low base level.

Just coming back to what you were asking about. How do we feed in... What are we doing about the invisible mass? Because we don't really know that. So the first thing you can do, if you are trying to make exclusions or reject standard model background, you should just put in the most conservative thing that you can 0 for the mass of the invisible particles. Because this sort of contracts your distributions. As a rule of thumb, almost all distributions I have come across and it is easy and sensitive to mass scales, but basically really predominantly are affected by mass differences. So if you were trying to measure the mass of a 200 GeV squark and you changed your neutrino mass to 100 GeV, there is a notable change to your distribution, because you have gone from 100^2 to $200^2 - 100^2$ and you would see that as a 20% change. So it is something you have to worry about.

So to first order you assume what really you are getting out of here is not an absolute mass measurement but really a mass difference measurement, or a mass squared difference measurement between parent and daughter. As I said, because it is built out of mt, mt2 inherits the smiley face, sad face exclusion kind of result that mt2 tells you which masses are allowed and which masses aren't.

MT2 inherits mass-space boundary from MT

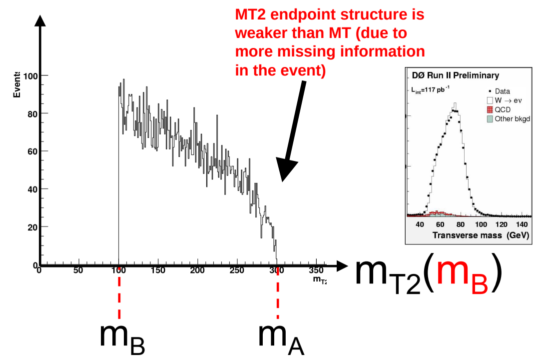


MT2 is defined in terms of MT

- Consequently, MT2 inherits the "kink structure" of MT and can (in principle) be used to:
  - EASILY measure the parent-daughter mass difference,
  - might PERHAPS measure the absolute mass scale using utm boosts kinks or variable visible mass kinks (HARD)

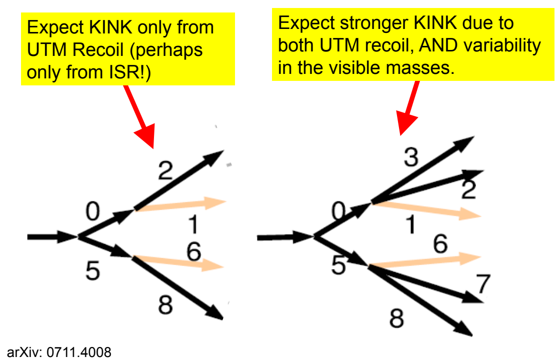
So mT2 inherits [11] all the kink's structure, at least in principle that you got from mt. So just as with mt you should say basically you can easily measure mass differences, but you might perhaps be able to measure absolute mass scales using these booster kinks. But it is harder than it was previously.

Perhaps: MT2's endpoint structure is weaker than MT's.

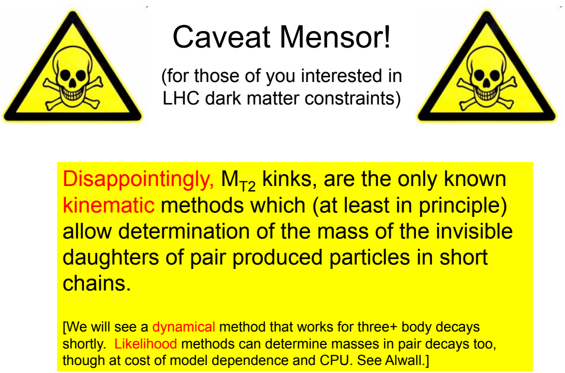


Not always impossible. There are cases, people have shown where we have the right sort of events happening where you can make it work. But I would say, as an order of magnitude estimate, that if you are writing a theory paper where it is vital you know the masses for these things, where you are reliant on UTM kinks, then it is going to be a lot harder than if you have got these things with the variable masses in as well. There you are going to be measure absolute masses a lot easier.

Are MT2 kinks observable ?



So that is me warning you again, that same thing.

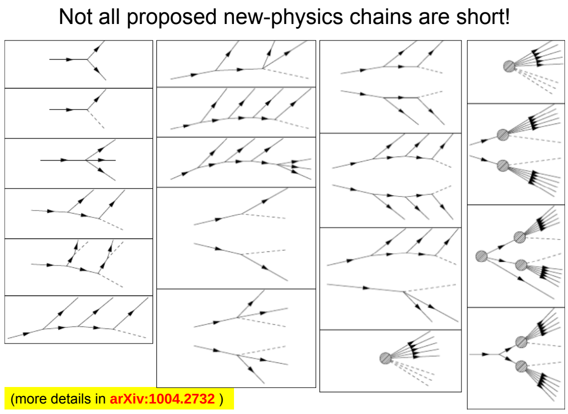


Google tells me that there is a different kind of mt2 kink. It is in this kind of drop front trousers, the people who have trouble urinating or something. It says here 'Mens drop front trousers mt2'. Price 38. Quantity 1. I don't know if that is... It is a shame it has come up in green. I think that is not the kind of kinks that you should be looking for, at this stage in your life. This made me think what other kind of kink... what other kinds of variables have... I typed in route s hap min into Google image search, and it told me that root-s-hat-min also has a connection with trousers, which I think are pants, in this audience. It says [55.23 le question du esat. french]. Which I think is something about why has he got his pants on the outside of his trousers. Isn't chat, C H A T in French? Anyway change of topic.

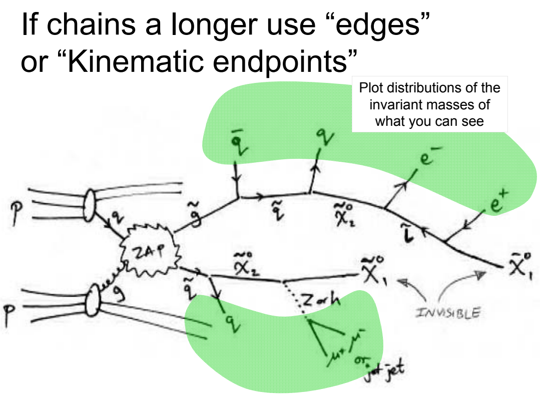
I don't know what this topic is.

[Laughter]

The change of topic is that...

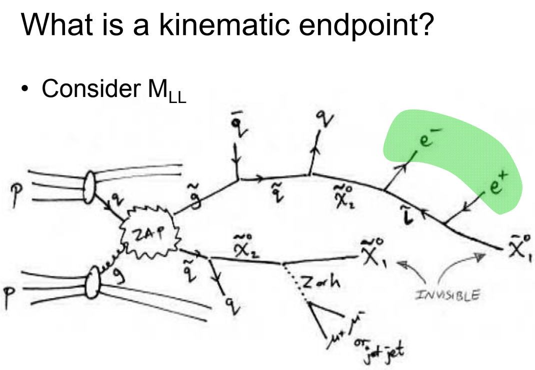


Ah yes, we should now dispense with all this stuff and say 'Well that is what we had to do when chains are really short'. If you are unlucky that you only produced a particle that... only really had decayed step and then stuff was produced, and there was invisible stuff. What can you do? And I think the answer is largely speaking not much more than what we have talked about, although you can do it in different ways, and there are sort of subtleties . But in that big index of hypothesis that you could make, you see that there are plenty of cases where the decay chains are not short. That is not short. [73] These aren't short.[74] So we are now leaving this stuff behind and going to long decay chains.



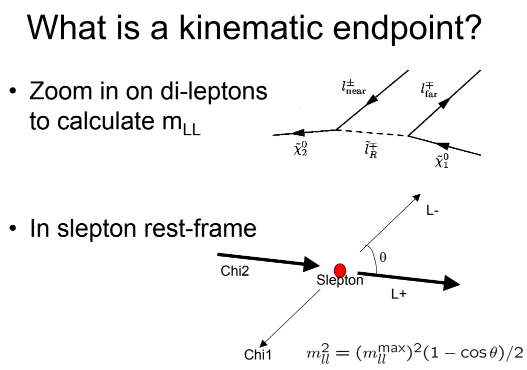
[73] [pointing at a random complicated topology]
[74] [pointing at some more]

So when we are analysing long decay chains we use, or a lot of people suggest we should use, kinematic endpoint. From before 1996 I think these were proposed and I think they have stood the test of time. People haven't changed their mind too much about these things, although certainly more things have been added to the armoury. As I say you are looking for this stuff. You have got a long decay chain. We could plot invariant mass distributions of combinations of these things, such as you could plot the invariant mass of the two leptons you might see in an event ...

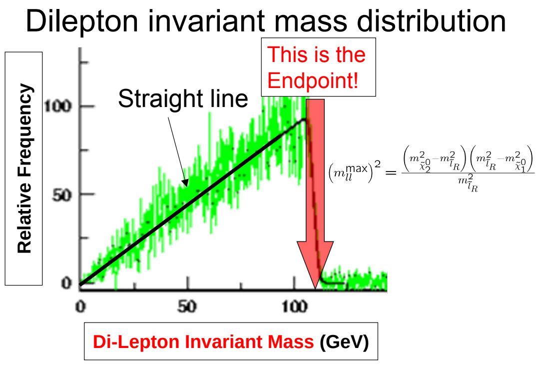


... which you hope has had that dileptonic system resulting from a SUSY chain, so maybe you would be asking for missing energy in your events, to sort of try and get events, it might be SUSY. You would be asking for high pt jets, and if you ask for leptons as well, you could plot the invariant mass distribution of them.

To emphasise this is kind of back... In a way this is sort of slightly back-tracking, but we are starting to make use of... we are making use of our hypothesis for what is going on here to motivate maybe the cuts that we are using to select these events. But instead of trying to set a bound on something in particular, here we are sort of going back to the hot pants idea of let's plot things and see what they measure.



So if you zoomed in on dileptonic system, neutrino2 to slepton to neutrino1 radiating leptons and moved into the mass, and into the rest frame of the intermediate slepton, then you would see that all the momenta in this frame, all the moduli of the momenta are fixed in terms of the masses of the particle. So if you propose the masses of your guys, you know what these momenta are. But there is still a freedom as to where the thing can radiate. And if you do the maths on this, assuming say just isotopic production or ignoring spins, and even in fact when you put in SUSY spin, you will find that the dileptonic event mass distribution should have, as it happens, this shape.



Invariant masses cannot be bigger than a certain cut off and distributed between the cut off and the 0 with this triangular straight line distribution.

So the salient feature, if you plotted this out in a detector, if you have got your events through and you plotted a dileptonic mass and you saw this, the thing that you really get out of this is the endpoint. The height of course is dependent upon the cross-section, but you have had a huge number of cuts producing these things, so you can't... the cross-section is affected by whether they were squarks or glunons or whatever was producing this thing. So the real measureable thing you get out of it is the endpoint. And once again we like endpoints because if this is sitting on a background it will raise it all but there will still be an endpoint, you will still be able to see it. You will still be able to see the edge even if there is some variability in acceptance across this plot.

Where is that endpoint? Well it can only be affected by what masses you put in and that the endpoint position is a product of mass squared differences over a mass squared. So again that sort of... It is a completely different thing we are doing but it is mass squared differences that you tend to constrain. There is some sensitivity to an overall scale here, because it is not just mass squared differences, there is a mass in there as well. So you might like to go away and just check where that comes from. See if you could prove where that endpoint is. See if you can prove that it is a triangular distribution, not too hard to do the triangular distribution.

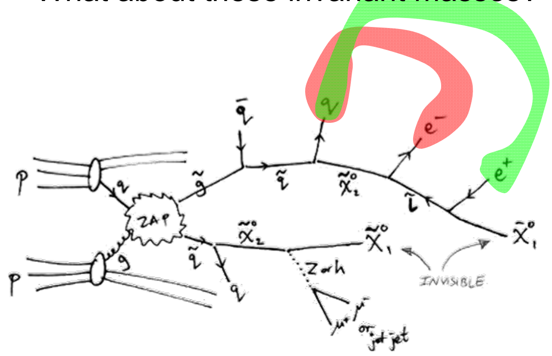
Exercises

- (8) Prove that the phase space distribution for the M_{LL} invariant mass is has the triangular shape shown on the previous slide, and
- (9) Show that the endpoint is located at

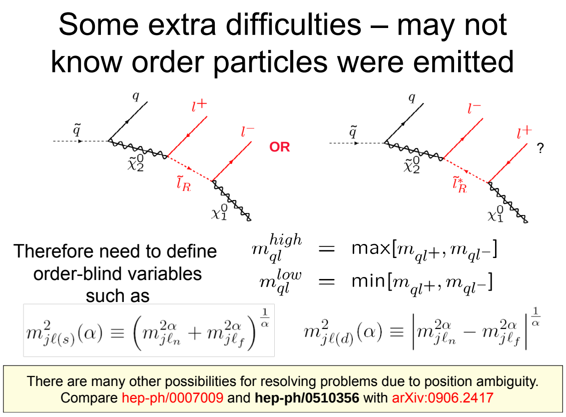
$$(m_{ll}^{\text{max}})^2 = \frac{\left(m^2_{\tilde{\chi}^0_2} - m^2_{\tilde{l}_R}\right)\left(m^2_{\tilde{l}_R} - m^2_{\tilde{\chi}^0_1}\right)}{m^2_{\tilde{l}_R}}$$

So... Okay if we can plot in invariant mass of those two, I guess we can plot the invariant mass of any two things. We could plot the invariant mass of maybe that jet and this lepton, or that jet and that lepton...

What about these invariant masses?

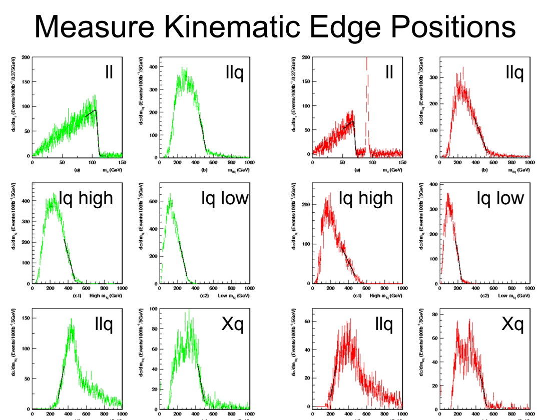


Ah but problem. Problem. The problem we have is that you could have put an anti-slepton in here



instead of this intermediate particle,[75] so we could have radiated e+ first and then e-. What does the detector see? The detector only sees that you had an electron and a positron but it doesn't tell which came first. It doesn't tell you which one was "nearer" the quark jet in time ordering. But equally, here I am just ignoring this quark at the moment, how we are going to separate the jet from this? But maybe start this whole chain off at a squark if you like. But still... We don't know... You can't form an invariant mass combination and know for certain that you are forming this one, rather than this one, because we don't know which order the leptons came out of.

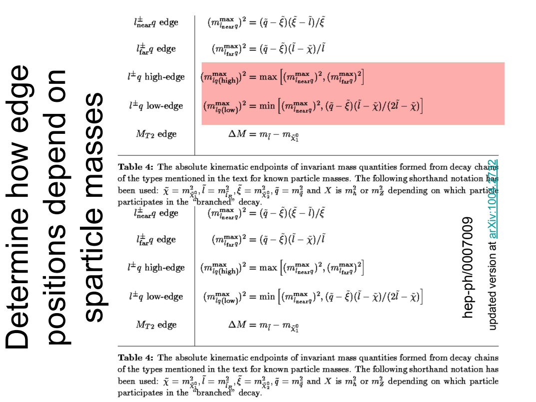
People tend to call this lepton [76] the 'near lepton' because it is nearest to this quark, and this other one the 'far lepton' as it is further down. So the quark near lepton invariant mass distribution - this red one - well it will have an endpoint and it will stop somewhere, and there will a function of only that, that and that mass, because those are the only ones that are involved. The quark lepton far less distribution would have an endpoint, and it will be a function of just of those four masses this time, because they are the ones that are involved. But we are going to have to go for different distributions. We are going to have to say plot maybe the distribution of... How could we divide things up in a symmetrical way. We could say: Look at the... compute both quark lepton invariant mass distributions with the two leptons you have got. One of them bigger than the other. So you could plot the higher and lower, or you could add them together and... look at some invariant masses. Anything that sort of symmetrises over the things that you don't know, so that you get bone fide of several distributions, and then plot their invariant... plot their distributions. Figure out where their endpoints are supposed to be. [77]



[75] [pointing at slepton]

[76] [pointing at the left-most one on the slide]

[77] For further reading compare [21] amd [22] with [23].



[78]

How might you choose between these things? Well it might be by... some choices, some of these down here in particular are motivated by the fact that when you are working backwards... when you are trying to go backwards and find out what the masses of the particles that were in your events were, based on what you saw, it might be beneficial to make sure that the sort of symmetrised quark and lepton distributions you plot, are those that are easy for you to invert, or have nice properties, or are less sensitive to mis-measurement errors. So if you want to read about these things then go and look here.

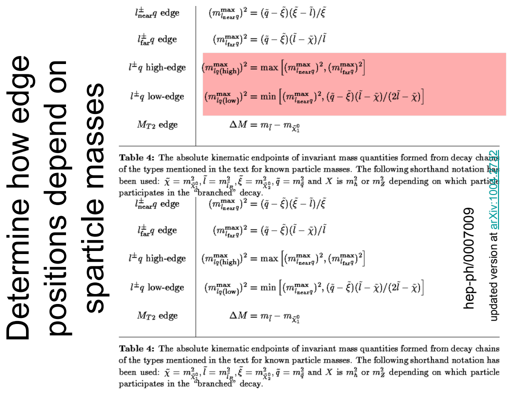
[Student asks a question about whether it is possible to cope with events where there are two long chains of (possibly) different types present simultaneously.]

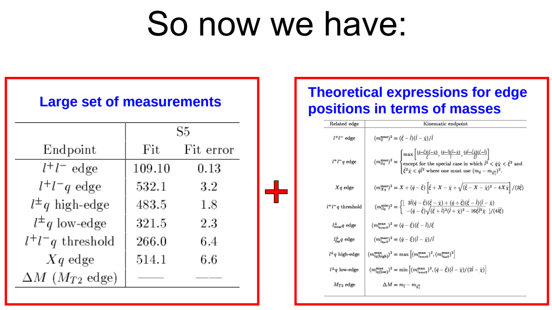
[Lecturer]

Then of course there would be effectively a background, so if at some point you do a quark and a lepton from down here say, well obviously that won't be bounded above by the relevant function of these things, so it will produce some... very often, very very high value typically for your invariant mass, because it has come from very widely separated things. So that will form a sort of continuum distribution on top of which your single events lie. Sometimes this can sort of help... sorry it doesn't help you. But sometimes you can helped by the fact that these chains themselves are quite rare. There's lots of choices for things to happen at this stage. After this, maybe it goes straight to a chargino or something, and a different type of quark. In a sense it is quite hard to get identical chains on both sides, because so many different things can happen. Very often you get it right... That is just a sort of background.

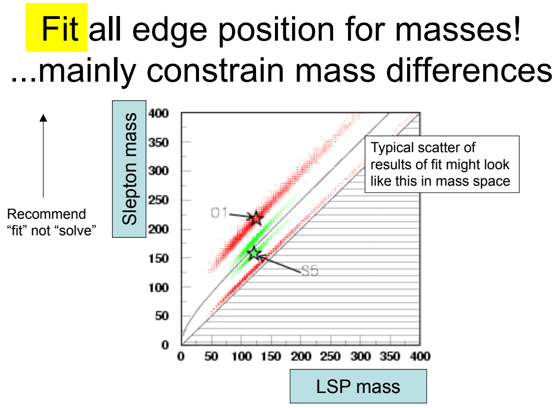
So you might eventually construct, not to disagree with the dilepton in this particular thing, some of the old stuff didn't make use of the newer variables we had, these kilo high and kilo high distributions and some thresholds, some distributions whose lower endpoint was interesting. And lq: that is the invariant mass of everything put together. All of the things, the quark and the lepton. So what you want to know is: Where would those endpoints be as a function of the masses of the things you are trying to find out? Once you have measured where they are and you know what the functions of the endpoints tell you what the masses are.

[78] Table from [21]



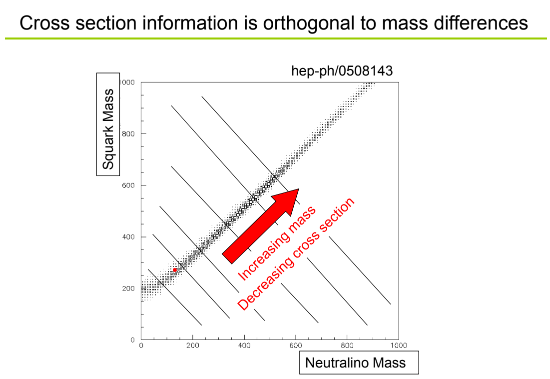


You are suppose to invert that, work backwards and figure out what sort of maybe LSP and slepton masses are consistent with what you have seen. Sometimes people say you should solve those constraints backwards. I think that is erm... Well everything has to be just right for you to be able to analytically invert those constraints, in particular if you had far more distributions that you had measured... If you had more distributions than masses you were trying to measure there may technically even be an inversion, your measurements may over-constrain the problem. So in practice you typically always want to fit what you saw to this formulae and you will get things... Here is a typical thing, you will get mass differences again very well constrained typically



These lines are sort of fairly parallel, if we... a heavier lsp mass is still consistent with what you saw, a slepton mass. Some vague sensitivity to overall mass scale, but not very much. And sometimes degenerates your answers, so when the model was where the star is, you get the inversions that might incorporate the star and also have isolated local minima in the sort of answer space that are just mirage solutions, and things that you haven't yet figured out how to bound.

The last thing. Given that you tend to get mass differences or mass-squared differences better constrained that masses, perhaps it is time to say 'Well what about non-kinematical things. What about cross-section information?'



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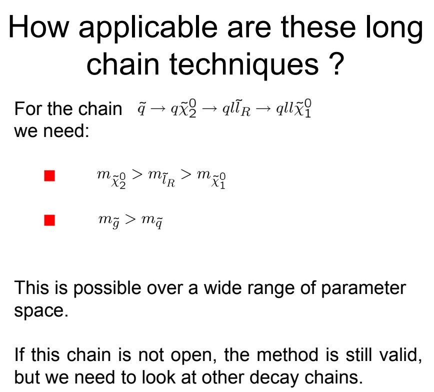


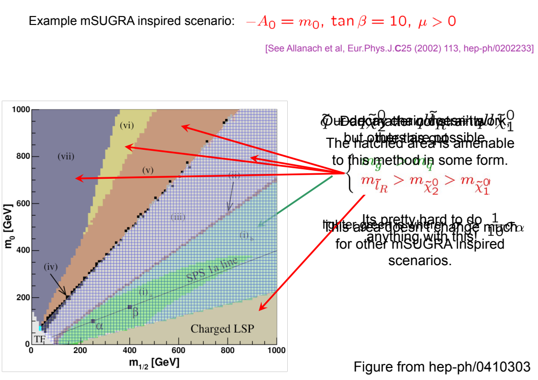
It tends to be... lines of iso cross-section are sort of at right-angles to mass differences. When you go up in that direction [80] everything gets heavier, it gets harder to produce and so the cross-section goes down. So if you ever felt inclined to put a real model dependent assumption about what sort of processes are making your things, and you get a cross-section estimate, and within the validity of that assumption you can sort of narrow yourself down and localise yourself. So that maybe a thing you have to add.

I will stop there for today. Thank you.

3. LECTURE 3

So at the end of the last lecture we were talking about long decay chains, and things that you get... measurements that you can make by looking at invariant masses of bits and pieces there. We got to a point where I said you might get things like some kind of largely constr-... mainly constrained to mass difference is mainly what you see, not exclusively, you may get a density, you may be lucky, it depends how many variables, and endpoints you put together, and cross sections might break you out of that if you are prepared to make a model dependent assumption.

So what I was about to get to now is: How applicable are these long chain techniques? So it's all very well me saying, you could put together those endpoint measurements, but how... is that really specialist? Will that only work if the conditions are just right? I mean the answer could be yes or no, it depends, there's no measure in SUGRA space, or SUSY space to tell you which bits are more likely, and which aren't. But in the sort of typical slices of the... well rather naughtily constrained models, SUGRA models that people show, you can... there are sort of bands.



81

I dont want you to worry about the details. The point is that I'm just showing you some sort

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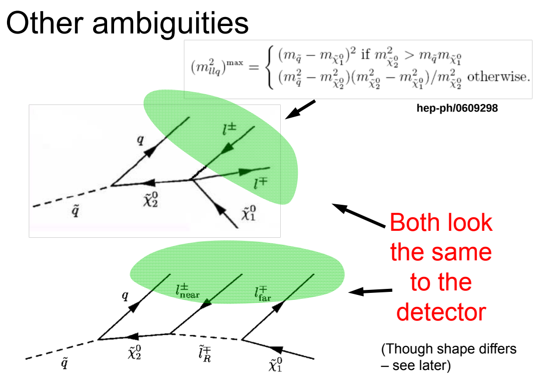
[79] Figure from [24]
[80] [pointing to the top right of the plot]
[81] Figure from [25]

of arbitrary slice in some sort of SUSY space. Some bits, okay, we know are ruled out, we can't... we've got the wrong kind of LSP to meet cosmogical requirements. But in the sort of... In this particular slice that's been chosen,[82] then some wedge here has the right mass hierarchy that we need for this in the model. So to get that long chain we needed gluinos to be heavier than squarks, because the squarks can go to the neutralino twos that the gluinos can't. And then to get the leptonic part we needed the hierarchy between the neutralino two, the slepton and the neutralino one. So you've got some sort of wedge in which that... with which those things are true. Then there's another wedge where, okay, you won't have the slepton part of the chain anymore, but on the other hand, at least these decays can still take place. There's things that can go on here, and you'll still get the squark being able to go to perhaps the neutralino two. So there are other decay chains that might work, and you could produce numbers, so you've got some reading where it could work. Other places where too many things are violated into... it may be pretty hard to do anything. So my point is just that you're at the mercy of fate as to which parts of the parameter space are open to you. But some sizeable fraction of the thing is there, and then people come up with other techniques to deal with the bits and pieces, the spaces that don't fit.

[82] [pointing at the green wedge]



[83]

Other ambiguities. Okay. You are plotting away and you've got your quark lepton, lepton invariant mass, and how do you know whether it's... whether the quarks and leptons came from this or came from something like this. Maybe this lepton is too heavy, so we don't have the decay chain that I just talked about, we don't have this one, but you could still get a virtual sort of decay with a... a virtual slepton in the middle of here. Now that... It'll still have a nice distribution. It can't... The invariant mass of these three things can't be arbitrarily big. Can't be less than zero. So it'll look like a sort of a distribution, similar sort of thing, but its endpoint is a completely different position, it's a completely different function of the masses. So if you naively interpret all your endpoint positions in this paradigm, but it's actually that what's going on, then you'll usually, unfortunately, still get very nicely localised parts of parameter space telling you, oh these are your mass differences. Yes, well done, well done. But they may all be

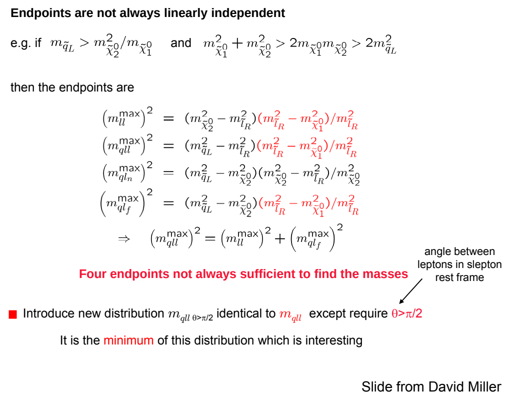
[83] Endpoint formula from [26]

completely wrong, you have to kind of reinterpret your results saying, 'But it could have been like this', and then you get the difference sort of slices. So you get lots of ambiguities, you might have to basically integrate both of these things.

[Student - asks a question which is inaudible on the transcript. Given the reply, it seems likely the question was about why/whether the endpoint formulae on the above slide are valid for arbitrarily high slepton masses, as in this limit the bottom diagram might look like it should encapsulate the top one.]

[Lecturer]

Yeah. So in a sense there isn't a... By the time the slepton gets far too heavy here its mass isn't affecting the shape of this distribution, it only depends on these three things. And it's sort of not quite... it's not... Sometimes these things aren't just a limiting process of this. Basically this formula, the formulae that I've shown for this, are only really valid for on-shell slepton particle masses. They also typically assume that all mass diff-... all the formulae that I've shown also typically assume that the mass differences between any of these sparticle guys is much bigger than the masses of the visible particles. So if you really put in proper visible particle masses then the formulae become much more complicated, but they do smoothly start to transit into one into the other, instead of being very different.

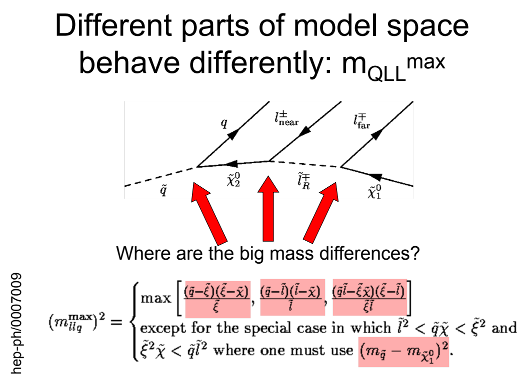


[84]

Other things. So, sometimes these endpoints, this is the... Sometimes the endpoint formulae aren't all lineally independent. So when you are looking at the quark lepton invariant mass, when you're constructing it, right, you've got those momenta, and you add them together, and you square them. Right? And so you'll get a quark, a lepton, and a lepton momenta. They're all mass-less things, but you'll get a 2 quark lepton, plus a dot... two times lepton lepton, and two times the other quark... the other... So, we'll call it lepton 2 and lepton 1. Okay? So you'll get... When you square that to work out your invariant mass you've got three bits. But what is that? That thing is the quark lepton invariant mass squared. So that's mass of quark lepton 1, basically, squared. Again, it's "inaudible" and similarly... So that's the third... that's the second, that's the third invariant mass. So what I'm trying to say is that the quark... the invariant mass of everything squared is just the sum of the mass of three of the individual masses

[84] Slide from David Miller, University of Glasgow.

squared. So there's some redundancy among the observables.



[85]

Now, although there's redundancy among the observables that you... whose distributions you're plotting, that doesn't mean that there's not different information in the end points, because when the quark lepton, lepton momenta happen to be pointing in the right direction to be maximal for the Mquark lepton lepton, these things may not be maximal. They may not be near their end points, they may be somewhere else. They may be just at some kind of nice intermediate positions that happen to give you the maximum up here. So the end point of this distribution, you can have an event that is near this endpoint, but it's not near the end points of any of those three, and vice versa, so these can be four completely different pieces of information coming from the end points, or they may not be. And in some parts of the parmeter space they are not, when the mass of the quark is bigger than this sort of geometric mean over here, and when those conditions are true, then you don't get completely independent information from each of these edges. And so you would need extra distributions in there to start plotting to try and separate these things, you might need more variables.

So specifically, let's give an example, when these things are all on mass shell, and when their mass differences are much bigger than the masses of the visible guys. The end point for the quark lepton lepton invariant mass... so the invariant mass of everything, could actually have basically one of four different functional forms. Roughly speaking, possibilities are, those three objects, the quark, the lepton and the lepton, could... sometimes they get very big invariant masses when two of them are co-linear, over here, and back to back with the third on that side. Or we could take one of those two guys and bring him over here, and then maybe that's the configuration that leads to the maximum invariant mass, and then there's a... So when you count it there's three sort of co-linear configurations. Or maybe the maximum invariant mass comes from a sort of a Mercedes three pointed sort of star. Right?

Now, which of those is going to be the maximum... lead to the kinematic end point? It

[85] End point expression from [21]

depends on the mass differences of the particles. If this quark is much heavier than this neutrino 2 it can kick out a very highly boosted quark here, in its rest frame, and so the kinematic configuration that maximises the invariant mass is probably one where that is opposite to these. Okay. But if it's a different mass difference that's bigger that may dominate. And so you end up finding that the invariant mass turns out to be the maximum of these three possibilities, most of which are mass squared differences here, but with some sensitivity to absolute masses in... But it could be this fourth thing down here if some condition on the mass is not true.

I've written them out here. This is a sort of compact way of writing it. Alternatively, you can say that the invariant mass maximum is that formula in that case, or this formula in that case, and so on, and you might like to prove these, if you want to have a play, check that you understand what's going on.

Exercise

- (10) Prove either

$$(m_{llq}^{\max})^2 = \begin{cases} (m_{\tilde{q}}^2 - m_{\tilde{\chi}_2^0}^2)(m_{\tilde{\chi}_2^0}^2 - m_{\tilde{\chi}_1^0}^2)/m_{\tilde{\chi}_2^0}^2 & \text{iff} \quad {m_{\tilde{\chi}_2^0}}^2 < m_{\tilde{\chi}_1^0} m_{\tilde{q}}, \\ (m_{\tilde{q}}^2 - m_{\tilde{l}}^2)(m_{\tilde{l}}^2 - m_{\tilde{\chi}_1^0}^2)/m_{\tilde{l}}^2 & \text{iff} \quad m_{\tilde{\chi}_1^0} m_{\tilde{q}} < {m_{\tilde{l}}}^2, \\ (m_{\tilde{q}}^2 m_{\tilde{l}}^2 - m_{\tilde{\chi}_2^0}^2 m_{\tilde{\chi}_1^0}^2)(m_{\tilde{\chi}_2^0}^2 - m_{\tilde{l}}^2)/(m_{\tilde{\chi}_2^0}^2 m_{\tilde{l}}^2) & \text{iff} \quad {m_{\tilde{l}}}^2 m_{\tilde{q}} < m_{\tilde{\chi}_1^0} {m_{\tilde{\chi}_2^0}}^2, \\ (m_{\tilde{q}} - m_{\tilde{\chi}_1^0})^2 & \text{otherwise.} \end{cases}$$

or

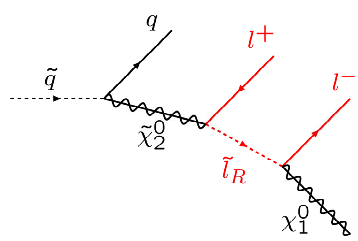
$$(m_{llq}^{\max})^2 = \begin{cases} \max\left[\frac{(\tilde{q}-\tilde{\xi})(\tilde{\xi}-\tilde{\chi})}{\tilde{\xi}}, \frac{(\tilde{q}-\tilde{l})(\tilde{l}-\tilde{\chi})}{\tilde{l}}, \frac{(\tilde{q}\tilde{l}-\tilde{\xi}\tilde{\chi})(\tilde{\xi}-\tilde{l})}{\tilde{\xi}\tilde{l}}\right] \\ \text{except for the special case in which } \tilde{l}^2 < \tilde{q}\tilde{\chi} < \tilde{\xi}^2 \text{ and} \\ \tilde{\xi}^2\tilde{\chi} < \tilde{q}\tilde{l}^2 \text{ where one must use } (m_{\tilde{q}} - m_{\tilde{\chi}_1^0})^2. \end{cases}$$

and show that they are equivalent.

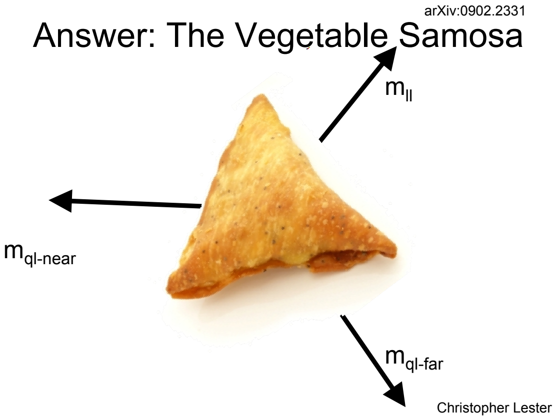
(See definitions of symbols approx three slides back).

So given that, one might like then to think... So you've got some kind of interdependencies, some relationships between these things, sometimes they're independent, sometimes they're not. Are we looking at this space from the wrong perspective. Maybe we're so busy projecting things down on to 1D that we've forgotten that we've really got observable... our real observables exist... live in a higher space. We've got a full momentum of a quark, we've got a full momentum of a lepton, and a former momentum of a lepton, maybe those really are observables, and we should look at it in high dimensions. The absolute momenta are not really important, because whatever conclusions we're going to draw should be events invariant, so it's... Really the key independent observables are really just events in invariant things, the dot products of this quark and this lepton, or this quark and this lepton, or these two leptons together. So perhaps we should really look in the free space of one of these three invariant mass squares. Yeah? Because if we work in that space maybe we just sort of see what's really going on. Which parts of that space are reachable by events?

Which parts of $(m^2_{\text{qlnear}}, m^2_{\text{qlfar}}, m^2_{\text{ll}})$-space are populated by these events:

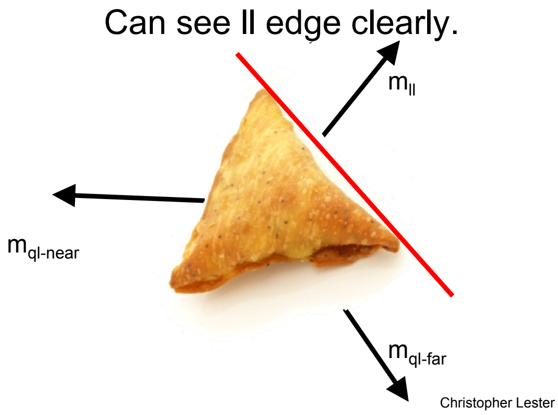


So I've got an event, and if I propose in my head, the masses of my particles are in M1, M2, M3, M4, then some parts of this mass mass mass space are reachable by actual events, and others are out of bounds. Right? They're too... the momentum would have to be too big, or something. What shape have we got? And the answer is that in that space things I can find to lie in, something I call the vegetable samosa [27].



It's a sort of slightly inflated tetrahedron. It's got one vertex at the origin, which you can't see. I don't know if real vegetable Samosas have four corners of just three. In this picture I'm not sure. But anyway behind it, okay, so it's like a cone coming out at you, there is vertex behind it in the origin, and then you've got these three corners coming out. And it's not a perfect tetrahedron, it's kind of slightly inflated. It has actually got straight edges, if you kind of plot the space, there is a straight line that runs along here, and along... and along here, but it can be actually blown up quite a lot, or almost be completely tetrahedral, depending upon the masses that you hypothesise. So this vegetable samosa is a function of the masses... of the particles that led to the chain, but it's in... not the space of the masses and particles, it's in an observable space.

Now we have been talking about the LL edge, the dilepton edge. The dilepton edge I showed you was the very first one. It's the one with that nice big triangle, where you put your dial up to invariant mass, and it's in things like SUSY, or face space, or UED to a good approximation. You get a nice triangular distribution of the MLL. Nice good strong firm edge there. Really good strong firm edge, one of the best observables we've got. Can we see it on this plot?



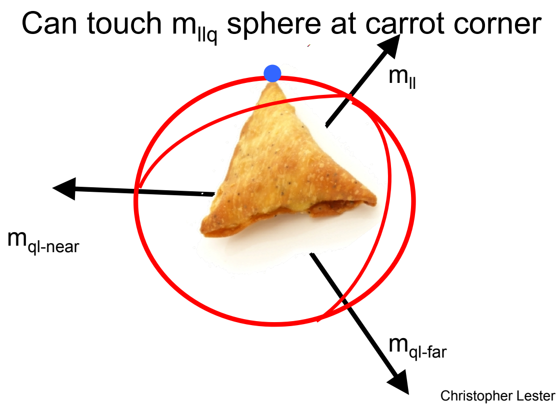
Yes. If you take all the events that are inside this vegetable samosa, and you projected them down on to the MLL access then clearly this edge of a samosa - and that's why I warranted it like this, that's really the right way up, will cause a huge number of events to be kind of near this edge, and I suppose it sort of tapers this way, and so there are fewer and fewer events projecting onto MLL of 0. You know? This is a projection of this samosa onto the MLL axis.

Unfortunately, because of the way the samosa is oriented you don't get such nice projections onto the QL near, and the QL far directions, but that's sort of okay, because we can't tell whether we're looking at QL near anyway, or

QL far. You may have forgotten, or fallen asleep at that point of the last lecture, where I sort of defined what those terms meant. I said that the first quark... The first thing that comes out is called the quark, and then this is the lepton that is called lepton near, and that is lepton far in the chain. And because we don't know which order the things came out in we have... we've said we had to plot symmetric combinations, like the highest QL invariant mass, and the lowest QL invariant mass. So we can't really project onto these axis, because we don't have access to the information we need to project onto those axis in the event.

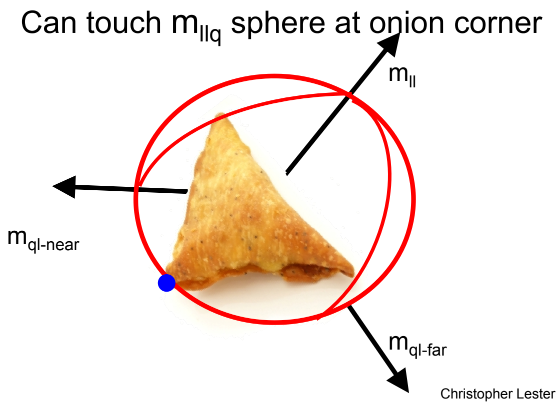
Nonetheless, what about the MLLQ invariant mass? Okay. MLLQ, all three of them put together. We've just shown here, MLLQ squared is this squared, plus this squared, plus this squared. So in other words, that's a sphere on this space. Yeah? In this space everywhere on a sphere of constant radius R has MLLQ of R. So where's the LLQ edge?

It's... Basically if you know what the masses of your samosa are, you should imagine sort of shrink wrapping a sphere from infinity... shrink it down and down and down, centred on the origin, until it touches the samosa. Okay? Because when it does you've found the radius, which is the maximum possible MLLQ value you can get. Right? Now where your samosa is, and how big it is, depends on the masses in your chain. So basically if your samosa is the one where a carrot corner sticks out then we have... then the maximum value of MLLQ, boom, will be achieved when it contacts here.

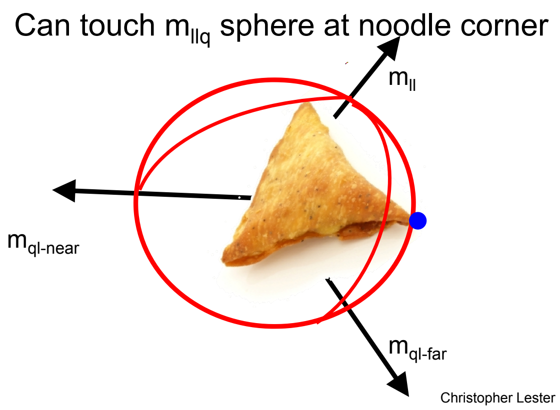


And that's why we have one of those four formulae that I had on this diagram here, one, two, three, four. Okay?

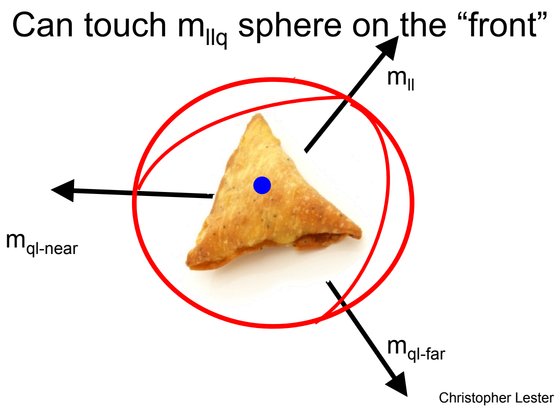
On the other hand, maybe the masses are such that the onion corner touches first, and we get a... That will be, I don't know, two things co-linear over here, and one over there. Right?



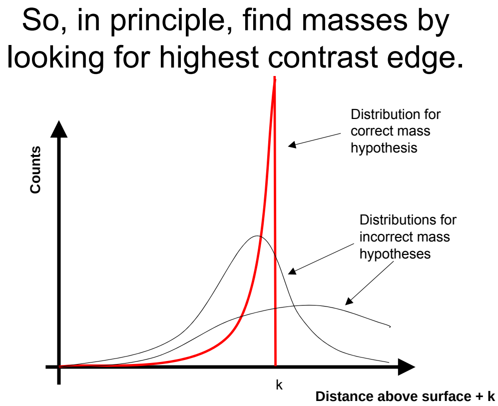
Or maybe it's noodle corner.



What is it goes into vegetable samosas? I don't know.[86] We don't get them in Peterhouse.[87] On the other hand, this Mercedes thing could be because it's so puffed up with good food from Old Corado that it actually touches on the front kind of puffed out face. Yeah?



So what Tovey and Costanzo suggested [27], and kind of related to the item and I have been thinking about, a number of years ago... Sometimes people say what we should really do is not try and project these things down onto funny directions, but we could try and get a really fancy edge. Why don't we invent vegetable samosa coordinates? Right? So that's to say you hypothesise... not "inaudible" loss, spherical coordinates, or something, or ellipsoidal, but you would specify as a function of the masses one, two, three, four, in your backbone, a coordinates system. And if you happen to get that coordinates system exactly right. If you've chosen the right masses, then everywhere... then the radial coordinate in samosa coordinates...: How far above or below the surface of the samosa you are, you'll have two coordinates, one will be like a feta and a thigh that will get azimuthally, and whatnot... polarly around a samosa, and then there'll be a kind of radial coordinate that says: Am I on the surface? Or have I jumped off? Or am I below the surface?



Now, if you chose to plot radial samosa coordinate R as your variable then all of your events should be inside the samosa, if you've got the masses right, and no events, no background events... only background events can be outside the samosa. But if you get the masses wrong, okay, then you're actually plotting the wrong samosa there, and some of your samosa, the real samosa, will be outside the hypothetical line, and some of it will be inside. So what does that mean? It means basically this distance above surface, plus some arbitrary constant, just because I don't want to get it conflicting with the axis over here. This is radial samosa coordinate in this direction. When you get your masses just right. When you hypothesise the right one, two, three, four, then your events will... basically almost all of them will be at the edge of

[86] After the lecture, some members of the audience from the Indian sub-continent pointed out to me that vegetable samosas contain very few of the ingredients I listed.

[87] [my Cambridge College]

the samosa. Why? Because that's just what volumes are like. Most of a sphere is near the edge of the sphere. It's because sort of the volume is increased, sort of quadra-... of shells, increase quadratically as you go outside. And very little of the samosa is right in the middle. So you could create... look for an edge in Samosa R, because that would be in a sense, the ultimate edge you could ever hope for. It's put all the things that are at your kinematic boundary at the boundary of the variable you are searching for. So it's sort of all edges at once. It's the LL edge, it's the LLQ edge, it's all of them. And when you get the wrong masses then of course you get some splurge from your stuff.

Now that's all very well and good, that's all very nice, and fine and dandy, but in practice no one has been able to make this work. Because the problem is, what happens to your cuts and the backgrounds? Okay. The backgrounds... In this space, a background event that has nothing to do with this chain has to end up somewhere in this mass space, if it passes your cuts. And it tends to be the case that most backgrounds like to be at low invariant mass, because it's just where there's a lot of QCD is and so on. So a lot of stuff tries to cluster near the origin, and then it doesn't like going further away, but it just sort of exponentially falls off as you go out. So in your fit, when you're tinkering around with M1, M2, M3, M4, trying to get everything just inside the samosa in the best sort of steepness function at your edge, it's very hard not to be sort of confused by all these backgrounds, and moved around. So I don't know. There's places where this will work, and places where it doesn't work, and people haven't played with it much, but I thought it sort of cute, so I'll just show it to you.

That's gone to the full three space that matters. We could just go to...

Exercise

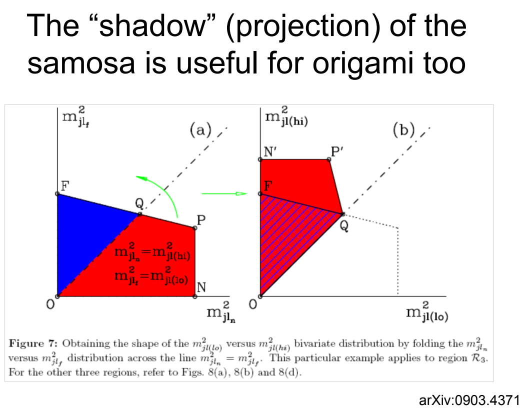
(11) For fixed masses of the four particles on the SUSY backbone, find a function $f(q^\mu, l_{near}^\mu, l_{far}^\mu)$ that is zero on the surface of the samosa, and is non-zero elsewhere.

[Hint: I suggest you try to solve for the invisible LSP momentum as a linear combination of the three visible four-momenta q^μ, l_{near}^μ, l_{far}^μ and a fourth four-vector that is a totally antisymmetric combination of them $\Omega_\mu = \epsilon_{\mu\nu\sigma\rho} q^\nu l_{near}^\sigma l_{far}^\rho$. Then see under what conditions this solution is meaningful.]

arXiv:0902.2331

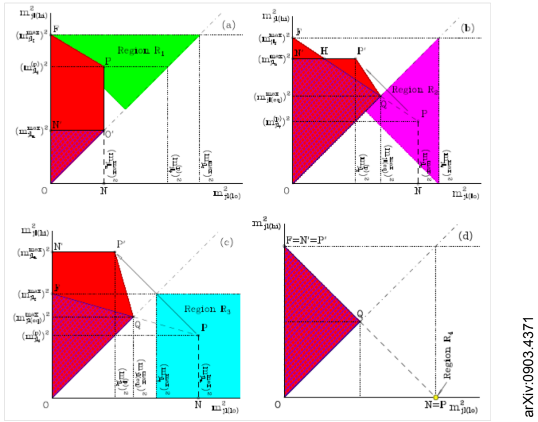
Oh, sorry. Exercise, I think you should show... derive what samosa coordinates are, they're quite fun. I've given you some hints there actually, and some clues on exactly how to do it, and particularly if you use this hint down here, which I won't talk about. I mean if people want to, they can talk about it to me afterwards between now and Friday, if they want to have a play, it's quite good practice for making sure you do kinematics efficiently. [The answers may be found in [27]]

But what I want to say is, that if in three space the problem is that you don't really quite know where your backgrounds are, and so you get confused and you choose the wrong values, maybe two spaces are more attractable. One space is, MLL. Nice, because we our backgrounds will sit underneath us, and we'll be on some kind of pedestal. Maybe backgrounds will increase at this point over here, but that's okay, we'll still see our edge if we are lucky.



[88]

ally a sort of origami - so that this projection of the samosa is one that is actually experimentally achievable. Then you can look for things like, is there a density of points mainly in a triangle?



[89]

So if 1D was okay, 3D was perhaps harder than we wanted it to be. What about 2D? So you can project your samosa down into 2D, and because of those straight edges that I mentioned that it has, even though it's puffed up it does have straight edges along its corners. And it turns out when you project it in a sort of mass squared, mass squared space, it sort of looks like the sort of rhombuses and kites and things. This is from one of Konstantin's papers, and I think it shows that you can do origami with these things. I think what it means is... Basically if you try hard enough... So first off projecting it down, and then realising, woops, I've projected in the lepton near, lepton far space, but I don't know which was lepton near and lepton far... So oops, I'd better symmetrise my projection to make sure I'm only projecting visible things now. So I will fold my space back on itself - it's liter-

Where are these shapes? Where are these edges here? Where are these 2D shapes? This one has arrived by construction, this one has arrived by construction, but the sort of density of points should be bigger in this region, and have a smooth edge here, and then there should be these corners like this. And you can look for these structures. I forget what people call them, kinematic boundaries or something like that, but they're sort of the two dimensional things, extension of edges and... I think if you try hard enough... It's a shame that Konstantin has gone, because I'd like to see him sort of actually fold these things up, and see if it turns into a swan, or whatever it is.

What you could say all these things are is... Oh sorry, question.

[Student]

How does all this fare under smearing?

[88] Figure taken from [28]

[89] Figure taken from [28]

[Lecturer]

How does it fare under smearing?

[Student]

Yes.

[Lecturer]

So your population of events that is projected down here will... this will become blurry. There will be a sort of uniformed distribution in here, and then it'll sort of start to fade out as you go across this region. So there will be fuzzy edges to these things. More worryingly is really kind of what do cuts do to you? I mean smearing is a problem. And you should never expect to see this thing like this in truth. But the problem is sort of more like cuts might give you a smoothly varying kind of reduction in acceptance as you go across here. So although in principle it may be uniformly populated, by the time you've got some cuts in the density of points might be decreasing, and things like this, and that will change the ability to find an edge. And also, just edge finding is a sort of a not very well defined thing. You can make it well defined, you can say, 'I have a template function, it's like a "inaudible" step that has been smeared, and I will try and fit this', but it's not always clear that you can really fit to what you want to fit. And you can be very distracted by backgrounds that happen to form a peak, or something, or cross the distribution.

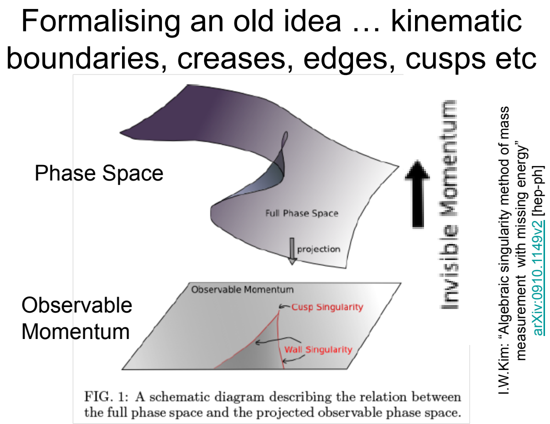
[Student]

So let me ask a question: so really you've got 3D kind of momenta, so things within three dimensions, and then you're folding things. and I can't help sitting here and thinking about the Dalitz plots, which of course live in two dimensions, and then they have very well defined kinematic boundaries, which you sometimes can see, and sometimes can't see, but then there is structure on the interior. Okay, basically associated with resonances. And so you're going to fold all this stuff over, and of course you have to do it, because you don't know which is which, so one verses high. But is there going then to be structure on the interior of the origami picture, which is associated, like in a Dalitz plot, with peaking at various values of invariant masses? Or is that too naïve?

[Lecturer]

It can happen. I mean there'll be some examples of some Dalitz plots later, and it is very closely related to Dalitz plots. In a Dalitz plot those features that you are describing, these bands and things that can appear, basically come when you unpick what you could first off consider to be an ordinary three body decay, if a particle goes to three, and we just ask what is the face space for this. When you then do your final diagram, or whatever, to unpick what's going on inside then the one goes to three becomes a one goes to two, and then a two goes to two... sorry, a one goes to two, and a one goes to two, and you get features from these sort of resonances inside. Now here... [someone talking "inaudible"]... Exactly. And what I'm saying is, here this is completely unpicked. Here we've placed the unpicked thing from the beginning. Yeah? So here we've not treated this as a blob, and then started to try and look for the internal features, we've sort of made a strong statement that we think the internal features are there, and then we're starting to map it into this space. So in a way in this particular context we sort of don't have room to see extra things like this in here, because they've already gone in by construction, in a way. But of course, yeah, if you do the reverse, if you just try and do a Dalitz plot like thing for this, this and this... Well, really for Dalitz plots you sort of want... directly for Dalitz plots you want three things, and you usually don't have any missing parts. You can construct things like Dalitz plots for these things where you are trying to be agnostic about what is in there to begin with, and then try and see this sort of shape and structure formation forming, in the way you described. So here no, but in related context, yes.

So what this is all really just could be summarised in a sort of a general of saying is, that whatever process you're looking at, and here we were looking very specifically at just this one, because we can talk about it sort of fairly finite, but that's not of course the only process you can look at, there's umpteen, hundreds of them. And the point is that there is some actual sort of face space... configuration space that things can reach, and that lives in some higher number of dimensions than you can actual see, because what you can see in your observable space is usually less than the full face space, particularly if there are invisible particles. So the available places that things can get to are sort of projected down onto our observable space, whatever it is, and there are things that we can look for in this observable space.



[90]

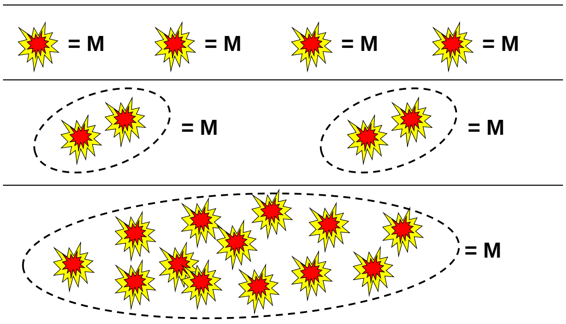
It could be shadows from... matrix elements may impose particular places of this face space as being more likely, and so you might have a band in this face space that is particularly well favoured, and it'll look like a shadow on here. Or this face space may fold round, and you'll get what I call cusps, high points, which will be the projection of this part, or wall singularities in these projections. So you can, if you like... Wang Woo Kim has sort of promoted and suggested ways of trying to approach the general face space for a general process without worrying about the details, and trying to derive variables that will tell you where these endpoints, and points, and cusps are going to be, and use them as your observables.

[90] Figure taken from [29]

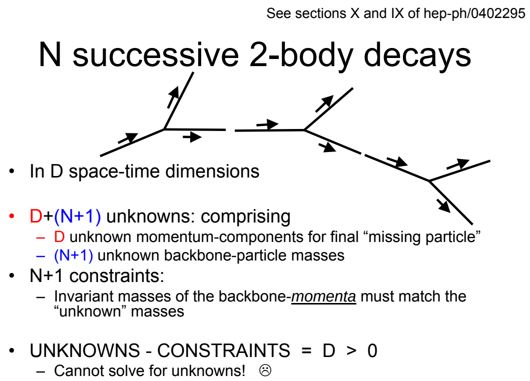
Adding even more assumptions …

Apparently we're now going to add even more assumptions, but I don't know what they are.

Let's consider what happens when we allow ourselves to look at more than one event ….



Ah yes. So up to now we've been considering single events at a time. We had this event here, we plotted something, it told us something. We didn't necessarily get the masses from that. We did have to plot many masses... many in a distribution to get an end point. But while I'm not for one minute suggesting you should ever just look at the highest energy event and say, 'Well, there's my end point, because there's the...' you've got to fit to end point positions to distinguish single from background. Nonetheless, in principle we're sort of getting a measurement of an end point from an extreme event. So we're sort of using one event at a time, even if we've got a distribution. That's not the only thing that people have suggested you do. Some people say, 'Oh well there could be reasons why what we should do is consider two events at a time, or...' And I actually start to feel a bit squeamish when people say that, because something about the sort of statistician in me says you just shouldn't divide our data set up into pairs of events, and start playing with them, even if you make all pairs of events in there. But I don't want to complain too much about that, because it may be the right thing to do.



But let's give you some examples.[91] So if you have... The picture shows three successive two body decays. What about if we imagine there were N of them? One, two, three, N. N successive two body decays, in D space time dimensions. So D is four for us, right? And we are trying to figure out something about this. The masses of the particles on the backbone. What don't we know? The things we don't know are the one, two, three... N+1, we don't know the N+1 masses on the backbone. That's the thing we're after, thing we're trying to find. And we don't know the four components of... sorry, the

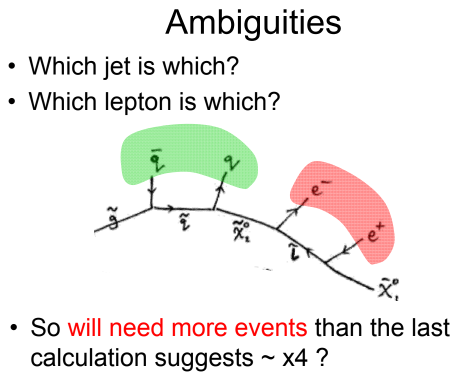
[91] These examples are closely connected to sections X and IX of [30], and [31]

D components, if you're working in D space-time dimension, we don't know the D components of the invisible guy at the far end. But we do have some constraints. Because the invariant mass of this invisible thing, plus the hypothesised momentum here should square to the mass... one of those masses we didn't know, and so on. So there are N+1 mass constraints that come from that. And the unknowns minus the constraints ends up being D. So we've got more unknowns than constraints. So basically you can never solve this process to find out where the neutrino is going if you don't know your masses. Right? Mainly we know two... it's possible if we might know two components here, because we might be able to use a PT mis-constraint perhaps, if we thought there was only one invisible thing. But then this would still be D-2, and that means that unless space time only has two dimensions we still can't solve.

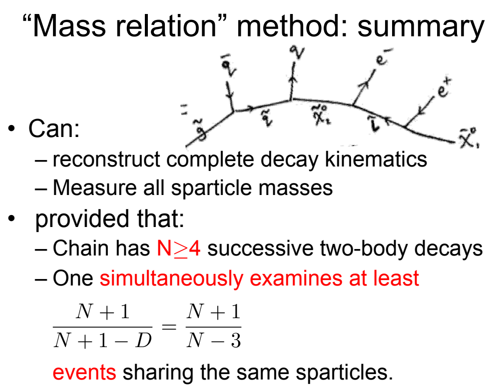
Why not look at K events?

- K events, each (N successive 2-body decays)
- KD+(N+1) **unknowns**: comprising
  - KD unknown momentum-components for final "missing particle"
  - (N+1) unknown backbone-particle masses
- K(N+1) **constraints**:
  - Invariant masses of the backbone-*momenta* must match the "unknown" massses
- UNKNOWNS - CONSTRAINTS = $K(D-(N+1))+(N+1)$
- System solvable for $K \geq \dfrac{N+1}{N+1-D}$ provided $N+1>D$ i.e. $N \geq 4$.

On the other hand, what if we had K of those events? Okay. K events, each containing in excess of two body decays. Well what doubles up? The thing is if you assume that they all come pharmageniusly from the same backbone, if for some reason... And that's a big assumption, and this is one of the things I'm not so pleased about here. But if you think, right, I've got one predominant chain, so it's the same masses each time, you don't increase the number of mass unknowns, but you do increase by D dimensions times K times the... What you do increase each time is the lack of knowledge of where the neutrino, the invisible particle was going. Every event has got four invisibles there, four unknowns. Anyway, do the [?K thing] now. Unknowns minus constraints is now... it's a bit of an offset, by the term that grows linearly with the number of events that you're putting together. And what we want is this thing to go negative, or hit zero. And fortunately, you can see here, although we can't play with the number of space time dimensions, we can increase N, we can increase the length of the decay chain. So there's a sort of break even point. You can figure out where your invisible particle is going by looking at K events provided K is greater than or equal to this thing, where N is the number of particles in your decay chain. What that boils down to for us in the number of dimensions that we've got in our space is that N must be bigger than four, greater than or equal to four. So if we've got a length for four successive two way decay chains then we can, if we take enough events, solve and find out where things are going.

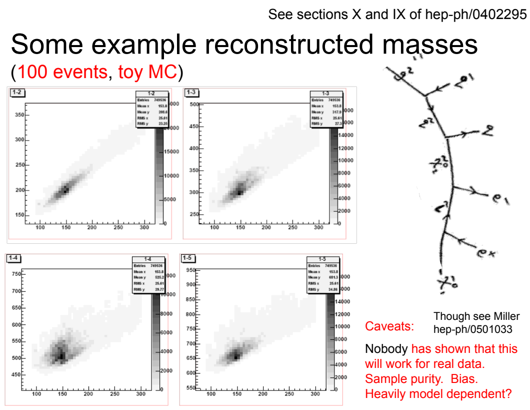


Okay, that bound is modified by a few things, typically the ambiguity is right. If you want to solve for these constraints, you want to know where things are going. You want to be able to apply the constraints, you want to take the momentum of this plus the hypothesis of momentum, must get the right mass and so on and so on and so on... And you don't know whether you're putting these in the right order so there'll be extra N fold ambiguities, so that bound of K, K events might have to go up from two to five or, you know, a few more, to get the system solvable. But, you can get there in the end. And so people who play with these methods call this, sometimes, mass relation method is sometimes a good term to use Google to search for.



And refers to apply to these length for chains. And with your process, you go through and take your events in whatever they think, when people, Giacomo Polezello and Mihoko Nojiri and somebody else, I forget, who first played with this, they actually played a sort of modified game. They said well we already know three of the masses from dilepton edges and things like that and were just trying to find the remaining two. So they only had to look at pairs of events. But you should look at quintuples of events if you want to do it from the beginning and that, they say, 'Okay, we can... Once we where this thing is going... Once we know all of it's four components then we know what it's mass was because it's just the invariant mass of that thing and we can plot it on a... we can plot all the masses we find... we find that we get nice little localised spots.

So, I suppose in a way, this should really come with a big health warning. Which is... you're making a lot of assumptions now. We're starting to go through that process of making lots of assumptions, that you're quintuple of events all have to have the same backbone. How are you going to get that? Now... But maybe in a sense that one redeeming feature perhaps of taking odd things like quintuples of events or all quintuples is that, sometimes, your quintuple will actually be right. You will have taken four events, five events, that really do come from the same backbone. And at least for that event, you plot the right invariant mass. For others, you don't have the right combination, but that sort of means that they can go anywhere because the things are really under a poorly constrained order. There's no meaning to the solution in some ways. So, you can be sort of lucky and this what people sort of see a lot of time that, even if the fraction of events where they get this combination right is small.



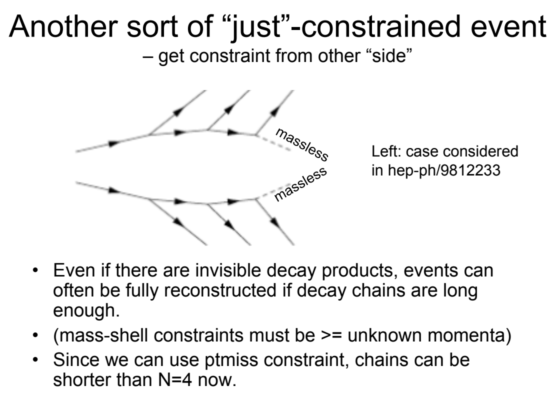
The plot has got some structure and the wrong combinations don't have structure and are spread out. So you can sort of try and... so you can still try and see a localised peak where you've got the answer right and then some wrong answers spread around it, but with some structure leading you to believe you've got the right place.

Dependence on reconstruction resolution.

N=4 two-body decays

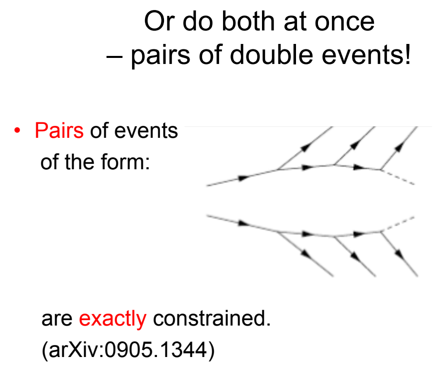
- Fewer than 5 events
  - Under constrained, cannot solve
- 5 events
  - Can solve in principle (ignoring ambiguities)
  - Can treat events as "ideal"
- More than 5 events
  - Over constrained. Potential for inconsistency.
  - Reconstructed events will not "make sense" until resolutions are taken into account.

Just on a sort of note here. So I said if we had this N equals four system. Fewer than five events we can't solve, we can't do things. With five events then, at least in principle we can solve and I have said ignoring ambiguities. And so sometimes people, what I think is fall into the trap of actually trying to write down analytical solutions for this solution, and doing this and solving. But I think that's a bit, sort of, wrong, or misguided because what if you looked at more than five events because this is what the statistician in me wants to do. It's says "Forget about it". You know, a likelihood basically looks at all events at once. And what this is, is sort of a half way house between doing a full likelihood and not. If you had more than five events and took the attitude "I'm going to solve these for my constraints" you find you couldnt solve them because this system would be over constrained. You'd have too much information to be able to solve, there wouldn't be a consistent neutralino, slepton, squark, mass, that fitted these things. Of course, this is because, the momentum that you get out of your detector, have experimental resolutions, they're pointing in slightly the wrong directions. And if you put in your knowledge of how much these things could be wrong by and you put in extra degrees of freedom, extra unknowns, to account for the fact that none of momenta are really correct then this sort of over constraint relaxes and it becomes something that in principle, you could solve again. But what it tells you is, what you're really doing, is really fitting, you're really fitting these things. You're trying to get consistent answers for reasonable assumptions. And so I encourage you not to think of this of sort of, solving things, but more like fitting multiple events. And so, in principle, you could imagine always extending it to look at the entire event sample, at once, if you wanted to. The disadvantage of doing so, would be that, then you can guarantee that loads of your combinations are wrong and your fit will be sort of heading off. So the advantage of fitting only five at a time is at least some of the time, you've got it just right.



Okay, other sorts of things that are getting very, very specific now. You don't have to have the two events that you're sort of putting together, or the N events, in different events, they could be two parts of the same event. So for example, the first case back in 1998, Hinchcliffe and others [32] were looking at the GMSB model, where the K chain was as short as this but it was in GMSB so this was supposed to be a gravitino at the end, and so, it was mass less, and so, that was one thing you already knew, at least if you were hypothesising that you were working with GMSB. And in that situation, when, when you see that these things are mass less, even though there are only three successive two body decays on each side, you have a common PT mis-constraint relating these things, and you do the maths and find out that a single one of these double events, is sort of solvable or fittable.

What if you relax that constraint and don't think you're in GMSB and you don't think you know that that's supposed to be a gravitino and massless. Hang on.[92]

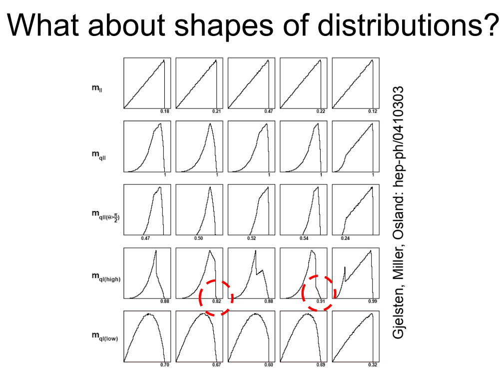


Pairs of events. Yes, that's right, yes. So when you knew it was mass less you only need one event to do it, if you believe that it's not, then you need two of these events to do it. And McElrath and others [33] will tell you how to do that within... There's actually quite a lot of complicated maths to try and get the answers out, and what they have got is some well written libraries that take you away from the trickiness of doing the maths.

So then it got really complicated because we are having to make a lot of assumptions and we are trying to believe that they are all true. What about the next stage up in complexity. Maybe it doesn't seem like a stage up in complexity, maybe this will seem like a step backwards, but to me as a experimental physicist it seems worrying, and more of a step up. Now that is people

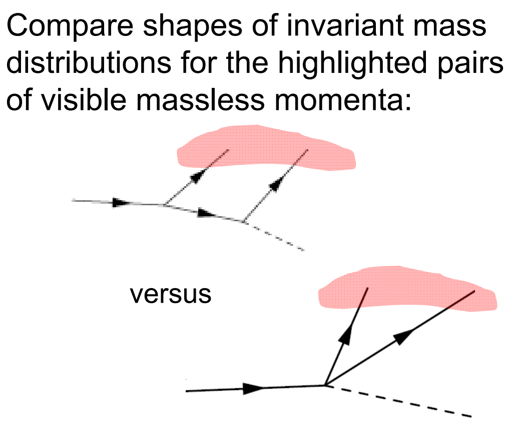
[92] I was getting a slide ahead of myself here

'Well you have been talking endlessly about endpoints, but these distributions have got shapes, so why don't you fit to the shapes?'

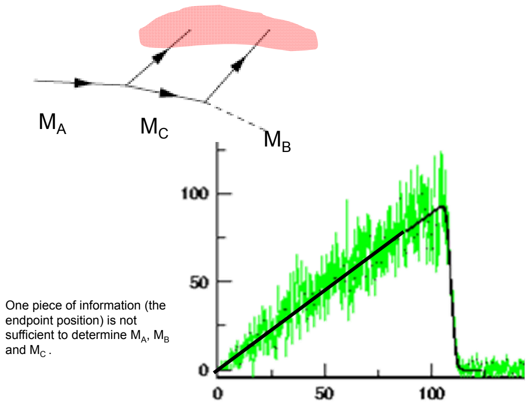


And... let me get rid of one sort of slight misconception that I may have given you. I don't want you ever to think of an endpoint as just being a number or anything. Whenever you find your endpoint you have got to find it by a fit, and this will be smeared to a slightly slumpy thing by any realistic experimental resolution. And so you will always be doing shape fits to get your endpoint position, and the point is the end point you are measuring has sensitivity, because you have started something that was upright and firm to start with. So you are using the shapes anyway when you are fitting these endpoint positions. But what I mean is here, what happens if you try and use the shape of the whole distribution to tell you something. Before we leave endpoints behind, if you look at the LL endpoint. However you fiddle with the mass it is always like a triangle, but the... some of these endpoints, the QL high, that is the invariant mass distribution of the higher quark lepton invariant mass you can construct has sometimes a steep edge, sometimes a jagged edge, and sometimes a tiny foot. It is jolly important, and there are some papers, Konstantin and others emphasising this, by Miller and Osman earlier saying that, when you do parameterise this shape you really should be careful with your choice of distributions, because some will have these tiny feet and if you interpret the salient part of the edge as being your endpoint you will get completely the wrong answer.

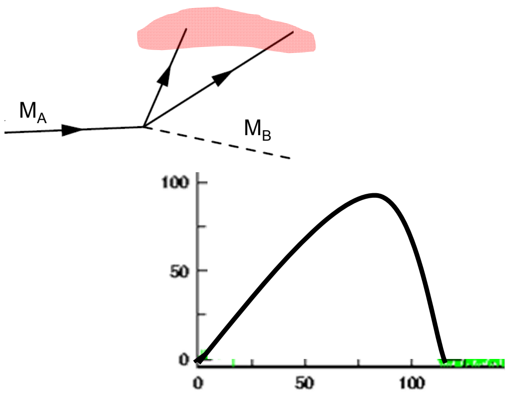
That is not what I am talking about here. Here I want to talk about what happens if you try to use the shape of the distribution itself, not with reference to the endpoint, but trying to get something from the actual shape. So let us compare this situation where you have a three body decay that is at once versus with an on shell process in the middle:



We know that when there is an on shell process in the middle we get this straight shape:

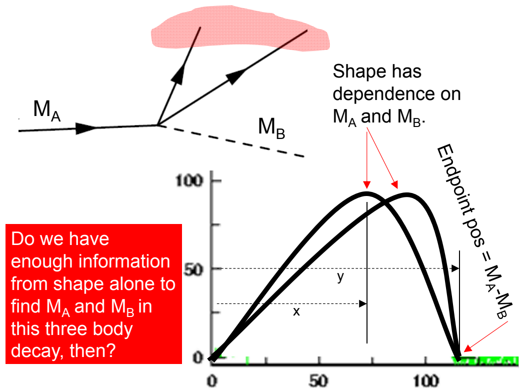


... if you have phase space or SUSY spins. When you have this [93]:



... which is a bit of a decay - if these were all visible you would be analysing as a Dalitz plot - but when it is invisible you don't have access to the variant masses that you need. This will have an invariant mass distribution which is not triangular. Here I am trying to emphasise the difference between that and this. That and this.[94] Right!

So shape information. Shape is useful. Hopefully if your detector resolution is good enough you can tell the difference between that, once it has been smeared and this. So you have partly helped to separate between these two possibilities. So maybe it has even told you that the slepton is too heavy or something, but there is something there.



In fact curiously if you play around... there is only two degrees of freedom of here, assuming that these things are mass less. You have got a MA and MB. You have got two degrees of freedom here.

The endpoint of this distribution is at MA-MB And that is a salient... that is an endpoint like feature. That... Yes fit with the shape, whatever, get it out. But that is the endpoint, and that is a salient piece of information. I have always said that repeatedly in this thing: Mass differences are relatively easy to get out. But when you fiddle around with MA and MB although at a fixed MA-MB that will stay where it is, but this shape wobbles backwards and forwards a bit.[95] So we should be able to, if we get a shape fit here, extract *not just* the mass difference but (from this variability) the mass MA (or MB) itself! Yes. The second degree of freedom!

[93] [all-at-once three-body decay]

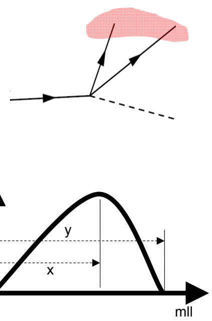
[94] [Here I flicked back and forth between the last two plots emphasising the change of the lineshape from triangular to rounded and back again.]

[95] [here I was emphasising the slighly different peak pos in the black curve at the bottom right of the slide, when MA (or MB) is varied at fixed MA-MB]

If you define... Where's this peak? I have labelled this the peak position x and the maximum point y. So this looks like the ratio of x and y here is about two thirds. The peak is coming two thirds of the way long. Now if I call that distance two thirds along, I call it R. The ratio of X and Y two thirds of the way along is R. You can prove. And this is one of the exercises that I think you could do and it is quite instructive. You can prove that R is stuck between $1/\sqrt{3}$ and $1/\sqrt{2}$.

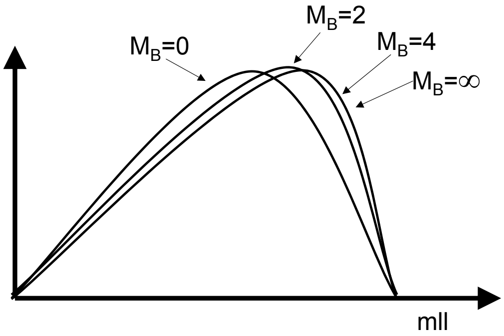
Exercises

- (12) Determine the shape of the phase space distribution dσ/d(mll) (up to an arbitrary normalizing constant) for the three-body decay shown below. Assume massless visibles, and arbitrary masses for the parent and invisible.
- (13) Prove that r=x/y must lie in the range $1/\sqrt{3} \le r \le 1/\sqrt{2}$. (Note this means r can only move by ±0.06 … not far!)
- (14) Estimate how many events (approximately) would be needed to distinguish two r values differing by 0.012 (i.e. ~1/10th of allowed range)



Now no matter how much you play with these masses, you can come down to R of $1/\sqrt{3}$ and go up to R of $1/\sqrt{2}$. So around its sort of central value it can move by plus or minus .06. 6%. That is not very much. If you then add a bit of smearing to this, and the thing sort of splodges out, and you are also trying to find that, you can actually really surprise yourself just how many events you need to be able to spot a distant... to tell the difference between two distributions that have got say R up at 0.02 and R at 0.01 or something like that. You know to the relatively reasonably spaced bits of R.

At fixed M_A-M_B you should find



In fact if you plot this on sort of mathematica and you sort of do a bit of "inaudible" what you will find is, for fixed mass difference then when the daughter particle is mass less you have this thing down at the $1/\sqrt{3}$ position, and then when you sort of increase... I mean I haven't got any particular units here, but when you increase it, it will move by some sort of small reasonable number of DeV. You will almost completely end... and infinity is just right beside you. So basically Yes in principle you have sensitivity to the absolute mass scale because there is a second measurement you can make here. But, the moral of the tale is that you have almost no ability to distinguish between bigger masses in a way, because the distribution saturates very quickly, as you up that mass scale.

Yes and no ..

- Putting aside experimental fears concerning efficiency and acceptance corrections …
- … huge errors in the fit, and very poor sensitivity to absolute mass scale. See next exercises.
- This is why endpoints, edges and resonances are good, but shapes less so

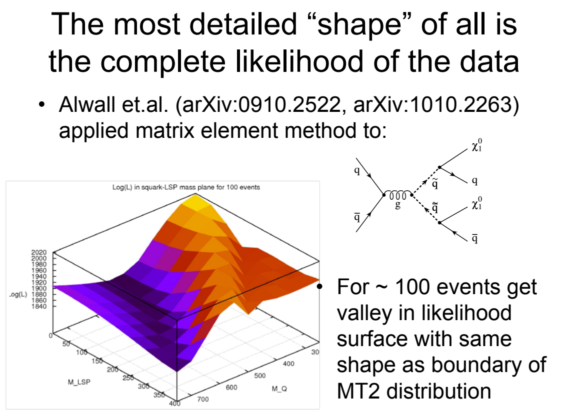
So it all depends on what nature... If nature is giving you a very low mass, a visible particle then you have got... your distribution will be at the end where it has the biggest velocity and you might be able to detect it. But if you are unlucky and things are quite heavy, moderately heavy you have no sensitivity to the mass absolute scale.

[Student asks a question that is sadly inaudible on the record.]

[Lecturer]

Yes, that's right. So the things that I have been showing you here have all assumed mass less objects in the invisible part, and the mass differences between any of the backbone particles are all much bigger than that. So when you get things that are almost... When you crank up the mass so things become almost mal-shell, they reach a point where they violate the assumptions that I have just made. So yes. In reality of course you can smoothly dull natures nobs and get this thing to smoothly move into another thing. But it basically spends a long time in this regime and then goes [erruppp!] as it flips into the other regime quite quickly. So what is happening in the middle is a small part of stuff to worry about.

Okay. So shapes. They can contain information in additional to endpoints but they are very hard to use, I think. So what is the most shape you can ever use? The most shape you can ever use is basically the likelihood of everything, like all your events. So you take your entire data sample and you just say "How likely is this under my hypothesis?" Now that is in a sense, it is a multi-dimensional shape fit. They are very costly to do in CPU. Sometimes you cannot process many events and you have to be very very careful of how the backgrounds affect those fits, because you need a model for the likelihood of the background. And that is often very tricky to get.



Someone has managed... Alwall and his friends [14, 15] have managed to do a full likelihood analysis - they are the only people who have - for the squark squark to quark, neutralino, quark neutralino case in a background free scenario. So they got rid of all backgrounds, just detector level only and said "What can we learn if we do the full likelihood to this, can we extract the neutralino and squak masses, because we should get the best possible measurement at all, if we try and do this as a likely fit". And I think they could do it for about a hundred events, they could kind of crank up the code and they could process about a hundred events at once. This is the plot of the likelihood surface and it is the valley - the way they have drawn it - the valley is what they think is important.

What we have got on this axis is the mass of the squark and the mass of the LSP. Unfortunately they both go backwards, 700, 600, 500, 400 and LSP is going in the right direction.

The point is that in this space of squark neutralino mass what they found out is that from a hundred events you are localised to a degenerate kind of bottomed curve. I mean it is not completely flat but it is pretty flat, and if you draw with pen that line there, and turn it round, what you get is the MT2 kinematic endpoint distribution. The dividing line between happy face and smiley face.

[Student] "inaudible"

[Lecturer]

Well I don't know all... Sorry... You think they are using rate information of the degenerative/

[Student] "inaudible"

[Lecturer]

I know they certainly use PDFs and things like that and they are happy to go with PDFs. So yes they have a cross-section model that's... Well maybe they took out. Heavier things are harder to produce and the absolute mass scale will go down. So if you have that cross-section information available you should go bam straight to the mass scale. So I think what they are doing here and I am guessing here off the top of my head, is they are trying to look at what is the information that is all cross section information. So that is probably the one thing that they have taken out. I think that is what they have done.

The surprising outcome, from my point of view, of this is that... okay that is only a hundred events but it is the same information that we already thought we could compute from some other process. So what I am telling you is this kind of non cross-sectional information from the likelihood is of the same type of information that we already know we can get out. It may be more powerful... I haven't actually managed to figure out... the steepness of this well tells us that from those 100 events they are squeezing more information than we could get out of the kink. But the kinds of information that they squeezing out is the same kind of information that is squeezed out by those rather worrying methods that we have already talked about, which makes me fear that we might not be able to do much better in many cases than the kinds of things that I have already been talking to you about.

[Student] "inaudible"

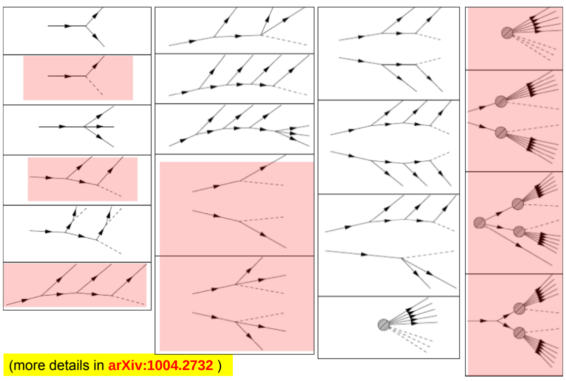
[Lecturer]

Yes. If you turn that the right way round and plotted it then that... I do lots of curves for MT2 that were sort of shaped like this - there is a kind of boundary thing happening inside. And if you kind of flip that around that is what you get. It is the maximal curve that I am talking about, because an individual event may undershoot this. But the maximal endpoint curve is the same one. And there is a tiny dip here, that localises you to, in principle, you wouldn't believe it... but it has got about the same power as the kink method has, which is not very good at localising you to a particular mass scale either. So I think this should all... I think this is good work and I think it should worry you a bit... Or it should perhaps encourage you to try and beat this, or to find out how... whether the information content here is... actually the same information might be coming at five times the rate from doing it like this, than it comes from doing it like the kink, because perhaps you are using other bits of your events to give you this information.

That's probably enough on mass measurement techniques!

So that is probably enough on mass measurement techniques. So I don't have conclusions. I never really have conclusions ... I think my things are just a ramble. Oh.... Yes I did try to write a conclusion. But it is just one of those kind of caveat conclusions where you say 'There's loads of things I didn't talk about'.

Have only begun to scrape the surface.



Not time to talk about many things

- Parallel and perpendicular MT2 and MCT
- Subsystem MT2 and MCT methods
- Solution counting methods (eg arXiv:0707.0030)
- Hybrid Variables
- Phase space boundaries (arXiv:0903.4371)
- Cusps and Singularity Variables (Ian-Woo Kim)
- Why wrong solutions are often near right ones (arXiv:1103.3438)
- Razors
- and many more!

I have only scratched the surface of the variables that have been discussed. Even the review of mass measurement methods arXiv:1004.2732 makes only a small dent in 70+ pages. However it provides at least an index ...

They kind assume that I don't wish for the record, camera, I don't wish to cause any offence to any person living or dead or otherwise whose work I haven't mentioned because it is all very good.[96]

Take home messages

- Lots of approaches to kinematic mass measurement
  - some very general, some very specific.
  - very little of the "detailed stuff" is tested in anger. Experimentalists not universally convinced of utility!
  - very often BGs present serious impediment.
  - theorists and experimenters should pay close attention to zone of applicability
- BUT
  - Finding sensible variables buys more than just mass measurements - e.g. signal sensitivity

So there you have got lots of kinematic measurements. Some of them very specific... - I hate it when I can't read my own transparencies -. Let's skip that.

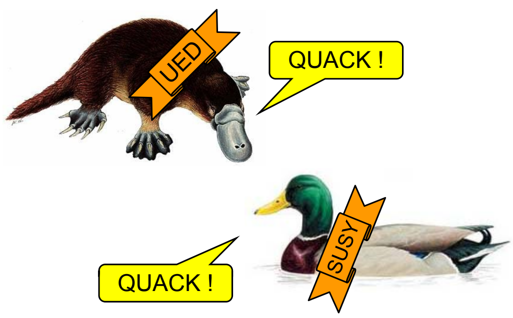
[96] The slide mentions "parallel and perpendicular MT2 and MCT" [34, 35, 36], "subsystem CT2 and MCT" [37], "solution counting methods" [38], the Razor [39], Hybrid variables (e.g. [40] or [41]), "Cusps and Singularity Variables" [29], why wrong solutions are often near right ones [42]. Many more could have been added, and pointers to those extant prior to 2010 may be found in our review [1].

What is "Discovering SUSY" ?

- E.g. – what makes Supersymmetry different to Universal Extra Dimensional models with Kaluza-Klein particles.
- One part of the answer:

SPIN

So now I am supposed to tell you something about spin. So what is discovering SUSY? You can maybe find chains of particles, perhaps SUSY is now going to be ruled out by the LHC. If you have got some kind of chain of particles you want to say things like: Well is it SUSY? What else could it be? And one point of the answer is: basically spin. What might distinguish SUSY from UED? What the spins of the particles are. Now at some point in 2009 I gave a very short talk on the spin at some kind of thing in Prague somewhere. And I had this slide, not all things that quack are ducks.



Not all things that quack are ducks!

QED SUSY, quack quack. The thing is I... It is very visually exciting but I cannot remember what connection this has to spin. It is something to do with that you should feel it is vitally important to separate these things, but I don't know what the quack... Maybe the quack is to do with the shapes of the endpoints or something.

We will see two important themes:

- Mass measurements will precede(*) spin determinations
- "Spin measurement"(**) should not be confused with "sensitivity to spin"

(*) or will at best be simultaneous with
(**) Here "spin measurement" means "determining unambiguously the correct nature (scalar, fermion, vector) of one or more particles in a decay chain or model

So the two important themes that I hope that will emerge from this by the end of this summary of spin measurement methods of the LHC is that you should see that mass measurements have to precede spin determinations or at best start moving at the same rate. And that spin measurements... So saying the spin of this part is to spin half particle shouldn't really be confused with sensitivity to spin. Does it have spin that is non zero? Most of the time really all the LHC might be able to do, if the wind is blowing in right direction, is say things like: This thing has spin. Rather than perhaps exactly what the spin is. That is partly because of how inter-related spin measurements are to mass measurements.

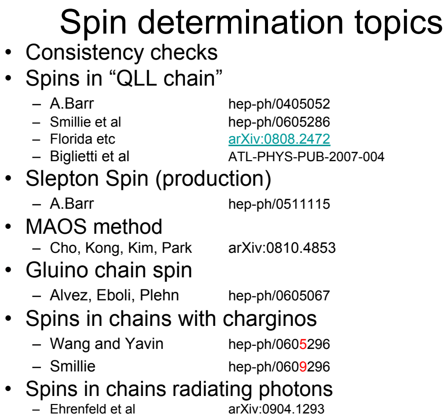
(more info at)

A REVIEW OF SPIN DETERMINATION AT THE LHC

Lian-Tao Wang and Itay Yavin

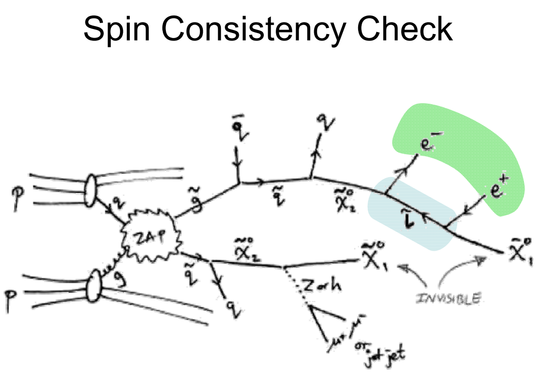
arXiv:0802:2726

There is a review... So whereas I... I have written a review of mass measurement methods, about a year before that, Lian-Tao Wang and Co. [43], produced a review of spin determination at the LHC. This talk will only cover a small sub-set of that.

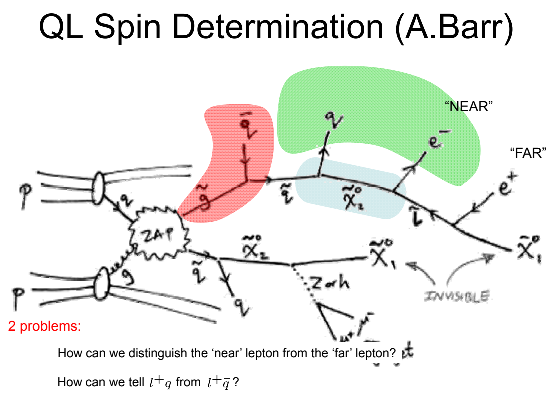


97

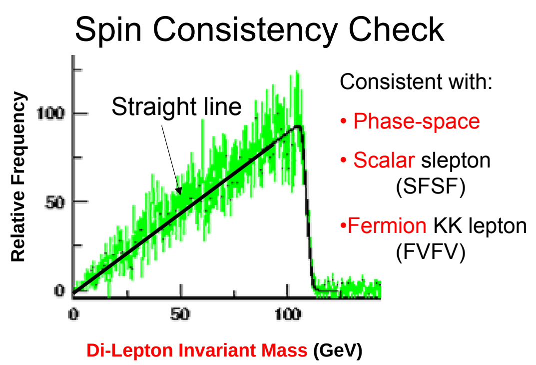
They shouldn't be blamed for anything that I say that is wrong, because I haven't taken my material from their review. But if you want to go to a reference material that will help you, then that is a good one.



The kinds of things that people have been able to look at is trying to think about spins in these QLL chains, these sort of longish chains. Then there's sort of separate process, I will talk about looking at spins in direct production, sleptons. I will talk a bit about something called the mouse method and how to look at spin with gluinos. And then other methods that I will not talk about.



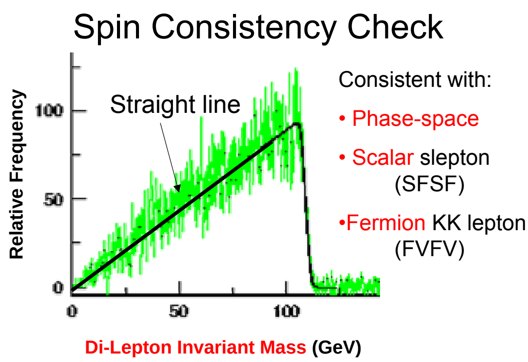
So the method... the first paper that came out of the LHC... from someone worrying about how we will measure spins on the LHC, was from Alan Barr [44], and it tried to identify... It tried to say: "Could we figure out the spins of anything down here, in this chain. Let's focus on say the neutralino2. I said to you earlier... we have seen it before, that the shape of the... this E+E- spectrum is the triangle.



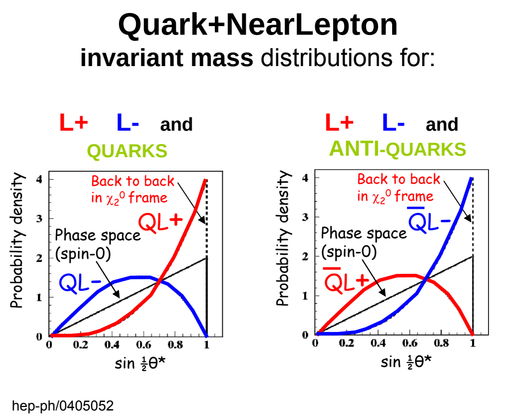
We have been over that many times. That is true for that SUSY spin, or for face space. But this has not got the same spin as that. This

[97] This figure summarises the spin determination topics that were intended to be covered in the lecture: consistency checks, spins in the "QLL" chain [44, 45, 46, 47], slepton spin (pair production) [48], MAOS method [49], Gluino chain spin [50], spins in chains with charginos [51, 52], spins in chains radiating photons [53].

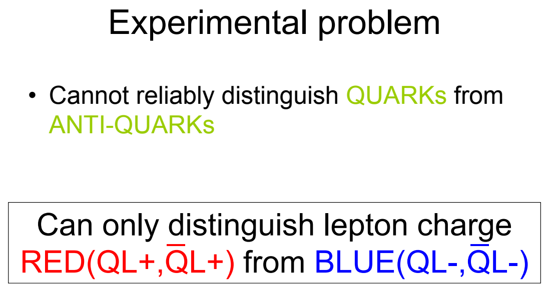
particle here is the neutrolina2, that's a fermion. So the invariant mass distribution of the quark and the E- are there, just ignore the... Pretend I haven't drawn the first quark. I am just assuming the whole process has started with a quark so there is no ambiguity you have to worry about, whether we have got quark and anti-quark.



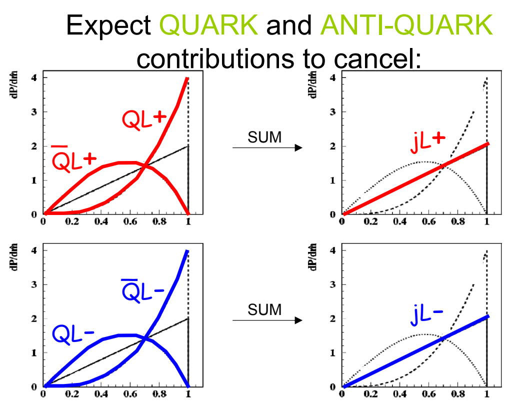
That invariant mass distribution - in the green - will not be a straight line. It will not be a straight line like this, it will be,



if you are doing quark and combining it with a lepton+, okay because the leptons and anti-leptons have opposite helicity. The distribution for a quark lepton+ should actually be more peaked. In the quark lepton- the distribution turns over and has a 0 at the endpoint. But of course we can't tell whether we are looking at quarks or anti-quarks, because we are a hydron collider with an actual machine, not some hocus-pocus thing in a theorist's head. So we actually may well be plotting... we may have a jet that came from an anti-quark. And the anti-quark lepton+ unfortunately turned over just like that one did - this blue one here - and the anti-quark lepton- distribution goes up.

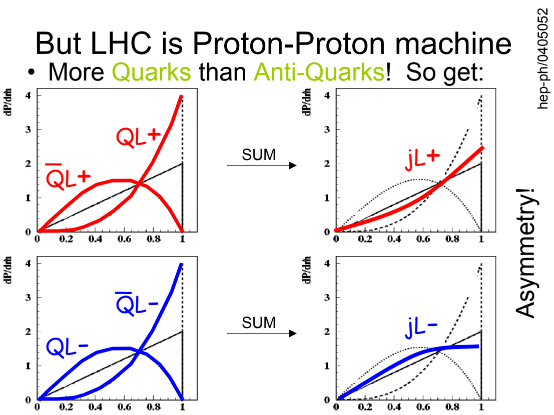


So since we cannot distinguish red from red - the things I have coloured in red are the things that have got lepton+ in. So I can generate red curves, at least the sum of these two, because I can measure the size of my lepton. So I can construct a jet lepton+ distribution and a jet lepton- distribution but they will be the sum of red and red, or the sum of blue and blue.

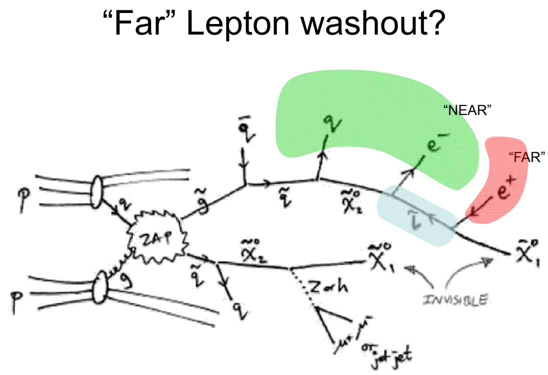


When I add those things together, because we can't distinguish those things. Adding that to that gives that. Adding that to that gives that.

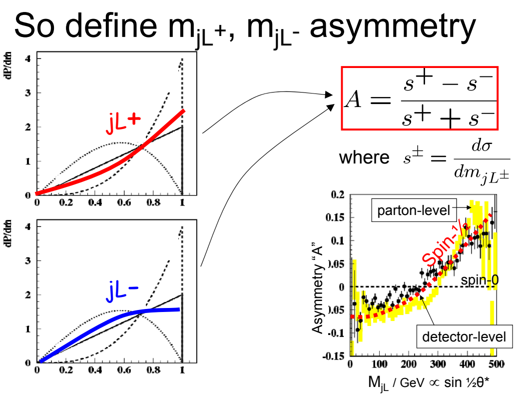
So the actual observable distributions that we can generate hide, do not tell us anything about the spin. What a shame. And I remember saying to Alan, when he was... 'So you will never measure the spins of anything... don't bother. You will never measure the spins of these things'. But he persisted and he said "Oh actually I am going to look at this anyway. What does the Monte Carlo tell me?", because we are experimental people. And actually he got that and this:



, and we thought "bugger", it is the PP collider. You have got... It is quark quark, not quark anti-quark at the Tevatron. You have got more quarks in your initial state so you will have more quarks and squarks in your final state. So actually you get a slight excess of these over those, and the slight excess of these over those. So you can't... So the jet lepton+ distribution, fortunately because we have got a PP collider can have asymmetry in there. So I was very disappointed [98] ...



There is this other electron down here and we don't know which order these things came out in, so in fact when we construct our quark lepton-distribution some of the time we will pick up the lepton- down here, and that is not supposed to have this spin correlation, that is going to be... Unfortunately that gets sort of washed out. It has got spin correlations but because there are extra unknowns and things and decay things in there, they don't... It is not a strong...



[99]

And so even when you add in those things you still retain some asymmetry [44] and if you plot this over this, that says is the recommended thing, to get the asymmetry safe from too measurement effects to make your systematic and

---

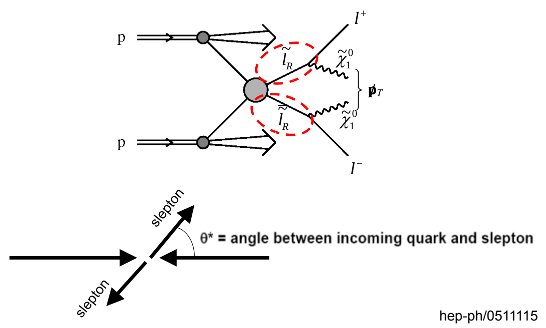
[98] [that he saw this and I had said it was impossible]

[99] Figure from [44]

certainly "inaudible". The ratio of these things is this, asymmetry curve. So if there was no spin whatsoever this would be flat, and deviation from flat tells you you have got a symmetry between these distributions and gives you sensitivity to spin. Now, I emphasise that sensitivity to spin rather than say a measurement of the spin because as you will see from later... because this is highly... The way these things move is very related to whether or not this is a right squark or a left squark, and a right slepton or a left slepton and so on. So what you get is sensitivity to spin but not always the measurement of the spin.

Different method altogether

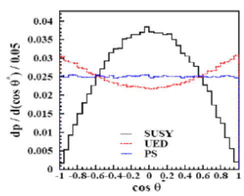
Direct slepton spin detection:
qq→Zγ* →slepton slepton



I am going to give some glu stuff. Different method altogether. This is the second idea. For a long time people at the Linear Collider had been saying: The reason you must build a linear collider is because we can measure the spins of particles. We will collide our particles and you will make... say slepton slepton. And the angle feta star between the slepton that is omitted and the beam line is clearly very sensitive to the spins of these sleptons. And you have got a nice clean environment and so you can work with it and by looking at the distribution of that feta star, you can find out how... what spin of your slepton was. [48] Now, this is a bit harder at the LHC because it is a proton proton collider, messier and so on. Let's just have a quick look at what that the theta star distribution looks like.

Look at slepton production angle in c.o.m.

- θ* = angle between incoming quark and slepton



$$\left(\frac{d\sigma}{d\cos\theta^*}\right)_{\text{SUSY = LHC point 5}} \propto 1-\cos^2\theta^*$$

$$\left(\frac{d\sigma}{d\cos\theta^*}\right)_{PS} \propto \text{constant}$$

$$\left(\frac{d\sigma}{d\cos\theta^*}\right)_{UED} \propto 1+\left(\frac{E_{\ell_1}^2 - M_{\ell_1}^2}{E_{\ell_1}^2 + M_{\ell_1}^2}\right)\cos^2\theta^*$$

Sensitive to spin, but can we measure θ*?

Define: $\cos\theta^*_{ll} \equiv \cos\left(2\tan^{-1}\exp(\Delta\eta_{\ell^+\ell^-}/2)\right) = \tanh(\Delta\eta_{\ell^+\ell^-}/2)$

hep-ph/0511115 ATL-PHYS-PUB-2005-023

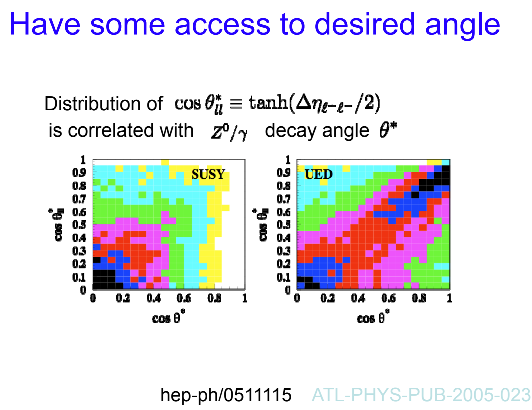
[100]

For SUSY it is basically fairly central. It has peaked at the central value. So if you are a SUSY event at the LHC you will tend to make up and down sleptons. Ones going transverse to the beam. Whereas if you are UED you peak more at the ends and you tend to kind of produce events that look like this, or like this, and then Face space is uniform in between.

What we really want to do is just measure theta star at the LHC. In fact it turns out that

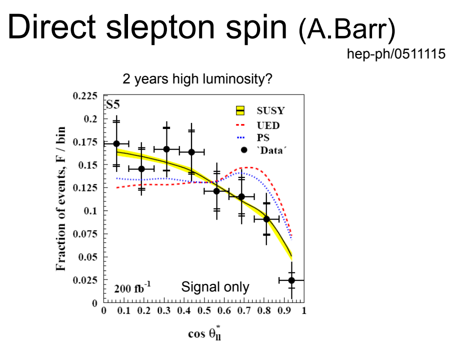
[100] Figure and content from [48]

it is not too bad, okay really if what we are trying to tell the difference between is this, which is SUSY and this which is UED, but want to be insensitive to boosts in this direction, this direction, then you should just look at repeated differences, because repeated differences are invariant under boost in this direction, and the repeated difference here is zero and here is positive or negative. So you can fiddle around a bit with your normalisation to try and take your repeated difference and turn it into an angle. And yes you have to worry about that you can't see the sleptons themselves - these sleptons are what you would really like to measure the thing for, but all you can actually see is the leptons. So how are we going to track back from these to those. Well if these things are going fast enough and they are sort of collimated, these decays will be collimated and this will retain some of the direction information of the thing that it came from. So that means, if we are going to use the momentum of this to define the direction of that then we are sort of restricting ourselves to cases where these things had enough boost, that is a valid assumption. And when you do so, then... that basically means, you plot a distribution of your effective theta star...[This effective theta star is labelled $\theta^*_{ll}inthenextslide.$]



[101]

This effective theta star is not really theta star but it is something that correllates with it.



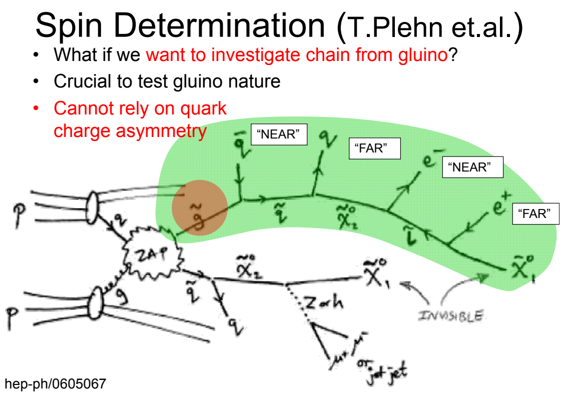
[102]

And for SUSY you peak at central theta star and you go down at the edges. Whereas for UED, say, you have less in the centre... It is trying to actually peak up here [103] a bit, but the detector efficiency is falling off at higher repeatedity as well. So it manages to get up for a bit and then comes down [104]. And you just have an ability to distinguish between these things. You need quite a lot of data though. That says two years high luminosity but it was written in 2005,

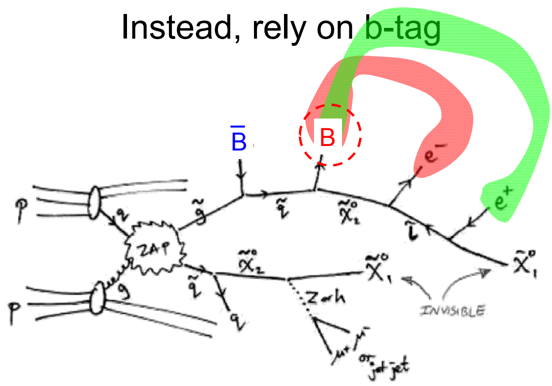
[101] Figure and content from [48]
[102] Figure from [48]
[103] [at the edges]
[104] [at the extremes of theta star]

which is the time when ATLAS was always quoting I think a hundred inverse bounds a year as high luminosity. So this is something like three years of running at a rate that is far in excess of what we have presently got. You need a hundred times as much data as we have currently got at least, to be able to do this.

Different again



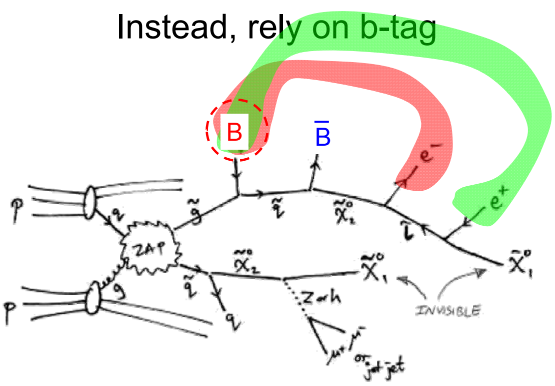
Different method again. Gluinos. I told you yesterday how not to write a paper. Do not call your paper something that it is not. Never accuse Tilman Plain of producing a paper that is not about what it is.[105] This paper was called 'It's a Gluino" [50]. What his point was, was that if you want to see a gluino, you want to tell its spin, otherwise it is not a gluino it is a Kaluza-Klein G thing or whatever. If you can say what its spin is, you are saying you know what it is. Now we have two near and far situations, you have got to have a near quark and a far quark, a near lepton and a far lepton. And we can't tell the quarks from the anti-quarks... - Ah yes. The crucial thing here, which I think probably motivated your question, is that although we might have an excess of quarks over anti-quarks in the initial state, once we make a gluino... What we are trying to measure is the spin of the guino, then this is kind of sorted all that out. That is now by the by. Now we have got something that is exclusively the daughter of a gluino, there's as much quark as anti-quark in there. We can no longer determine spins in this chain, if we start with gluino, because at this point that quark there is likely to be a quark as an anti-quark. So can things still work?



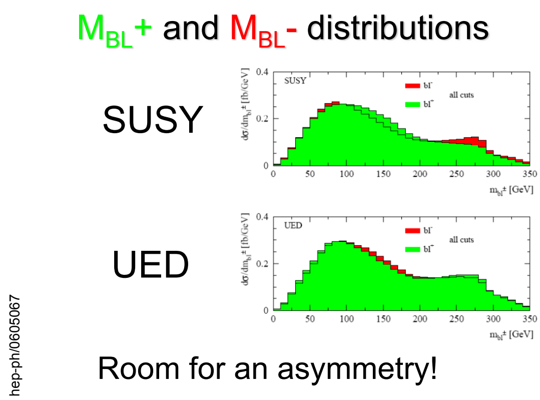
And Tilman's idea was that yes the problem... The reason we had to resort to the trick that there are more quarks than anti-quarks, and more squarks and anti-squarks LHC was because we didn't know whether this was a quark or anti-quark, but by the point this paper had been writ-

[105] This is a reference back to the discussion on Cricket in the last lecture.

ten, ATLAS were saying... and calibrations were claiming gradually tiny ability for at least for B quarks, to tell the difference between. At least statistically some small fraction of the time between B quarks and anti-B quarks. So in other words we could make B tags to try and construct these invariant mass distributions to figure out what is going on down there.



Yes our B could have come from higher up in the chain, rather than lower down in the chain. But that is sort analogous to the wash out process that one has for leptons in the far chain. You can cope with it, it is just a confounding annoying thing but you can cope with it.



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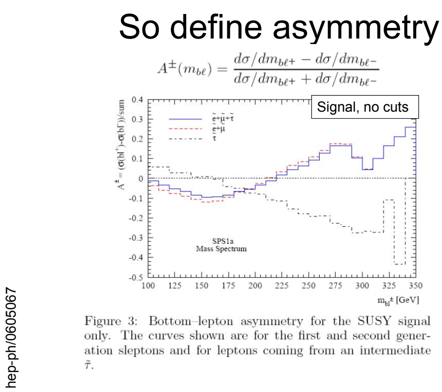
So the up shot of it is that the invariant mass distribution of the B lepton + or -... lepton minus is in red, lepton plus is in green - or the other way round, it doesn't show up very well here. Is a distribution where the red peaks are found a bit higher in these places if you have got SUSY like events with peaks higher in the middle, when it is UED event. So this is a much reduced thing because you are having to fight with the B tagging efficiencies and things like this. But you have got room for an asymmetry, so you could plot red over green... or red over green in either of these two cases and get some curve that is not flat,



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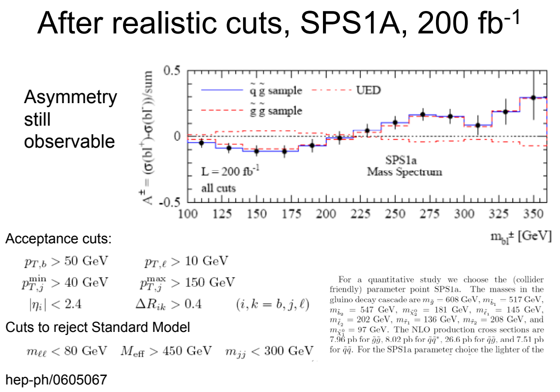
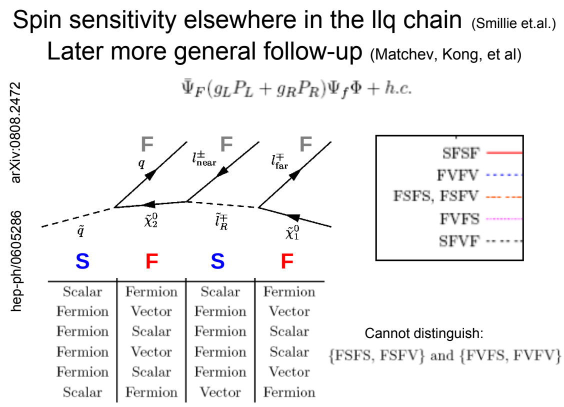
which is an indication that you haven't got just scalars everywhere. And even after realistic cuts

[106] Figure from [50]

[107] Figure from [50]



[108]



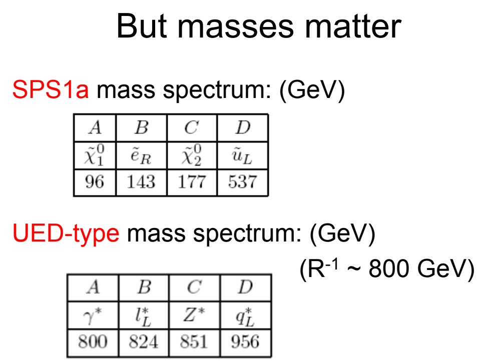
you can retain that ratio that deviates from just plain flatness. So there's sensitivity even there.

Back to long chains

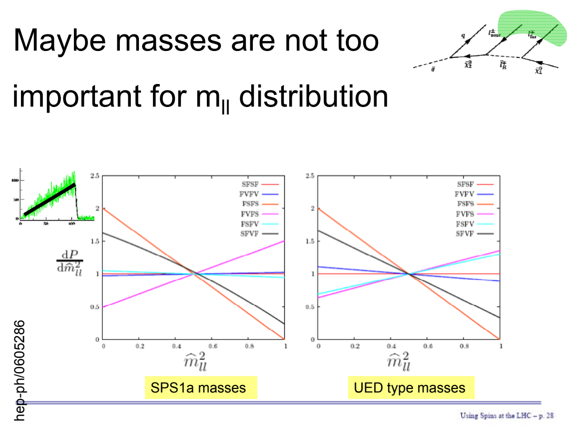
Back to long chains. So the last thing I want to say on long chains... Alan in his paper was looking at the spin in here, just really was saying: "Well what happens if we... Can we discriminate SUSY from Face Space or UED?" But who is to say that we should be just looking at SUSY and UED. So in the SUSY situation we have got scale of fermion, scale of fermion. In UED you have got fermion vecta, fermion vecta. But you could have... There are lots of other combinations that you could have, and so they are labelled here FVSS, FES, V and so on.

So people spent a little bit of time trying to figure out whether any of these were separatable from any of the others [45, 46]. Okay we can or can't separate UED from SUSY, but maybe it is easier to separate them from these guys down here. There was some 1996 work and some 2008 work on this. There was a long gap in between. No... 2006 and 2008. The difference between them is that these ones here tended to assume mis-use of these things were, as they are in SUSY, whereas in the later works people were like, hang on you should really be a bit more general and assume you could have sometimes left-handed, sometimes right-handed couplings. From the later work some certain results - I think I am correct in saying... It is a long time since I looked back at these papers - came to some generalised conclusions. There's people in the audience, I think you are on the paper, who could correct me if I am wrong here. I think the overall conclusion was you could never distinguish some spin combinations. You can't distinguish that from that or this from this. Correct me if I am wrong.

[108] Figure from [50]



But the thing that I want to focus on, just this business about masses, and why masses matter. SUSY tends to have masses that are sort of... People, when they construct SUSY models they tend to spread the masses out in particular ways, because that is what certain high scale theories tell them. Whereas in other models like UED type things the masses might all be quite high and close together. And the question is: Does the masses of our particles affect our ability to determine spins?



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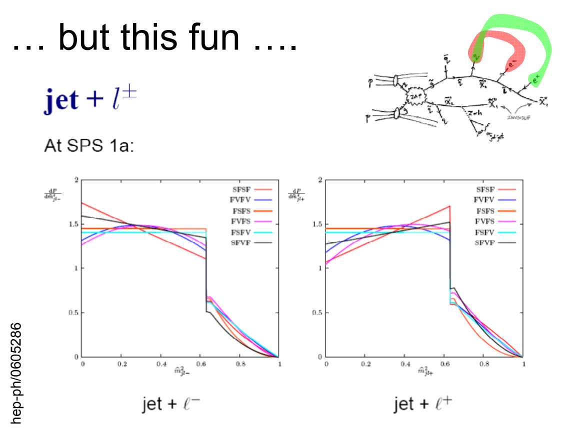
So what you have here is in mass-squared space. So let's see... The invariant mass, lepton system - as I have said for about the fiftieth time it is triangle, when you put it at MLL. But when you put it in MLL space it is a heavy side function, it is a flat top hat. That is what you can see here in the little red line running across here. Top hat going up to endpoint. Top hat there in red, the SUSY MLL distribution. That is supposed to be histogram effectively, it is not a graph, it is literally a histogram. UED is in blue, and it is running along there as well, at least if you have similar masses. Now these other weird exotic combinations, FESS, FE... things that are not SUSY or UED, some of them have quite different slopes. So it might be relatively easy to discriminate SUSY or UED from some of the other things that are doing this. Let's compare this to when we are trying to do the same spin discrimination, but with UED type masses. Masses that are all bunched up together, up at the top. Now there is ever so slight deviation between SUSY and UED, but you would never see it really, well you would have to have a lot of data. And other things are still very separated. So what we are learning here is that masses don't matter to us too much and we can determine, separate SUSY and UED from other things, but maybe not from each other yet.

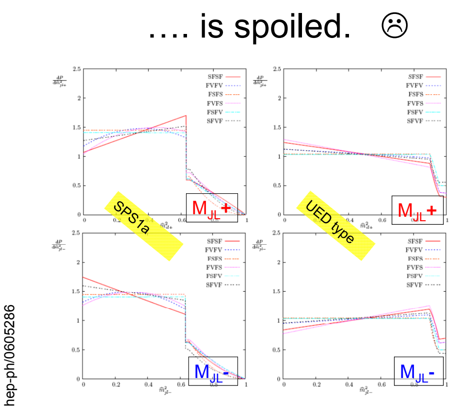


109 Figure from [45]

110

However with the jet lepton distributions where you would think that we might be all right, because when we put the jet lepton minus, the jet lepton plus distribtuions - these are the distributions that Alan was plotting to get a symmetry to measure the true sensitivity of the slepton spin - those distributions... Well they are all rather similar to each other, but there is variability and in the jet lepton plus they are all different to those. So if you were to divide this red line by that red line you would get asymmetry in each case. But alas, when you... What do those asymmetry plots look like?

So this asymmetry would be this over this. What does it look like if I switch from SUSY like masses to UED like masses? The answer is, that things can completely switch around.



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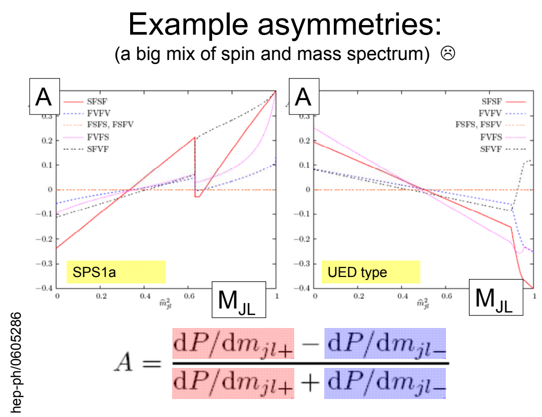
Whereas for the SUSY case here, you get a rising distribution here on your numerator and a falling on your denominator and so when you take a ratio it is nicely rising. If you change the spectrum and have a lot of bunched up heavy particles, now it is a descending distribution, and a rising distribution and so... Whereas you might have thought that you could divide this red by this red to say that you are SUSY and it looks like a different ratio to say one of the dotted things. In practice all you can see if you get an asymmetry is that you have got some spin going on there.

To actually know what spins you have got would require you to have found the masses, to know whether you are in this case or in this case. You can only find out what spins you have got when you know what masses you have got.



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So here is the asymmetry. Here actually is the ratio of numerator over denominator. You see SUSY has done that boom boom boom, lovely deviation, if you had a mass spectrum like in SUSY, but booom booom , going the other way down if you have got a different kind of mass spectrum.

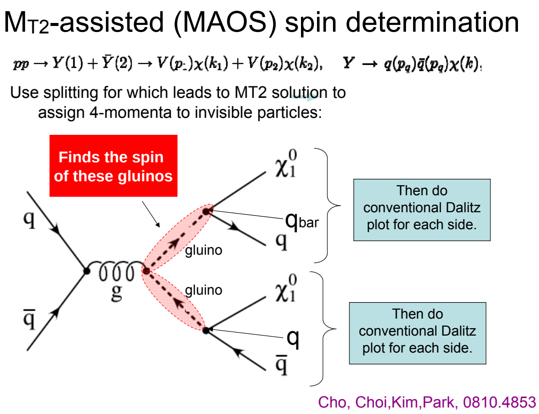
110 Figure from [45]

111 Figure from [45]

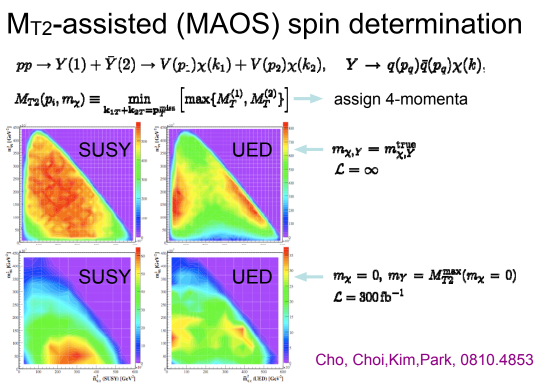
112 Figure from [45]

Yet another game one can play

So one last game you can play which goes back to the idea of Dalitz plots. Most of the spins in the good old days of meson physics were done by plotting a nice honest to goodness Dalitz plot. You have three part... indicate three objects and you plot invariant mass combinations of pairs of them against each other. Most of the time you can't do that in this New Physics business, because you have got an invisible particle on the end and you don't know where it is going, so you can't make enough invariant mass combinations. But what if you could make a sensible guess as to where that invisible particle might be, it doesn't have to be right all the time, as long as it is right some of the time.



Some people whose names can be found down here, they had this idea "Why not use the MT2 splitting that achieved the MT2 bound?" [49]. When we were calculating MT2 we said, there's two invisible particles and we don't know where they are going, but we know they must add up to the PT Miss. And we had to try all possible splitting of these things, until we found the one that minimised the maximum of two transverse masses, and that... I am going to call that the MT2 splitting. They said: Okay why don't we... If we have got an event like this glu glu to 3 bodies, and one is invisible, why not calculate MT2 for this event, but they are not really interested in the MT2 value, but instead pick out the splitting that it gave to these two neutralinos. It only gives you a transverse splitting so they had to invent a longitudinal momentum for them, I think, they just the longitundinal momentum zero, or something like that. That means you have now invented momenta for these things and you can make a Dalitz plot for them. You could make a Dalitz plot for that side and a Dalitz plot for that side and put them both on the same Dalitz plot, and in doing so attempt to find a spin of the parent.



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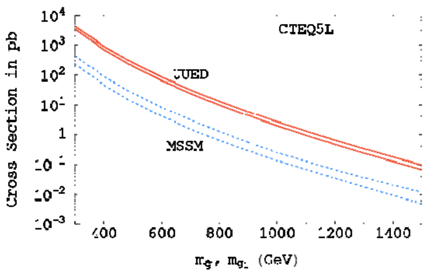
[113] Figure from [49]

What do they get so er... What is the difference between these things. So basically here you see a Dalitz plot like thing and I presume SUSY is supposed to peak somewhere in the middle, and SUSY is supposed to have this sort of structure that is pointing at the UED because of the gluino like thing, and UED has a different spin.

Yes there is some sensitivity to the masses of these things. Here they have cheated. The top plot that looks really good but it is a cheat, because they have put in the true values of the invisible masses, which is a thing that you don't a parino. Down in the bottom they have done the more honest thing of saying: Well conservatively we won't just put in 0 as the mass of the guess of the invisible object and we will put in as the mass of the heavier object the mass difference we would get by looking at the endpoint of the MT2 distribution. So much reduced stuff, but still lump in the middle, bits on either side. So it is kind of like a return to the old days for meson physics. Maybe that will work.

Reminder: cross sections reveal spins



⇨ Higher spins mean higher cross sections (for given masses)

Datta, Kane, Toharia hep-ph/0510204

[114]

Lastly. The very last thing I would say, cross sections they can sometimes reveal spins to a... As I say sometimes, in principle should always reveal spins. For fixed masses higher spin stakes have higher cross sections. So if you are willing to make them model dependent assumptions about what is going on - more details in there of course - then you could say why are we bothering with this kinematic stuff, let's just do dynamics. Let's work out the cross-section and it will tell us the spin.

End Notes

- QLL chain
  - Some spin "sensitivity" – but no strong UED/SUSY separation
  - Reduced discriminatory power when considering general couplings (Matchev/Kong).
- Di-slepton production
  - Better chance of separating UED/SUSY
  - Still model dependent
- Both require large cross sections
- Masses inextricably intertwined.

So that's it really. That is the end of my spin summary. The basic overall message is that we do have sensitivity to spin, but not really very strong UED SUSY separation. A little bit there, but it is quite tricky at least, unless you are prepared to imagine that you know that you have got a left squark and a right slepton or something like this where upon you are no longer sensitive to these mass specs. It is only when you blind yourself to the knowledge of which type of objects you have got in there, that you start finding new... you can find out less than you otherwise could. The dislepton production process is like the ILC, and is completely separate from long chain spin-reconstruction methods. Alan

[114] Figure from [54]

regrets, to this day, that he gave the two independent spin-analysis papers that he wrote on those two topics titles which differed by only one word. As a consequence, no one read the second paper. They thought the second paper was the same as the first, just re-listed. But the overall message that both techniques require very large cross sections and the masses are inextricably intertwined.

[115]

Sorry I have gone two minutes over, but that is not bad for about 290 slides in three days.

[Applause]

[115] This is a picture of The Deer Park, Peterhouse, in bloom with daffodils.

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