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Plenaries 10am to 11pm

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>> Hi, Julie, can you hear us on Zoom?

>> Hello, yes, I can.

>> So you'll be coordinating the Zoom for this morning, for

the next hour at least?

>> Yep.

>> And here in the room will be Laura and Jonathan and we

will start when, when people trickle in, we'll let you know.

>> And we're set with Lora from White Coat. We are ready to

go when you're ready.

>> Good. You, Alessandro and Julie are cohost.

>> Yes, I am.

>> So we'll let -- I guess nobody else is trickling in.

>> My audio is working? You can hear me out in Zoom land?

>> Yes, we can.

>> Should I have a mask on?

>> No. Sorry, you can take it off.

>> In the room just make sure you use the microphone when

you speak.

>> That microphone, okay. You have --

>> I have a microphone. For any questions that come from

the room, it should come from that. I don't think it's on at the

moment but the button is on the top. I don't think we can turn

it off now.

>> I'll turn it off after we get going.

>> All right. Now it's 10. Why don't we go ahead and

start. We have three talks in this first hour and Jesse will

kick us off. Please.

>> Thank you very much. It's a pleasure to be here with the

energy frontier folks. I'm involved in the theory frontier in

Snowmass and we had a fantastic meeting talking about the

frontiers of theory. One of the concepts is machine learning in

the theoretical context and I want to talk about it in its

relevance for collider context.

I'm new to the machine learning world, but I've been forced

into it for the NSF Institute for Artificial Intelligence.

One of the things that convinced me that machine learning

can be used for experimental collider physics is the realization

we can connect physics intelligence into artificial intelligence.

Some of the techniques that we already done, like uncertainty

quantification or employing symmetries to understand our data,

can be enforced in a machine learning context. And that machine

learning suspect quite the black box that some of us are squared

of.

For relevance for the energy frontier, we have an

opportunity in our community to rethink every element of the

standard formula that we use from the theory side to compute

observables and on the experimental side, if you put in response

functions, how you reconstruct collision de-bray.

We have an opportunity to think of what are we measuring in

terms of cross section, phase space integration, what observables

do we decide to measure.

We have an opportunity to leverage completely new frameworks

to enhance our thinking beyond standard model physics.

I'll be talking about machine learning and optimal

transport. And in Ian's talk next, he will talk about conformal

physics and Brian is going to tell you about broader theoretical

frameworks and naturalness and unnaturalness.

My perspective which has evolved over time but what I have

come to think as part of the Snowmass process, is that collider

physics, theory and experiment have been irreversibly impacted by

the rise of deep learning. This is a technology that we need to

leverage and not run away from it.

The buzz is around AI and I can talk more about this. But I

feel like we should leverage analysis strategies from various

areas of math and statistics and computer science. As director

of the AI institute, a lot of my colleagues are upset when I say

they work on AI. They say I work on computational geometry.

What does that have to do with AI.

And then kind of as an invitation to our field, we have an

opportunity to translate aspects of collider physic into a

computational language. We can go furtherer and think of the way

that we analyze data and big data and expand the spheres.

In the spirit of Snowmass I'm looking to your ideas and

perspective so my perspective keeps evolving.

As I said, collider physics is irreversibly impacted by deep

learning.

And coming from my theory frontier perspective, TF07 is the

collider phenomenology and the deep learning montage. In 15

minutes I can't tell you everything. Let me tell you quickly

about some things that are going on. And these citations are not

exhaustive by hopefully representative of the things going on in

our area.

As I said, we have an opportunity to rethink every element

of what we do in collider phenomenology. One of the flag ship

things that is done with deep learning is classifying jets,

telling whether it's a Quark or gluon. For many years part-time

distribution functions are parameterized using neural networks

and machine-based strategies to figure out best fits.

Parameter inference is something where machine learning lets

us do simulation-based inference which is cheaper that straight

likelihood based inference techniques and this is a fit in the

context of the standard model effective theory.

And parton shower modeling and tuning. Parton showers are

how we generate collisions but there is tuning to do and machine

learning is offering us opportunity to have flexible modeling in

the parton shower context.

Phase space integration. Beyond standard sampling and using

machine learning based approaches including those based on

normalizing flows. And using techniques to approximate

scattering amplitudes which are expensive computationally and you

can use machine learning to approximate those functions and pay

one up-front training cost.

And pilot mitigation is an experimental challenge which has

very interesting machine learning strategies for mitigation. And

deconvolution and unfolding is something I've been interested in.

How do you correct for detector effects in high dimensional and

very dimensional parameter spaces and this has an interesting

interplay between theory and experiment that I can tell you about

if you're interested.

That is my TF07 deep learning montage. When we're talking

about opportunities for machine learning and collider physics,

okay, what is the machine learning. There are tasks that

machines are doing better than tradition mall methods. What is

it leveraging. What additional information is it leveraging.

This is something around 2016, 2017, I personally was

feeling threatened by machine learning because the techniques

that I was developing were being overtaken by neural networks

and what was this machine learning. What is the machine learning

that we're learning is anthropomorphizing an algorithm. In some

sense it's the wrong type of question. And one of the ways that

my brain shifted in thinking about machine learning was to

realize that there is a space of different analysis strategies

and machine learning is a space where the machine is learning or

finding a solution or finding an approximate solution to some

well specified optimization problem.

We can talk about the machine as a black box but the black

box is doing something in the same way that a numerical integrate

phase space is doing something. We as physicists get to leverage

the algorithms in the way that we think is most appropriate for

the problem we're trying to solve.

Machine learning is algorithms based on learning solutions

and we should change learning to finding or approximating

solutions through the use of data. These are algorithms that

respond to data in interesting ways, of which linear regression

is an example.

Deep learning is algorithms based on learning parameters of

multilayer neural networks.

But sometimes we kind of, in fact, depending on who you talk

to, if we combine machine learning under the broader umbrella of

artificial intelligence and in my mind those are different

things. Artificial intelligence are algorithms to perform tasks

that are typically associated with intelligent beings. There is

the appearance of higher level logic.

There are cases when linear regression is a perfectly fine

solution and I'm not sure I want to think of linear regression as

a type of AI in which it's an algorithm that I want to know what

the machine is doing.

I think there is value, a lot of value in having higher

order logic but we need to distinguish algorithms that are doing

a good job at optimization verses algorithms that want to explain

their reasoning. I think we need to do more in the AI sphere but

will are computational strategies that we want to leverage.

I will call this physics intelligence. Algorithms that

performance tasks typically associated with physics majors and

Ph.Ds. This is implying that physics majors and Ph.Ds are not

intelligent beings, we'll put that aside. But phase space

integration, is that AI? Is detector unfolding a type of AI.

These are techniques that we think are valuable for the problems

that we're trying to fold. There is a broader space of

strategies and we need to pick the strategies best suited for the

problems that we have. Sometimes it's employing a deep network

or something you interpret or a black box you can run and then

you use post processing to guarantee exactness, for example, in

the case of adaptive Monte Carlo where you can use sampling to

guarantee your answer is correct even if the machine doesn't give

you a good approximation.

There is a space and I want to emphasize progress being made

in my montage is not just because of increased computational

power and large datasets. That is one of the things driving

innovation but it's because we understood the structure of the

problem. And when assigning uncertainties, this is something we

have to pay attention to. And many problems in high energy

physics can be phrased as optimization tasks and if you

understand that, you're optimizing with, you can do better than

out of the box machine learning strategies developed for other

fields.

I talked about AI things or deep learning things, but I

think we should be open to leveraging analysis strategies from

other areas of mathematics and statistics and computer science.

I have a lot more I can say but here is one example of something

that is a type of machine learning but it's not deep learning and

it's connected in a really fundamental way to the things that we

do in collider physics.

This is an area that I only learned about because I talk

today my computational geometry experts. The field of optimal

transport. This is a field you use if you have a bunch of

packages to deliver and you have to choose how to route the

packages on trucks and figuring out the optimal way to do that is

optimal transport. What does moving stuff around have to do with

high energy physics.

But what it allows you to do is define a geometry for

colliders and you can start to use a geometric language to talk

about collision. In computer vision there is earth mover

distance. We move energy around. Here I have two jets with

idealized calorimeter deposits. Jet one in red and jet two in

blue.

And the black lines indicate the transportation plan to

distort one energy flow and make it look like another. And this

energy flow will show up in Ian's talk.

This distance you can use to triangulate a space. This is

an analogy that gave me new insights into my field but

facilitated by conversations with other disciplines.

This field has in particular, in my research group, revealed

that six decades of collider physics, concepts like infrared and

colinear safety. Event shapes, jet clustering, substructure,

pileup mitigation. These can be phrased in a different

computational language and you can reap benefits in terms of

understanding what is going on and computational benefits and in

further cases which I'm happy to talk about, experimental

benefits.

This isn't really AI. This is a different type of machine

learning strategy. It's based on the use of data but one of many

different strategies that we should think about leveraging as we

advance the energy frontier.

In my remaining few minutes, just wanted to bring up this

possibility that once you realize we have this computational

resources to deploy, we should think about translating more

aspects of collider physics into a computational language.

The most extreme version of this in a New York Times

article, maybe a compute ore can device a theory of everything.

Can you trans-plait what theoretical physics dream about,

translate it into a computational language? When you first

encounter this question, my first reaction is to kind of vomit a

bit. I loathe this question. In one case, it's talking about

deep learning. One, it's the wrong computational framework but

it's one of many strategies relevant for the physical sciences

and we're way off from ever doing something like this.

On the other hand, I love this question. Because it

reframes what we're doing in the scientific process. Why do we

think that we can't have a computer come up with a theory of

everything. Why can't we come up with a search space of all

possible theories and have the computer do the best with that.

And do we have an opportunity to maybe automate.

And the energy frontier, one of the things that I've been

interested in is machine learning beyond the standard model

physics and whether there is a possibility of deploying -- for

deviations from expectations. There is an LHC Olympics

challenge. But what aspects of BSM can be streamlined and

systematized and automated and made statistically robust.

Anomaly detection is not well defined. If you search for

everything, you search for nothing. That is correct.

Machine learning can't do everything because you will

overfit and machine learning generalizes well but we need to

understand what it means to do anomaly detection, cast it in a

statistical language and deploy computational strategies, for

example, to analyze our data. This is an opportunity that we

have in thinking about high luminosity LHC and future colliders.

For standard model physics, I'm wondering if we can tightly

integrate theory and experiment to future proof analyses. I've

been an advocate for public data and seeing what can we learn

with new theoretical tools to go back to old data and analyze it

with new understanding. How do we future proof measurements and

do proper uncertainty quantifications that stand the test of

time.

These are challenging things and they're computational

challenges and something to think about in the Snowmass process

about the legacy of the colliders that we build. The legacy of

the datasets and understanding that our understanding increases

over time and we may have new perspectives to employ on old data

and going back to old data might be beneficial for on going

strategies.

And so with that, let me leave you with my evolving

perspective and take your questions. Thank you.

>> Thank you very much. We have time for questions.

>> Please use the microphone. We cannot hear in Zoom.

>> Now can you hear?

>> Yes.

>> Sorry it was off. We have time for questions. We'll

start with any in the room, I guess.

None immediately. Any online?

>> Michael?

>> Yes, just a quick question. It's a wonderful talk Jesse,

thank you very much. You pointed out that the earth movers

distance and I agree that comes across to a collider

physicist as a brand-new concept. But you pointed out this would

enable an insight into all theoretical questions. I was

wondering if you can give an example or elaborate on that point?

It's a really important one in my mind.

>> Yes. So there's a fascinating debate in the theoretical

world about what is or is not compute able using quantum theory.

This goes back to the 1970s and some of these ideas were

refined and I came out with a paper with others that challenged

that lore. And it was confusing. We were able to do a

computation that the standard lore would say is not doable.

It was confusing why we succeeded. It's from the collider

geometry perspective. Fine man diagrams, when you do the

projection, you get singularities. If you blow yourself up, the

singularities are resolved and you can do computations that might

not otherwise think to do.

When doing high order precision calculations about the best

subtraction seems to use and using this geometry, you can figure

out how to define observables that regulate all singularities.

And this technique gives you guarantees that those calculations

can be done in principle to arbitrary perturbative accuracy.

Those are examples on the theory side where having this

language would enable you to understand things because there was

a space to explore and you can use techniques from topology and

geometry to understand those things?

>> That is great. I'm wondering whether this work is having

impact on the theory frontier.

>> I don't know if I have a slide. But there is a

fascinating paper in HEP TH that talked about context where

optimal transport can be related to renormalization group flow.

This is on my reading list. My sense is that this particular

idea of optimal transport, if you can understand what you're

trying to transport, can be relevant for all types of

calculations.

It's a generic physics way of understanding why this is a

powerful technique but it's not just this type of context, this

transport plan turns out to be more robust in a variety of

circumstances. It's a robust way of doing probability densities?

>> Sounds great. Thank you very much.

>> Thank you, that was a very nice talk.

I just wanted to point out that a lot of the concepts you

explained, a lot of that language exists in the world of advanced

statistics.

And we as a community just don't take the course. Okay. So

this stuff is in a Russian textbooks from the last 100 years.

>> Yep.

>> I don't know how to fix this. This concept came from

1780 I think.

>> Yes. Literally moving dirt around.

I can make a comment about that. So at MIT as part of this

AI institute, we launched an interdisciplinary Ph.D. program.

And my colleagues said it should be in physics and AI. And we

call it in physics and statistics and data science. A lot of

this is just statistics. I was not educated enough in that. How

the physicists have been the users of these techniques for many,

many years and there is a New Castle moment where we've been

using machine learning in various forms and using statistical

techniques but it's not embedded in our community the way it

probably should be?

>> One last question from Michael.

>> So one thing I worry about a lot is connected to Higgs

physics at the LHC. You have processes like Higgs to bbBar that

are deeply buried in hadronic backgrounds. And then you use some

sophisticated neural network classifier to bring out those

events.

And we have to now do uncertainty estimation on those

classifiers. How are we doing on this problem? Do we really

know how to do that?

>> Good. No, I would say.

And I would say that the way that we're doing it right now

is we use machine learning to train a classifier and pretend that

classifiers is a fixed observable and do whatever our calibration

technique is. You can try to use some ttbar or test sample to

see how well you pick up B jets. There is uncertainty from the

training procedure itself. What is really cool is that I think

we all have the sense of all these nuisance parameters that we

should be taking into account but we don't because it's too

computationally expensive. With machine learning and the ability

to interpret late, we can increase the number of nuisance

parameters and turn theoretical uncertainties into nuisance

parameters that you can try to fit to data which would be

amazing. If we have a combination of theory interfaced with

experimental measurements and there is a way of thinking about B

jet classification as fixed, fitting a fragmentation function and

trying to extracts a fragmentation function associated with the

aspects of the B fragmentation that we haven't done the theory

calculations of.

I think there is opportunities but right now, we're not

doing it and I blame myself as well for this, not really doing

the full uncertainty quantification that we should?

>> Thank you so much. We're out of time in the Q&A. If you

want to start running behind, that is up to you.

>> There are a couple more hands on Zoom.

>> There are two hands in Zoom.

>> Okay. So let's, okay, I'll ask the question. Maybe it's

a very naive question, is there work done which incorporates

anomaly detection and uncertainty quantification? Or is there

something.

When you predict something, you need to know the uncertainty

and there are traditional ways of doing that and we use that for

current anomaly detection techniques but with the AI

quantification, if you merge those, we could have more robust

anomaly detection possibly or not?

>> It's a comment, some people work more with anomaly

detection, Phil might have more to say about that.

>> We can talk about this.

>> I'm stuck on trying to define for myself as a theorist

what is anomaly detection. And I get myself confused. But one

wants to do uncertainty quantifications.

>> So there is uncertainty and uncertainty, you can embed

uncertainty in anomaly detection. The way people are going about

it these days from a very high level is make ensemble of anomaly

detection. You don't just make one, you make a spread of them.

Jesse talks about embedding uncertainties into your

training. You can build this concept and try to embed the

uncertainty in the way you program anomalies. It extrapolates it

but I say all of this stuff is very, very experimental at this

point. People are throwing ideas and I don't think there is a

real solution?

>> Yet.

>> Okay. So I think we are getting quite behind. Why don't

we close out.

Let's thank Jesse once more.

I'm sorry for the questions that are online. Maybe we can

take those through chat or something?

>> I can ask people that have their hands raised online, put

your question in the chat so maybe it can be answered in the

chat.

>> I tried. Chat is disabled.

>> Maybe we can try to enable it.

>> Okay. I hope you can do that. Let's go onto the next

talk. Ian.

>> Try now on Zoom to chat. Sorry to interrupt. Try now to

chat on Zoom.

>> Ian, go ahead. We'll give you a two-minute warning.

>> Perfect. Thanks a lot for the opportunity to speak. I'm

very sorry I couldn't actually come in person. Today I wanted to

tell you about some progress from the theory frontier and in

particular from some ideas from more formal theory, in particular

conformal field theory and how these have an impact on how to

analyze real world collider data such as at the LHC.

I'm pleased to give this talk after Jesse's talk because I

think the under lying philosophy of these talks is similar. By

trueing to formulate your problem in different languages, you

often get either new insights or completely new ways of thinking

about the same kind of physics that you've been looking at for

quite a number of years.

And so Jesse did this under the lens of the machine learning

or data science side of things and here I'm going to try to look

at colliders and jets in particular from the more formal field

theory side and hope to convince you this is equally exciting and

will play an important role going forward.

This is not a review, it's a particular perspective. So if

there are citations that I neglected, I hope I don't offend

anyone.

Just to start with this nice and familiar picture. As you

know, at Hadron Colliders you get out these remarkable sprays of

energy and the classical way of analyzing them, going back to

this picture from 50 years ago is look at jets as cones of energy

coming out of your detector. So these are some blob of energy

with a particular direction.

And so these are now not quite well understood and these

probe an aspect of under lying collision and can be thought of as

a proxy for Quarks and gluons.

There's been a push to move from this classic picture to a

more refined picture where one measures statistical properties of

the energy flow at infinity. In this case here, one has this

three-point correlator of infinity.

The reason why one wants to do this was originally motivated

by searches for new physics. This is under the name of jet

substructure. The idea of jet substructure is using the internal

substructure of energy flow within jets to apply new ways to

study physics at the LHC.

If you have a new particle produced, shown here, if this

decays hadronically, the only way you can ever detect it existed

is through understanding the subtle patterns of energy flow

arriving off infinity.

So the introduction of this idea around 2008 as well as

robust jet algorithms reinvigorated the study of jets in QCD.

And so as many people here probably know, this had a huge

impact on the way that one does searches at the large Hadron

Collider and will play an important roam in future colliders

because it's a generic approach to search for new physics or

known physics such as the Higgs.

This motivated a lot of the early work in machine learning

which was nicely highlighted in Jesse's talk.

So of course, eventually one wants to do more than just

searches. So the LHC and future colliders in the energy frontier

provide a huge number of very interesting opportunities to study

both QRD et cetera and the standard model at very high energies.

At the LHC you get beautiful QCD jets at the TeV scale.

This allows us to study very interesting quantum field theories

in completely new regimes and allows us to perform measurements,

for example, standard model parameters like the top Quark mass in

completely new regimes like top Quarks produced inside jets.

One wants to fully take advantage of these remarkable

colliders and we need to transition from searches to precision

jet substructure program where one can compute these like one

does for the standard Higgs cross sections.

And so to be able to do this, this requires the development

of a whole set of new theoretical tools and qualitatively new

ways of thinking about jets. So this is because one is asking

much more complicated questions. For example, statistical

properties of this energy flow in this extremely complicated

context of the LHC.

You want to give new ways of doing things compared to how

jets were analyzed in the past.

And so remarkably, one of the main ideas for how to actually

think about jet substructure in a proper formal field theoretic

context came from developments in conformal field theory and

string theory.

The reason it came out of these more formal areas is if one

is interested in how to ask questions about energy flow and jet

substructure in string theory or strongly coupled field theories,

one has to really kind of rethink and ask -- you need to

precisely formulate your question and understand what you're

actually doing from a quantum field theory perspective what was

shown in these papers is in a very formal level, jet substructure

is the study of correlation functions of a particular set of

energy flow operators.

These are the correlation functions shown here. And energy

flow operators should be thought of as quantum field theory

definitions of calorimeter cells. What they are is quite

intuitive, if you think of them as a calorimeter cell. You take

the stressed energy tensor shown here, you dot it into some

particular direction. Integrate over all time because the

detector is always on and move it off to infinity. These are

points on the celestial sphere or literally calorimeter cells.

What are you actually doing.

This is where this relates back to Jesses talk, if you think

about unfolding the detector, then it lives on this 2D plane, and

so when you, if you just think about you're detector, it's

detecting a bunch of particles and from this you can define an

energy density fields shown here.

So the key insight of these papers was that one should

consider statistical properties of this energy density field much

like one does for example for 2D statistical systems like the

icing model.

So the real meat behind this is this is more than an

analogy, you have a precise theoretic field interpretation and

you have a product expansion shown here, you take two operators

and understand how they behave when brought together.

This predicts a scaling law and it allows one to understand

these energy distributions in real world jets at the LHC in terms

of symmetry and operator product expansion structure exactly as

one analyzes conformal field theory.

This is a completely fresh perspective on how one should

think about energy flow in collider experiments.

While this may seem beautiful, there is unfortunately a

large gap between formal theory and experiment. There is this

nice exchange shown here where Polchinski is asking, the scaling

should be seen in a lot of Q CD data and the response was people

do not do this. I haven't figured out why they don't. I think

they haven't thought about this.

As a result, even though the LHC has been running for

approximately 10 years now, none of these correlators has been

directly measured. And so there's a divide between the beautiful

theory ideas on the theory frontier and the wealth of collider

data on the energy frontier side that we want to bridge.

So what I want to talk about in the rest of the talk is how

this development of these new ideas from the theory allow one to

start talking about collider physics shown here in terms of

scaling and shapes of correlation functions exactly how one does

kind of statistical physics and let's say phase transitions.

One can really go into the data of the LHC and observe

beautiful scaling behavior and this allows one to formulate jet

substructure in terms of these beautiful correlation functions

and opens the door to the use of a variety of techniques from the

more formal theory side and a nice interplay of these techniques

and real world QCD at Hadron Colliders.

In the rest of the talk, I want to step through these basic

things and show how they have an impact on the analysis of QCD

and Hadron Colliders.

Okay. So the first thing is just is this scaling behavior.

As I mentioned, the most basic pre-duction of this operator

product expansion is as you take two calorimeter cells in the

detector and bring them together, the OPE predicts you should

have a scaling law exactly like in phase transition.

And so this is just a generic prediction in any quantum

field theory but it's never been verified. So one can go to the

LHC and see that as you bring these two detectors together which

are shown here, this is the angle between the two detectors here.

And this is a plot using open data from the CMS collaboration,

compared within a theoretical calculation. You see this you get

this beautiful agreement. This is an illustration of the

universality of the operator product expansion limit in quantum

field theory.

This is really in the full LHC environment with all these

complications but if you ask the correct question and look at the

small angle limit, the quantum field theory predicts these

beautiful uniform behaviors that probe the under lying structure

of the dimensions in QCD.

This is what was originally imagined. The LHC dataset is

remarkable and allows us to go very much further. So in

particular one can measure higher point correlators so you don't

have to stop with the two point. You can measure higher and

higher point correlators and how these change as a function of

size.

These probe the spectrum of anomalous dimensions in your

theory which is known as this Regge trajectory. Just to

illustrate doing this in reality, this is a plot from open data

showing the scaling behavior of the correlators as a function of

size. You can go all the way up to ten-point correlators and you

get this beautiful scaling behavior shown here which is really a

kind of study of the quantum anomalous dimensions-over these

light ray operators at the quantum level and the verification of

the fundamentally predictions of quantum field theory.

You will notice that you can see the convexity of the

trajectories directly from the measurements of the scaling of

each probe of different dimensions in your theory.

These can be predicted quantitatively in your under lying

theory and these are examples of the first six scaling behaviors.

The first six correlation functions. These provide quantitative

tests of the anomalous dimensions in QCD and the strong coupling

constant. These provide fundamentally new probes of the

structure of jets at colliders and want to study them in a way

that is tightly connected to the underlying structure of the

field theory and particular to more formal concepts that are

interesting to a more formal field theory community.

And so, I think this is really now kind of a -- first of

all, we can answer this question strongly in the affirmative, you

do see all these beautiful scalings which were predicted in

formal field theory. This is a beautiful example of abstract

theory ideas. This is the original picture of the two-point

correlator in the string theory context and this progressed to a

comparison between calculations and data showing this under lying

beautiful feature of the field theory and showing the wants and

needs of the drivers to give new ways of thinking about jets

which have been around for a very long time. There is still very

interesting new physics to look for in QCD and new ways of

looking at colliders.

So this scaling behavior is nice but of course, what we want

to do is to study the shape dependence of higher point

correlators and really understand how QCD radiation is

distributed on this detector or celestial sphere.

So beyond these kind of scaling behaviors, the next thing

one wants to do is probe the structure of higher point

correlators. And so one way of thinking about this is that it

probes non-Gaussianities in the energy flux. This is an analogy

where studying the cosmic microwave background, you study the

two-point correlation functions but what we want to do is study

the higher points. These three-point correlation functions here.

After scaling, the three-point correlation function, you put

one point at zero and one point at one, it's a complex of

variable Z.

One can go out and directly measure the multi-point

correlation functions at the LHC. This is a plot from open data

and a comparison to a calculation shown here. These are

measurements of true correlation functions living on the

celestial sphere. One can use all of the techniques of standard

conformal field theory.

This is a function of also the overall scale and so as you

zoom in or out, eventually you hit the confinement transition and

this washes out all the structure that is in this three-points

correlator formed by the Quarks and gluons. You can see the

confinement transition happened in this nice You Tube video shown

here.

Here I focused so far primarily on measurements in QRQCD.

This allows one to study other features of the standard model

that are imprinted inside jets. For example, if you produce a

top Quark shown here, this will imprint itself by breaking the

scaling symmetry of QCD into a characteristic angular scale

within these distributions.

This is a plot of the three-point correlation function as a

function of size. Instead of seeing this scaling behavior that I

showed before, you see a bump at the particular value of the top

mass that you put in. This can hopefully allow for measurements

of the top mass at the LHC within highly boosted jets. So again,

this kind of interplay between the standard model measurements

and these beautiful correlation functions further emphasizes a

desire to better understand their mathematical structure on the

theory frontier side.

And so, just again highlight how this is having a kind of

cross disciplinary interest on the theory frontier. There is a

nice recent white paper on the analytic conformal bootstrap that

highlighted the interest in understanding these structure of this

field theory living on the 2D celestial sphere. And showing the

light ray operators exhibit, probably by now this famous equation

as the one to think about jets also in this language of crossing

symmetry. And this has a whole bunch of applications to the

study of jet substructure in QCD and there is really this overlap

of interests in this quest to understand non-perturbative

Lorentzian dynamics of quantum field theories which talks about

how we perform calculations in real world collider physics?

>> Two minutes.

>> Sorry.

>> Two minutes.

>> Sure. Perfect. I'm just about done.

Just to kind of conclude or show one final thing, one thing

where this overlaps very nicely with what Jesse was discussing is

you will have recognized this 2D plot where he was talking about

optimal transported. These observables that turn out to be nice

from the theory perspective are studying the statistical

properties in the same, essentially space. Singles these

observables probe jets in fundamentally new ways, these require

new data analysis techniques to properly unfold them.

So there's been a lot of recent developments, a number of

which were highlighted in Jesse's talk. Certain unfolding

measurements like omni fold based on machine learning that

directly work in this space and are suited to understanding these

much more complicated observables.

I think this synergy of the new data techniques provide

exciting new opportunities to probe the standard model at

colliders in completely new ways.

And so just to summarize, I think these insights both from

the pure formal theory side, in particular from quite unexpected

areas combined with data science are really transforming the way

we think about collider physics and so many people think of jet

physics as an old and well-established field. But really in the

last couple of years it's seen a revolution and completely new

ways of thinking about it.

Here I highlighted one, how this jet substructure provides a

physical realization of this operator product expansion of light

ray operators. And this builds a direct bridge between a huge

amount of interest in the pure field theory community and real

world QCD phenomenology where you can go out and directly measure

all these properties. So it allows us to have a whole host of

new techniques for performing calculations for QCD and the

standard model at collider experiments.

And so I think these advanced combined with the machine

learning and data science side open the door to a precision

physics program using jet substructure at the LHC or any future

colliders.

Thank you for your attention.

>> Thank you, Ian.

I guess the same thing, questions from the room first.

Okay. Question online then?

>> Michael.

>> Yes, thanks for a very interesting talk, Ian. I have a

question about that proposal to measure the top mass from the

correlations. I'm not familiar with the paper unfortunately but

I noticed it's E plus E minus study. My question is whether

anyone who tried to adapt that for the LHC, since we have boosted

top and LHC already, it's not something we have to wait for. Of

course, you're doing only a transverse momentum picture and you

have backgrounds and resolution to worry about. I'm wondering if

these beautiful peaks here would survive reality.

>> So I should say, this is a paper I can put the archive

number here. It's a recent paper. In it we consider the case of

the LHC. Just the particular plotting side for this E plus E

minus plot is the one that I chose for this talk.

The paper is 2201.08393.

There we, the main focus is on doing it at the LHC inside

boosted jets. And this peak, it all works perfectly there. That

was our main excitement. And my apologies, I took quickly this E

plus E minus case when making the talk. But the plot looks

essentially the same in the case of LHC?

>> That's great. No need to apologize. Does this

definition of M pop, is it a way to get around the usual

theoretical problems of what is M top?

>> That was the main motivation for trying to do this. One

of the things about these correlators which hopefully I tried to

emphasize a bit, in some sense they're the simplest field theory

observables that one can essentially compute for energy flow.

This is why they were of interest to the more formal

community. If you want to get around these arguments about the

precise definition of the top Quark mass, you want to formulate

things in the cleanest field theoretic context. In my belief,

this 3 point correlator is the simplest possible observable with

sensitivity to the top mass.

And so because you have this very clean field theoretic

definition of it, you know exactly, precisely what is meant by

the top Quark mass in these calculations. So our hope, although

it requires kind of experimental studies and stuff, but from the

theory perspective one knows exactly what is going in and if one

can measure it nicely -- because it's such a simple observable

theoretically -- you understand exactly what is going on and

there are none of these arguments. That was our driving

motivation for trying to formulate the problem in this very

simple theoretic way.

>> Okay. Well, this is very interesting, thanks a lot.

>> We have a question from the room. From Nora.

>> Hi, thank you for the nice talk. I'm connecting to what

we just, what you were just discussing. So you gave us the

example of the top Quark mass measurement. In your view, what

are the top jet observables that the current procedure and new

physics searches at the LHC and in the future the high luminosity

LHC would most benefit from these new approaches and at what

level?

>> So I think one of the motivations more broadly for this

work is to try to understand, jet observables have been around

for quite a long time. There is essentially, if you want -- now

it's kind of understood. If you have some particular goal, there

was a way before of designing some observable to probe that. So

the goal of this work was to try to reformulate that question in

terms of observables that are simpler to understand

theoretically.

The hope is if one has, say, some particular physics goal,

then one can translate it into a precise question about these

correlators which can then be addressed using more sophisticated

field theory techniques.

So at this stage we're primarily focusing on if one is given

a correlator, how to understand it from the field theory side.

We're starting to apply these on how to map physical questions to

precise questions in terms of the correlator approximates.

I mentioned here the top mass. Another example are

measurements of, for example, of the strong coupling constant

shown here from the precise slope of these lines. One can also

rephrase a lot of questions of fragmentation or B physics in

terms of this language. And so more generally it also opens up

the door to asking much more sophisticated questions. For

example the structure of angular correlations in jets is

something that hasn't been addressed very much. There is

interplay between calculations of essentially spins of gluons

inside jets and trying to implement this also in Monte Carlo

programs where you can then compare with the analytic

calculations and then use them more broadly for searches.

And so I think the way one should view this is really trying

to reduce it to the simplest possible, reduce physics questions

at the LHC to the simplest possible field theory context where

you can then apply these powerful tools. Our hope is any physics

questions that one wants, should be reducible to a simple

question that can be posed in terms of these correlators?

>> We're out of time and there are no more questions from

the rooming.

>> One more on Zoom from Mike.

>> Yes. I missed the beginning of your talk. I'm sorry.

I'm wondering if this is applicable to heavy ion collisions.

Particles going all directions and you make the correlators and

consider the analogy of every event is equivalent to a big bang

microwave background and analyze it the same way. Have you

considered heavy ion collisions.

>> Yes. We are currently, I have someone who is trying

that. I think it's extremely interesting. The points of these

correlators and this is related to a question in the chat, as you

or the kind of general point of correlators is to identify

particularly interesting physical scales that appear in the

problem. So here I focus on the regime where QCD is essentially

conformal. If you extended this into the regime with

Hadronization, what happens is at the scale of lambda QCD, you

see an exact phase change and transition to having a flat

correlation.

So you can kind of see by eye the presence of scales. So in

this heavy ion context, you have a bunch more scales. For

example, the temperature. And so one, the hope is that one can

very cleanly identify physical scales appearing in the problem

but studying these correlators. I think it's a very exciting

area to try them out in and we are definitely doing that.

But not quite at the level that is ready for a talk yet.

But hopefully, in the future. Yes?

>> Thank you.

>> Okay. Let's thank Ian again.

And now we're ready for Brian Batell who is patiently

waiting up here.

>> Thanks, can you hear me? Great. First, let me thank the

theory frontier conveners for inviting me to give this talk.

I have been asked to talk about the status of naturalness or

Higgs naturalness or the hierarchy problems and experimental

signatures.

Let me start by backing up and talking more generally about

why we expect BSM physics, beyond the standard model. There are

a lot of reasons. A number of conceptual hints that are listed

up here.

As well as empirical facts that require new particle physics

beyond the standard model.

A question that always comes up when you try to address one

of these problems is what is the scale that we should look for?

Where is the scale of new physics? With many of these problems,

we can't point to a particular scale.

We could ask why BSM physics at the energy frontier? And I

think the hierarchy problem or the naturalness problem is a clear

and guiding principle in that case.

That tells us the Higgs mass parameter is sensitive to short

distance scales and naively suggests new physics near the weak

scale. This is effective field theory. We don't see other

scalars observed in nature.

And so it's curious that we now have a standard model Higgs.

And there are historical examples that can be used to point

towards naturalness reasoning.

On the other hand, we have a paradox now, there is no new

physics observed at the LHC, at least yet. Maybe this

naturalness is not an issue at all. We're asking a misguided

question. And there's the cosmological constants problem which

is a bigger problem in terms of naturalness.

Nevertheless, you can take naturalness as a hypothesis. We

should go and look.

Now, if I think of how the landscape has changed since the

Higgs discovery and since last Snowmass in 2013. There are

direct LHC constraints on a number of traditional solutions to

the hierarchy problems and these constraints have been

tightening.

Nevertheless, I think the naturalness puzzle remains. It's

still there. Perhaps more paradoxical now and more pressing in

light of LHC Run2 data.

This is inspired renewed efforts from the theory community

to address this problem. There is a number of creative model

building approaches as well as new phenomenological avenues to

attack this issue.

I'm going to give an overview of some of the new ideas.

This is not exhaustive. I apologize if I don't cover your

favorite new idea but this will give you an idea of what some of

the activities have been.

Let's start from some of the traditional solutions,

supersymmetry. Supersymmetry is nice. It buys you a number of

different things: It solves the hierarchy problem and it's a

beautiful extension of space time symmetry. And gauge coupling

is unified in supersymmetric expansions and it might be an

important thing in quantum gravity.

Naturalness and supersymmetry, there is a number of

statements we can make. First of all, SUSY addresses the big

hierarchy problem. Naive naturalness arguments from a bottom up

perspective suggest there should be light Higgsinos and light

stops and large gluinos. In the MSSM there is another twist,

that is getting the Higgs mass at 125 GeV points you towards

heavy stops which is in that -- naively intention with these

naturalness bounds or arguments.

This can be alleviated in ex-pensions of the MSSM when you

have new -- and you can ask how tuned is it. If we find, put a

bound on the gluino for example, this depends on the tuning

measure. Depends on correlations between UV parameters and

depends how precise your calculation is.

We can take stock now of how SUSY is doing after LHC Run2.

We have one for light Higgsinos and one for stops and one for

gluinos and light Higgsinos are still allowed. There is still

wide room for order of 100 GeV Higgsinos.

On the other hand, stops and gluinos are starting to be

pushed towards potentially uncomfortable masses. By no means

does that mean we should give up on SUSY. We should keep pushing

towards higher masses. Could be the gluino is at 2.3 TeV. That

would be great.

On the other hand, you can take this perhaps as a hint that,

well, maybe something different is going on. Perhaps there's a

little hierarchy and the story is not as simple as the MSSM or

the weak scale.

There is a number of directions that we have to keep

pursuing. The MSSM phenomenology is well established. We should

keep pushing. Compressed spectra, multiple branching ratios.

There is the possibility of R parity violation that leads to a

different character of signatures. Missing momentum, resonances.

Displaced signals if they're small.

This doesn't allow you to hide SUSY that well beyond what

you would get in the normal MSSM with R parity.

So stops are still bound to be heavier than 500 GeV and

gluinos heavier than about 1 to 2 TeV.

You can have a mini split where the scalars are heavy and

the guaginos and Higgsinos are light.

This can allow you to address the outstanding questions

like the flavor problem and allows you to get the Higgs mass. So

you live with heavy scale layers in that case.

There have been efforts to model build novel signatures.

This is stealth SUSY. In this case there is a SUSY hidden sector

and essentially suppresses the missing momentum signature.

So that's the state of supersymmetry. Another traditional

approach to the hierarchy problem is making the Higgs composite.

In this picture the Higgs is composite state. There is new

dynamics at a higher scale. There is global symmetry that breaks

down to a subgroup.

And the Higgs is a pseudo-boson. Couplings to the standard

model break this global symmetry and generate a potential. If we

require typically we need to require that the electroweak scale

is a little smaller than the global symmetry breaking scale to

accommodate electric precision tests and Higgs coupling.

This corresponds to the moderate fine tuning and a little

hierarchy problem.

How do we probe a composite Higgs? We should look into its

properties in great detail. This implies modifications to the

Higgs couplings. And typically, these are the ratios of the Higgs

couplings to the standard model value.

These are typically predicted to be one plus a correction of

D squared over F squared.

With the current coupling precision, this is of order ten

percent. Implies the scale F is larger than 600 GeV. If you

study it carefully this is what you find. This is a nice study

you can have a look at.

In these models there are other predictions that come along.

You expect some light fermionic top partners. Additional

pseudo-bosons can be present depending on what the nature of the

coset is. These are things to keep an eye on.

Another view on the composite picture is to think of a

holographic interpretation in terms of a warped extra dimension.

You have an extra dimension or a slice of space, and the physical

states are warped along the fifth direction. It's

holographically dualled to strong dynamics. When you look at

these models, there were strong indirect bounds coming from

flavor and electric precision tests before the LHC that suggest

the states are heavy in the few to 10 TeV range.

You can take this as a sign of a hierarchy problem and just

run with it. So some recent work has been looking into warp

models where you postulate two dynamical scales or two IR branes.

One of the Higgs brane at an intermediate scale and a lower IR

brane scale. Where different standard model fields propagate to

different steps in the extra dimension. This can lead to some

very interesting signatures. Like a KK state decaying to a

standard model and a boson which decays into standard model gauge

bosons. You can have interesting novel cascade decays.

These are the traditional solutions to the hierarchy

problem. Since maybe a reaction to the Higgs discovery as well

as the bounds placed on traditional naturalness approaches, there

have been studies looking in different directions, for example,

you could imagine there are top partners that don't carry SU3

color.

If you have a table of all the different possible top

partners you can imagine, whether they are scalars or fermions or

color or electroweak or are completely neutral. This idea of

neutral naturalness is to say maybe the top partners don't have

SU3 color. In that case the direct searches at the LHC wouldn't

apply. You couldn't produce them with a large rate.

And so then you would have naturalness but somehow it's

hidden at the LHC.

And you can build models of this type. In fact, recently as

part of the Snowmass process along with others produced a review

and sort of a current state of these models.

Okay. So here is the prototypical example, the twin Higgs

model. It's probability the most studied and also the first

example of neutral naturalness. In this model you postulate an

exact copy of the standard model. Related to our standard model

by a Z2 symmetry.

And the Higgs in this case is an approximate, it's a

pseudo-boson of approximate SU4 symmetry, you group together the

two Higgs into a sector. And then you can see that the low

energy effective theory consists of our Higgs interacting with a

neutral colorless top partner. So that could be essentially

protecting the Higgs from a higher physics scales.

And but it's neutral so you can't produce it in abundance at

the LHC.

These models have also some very interesting phenomenology.

So again they are -- boson, reduced couplings to standard model

states. The Higgs portal coupling allows our Higgs to decay into

this mirror sector and depending on the nature of the mirror

sector you can have different possibles. One is to have an

invisible decay, this is motivating searches for Higgs to

invisible.

The fraternal twin removes the first and second generations

in these models. And the idea here is that these states are not

really required for naturalness, only the third generation is

required to protect the Higgs.

And also helps with some of the cosmological issues. And in

that case, once the Higgs decays into this mirror sector, these

states can hadronize and decay back to the standard model and you

can have a variety of interesting exotic Higgs decays in this

case.

Another novel approach is to think about a cosmological

explanation of the Higgs. Of the hierarchy problem. One example

is the cosmological relaxation of the electroweak scale. In this

case, we imagine that the bear Higgs mass, M is higher than the

electroweak scale. This scalar field phi, the axion evolves and

scans the Higgs mass.

So there's a three-stage process where at some point as the

relaxion roles into the potential, the electric symmetry is

broken. And then this triggers a back reaction and eventually

stopping the relaxion in such a way the Higgs mass is

parametrically light compared to the cut off of the theory.

There has been a number of distinct cosmological approaches

that I don't have time to review but you can look into the work

of these authors.

Maybe I'll skip this. This is just to say it's very

interesting to combine the relaxion idea with traditional

approaches like SUSY or composite Higgs. You can explain the

little hierarchy in these scenarios using the relaxion. The

relaxion has an interesting phenomenology. For the energy

frontier I will highlight one possibility, you can produce the

relaxion at accelerator experiments like the LHC. Produce it in

a B meson decay and the relaxion might be long-lived.

This connects nicely with some of the recent

phenomenological experiments with the new detectors at the LHC.

We have the LHC, it's going to run for a long time. Why not

think of other detectors, other experimental approaches that we

might use to leverage all of the intense collisions.

So we'll have a talk later by Jonathan Feng. You can see on

this plot, you can probe, it's hard to see, but you can probe

relaxions at experiments like phaser and other experiments like

Methuselah and so on.

What we do in terms of probing this. We can measure the

Higgs. Higgs coupling deviation shows up and we can improve by a

factor of 2 to 5 with high luminosity LHC. We'll be able to

probe the Higgs sub coupling. That will give us information

about the Higgs potential. There is room for discovery at the

high LHC. We can discover gluinos up to 2.5 TeV.

And looking towards the future, I think it's fair to say the

LHC is not going to definitively settle the naturalness question,

we need higher precision measurements of the Higgs. We need a

Higgs factory. And we need to go to higher energy. Even if we

discover something at the LHC, we'll need a new high energy

collider to probe it and understand it in detail.

Okay. Let me just end with some outlook. Higgings

naturalness hypothesis is being put to the test at the LHC.

There are exciting opportunities that remain. The question will

not be decisively settled for that we need to go to a future

Higgs factory and measure the Higgs precisely as well as the new

high energy collider.

Traditional approaches are increasingly constrained but

still viable and well-motivated and interesting, a number of

signatures to look for.

There are these new novel ideas to approach the hierarchy

problem. Neutral naturalness and cosmological approaches.

There is a nice interplay with other frontiers like the

cosmic frontier as well as other outstanding questions. For

example, dark matter and so on.

And finally precision Higgs measurements, dedicated searches

at the LHC as well as future high energy colliders and

experiments at other frontiers are going to be needed to

thoroughly explore Higgs naturalness. Thank you.

>> Thank you, Brian. Questions?

>> Thanks, very nice overview. In the traditional solution

for looking for top partners, at what point do we get

uncomfortable? In other words 2 TeV, okay. It's all very soft

boundary I guess. But at what point do we abandon the idea these

solutions might be the right ones? Or in other words, are we

motivated to keep pushing this up much higher or --

>> Yes. Just taking supersymmetry as an example. If you

ask some people, they would say, okay, we shouldn't have

discovered stops yet. They should be in the multi-TeV range.

And I think that's a reasonable point of view. You can

approach that from a couple of different reasons. One is that

having scalars or stops being heavy can help can various other

puzzles. For example, you can avoid large flavor changing

neutral currents that you might expect otherwise. As well as

perhaps from just a naturalness perspective, depending how you

view tuning, you can try to quantify it in different ways. From

that perspective you can expect it to be heavy.

We're not even close to the point of giving up. We should

keep pushing as high as we can. Especially if we can go to a

future high energy machine, then I think if we, if we can exclude

stops up to 10 TeV or whatever it might be, right, let's see

right here, for example, at the FCC, we can go up to 10 TeV.

Then, even the strongest advocates at that point may start to

worry. We're not even close, right. We still have a lot of work

to do?

>> Okay. Thanks and the relaxion is necessarily light?

>> Yes. It's a, it's just like a usual axion, the mass is

very light compared to the weak scale. So in this case, this is

even sort of heavy for what you might expect, you might expect it

to be much lighter. And then in that case you look for it in

various ways. Use traditional axion searches. Dark matter, you

can look for it various ways.

>> Okay, thanks.

>> If you just go back to the previous slide on the

Relaxion.

>> This one?

>> Yes. The full citation list. It starts out extremely

light and as you go down the list it gets heavier and heavier

until it's discoverable at the LHC.

>> This is more like a comment addressed to the previous

question. I think we always get the question where should we

give up? Where at stop mass should we give up? I think as Brian

said, it's a soft boundary. It's just a measure of

uncomfortableness and for that you don't have to ask us. We can

compare the numbers yourself and basically the stop mass squared,

Pi square, and the, some people say we should multiply by a

factor of a few. But I don't think that qualitatively changes

the picture. Compared to the Higgs mass. You tell me how much

you're comfortable and uncomfortable.

Each of us can make a decision. Perhaps, it is just a

motivation of thinking about new things. And if that number is

too different, maybe we all can agree that it's really

uncomfortable.

Anyways, I also, as a more general comment, I think the

talk, I think your talk is very clear. It's very clear

discussion. I mean, just more general, I hope this Snowmass, you

know, we really have to somehow decide how we want to talk about

the naturalness problem. Because it's an old problem and somehow

it's getting a bad name, I think, in the recent years. There is

lots of misunderstanding. I think it's, again, this is a general

comment, I think it's important to come up with a good way to

talk about naturalness problem. I think it's a very important

discussion we need to have.

>> Last one from the room.

>> Thanks, Brian for the nice talk. I want to make a quick

comment. I think the real problem here is that, okay, I say it

in a different way, we really want to have a dynamic explanation

for the Higgs potential. Top partners is traditional, like

paradigm where the supersymmetry composite.

I don't think we should stop at the point, I think it's

before we actually find this mechanism to explain the Higgs mass

or Higgs potential.

>> Thank you, are there questions online?

>> No. No questions online.

>> Okay. Brian, did you want to have one last word on these

comments before we end?

>> I'm okay. I wholeheartedly agree it would be important

and good to spend time thinking about how to frame this question.

People look at it from a very, a variety of different points of

view.

And I think, and some people object to the whole endeavor.

But I think it is, does provide a good motivation, as some people

say a strategy for looking at new physics, especially at the weak

scale and at the energy frontier. I think it's definitely worth

considering how to best frame that?

>> Okay. Let's thank Brian again.