ANALYTIC STACKS

Lectures by Dustin Clausen and Peter Scholze

Contents

These are crowd-sourced lecture notes for the Clausen-Scholze course on Analytic Stacks,

`https://www.youtube.com/playlist?list=PLx5f8IelFRgGmu6gmL-Kf_Rl_6Mm7juZO`

The repository for these notes is

`https://github.com/ysulyma/analytic-stacks`

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1. Introduction (Clausen)

https://www.youtube.com/watch?v=YxSZ1mTIpaA&list=PLx5f8IelFRgGmu6gmL-Kf_Rl_6Mm7juZO

1.1. Motivation.

Thank you everyone for your patience and welcome. This is going to be a course about analytic geometry. The title is "Analytic Stacks", and we're going to be trying to explain the foundations for analytic geometry that we've been trying to set up for the past few years.

In this first lecture, I want to give an introduction to the first half of the course (because otherwise, I'd be talking about too many concepts in one single lecture). Let me set the stage by giving some **motivation**.

Classically, there are several different theories of analytic geometry. I'll just list the ones that I may be familiar with.

(1) The gold standard is the usual theory of complex analytic spaces. In the smooth case, these are the complex manifolds. So, these are things you get by gluing together open subsets of $\mathbf{C}^n$ along biholomorphisms (maps that locally admit a power series expansion). There's the nonsmooth case as well, where you also locally allow the zero locus of some finite collection of analytic functions on those open subsets.

(2) There's a generalization of this which was presented in Serre's book on Lie groups and Lie algebras cite me, which is the locally analytic manifolds (or generalization of the smooth case at least). Consider a complete normed field $(K, |\cdot|)$ which can be archimedean or non-archimedean. Then you glue open subsets of some K^d along locally analytic isomorphisms. That means we obtain locally around every point you have a convergent power series expansion with coefficients in the field K.

In (2) when $K = \mathbf{C}$, it does just recover the smooth case of (1), and that's a nice reasonable theory. When $K = \mathbf{R}$, it's a version of the theory of real manifolds, which is also a nice reasonable theory.

But when K is non-archimedean, it's not particularly geometrically rich unless you have some extra structure like a group structure or something like this, which gives it more geometry. The reason for this is as follows

In the non-archimedean case, for example, when $K = \mathbf{Q}_p$, there's the structure is not rich enough because the topology on $\mathbf{Q}_p$ (or $\mathbf{Q}_p^d$) is totally disconnected.

Remark 1.1. Every unit ball will break up into p many other unit balls, and those break up into p many unit balls. For example, in Serre's book cite me, you can find a discussion of the classification of compact p-adic manifolds, and it's just a very simple combinatorial classification. There's the dimension and another invariant. There's just really not much going on there.

(3) This and some examples coming from uniformization of elliptic curves led John Tate cite me to introduce **rigid analytic geometry**, which is over a non-archimedean field. So, it's a geometrically rich theory which works in the non-archimedean case.

In contrast to the theories above, you don't kind of really think of it in terms of specifying some ways of gluing open subsets of K^d, or you don't even necessarily think about it in topological terms at all. You more actually think about it in algebraic terms. Instead of focusing on a local model, which in the classical case might be something like an open polydisk, you instead concentrate on a class of locally allowed functions. So, there's a turn here.

- Focus on local rings of functions instead of local topology and let the ring of functions tell you what the topology is supposed to be.
- The local model that Tate uses are not functions convergent on an open disk, but functions convergent on a closed disk, which is something that makes sense to use in the non-archimedean context. The local rings of functions are well quotients of functions convergent on a closed polydisk, and those are the so-called **Tate algebras**.

The manner in which you're allowed to glue these local models to get global rigid analytic spaces is halfway between algebraic geometry and usual analytic geometry.

(4) This was generalized by Huber to the theory of **adic spaces**, and this is a generalization of rigid analytic geometry. Note that rigid analytic geometry takes place over a base field, and a very loose

analogy would be that adic spaces are to rigid analytic spaces as general schemes are to varieties over a field. So it is useful generalization where you don't have to have a fixed base field.

Varities over a field k	Schemes
Rigid analytic spaces	Adic Spaces

Again, you don't think necessarily of your local models topologically, kind of think of them as being algebraically specified in terms of a ring of functions. But here also there's a new twist.

- You still have local model as some ring of functions called A, but you also include extra data of a certain subring $A^+ \subset A$, and what A^+ does to A is you attach some space of valuations and then A^+ will single out those valuations which view this subring A^+ as consisting of integral elements.

It's actually a very nice extra flexibility you have in Huber's theory that you can consider different choices of A^+ on a given appropriate topological ring A. We'll see from a different perspective what this choice of A^+ is really doing later in this lecture.

Then we proceed to Berkovich's theory. The rings of functions you see both in the context of rigid analytic spaces and in the more general context of adic spaces. The basic examples are things like you have a ring and it's complete with respect to a finitely generated ideal and you give it the inverse limit topology where all the quotients are discrete and you are also allowed to invert something, provided that the inversion is inverting everything you completed along.

So, the most basic example of these things is you can take $\mathbf{Z}[T]_p^\wedge[1/p]$, the ring of polynomials in one variable, and you can complete it with respect to the p-adic topology and then you can invert p and that's the functions on the closed unit disc in the $\mathbf{A}^1_{\mathbf{Q}_p}$. So, we complete along something and then invert it, and these are also examples of p-adic Banach rings, and what Berkovich does is he says let's just work with arbitrary Banach rings to start with.

(5) **Berkovich Theory**: The local models are given by Banach rings $(R, |\cdot|)$, note while the theories (3) and (4) are confined to the non-archimedean case, this theory over here actually allows archimedean phenomena as well because the real numbers and the complex numbers also count as Banach rings.

The global theory here is not quite as smoothly functioning as in the case of (3) and (4). To the ring $(R, |.|)$, Berkovich attaches the space of multiplicative seminorms $\mathcal{M}(R, |.|)$, which is some compact Hausdorff space, and the kind of gluing you're allowed to do is in some sense organized by this Berkovich space and I don't necessarily want to get into too much detail about that right now. But for example, you can see the complex analytic spaces as a special case of this, and you can also see rigid analytic geometry in some sense as a special case of this. So, this is my very, very brief review of classical theories of analytic geometry.

We have all these theories, that's great, but now what again was the motivation behind coming up with a new theory? Is just going to be point (6) on the list? Well, not exactly.

Question 1.2. Why introduce a new theory?

Well, all from (1) – (5) are theories of analytic geometry, and kind of the relationships between them are more or less well known and one can formulate comparisons and the web of these things is kind of well understood, in spite of the subtleties sometimes involved in the comparisons, it's fairly well understood.

But so far, there's no common framework in which you can put all of these examples, they all have their own flavors, and while you can formulate comparisons, it's not that those comparisons are taking place in some larger category that you consider, it's kind of done by hand in every situation, formulating the comparisons between these things.

Answer. We want to accommodate all examples in a single theory, so that's one thing, um, you'd like a general theory of analytic spaces which can be specialized to whichever context you might uh be interested in.

The second reason would be, out of all the above theories the only rich theory allowing both archimedean and non-archimedean geometry is Berkovich's but the gluing is not so well worked outand in particular, the gluing was investigated by Berkovich he basically restricted to the non-archimedean case almost from the start and then more general gluings were investigated by Piono cite me for example but in that they always do the same thing they fix a Banach ring as the base ring and then they define an affine space of some dimension over that base ring and then they glue along some kind of subsets of a affine space that they that they pick out that's sort of how the gluing works in Berkovich's theory. The Banach rings they take as their base rings often have restrictive hypotheses on them[1] such as finite type.

Even individually in their own context in which they're supposed to operate uh these theories are less flexible than for example the theory of schemes and one major reason has to do with issues of descent.

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Uh, so for example, one of the main constructions when you have a scheme is the category of quasi-coherent sheaves. Um, and that's a big fancy name, but it's really something simple. When you have a commutative ring, you look at the category of modules over that ring, and then it just glues to a general scheme, and that's what a quasi-coherent sheaf is. Um, but you don't have that in analytic geometry in any of the classical theories, and the reason is, okay, I said you always have some local ring, um, which is describing the local geometry, so to speak, and of course, to that ring, you can certainly assign the category of all R-modules, but then it doesn't glue. So, it's just not the case that if you have, you know, in any of the allowed gluings that people choose, it's just not the case that an R-module on two open subsets or closed subsets or anything that have gluing data on the intersection globalizes to a general R-module. The view point was in analytic that coherent sheaves are the basic tool, yes, this is the classical person, yes, and I was going to say this, um, so you only can glue, uh, so maybe finite type or finally presented modules, and this does give rise to the theory of coherent sheaves, which is a beautiful and extremely useful theory. It's one of the main tools you have in analytic geometry, but it's still constrained by the finiteness hypotheses that come into it. So, for example, if you have a map of analytic spaces, you can't consider like the push-forward of the structure sheath as a coherent sheath, but that should be that's one of the main examples of quasi-coherent sheaths you like to play with in algebraic geometry. I mean, unless the map is finite or something, um, so the theory of coherent sheaths is really nice and it works well in analytic geometry and basically all of these contexts, but it's still not as general and flexible as we're used to from algebraic geometry with quasi-coherent she which have no inherent finesse conditions, um, so, so these are all kind of, you could say, theoretical reasons why we might want to new theory, um, but there's also potentially a practical reason, so this is much more speculative, um, so coming from the Langlands program, so, uh, far Fargan Schulza, they famously geometrized a local Lang lens and that led to kind of a clarification of the local Lang lens program, and what was this geometric geometrization based on, it was based on replacing QP, uh, by some more exotic object, uh, far Fanten curve or really you have to let the curve vary in families in some sense you could say far Fen curves, um, and this was produced in the language of attic spaces so it was attic space over QP not at all of finite type, um, so quite a somewhat exotic Beast which thankfully this theory of attic spaces existed to accommodate it, um, and um, again quite speculatively one might hope that not just the local uh Langlands program but the global Lang L program can also be geometrized, um, very okay Peter's always very optimistic but I'm always uh of global Lang length but this would involve replacing say Q or Z by some family of exotic analytic spaces, um, and whatever such a thing is it's going to have to have both archimedean and non-archimedean aspects, for example, there should also be a version over the real numbers which I believe Peter is is working out, um, and it also is not going to be finite type in any sense so there is simply no existing theory which could possibly give the language to describe such an object if such an object even exists, yeah, but it's good to have a a theory a precise theory to to guide exploration of of the possibility of such exotic things, um, so that's another motivation um, so that is the end of my um motivation section, uh, so now's a good time for questions if people have them, so you you are going to introduce this B SP I mean theory what what is are more general theory encompasses all of this yeah I'm going to we're going to introduce a new theory and explain the relation to the previous theories, yeah, yeah, so that yeah that's good to say yeah so our our goal is in this course is to introduce a new theory of analytic geometry and to explain the relation with the previous theories, yeah, so you will let not only the basic analytic ring but also some analytic spaces analytic yeah yeah but today I'm only going to give an introduction to the apoid situations kind of analytic Rings because it'll already be enough, uh, already be enough there, yes, why is the theory of BL spaces insufficient, oh, it doesn't have any archimedean, it

[1] so its not really understanding how you can glue two Banach rings together to get some more global object and indeed the things you're gluing along in these a affine spaces are oftentimes not even controlled by Banach rings themselves they're just some other kind of objects so the nature of the gluing is constrained to a finite type situation and it's a little artificial, maybe not necessarily artificial but it doesn't quite fit the mold when you think of how you go from like affine schemes to general schemes for example just by gluing those local models in some naive way.

doesn't, there are no, it's non-archimedean by Design, yeah, yeah, Al, some I remember forgot now how it was called some kind of spectrum that they combines and forgot the name of this someone consider but they didn't develop it so uhhuh yeah there could be other theories than the ones I

listed I just listed the ones that I knew were well studied and yeah, okay, so then let me move on uh, so continuing the introduction um, I actually had a question, yes please, so um, in the previous uh, sort of theories like uh, I think like at the birk ofage setting there were like Rich uh, theories of chology itology and stuff that were Vari that yeah but I mean um yeah sure bur deeply studied atal kology in in setting of burkich spaces yeah okay and okay um so the next section is called condensed math so the I mean the cond condensed math yes uh the the F this this issue here the issue with the scent um it can be attributed to the fact that these in all these different theories these local rings that you have describing the local models they're not just abstract rings they're topological rings and for many purposes for example uh for uh well yeah so the local models classically are in fact topological rings and it's important to remember the topology so for example okay an algebraic geometry the uh polinomial ring in two variables which is functions on Aline and two space is the tensor product of polinomial ring in one variable and polinomial ring in one variable okay but in say rigid analytic geometry if you take the T algebra in dimension one and tensor it with a tate algebra in dimension one again you're going to get some crazy thing because you took an algebraic tensor product and you forgot the ptic topology but if you do a a Pally complete tensor product you get the ring of functions the correct geometric ring of functions the two variable case so you need to in performing constructions such as tensor products which are basically calculating fiber products geometrically you need to remember the topology that much is clear um so and that's at the basis for the reason why you don't have a naive theory of quasi-coherent sheaves and you have

Quasi-coherent sheaves and you have naive problems with gluing outside the finite type case. I mean, finally generated, uh, case, but topological rings, uh, and topological modules over them, which would be the kind of natural thing to do if you're thinking about quasi-coherent sheaves, are not suitable, uh, for a general theory, and the basic reason there's, there's many ways of saying it, but the basic reason is that, uh, you know, if you form this category, it's, it's not going to be a billion, the category of topological modules over a topological ring, and you can dress it up however you like, you can make it much more specific or whatever, it's just not going to be a billion, and the phenomenon is that if you have a dense inclusion of modules which happens all the time when you have infinite-dimensional things, uh, then it's going to be both an epimorphism and a monomorphism, uh, generally speaking, uh, in your reasonable categories, but it's not going to be an isomorphism, so you separated, yeah, yeah, I would have to say separated to make that literally a true claim, so you have a non-strict map, then the image is two topologies, and this is not yeah, CEG world, yes, yes, yes, yes, um, right, so, uh, so what we do is we kind of go very back to the start, um, so we, uh, go back to basics and Define a replacement for the category of topological spaces, and we do that in such a way that it's then very easy to pile algebraic structure on top of those things and talk about the analogues of topological rings and topological modules over them and that such that we will get an aelion category, uh, in the end, um, and the basic idea is one that is very old and I'm not sure, so certainly it was, uh, certainly, you know, Gro deck used this idea many times and I don't but I don't, I think it might even be older than Gr, topological space is kind of funny because you have second-order data, you have a set of points and then you have a set of subsets of that, and that's fundamentally what makes it difficult to mix with uh, algebraic structures, so instead you'd want to stick with kind of first-order data just points and the basic idea is you single out uh, a collection of nice let's say test spaces, uh, s, and then instead of uh encoding a topological Space X as traditionally so a set and some set of open subsets um, we just record the data uh of what should be continuous Maps uh, from your test space to that topological space, and kind of atati the structure and properties you see in that situation so we were only we we're going to choose a nice collection of test spaces and then we're going to say we only we only care about a topological space in so far as uh, the phenomena are seen by maps from these nice test objects, um, so you still have X set I put quotation marks around this so so I I'll become a little more formal in a second and then you can ask your question, no, but traditionally like they wanted to doop Theory with then so one way was to have ACC a set and to fix a collection of S to X some people some work with this yes, yeah, yeah, exactly substitute usual on you can Define homotopy and ology and do something yes EX exactly yes, yeah, um, so yeah, maybe spanner is a person and yeah, I don't know, yeah, um, so formally, uh, so formally, uh, our test spaces, uh, will be profinite sets so-called profinite sets, uh, which is which is the same thing as a totally disconnected uh, compact house door spaces, um, it's also just the same thing as inverse limits of finite sets where the finite sets have a discrete topology and the inverse limit has the inverse limit topology, um, and so this is what we we uh used in a previous iteration of this kind of course, um, so the first course Peter taught on condensed math was uh with this class of test spaces but it does cause some troubles because uh uh, it's a it's a large category so there's no if there's no Cardinal bound on the profinite sets then when you encode all of this data you're encoding more than a set's worth of data and it it does cause some technical troubles we more or less worked around them but so we're actually going to take a a slight variant of this for the purposes of this course um and we'll explain in more detail in the first few lectures I think um why we make this precise choice but so so a we'll say a light profinite set uh is a accountable inverse limit of finite sets it's also the same thing as requiring it to be metrizable um and then so uh a light condensed set uh is a sheath of sets on the category of light uh, profinite sets uh with respect to the groi topology uh, and now I'll explain the groi topology so so it covers our finite collections uh uh of jointly surjective Maps continuous Maps yeah um um so finite dint unions are covers and a surjection gives a cover so this is like theology because you could add you can add also okay a covering seeve is one which contains a finite collection of jointly surjective Maps Okay um so okay this is we're using the language of gr toies here uh to be sure and possibly not everyone is familiar with uh the language of Gro to and kind of the general how you play with categories of sheaves and so on let me make it more explicit uh so more explicitly uh a light condensed set is a functor okay I'm not going to avoid the language of categories and functors uh so light profinite set up to the category of sets uh such that so the first thing is is that well X of empty set equals Point second thing is that X of a disjoint Union of two things is the product uh and the third thing is that if you have a surjection t to S then uh XS is The Equalizer of uh XT and then the two different pullback Maps you have to the fiber product um and and the example example is any topological Space X gives a condensed set where the funter of points is just given by The Continuous maps from s to X so it's easy to see well the the functor

iality is just that you if you have a map from T to S and a map from s to X you get a map from T to X and it's easy to verify all of these properties uh for continuous Maps out to an arbitrary topological space so groi topology I mean you can just unwind it to this but um actually it's kind of a bit of a bit elusory the elementary nature of this definition.

Because um we are going to fairly seriously make use of the theory of Gro IND topology and sheaves in this course, it's just not worth it to avoid that theory, especially since it comes up both here in the definition of light condensed set and also later in the way in which you GL glue uh the apine case of analytic spaces to General analytic spaces. Um, it's not going to be we're just going to use the theory, so if you're not familiar with the theory of Gro deque topologies and sheaves I suggest and you want to follow this course I suggest you read up on it, um, okay, yes, question, so what is a morphism between two uh, two profile sets like Prof set, yeah, just a continuous map, require some filtration it's just a continuous map but it's also a map of pro systems if you're thinking of it as a accountable inverse limit it's equivalent proem it's yeah, yeah, it's the same same thing yes, do we know what are the points in the category of FL content sets points in the c ah oh oh in the topos thetic sense yeah, I think so yeah they gean um debatable so we we'll discuss more such things in the in the coming lectures um isance of topological space in is that skul no not not for General topologic IAL spes but for a large class of topological spaces it is yeah um right so so some there's one thing that's clear right I started with a general idea or you take some collection of test spaces and then you okay then you say what you axiomatize the properties of maps from test spaces to a given topological space and you arrive at this axiomatics but okay it's actually not so simple because there's many possible choices of test spaces and beyond that there's many possible choices of which properties you want to put in your act which properties you see mapping out from those test spaces to x that you put in the axioms uh for your general objects like condensed sets actually there are other properties Satisfied by X of s when X is a topological space that I didn't put in the axiomatics um so there's kind of a little bit of a delicate balance here and we will discuss more about how we arrived at precisely this this Choice both of the test category of light profite sets and this for now I just want to make a couple of remarks which will maybe give a a sense for why we make this precise definition so so first of all uh two maybe the two most important examples of light profinite sets are the point uh and then there's this uh the one point compactification of the natural numbers um and with this you kind of get the underlying set so if you take X of s then you think of that as the underlying set of your condensed set and with this you get kind of a notion of a convergent sequences uh in your your condens set you get a set of conver set of convergent sequences again it's abstract so it's not literally well in the case of a topological space it literally is the set of convergent sequences so also those things were considered in this topology of oh yes yes yes that's correct yeah and but they had this they use this projective things projective covers which are not light that's correct so this will be discussed this will be discussed in in in due time yeah so but yeah so yeah it's true bot and schultza had this definition but that doesn't mean that it was necessarily the correct thing to do for the purposes I'm about to discuss but yeah it turned out it was um okay and then but then um uh so then another remark is that allowing all surjections uh to count as covers gives a nice simplification of the structure of the category and in particular it gives some good homological algebra properties when you pass to light condensed ailan groups um this you have lot of flexibility of working locally when you allow arbitrary surjections to count as covers but on the other hand uh restricting the topology or requiring uh the topology the gro de topology to be finitary uh gives good categorical compactness properties for light profite sets uh sitting by the on meding inside all uh light condensed sets and and moreover and even and even for uh all metrizable compact house D spaces so for example uh the unit interval famously is a has a surjection from the canra set given by decimal expansions uh and this is a light profinite set and the fact that you have a finitary Gro deque topology and on the other hand this this guy is covered by this guy which is one of the basic test objects it means that the compactness of these compact hous door spaces which kind of we know from General topology actually translates into a nice categorical compactness property inside this larger category um and for this it's actually important to have a these larger light profinite sets than just the sets of convergent sequences okay um let's see okay questions Prof at light it's countable no no that's not countable I mean you you could ask the collection of clo and subsets to be countable so count yeah or yeah something like or it's the same as metrizable for sure so yeah no no they adjust every point of the accountable neighborhood B is weaker than the second count yeah some way yeah yeah you need you need a accountable basis for the topology in total yeah so anyway okay yeah it is the same thing as opposite category of the countable yes I guess yeah exactly yeah it's the same saying that the set of clo and subsets is is countable yeah okay further questions um so I remind you that this is this is just an introduction we will go into much more detail in the in the coming lectures okay so so what do we have now we have uh oh maybe I give myself a Blackboard um so what do you have so far when now I've explained the light Prof finite sets and so um we're going to move on to analytic rings and I'll start with a point which is that these okay from now on sorry so I'm going to drop the light okay so just so I don't have to write it and say it all the time from now on condensed set means light condensed set and profite set means light profite set I always have this cabil uh hypothesis floating around um so condensed sets uh gives rise to the notion of condensed ring and condensed module over condensed ring and it's really uh if you're familiar with the gr topologies and so on it's completely immediate so it's just a sheath of rings and then a sheath of module over that sheath of rings on this uh on this site here it's also just a ring object in this category

and a module object over the ring that ring ring object in this category yeah a question Zoom to write a little bit bigger oh a question to write a little bit bigger that sounds like more of a comment or a request okay uh I will do my best and please hassle me again if I don't live up to it yeah

Um they didn't ask me to write more clearly. Well anyway, Tech, sorry, the formulas more clearly, thanks. Yeah, okay, um. And that's all well and good and you might think naively, okay, so now we have a category of condensed rings, for example. Why can't we just use that as our uh local models for our analytic geometry kind of by analogy with schemes, where schemes are based on discret H. By the way, ring means commutative ring. Um, schemes, you start with discrete rings and then you figure out a way to glue them and then you get schemes. Um, but it's not enough just condensed rings to get a good theory of analytic geometry.

But no, please, cont string is the same object where SE is replaced by a. Yes, that's correct. So, uh, but there's no additional structure on ring, just a abstract ring, right? But condensed ring, it's just an abstract ring with those properties. No, no, but no, no, no, it's a collection of abstract Rings, one for each s, yeah, yeah, okay, right, yeah, but it should satisfy all those properties and Xs cross XT, what does it become, the tensent product of the Rings? No, the cartesian product of the Rings, yes, I see, okay, um, yeah, U where was I H, but are not enough uh to give a good geometry. And the basic reason is as follows, so maybe I should say condens ringings alone. So, the category of condensed rings has pushouts given by relative tensor products, just like in classical commutative Rings, um, and those relative tensor products are what geometrically speaking should calculating fiber products for you, um, and they're the things that I said should correspond to completed tensor products, right, um, but if you have condensed strings, a and b, over a condensed string K, and you form this relative tensor product in this category, you can ask well, what is the underlying ring of this and it turns out it's not actually hard to see from the nature of the grot topology that this is the same as the abstract tensor product of the underlying ring rings of all the individual things. So, this condensed ring here is just, just gives a condensed structure just on the yeah, yeah, just gives a an well non-trivial to be sure but just gives a condensed structure on the on the abstract tensor product, so it's not in particular is not giving a completed tensor product the completion procedure does change the underlying set, right, so this is not not yet doing the correct thing, uh, okay, so to fix this uh, we put additional structure on a condensed string, um, so we record some class of modules, I mean condensed modules, uh, which are to be considered as complete in some sense, complete. So, the basic um, yeah, and that will uh, oh, I forgot to write larger, oh, that would give the notion of analytic ring.

So, an analytic ring will be a condensed ring together with some extra structure which will tell you which of the which of the condensed modules over that condensed ring you should consider as complete with respect to the theory that's being described by the analytic ring, um, but before I make the definition more precise uh, I have to scare more people away. I already said you should know Gro de apologies, get ready, um, so I kind of I have to say more precisely what I mean by a ring, so but I'm going to scare you but then I'm going to say well you shouldn't be too scared, uh, so why is this why is this a question I just I already told you ring means commutative ring right, but um, You didn't say no, I I did you need to pay better attention um, so what kind of ring um, okay so experience in algebraic geometry shows that the generally correct notion of a fiber product of schemes is actually the derived fiber product which on the apine level corresponds to derived relative tensor product of rings. Now the reason more people don't do it that way is because uh, it it's a technical hassle to talk about these things, these derived tensor products and derived rings and so on, but actually that that's not true anymore we have Jacob L's works it's not a technical hassle anymore you just have to you just have to do it and it's no problem so we're going to do it because it's the the correct uh it gives you the correct relative tensor products with giving a good theory in general.

Now that being said basically all of the basic examples that we discuss almost all of the basic examples that come up will not have any derived structure they'll just be ordinary rings so you can comfortably follow the course even if you're not very familiar with derived rings but you should bear in mind that for you know for the for the general claims that we're making it won't necessarily be true if you imagine everything to be an ordinary ring although in examples many things will indeed be Ord Ary Rings um, but now we go down a rabbit hole because once you decide to work with some notion of derived Rings there's actually several inequivalent choices of what you could mean by that um, so so should be derived uh, but which kind there's there's two basic options and that's kind of uh, e infinity algebras and what some people call animated commutative rings. I'm one of those people uh, which

Are the things that are presented by simplicial commutative rings, and then there's also the choice of whether you want it to want to allow negative homotopy. In both cases, um, we won't allow negative homotopy, and we're going to, for the purposes of this course, we'll choose this one here. It's more directly tied to classical algebraic geometry. When you start with ordinary schemes and you take derived tensor products, the things you get always have this extra structure, so it makes sense to remember that and not think about these more general things. But actually, the whole theory that we're developing works perfectly fine in any of the different variants, and in fact, it's even less technical to set up in this seemingly more complicated setting here, for reasons which I think we'll get into. Um, but so algebra in the sense of Spectra exact, there's also that choice. Yeah, you could do you mean infinity algebras over Z or over the spher Spectrum. Yeah, um, so okay, so formally then, so just to get it on the so formally, uh, a light animated ring, oh, sorry, condensed animated ring, is a hypersheaf of animated rings on the uh, site of light profinite sets, but again, I'm not saying light. Um, okay, and now I'm going to make another convention that I'm probably just going to say ring when I mean animated ring, and if I'm want to stress that it actually just lives in degree zero, I'll say, uh, classical maybe or static. Um, static being kind of the opposite of animated, um, right, and that should hope also help those of you who are not familiar with the theory to just pretend that everything is an ordinary ring because that's that's pretty much okay. Um, all right, um, and the basic invariant for us of such a condensed animated ring is its derived category, so the, oh, I need to write bigger of such, uh, such an R is its full derived category. Uh, uh, is this the in L in somewh, yeah, so yeah, you look at just at hypersheaves of modules over this, you know, up unbounded modules over this sheaf of rings, are yeah, justo just a sec, do simpli and coal or does it do it, I don't know, coal no if you want to, you have simplicial modules of simplicial ring, this is not enough to have unbounded in the, no yeah, you don't I mean yeah, you don't really set it up like that, you don't talk about simplicial modules over simplicial ring because then that would be the connective part, you could do that and then just say you kind of formally add in the negative things by by filtered coits or something, I mean by yeah, you do it by I mean the way he does it is he forgets the infinity algebras and then that's just a commutative algebra object in D of Z, and then that has a natural notion of module in the infinity category Theory, so so I mean the theory of modules factors over the underlying infinity algebra and that's just and this is in in which in the in in higher algebra maybe or or S AG probably discussed in more detail spectral algebraic geometry, so but there is no uh, I'm going to say something to help Orient yeah, so if R is static so again that means it's just an ordinary T string uh, then this is the usual or the infinity category enhancement of the usual uh, unbounded derived category uh, of the aelon category of condensed R modules again in the in the totally naive sense of you have a sheaf of rings and you take a sheaf of modules over that sheaf of rings yes, can I ask you to comment on specifically why you want hypers sheaves everywhere it's because we want things like convergence of posnov Tower and we know we can prove that in the world of hypers sheaves but we can't prove that in the world of sheaves so we don't know that sheaves and hypers sheaves are the same thing and also we can always prove everything is hypers Chief and we can always I mean hyper covers never give us more trouble in practice than ordinary covers okay so we're not losing anything by requiring that yes just have a questiony like yes like we can Define schemes in general like just Co limits of representable sheets uhhuh what happens if we do the same here like we take like we glue representable shav over condensed strings no with just condensed strings you're never going to get a good theory you need you need this extra structure yeah I mean if you take sheet over condens strink Sal doesn't I mean the same reason will hold yeah I mean this will be this will be your pullback in pre- sheeps and it's just not the right thing so yeah uh other questions what oh yes hi Matthew yes is not admitted a static animated ring is a condensed anim in the new sense seems like you need some Vanishing of chology because was she condition in just a one categorical s it works I'm not going to get into it right now we'll talk after yeah yeah this is not the time for such a technical question apologies but don't worry if it's correct yeah um okay so uh okay so now I can give the the formal definition um wait did you define are theyre uh yes yes I don't want to say discreet because we also have this condensed stuff and then you discreet could mean yeah so that's the reason for changing the terminology yeah okay so what M no for no no right exactly exactly um okay so the definition is so an analytic ring uh is a pair R uh and then I'm going to use funny notation uh uh where this triangle thing uh is a condensed ring and uh the derived category of the analytic ring is supposed to be a full subcategory of the derived category of this condensed ring which is sort of its envelope um is such that and then we're going to demand some rather strong closure properties remember the idea was this was supposed to be singling out a collection of complete modules um and the first condition is that uh this full subcategory is closed under so inside this ambient category here is closed under all uh limits and colimits uh the second property is that if let's say n Li in here and M lies in here uh then kind of the internal Ram uh from M to n still lies in the smaller thing um the third condition uh is kind of technical so I said we uh we wanted our rings to be connective so no negative homotopy in some sense we also want to require that our analytic Rings be connective and we say it like this so if uh if uh I'd say

This uh denotes the left ad joint to the inclusion. Um, so again that's some kind of completion functor. Then uh this completion uh sends uh the connective subcategory here to the connective subcategory again so it preserves connective objects. I should have said I'm sorry I meant to remind over here so I said if R is static this is the usual ual unbounded derived category of this ailan category. In particular, it has a t structure, has a notion of connective objects and anti-c connective objects but even in general for a general R you still have a t structure but it's not the derived category of its hard anymore. When your ring is not static, say which kind of structure you have in just in terms of the vanishing of of chology, yeah, exactly, and you use homological notation. I always use homological notation. Yes, yes. Do we need object uh H why do we require this? There's some statements uh for some statements it's convenient to have a kind of reduction from a general animated ring to the static case, namely, it's Pi 0 and for this kind of reduction, it's um, it's important to have this kind of control on connectivity. So once you assume the limits and C this is under usual categories or Infinity categories, the same that is well if it's a triangulated subcategory closed under products and direct sums then yeah then to have the int you need. Sometimes some Cal condition is it automatic yes yes so like being generated by a set yeah it's automatic it's automatic yeah Qui one question yes uh with condens ring you mean condens animated ring yeah I I made that convention maybe only in words but yes exactly uhhuh so from now on a ring is a static ring and an animated ring is a ring um okay uh over let's let another one have a chance first uh yes the analog of the ICL inside that indeed it is indeed it is yes uh over no just to make sure so one and two implies that the left agent exists right okay um ah right I forgot to say what a map is so a map of analytic rings sorry I I I I I need to hear what you said so the cond ring is a animated for always anim anim yeah uhh is just a map of condensed rings such that so it's just a condition uh if M lies in D of s uh then the Restriction of scalers of M uh should lie in D of R so so uh along R to S yeah I wrote it a little funny but I hope I hope the meaning is clear so if you have an object in D of s triangle uh which happens to lie in D of s then when you restrict to an our triang our triangle module it should line D of R um okay so I'll make some remarks so uh there's always a t structure on D of R and in fact it's quite naive so the connective part is just uh the intersection of the connective part for the enveloping ring um and same with the anti-c connected part so you can check everything in this potentially more familiar category here so it's actually zg graded not just positively graded the modules are z-graded yes the ring is positively graded that's what I thought okay but the modules you allowed to be zrad indeed indeed um and in particular you get an aelan category so D of R the heart of the T structure which again is just a d of R intersect this um and actually this ailan category also determines the the analytic ring structure so I can also so I can also say that uh D of R is giving an analytic ring structure on our triangle and um there's actually an equivalent axiomatics uh just at the aelan level so I could instead say that I give a a anim a condensed ring and an aelan subcategory of you know the heart of D of r or D of R triangle satisfying certain axioms yes sorry um just maybe kindy of slow it down but for the definition an analytic R yeah I'm just trying to like understand why like uh why in what sense is it analytic like like why yeah so that's something I can't answer right now it'll come when I when we discuss examples and so but the motivation was the simple thing I said that we want relative tensor products to be completed and okay uh yes Robert can I drop hard triangle if I want and just remember the category as a I forgot an axium sorry you just reminded me that because the answer to your question is no because of this axium um sorry I forgot to require that the the unit should be complete so yeah the ring itself should be complete then why can't I drop the Top our triangle entirely well you yeah and then you need some extra structure on D of r i remember as a condensed category yeah yeah that's still not enough because I'm doing the animated context and not the infinity context but if I remember yeah then that's enough yeah yeah yeah yep um okay so that's another perspective is you were just giving an Aon category of complete modules there's also a third perspective which is useful so so to to understand

What an analytic ring structure is, this is very abstract, and it's talking about big categories and stuff. But for a light profite set, I wasn't, I said I wasn't going to say light S. We can consider the free S, the free module. So it's denoted R bracket S. And what is it by definition? You take the free module over the condensed ring, and then you just complete it. So put in there via the left joint. And these generate D of R greater than or equal to zero under co-limits. So those are kind of your basic basic building blocks, your basic generating objects.

And again, there's another equivalent axiomatics which takes as the second data in the pair, not the category full subcategory or Dr, but just the collection of free modules on profinite sets, objects in D of R triangle. So to gain intuition about what this thing can kind of look like, it's useful to think. So this is not in the heart in general. In general, it's not in the heart. In basically all examples, it is, but in the general theory, it's not.

So think, so intuition, so this R triangle bracket S is kind of, well, it's just R linear combinations of points in S, kind of completely intuitively, finite R linear combinations of points in S. But you can think of it as a space of R linear combinations of derac measures. Hey Robert, I did something for you. And then, then this RS is some completion. So that's a bigger space of measures.

So again, an analytic ring structure can also be thought in terms of as specifying some space of measures on a profinite set. And what is the role of this space of measures? So an M, let's say in the heart for simplicity, lies in Dr heart if and only if for all maps F from our triangle S to M of our triangle modules, there exists a unique extension along R bracket S.

Or in other words, if F is kind of a function from your profinite set to your module, which is some kind of linear algebra object with a topology, and mu is one of these kinds of measures that you're allowing, then we get a well-defined integral of this function along the integral over S of the function. I don't know D mu or whatever you want.

You can pair them to get a value in the target module. So this is explaining some sense in which this is this behaves like a completeness condition. It's complete enough that you can do non-trivial integrals against certain classes of measures, which you specify as part of the data. You could say yes, is your D the full subcategory? Yes. And is it compa... no, even this one isn't. They're all presentable, but not, yeah, yeah.

And that's because we did this light restriction, yeah. So we'll have to get into that. Yes, how is the light restriction? Is it Omega one compactly generated? It is, it is Omega one compactly generated. Yes. Is it dualizable? No, I don't think so. Yeah, yes. Well, I wasn't claiming there exists a unique extension. I was claiming this condition is equivalent to this condition.

So were you saying why if this lies here, does there exist this unique extension? Yeah, so that's because basically just by left ad jointness, but it comes from unraveling the definitions. What is the question? Was which one is is only one comply generated? The big one or the small one? Neither. Oh no, both of them, sorry, both. Both. I marred the Omega one, yeah. Both are, oh, both, yes, okay.

So I don't think I'm going to have time to get to examples, which is rather unfortunate, but I don't know, maybe Peter will, I don't know, we'll see what Peter plans for Friday. Be nice to talk about some examples. Um, but instead, I think I'll probably finish, well, yeah, I'll probably finish with a discussion of co-limits in the category of analytic rings, and in particular, I want to talk about pushouts because this is the crucial thing which is supposed to give completed tensor products which correspond to geometrically good fiber products.

So where am I? Here, so your anima structure on... sorry, anity, oh yeah. So one of the things we prove is that it comes for free, okay, yeah. I mean you have, I mean this is a, I mean no make no mistake in the maps. You have a map of animated rings from the r triangle to S triangle, but then it turns out whatever the linear algebra operations you might expect to like symmetric powers and so on will sort of automatically go through.

Yeah, it's not obvious by any means. So your definition of you back left that, yeah, that's right, that. So that's the most important functor, is the left ad joint to the thing that was in the definition, that's a very good point, that's yeah. So there's some things I'm not mentioning like that D of R is actually symmetric monoidal and this completion functor is a symmetric monoidal functor and these pullbacks, the left ad joints we were talking about, are symmetric monoidal.

So these are the things that um and preserve. Subat, well by definition it's defined to be the left adjoint to this restricted functor, yeah, or you take the but but is not true that if you do the left joint on the level of the these envelopes that it necessarily preserves the category, you have to complete at the end.

So again it's kind of a completed tensor product in this base change here, um, thanks for the question, yeah. Okay so so co-limits in analytic rings, um

so so so um filtered co-limits or more generally sifted co-limits. I'm sorry, oh, the usual notion. So based on finiteness, I mean yes, oh Alf not filtered, yeah. I thought it might be important because you have, I mean indeed one could imagine it might be important. But when I say this then there's no discrepancy, so it's a, I mean there's no ambiguity, so um so if you have filtered co-limit of RI, then the underlying animated R is just the filtered co-limit of the underlying things, um, and also the free modules are are similarly described is just the filtered cimit of the the free modules on the um so that's rather rather naive, um.

And what's kind of left is pushouts and this is more interesting so pushouts so if again we have maps of analytic rings I'll call them K A and B um I'll write the push out as a relative tensor product just because um then the derived category of the pushout can be more or less immediately described so I'm not talking about the first point in the data but just the second Point um uh so abstractly this category will be the same thing will

Actually, be a full subcategory of. You take the push out in condensed rings, and then it's the full subcategory such that the underlying a triangle module lies in DA and the underlying B triangle module lies in DB.

But, but this caution. A triangle tensor K triangle B triangle D A Tor K B is not an analytic ring. So, it satisfies one through three but not four. So, it's almost an analytic ring. The only thing is that the unit object, the underlying ring, is not complete. But then you fix that by applying a completion procedure to fix this. You can still prove there's a left adjoint to DA Tor K B sitting inside DA triangle tensor K triangle B triangle.

So, I think you're using D in two ways here. Simp, you mean the category of A is a ring. This when you're writing it, it's also it's also part of the definition of anal. That's true, that's true. So, let me make a remark to reconcile this that there's a trivial example of an analytic ring structure on any condensed ring, which is that you take D of A to be equal to all D of A triangle. And with this interpretation, the notations are completely consistent. So, you could call that analytic ring. You could call that analytic ring a triangle. It's just the um. So, every condensed ring can be viewed as an analytic ring with kind of trivial analytic ring structure or maximal analytic ring structure. Everything is complete. And with that in mind, then there's actually no conflict in the a different thing you could have done here, which is take so you have a an antic R. So, it has a subcategory is part the data. You could take DA tensor DP as the categor DK. And that would be a different thing then doing this. Oh no, actually, it's the same. Yeah, it's the same.

Yeah, so um okay. What next? Um, yeah, question. Oh, question. Oh, I'm going to call on the person who raised his hand first. Sor yeah, that one just the same as the tensor product of the categories. Oh, that was already asked and answered. Yeah, over okay, is it, do you change the ring when you apply completion? You change, you change the, because the simpli R is D anyway, you apply the completion in the category D and then you must get another simpli ring. Yes, we have to prove that it's not obvious but yes. So, there's a so yeah, so then when you apply that completion procedure, the category stays the same but then this becomes completed. And also, I should make a remark that in complete generality, it can be rather difficult to understand this completion process. You kind of have to iterate applying the completion for A and the completion for B sandwiching them between each other take account do it countably many times take a co-limit like abstractly that's the formula for this completion procedure here. Now in practice, it turns out you can calculate it and this is one of the points uh in practice I don't think I have time to discuss examples today but this completion procedure which replaces this by the true underlying ring of uh the the the pushout in uh analytic Rings it produces the geometrically correct completed tensor products in analytic geometry.

Um okay so maybe um there's a question from Zoom. Yes, is the condition on the fenus still necessary or did you show that it is always satisfying it's always satisfied in this that's one of the reasons for choosing this light condensed set framework it's actually always satisfied it doesn't matter um so maybe okay maybe I'll start to talk about examples so I risk kind of getting cut off in the middle of an explanation but I feel like it's just too dry without well I won't get very far okay let's do let's try let's try um so I want to talk about uh maybe solid analytic Rings um and this will relate to attic spaces or hu pairs um so uh it's kind of going to be a non-archimedian addition so so I mentioned that if you if you have an analytic ring and we haven't talked about how to produce them yet but if you have one um then it's nice to look at these free modules and profinite sets to get an idea about what what's going on what what spaces of measures do you are you actually looking at here and I also said that the basic example of a profinite set well besides the point was this n Union Infinity classifying convergent sequences so but um in this linear case it's it's natural to consider the following so given an analytic ring R it's natural to consider what you could call I guess space of measures on the natural numbers and I don't mean the free module on this discret thing but what I mean is you take uh the free module on this sequence space and then you mod out by Infinity so this in some sense classifies null sequences in our modules oh I have a backboard up there too and it turns out it's not hard to show that um addition on n induces a ring structure on uh on this MRN um and as a ring it kind of sits in between two rather extreme options uh uh so maybe maybe you want to think of this as t to the N for the purpose of this kind of discussion um so it's sitting somewhere in between the polinomial algebra and the power series algebra over your ring R um as you could imagine for something like a space of null sequences right uh or sequences with some gross growth condition I mean it's really dual to null sequences it's maybe some kind of summability condition um so geometrically speaking we have the apine line and we have some version of the formal neighborhood of the origin and then we have something that sits somewhere in between right um and now I'm going to single out a condition which is kind of a non-archimedian condition that morally speaking will mean that this guy uh lies inside the open unit dis of radius one um but formally so let's say definition uh R is solid uh if um if you if you do this you get zero but since you qu by the constant R Infinity is just the ah okay this is the home yeah that's just R and then but you know and then it's mapping into r n Union Infinity by the inclusion of the point Infinity so R infinity is the free yeah in this the free R module on

this finite yes view as a as a as a condensed object or no no it's not cond what what is that like solid over Z or uh just a sec I mean this is the i' so far I've just said this definition right so you I mean in this sense but

Just a second.

Okay, so there's an interpretation of this which is well if you have this and it's actually equivalent then you get a measure so to speak. So, $T-1$ has to, you know, multiplication by $T-1$ has to kill. So, if you have anything here there has to be a pre-image under multiplication by T minus 1. So, this means you get some measure here such that $T-1\times\mu$ is equal to the unit object in this ring MRN, which is kind of the sequence $1,0,0,\ldots$. And if you think about what this means, thinking about this measure space sitting between polynomial and power series, this corresponds to kind of sum over n, t to the N, that's at the very least what it maps to in the formal power series ring. But on the other hand, this measure space, as I said, classifies null sequences.

And you know, so and this measure pairs with a null sequence to if you have a null sequence in an R module M and a measure, I said you can pair the two things to get a function. And the way it works is you take your null sequence, you put it as coefficients here. And yeah, you set t equal to one. So, what this, the interpretation of this is that every null sequence, you kind of have to work it out but the interpretation is that every null sequence is summable, which is kind of classic non-archimedean condition. So, solid is kind of one way of saying non-archimedean in this context but it's kind of fun that geometrically you can think of it as constraining the location of something between zero and the whole real line.

Maybe I will state the theorem. Yes, no. Okay, so theorem, there's a question from chat, the multiplication closed in MR, multiplication closed, I mean, it's a ring. I don't know what may I have sort S as you construct it. I guess it's ConEd as some cone it's not the best. Okay, okay, yeah, yeah, yeah, yeah, yeah, yeah, yeah, but in the Universal case Z with the trivial analytic ring structure, it lives in degree zero, there's no, I mean and also that's a summand, it's really but it's still not it's not in the heart it is in the Universal case it is and to produce a ring structure I can work in it's a ring yeah in the Universal case it's a ring and then by base change it's a ring in whatever sense you want in whatever other context I okay, yeah, okay, um, right.

So theorem, so there exists a solid analytic ring, so it's called "zolid" and well, the underlying condensed ring is just the usual integer Z kind of discret topology. And then well, the derived category is something which I'll discuss in more detail such that an analytic ring is solid if and only if there exists necessarily unique map from zolid to R and moreover, you can actually understand this analytic ring very very explicitly. So, there are some nice results on linear algebra in this basic category.

So, the first thing is that you, well, the first thing you want to ask is what are the free modules on profinite sets. So, let's say S is some countable inverse limit of finite sets then this is just the inverse limit of the free module on the finite set which is just a finite direct sum of copies of Z. And also, this is abstractly isomorphic to some countable product of copies of Z countably infinite unless of course S is itself a finite set. What does that say next to the there exist is that say unique? It says necessarily unique, yeah, yes.

So, the second thing is that, oops. All right, so these, so these. These are remember I said that these guys always generate the category so in this case these products generate the category but moreover these guys here are Compact and projective generators of the well let's say the of the heart um but they live in degree zero so another thing you have is that the derived category here is just the D usual derived category of its heart so it's enough to talk about the bilan category um and also these are flat with respect to the tensor product the completed tensor product uh which I'm kind of mentioned exists um so here's another point where we use the lightness because Sasha fimale proved that this is not hold if you increase the cardinalities on the profinite sets um and moreover you can calculate tensor products rather easily so tensor product of this with this uh over Z solid is just have the infinite distributive law so to speak um that makes for very easy calculation and uh sorry so just asking the the regular is not enough it is really yeah, it's really accountable really yeah, yeah um right so the and the the collection of finitely presented objects in D of Z heart uh which is it generates it under filtered Co limits um is a bilan and closed under extensions and every every finitely presented M has a resolution a free resolution you could say by product of copies of Z's of length at most two meaning a complex where there's three non-trivial terms and two non-trivial maps uh so this kind of gives you a very good hold on calculations in this category very very explicit um so note that you can interpret this sort of as saying that uh so zolid behaves like a regular ring of Dimension two uh so Z is a regular ring of Dimension one we somehow picked up an extra dimmension and that can be attributed to the nonous dorf phenomena that you see in uh in solid ailon groups um but in in all things told you get a very good handle on this category so I think that's the the only example I have time to discuss and thank you for your attention last actually there's only isable there's only one uh right there's a single generator actually this is kind of a general phenomenon because the the free module on the canra set will always will always generate yeah, yes when when when we taking the pushup uh there's a completion fter AI gives you a DED category object why is it that it's a ring yeah that's something you have to prove and

it's not obvious thank you yeah mhm yes Chad ask question if there will be lecture notes and video recordings video recordings yes lecture notes we're kind of trying to write a book at the same time as we give these lectures and it's not clear to what extent we'll be releasing things sequentially or all at once at some point so in Uh, uh, uh. I mean, keep me motivated to. No, I'm sorry. . What can I say? Yeah, I don't know. Or do you like Reman Rock? Of course, I like Reman Rock. Okay then, it proves the most general possible Reman Rock theorems in analytic geometry. Yeah, so he crucially uses these derived categories and the fluidity of the formalism and so on.

Yes, what's the notion of the right-hand triangle? Um, Peter told me Huber uses it. Uh, had to find something. Yes, is there technical advantage to using a light condens set instead of the general condens? I mean for this antic. Yes, advantage, but for just topology. Oh, for general topology, well, I don't, yeah, I mean, there are some more subtle properties that tend only to hold for general topology maybe, not, but once you start talking about topological groups and so on maybe, yeah, yeah, nice. The same question but backward, yes, go ahead.

I just strictly prefer the light setup to the other one, unboundedness and yeah. Is there any case where I would actually want to go back to that one? Well, it's, at the very least, psychologically comforting when you have a strong limit cardinal that you get like compactly generated derived categories and so on. Turns out it's not so important necessarily in practice, but it's kind of maybe quite important psychologically. But then okay, that's just a larger cardinality bound why you would go all the way, it's just to avoid choosing a cardinality bound and so you can say all compact Hausdorff spaces are profinite I mean or condensed sets but there's no real reason necessarily.

What is dist use Infinity algebras? What are those? The structure, can we compare? But I don't, I don't understand the question but what is this term infinite? Oh, e-Infinity, ah, okay, uh, uh, everything works the same except it's a bit easier if you use e-Infinity algebras, yeah, the people who are comfortable with the infinity algebras are not laughing, why, why not, why not even better? Why not E1? I what is the commutativity, oh yeah, so you could, you could do a version of this theory of course with E1 and E2 but but there's something very special about e infinity or animated commutative which is that co-products are the same as relative tensor products that's very nice and moving to realms where that's broken can be a real pain.

Okay, so that's it, thank you.

2. Light condensed sets (Scholze)

https://www.youtube.com/watch?v=_4G582SIo28&list=PLx5f8IelFRgGmu6gmL-Kf_Rl_6Mm7juZO

Unfinished starting from 0:00

All right, so welcome back to the second lecture. I guess I should make an announcement about the schedule for the next few weeks, which is a little bit different than usual. Usually, it should be the case that Dustin lectures Wednesdays at PS and Fridays I lecture here. But due to some traveling, I will give the next few lectures all at MPI, but note that there will be a gap of two lectures. So there are no lectures next Friday and the Wednesday after.

So the goal of today's lecture is to recall—partly this will be much repetition of a very similar lecture I gave four years ago, also five years ago. But something I want to stress throughout is why we switch to the Huber setting and which properties you gain when you make this restriction.

Before I go there, let me just say a few introductory words from my perspective about what was said last time. So what's our goal with all of this analytic geometry? For me personally, one goal since 10 years or so has always been to find—there's all this fancy geometry that has been developed p-adically, like perfectoid spaces, prismatic cohomology, p-adic shtukas, and the geometrization of local Langlands. This all works quite beautifully over the p-adic numbers. But it has to use some quite fancy p-adic geometry, like these highly non-noetherian perfectoid spaces and so on.

I mean, I'm always hoping that similar ideas or techniques could also be applied not just over p-adic local fields, but also real local fields. That's actually something that I think really is possible now. But then also over the whole integers, globally.

So in some sense, what I want is some kind of notion of a global $\mathrm{Spec}(\mathbf{Z})$, something like that. And this guy, this should definitely be some kind of analytic space, some kind of adic space, some non-affinoid—I mean, some model by some rings with a non-archimedean norm. Well, it should definitely include those archimedean and non-archimedean parts, $\mathbf{R}$ and $\mathbf{Z}_p$. You know, hopefully a kind of uniform language to talk about these parts. But it should also be extremely non-noetherian—you can't hope for any of the usual finiteness properties often imposed.

So it's clear that you need some new language to talk about these things. And so basically, the goal of this course is to develop a language in which it is at least conceivable that such objects exist.

I mean, here's some kind of very vague idea that's possibly completely misguided. So when you do these perfectoid things, you have some kind of perfect field, maybe $\mathbf{C}_p$ or something like this. But then when you have an atlas over this, you're always doing the thing where you adjoin a variable, but with it also all its p-power roots. So I mean, this is a prototypical example.

Well, if you think about how you would do something similar, like not a fixed prime but kind of globally—well, then you say you definitely want to get rid of the choice of the fixed prime here. So okay, so maybe you have no idea what the base is, but at least you can try to understand what happens relatively. And then relatively, I think you at least certainly want to adjoin some variable with all of its rational powers. Because each prime p is forced in some way.

But then you're only taking care of the finite primes, and then you maybe wonder, what could be the analog of this at an infinite prime? I mean, something that suggests itself—and again, maybe it's completely misguided, but something that you tend to wonder is whether you should try to form some ring where you adjoin all real powers of a variable T. But then you begin to wonder, what kind of object should this even be?

I mean, so you can definitely just treat the real numbers as a discrete thing and then adjoin here and all real powers, and then this would be some kind of algebra. But I mean, this feels somewhat artificial—that's the real, I mean, and sometimes then the real numbers are just some uncountable dimensional Cantor space, and you don't feel like you really made the situation any better.

So you definitely want to kind of keep track of the topology on the reals here. But then it's mixing the topology, like the profinite topology you usually have appearing, in a very strange way with the real

Correspondences, F-schemes and similar—this should be some algebra modeling for some local model of the space St I called this last time. But even I'm more unclear what kind of geometry that should be.

So maybe these are not at all the correct objects. But maybe we could at least, if these would turn out to be the correct objects, want to have a language we can talk about those. So some such things will be involved.

Okay, and so when Dustin came to Bonn in 2018, he somehow made the suggestion that one should really everywhere kind of replace topological spaces by condensed sets. Since 2018, we've been pursuing this path of replacing topological spaces by condensed sets.

Something that was definitely clear from the very start is that this switch does resolve a lot of the foundational issues you usually have when you work with some analytic geometry. For example, it makes it possible to remove Noetherian hypotheses that are quite pervasive, for example for adic spaces or formal schemes. I'll talk about abelian categories of complete modules and so on, and derived categories.

It was clear that a lot could be gained by doing the switch. Simultaneously, something that I always liked is that suddenly such a thing as $\mathbf{Z}[[T]]$ does make sense. This is something I will in some form also discuss today. Maybe this is not really the correct thing to consider, maybe some other thing, but I was quite happy that this kind of object at least exists in this framework.

So our attitude then was that maybe we have really no idea what we're really looking for, but let's just try to develop the foundations of some kind of analytic geometry from the perspective that you should start from some kind of condensed rings. Then try to build as natural as possible a framework so as to at least accommodate all the known examples and possibly extend them quite a bit further, for example allowing Schemes over $\mathbf{F}_1$ and $\mathbf{F}_{1^2}$.

Then just see where you're led to, and maybe someday one can hope to understand how such more exotic examples might be related to some kind of interesting geometry or number theory. Basically, try it out.

Also, in the known formalisms, there are sometimes things that are slightly beyond the traditional categories. For example, if you work with adic spaces, there's the usual things built on Banach algebras. But Grosse-Klönne for example has a theory where you have some overconvergent functions instead, and usually this is yet another category. You would also like to accommodate all these variants.

Peter, can you hear me? Great. There's a request to write a bit larger if possible. Oh, we cannot. Okay. Maybe we can increase the– [Camera discussion]

Okay, so this course will not really touch on anything fancy like that. It will just try to really lay out what the formalism should be and how it accommodates the known examples.

Okay, let's start the course. Let me start by talking about condensed sets. The starting point for the whole theory is profinite sets, which will for the moment be restricted to these small things. First of all, I want to recall the following proposition. Sorry, I'm not writing bigger.

Always, the following categories are equivalent: The pro-category of finite sets. Sometimes this is a purely combinatorial kind of category, where the objects are just certain diagrams of finite sets, formal inverse limits, where the S_i are just some finite sets and I can be taken to be cofiltered.

Recall that this means that I is not empty, and whenever you have two elements in I, you can find one that is strictly smaller.

The morphisms, well, this is an inverse limit, so you can pull that out first. But then there are two traditions about writing limits and colimits, either with an arrow and just a "lim" or writing "lim" and "colim". I'm kind of combining the two for maximum clarity.

I might at some point just write lim and colim, but for the moment I'll use the directional arrows. But then when you have a formal inverse limit, nothing just means one in TJ. This–

One can also think of this S more concretely, but now using—in some sense this whole theory could be developed without mentioning topological spaces, but let's not try to do so. It's also equivalent to totally disconnected spaces, where here you send—let me write the functor.

A third possibility is to take the category of Boolean algebras. So recall what a Boolean algebra is: it's a commutative ring R such that for all $x \in R$, $x^2 = x$. In particular, $(-1)^2 = -1$, meaning that $2 = 0$. So these are—let me write down some functors.

So if you have a formal inverse limit of S_i, you can map this to S, which is the limit of S_i but with the inverse limit topology. And then this would map to the continuous functions on S valued in $\{0, 1\}$, which then also, if you compose the two things, is just the colimit of all $\{0, 1\}$-valued functions.

And one can also go back. If you have a Boolean algebra A, this maps to $\mathrm{Spec}(A)$, which is also—you want a map from A to—so actually all points of the spectrum are just $\mathrm{Hom}(A, \{0, 1\})$. And actually, we can also write this Hom as a colimit, a limit over all finite subalgebras $A_i \subset A$ of $\mathrm{Hom}(A_i, \{0,1\})$. Alright, let me not say anything about the proof. I mean, one can actually quite easily check these functors.

So most often, I will actually think in terms of this presentation. Whenever I have a profinite set, I will usually just present it in some way as such a limit. I'll most often think about it that way.

And now that I want to come to the main things, I need to tell you two ways to measure how big—well, let's say S. When I write this, I implicitly mean that the S_i are finite sets and the index category is filtered.

The size of S is just the cardinality of the underlying set. And then there is something that's traditionally called the weight. Maybe this has a different name, let's just—so this is the cardinality of the corresponding Boolean algebra.

Let me make this definition. Here, S is light if it has kind of the smallest possible weight, except it could be finite. We definitely want infinite things. S is light if the weight is at most $|S|$. It's countable, yeah, so this is also equivalent to— actually, if λ is infinite, then it's also equal to the smallest possible cardinality for a small, possible—

In general, there are many ways to present a profinite set. For example, a point you could present as an index limit of a large profinite set of just always a one-element set, which is kind of silly. But there's always a minimal possible choice. And if it's infinite, then this is unique.

So in other words, a profinite set is light if and only if it's at most countable. Alright, so let me give some examples.

Just a quick question: "How does it compare to having countably many non-trivial closed subsets?" How do the properties compare to having countably many non-trivial closed subsets? If you take the maximum—I mean, the closed subsets are basically the ideals, right? How many of them—what does it mean?

Yeah, I think that's also the same thing. Because any such collection is given by countably many finite collections of such subsets. And I think this is still the same.

So let's do some examples of profinite sets and of their size and weight. Okay, so for finite sets, you can figure out what the size and weight is.

And then maybe the first, the smallest kind of infinite profinite set is the one-point compactification of the integers. And one way to think about this is that it's a limit of all maps $\mathbf{N} \to [n] \cup \{\infty\}$ counting up to

Limit. So it's still light, but obviously it's much bigger than this one if you just think in terms of the number of points. Number of points is, in general, these are really different.

Let me give one more example that has some relevance in the theory, although excluded. So that one very important thing: any locally compact space has two compactifications—one compactification, and the Stone-Čech compactification. This one is small, this one is really huge. So let me not try to present this as a limit.

Well, let me instead of presenting it as a limit, let me really say what are the continuous functions from S to $\mathbf{F}_2$. These are the continuous functions from the Stone-Čech compactification. But by the universal property of Stone-Čech compactification, this is really just a continuous map from the natural numbers to $\mathbf{F}_2$. But these are discrete, so this is really just the set of giving such $\mathbf{N} \to \mathbf{F}_2$. You just have to specify the image of zero, and this is just any subset of the integers. So this is just the set of all subsets of the numbers.

So we see that this actually has weight $2^{\aleph_0}$, and it's not super clear, but you can show that it has size $2^{2^{\aleph_0}}$. It's pretty large. So in all those examples, it turns out that, well, in the example that you gave, the size in the infinite case—the size is either equal to the weight, or it is bigger.

For some reason, I only hear you very softly. We need to get CL. Okay, okay. So I just... There is something that I don't remember the answer to. Do you hear me now? Yes, now I hear you better.

Okay, so in the examples, of course there are trivial estimates that the size is at most 2^{weight} and at most 2^{size}. And so you can ask whether there is about inequalities between them. So in the case where you consider, like you have the countable set and the Stone-Čech, the size is 2^{weight}. So the question is whether it can be the other way in the infinite case, that is, that you have actually... When I prepared, because I wanted the same question and I didn't, but I think I know an example, but anyway it's a bit complicated. But that's also what I suspected.

So let me tell you what I know is easy and sufficient for us to know. Here's a proposition. As Ofer mentioned, you have trivial estimates, and I will tell you they are trivial, that λ is at most 2^{κ} and κ is at most 2^{λ}. And you see that this one can be attained, and actually in quite large generality you can make examples where this becomes as large as this. But it's actually quite hard to give examples where λ becomes bigger than κ.

So let me just note that in the case that's most relevant to us, if you have a profinite set that's countable, then actually it can also be written as a countable limit of finite sets. So all the ones, these are like, I mean, this wouldn't follow from this inequality, right?

Why are these called "trivial"? λ, I said, was the continuous functions. Certainly bounded by all the maps from, this is obviously 2^κ. And on the other κ is the homomorphisms from the Boolean algebra, right? Because I said you can recover the profinite set as a map from Boolean algebra to F_2. And again, this is bounded by all the maps from λ.

So what's quite nice about this is that these estimates even hold true in the finite case. In the finite case, actually this one becomes $\kappa = \lambda$.

Let me do this one case where κ is equal to $\aleph_0$. So then you can enumerate S, we can enumerate the elements. And then for each n, inductively choose a quotient S_n compatible, so that first three elements z_n inject any finite set. You can always find such a quotient.

Then you see that the map from S to the inverse limit of S_n

As Scholze mentioned, you build a finite quotient where these elements are distinguished and then take the limit.

Alright, okay. So because this SL condensate will be so important for us, let me just rephrase the first proposition for light condensates. The following categories:

First, let's call it Pro^N, the sequential pro-category of finite sets. The objects here are not some fancy limit along some cofiltered poset, but really just some limit along the integers of some finite sets. And the morphisms are as before, but you are not allowed to change the m, so that's an important thing. So the Hom from $\varprojlim F_m$ to $\varprojlim G_m$ is again, you can first pull out the limit and then take the other thing to be the colimit. Let me actually note that there's a different way to think about this. Basically, something you are always allowed to do in a pro-object is to pass to any cofinal subsequence. And then, if you want to give a morphism from here to here, you think that first you extract a subsequence of the ns, and then you really just give compatible maps. So one way to express it is, it's really some colimit over all possible strictly increasing functions φ of compatible maps for all m towards $G_{\varphi(m)}$, but from the three-scale version.

You can also phrase this pairability condition in terms of totally disconnected compact Hausdorff spaces, and it's precisely the condition that they are metizable. And the last one was Banach algebras, and for Banach algebras, we precisely made the condition so there we made it.

So let me just note one very simple proposition that will be used throughout. If you work in Prof_{fin}-sets, and this says you have all limits, but if you restrict to light Prof_{fin}-sets, then you still have all countable limits and sequential limits of surjections are surjective. Maybe I should say surjectivity is just meant in terms of the underlying sets, like taking the actual limit. So in other words, surjectivity on the point set of the component processes. And I mean, something similar is true, that any limits of surjections are surjective in all Prof-sets, but there it uses some rather high-powered compactness result, like the Tychonoff theorem. In the sequential case, it's really kind of stupid, you just successively lift.

Okay, so the first interesting thing I want to mention is that the natural numbers actually play a bit of a universal role within the light profinite sets. That is, if you have any light profinite set S, then there exists a surjection from $\widehat{\mathbf{N}}$ onto S. I mean, the proof is really simple induction. It just writes S as a sequential limit and then at each point, just pick a large enough finite quotient of $\widehat{\mathbf{N}}$ to accommodate everything you already have.

Alright, so now I want to come to two properties that light profinite sets have and that fail in general, and that will play a technical role in what we're doing. These are some of the key reasons that we make the switch. Now, two properties that are special to light profinite sets, they may also work for some other Prof-sets, but that would be hard to single out there.

First, open subsets of light profinite sets can be too wild. If U is an open subset of a light profinite set S, then U is actually a countable disjoint union of profinite subsets of S. So this is to be contrasted with the following: In general, there exist open subsets U in a profinite set S that are not disjoint unions of profinite subsets. One way in which this can fail is, for example, you could take a product of profinite sets. I mean, disjoint unions take coproducts to products, and on profinite sets, these are totally disconnected. So you can't have non-trivial open coverings in this sense. But in general, the structure of these open subsets

Let U be an open subset of a profinite set S. Then U is a union of clopen subsets, so we can write $U = \bigcup_n S_n$ where each S_n is clopen in S. More precisely, choose a presentation $S = \varprojlim S_n$ where the transition maps $S_{n+1} \to S_n$ are surjective. Then U is the union over all n of the images of $S_n \setminus f_n^{-1}(S \setminus U)$.

So now you've at least written it as a sequential union of closed subsets. Maybe I should have said, one way to think about open subsets purely in this language of pro-categories and profinite sets is to think about the closed things instead. Closed subsets should themselves be profinite sets, and then the closed subsets are precisely the injective maps of profinite sets.

The closed complement should be a profinite set. Someone injects, you take the preimage of this. Then if you want, you can write U as a union over all n. Now these are subsets and are themselves profinite sets. Okay, so that's one nice thing.

Here's another one. Then S is an injective object in the category of profinite sets. I will spell out what I mean by that. This means that whenever you have an injection of profinite sets $Z \to X$ and a map $Z \to S$, you can always find an extension $X \to S$.

Assume that these don't characterize all injective objects. Or are these exactly injective objects? So there are more injective objects. For example, any injective object in general is closed under taking products, so any product of profinite sets would also be allowed. But the profinite sets in particular are profinite.

So let me prove this. Actually, the first thing one should check is the case where S is just $\mathbf{F}_2$. In this case, it means that the continuous maps from X to $\mathbf{F}_2$ are in bijection with the clopen subsets of X. Or equivalently, any clopen subset of Z can be extended to a clopen subset of X. Consequently, I don't know what an exercise to do.

Okay, let's assume we know this case. Then in general, just write S as a limit of finite sets S_n. In general, the transition maps are not required to be surjective, but I will assume that all the maps are surjective. You can always assume that.

In this case, you will argue by induction on n. If you want to extend the map to S, you need to extend it compatibly to all the S_n's. But if you've already extended the map to S_n, then extending further to S_{n+1} means the whole situation decomposes into a disjoint union over all the fibers over S_n.

So you can assume that S_n is just a point, but then S_{n+1} is just some finite set. Then it's a very easy exercise to extend, maybe just have to extend a bunch of clopen subsets. Okay.

Exercise: Figure out why this argument doesn't apply to a general profinite set. You might still think that you can similarly extend as a limit along surjective maps. You can always do that and then try to inductively extend.

Just a small remark. If Z and S are both empty, do you need something? No, Z is empty and X is not empty. Okay, if S is not empty, it's okay.

Okay, I think that's it for my general preparations about profinite sets. So let's now finally come to the definition of profinite condensed sets on a site with the following covers. We always take a profinite set S and a cover of S by other profinite subsets. This is the cover, and also surjectivity.

So let me, as last time, spell out what this really means. A profinite condensed set X is a functor from profinite sets to sets satisfying the following conditions:

Just need to M both of them individually, and then there is this funny condition. This comes from allowing all the surjective maps. This means that whenever you have any surjective map of profinite sets, then to give a map from S to X, it's sufficient to give a map from T to X.

At least as a first approximation, you want that any map from S to X is determined by what it does on T. But actually, you also want to characterize which maps from T to X actually come from S, and these should be the ones that agree on fibers of this map, so to speak. The good way to say this is that the two ways you can make a map out of the fiber product, either first projecting to the first coordinate or the second coordinate, this should agree. This is what the sheaf condition for this surjection says.

Before expanding machinery, let me just tell you this key example to have in mind. Let's say A is a compact Hausdorff topological space. Then we can define $\underline{A}$, and this is the thing that takes any S to the continuous maps from S to A. Here the presheaf on profinite sets precisely remembers how profinite sets map continuously into your topological space. This has all the fun properties.

Whenever you have a map between profinite sets, that remembers how a continuous map from one gives one to the other. It turns out that this condition is always satisfied. There's actually not a concrete topology, so if you would omit continuity then this would be clear. If you want a map from S into A, it's sufficient to give one from T into A, and then it factors over S if and only if on the fibers the map is constant, which is kind of expressed by this.

But you're saying more than that here, because you ask that the maps have the continuous property. In other words, if you have just any map from S into X and you know that if you restrict to T it becomes continuous, then it was actually continuous to start with. Equivalently, S is actually a quotient and has a quotient topology from T. This is actually a general property for profinite compact Hausdorff spaces - they are actually quotients. In particular, $|A|$ is just A as a set.

In general, for any condensed set X, we think of $|X|$ as the underlying set. But you can also evaluate on some of our other favorite profinite sets, and maybe the most important one is this one-point compactification of the integers. What does this correspond to?

It's a continuous map from $\mathbf{N} \cup \{\infty\}$ to A. In other words, it's a sequence in A together with a limit point. So in other words, it's a convergent sequence, generally with the choice of a limit point, though most often there's at most one limit point.

Similarly, when evaluating X on this, where giving such a convergent sequence, we also have to give a witness for what the limit is.

Okay, and then you can do more wild things. You can evaluate this at the Cantor set, and well, this is what it is. One thing to note however is that it's just a set, but it comes equipped with all the continuous endomorphisms of the Cantor set.

Let me just give one remark here and then forget about this forever. If you have a condensed set, it's completely determined by what I will describe as a functor from profinite sets to sets. By $|X|$, the Cantor set together with the actions, where this is really just an abstract set. This guy here is just, so you could think of a condensed set purely algebraically as a set equipped with an action of the profinite monoid. But I think this is a strictly worse way to think about this. Don't do that.

But I mean, it makes the point that in some sense such a set is a very algebraic kind of thing. But why, maybe I should at least say why this is the case. This is precisely the case because if you want to know what the value on any other S is, you can cover it by Cantor sets. And then for the fiber product

You're saying it into the category of what, exactly? Well, I think just this functor from sets to... No, sorry, this presheaf on this abstract monoid. Right, just consider sets equipped with an action of this monoid, this abstract monoid. I think this is a fully faithful embedding.

Maybe put a topology on this that... Actually, no, because you need to ensure that this funny covering condition... Here, I mean that for any surjection in the category, in particular, you need to ask the sheaf condition. And this amounts to some conditions that are possible.

Right, so this functor that takes a topological space to... At least for today, I want to stress the other direction, because I'm also discussing something related.

So this has a left adjoint that takes any condensed set X and maps it to the underlying set (what you want to think of as the underlying set) and canonically equips it with a certain quotient topology.

So whenever you have anything that you want to think of as a continuous map from S to X, you in particular get a map from S (or rather, just the underlying set of S) to this X^*. This gives us a map from S^* to X^*. Take this union here and endow all of these with their natural topology as a condensed space, and this was an isomorphism.

Okay, so the image actually lands in... This will always be like "mildly compactly generated". What is this? This is for fixed S, and all X. Or if you want, you could just take the full subcategory, because everything is a presheaf on this.

So for topological spaces, there's a notion of a compactly generated topological space, which is one where, when you want to test whether a map is continuous, it's enough to test it on compact spaces mapping to it. And this is by definition the case for this X^*, because if you want to test continuity from here to somewhere, by definition of the quotient topology, you only have to test it from here.

So you only have to test continuity on these condensed sets, but these are actually metrizable. So it's actually in this sense "mildly compactly generated". I hope you can imagine what this should mean.

And conversely, if you start with a mildly compactly generated space, first treat it as a condensed set and then go back, you're precisely recovering the correct topological space, because basically exactly this condition here, and because the condensed objects anyway will come back.

In other words, there is any... Basically, any underlying... So in other words, the kind of unit of the adjunction map in this case.

So they have the... I mean generally, any X maps to this X^{**}, the unit of the adjunction. And on these guys, it's an isomorphism.

And so in particular, this means that these guys will form a full subcategory of all condensed sets. And it's... This is a very weak condition to be in this subcategory. I mean, virtually all the topological spaces that ever arise in nature... I mean, for example, we're mostly using these condensed sets in our, like, functional analysis, so to say, for topological modules. And any kind of Banach spaces, Fréchet spaces, whatever, they all have this property. They are all usable. So it's not so bad.

I wanted to make one more remark here about the relation to other similar tools that have been considered in the literature.

One is something that I think was quite influential. There's a paper of Johnstone called "On a topological topos", where he has a similar idea that, because topological spaces are not such a very well-behaved category, you should rather try to find a topos that is very well-behaved, which is very close to topological spaces.

So this is something that's achieved by this condensed sets. They form a topos, and one that is extremely closely related to topological spaces. And such things have been done before. One is Johnstone's topological topos. This is based on just the sequence spaces, so no larger profinite sets appear, only the sequence spaces.

And he uses a canonical topology. So on any category, there's a so-called "canonical topology", which is the finest one for which all the representable presheaves are sheaves. In general Infinity that are really just infinite covers, so it's an infinite collection of them that covers, but no finite subcollection will cover. But this actually leads to a Banach-Alaoglu property.

You could also just use a finitary one and then get a version of astosch where I think most of what does also works. And then actually there's one that extremely close to what we're doing. I think a paper by es, if I remember right. Basically what they're doing is they take like profinite sets, but only finite. This is a finitary topos, so that's nice, but they don't allow all continuous maps.

I'm not sure if I will come to it today, but it's really important for the good algebraic properties for the functors we want to do to allow all here. I might come to such a point today.

I think they make explicit what their stuff is in terms of this picture, but I think when you only allow the discrete, it is slightly better to make this exclusive.

So you said that being finitely generated is a really weak property. It gives some wellability, for example, in schwartz, or even weak sequential implies sequential. Sequential just means that this sequence space is enough to check, and even that is basically always satisfied. That's why I mean Johnson I think came up with the idea to just use this one, because usually it's actually too small to around with a version that uses even smaller spaces like this.

But in the end, it's actually quite important for us to keep the countable sets in, because if you want like a countable set that surjects onto any metric compact space, for example any like closed manifold or something, you can always cover it by countable. And this is actually important for us, that you can always find from like one profinite the whole thing. Otherwise we couldn't control the other at all, it would become infinite.

Right, so I have here something about this, but look not now.

Right, so maybe this is all I want to say right now about condensed sets, and as I said, for us their main importance is as a home for doing homological algebra. So let's talk about like abelian groups.

Recall like on any topos, abelian groups always form an abelian category. In particular, colimits exist, and there's a set of generators. In particular, this applies to condensed sets.

And so in particular, it's definitely abelian, and now something must have happened that like in topological abelian groups, we run into this issue that they are not at all abelian categories. But now we have abelian categories, so let me briefly discuss how that's possible.

Dustin mentioned the inclusions can be problematic. So for example, you might take $\mathbf{Q}$ inside $\mathbf{R}$, which is a natural topology, or even more drastically, you could have $\mathbf{R}$ a discrete topology inside $\mathbf{R}$ with its natural topology. So these are maps of topological abelian groups, perfectly nice ones, but where the cokernel is kind of problematic.

So let me briefly just compute these cokernels and abelian groups, or like what happens if you take underline. Well, first of all, a point definitely just quotients.

But more interestingly, what happens to give a condenser, we also have to give the values at any set S. And so like, you have to be slightly careful when you take quotients, because now the sheaf condition actually becomes important. And like the naive answer you might guess is that this should be continuous maps to the reals mod continuous maps from $\mathbf{Q}$, where $\mathbf{Q}$ is discrete. These are just locally constant.

And you can actually prove that you don't have to sheafify in this case, and this is already a sheaf, and this is the true answer. Okay, so this means that this quotient still kind of remembers something about how there was a topology on this guy, even if you can't really phrase it in terms of the topology itself in this guy. It still remembers that on this part you should take all the continuous maps, but on this part you should only allow the locally constant ones.

And so now let's even do the more drastic thing where you modify all of $\mathbf{R}$ by a discrete guy. Well, then the underline set is just zero, right? It's $\mathbf{R}/\mathbf{R}$, which is zero.

But there should be a value at Very much nonzero. So here's some controlling this funny thing by observing that it has nontrivial maps on general. So, say again... Andity, it's not... Sorry, I again meant to... The, yeah, think... I mean, the rational numbers are not at all embedded in the real numbers. Okay, so let me just state the theorem and then maybe prove it next.

So, part of this I already said. Condensed light condensed... In particular, the exact... But even better, countable limits. Countable products. And for the people that know this funny XM, that's Soal 86 XM inen Le's hope paper, which is about some funny way that products can affect the co-limits. And this is satisfied for products.

This is worse than in all condensed being GS, where all products are exact. Here it's just the countable ones, but I don't know, I mean, most of them really only take countable limits, so it's not so bad. But it's one reason that we were at first a bit hesitant to make this switch. It's also now not anymore... It doesn't have enough compact projective objects.

But one thing that's extremely nice: there is a free guy guys stop. So you can take the light profile set convergent sequence, and then you can always build a free condensed group on there. This turns out to be internally projective. And this is really a property that's extremely specific to the light setting.

Within all condensed being groups, we have plenty of projective objects, but none of them are internally projective, except trivial cases like $\mathbf{Z}$. But also, all of them are really, really big. I mean, they all come from extremely disconnected sets, so they are kind of impractical.

This one wouldn't be projective in condensed groups, but within light condensed groups, it just so happens to be projective and even internally projective. And so this is the only setting of any variant of condense theory in groups where I'm aware of any non-trivial object that's internally projective. And like, the free on a convergent sequence is actually kind of important as a very basic object in the theory. So it's really nice that within this setting, it has its good categorical properties. And it's one of the main reasons we made the switch to the light setting.

The forgetful functor, say, from light condensed groups to the underlying light cond... This has a left adjoint, the free functors on objects. Any light profile sets with free being group on that. I will discuss it more next time. In particular, inside here you have light profile sets while... And in particular, you can take on the light profile set and infinity fre, okay?

So I have this and then discuss some other things on... One questions? Can I ask a question about the... So you said you stated two facts about light case that are not true in the general case. This is a technical question to understand the contra in fact.

I found... So the one can give cont example using the interval from zero to the first uncountable ordinal, which is a forfinite set. And so for the second statement, I take this cross itself and then the closed subset, which is the first uncountable ordinal cross the set union, the set cross this last element, and then the whole thing, the overall product. This is not the retract of the whole product. And for the first thing, I take the complement of the last element, and this is a big open which is not disjoint union of Clen. This is not difficult to see.

On the other hand, you state that there are cases where the shift topology is nonzero. So just for a general interest, I want to know if this is true in this case, and what is the reference for this fact? The shift topology can be... No, if you take the first uncountable order, treat it as a profile set, and remove the limit point, this is a case where the open is not a disjoint union of profile sets. Doesn't high... I think it has.

But you said that people know that sometimes it has... I think you can definitely put examples where there is... You can also take the Stone compactification of integers and remove a part of the boundary. Also pretty, maybe less computable in general.

Is it still true that light top is... You take category stop this... Yeah, you know, the underlying set, right? So that's enough for face.

There no further questions, and let's stop here and we resume on Wednesday.

3. Light condensed sets II (Scholze)

https://www.youtube.com/watch?v=me1KNo3WJHE&list=PLx5f8IelFRgGmu6gmL-Kf_Rl_6Mm7juZO

Unfinished starting from 0:00

Okay, so let me recall a little bit where we were last time. Last time, we had this category of profinite sets. There are several ways to think about this: either as sequential limits of finite sets, or as metrizable totally disconnected compact Hausdorff spaces, or as countable Boolean algebras. We equip this with a Grothendieck topology where covers are generated by the following two families: finite disjoint unions and all surjective maps.

As I kind of stated last time, this has a very important consequence that I want to stress again: sequential limits of covers are still covers. This property will actually be extremely crucial in order to have a good resulting theory of something like topological abelian groups that we want to use to have good homological algebra properties. This is something I maybe want to stress today a little bit, where this appears.

Then we had this category of condensed sets, which were these sheaves for the Grothendieck topology on this category of profinite sets. In general, whenever you have sheaves on some site and the generating site embeds (well, at least if it's a subcanonical site), you have a Yoneda embedding. You have the profinite sets sitting inside there. This sends any profinite set S to the functor that takes any T to the maps from T to S, maps in profinite sets. If you prefer to think more concretely in terms of compact Hausdorff spaces, this is continuous maps.

One important property, which is also completely general, is that the image generates under colimits. You can think of general condensed sets as being built out of profinite sets by some kind of gluing procedure.

Then we also compared this to the category of topological spaces. In particular, we can embed this here as certain compact Hausdorff spaces. In fact, more generally, for any topological space A, you can build such a thing. I think I called a topological space A last time $\underline{A}$, which is given by the same procedure. You send any S to the continuous maps from S to A. So this diagram commutes.

As I also said last time, and I kind of want to improve on something I said last time, this $A \mapsto \underline{A}$ is left adjoint. It takes any condensed set X to, well, if you have a condensed set, in particular you have the value on a point, which we think of as the underlying set of this condensed set. Then we were equipping this with a certain topology.

Here we're taking the disjoint union over all possible maps from the countable set into X of the resulting map from the countable set onto X_{top}. The reason for the countable set being that it's any profinite set that admits a section from the countable set. So it's enough to allow the countable set here.

But something which I kind of forgot about and that Yok Morita reminded me after my lecture is that actually, you can describe this quotient topology differently. Because the countable set, I mean, convergence of the countable set can be detected by sequences. Or more precisely, if you map all possible convergent sequences to the countable set, it's still a quotient map.

So actually, you can also consider all, let's call this β, all convergent sequences in X of just the convergent sequence. This is also a quotient map in topological spaces.

So if you want, the countable set can actually be thought of as the colimit over all, say for example, countable closed subsets. So for example, finite unions of convergent sequences. This is in **Top**. It will not be true in condensed sets, and I will discuss this in a second. But it is true in **Top**.

This means that equivalently, you can describe this corresponding topological space as just the one where you just remember what the convergent sequences are. That's enough to determine this topology. In particular, this means that this funny notion that I was talking about of being compactly generated is actually just the same thing as being sequential. So that continuity can be checked by checking whether convergent sequences go to convergent sequences.

And so the thing I said last time, someone, more succinctly, is saying that sequential topological spaces embed into light condensed sets. Okay.

Okay, so from this perspective it seems that allowing the countable set didn't really help at all. But now I want to say why we allow the countable set. So is it the same or more general than first countable topological spaces? I forgot all my general topology. Does somebody know, is sequential the same as first countable? It could be, discrete on huge, but if the greatness of first countable... Because in the previous version of the theory it was first countable, I forgot, I think first countable with all points closed. But now we don't need all points closed because we don't have the set theory, right?

I mean, why allow the countable set? We're frozen, I'm frozen. I hope I'm un-frozen.

So the reason is that in any topos, for example, whenever you have sheaves on any site, there is an extrinsic notion of being compact and of being Hausdorff. So these are the following notions that I want to recall. Frozen again, frozen again, why? Oh my God.

So this general topos notion is what's called quasicompact. If any cover admits a finite subcover, here this general notion would amount to saying that there exists a surjection from the countable set onto X, or X is empty. Yes, thanks.

And then there is this intrinsic notion of being quasiseparated, of being Hausdorff. And this notion was introduced by Grothendieck in SGA4. And of course, Grothendieck was using algebraic geometry lingo, so instead of Hausdorff he said separated. And because it's just a general topos notion, and not the specific separated notion in algebraic geometry, he called this quasiseparated.

So in general this would say that for all quasicompact, well maybe I'm assuming here something about enough quasicompact objects, but there are enough quasicompact objects. And for all quasicompact Y and Z mapping to X, so we hope that the technical problems got fixed, let's see, the fiber product between the two is quasicompact.

And again, let me simplify this here again, because one can always surject onto any quasicompact guy by something by the countable set. For all two maps from the countable set onto X, the fiber product, I should have chosen notation for the countable set, is still quasicompact.

So this I'm basically trying to say that, sorry, need not be surjective, just a map, that if you map a countable set into X, then it may not be injective. So this might factor over some quotient of the countable set. But you're somehow declaring here that it's the quotient by a closed equivalence relation on the countable set, which is one way to express some kind of Hausdorffness.

Okay, so in any topos you can talk about these quasicompact and the quasiseparated objects. And at least if your topology is finitary, the quasicompact guys are exactly those where like a finite union of the generating objects maps onto it.

So if we only allowed, for example, the convergent sequence as our basic guy in the test category, and the quasicompact objects would all be quotients of this guy, or at least be countable. So in particular, the countable set would not at all be quasicompact. And if you want to access the countable set, it would be precisely by such a colimit, that would be this huge colimit of smaller objects.

And this would mean that the intuition for what a compact object is, like the countable set is very much a compact object and you want that, this would be destroyed if you passed to such a topos.

And in fact, I mean, with our notion of what the generating objects are, you in fact get this really nice proposition that, by the way, quasicompact I very often abbreviate to "QC". I mean, in principle I hate abbreviations, but okay, this is QC and this is QS. And if both of them are assumed, then these are called "QQS".

So you can wonder, what are the compact Hausdorff, so to say, light condensed sets?

So this means that the topos, abstract topos theoretic notion of quasicompactness really very closely mirrors the idea of what a compact space is. And similarly, the general topos theoretic notion of what Hausdorff means mirrors precisely what you think Hausdorff should mean. And so this forces our Grothendieck topology to be finitary, because otherwise the basic objects wouldn't be quasicompact. And it forces us to include the countable set in the formalism.

Then you can also wonder more generally, if it drops the compactness hypothesis but still requires a Hausdorff condition, what are those things? So what are the quasiseparated condensed sets? Those also can be described in more standard terms.

So we know that the metrizable compact Hausdorff spaces are allowed. And in general, you should think of these guys as being rising unions of quasicompact subsets. In fact, they are exactly the ind-category, where all the transition maps are closed immersions of metrizable compact spaces. Let me write injections—I really mean all maps are injective, in which case they're automatically closed immersions of metrizable compact spaces. In the ind-category. So last time I was talking about the pro-category, which were formal inverse systems, and these are formal directed systems. So there is a category, where the objects are functors from a filtered poset towards metrizable compact Hausdorff spaces with all the maps injective. And functors, I mean maps, are the usual thing on ind-objects.

Yeah, so you could also drop the Hausdorff here and then drop this here. And let me just point out that within this category you have what topologists call the compactly generated spaces. Probably meaning sequential. But note that they are not the same, because in this category, as I said previously, you can write the countable set as a huge colimit of countable closed subsets, but not in the category of quasiseparated condensed sets.

I mean, these are both condensed sets. This is the prototypical example of an ind-system of metrizable compact Hausdorff spaces. This is also an ind-system where you just have one term, it's actually some quasicompact quasiseparated guy. But as condensed sets, they are very different. Because this is really just a formal colimit, and this is just one object. In particular, this guy is quasicompact and this one is not.

But actually, if you ask for ind-systems that are countable, then on such things, these all come from here. So some of the difference between these two categories only comes when the colimit category is pretty large, as it is in this case.

So I don't understand the subtlety. Is there a condition on—so if you take the condensed set which is the direct limit of the injections of the countable unions of images of the sequence space, does this give something by the equivalence of categories which is in this category MCG? It's not an equivalence of categories, right? This is just a full inclusion.

What is included in that? Ah okay, this one is included. Okay, I did not realize that this is what you were saying.

All right. So let me just say something that's implicit. We said that something very nice, like the interval, is definitely a nice metrizable compact Hausdorff space. We said it should be quasicompact as a condensed set. And I said that quasicompact means that there must be a surjection from the countable set. In fact, that is true. You can find a surjection from the countable set, and in fact some kind of canonical one.

I mean, if you have a sequence $a_0, a_1, a_2, \ldots$ of either zeros or ones, you can send this to the number in binary $0.a_0a_1a_2\ldots$, the binary expansion, which is an element in the interval. And any point in the interval admits such a representation. So you get the surjective map.

But we definitely need the countable set, or something as large as a countable set, to surject onto here. And so you can then recover this whole guy as a certain closed equivalence relation here, where the equivalence relation is precisely this nasty thing that $0.111\ldots = 1.000\ldots$. All right Okay, so we're mainly interested in this formalism of condensed sets as a framework for doing some kind of algebra where all the objects have something like a topology.

To get this started, let's talk about condensed abelian groups. There are actually two ways to think about this, which I didn't say last time. Either these are abelian group objects in condensed sets, or these are sheaves on this category of condensed sets with values in abelian groups. Let me just write in symbols: sheaves on Cond with values in Ab. Similar remarks apply to any kind of algebraic structure. If you have rings, for example, you can view them as ring objects in condensed sets or as sheaves of rings.

From the general theory of sheaves and Grothendieck topologies, we know that this is an abelian category, in fact a Grothendieck abelian category. So it has filtered colimits, etc. It also has a tensor product. Let me describe a little bit what the properties are.

The unit object is just the condensed set $\underline{\mathbf{Z}}$ associated to the integers as a discrete set. If you have two condensed abelian groups M and N, then $M \otimes N$ is the sheafification of the presheaf that sends a condensed set S to the tensor product $M(S) \otimes N(S)$. This is just a functor from condensed sets to abelian groups, a "presheaf", and then you can always sheafify.

There's a further property that I want to stress, which is also completely general. If you have a condensed abelian group, then you can forget its abelian group structure and just have an underlying condensed set. But this has a left adjoint, a kind of free construction. A "free condensed abelian group"—let me drop the "condensed" when I say that now—takes any condensed set X to the free abelian group on X. Here, as in general for these colimit-type constructions like tensor products and left adjoints, you always have to sheafify. So it's the sheafification of the presheaf that takes any S to the free abelian group on $X(S)$.

There is already some structure here that you don't often think about in topological abelian groups. You don't really often take the free group on a topological space. Here it exists, and it's actually a completely fundamental structure.

The idea is that this free abelian group on X is some topological abelian group. What is its underlying group? It's just the free abelian group on the underlying set $X(*)$. So if you have any kind of topological space, you can just take its free abelian group. Of course, this machinery will then put some kind of topology on the free abelian group.

Let me actually discuss this in an example. Sheafification won't change the value on the point, because any cover of the point is split, so the sheaf condition is kind of vacuous on the point.

Here's an example, which is kind of related to the introduction I gave last time. We can take the real numbers $\underline{\mathbf{R}}$. Very soon I will forget to write the underline all the time, because implicitly everything has become a condensed set. But okay, so we have the real numbers as a condensed set, and then we can take the free condensed abelian group on that.

What kind of object is this? It's sums, if you want, of real numbers x weighted by integers n_x, where the n_x are almost all zero. You might think of these points x as measures, and then these are finite sums of measures.

But now it also has some topology, where you kind of remember that these x's are allowed to move continuously. But then, if you have $x + y$ in general, that's a non-zero element. However, when x and y become the same, then suddenly this collapses.

It's maybe not so clear how you would actually describe this topologically. If you wanted to describe this as a topological abelian group, you would have to declare what the open subsets are, and I think that's a little bit tricky to visualize.

But you can actually say what it is as a condensed set. First

Again, the $C_0(\mathbf{Z})$ itself can be written as a rising union over all integers n of subsets where you are only allowing sums $\sum_{i\in I} n_{x_i}[x_i]$ where the sum of the absolute values of the n_{x_i}'s is at most n. Everything is contained in something like this, as condensed sets.

So whenever you have a profinite set mapping into here, it will actually factor over one of these subsets. And these guys, they are compact Hausdorff and metrizable. It's a kind of fun exercise to figure out how to describe such a compact Hausdorff space.

I claim that whenever you have any compact Hausdorff space and you look at finite integral sums of points of them where the sum of the coefficients is at most n, there is a canonical compact Hausdorff topology on that. Classically, this takes a little bit of thinking. But in this formalism, this free construction just produces it for you automatically.

The subtle part is that there are some kind of non-trivial identifications you have to make. In general, $x + y$ is a non-zero element, but when they become equal, you have to collapse this to zero. To make this true, you actually have to use that in our Grothendieck topology, we didn't just allow finite disjoint unions but also effective epimorphisms. Otherwise, this wouldn't come out right.

Also, by general nonsense, if you start with something that already had a group structure, then if you pass to the free ring, this now has a ring structure where the multiplication comes from the addition. In particular, this is actually a condensed ring completely naturally. This is kind of related to the question I had in my first lecture, like how do you draw an element in all its real powers? It's just done by this construction.

These are general features. Now I want to mention a few features that are quite specific to this light condensed setting. The first thing is that countable products are exact. This might seem like an operation you're maybe not so often doing, but something you are certainly very often doing when you do some kind of functional analysis is to take sequential limits of surjective maps, and they are still surjective.

For example, in functional analysis, maybe you have some kind of Fréchet space and it's a sequential limit of Banach spaces. Maybe in that case it's not even surjective, but it will also come out right. You definitely want sequential limits to behave nicely. For example, if all the transition maps are surjective, you definitely want the limit to still be surjective. As I will argue in a second, this more or less forces you to go where your basic objects that define your site must be some kind of totally disconnected things.

You have these, and there is this other property that I will explain in a second. You have the sequence space $\mathbf{Z}^{\mathbf{N}}$, it's a profinite set or light condensed set, and you take the free ring on that. This turns out to be internally projective.

Recall that one way to say what projective means is that in any abelian category, you can define Ext groups. It means that $\mathrm{Ext}^i(P, -) = 0$ for $i > 0$. Internally projective makes sense when your category also has a tensor product, because if you have a tensor product, then you can define an internal Ext, and internally projective means that the internal $\mathrm{Ext}^i(P, -) = 0$ for $i > 0$.

The first two properties are actually things that are better in all condensed abelian groups, because all products are exact. Well okay, the third property is also solely still true. However, the third property is something that's only true in the light setting.

Let me just define the internal Ext. Basically, if you have a tensor product, then you can also ask for an internal Hom, which is some kind of adjoint of the tensor product. Similarly, the internal Ext will be some version of the same thing on Ext groups.

Okay, so let me prove this actually. Let me first note that one reduces to showing that taking products of surjections is surjective. The only thing that's not true for general products is that a product of injective maps is always injective and so on. The only thing that's not clear is that the countable product of surjections

For all M, you can take the product over all n at most M of M_n, and then the product over N bigger than M of N_n, and this surjects onto the product of the N_n's. Now because a finite product is the same thing as a finite direct sum, they always preserve surjective maps. They're always exact, and so you can always, like, for finitely many coordinates, kind of do the lift. But then this guy here, I mean, this map is the limit now over M of these things.

And so if we're asking whether a countable limit of surjections is still surjective, let's assume you have such a diagram. We have M_0, M_1, M_2, and M_∞ is the limit of these, which certainly maps to M_0. We ask ourselves whether this is surjective. So what does it mean to be surjective in the sense of sheaves?

This means that whenever you have one of your objects generating your site, so any light profinite set, and a map from here, then, well, if it was a surjection as presheaves, you should immediately be able to lift that to here. But in fact, it's enough to find a surjective map from some—let's call it S_∞—and a lift to here. So does there exist some other light profinite set surjecting onto S and a lift to M_∞? That's the question. That's what surjectivity on coverings amounts to.

But let's just see what happens. We definitely, well, right, so this is this map to M_0. But we know, because this map is surjective, we know that there is a light profinite set and a lift to here. And then again, because this map is surjective, there exists some further light profinite set and a lift to here.

And so now you can just take S to be the limit of this diagram. So inductively, you construct light profinite sets with a map to here, and then take the countable limit of these surjective maps. As I said last time, and I think I recalled today, countable limits of surjections are still surjections in light profinite sets. Hence, this guy is still allowed as a cover in our Grothendieck topology.

So in the definition of your X group in the sheafification, you drop the underline and the left p. So $\mathbf{Z}_p$ s, you mean $\mathbf{Z}_p$ s underline? Yeah, let me drop the underlines. Okay, so S is for me a light profinite set, and they sit inside of light condensed sets. So yeah, if you feel better, make an underline, I mean, yeah. And like, the Yoneda embedding has no decoration for me, it's just the same thing. But yeah, also in three, like, this threefold guy, it's like, maybe I should also underline 10 here.

Okay, so before I go on to the proof of three, let me reflect a little bit on what happened here. This finishes one and two, and let me come back to that in a second.

So here, the critical thing—we definitely, for doing good homological algebra, like a functional analysis kind of homological algebra, you definitely want property two. But critical for two was exactly this property that countable limits of surjective maps are still surjective, that limits of covers in your Grothendieck topology are covers.

And I claim that this basically forces you to use totally disconnected spaces as building blocks. Why? You might also have the idea, and I think people are doing that, that if you want some kind of nice category of something like topological real vector spaces, you might work in this kind of smooth setting where you take your defining site to be like smooth manifolds, and the Grothendieck topology just the usual one of open covers of smooth manifolds.

But in that case, this property that countable limits of covers are covers is just not true. Because if you have, like, maybe just the real line, then you can cover it by two intervals. Each one of them you can again cover by two intervals, and then keep doing that. But then the intervals shr

Disconnected compact Hausdorff spaces. And initially, we took all of them. We realized it's slightly better to restrict to symmetrizable ones and to have all surjective maps as covers. Because you can actually show that any surjective map of topological spaces can be written as a sequential limit of actually open covers. So if you want open covers and their sequential limits, you need all of them.

Actually, I should maybe say that this is not some kind of ahistorical comment. This pro-étale topology was, in fact, first—I mean, so this stuff about condensed sets, this comes from something called the pro-étale topology, like originally in Bhatt-Scholze for schemes. And there, the wish was precisely that limits of surjective maps should still be surjective. Because this was—we wanted to have certain sheaves which were naturally certain inverse limits, and we want them to be well-behaved. And for this reason, we allowed these countable limits, or all limits of covers, to still be covers. So this is really the origin of this whole theory.

All right, so this was a small interlude. Excuse me. So if you have a surjective map of topological spaces, for example, you take the $\mathbf{N} \cup \{\infty\}$, two copies of $\mathbf{N} \cup \{\infty\}$. So all of the two sequences will converge to infinity. And then this is covered by two copies of $\mathbf{N} \cup \{\infty\}$, and they intersect at infinity.

You made a statement to the effect that—yeah, well, okay, not all transition maps are surjective. But actually, you know, I mean, actually something slightly stronger is true. I don't need all the transition maps to be surjective. I only need that the maps down to S are surjective, and the limit is still okay. And in that sense, you can realize S as okay in this other sense.

Okay, something slightly weaker would be—yeah, I'm not sure. I would have to think how much difference it makes.

Okay, so I want to prove this thing that this is internally projective. But before I do that, let me make a warning that this is a phenomenon that's only true once you pass to groups. So $\mathbf{N} \cup \{\infty\}$ is not at all projective in condensed sets.

And I actually kind of expected Gab wanted to go there with this remark. Because this is precisely the example that I need to do. So you can have a surjective map of topological spaces and the convergent sequence downstairs, so that there does not exist a lift. And one example for this would be to just take this to be the convergent sequence itself, and this to be like, I don't know, $2\mathbf{N} \cup \{\infty\}$ disjoint union $2\mathbf{N}+1 \cup \{\infty\}$. Some breakup of $\{\infty\}$ as like the limit of the even guys and the odd guys.

Then like on the integers, you only have one possible lift. But the even guards converge to something else than the odd guards. But miraculously, once you pass to groups—it so happens that if these were condensed abelian groups, in fact you don't—then any convergent sequence can be lifted.

All right, let me actually use a slightly different guy. So let M be the free group on the null sequence. So you can take $\mathbf{N} \cup \{\infty\}$ and mod out by $\{\infty\}$, which is actually a direct factor, right? Because you have $\{\infty\}$ mapping here and then projecting to a point. So this splits, actually has a direct summand.

And so, and of course, the integers themselves, they are projective. That's okay. So the question is really whether this other part—so this classifies null sequences mapping out of M, is the same thing as giving a null sequence in the other guy. Or in other words, it's a free condensed abelian group on a null sequence. And free group on a convergent sequence, but then the limit point should be zero.

So if we want that this guy is internally projective—and let me actually focus on the projectivity, and then just The property that ∞ goes to 0... Because this is a surjective map of condensed groups, this means that there is some surjective map from a profinite set that lifts to $\mathbf{N}$. That's just what the surjectivity means.

Okay, now how do covers of $\mathbf{N} \cup \{\infty\}$ look like? $\mathbf{N} \cup \{\infty\}$ is like you have a discrete set and then it accumulates towards infinity. Now, in the pullback, you have some complicated S upstairs here. But you can always make that smaller because, for each of the discrete points here, you can just pick any lift. I don't care which one, and then all these lifts together with keeping everything at ∞, this is a closed subspace. So there exists S', a closed subspace, so that over the integers, it's just always a point.

You can assume that this S here is somehow a different compactification of $\mathbf{N}$. Without loss of generality, we can always make S smaller here, as long as it's surjective. So we can assume that S, the part over any finite thing, is just a point. Okay, but there are many compactifications of the natural numbers. For example, this one, so we can't expect that we can directly split that.

But now we actually use a property of profinite sets that I mentioned last time. Okay, so let S_∞ be the fiber over ∞. This might be profinite, so there's a certain subspace in this, but everything here is profinite. In particular, S_∞ is profinite. I stress it here because here it's really the critical point where profiniteness is used. This means that it's injective in profinite sets. In all profinite sets, it's also non-empty. And so there exists a retraction r from S to S_∞.

Okay, so let me call this inclusion here i. Maybe this was my original map f and this was a map g. Now we can just write down the lift. Consider the following map from S to $\mathbf{N}^{\sim}$, which is the map g that we had. But then from it, you subtract what you would get if you first use the retraction and then re-embed into S and then apply g.

So if you look at S_∞, then on S_∞, these two maps are just the same because this was a retraction. Am I frozen again? So on S_∞, it's actually just the constant map 0. Was that a question? Am I frozen again? Do you see me now? Yeah, okay.

But S surjects onto $\mathbf{N} \cup \{\infty\}$ and this map to ∞. Because this is actually a surjective map here, you can show that this is a pushout in profinite condensed sets. That's basically a version of this gluing condition that I gave. If I want to give a continuous map from here to somewhere, it's enough to give one here and here which agree on the overlap. But it's definitely enough to give one here, and then the question is just when does it descend down to $\mathbf{N} \cup \{\infty\}$? This means that on fibers it must be constant, but there's only one fiber in some sense where there's something to check, which is the fiber at infinity. And there we precisely assume that it all just comes from a point.

Okay, but now you're precisely in this situation. You have a map from S, you have the zero map, and they agree on S_∞. So this means that actually this whole map factors over a map from $\mathbf{N} \cup \{\infty\}$ to here. Let me call that map f.

Okay, and now we basically have all the structure we need, and we just need to check that we actually produce the lift. So what do we have now? Now we have a map from $\mathbf{N} \cup \{\infty\}$ to $\mathbf{N}^{\sim}$ which vanishes on ∞. So in other words Because this was a retraction, and so this means that, because of this pushout property, you really get an exact sequence. You have a map from the convergent sequence modulo infinity to N.

Now the question is whether we've actually lifted f, and the claim is that we did. To check that we did, note that this guy here is still surjected on by the free guy on S. So to see that this map here is f, it's enough to check for the composite.

The point is that if I take this thing that I used here as a correction term and I project that down to N, then this map from S to N vanishes on all of S_∞. This term projects to zero in N, because its composite map from S and N vanishes on S_∞. So the correction term indeed vanishes, and g was a lift, so we're fine.

For internal projectivity, let me just not do it. It's essentially the same argument, you just have an auxiliary profinite set floating around that you also need to cover a bit. Again, it's critical that sequential limits of surjections are surjective.

Here's just one remark about the comparison to all condensed abelian groups. This is something that I already said a bit earlier, but just to say it more explicitly:

In all condensed abelian groups, products are exact and you have projective generators $\mathbf{Z}[S]$ where S is what's called an extremely disconnected set. For example, it's a Stone-Čech compactification of some discrete set. These are huge things, they have cardinality $2^{2^{\aleph_0}}$.

One thing that's better there is that you really have projective generators. You don't have projective generators in light condensed groups. You have this one guy which is the free guy on a null sequence, but the free guy on a countable set cannot be covered by a projective object. This is slightly unfortunate, but actually not that bad in practice.

The good thing is that in light condensed groups, you have this really explicit projective object which is the free guy on the $\mathbf{N}$ sequence. Whereas here, these projective guys exist by the axiom of choice, but they are completely inexplicit and their precise structure depends on which model of set theory you're working with.

But one thing I want to point out is that none of those projective generators there are internally projective. Basically, if you take a product of two Stone-Čech compactifications, and I and J are infinite, this product is never projective. That's actually not so easy to see, but we can prove it. This is a rather severe technical issue in the category of all condensed abelian groups. It's extremely nice that you have these projective guys, but for many arguments you would really like to know that they are internally projective, and they are not. That's bad.

In light condensed groups, at least we have this one really nice internally projective object. That's good.

The other thing I wanted to point out explicitly is that the free guy on the $\mathbf{N}$ sequence is not projective in all condensed abelian groups. The light ones embed into the category of all condensed abelian groups, and you could ask if it's still projective there. But there, precisely the Stone-Čech nonsense gives the obstruction.

When some universal compactification of the integers, the biggest one, is the Stone-Čech compactification, this rejects. You can show that there does not exist a splitting here.

Maybe another thing I should say is that this is also related to certain questions about Banach spaces that people have studied in literature. There, the only known injective Banach spaces—there's some kind of duality that what used to be projective in this condensed stuff will become injective in the category of Banach spaces—are continuous functions from some S into $\mathbf{R}$, where S is one of these extremely disconnected guys which I could have also allowed. Basically, such a Stone-Čech compactification or a retract of it.

It's known that all injective Banach spaces are retracts of continuous functions on a Stone-Čech compactification.

What about injectives? In your case, it is for light condensed abelian groups. Is it enough injective? For all condensed abelian groups, there are no nonzero injectives. In all condensed abelian groups, there are no non-zero injective objects.

Okay, yeah, they exist for set-theoretic reasons. I mean, for like general nonsense reasons in light condensed abelian groups. But I don't think you can write any of them down.

And what corresponds to this free abelian group sequence is the Banach space of null sequences. This is not injective as a Banach space. But it is what's known as separably injective, where you only test this injectivity against separable Banach spaces. This is very much related to the thing that this guy is not projective in condensed abelian groups, but it is in light condensed abelian groups.

Actually, I think I made this realization that it is projective in light condensed abelian groups after looking up this proof that this guy is separably injective. It was an open question in the Banach space literature whether the continuous functions on this guy is a separably injected Banach space. In the notes on complex geometry, Kan kind of proves that it's not separably injective. In particular, this guy also doesn't behave like a projective object, even when you test against light condensed abelian groups.

Anyways, maybe one thing to take away here is that you might think that all this totally disconnected nonsense shouldn't really appear when you do function analysis over the reals. But actually, people that studied Banach spaces intensively, they are very much studying continuous functions on totally disconnected things.

Okay, right. Yeah, maybe let me finish by talking about homological algebra. You also said something, I forgot in which context, about the axioms AB5 and AB6 of abelian categories.

In Grothendieck's Tohoku paper, you can find a lot of axioms that an abelian category might or might not satisfy. AB5 is the question of whether all products are exact. This is true in all condensed abelian groups. It's not true in light ones, but at least the countable ones are okay. When AB5 is satisfied, you can ask about AB6, which is a certain question about the commutation of infinite products with filtered colimits. This always sounds confusing, but you can actually make a statement that's true.

This property, AB6, is always true if you have enough projective generators. In particular, it's true in condensed abelian groups. If you restrict this question to countable products, then it's also true in light condensed abelian groups. So AB6 holds for countable products. One way to see this is that you have this fully faithful embedding of light condensed abelian groups into all condensed abelian groups. This commutes with all colimits and countable limits. In particular, these countable products are some of the correct products there, the ones that you would also compute in condensed abelian groups. But you can also just check it by hand.

In the last bit, I want to talk about cohomology. When you study topological spaces, you probably also care about the cohomology, in particular for manifolds where you would want that to be the singular cohomology. When you think about a topological space, we already have an idea of what the cohomology should be, just as we already had an idea of what a complex thing should be like, and so forth. But on the other hand, whenever you work in a topos, that topos somehow comes with its preconceived notions of not only what compact objects are, but also what cohomology is.

If X is any condensed set and M is any abelian group (it could even be a condensed abelian group, but let's restrict to discrete ones for the moment), you can define the cohomology of X with coefficients in M. This is just something that is there whenever you work in the topos.

One way to define the cohomology of X with integral coefficients, in terms of the formalism that I now introduced: You take R Hom in the category of X-groups in light condensed abelian groups (actually they would also be the same as in all condensed abelian groups, oh well, I'm doing light stuff so let me stick

So, um, if say X is a CW complex, this thing is $X_\bullet$ or it's underlined. It's exactly the singular cohomology. This might seem a little bit weird because we didn't really put any geometry into the definition of these condensed sets, right? We were just using totally disconnected things, and suddenly using totally disconnected things, we are still able to probe whether that circle is not contractible. Um, but it comes out right.

Um, and, um, right, uh, so let me actually recall that. I mean, yeah, let me make one remark about this and then stop. So as I said, this is here the $\mathbf{Z}$ of these $X_\bullet$ groups, and I told you how to think about this, right? So this was, uh, the thing where you take finite sums of X-points of X valued by integers. Um, and so this is actually—there's a Dold-Thom theorem which basically tells you that this guy is something like a model for the homology of X once you, once you pass, once you treat this up to homotopy equivalence. This kind of guy is a model for the homology of X. Uh, and, well, for us the interval in condensed sets is like an actual interval, it's not a point. So we didn't yet pass to homotopy, uh, but one way in which you're doing that is by taking X^s out of it, because an interval cannot map to something, uh, this being discrete. And so, uh, yeah, so this dualizing, I mean this is like homology, so the dual should be like cohomology, which fits the picture. Uh, and yeah, let me just, uh, stop here.

Other questions? Uh, all right, so we can also consider the internal Ext-functor. Um, in this case, this would just unravel to an adjunction to, uh, also replacing M by the continuous functions from S to M, which is still an abelian group, so it doesn't really do that much more. Um, uh, maybe one comment to make is that, and maybe I won't talk much about it, but, um, for all sorts of things like continuous group cohomology and so on, sometimes it's not quite clear, like, what is a continuous representation of a group, what is the right notion of continuous group cohomology? Because often this is just represented by some explicit cochain complex, uh, but if you just treat your topological groups as condensed abelian groups, uh, then it's clear what an action on, like, a condensed abelian group should be, and it's clear what cohomology should be. There's some kind of general 2-categorical answer to what it should be, and this always gives you the expected answer. It's not always the same thing as the thing computed by continuous cochains, but when it's not, it's a better answer.

So, so here, when you, when you have a CW complex, if you have more generally a local system, I think it will give a sheaf on condensed $X_\bullet$ over X-underline, and then you can say that the usual cohomology, it's like singular sheaf cohomology. It satisfies cohomological descent, for, for at least for compact Hausdorff spaces, it satisfies cohomological descent of surjections of compact Hausdorff spaces. So to compare the two sides, it looks like using the theory of cohomological descent to, to, well, essentially to do the, should give, to do it with local systems. By, yeah, you can, you can use any coefficient system, really, on X-underline. This is, yeah, it's a really robust result.

Um, but if you, for example, work in, um, like, this topos of sequential spaces, where you only allow $\mathbf{N} \cup \{\infty\}$ as a generating object, then if you would try to compute this cohomology of, like, the interval, then you would first have to express your interval in terms of your generating object. So we would write this as this huge colimit of all these, uh, small countable sub-closed subsets in the interval, and then this C

4. Ext computations in (light) condensed abelian groups (Scholze)

https://www.youtube.com/watch?v=EW39K0J7Hqo&list=PLx5f8IelFRgGmu6gmL-Kf_Rl_6Mm7juZO

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All right, I think that's a signal to start. Good morning, welcome back. So today I want to talk about some EXT computations in condensed mathematics, and then some...

So I guess the basic claim is that this category of condensed abelian groups is a very convenient framework for doing homological algebra. It's a very nice abelian category. You can, without any problem, form a derived category. And because topological spaces essentially embed faithfully into condensed sets, topological abelian groups also basically embed faithfully (under some mild assumptions).

So now you have a nice home to play with them, but then because you have an abelian category, you can ask about EXT groups, and then you can wonder what they actually are. And for this theory to be somewhat useful, you want all the answers of these EXT computations to give you reasonable answers that are interpretable.

So the first theorem that I stated last time is the following theorem. One thing you can do is start with, for example, some nice geometric object like a CW complex X. And let's say M is an abelian group. Then one can look at the EXT groups in condensed abelian groups between the free condensed abelian group on X (or rather, on the corresponding condensed set), and M treated as a discrete abelian group.

Basically, if you treat X as a condensed set, then in any topos there is this internal notion of cohomology, which one way to express is as EXT groups in the topos. So this is the internal notion of cohomology of X with coefficients in M in the condensed world. And fortunately, it turns out that this precisely recovers the singular cohomology of X with coefficients in M. Okay.

So the first thing I probably want to do today is sketch a proof of this theorem. Maybe I should say that to a large part, in this lecture and maybe also the next one or two, I will still follow the first course that I gave on condensed mathematics some years ago. You can find lecture notes online, so for much of what I'm talking about today and last time, a reference is this "cond.pdf" that you can find on my webpage.

Okay, so proof sketch is that, well, as X is a CW complex, it's an increasing union of some X_i, where the X_i are compact. It's filtrable, built from finite dimensional cells. And then on both sides, you take colimits (direct limits). Yeah, you probably need a slightly better statement there to really compare the complexes, not just individual groups. But basically, you can reduce to the case that you just have a compact space X, a compact CW complex.

Then there's actually a more general statement that works for any compact Hausdorff space X that may not be a CW complex built from cells. That's the Čech theorem. Again, you still have these EXT groups between the free abelian group on X and M. It turns out that in this case, you can always compute this as what's known as Čech cohomology on X with coefficients in M.

So here, Čech cohomology is defined in terms of the category of sheaves of abelian groups on X. You consider a topological space X (or the corresponding site), and abelian sheaves on X. Then you have a global sections functor from abelian sheaves to abelian groups (I'll write this as $H^0(X, -)$ to distinguish it from other notation). You apply this to the constant sheaf M on X, and take derived functors.

It is known that Čech cohomology, when restricted to CW complexes, satisfies the Eilenberg-Steenrod axioms and agrees with singular cohomology. So for X a CW complex, the two notions coincide. But not in general - in fact, singular cohomology is really only valid for CW complexes. In general you should use something like Čech cohomology (or really the same thing).

So here's a key example to keep in mind, of relevance to us: If X is totally disconnected, then you can actually show that the global sections functor $H^0(X, -)$ is already exact. So in this case, Čech cohomology vanishes in positive degrees. Okay. So here is a proposed correction of the transcript:

Compact, I mean... Yeah, I'm still... Sorry, yeah, the prof fin set. These are the global sections and the global sections are just locally constant maps from X to $\mathbf{N}$, equal to zero, and there's no higher cohomology.

But if you would consider the singular cohomology, this is defined in terms of the singular chain complex, which is built from just mapping points and simplices into X. But from the perspective of all simplices, there's no way to tell apart X from just a discrete set of points, right? Because simply connected... So any connected space will just factor over one point.

But when you... So you can also compute the singular cohomology of $\mathbf{N}$, and again it's $\mathbf{R}$ in degree zero, but in degree zero you get all functions.

Yes, continue. So this means that thinking about cohomology doesn't really see anything about the topology of X anymore. The sheaf cohomology does.

I think another possibility is to consider locally contractible spaces. I believe that when you cross X with an interval, this sheaf cohomology group should not change. I'm not completely sure what it looks like, and then one can do for locally contractible spaces a comparison with sheaf cohomology. And if in addition it is paracompact, then you get comparison to singular cohomology. But the usual results on...

Yes, yes, right. Yeah, I think the key statement you really need about X is that it's locally contractible in this funny sense. "Locally contractible" doesn't mean it's covered by opens, or that any point has a basis of open neighborhoods that are contractible. But rather that for any point and any neighborhood, you can find a smaller, possibly smaller neighborhood that can be contracted in the larger one. And under this funny assumption, that's the official definition of locally contractible.

You can actually show that yeah, I think everything that I said about CW complexes extends to this. But to compare sheaf and singular cohomology, the usual treatment requires paracompactness. I'm not sure if it is... So yeah, maybe paracompactness...

Yeah, I think these paracompactness assumptions, they would kind of only matter in intermediate... If you go all the way back to here, I think it probably disappears again.

Yes, the question... So the question was, often one considers topological spaces up to weak equivalence. And then obviously any space is weakly equivalent to a CW complex, and for example X is weakly equivalent to just a discrete set of points. But here I don't want to consider topological spaces up to weak equivalence, because otherwise I wouldn't be able to treat totally disconnected spaces at all. I'm not using here topological spaces as a way towards pure homotopy theory, I'm really interested in the actual topology.

Um, right. So okay, so here is what I want to prove. If I have any abelian group, then I can compare these sheaf cohomology groups. And this can actually be further upgraded, and I mean, I'm not doing this just for fun, actually...

So here's a first upgrade you can do. Sheaves were defined... So there are two sides. On the one hand, you can fix X, let's call it the topos of X. So this is what's used on the right-hand side. And on the other hand, you can consider the category of slices of the topos, defining light contravariant set-valued functors. You can consider light profile sets together with a map to X.

And once you pass to sheaves, or once you pass to the corresponding topos, it turns out that that's actually a geometric morphism here. So... These here are light sets with a map to X, and these are X, and there is a map here.

So what I'm telling you is that whenever you have a sheaf over X, you can pull it back to get a light functor on contravariant sets over X. And to define what the pullback is, you really only have to define it on those generating objects. So $\mathcal{L}$ of U, when U is an open subset of X, is just U.

Okay, so here's the usual sheaves on X, and you can pull them back to get light sets over X. And correspondingly, you also have the topos of sheaves on

This means that X is a sheaf group, which are some kind of forms. In particular, for all $\mathcal{F}$ which are abelian groups on X, the cohomology on the site of condensed sets over this coefficients in $\mathcal{F}$ is the same thing as the cohomology. If you apply this to a constant sheaf, you recover the previous result. This is for X locally compact.

The kind of X I was having in mind is the case where X is a compact Hausdorff space, probably profinite would be good. So if we apply this to the constant sheaf on the abelian group, we get the previous result.

Maybe this all sounds a bit scary, but the point is just the sheaf is defined in terms of cohomology on one side, and the other is basically also just cohomology on the other side. The original claim was that for a certain very specific sheaf, namely a constant sheaf, they have the same cohomology. But actually something much more robust is true: that any sheaf $\mathcal{F}$ which can be represented in terms of such a functor is fully faithful.

Okay, so let me sketch. We need that on any object, the pushforward of $\mathcal{B}$-cohomology sheaves on X, the adjunction map from $\mathcal{A}$ to the pushforward of the pullback of $\mathcal{A}$ is an isomorphism. This is a certain map in $\mathcal{D}^+(X)$, and then just a general fact that you can check whether such a thing is an isomorphism by checking it on stalks. This is really the key point where I'm kind of using the topos-theoretic formalism in order to make a reduction to checking something on stalks.

We want that a_x maps isomorphically to—oh, right, we wonder what this maps isomorphically to. Now the key point is that you can actually have suitable base change properties that allow you to pull taking the stalk into this pushforward operation.

The key base change property, taking stalks at x, uses the following: this is where you actually use something about the nature of the stalks, namely you use a general cohomology result for taking the stalk of some kind of $\mathcal{F}$ and taking cohomology of some kind of formula. There are certain statements when you can interchange them. Such base change results hold in the generality of what's called coherent topoi.

Coherent topoi is just basically the same thing as being quasi-compact and quasi-separated. So on the end, what you see is that the key thing you really need at this point is that this condensed set corresponding to X, in this internal language of topoi, means to be quasi-compact and quasi-separated. This was precisely the thing I mentioned last time: that if you want to do this, for example, for the interval, you need to know that there is a surjection from one of the generating objects, the contractible sets, towards the interval.

So implicitly here you're using that you can cover something like an interval by contractible sets. Sorry, yes, thank you. Why did you use the boundedness assumption? Because otherwise this statement doesn't hold true in general. If you don't have things that are bounded to the left, there's always some issue of how cohomology changes with cosic limits, and there are all sorts of questions.

Basically, there are at least three versions of which way you can do this. There's Lurie's version, which may be the best one, and then there is just the rough category of sheaves, and then there is the left completion. If you want the functor to be fully faithful here, you should always take the left completion.

All of this matters if X is not locally compact, or if it's five-dimensional, then everything's the same. But this kind of general statement, that it always holds for any Grothendieck topos, only works when you're bounded to the left. Otherwise there are some issues. I believe you want to regard the point as a limit of its closed neighborhoods rather than open neighborhoods, yes, exactly.

So if you actually want to execute what I gave here as a hint, then you actually have to use that. I mean, this is cofinal with all open neighborhoods of x, but when you're in a compact Sure, I'll help clarify this transcript. Here's the revised version with added punctuation, capitalization, and paragraph breaks:

You can take the closed neighborhoods here, which has the advantage that if you pull back a closed neighborhood, you actually get some profinite set. You want to stay within the realm of proper, compact, separated objects, for which you need to take a closed neighborhood. But you can just do that.

So, we can pull in—taking the stack X, this basically means that we've reduced to the same statement, but now the space X has to just become a point. Let's do this. Okay, but if it's a point, then all these are just abelian groups. Well, this is still something—abelian groups embed fully faithfully in there.

This basically means the integers, as a profinite group, are still projective. So the X—yeah, maybe you have to explain, but it's obvious that this $Hom(S, lim M_i)$, with this topology—you have to use that these are in between the open and closed neighborhoods. So if you pull back to open neighborhoods, but then the transition maps, if you have two open neighborhoods and their intersection, then the intersection on the closed neighborhoods sits in between. So it doesn't matter which one you use.

Let me just pause a second and let me try to understand what happened here more completely. So we're interested in computing $X(S, G)$ for some X, and we're interested in computing, well, let's just say, in arbitrary degree. Then we would like—now I took a very fancy approach, but we could try to do it more down-to-earth. How would you actually try to compute $X(S, G)$ in abelian groups? You would try to find a projective resolution, and if you cannot find one that's projective, at least you would try to find one that's a free resolution.

This actually breaks the proof in two steps. Step one is to show that if X is actually totally disconnected, then you don't actually have to do anything. So in this case, $X(S, G)$—in this case it's one of these generating objects in our site, it is in this case some profinite set S. The continuous maps from S to G are equal to $G(S)$ in degree 0 and nothing in positive degree.

Okay, so for S being, for example, the profinite completion of the integers, this really just follows from the statement from last time that this is a projective object. But in general this is not true, and for the general case you still have to resolve this S. But for—so this comes down to the following:

If there was some, for example, some $H^1(S,G)$, you could always split this H^1 after you pass to a cover of S by what it means that it's injective. Similarly, if there's any higher $H^i(S,G)$, you can always split it after passing to some kind of infinite resolution of this S. So concretely, what this amounts to is that for hypercovers—so this is a simplicial object in profinite sets—there's S_0, S_1, and so on.

Concretely, this means that S_0 is a cover of S, and then you can recover S as a quotient by this fiber product $S_0 \times_S S_0$. But then the next one S_1 is required to surject onto the fiber product, and so on. Here, it takes a little bit of effort in writing.

So whenever you have such a hypercover, there's actually some completely general thing that in any site, if you have an object with a hypercover, then you can build the corresponding chain complex. This is actually always exact, and you need to see that when you dualize and pretend that this should be the answer you would want, if I now pass to the corresponding complex of continuous functions, that this is still exact. This is not automatic, this is what you have to prove.

So we need to show for all hypercovers—the exactness is automatic, what we need to show is that the corresponding complex of continuous maps is also exact. There are several ways to prove this. One is to use the same argument I used, which is to argue that in order to prove this exactness, you can treat everything here as a sheaf on S, because instead of taking global

Stalls, and then again when you pass to stalks, you realize the same thing. Where now you're covering a point they have to cover a point, but then again, because it's a point, their cover—there's nothing.

So that's one way. The other way is to prove something, some abstract lemma, that whenever you have a hypercover of profinite sets by profinite sets, you can always write this as some cofiltered limit of hypercovers of finite sets by finite sets. And then this writes this thing as some filtered colimit of corresponding things where everything is a finite set. But then for finite sets also, hypercovers are split and the exactness is automatic.

I think that the latter argument is the one I actually used in the lecture. The first argument is like cohomological descent in SGA 4, and I think that they also check—I forgot in which reference, but in one of the proper base change theorems for separated, proper maps. And then the arguments in cohomological descent go through, and it will give this. The cohomological descent spectral sequence will give you this result. Right?

Yeah, I guess the previous thing on the right was, I guess, it's called Mor. Either that, or you have a colimit, and it actually takes a little bit of unwrapping that you can always do this, but it's okay, to reduce to the case of finite sets where it's obvious.

Okay, so that's the first step if you try to do this more concretely, but maybe also the less interesting step, because we're still just treating these totally disconnected spaces. And now, step two is to treat general spaces.

And so we would again like to find such an acyclic resolution, and now we actually found a lot of acyclics, because now we know that for these connected guys at least this is all right. So now it's enough to find a projective resolution, but one by such joins.

So we want to resolve $\mathbf{Z}$ on X, but on $SS^{-1}S$ is lol. And so how does one actually do that? But one uses precisely that—one uses that you can always find the surjection from the constant set, so from some profinite set on to X. And then you get some equivalence relation $S_0 \times_{SS} \mathbf{Z}$. And this actually, I mean, if S_0 is already totally disconnected, then $S_0 \times S_0$ is, and this is a closed subspace if you take the fiber product. So this is actually always totally disconnected. You don't even have to do any further resolution.

So you can take Čech's nerve, where this is S_0, this is S_{-1}, this is S_{-2}. In general, these S_i are just some $i+1$-fold fiber product. We have some hypercover, in fact Čech cover, of X.

And so again, by the general principle that hypercovers give you resolutions, you now get such a resolution of $\mathbf{Z}$ on X by these three guys. This is our resolution by things that are Čech for functions.

And so this tells us that, if you're interested in $R\Gamma(X,\mathbf{Z})$, this can be computed by taking these guys. And now we know that this guy here is computed by this complex, where you take here the continuous functions from S_0 to $\mathbf{Z}$. Which is some really awkward formula for something like singular cohomology.

So you take your nice base X, maybe the interval, cover it by some constant set, and then take all the fiber products. And then everywhere you take just the locally constant functions to the integers, and build this cochain complex. And the claim is that this computes $R\Gamma(X,\mathbf{Z})$.

Now, it's not so clear a priori that it really computes the right thing. What the previous proof amounts to is to check that, to again treat all of these things here as sheaves on X, so as global sections of some sheaves on X. And then to check that this computes the right thing, you can again somehow compute on stalks, and then you're done. And then we check that it resolves the constant sheaf.

Sorry, sir So what are some examples of objects in here? Well, also the $\mathbf{Z}$ are in there, and then I don't know, the real numbers $\mathbf{R}$ are there, or something like the real numbers modulo the integers $\mathbf{R}/\mathbf{Z}$, or the p-adic numbers are in there. Also something nice like the adeles, which are the restricted product of the completions of the integers.

One nice thing about the adeles is that they sit in a short exact sequence where you have $\mathbf{Q} \hookrightarrow \mathbb{A}_{\mathbf{Q}} \twoheadrightarrow \mathbb{A}_{\mathbf{Q}}/\mathbf{Q}$, and $\mathbb{A}_{\mathbf{Q}}/\mathbf{Q}$ is compact. So there is some kind of structure theorem for these guys.

Each object can be broken up into three pieces: one piece is the discrete piece, one piece is a finite-dimensional real vector space, and one piece is a compact piece. As you see, there are some kind of interesting exact sequences in there. So you definitely expect that there is some kind of Ext^1 group, like $\mathrm{Ext}^1(\mathbf{R}/\mathbf{Z}, \mathbf{Z})$, which classifies the extensions of real numbers by integers, something like this.

Two things that one knows about this category: it seems to be like an exact category, and computing Yoneda Ext from the category, one knows that all the $\mathrm{Ext}^{\geq 2}$ groups are equal to zero for these objects. I don't know about Ext^1 groups.

So you can wonder whether something similar holds true now if you compute Ext groups inside the category of condensed abelian groups. Okay, so let's take any two locally compact abelian groups which are also metrizable. Then, again, this metrizable assumption can be ignored; it only comes from the restriction to metrizable condensed abelian groups. Then you can compute the Ext groups as condensed groups from the corresponding guys as topological groups.

I mean, just from the fully faithfulness, you already know that the Hom groups, they can't be changed; they must just be the usual topological homomorphisms. But a priori, there could be some weird Ext^1 groups that you didn't know about. Here you're computing Ext groups within the whole category of condensed abelian groups, and there could be some really weird extensions between them. Actually, later, maybe hopefully today, I'll give an example where this actually happens.

But here it turns out that's not the case. All the Ext groups, all the extensions of a locally compact group by a locally compact group in condensed abelian groups, they all are themselves extensions of locally compact abelian groups. You can really identify the abstract Ext group with the usual thing, where we're looking at short exact sequences $B \to X \to A \to 0$, so exact sequences in locally compact abelian groups up to the appropriate notion of isomorphism.

Let me give some key examples. Actually, something slightly better: you could even consider this. So first of all, you can compute Ext groups of anything against the circle group $S^1 = \mathbf{R}/\mathbf{Z}$. This is actually $\mathrm{Hom}(-, S^1)$, so this is Hom as condensed groups, and it's precisely the Pontryagin dual group as a compact group.

Or another example, one could also try to compute some Ext groups like $\mathrm{Ext}^\bullet(\mathbf{R}, \mathbf{Z})$, where some of the intuition is that the real numbers are something connected, so they can never map to the integers which are totally disconnected. So you would expect all these Ext groups to be zero, and indeed, if I remember correctly, that's true.

I think actually these are more or less the key examples. To understand this computation, what you have to do is, you can do a devissage where A and B can be reduced to these basic cases. So for example, if you map from something discrete, then there's not really anything to show, because then the Ext groups are easy to compute. So you can assume basically you have a compact abelian group

Algebraic geometry to do some computations. But the statement we needed was never actually put in by Grauert. There is an unpublished letter of Thom to Grauert, where he proves the result, but it's some unpublished—but it's a very nice theorem. Let me use $\mathbb{B}$ for this.

There is a resolution of the form, something functorial in abelian groups S. So what is a resolution? You're trying to resolve any abelian group M, and you're trying to find some kind of universal projective resolution of this. They're trying to resolve by free abelian groups, and there's of course a very easy way to at least find a projection onto them. You send some of the generators, given by some element of M, M as an element in here, some the finite free abelian group generated by the elements of M, right? But of course, this is way, way bigger than this.

So the standard way actually to do this is to do some kind of monadic resolution, where you use a free abelian group monad. And then there is some kind of general thing that the next term—I mean, you might put here, and this would be (it's not what I want to do)—you could put as a free abelian group on the free abelian group on M, and then two maps here, and then the difference you use here. And then you can continue, but then these things get uncontrollably large. So this is not what you want to do.

And you realize that actually, you don't need something as big as this to generate the kernel. Because really, the only thing you really have to enforce is that when you add two elements of M, then they become the same as the sum, right? So basically, whenever you have a pair of elements of M, so here generators are certain pairs (a, b), you can send that to $a + b$, $a + b$. And it's easy to check that this actually generates the kernel of this map, because once you prescribe those relations, then you can uniquely sum any such thing. And you realize you used to just ignore time.

And then you can continue. So each term will just be $\mathbf{Z}$ joined with $\mathbf{Z}^M$ to some power. And there are some transition maps that are given by some universal formulas like this, except nobody's able to write them down.

Do you have to take a finite direct sum or in each term of such powers? Or is it enough to have one power? Like when we originally wrote this up, we used to find some of these things. But you can actually, by some stupid argument, you can basically cover any such finite direct sum again by such a free guy. Okay, I see, I see. You will actually realize that there's a small issue with a zero, but you will figure it out.

You can just choose one term in each degree. And so all the differentials are given as some universal formulas, which actually have functoriality. All the differentials are universal. So it's a little bit of a mathematical result.

And surprisingly, the proof of the theorem uses a little bit of stable homotopy theory. So in some incarnation, it uses something like the finite stable homotopy groups. And that they appear in the proof is also the reason that you don't really know how to do this explicitly. Because at some point, you need to basically kill something like stable homotopy groups, $\pi_n^s(S^k)$, using surjections from finite free groups onto them. And you can do that, but you won't get anything.

But one very nice thing is that this is really functorial. And so this means that this immediately works in any topos. In any topos, you can write down the same complex, the same universal formulas. But whenever you have a sheaf of abelian groups, you can write down the same complex as sheaves of abelian groups on that side, and it will automatically be exact.

So functoriality, or really the universal formulas, also works for sheaves. And so we now get a resolution of our A that we're interested in, where now all the terms are some $\mathbf{Z}[A]$ or $A^{\square n}$ or some such. And so in order to compute X from here, there's some reduction to computing Ext Only did the case where the target was the $\mathbb{S}^1$ group for the $\mathbb{S}^1$. I also need a case where the target are the real numbers.

Yeah, so if you look back at what I did in these notes on p-adics, then there are also proofs that for any compact Hausdorff space X, you can compute some of the X groups of $\mathbf{Z}$ (join X into $\mathbf{R}$) as also needed for all X to compute X, so from all the X for X, but now with real coefficients. And then the claim is that this actually doesn't have any higher cohomology, whatever X is. And $\mathbf{Z}$ of course, it just gets a continuous $\mathbf{R}$.

This also works with $\mathbf{R}$ replaced by any Banach space, but it doesn't work if it's just $\mathbf{R}$. But it really uses local convexity because there are some partition of unity arguments in the proof. You know, for the partition of unity to behave nicely, you need that the target Banach space is locally convex.

Sorry, that's important. So as a preview of something that will happen later: when we consider real vector spaces in $\mathbf{R}$, we will actually have to consider non-locally convex cases. And so we will actually really be interested in situations of such computations where comp, and then we do not have something on all comp spaces. And then this means that we will actually have to resolve. So when we want to resolve by a simplex, we really have to go to totally disconnected.

All right, so let me give an example of how such computation will come out. So when you're trying to compute the X groups of the reals against integers, then X is–no I think that's right, it's a shift from the free on X.

All right, if you want to $\mathbf{R}$, you just have to map into $\mathbf{R}$.

Yeah, thanks. So when you computed, when you compute that, then you're resolving this by the $\mathbf{R}$ on $\mathbf{R}$ on $\mathbf{R}$'s and finite vector spaces, and then we know–I mean, finite vector spaces, that's definitely a CW complex. So we know that the X groups, you can see, um, sorry there's some $2i$'s here, that this is, well, it's the same thing as a singular cohomology $\mathbf{R}$. And so this is of course $\mathbf{Z}$ in degree 0 and 0 otherwise.

And so this means that when you compute the X groups out of this, then each term will just give you one copy of $\mathbf{Z}$. You would like this to be 0 for all i greater than 0. You might be worried that there's some more lots of $\mathbf{Z}$'s remaining, and maybe you don't know what the differentials are. But one way to control this is to observe that you can do a stupid thing of also using this resolution for $\mathbf{Z}/2\mathbf{Z}$, which is also 0 by several properties of the integers.

And then you realize that when you compute X out of this sequence, it's the same thing as computing X out of here, because each term individually has the same X groups. And so then out of here is the same as out of here here, but this just results 0.

Okay, so this is how one can leverage this knowledge about the X groups from these free guys on reasonable things into such X groups from locally compact groups.

Let me actually mention the variant of this argument. So one might be worried that it's kind of weird trying to do very explicit computations by using an inexplicit resolution. But some of the inexplicit nature of resolution somehow never becomes an issue. Just the existence of such a resolution, its properties is enough. But actually there is a resolution that is explicit and that can also be used. This is something known as the Eilenberg-MacLane or Q-construction, which was rediscovered in the process of this formalization as well.

So this is an explicit complex. It starts just like we expect.

Do this on the other face too. So you take $-a$, c, $-b$, $a+b$. I hope you can check that if you compose the two differentials, you get zero. If not, then there is some easy variant of this that should work.

Now you can imagine how you do this one step up. Imagine $\mathbf{N}^8$ is where the eight elements sit on the vertices of a cube. Then you take this side minus this side, and this side plus this side, minus the sum of the sides.

Then there is a theorem that this is kind of linear. More precisely, $Q(M)$ is always quasi-isomorphic to $Q(\mathbf{Z}) \otimes_{\mathbf{Z}} M$. In particular, if you look at all the homology groups, everything is free. This just means that the homology of this cubic construction is just a linear functor. In general, there's some extra 2-torsion.

Yeah, this linearity in M turns out to be kind of sufficient. So $X_i(A, B) = 0$ for all i if and only if all the EX groups from this explicit complex vanish. Basically, all we are trying to prove is that they can be put into the form that some EX group should vanish for all $i \geq 0$. Then we can use this explicit resolution of A. I mean, it's not quite a resolution of A, but for this purpose, it's good enough.

Another thing you can actually compute is what this thing is, using some stable homotopy theory. You can show that it is actually $\bigoplus_{i\geq 0}(\mathbf{Z}[S^i])^{\oplus 2^i}$ shifted into degree $-i$. So this is where a little bit of stable homotopy theory comes in. Basically, whenever you write down something explicit, the explicit answer should have something to do with stable homotopy groups of spheres.

Okay, this is just to say that instead of an explicit resolution, you can also do all of these arguments with this explicit complex instead. It doesn't really change any of the arguments, except you have a little bit more comfort that you actually know which objects you are dealing with.

Just to clarify, it's not direct. I think it actually is a direct summand. You think no? Yes, I think the statement is that there are Q and Q', so there is actually a modification that kills something extra, and then you just get this summand. But then you are removing the extra stuff.

All right, so finally I can say one corollary that's really important for the theory of condensed abelian groups. One thing you can now compute is an EX group of something that's not at all locally compact anymore, but actually quite big. You can take the whole product of a countable number of copies of the integers, so the profinite integers. Or if you want, you can take the product of p-adic integers.

It turns out that these EX groups are actually just a direct sum of countably many copies of $\mathbf{Z}$. This is very different from the classical answer, where if EX was the naive dual of a countable product of copies of $\mathbf{F}_p$, which is a profinite group, now we are looking at some continuous things, and we just get the direct sum. The really critical thing is that there's nothing in higher degrees.

So this guy will be the compact projective generator of solid $\mathbf{Z}$-modules, which is a full subcategory of condensed abelian groups. All the solid $\mathbf{Z}$-modules should be solid, and then if you want it to be projective, you definitely want all the higher EX groups to vanish.

Okay, so let's prove that. This actually uses a weird trick. Naively, you would now again try to resolve this by free guys on profinite sets, but the issue is that this is a really large thing. Here's a warning: if you treat this here as a condensed set, then this is a union over all functions $f : \mathbf{N} \to \mathbf{N}$ of the product over $n \in \mathbf{N}$ of the interval from $-f$

And so this approach would actually be extremely hard to execute, but there's a trick you can do. You can resolve now in the other direction, which seems weird. You can embed this into a product of copies of the real numbers. This is a similar thing.

At this point, we crucially use that products are exact in condensed mathematics. It might be easier to justify this specific case. Okay, so this is exact.

And now we're trying to compute X against—so this maps the X from here into the X from here, and the X from here. Let's do this one first. This is actually a compact Hausdorff space that is totally disconnected. It's still profinite, right? So it's a countable product of profinite groups.

And so we know what the X groups are. The X^0 is a product of $\mathbf{Z}$ copies. It turns out that this is, well, this is discrete and this is something connected, so there shouldn't be any maps. And then it turns out that in degree one, the computation will show that this is just a direct sum of copies of $\mathbf{Q}/\mathbf{Z}$.

Nothing else. This is what the result of the previous lemma refers to. Then if you write the long exact sequence, you realize that what you need is that the X^2 groups from this product of copies of the real numbers is equal to zero for $i \geq 2$.

And now, still in the same kind of situation, we do some kind of huge diagram chase. And now there are two ways to finish the argument. One way is you can observe that if you do a similar operation now with this guy, then each of the terms here becomes a product of intervals. So each of the guys becomes an H^n-cube, and actually you can show that also for an H^n-cube it behaves like the highest degree—this comparison with singular cohomology is also true for the H^n-cube. There's no higher cohomology, so the argument we gave for the real numbers works also for this H^n-cube variant.

Oh, there's a slightly different way of arguing using a little bit of adjunctions. The source here is some condensed module, it has a module structure over the real numbers. And so this means that maps from the product into any module M will always be the same thing as the internal Hom in condensed modules over this condensed ring $\mathbf{R}$, from this guy into M, since internal Hom and external Hom agree here by this general adjunction.

But now this internal Hom is already zero by the result for $\mathbf{R}$. And so the whole thing vanishes. So we don't really need to know exactly what this is, it's enough to know that it's some module over the real numbers. And then because the real numbers don't map into any module was real analytic functions.

Okay, okay, so that's one of the key computations that we needed, which kind of gives me—so I have maybe five minutes left. Let me talk about a fun theorem with some set theoretic stuff.

So here's a theorem. All right, let me consider the following conditions, consider the following assertion $(*)$:

For all sequential limits $M_0 \to M_1 \to \cdots$ of countable condensed abelian groups, and all possibly non-abelian condensed sets N, the X^i groups from the sequential limit of $\underline{\mathrm{Hom}}(M_n, N)$ towards n, this is the colimit of $X^i(\underline{\mathrm{Hom}}(M_n, N))$. Sorry, and of course this vanishes at least for $i \geq 2$, because these are just X^i groups and condensed abelian groups, and X^i groups in condensed abelian groups agree for $i \leq 1$.

So they vanish at least for $i \geq 2$. And then the colimit also, it's actually equivalent to the following statement: that if I take the X^i group of the product

So first of all, it's not just that this is excluded by $\star$. In fact, $\star$ implies that the continuum must be really large; $2^{\aleph_0}$ must be bigger than $\aleph_\omega$.

Basically what happens, I think, is that if $2^{\aleph_0}$ is some $\aleph_n$ for some finite n, then you will get some Ext^n problems. But if you make it larger than all n, then you have a chance.

In fact, it cannot actually be equal to $\aleph_\omega$ by some GCH. What I proved is that actually the smallest bigger thing is consistent: $\star$ holds and $2^{\aleph_0} = \aleph_{\omega+1}$. You can also—the first possibility after this can be realized.

In fact, it holds whenever you have any ground model. Then you can extend this ground model by doing a Cohen forcing; it holds in the forcing extension.

By joining $\aleph_\omega$-many reals, Cohen invented this notion of forcing that takes one model of set theory and builds another one, a bigger one, in order to show that the continuum hypothesis may be false. This is like the most basic forcing. I mean, now they have billions of different types of forcing, but this is still the most basic one, where you're just adjoining new real numbers, so to say, to your model. And you're adjoining quite a lot of them; $\aleph_\omega$-many would be the minimal thing that you can do in order to have a chance.

$\aleph_\omega$-many, but once you join that many Cohen reals to your model, the simplest kind of forcing is done, which will ensure that this is true. It turns out, always.

Okay, and why do I mention this? Well, we don't actually ever in this course—I will never use this principle $\star$. But it's kind of neat to know that you can use it. Often when you try to compute certain things, it's easy to figure out what the answer would be if this was true, and then the things you really need, you can usually prove them without invoking this general principle.

But there are also some situations where you might want it. For example, in order to control Ext groups out of Banach spaces, where it really is the case that you get the expected answer under this principle $\star$.

But in general, these Ext groups are just some...

Question from audience: Why is it $\aleph_\omega$ and not $\aleph_1$? I mean, as I said in the first lecture, it comes from having the smallest possible .

Another audience question: I have a basic question. When you said that, um, when you computed the Ext groups for $\mathbf{T}$ locally compact, and then you said that this is zero for $i > 1$, right? You said just because you ...

Lecturer: I don't know, I mean, I'm sorry, I didn't want to refer to–there's no simple reason. You actually have to do a computation. It comes out as zero, but in the end it's nice, it matches what ...

Audience: Thank you. I didn't catch the previous answer, but I want to ask again on this computation of Ext. So for example, if you have a compact abelian group and you take the Ext^i to the reals, via the condensed formalism, you have a complex that computes it where the terms will be continuous functions on various powers of this compact group to the reals, right? You have to compute that this is acyclic in higher degree. So I don't see exactly how you do it. For the group cohomology complex, you have this averaging by integrals, but I don't know exactly what is ...

Lecturer: I didn't actually look it up in preparation for this. How does it work? So instead of mapping to $\mathbf{R}$, it might also be that you want to ... How does it work? No, so it's different. You use that, um ... Where's–just go–I mean, I can't think right now, but you can find it in the notes.

I don't see any further questions, so let's–so in the set-theoretical setting, what was the colimit on the blackboard you're now looking at? What is the X_θ? This is the colimit of what?

So he writes this thing as this huge colimit of all functions, and then you can write, indexed by the same index category, where all the terms are now something you can write down. And then it's precisely these δ-limits that they are studying.

Okay, I understand now, because we know that there is a cohomological dimension result for Alf^n. I see how it's related, because if I think there were all $\dim_{\mathbf{Q}}$ functions–suppose that of eventual dominance–then under, for example, continuum hypothesis, this would be the same as ω_1.

And then such X-groups indexed by ω_1 should be bad. But the specific order type of this poset of functions ordered by eventual dominance depends extremely much on the specific model.

5. Solid abelian groups (Scholze)

https://www.youtube.com/watch?v=bdQ-_CZ5tl8&list=PLx5f8IelFRgGmu6gmL-Kf_Rl_6Mm7juZO

Unfinished starting from 0:00

Right, so today I want to talk about solid abelian groups. The goal is to isolate a class of intuitively speaking "complete" objects. The point being that in the previous lecture, we've seen that when we work in the category Cond(Ab) and start with reasonable examples, then the outcome is also reasonable. But when you instead do some free construction, for example you form some tensor product—I don't know, you take one power series algebra and tensor it with another power series algebra in abelian groups—then ideally speaking, there should be some kind of completeness, where this comes out as complete in both variables. But if you just naively form the standard condensed abelian groups, then the underlying object is still just the algebraic tensor product of these things, which is some nasty indescribable thing. Product-like condensed groups are not so nice.

So the idea is that you would want to define a subclass of complete objects, and then you would like to complete things, and hope to get a reasonable answer instead. Idea being, there should be some notion of completeness for condensed abelian groups. But we definitely also still want that complete objects form as nice a category as condensed abelian groups themselves, they should still be an abelian category.

You definitely want something like the integers to maybe be complete, and maybe the p-adic numbers should be complete. But then if you want this to be an abelian category, you also want some kind of extension of p-adic numbers by the integers to be complete. But this is a very non-Hausdorff thing. So you definitely can't phrase completeness as meaning convergent sequences have a unique limit. First of all, it's not really possible to say what a convergent sequence is, other than one that already has a specified limit point, in this condensed setting. Abstractly, "convergent sequence" isn't really a notion. But also, even if there was, you couldn't ask that they have unique limits, because you want these non-Hausdorff examples.

Okay, and so back in the day when we were first thinking about this stuff, this was really one of the key questions for us: how to define such a notion of completeness. Originally we wanted one fixed notion, where in particular the real numbers should be complete, the p-adic numbers should be complete, maybe all locally compact abelian groups should be complete. And still something where you can form cokernels and it's stable.

In the end, we couldn't directly make that work. But we realized that if we for the moment forget about the real numbers and just want something non-archimedean, then there is something that works quite nicely. So it turns out it's difficult to find a notion where the real numbers are complete, but there is a theory that works well for all non-archimedean fields.

Later we will recover the real numbers, but that's a different story. Today I want to talk about a theory that works well in the non-archimedean context. Originally we did things very differently, so today I actually want to give a presentation of the theory of solid abelian groups that's very different from the one you will find in the first lecture notes. It's a presentation that really only works in this nice way in the condensed setting.

Okay, but in the condensed setting, one can base everything on the following idea: being complete in some sense means any null sequence is summable. One basic nice fact in non-archimedean analysis is that sequences are summable if and only if they go to zero. You can just try to turn that into a definition. It turns out this is actually a definition that makes very good sense. So let me try to indicate how you formalize this idea.

Here's a formalization of the idea. Let me consider this projective object that we had: $\underline{\mathbf{Z}}^{\mathbf{N}}$, the constant sequence. We want to say something about convergent sequences, so this should play a role somewhere. Recall that this is an internally projective object. Let me recall what this means.

One way to say what this means is if you look at the internal Hom functor into any condensed abelian group, ...

And what does it do? It takes some life Beilinson group, sends to life Beilinson group. That is, the one that takes any life problem to the internal hom from P. So the underlying, so if we evaluate this at a point, and I just get hom from P2M, but then I've enriched this back into the solid Abelian groups. And then the thing is that this functor is actually exact, but also preserves all limits and colimits. Can you find all that? But actually these are compact objects, right? Okay.

Okay, so this will become important in a second. And now we want to phrase the condition that any null sequence is summable. And one way to do is the following. So let me write something down. I consider the endomorphism F, which is the identity minus the shift map, which is an endomorphism of P. So complete P has a basis generated by some $\exp(n)$, and we make $\exp(n)$. So this is the basic space of null sequences m_0, m_1, and so on. And what does F do? If you just translate the formula there, it just sends such a null sequence to a new null sequence, which is $m_0 - m_1, m_1 - m_2$, and so on.

And what would the inverse be? The inverse should send a null sequence here–well, you should be able to recover m_0 as $m_0 - m_1$ plus $m_1 - m_2$ plus $m_2 - m_3$ and so on, by telescoping, right? And so if there is an inverse, the inverse has to be S, which takes a null sequence and produces a new sequence where the first term is just the sum of all of them.

So this is one way to phrase the condition that any null sequence in M is summable. It's a very structured way of saying this, because maybe to say that, you would only have to say it on the actual hom, on the internal hom. But it turns out to be much better if you ask the condition on the internal hom.

Okay, and so the goal of today's lecture is to understand this C(Solid Ab). And in the original approach, proving that this is an Abelian category was actually the last thing one could really prove in this live setting. It's actually the first thing one can prove.

If you consider the subcategory of Solid Ab, it's actually a reflective subcategory, stable under internal colimits like tensor products, all limits and colimits, even internal homs, and internal exact sequences if you want. This also contains some objects–it contains the integers. And once it contains the integers, everything you can build by limits and colimits and kernels and whatnot will contain all this. It's not everything.

It contains the real algebraic numbers. It does not contain the reals, because of course the reals are just wrong. Okay, I understand. Because there is, yes. So it's definitely current.

And so Jaron, when you introduce later this proposition, it basically tells you that this is an analytic ring structure on the integers here. Okay.

And so previously, I mean, the way we set up the theory previously, it was a bit of an art to construct analytic ring structures, because there were a lot of things you have to check, and none of them were easy to guarantee. And so there were basically only two examples one could construct, which is the solid ring structure on the integers, and then there is this liquid ring structure on the reals and some related rings. Those proofs were pretty hard, and they were really very handcrafted things. But it turns out that because of this internal reproductivity of P, it's now trivial to construct analytic ring structures, because for any endomorphism of P, you can ask such a condition, and all this will come for free.

I mean, yeah, so let's just try to prove that for example, it's stable under kernels. Say you have a map $M \to M'$ where this happens, and then you want to know that for the kernel it also happens. But the internal hom from P is an exact operation, so it just preserves the kernel. So of course the same condition is true for the kernel, the same for the cokernel. Or if you You have stability under retracts and finite limits automatically in an abelian category. This limit is also clear, and this colimit also, because just the internal Hom has these properties. So it's all for free.

What about internal Hom from X to internal X? This looks more difficult. It's also trivial, no? Because internal Hom you can also pull over, right? I mean, just by adjunction. Ah, but then you need only internal Hom on the second... I mean, you need only... Okay, let's try Hom, because maybe I didn't... I just wrote that down on the board and I didn't write in my notes.

Something guaranteed. Okay, so here is stability under... Let's first do the internal Hom, then see whether it works for the internal tensor product. Let's say M is solid and let's say actually any N. What's the internal Hom from N to M?

Okay, I see it, because you can permute... Write it down. See, so what is this? For this, we have to prove something about this guy theater. But by the Hom-tensor adjunction, that's also the internal Hom from $- \otimes N$ to M. But then, if M is solid, you can commute the two Homs and it becomes internal Hom from N into the internal Hom from $-$ to M. But then, if you come as M here... So of course, another fun still.

And if you look at internal tensor product, then because internal Hom from $-$ is exact, you also have that the internal Hom from $-$ into the internal will also just come out as the internal tensor product from the completed tensor product. And then again, the internal tensor product from N internal Hom...

Okay, and so... Yeah, so I mean, this is something extremely... that when you just enforce such an "is solid" condition on the internal Hom from $-$, you get these very... Okay, so everything is for free, except possibly that there is any object that satisfies it. But okay, so you can just compute that the internal Hom from $\mathbf{Z}^{(\mathbf{N})}$ into the integers is just a direct sum of copies of the integers. Because if you have a null sequence in the integers, then it must be eventually zero because they are discrete.

Okay, it takes a little bit to see that it's really internally Hom, but that's true. Again by adjunction, if you want. And yeah, so if you just compute what identity on $\mathbf{N}$ here... So you want to show that any null sequence is summable. But any null sequence is eventually zero, so it's easy to sum it.

Okay, before actually focusing more on this specific example of like completeness condition, let me draw some... It's good to know. So just by some abstract adjoint fun, this means that there... this inclusion here... will typically have a left adjoint, some kind of completion. So fun that takes any abelian group to a solid one. This is what we call solidification. And so, this is characterized by the property that for all solid N, what do we have... that for all solid ones, the Hom from N to M is the same thing as the Hom... The completion does make it the right adjoint, as opposed to the left? It's the left adjoint to the inclusion, because I'm using the inclusion. So I should write some kind of fun here, which is inclusion fun. Because this... No no no, my question is that... So but you see that both are stable under all colimits. It also has a right adjoint, which I've never considered. Yes, but there is some... also some small solid sub, I guess.

Okay, but over solid objects... requires need product... product... making the solidification. So concretely, this is just if you have two solid abelian groups and you want to form the tensor product, you first form the tensor product inside the abelian groups and then pass to solidification again. There's a little bit of unraveling to show that this is really some half-tor operation. But the key thing that you need for this is that the class of solid modules is stable under pullback. And...

Okay. Is this required to be symmetric? No.

Symmetric. Let me just... So the existence of solidification is just an instance of trying. Basically, if you want to have a left adjoint, it should commute with all limits. Then, under very mild assumptions, it also exists and they are satisfied here.

For the symmetric monoidal structure, well, we define I to just be the constant sheaf. Then we want the symmetric monoidal structure. So what we want is that for all M and N, which are just condensed abelian groups, first we individually solidify them, then tensor them back in the category of condensed abelian groups, and then resolidify.

To check that, you check that the solidification is a left adjoint. So to check that this is nice, you check that there are maps into all... For all Z, which is solid, we want $\mathrm{Hom}(M \otimes^{\mathbb{S}} N, Z)$ to agree with $\mathrm{Hom}(M \otimes N, Z)$. On the right, Z is still solid, so it's enough. But then the two Hom's are the same, so it's also the Hom from $M \otimes N$ to Z. This is not from the solidification, it's from the original one, but it's okay.

So we have a very nice category. It acquires its own enrichment, but now we would like to understand what it actually looks like. Actually, one thing we also need to understand is how to interact with the real numbers. We already said that the real numbers are certainly not solid, but something stronger is true. Namely, the real numbers do not admit any nonzero maps to anything solid. Or equivalently, the solidification of $\mathbf{R}$ is equal to zero.

Okay, so for this, note that $\mathbf{R}_{\text{solid}}$ is a ring, because the real numbers are a ring, and if you solidify it, it stays a ring. For a ring structure, to show that a ring is zero, it's enough to show that 1 equals 0. Now you want to make use of the fact that in an abelian group, you can uniquely form sums of $\mathbf{N}$-indexed sequences. There are lots of $\mathbf{N}$-indexed sequences in the real numbers that are not actually summable, so you would expect that using such divergent sequences, one can produce a contradiction.

It actually took Dustin and me considerable effort figuring that out, but just yesterday, Ian and then also Kobe found an argument. Here's their argument. We consider the $\mathbf{N}$-indexed sequence $1, \frac{1}{2}, -\frac{1}{2}, \frac{1}{4}, -\frac{1}{4}, -\frac{1}{4}, -\frac{1}{4}$, and so on. I hope you can guess how it continues. You take $\frac{1}{2^n}$ and take each one 2^n times.

This is a sequence $\chi : \mathbf{N} \to \mathbf{R}$. Then in the solidification, we have a map $\chi : \mathbf{N}_{\text{proet}} \to \mathbf{R}_{\text{solid}}$. When you compose further to the solidification, we have this map $\mathbf{N} \to \mathbf{N}_{\text{proet}}$, and then there exists a unique map $\mathfrak{m} : \mathbf{N}_{\text{solid}} \to \mathbf{R}_{\text{solid}}$ filling this diagram, because we are using that solidification forms a localization. So we can uniquely find such a "summable sequence" in the solidification, which intuitively speaking should be the thing starting the sequence with 1. In particular, if you restrict to the inclusion of 0 in $\mathbf{N}_{\text{solid}}$, you get a map from $*$ to $\mathbf{R}_{\text{solid}}$, an element of $\mathbf{R}$

Solidification is right exact if you have enough projectives. If M is an R-module, then you can resolve M by a solid projective resolution. Then, for example, if you solidify this resolution, you see that R solid is zero. By a spectral sequence argument, the vanishing of Ext groups here reduces to the vanishing of Ext groups from the reals. But the Ext groups from the reals, they are R-modules. A different way to state this would be to observe that all the internal Ext's of M against something solid will be R-modules, but the solidification of R is actually zero, so any R-module works.

When you compute the internal Hom Ext's, since you don't have enough projectives, the classical approach is to use injectives. But then they are no longer solid. I don't know if that's solid, but of course if you have enough solid projectives... The internal Ext's I was referring to here, they are the ones in condensed abelian groups.

If you compute them by injective resolution of the right hand side, the solid argument... If you compute them by projective resolution of M, then by what we proved before, all the internal Hom's are solid. But if you are obliged to compute them by injective resolution... I think it's okay. This is definitely an R-module, right? But it's also solid. Why is it solid? Because internal Hom against something solid is always solid. That's something I previously said. But it doesn't follow immediately...

Okay, so everything on real numbers is completed by a limit argument. But on the p-adic numbers and so on, you get them by a limit of discrete things. So everything that's pro-discrete is definitely solid.

So, next goal is to compute the solidification of $\mathbf{Z}$. The idea is that when you solidify $\mathbf{Z}$, you need to adjoin lots of new elements, because $\mathbf{Z}$ has no non-zero divisible subgroups. But then the divisible subgroups are summable. So the sum of the sequence $1, 1/2, 1/3, 1/4, 1/5, \ldots$ must now also be in $\mathbf{Z}$ solid, and so on. We expect there to be lots of new elements.

One way to make this precise: Consider the subspace $X \subset \prod_{n\in\mathbf{Z}}[-n,n]$ given by the union over all integers of the products $\prod_{k=-n}^{n}[-k,k]$. So it's a subspace of the whole product. Then there is a null sequence here, which is given by the sequences that are non-zero in only one term. Explicitly, let $e_n \in X$ be the sequence that is everywhere zero except the n-th spot is 1.

Then actually on solidifications, that e_n sequence becomes zero. Actually, I didn't talk about the verification, so I would like to even say that not just on solidifications, but in derived solidifications, this e_n sequence becomes zero. A different way of stating this is to talk about Ext groups against solid objects.

That the solidifications agree means that the Hom's against any solid object agree. But in fact, all the higher Ext groups agree as well. I cannot read what you wrote, just after the $\mathbf{P}$. Is it the map from which s_n to... And then here you have a sum of those basis vectors e_n that are everywhere zero except in one spot where they are 1. Let's organize this into sentences and paragraphs:

These e_n are just the basis vectors, which is a null sequence in here because the sum has the topology of pointwise convergence. Can you send n to the sequence where only the n-th coordinate is 1 and the rest are 0?

Yes, I think so. We know that this makes $\tilde{K}$ quite big, because the underlying abelian group of $\tilde{K}$ is actually an uncountable abelian group. You sum only along finite sums of these countably many basis vectors, but $\tilde{K}$ is a very large, uncountable thing.

So we've got quite a bit to prove. Let me draw some nice diagrams. Okay, maybe I won't do all the compatibility checks, but let me just draw the diagrams and then I'll leave a little bit of diagram chasing.

So we're trying to make good use of the fact that when you have solid abelian groups, the internal Hom's behave well. In other words, they commute with solidification.

Okay, so we have this diagram here. And then we have another map which is... Okay, so what are the maps here? Giving a map from $\prod_{n=0}^{\infty}\mathbf{Z}$ or something to here corresponds to a map from $\mathbf{Z}$ to the internal Hom, meaning it corresponds to a null sequence in the internal Hom. So to give this map, I have to give a null sequence of maps from the direct product $\prod_{n=0}^{\infty}\mathbf{Z}$ to itself, which are the projections to the coordinates greater than or equal to n. In other words, in the first n coordinates it's the zero map, and on the others it's the identity.

And so then what does this composite do? This composite here also corresponds to... Let me first describe this composite here. This corresponds to the sequence of maps which are just projection to one fixed coordinate. Because if I think about the difference between two consecutive maps, only one fixed coordinate remains.

But the thing is, the projection to the n-th coordinate actually factors over the integers. So in each case, when I fix one n here, the corresponding map is just projection to the integers and then adding back in. But this means that actually all of these maps that are parameterized here, all of them factor over the subspace $\tilde{T}$.

So this means that this composite factors over this subspace here. I actually need to use that I take a bounded product, because if the coordinates stayed unbounded, I wouldn't actually get this characterization.

Okay, so what's the idea here? You have the identity endomorphism here, and you write this as a sum of a null sequence of endomorphisms. And the null sequence of endomorphisms is the projections to the individual coordinates. So the identity endomorphism here is the sum of the projections to any coordinate.

But all of these endomorphisms that you sum, they all factor over the subspace $\tilde{T}$. And one other thing I should say is that because the first map that I project is really just the identity, if I project to coordinates greater than or equal to 0, I'm not doing anything. This actually means that if I come back, at the 0 end, this s here is the identity.

And so then I can solidify this diagram. I guess that's how to rewrite it.

Okay, so we have this diagram. Well, now this here is an isomorphism, because F solidifies to an isomorphism just by definition of what solid means. And solidification is symmetric, so both of these maps become isomorphisms.

But now this means that this map actually is a split surjection, because you can find an inverse by first going here, then going here, and then going here. So this is actually a split surjection, which is almost what we wanted to prove. It's actually an isomorphism.

To show that it's an isomorphism, you have to show that when you circle around this diagram, you get the identity. And this is another diagram chase that maybe I won't do, it's not difficult. Yeah, so all the maps are isomorphisms.

And so this means that this actually becomes an isomorphism too. I claim something slightly stronger, namely the agreement

So there's a derived solidification functor. And then the same argument is true on solid modules. This T that you solidified is p-completely solidified. A ring object now, it will be, but it's not obvious. Oh no, sorry, I think T itself is already a ring object. It doesn't have a unit, does it? Well, just zero, if you want. I mean, I'm not sure. Wouldn't the identity be the constant sequence 1? Which is not, I'm not sure how you get a ring structure component-wise.

I guess, in fact, once we're here, we can actually compute the solidification with another example. So that if we take S^1 product, then the whole product, but I mean, this is already solid. We don't need to solidify. And it's also a condensed ring, okay.

And actually, this is a case where I do need a little bit of this $\varprojlim^1$ business. Because it's actually, so we have this sequence here, $\varprojlim S^1$ is pretty large, and then we have this funny $\varprojlim^1 S^1$, which is some weird non-separated guy, but whatever. And so what we have to see is that $\varprojlim^1$ for all i equal to zero, and then this guy doesn't have any $\varprojlim^1$.

Now, of course, I've carefully planned my lecture. So now we know already one class of examples where the solidification exists, which is anything that admits a module structure with a real number. So the claim is that this guy is a condensed ring, which seems surprising at first.

But so why? Actually, there's a different way to write this. It's also the same thing as a bounded product of copies of the real numbers modulo unbounded products, where I define the unbounded product the same way, as an increasing union. And why is that? Because the difference between these two notions, mapping A to B to C to D, is the same as first taking ratios here and ratios here.

So what is the ratio here, or $\varprojlim^1$, whatever. Here the ratio between these two is, of course, just the product of z_i. But the $\varprojlim^1$ here is also the same thing. Because if you want to surject onto a product of circles, then of course you can keep the projection, can keep them z_i and 1. So this definitely surjects onto here, but then the kernel is obviously just B sequences of z_i. So if you want, you can draw some kind of three-diagram of short sequences justifying this. Like a short sequence here, short sequence here, and then they give you a snake lemma.

Right, and so this one is visibly a condensed ring. Apart from this discussion, the upshot is that the completion of $\mathbf{Z}$ is just the whole product, and the $\varprojlim^1$ is zero. So then, now where here on the right I'm taking the ext groups, all the ext groups I'm taking are currently still taking in condensed abelian groups.

Noting that, I mean, of course this is zero for i greater than zero. Because, so in particular, we recover that Ext^i in condensed abelian groups from this product of copies $\mathbf{Z}$ to say, $\mathbf{R}/\mathbf{Z}$, which is a discrete abelian group, is just a direct sum of copies of $\mathbf{Z}$ and $\mathbf{Z}$ for i positive and zero. So this is something that I also proved at the very end of the last lecture. And actually, I imagined it would be an input into today's lecture, but now actually one can present the argument so that it's a corollary here.

Did you use the solid analytic feature to show that Ext^1 vanishes?" Yes, I did. Well, I mean, they're also in condensed abelian groups, but yes, I definitely used a different argument. Also, when you actually want to justify that this map here is surjective, I mean, it's actually, if you think in terms of

Why? I mean, this is just given by... Think that, but p_T of P, you can take this to P. And basically, there's a free guy on, like, an $n \times n$ grid of elements that all jointly converge to zero. But then, well, you can just enumerate... Same, it's just, here is some kind of...

So, in particular, like, phrased in terms of... One way to think about power series algebras, in particular, two power series algebras together, and you get the power series algebra. And maybe up there I should have noted, in some cases, the product of n is some compact projective object. No, I'm sorry.

Alright, so we understand quite a bit. We don't yet necessarily understand all the objects and groups, because there are other generators in life groups: $\mathbf{Z}$ join assets fin. And so we could wonder what their solidification is, but this can also be understood.

And then the... exist from the free... toward using solidification and on X solidification. So you see that also these solidifications of these guys will be a product of... And finally, any assumption on S? Like, S should be non-empty or infinite, or you should... Yes, yes. Yes, infinite.

Canonically, one way to write a morphism is that if you write S as a limit of compact sets S_n with surjective transition maps, this will actually just be the limit of these... These are all find of free being in groups, the... the transition maps. So it's easy to see that any such limit would be isomorphic to a product of copies of $\mathbf{Z}$.

So, in our previous way of setting up solid series, we were actually taking this formula as the starting point for defining what a solid guy is. So we were just, on all the generators of our category, all these $\mathbf{Z}$ join S's for profinite S, we were by hand declaring what the solidification should be. We were defining it to this limit, and then we were checking by hand that this gives a valid pretheory. But this argument is actually much harder than the way I've set it up now. In some sense, the definition of what a solid group is, is much clearer, and then this really becomes a computation.

Okay, so let me quickly give a sketch. So we have our S and it's written as this limit of finite sets. I assume that all the transition maps are surjective and I inductively choose... Well, first the section $i_{\mathbf{Z}}$ from S second $\mathbf{Z}$ back into S. Then, on all elements of S_1 that are not in the image of $i_{\mathbf{Z}}$, you pick the first playing on on S_0. And then if you project back down to S_1, in particular, you've already some elements of S_1 which you've already lifted to S, but then there's new elements of S_1 that I didn't yet have. And so on, then I pick p_i new S sometimes on S_1 minus the image, and so then jointly these two things together now define for me a section from S_1, which on some elements is given by projecting to S_0 and then taking the lift you already have there. And the other elements, you make a new choice. And then you continue.

Okay, so this way, what in particular you will get is a countable sequence of elements in S, right? Because the union of all these maps is just a countable subset of this, but it will certainly be dense.

And so, let's also enumerate the elements. So, enumerate n as you start enumerating at zero, then start enumerating the elements of S_1 that weren't in S_0, one and so on. And we get a map d from $\mathbf{N}$, which is some of the free guy on this copy of $\mathbf{N}$, towards S. Right, on $S_{\mathbf{Z}}$ this is given by $i_{\mathbf{Z}}$, but on some higher S_{n+1} it's given about the difference, the next in

Canonically, it should be this thing. In order to prod such an isomorphism, I have to produce such an isomorphism. If you carefully think about how you would actually go about doing that, you have to choose a new base of elements. Then it is precisely what you want to end up to.

The argument is actually very similar to the argument we already did. We have our carefully here, and then sectors, where again, giving such a map means I give a pro-sequence of maps from S to S. The pro-sequence I consider here is a sequence of $S \to S_{\leq n}$. So you start with the identity, which we have to do because we want this splitting here, and then take I project to the $\leq n+1$ and take the splitting π_n.

Okay, so when I write these maps, I need the maps induced on quotients. It turns out that because some of these maps approximate the identity here, the same as $S_{\leq n}$, and so then this guy here, this corresponds to the sequence of the differences $\pi_{n+1} - \pi_n$. If you think about what these differences are actually doing, you realize that they are only changing something on a small part. I would mess it up if I try to say it orally, but you can check that you've exactly crafted things so that this difference will factor over the image of this $S_{\leq n}$.

The idea here is again that you take the identity here and try to write it as an infinite sum of maps, all of which factor over the submodule. For this, you use the sequence of functions $\mathbf{Z}_S$ which are factors of something of finite range $\mathbf{Z}_S$, and then you can do it. The argument is just the same as before.

What becomes critical here is that you really have a pro-finitely presented I. If I were to set up the theory of solid abelian groups back in alter groups, then the condition I gave that just talk about pro-sequences wouldn't be enough, because you will never see this I as a quotient of the integers. You're really using that you can still understand this via its finitely presented quotients.

Okay, so now we have that $\mathrm{Solid}(\mathrm{Ab})_{\mathrm{ST}} = \mathrm{Coh}(\mathcal{O}_S^{\mathrm{a.cg}})$. In particular, this includes that it has a compact projective generator, a single compact object which is a full subcategory of products $\mathbf{Z}_S$, and it's actually internally projective. In fact, if you tensor it with itself, it becomes isomorphic to itself.

Also, in the solid case, being solid is equivalent to only having the Hom-finite condition hold, which is what we took as our original definition. It had only $\mathrm{Hom}(\mathbf{Z}_S, -)$ preserve filtered colimits, and all maps $\phi : \mathbf{Z}_S \to M$ have a unique extension to $\mathbf{Z}_S^{\mathrm{a.cg}}$ from the free object, which is the colimit $\varinjlim_n \mathbf{Z}_S$.

And this, and then also because ϕ is actually zero. And maybe I didn't say, but you do it, you can do it by taking M to be anything, and then you go from the universal property for $\mathbf{Z}[S]$ to the same one for any M, because you represent the solidification of M as $\mathbf{Z}^S$. So the fact that any map from M, from other M (I mean M') to $\mathbf{Z}[S]$, factors through the solidification, and then by then is clear that this is the same as $\mathbf{Z}^S$. I think this also represents M. Part of it is clear, a direct fact.

So, time, so let me just give one kind of philosophical way of thinking about this solid condition. The very end condition was that all null sequences are summable. And something that we get out of this is that one can always integrate against certain kinds of measures. So one can also think of $\mathbf{Z}[S]$, it's actually the same thing as the continuous functions from S to $\mathbf{Z}$, because the continuous functions, they have a colimit of the functions on S to finite sets. This gives a description.

And so from this perspective, these are some kind of $\mathbf{Z}$-valued measures on S. And so this means that whenever you have a map from S to M, where M is solid, and whenever you have a measure on S, then this induces by what I said here, and this is the measure M to something here. So we can and this to the integral of f against μ. In other words, whenever you have a map from some profinite set into M and some $\mathbf{Z}$-valued measure on S, then you can form the integral.

One thing that I should stress here is that this characterization of solid at the end, I only talk about homs, not about internal homs. The first characterization I gave where I talked about differences, I had to talk about the internal homs. Here it's a problem enough.

All right, I'm out of time. Let me check why the composition is zero. Because these are 0, solidification is also 0. I'm slightly confused about my diagram, why it's still a presentation of M. That's maybe... First, you definitely get a right exact M with a solidification. Solidification is right exact. But then, and you definitely always have this map here, but then the observation that there exists this map back, which is zero here, means that this actually retracts. So M is a retract of its solidification by this argument. But retracts of solid guys are solid. So there is a single compact projective generator.

Right, so I mean we'll discuss this more next time. So one can give some purely synthetic algebraic descriptions of what the category of solid abelian groups is without talking about anything. And so you can say what they are, right? Products of sums of $\mathbf{Z}$, they are just infinite matrices which in every row are eventually zero.

Are you going to give a similar new way to define, a new definition for liquid modules? Or is this new formalism available only for the solid setting? The question is whether one can also characterize the liquid vector spaces in a similar way by mapping. I think it should be possible, and we're currently figuring out the details of what works. Let's see, we still have a few weeks.

6. Complements on solid modules (Scholze)

https://www.youtube.com/watch?v=KKzt6C9ggWA&list=PLx5f8IelFRgGmu6gmL-Kf_Rl_6Mm7juZO

Unfinished starting from 0:00

So today I want to, before Dustin takes over on Wednesday again, give some complements on solid modules.

Sorry, just briefly recall the definition. So P has always denoted the projective object which is, as a free $\mathbf{Z}$-module, $\mathbf{Z}^{\mathbb{N}}$, the $\mathbb{N}$-sequences. And then we consider this endomorphism of P which is the identity minus the shift n which takes the generator n to $n+1$.

Then the new definition of a solid module was, though entirely analogous to the old definition, but on P-modules. So a P-module M is solid if it is, by f, an internal Hom morphism, where intuitively speaking this is a map from P to n, the space of $\mathbb{N}$-sequences in M. So you take an $\mathbb{N}$-sequence in M and map it to the sequence of its differentials. Intuitively speaking, that this is an isomorphism means that conversely, if you have any null sequence in M, then you can sum it. Because if you think about it, then this m_0 must be the sum of all–

So this looks like some definition of derived complete modules, like p-adically derived complete. I suppose there is some mathematical relation to derived completeness. Let me talk a little bit about that.

I mean, I think on the face of it, it's not related, but okay, there will be some derived completeness today.

And so then the theorem we proved last time was that this category of solid P-modules is stable under all filtered colimits (let me just say limits or colimits), under tensor products, and also under internal Hom, making the left adjoint to this inclusion an "absolute solidification", making this left adjoint a symmetric monoidal functor. And it is generated by a single compact projective object. This projective generator is P itself, and on this generator the solidification functor is the identity.

And maybe the other thing I should say is that one can compute, for the solidification of all these free P-modules on a profinite set S, they are just the limit of the free P-modules on the finite quotients of S.

Okay, so I think this basically summarizes everything from last time.

Maybe the first thing–okay, then let me first discuss the derived enhancement of the situation.

So let's assume that instead of just considering P-modules, you really have a derived P-module complex. Then actually the definition still makes sense, and we take that as a definition. So we say that a complex of P-modules M is solid if, again, the internal Hom from P to M is an isomorphism. But again, because the internal Hom from P is actually exact, this is actually equivalent to asking that for all $i \in \mathbf{Z}$, the internal Hom from P to $h^i(M)$ is an isomorphism. So this is equivalent to asking that all the homologies of M are also solid.

And again, this is also true in our previous discussion of P being $\mathbf{Z}$-modules, and is in fact a general property for what we call an "analytic ring structure". That you can check completeness at the level of homology groups–classically this is a little subtle to prove, but here again it's an immediate consequence of the internal projectivity of P.

Okay, so then some corollaries of this. The class of solid complexes is itself a triangulated subcategory, or if you would work in the ∞-categorical setting, it would be a stable ∞-subcategory, and stable under all direct sums and products, so all limits and colimits.

And one can again show it's also stable under $R\mathrm{Hom}$, again from the same proof as last time. And then if you just repeat this discussion of tensor products now at the level of derived categories Not completely automatic, this uses that these projective generators here stay—they somehow have expected vanishing also in light condensed abelian groups. Or, rather, that's the solidification actually here, okay.

Is it the case that if you have any complex, possibly unbounded, whose terms are direct sums of those generators, these generators, then taking solidification term-by-term is a good derived functor? That it gives you the—yes, that's right. Solidification preserves, like, on the level of—maybe forget about the thing.

So then you have this whole subcategory in the derived category of solid objects. And again, by adjunction, you will actually—it will have a left adjoint, some kind of derived solidification functor, which again will commute with all colimits back into $D(Cond(Ab))$. And then this, on the generators, we kind of determined what it does last time, that it becomes this thing, even in the derived setting.

Yeah, okay. Also, on the level of derived categories, we have a full inclusion, really. So it's as nice as it could be. And again, by some adjoint functor theorem, this has a left adjoint. Let me write this as "derived solidification L-box".

So in the adjoint functor theorem, usually there is some set-theoretical condition, because you want—I'm thinking of everything as ∞-categories, and then everything here is a presentable ∞-category. And then L has this general adjoint functor theorem there. I think there are, for triangulated categories, theorems of Neeman and others which would also justify this, I believe. But I mean, they often assume actual compact generation, which is not quite true in this case, so I'm not sure. But the ∞-categorical version is definitely sufficient.

Okay, so for before, we wouldn't need the abstract theorem, because we can just prove the existence by hand in this case. Because on a class of generators, we can—we have identified that these guys already know that they exist. But the abstract theorem is good for both left adjoints and right adjoints, assuming that there are enough limits and colimits, right?

I mean, so the notion of presentable ∞-category is one which has all colimits and which is generated by a set of objects. And in this situation, every functor that preserves all colimits has a right adjoint, and every functor that preserves all limits and all sufficiently filtered colimits, so it's accessible—also has a left adjoint. And there's a unique symmetric monoidal structure making $A \mapsto R$. And actually, something even better is true, that this is literally the left derived functor of the thing you have between abelian categories. And similarly, you can also—I mean, the derived tensor product is also obtained by deriving the thing you already have on the abelian level.

But here, how do you know that the solid category is presentable in—before you proved the existence? Yeah, I think there are general theorems that if you just ask locality for such a map, there are some automatic presentability theorems, okay.

So once we pass to the derived setting, I can state things from last time. That really, the derived solidification of $\mathbf{Z}[S]$ is still $\underline{\mathbf{Z}}[S]$. So it is—under solidification, there are no higher extensions. And so also, the derived solidification of $\mathbf{Z}_p$ is this perfect $\mathbf{Z}_p$. And the derived tensor product of $\mathbf{Z}[S]$ with $\mathbf{Z}_p$ is the condensed ring $\underline{\mathbf{Z}_p}[S]$, okay.

So you might think that nothing derived ever appears in practice, but not so. Here's a very funny proposition. Let's say again that X is a CW complex.

So recall that in this situation, we previously had this thing that the X-valued points of $\underline{\mathbf{Z}}$ are the singular chains, right? But now we can also interpret the homology of X. And it turns out that the singular homology of X is exactly the homology of the derived solidification, more precisely.

I mean, there's some complex computing singular homology, and this is really isomorphic to the derived solidification. In particular, the 0-th soli

In general, if you take the union of circles of all dimensions, you get something which is concentrated in degree zero, but solidification goes arbitrarily far to the left.

So again, there's the formal reduction to the case of complexes—just assuming I keep, which, and so for a finite CW complex, how do we actually compute this? Assume X is a compact and finite CW complex. We know this homology is finitely generated in each degree.

I may miss the point, but you said that the derived solidification of $\mathcal{G}(X)$ is a singular complex, right? So it is a quasi-isomorphism? Ah, quasi-isomorphism, sorry. I see, sorry, it's only a quasi-isomorphism. Yeah, that does make sense. Sorry, I see.

Okay, so how do we compute the solidification? We use those things. We take some surjection from some profinite set onto X, like the constant map, and then we get all fiber products. This gives a resolution of this condensed sheaf by the constant sheaves, and then the constant sheaves on the fiber products, which are again some profinite sets, and then all the fiber products.

Okay, so that's the resolution of this guy. To form the derived solidification, we can apply the solidification to all the terms here. But for these terms, we know that it's concentrated in degree zero. We know that the derived solidification is computed by this, let me not underline.

Now, this might seem a bit hard to compute, but we know that each term here is actually just $\mathrm{Hom}_{\mathrm{cts}}(S, \mathbf{Z})$, the continuous functions from S into $\mathbf{Z}$. That's something I discussed at the end of the last lecture. I mean, this formula that this limit of the S_n can be translated into this formula. So these are some kind of measures on X, and similarly for the others.

This actually means that all the terms in this complex are isomorphic to their double dual. The continuous functions from S to $\mathbf{Z}$ is the dual of this one, and then I dualize again. The solidification is actually just Hom, and I could also put the $\mathbf{R}$ here, it doesn't change in those cases.

So this actually means that the same formula will be true for this guy, if you think about it. Because it's a resolution, this means that also this derived solidification here, one way to compute it is that it's isomorphic to $R\Gamma(X, \mathbf{R})$.

But now we're in business, because we know that this guy here is isomorphic to the singular cochain complex. Because we know that the singular cohomology for finite CW complexes is finite in each degree, it's taking the dual of singular cohomology against singular homology.

Okay, and so you get that the derived solidification of X is the dual of singular homology. This means that to some extent, this passage from condensed abelian groups or solid abelian groups is like passing to homotopy types, a little bit like, at least for CW complexes, it contracts the interval and then identifies two homotopy equivalent CW complexes. But on totally disconnected things, it's much finer information. You could still invert homotopy equivalences, but not invert weak homotopy equivalences.

Is it the case for locally contractible spaces, in the sense that you explained? Yes, so whatever I said about CW complexes also works for a locally contractible space. Actually, Zhouhan pointed out to me that the paracompactness assumption is actually not required in comparing singular cohomology and sheaf cohomology. No, they are not required. There are some recent papers to that effect. But I don't care in some sense right now.

Okay, so now let me go back and try to describe the category of solid abelian groups a little better. We want to understand the structure of solid abelian groups better.

So there is a compact projective generator. There's definitely a notion of finitely generated objects, and these are precisely the $C(S)$ for S profinite. And then there's a notion of finitely presented objects.

These are the cokernel of maps, and it turns out that the modules behave like modules in the following sense. They are presented objects, form themselves in a category, and the only critical thing is stability under kernels. But once you have that, it's automatically stable under kernels, cokernels, and really only the kernels are something to mention.

The whole category is just the ind-category of those. Also, any finitely presented object actually embeds into a cokernel of an injective map. This is actually something slightly better than what [name] announced in the first lecture. He claimed that the finitely presented objects have a resolution of length two, but actually length one is good enough.

There's a resolution that just stops. Okay, so there are some finite objects in your category which are exactly the cokernels of injective maps, and then everything is a kernel of a map from one of those.

The key step for this is the following. Trying to understand all the finitely presented ones, okay, you have a kernel. You can always find a surjection onto M, and then there's some kernel. The kernel, you still know, is at least a finitely generated submodule of this. So then it would be good to know that all the finitely generated submodules of this are actually such a product. This is what I claim now.

Zero? No, sorry. Let's say product of copies of Z. Generically it could be a product, of course it could be zero.

This definitely implies this statement here, but it also implies, like, okay, so objects are always stable under cokernels and extensions. Things like they're stable under kernels. But then, I mean, this also always reduces to identifying the finitely generated submodules of your generating objects. And if those are all finitely presented, then you're in good shape.

So the theorem is really easy to deduce from—okay, so let's prove it.

We know that it's finitely generated, so we know that there is some surjective map from a product of copies of Z onto M which injects back into [word unclear].

By the way, this is again the result. Previously, a lot of what I was talking about, about the general properties of the categories being Grothendieck abelian, they basically extend to the full condensed setting, not just the line one. This theorem is again one which only holds in the line one setting. And because we're again using some countability in just a second.

Okay, so we have some here. Let's call it g.

We know that the maps from a product back to Z, they are given by direct sum. So we actually know that this is dual to a map in the other direction from a direct sum of these. Basically, our task is to show that whenever we have such a map of a direct sum of these, and when we dualize, then this image here is itself a product.

For the proof, I will use the following fact, which I'm not sure if it's that well-known.

Let N be a countable group that maps into a direct sum of copies of Z. I mean, it's just an abstract—yeah, discrete, no condensed sets—maps into Z.

Maybe, okay, let me leave this as an exercise. I know it's definitely false if you drop the countability assumption, because if you take an arbitrary product of copies of an abelian group, it's definitely not a free abelian group.

But one thing this implies is that if I look at the image of such a map, then this is definitely countable, and it embeds in the direct sum. This also embeds into the direct sum, which implies that the image of the map is free.

For this, you don't need this [previous fact]. It's just the image. Okay, I will use this [previous fact] again in a second, so I wanted to mention it.

The image is free, but then you have a surjection here from this direct sum onto another free group. So this splits; in particular, there's a kernel here, but this must actually split back. So the image of the map splits as a direct summand.

I mean, maybe actually this [previous discussion] is completely irrelevant. Maybe I should focus more on the question at hand. But let me just try to understand a little bit about the structure of such maps and what it implies on the dual side.

So it kind of splits, but this means that you can basically replace this by the other direct factor, which is also [sentence cut off].

Freyd and replace H by the corresponding thing, because then this corresponding product will split into two copies, and this N will just embed into one of them. So we can definitely assume that H is indexed.

Okay, so now we have an injection. I'm going to direct-sum these, and now we have some quotient here. The next thing is to understand the structure of the quotient.

There is a certain quotient of Q, which is the image when you embed it into a double—sorry, let me just write it as a product over all possible maps from Q into $\mathbf{Z}$. So you can look at all possible maps that Q maps to a free group, or just to $\mathbf{Z}$, and take the corresponding maps to $\mathbf{Z}$. Then there will be some quotient here, and by the fact that this guy is actually free, this means free or in particular projective, so it splits as a direct summand.

Also, if you look at the torsion of this M, then it's easy to see from this that this Q' will have no more maps to $\mathbf{Z}$. Because if this had a map to $\mathbf{Z}$, then because this is a direct sum, there's another map from Q to $\mathbf{Z}$, which would make Q okay. So this means that this has this quotient $\overline{Q}$ which is free, and so this splits back here, but then also back here if you want. So then you can also get rid of that summand.

Okay, so without loss of generality, you can replace Q by Q', and then there are no more maps to $\mathbf{Z}$. Those are without loss of generality. Q has no more maps to $\mathbf{Z}$.

A different way to think about what I'm doing here is to observe that the category of countably generated free abelian groups has core kernels, and this $\overline{Q}$ would be the core kernel.

Okay, so where are we in the proof? We're trying to show that whenever we have a map from a direct sum of $\mathbf{Z}$, then once we dualize, the image of this map is itself a product of copies of $\mathbf{Z}$. Now we split off the kernel, we split off part of the core kernel, and now we're down to a situation where we have this map and Q has no more maps to $\mathbf{Z}$. We're trying to understand what happens if we dualize.

So we have here a product of copies of $\mathbf{Z}$. This map here is our old map D. Okay, sorry, this is precisely the internal Hom from Q to $\mathbf{Z}$, and then the image would be actually X_1.

Now, we ensured that the Hom from Q to $\mathbf{Z}$ is zero, but actually it turns out that the internal Hom from Q to $\mathbf{Z}$ is zero. The points of Hom from Q to $\mathbf{Z}$ join into $\mathbf{Z}$, which by using the adjunction in a different order, is the Hom from Q into the continuous homomorphisms from $\mathbf{Z}$ to $\mathbf{Z}$, which is a discrete guy and where this guy is solid.

But Q has no maps to $\mathbf{Z}$ and just no maps to any abelian group. Okay, and so what does this mean? This means that the image of D is actually just this product of $\mathbf{Z}$s. So some of the reductions we made in the beginning precisely ensured that actually our map D became injective.

Okay, I guess strictly speaking when I did some of these reductions, it could have happened that an infinite direct sum became a finite direct sum. But just take the direct sum with an infinite thing, whatever.

Okay, so that's good.

Right, so actually call The light condensed rings are true again, which is also kind of the reason that we never saw any obstruction to this in actual mathematical practice.

We need for all solid $\mathbf{Z}$-modules, the product is solid. To see this in generality, we actually have a resolution of length one. But now, if I tensor this with a product of copies of $\mathbf{Z}$, then this map stays injective. I mean, actually, what you really see is that if you take the derived tensor product of this, you actually just get a product of copies of $\mathbf{Z}$, because you can just write this as a finitely presented guy.

So one thing that's really nice about this whole tensor product is that it really gives a lot of—I mean, many, many computations come out right, sometimes in non-obvious ways. Just like maybe previously, a lot of Ext computations came out right in non-obvious ways.

Let me do some tensor computations. Often, you maybe care about things like: you start with some N being a $\mathbf{Z}$-module and G being a group, and then you form the p-adic completion of them. In such situations, one often uses the completed tensor product, which is like the completion of the usual tensor product, if you do some kind of formal geometry or something.

Here's a theorem that is actually what the solid tensor product does. Actually, in full generality, for any abelian group, it's better to replace this by the solid derived p-adic completion, which is the derived p-adic completion operation. Here, this derived thing just means a complex represented by multiplication by p, where this sits in degree zero and this sits in homological degree minus one or homological degree one. Almost always, this agrees with the usual p-adic completion in degree zero, but in complete generality, that's a better operation.

Generally, it makes sense to talk about derived completions—I mean, these things are the derived complete modules. Here's a proposition, and because we're doing this, it's slightly better to from the start work in the derived category. Let me assume I have things which are concentrated in non-negative degrees, so things go to the left.

Say I have two of them, M and N, and they are p-adically complete. So there are isomorphic to the derived p-adic completion of M tensored with $\mathbf{Z}/p^n$ over n. Obviously, it's actually also a condition you can check on homotopy groups.

Then also their solid tensor product is the derived p-adic completion of their usual tensor product. Let me prove this in a second, but let me just note one corollary, which is, for example, that if you take an infinite direct sum of copies of $\mathbf{Z}$, take the p-adic completion of that, and then take the tensor product of that with such a p-adic completion, you just get the similar p-adic completion.

This means that in such situations of more or less formal geometry, for derived p-adically complete things, the solid tensor product does what you would expect it to do. There's really nothing special about the number p here—I mean, you could work with any base ring and solid module structures over that ring, and then any element of that ring.

So this p-adic completion of the free abelian group, you consider it as a solid abelian group, or as a condensed abelian group. It is solid because the class of solid modules is stable under all limits, and so on. You start with something solid like $\mathbf{Z}$, take a direct sum—it's solid. Take the p-adic completion—it's solid. Take a limit, and so on.

Remark: there's nothing special about p. For any ring R and any elements $f_1, \ldots, f_n \in R$, and solid R-modules M, N which are derived $(f_1, \ldots, f_n)$-complete, then the derived tensor product of M and N is the derived $(f_1, \ldots, f_n)$-completion of their usual tensor product.

I should mention this is actually a corollary of the preceding proposition. We know there's definitely a map from here to here, because the soli To check whether we can actually check after reduction mod p. Because for derived completed things, this can be checked modulo p. But modulo p, both sides just become the tensor product, and you're just taking the usual tensor product of FP's and product, a direct sum of FP's, and a direct sum of FP's.

Okay, so let me give a sketch. First of all, maybe in this situation you can actually work with $\mathbf{Z}_p$ instead, because the derived completion of $\mathbf{Z}_p$ with respect to p is just $\mathbf{Z}_p$. That's because you can take $\mathbf{Z}[[T]]$, $\mathbf{Z}$ power series in T, and then you quotient by $T - p$. And mod p, you get here $\mathbf{F}_p[[T]]$, but also when you take this power series in two variables and quotient out by p, then you get $\mathbf{Z}_p[[T]]$ quotient by $p\mathbf{Z}_p[[T]]$. So this actually means that the derived category of solid $\mathbf{Z}_p$-modules is automatically a full subcategory of solid

$\mathbf{Z}$-modules. So being a $\mathbf{Z}_p$-module is not a structure on a solid, it's just a condition. And if everything's complete, then everything becomes a $\mathbf{Z}_p$-module.

So $M \otimes N$ is actually here, and everything is taking place in the subcategory. Okay, and let's actually—let me do the case which is maybe the most destructive case, where M is really just, take a direct sum of copies of $\mathbf{Z}_p$, but you complete the direct sum. Then in order to compute what the solid tensor product does, we have to write this in terms of our generating objects. And this is actually a slightly non-trivial exercise, because there is actually a rather large collection of compact projective objects.

So what's happening here is that this actually is a colimit over all functions from $\mathbb{N}$ to $\mathbb{N}$ which go to infinity, so they only take finitely many values below some constant, of the product over n of $\mathbf{Z}_p$ to the $f(n)$. So what's happening here? First of all, why is there a map? So whenever you have a map f which goes to infinity, you can take the product of these copies of $\mathbf{Z}_p$ to the $f(n)$, and this actually maps to the completed direct sum, because modulo each power of p, almost all of the terms in this product will go to zero. That's because f is going to infinity.

And quite obviously, this is also injective. But then you still have to show that it's a surjection. So you have to show that whenever you have a map from a compact object to this completed direct sum, it actually factors over one of these terms. But if you have a map into this completion, then pick any test object S and a map from S to this copies of $\mathbf{Z}_p$, which is by the definition the limit. And so we have, I don't know, g here, and so we have a collection of $m_{g,n}$ here. And then each $m_{g,n}$ comes from some direct sum over integers at most some a of $\mathbf{Z}_p$, right? Because this is a compact object, and maps out of a compact object are finitely generated.

So okay, so let me go on. So there are just a bunch of elements where you get something mod p, and then there is maybe a larger bunch where you get something nonzero mod p^2, and then there's something even larger where you get something nonzero mod p^3. But then you can just find some slowly, very slowly increasing function f which factors over this product. Right, so what's my diagram here? These are my m, these are my m_n, and okay, this is $f(1)$, this is $f(2)$.

So this means that this tensor product of M and N, each of those, is a product of $\mathbf{Z}_p$

And now this has an obvious map, so f on it, over all functions. Let me call them H, which are now functions on both coordinates, which just go to infinity. Yeah, H is a function of two variables, and then you put the function of H and n here, where this is completed.

Now, at first you might think that this will surely not work out, because here you're allowed to quantify over all arbitrary slowly increasing functions of two variables, whereas here you're just getting those that are sums of functions of one variable individually. But then there's just—maybe at first slightly surprising but not that hard to prove combinatorially—that whenever you have such a slowly increasing function H, you can always find one that's even slower increasing, which is a sum of two functions of individual coordinates. I'll leave it to you as an exercise to figure that out. But that's what I mean, that the softer approach, like for non-obvious reasons, gives you the correct awesome.

And so, yeah, general argument is saying you can reduce to the case that n and m are just some completed direct sums of copies of the generators. And then, although the same argument—you actually have to be slightly careful because a priori, you can also have the case that m and n are completions of a direct sum which is over an uncountable index set. And then you still want that. But then there's actually an argument that the uncountable case just, by more formal arguments, reduces to the countable case. Because these derived completions, they always commute with, I mean, you can always reduce uncountable things to countable things. Just anyway.

Right, so this is a nice computation. There are some even further computations that come out, which really appear when you do some solid functional analysis. So let me work over $\mathbf{Q}_p$ for simplicity, but most of what I'm saying works over any field.

So then again, the solid p-adic Banach spaces, or even the derived category, it's just the full subcategory of p-modules, and those where p is invertible. And it's the full subcategory of abelian groups, and if you form tensor products and direct sums, it's in the subcategory.

And this category itself, it now has a compact projective generator. Well, here it will be a product of $\mathbf{Z}_p$'s, and here you have to invert p, take $(\mathbf{Z}_p)^\vee$. This is a slightly curious kind of topological vector space, or like a condensed vector space, but it actually comes from a topological vector space. So it looks like one where, somehow, the unit ball is this thing, where the unit ball is compact. But actually, there's this general thing that you cannot have Banach spaces or normed vector spaces of infinite dimension where the unit ball is compact.

So it actually turns out that if you were trying to endow this with a norm in the kind of obvious way, where this would become the unit ball, then the norm map would not be a continuous map. And so, yeah, it's not really a normed vector space, it is what it is.

So these things have appeared a little bit also in the classical functional analysis literature. Maybe first Lefschetz called them Smith spaces. And she was studying the analog of those things over the real numbers. They can also consider some type of topological vector spaces, which are well-defined unions gotten by scaling out some compact convex set.

The more usual objects we consider are Banach spaces, and at least separable examples of those are exactly the ones that we considered previously. You take countable copies of $\mathbf{Q}_p$, complete them, and then these things are actually in reality Smith spaces. So either you put "separable" and "light" on those sides, or you... So "separable" on the Banach side means you can take a countable direct sum, and "light" means on the other side, you take a countable product. And this is just the obvious: you take a Smith space and dualize it, but you can also take Banach spaces like so. Yeah, so the generator that we use here, the $(\mathbf{Z}_p)^\vee$, is sometimes dual to Banach spaces. Okay, we're basing everything on them.

Here's again a curious remark, and maybe people will Just the internal dual, that's true and kind of obvious. But if you go the other way, you can wonder: if you take a b-space and take the dual of that, is it just the usual dual? And the answer is, it depends on your model of the set theory. In the same way as this came up before, it fails under the continuum hypothesis, but under this principle $*$ that I mentioned (that is consistent), it's true. Because a b-space is a special type of pro-space—a limit of direct sums over $\mathbb{N}$ of $\mathbf{Q}_p$-mod-$p^{t(n)}\mathbf{Z}_p$. And so this duality between pro-things and ind-things is precisely the thing that's controlled by principle $*$.

Yeah, the more I think about it, the more I'm actually tempted to just use a set theory model where principle $*$ is true. Because you have the choice of either these higher Ext groups being some junk that really has no meaning at all, or them being zero.

Okay, but other than that—this question was never really relevant for what Dustin and I have been trying to do. Sometimes you run into this thing. I mean, it's definitely like, if you do some kind of computations now within this kind of solid functional analysis, you're doing lots of homs, lots of tensor products, lots of homs, and so on. At some point, surely you will take some RHom of a b-space against something in $\mathcal{D}$ somewhere. And then you can decide whether you work in a model where it's just some junk, or you work in this model where it's what it's supposed to be.

So, what I really wanted to say is, you also have fréchet spaces, which are countable limits of banach spaces along compact maps. These really pop up a lot. And there is a notion—a standard notion—of a completed tensor product for them. In particular, it's the one that's compatible with limits. So on b-spaces it's the usual projective tensor product, and when you have fréchet spaces (in such a countable limit), then it's the corresponding limit of completed tensor products of b-spaces. A general thing that usually happens when you define such tensor products in functional analysis is that you're trying to make them compatible with limits. For example, in this case, which is not related, because our solid tensor product was defined to be compatible with colimits. And usually there are very few functors that are compatible with both limits and colimits—certainly the tensor product is not compatible with all limits. But fortunately, it so turns out that in this situation, it does give the correct answer. So here's a proposition:

If C, M, W are fréchet spaces (let's say considered as topological spaces for the moment), then you can pass to the condensed world, take the solid tensor product (or even the derived solid tensor product), and it turns out that this is really just the completed tensor product of fréchet spaces. And then pass back. In particular, it's degree zero.

For example, if you take a product of copies of $\mathbf{Z}_p$ and take another product of copies of $\mathbf{Z}_p$, it becomes the bi-infinite tensor product $\bigotimes_{\mathbf{Z}}^{\infty} \mathbf{Z}_p$. Which might seem like it's a standard thing on compact projective generators, but it's absolutely not. Because these things are very, very far from being compact projective—only these kind of unit balls in there are compact projective guys, but this is a huge space.

Okay, and so let me again just give a proof sketch, or an example. The general proof is, in some sense, combining the proof I did there with those points, with the one I will do here. Bhargav and I kind of tried a minimalistic approach to combining these, but Sasha actually has a very fancy way to combine them that he needed for some categorical stuff.

Okay, so how do we write this product of $\mathbf{Z}_p$'s? It's again such a huge projective limit. And this time, you're taking such unit balls, but where you make the denominators larger and larger. So you take $\varprojlim_f \prod_{n\in\mathbb{N}} p^{-f(n)}\mathbf{Z}_p$, where again f is some function from
$\mathbf{Z}_p$, and again, this is next to similar Schwartz functions which are functions of both variables going to infinity very fast.

Again, you might naively think that surely if I have two variables, I can build functions that are so fast-increasing that I can never dominate them by something that's a sum of functions in both variables. But it turns out, again by basically the same argument as there is, you can always dominate any such function by a function, alright? That's fun.

I should maybe say, this is some filtered colimit here, but the precise structure of this index poset—this is actually where all the subtlety appears. Here we kind of don't actually have to understand what this looks like, because this colimit you can just prove. But for these X computations, you really have to be starting with such a description in the case of B space and then do that. And so then this huge colimit becomes a huge derived limit, and then you really have to grapple with the structure of this kind of poset of functions that grow arbitrarily fast. This is something that very much depends on your model of set theory, particularly on the cardinality of your set theory. Alright.

Yeah, I think that's basically all I really wanted to say. But if there are questions, we have a few minutes.

Q: For functional analysis in some very large Banach spaces, does it make a difference if you work in liquid condensed sets or all condensed sets?

A: It doesn't make a difference, because all Banach spaces are liquid. All Fréchet spaces are liquid, no matter whether they're separable or not. It's just the Preditorials that aren't factored.

Q: Is the relation between the homology of the C complex and solidification kind of an accident, or should I take it as something deep? Can I expect that we will have another example, in condensed spectra or something like that, for other cases?

A: Well, instead of taking the de Rham C being groups, you could everywhere work in a category of spectra. And then you could also... I mean, maybe a good point to point out the following: There's a spectrum called ku, which I like to think of as taking the algebraic K-theory of the complex numbers, but you take into account the topology that the $GL_n(\mathbf{C})$ has. This makes it homotopy-theoretic. Classically, this is a little bit hard to phrase what this is supposed to mean. I believe there are ways to do it anyway.

One way you can do it in our formalism is you can take the K-theory of the complex numbers as a condensed spectrum. Because the complex numbers, this is a condensed ring. And so this actually means that the K-theory series kind of has some automatic structure as a condensed spectrum. Because to give a condensed spectrum, I have to give a function where for each profinite set S, I have to produce a spectrum. But I can just take the K-theory of the continuous functions from S to $\mathbf{C}$.

So far, this is not at all true, because here the π_1, for example, will be $\mathbf{C}^*$ as a condensed being group. But now you can take $K(\mathbf{C})$ and solidify. Just like we had solid sheaves of solid being groups and sheaves of condensed being groups, you also have solid spectra inside of all condensed spectra. And this solidification does exactly what you wanted to do. It somehow contracts all the $GL_n(\mathbf{C})$ that this was built out of, topologically, essentially by the proposition I mentioned in the lecture today.

And then you see that this is actually exactly the... I mean, you could do the same thing really not just for the complex numbers, but for any algebra over the complex numbers, or even a category over the complex numbers. And then take the K-theory as a condensed spectrum and solidify. This actually gives a construction of what's known as a semi-topological K-theory. So solidification is what makes semi-topological K-theory.

If A is any kind of $\mathbf{C}$-algebra with a topology (I don't care about the specific topology), then you can similarly take the K-theory of A and solidify. This recovers something that's known as the semi-topological K Think: can you get a similar result for KO or KR? Sure, just write $\mathbf{R}$. I mean, all the obvious variations. So in particular, KO is $K(C^*(\mathbf{R}))$. There's something KU^{top} or whatever it is, and then maybe related to the...

So here the K is the periodic (or the full) spectra. I mean, in this notation, the K-theory spectrum... It turns out to be connective for the inputs that I'm using, maybe not quite obviously so, but it's true. And so there is a connective KU. It is much more subtle to recover the periodic KU. I mean, you can of course just invert Bott at the end, but...

And sometimes, in the p-adic case, the stuff that Dustin Clausen and I developed, which is a different way to approach this kind of stuff, is aimed at defining some kind of version of $C^*(\mathbf{C})$, but now an analytic ring, where the answer certainly has homotopy in negative degrees.

Ian: Is it somehow related to the description of the small KU or small $K(\mathbf{R})$ as some configuration space-like description given by Segal?

I don't know about the... Sorry, there's some description for small $K(\mathbf{R})$ and for KU using a thing given by Segal. I think this statement is much more naive. I mean, this statement is really just one way of encoding the intuition that... Or maybe see, but in some sense this is false. Something that's true before group completion, namely that if you take the infinity groupoid classifying space of $\mathbf{C}^\times$ and then pass to homotopy types here, then KU is a group completion of that, and basically solidification factors over passing to homotopy types. So this is Segal's...

But they have, I don't think, any configuration spaces.

Audience member: To what extent does the solid tensor product combine two topologies? Say we have M and N, and M is x-complete and N is y-complete. There was a thing that I mentioned: if you take power series $\mathbf{Z}[[T]]$ and then $\mathbf{Z}[[U]]$, then $\mathbf{Z}[[T]] \otimes \mathbf{Z}[[U]]$ becomes the (U, T)-adic one. So there it does do this thing.

In this more general condensed setting, it doesn't do it. So when you $- \otimes_A -$ and you have this x-completeness statement, now when you complete the ring A, this one is no longer the derived completion. Does it make a difference for the proof?

Yeah, so sorry, I actually realized that probably the most general version for solidification might not be expected to be true. I was getting confused about this. Well, I mean, if you want to compute some product of a solid thing or whatever, one way is to kind of resolve by the absolute one. And then you're also taking a product over A, and so to show that this here is the xy-completed one, it's enough to know that all the other ones are. And this way you can kind of reduce to the case that the ring A is actually just $\mathbf{Z}[[x]]$ here. So then the derived x-completeness of this one reduces to the one of $\mathbf{Z}[[x]] \otimes \mathbf{Z}[[y]]$ here. But A itself is derived x-complete. I mean, if it's basically x-complete, it's also derived x-complete. So you can kind of reduce this statement to the case that the ring is really just the join $\mathbf{Z}[[x]]$.

Audience member: What did you mean by "passing to homotopy type" in $B(GL_n(\mathbf{C}))$? So $\mathrm{GL}_n(\mathbf{C})$ is viewed as a topological space, and therefore as a condensed set or abelian group?

Yes. So when I define $K(\mathbf{C})$, one way to do that is to take this classifying space

7. The solid affine line (Clausen)

https://www.youtube.com/watch?v=fUjn2rGw9SA&list=PLx5f8IelFRgGmu6gmL-Kf_R1_6Mm7juZO

Unfinished starting from 0:00

Today we're going to be talking about a little bit of geometry, maybe the solid affine line. So let me start with a recap of what we've seen so far.

We had this category of solid abelian groups, which was a full subcategory of condensed abelian groups. This was some kind of analog of complete non-archimedean topological abelian groups, but it's kind of, from a formal perspective, easier to work with. It's an abelian category. In fact, it's an abelian category closed under limits, colimits, extensions, and many other things besides. Also, we had this left adjoint called solidification. There's a symmetric monoidal structure here, which you think of as a completed tensor product, such that this completion functor is symmetric monoidal.

The definition was in terms of this object P, which was kind of the free module on a null sequence. So the definition was that M is solid if this map from null sequences to null sequences given by identity minus shift is an isomorphism. Okay, so that's what we had last time.

Now I want to start doing a little bit of geometry. We're going to be modest and look at the affine line, which is actually the most important case. So about this P, it turns out that P, even before solidification, is a ring. There are two shifts one can look at, because you can shift one to the left and one to the right. I want the one that's injective.

P is a ring, and in fact, there's a ring map from the polynomial ring in one variable to P, and the shift is multiplication by T. So that's what you have before solidification. But after solidification, something extremely nice happens.

When we solidify, we already explained what the free module on P is. There's a compact projective generator here, which was a countable product of copies of $\mathbf{Z}$. That's kind of the basic object from which everything is built. You can get that object just by solidifying this basic sequence space P. If you take into account the ring structure, what happens when you solidify is you just get the power series ring in one variable. So in the solid context, the universal carrier of a null sequence is just this power series ring.

But moreover, there's a very nice property of this situation. Lemma: If you do the tensoring in the solid world of this power series ring with itself over the polynomial ring, you just get the power series ring again. Maybe the multiplication map is an isomorphism, you could see, even the derived tensor product.

Well, this is quite elementary, because we know how to do tensor products in $\mathrm{Solid}_{\mathbf{Z}}$. Peter gave lots of examples of calculations in this category, and the tensor product of two of these basic guys is just another one of them.

Proof: If you do $\mathbf{Z}[[T_1]] \otimes_{\mathrm{Solid}_{\mathbf{Z}}} \mathbf{Z}[[T_2]]$, that's just $\mathbf{Z}[[T_1, T_2]]$ by the basic calculation of the solid tensor product. Then if you want to get to this, you just mod out by identifying the two different variables $T_1 - T_2$, and the power series ring modulo $T_1 - T_2$ is the power series ring in one variable.

There's maybe another perspective Peter also mentioned. So, fact: if you take the solid tensor product of two derived complete things, then the result is still derived complete. Well, maybe you have to worry a little bit about this, but morally, if you want to check this is an isomorphism, both sides are derived T-complete. You can check it modulo T, and modulo T it's really a triviality.

Okay, the derived solid Tor product—is it defined as the derived T-product, then you solidify? Or how is it defined, exactly?

Yeah, you can either say it's the left derived functor of the solid tensor product, or you can say that you take the derived tensor product and then derived solidify it. It's all the same.

And do you need to know there are somehow enough flat things to construct the—you don't need to know that. But it is actually true that all of these guys are flat. I don't know, did Peter mention that? I forget.

Um, okay. Yeah, so a priori, you need to resolve both variables, but since you have flat objects, you only need to resolve one.

Okay, so what is the interpretation of this? This corresponds to the affine line, you could say. And then, since this satisfies this idempotent property—sort of like when you invert an element in a ring, you get the same thing—you can think of this as corresponding to some subspace of the affine line. A subspace, as opposed to just a random space with a map to the affine line. And that's the interpretation of this idempotency.

So then you could ask: how should one think of this subspace? What subspace is this? The naive thing to say would be that it's the formal completion of the affine line at the origin. Well, because it's a power series ring. But that naive interpretation is not the correct one.

So $Z[[T]]$ does not correspond to a formal neighborhood of zero in $\mathbb{A}^1$. And the reason is that that interpretation is not stable under base change.

So look at base change, by which I mean this is all implicitly over $\mathbf{Z}$, but we could tensor it to any ring, or any solid ring, and see what pops up. Let me do a little calculation.

Let's base change to a non-archimedean field, say—I'll just take the simplest example, $\mathbf{Q}_p$, and we'll see what pops up. So we take the affine line over $\mathbf{Q}_p$. Oh, sorry, well, maybe I'll just say: we take $\mathbf{Q}_p$ and then we do a solid tensor product with this thing here, and we want to compare this to $\mathbf{Q}_p\langle T\rangle$.

So $\mathbf{Q}_p$ is $\mathbf{Z}_p$ with p inverted, and the solid tensor product commutes with colimits in both variables. So this is the same thing as $\mathbf{Z}_p$ solid tensor product $\mathbf{Z}[[T]]$, and then you invert p.

But this $\mathbf{Z}_p$ has a resolution by two copies of—so $\mathbf{Z}\langle U\rangle/(U-p)$, it has a resolution by two copies of this power series ring. So we know how to do these tensor products, and it just does the naive thing of pulling in the limits.

So the result of that is that you get $\mathbf{Z}_p[[T]]$, and then invert p on the outside, which is not the same thing as the formal completion at the origin, namely $\mathbf{Q}_p[[T]]$. It's contained in there, and it's the subset where the coefficients are bounded in p-adic norm.

So what is the interpretation then of this ring? This ring is the ring of functions on the open unit disc in $\mathbb{A}^1$ over $\mathbf{Q}_p$.

So if you, maybe after an arbitrary non-archimedean field extension of $\mathbf{Q}_p$, you could ask: when you plug in a value in that field, when will such a series converge? And the answer is, it will converge if and only if the number you plug in has absolute value strictly less than one.

It's the ring of bounded holomorphic functions on the open unit Unit disc is you can imagine putting the open unit disc at infinity instead. So then you're looking at the complement of the closed unit disc. The complement of a thing is just as good as describing the thing.

Let's take this guy and put it at infinity instead. The complement—well, this is all just at the level of loose thinking. We can take this power series ring in one variable and tensor it over... but let me call the variable T^{-1} instead, because I want to be thinking of putting it at infinity. Then I tensor over $\mathbf{Z}[T^{-1}]$ with $\mathbf{Z}((T^{-1}))$, so then I'm puncturing at infinity, so that I actually live in the affine line.

So I have a homomorphism from $\mathbf{Z}[T]$... And what is this ring? I'm just taking this power series ring and inverting the variable there. Another way of describing this is as $\mathbf{Z}[[T^{-1}]]$. That's the open unit disc centered at infinity.

If we want to understand the closed unit disc, we should in some sense be kind of localizing away from this complement. Another way of saying that is that we should be killing this object. To get... we need to kill this thing here.

So... I'm just going to make a preliminary note. This $\mathbf{Z}[[T^{-1}]]$ is a module over $\mathbf{Z}[T]$, as I just exhibited, and it also has a very simple resolution.

Note that $\mathbf{Z}((T^{-1}))$—let's disambiguate and call the variable U—we can think of that as $\mathbf{Z}[[U]]$ and then invert U. One way to invert U is to adjoin the thing that you want to be the inverse, and then enforce the equation that says that they're inverse to each other. When you look at it like this...

I'll rewrite that. We get $\mathbf{Z}((T^{-1}))$ has a two-term resolution, where you have $\mathbf{Z}[[U]][T]$ and $\mathbf{Z}[[U]][T]$. Here you have $U-1$, so it's a two-term resolution. What are these objects? These are just the base changes of our fundamental p-... not to Solid $\mathbf{Z}$ but to Solid $\mathbf{Z}\{T\}$, and then adjoin a variable T. So these are resolutions by the compact projective generator of the category of $\mathbf{Z}\{T\}$-modules in solid abelian groups.

So killing this thing should be the same thing as requesting this map to become an isomorphism. Now this suggests the following, based on an analogy with the definition of Solid up there.

Definition: Let's say we have one of these guys. Let's say that M is $\mathbf{Z}[T]$-solid if and only if when you take $\underline{\mathrm{Hom}}(P, M)$ and then $\underline{\mathrm{Hom}}(P, M)$, and then you take the map which is given by—so now U corresponds to the shift, and T is the extra thing we have acting on M, because M is a $\mathbf{Z}[T]$-module, so we have (shift $\times T-1$)—we want this map to be an isomorphism.

Then this note up here is saying that that's the same thing as requesting that if now I should maybe pass to $\mathbf{D}(A_\infty)$... it's the same thing as requesting that there are no $R\,\mathrm{Hom}$s from this object we're trying to kill into M.

Okay, so now the theorem is basically that this definition, the name is well chosen. So this $\mathbf{Z}[T]$-solid theory over $\mathbf{Z}[T]$ is very similar to the Solid $\mathbf{Z}$ theory over $\mathbf{Z}$

Derived category of the abelian category. Okay, so that's all the kind of formal stuff. But in the solid $\mathbf{Z}$-theory, it was also important to understand the basic compact projective generator, which is always gotten by just solidifying the sequence space.

So let me make a claim about that. If you take the $\mathbf{Z}$ power series—well, maybe I'll just say let's not think of it as a ring; we're thinking of it as a module now. If you take a product of copies of $\mathbf{Z}$, base change it to $\mathbf{Z}[T]$, and then solidify, you get a product of copies of $\mathbf{Z}[T]$.

So maybe a little remark about an interpretation of it. I said "solid $\mathbf{Z}$" was kind of analogous to complete non-archimedean topological abelian groups. Non-archimedean means, say by definition, that there's a basis of neighborhoods of the identity consisting of open subgroups.

So what would be the analogous interpretation of "solid $\mathbf{Z}[T]$"? It would be that you have a complete non-archimedean thing where moreover there's a basis of neighborhoods consisting of $\mathbf{Z}[T]$-submodules. And you can think of—for a basic example of a $\mathbf{Z}[T]$-module which is non-archimedean but does not satisfy that property, you can think of this ring. You cannot find a basis of neighborhoods of zero which are stable under multiplication by T, because they're stable under multiplication by T^{-1} instead. But in some sense, the theorem is that if you kill just that one guy, then you've explained the difference between the two notions.

Okay, so in the previous theory without "$[T]$" it was a little bit different. It was not defined using this "p". Well okay, it basically was done this way. I mean, maybe we didn't make this explicit and we maybe more talked about just this, but we definitely talked about this. Yeah, but it does make it more clear to think of it that way with the p.

Okay, so the proof. Yes, confused about the $\mathbf{Z}[T]$ itself. Is $\mathbf{Z}[T]$ solid? Ah, so it is. That's part of the theorem, because I'm claiming in particular that this is $\mathbf{Z}[T]$-solid and $\mathbf{Z}[T]$ is a retract of this. So it's something that we need to prove and we will prove it. But it's not "solid", what $\mathbf{Z}[T]$ is un-"solid $\mathbf{Z}[T]$". Oh no, it is. As an abelian group, you mean? Yes. No, every discrete abelian group is solid because it's generated under colimits by $\mathbf{Z}$. No, it's an important point. Thank you for bringing it up. $\mathbf{Z}[[T]]$ power series is also solid. $\mathbf{Z}[[T]]$ power series is also solid, yes, because well, it's a limit of things. I mean, you can build it from limits and colimits from $\mathbf{Z}[T]$. So it's all right.

For the proof, well, all of these properties, all except the last. Those are exactly the same. The arguments for all of those things were completely formal, just based on the fact that you have this internally projective object p and you're asking that some endomorphism of internal $\underline{\mathrm{Hom}}(P, M)$ become an isomorphism. That was all that Peter used when he was proving the analog of these claims for solid $\mathbf{Z}$. So the fact that we have a good formal theory is already contained in there. And then in Peter's lecture, the hard part was identifying the free modules, that when you solidify this sequence space p, you actually just get a product of copies of $\mathbf{Z}$ fills up the whole thing. Thankfully, that part is actually going to be easier here because we already have solid $\mathbf{Z}$ and basically

Exactly. I mean, you basically—I don't know. Like I said, I sort of tend to think on the derived level from the beginning, but the basic point is that everything has a resolution by these internally projective guys. And then on those, they're the same, and then it's some derived limit of that. And it's just internally projective in h in all condensed, in light condensed, I mean. But it's really not... that's really not necessary either. I mean, this also works in all condensed. I claim it's formal, and I also claim I don't want to get into the detail right now. So yeah, let's maybe discuss after if you still have questions.

Okay, right. So the proof of claim: This follows formally from the fact that this ring $\mathbf{Z}[[T^{-1}]]$ is idempotent over $\mathbf{Z}[T]$, which follows from the very first description I gave of it as the base change of—we check that the power series ring is idempotent and we put that at infinity. And the idempotency is preserved.

So for example, using the idempotency of that, what is this thing here? It's just the homotopy fiber of the inclusion of this into this, and this is the base ring. And you can easily check that idempotency is equivalently to equivalent to the derived idempotency of this object. And that means that if you take this expression and you apply it again, you get the same thing back. So that's kind of an idempotent operation.

And again, the same idempotent will prove to you that if you take this operation and RHom from this guy, you get zero. So this thing is $\mathbf{Z}[T]$-solid. And that's not quite everything you need to check, but it's basically everything you need to check. It's an idempotent operation, and m is solid if and only if this map is an isomorphism. So it's just completely formal, and I won't write out all of the details.

Note: This is a formula for the derived solidification of a general $\mathbf{Z}\langle T\rangle$-module. But something nice happens if your $\mathbf{Z}\langle T\rangle$-module is base changed from a solid $\mathbf{Z}$-module.

And next claim is that the functor—the sort of pullback functor from $\mathrm{Solid}(\mathbf{Z})$ to $\mathrm{Solid}(\mathbf{Z}[T])$, sending a module M to $M \otimes_{\mathbf{Z}} \mathbf{Z}[T]$ and then you solidify—this functor is T-exact, preserves limits and colimits, and it sends $\mathbf{Z}$ to $\mathbf{Z}[T]$.

If we prove this, then in particular we get this claim, because this is that functor applied to product of copies of $\mathbf{Z}$. I claim the functor commutes with products, so then it's enough to understand what happens with $\mathbf{Z}$. But I already also claimed that $\mathbf{Z}$ goes to $\mathbf{Z}[T]$, so we'd be done.

Proof: We just take this formula for the solidification, and we plug in the case where m is also induced. We get that $(M \otimes \mathbf{Z}[T])^{LT,\mathrm{solid}}$ is the same thing as $m\langle T\rangle$ (by that I mean $m[T]/\mathbf{Z}\langle T\rangle$). And now we're computing an RHom over $\mathbf{Z}[T]$.

So the way to do that is to disambiguate the two occurrences of T and then equalize them at the end. So that's calculated as $\mathbf{Z}[[U]]$, and a two-term complex (I'll write it vertically so to speak). Then you equalize T and the shift operator on here, which is induced by multiplication by T

Multiplication by T is here given by sending U to zero, U^2 to U, U^3 to U^2, etc. So if you do that there, then that induces an endomorphism here, and that's the multiplication by T on this resulting thing.

Sorry, I missed what you said. What is the isomorphism to this? This bit of somewhat silly notation is the homotopy fiber of this map, or some kind of shift of a mapping cone, and that's the same as this. It's also the same as this.

Okay, and now we're done. Well, now this functor again—this is internally projective in $\mathrm{Solid}_{\mathbf{Z}}$, so this functor is t-exact and it preserves limits and colimits in $\mathrm{Solid}_{\mathbf{Z}}$. But limits and colimits in $\mathrm{Solid}_{\mathbf{Z}[T]}$ are calculated on the underlying level, because it's just part of a module category.

The last claim is that $\mathbf{Z}$ goes to just the usual polynomial ring in one variable. What happens when you plug in $\mathbf{Z}$ here? You're taking RHoms from this product of copies of $\mathbf{Z}$ to $\mathbf{Z}$. It turns into a direct sum of copies of $\mathbf{Z}$. You can check that what you get is just the usual $\mathbf{Z}[T]$, even with the $\mathbf{Z}[T]$-module structure. Really, there's a natural comparison map which you see to be an isomorphism.

Okay, so there was this—someone proved it's not related directly to the theory, but someone proved RHom from the infinite product of copies of $\mathbf{Z}$ to $\mathbf{Z}$ is a direct sum. But this is not—here you are doing it in—this is easier, yeah.

So we can also do other examples of such a thing. Let's do another example. That finishes the proof of the theorem, so we now have a grip on this $\mathrm{Solid}_{\mathbf{Z}[T]}$ theory. I want to advance the interpretation that the $\mathrm{Solid}_{\mathbf{Z}[T]}$ theory is like working over $\mathbf{Z}[T]$. Without the solidification, it's like the affine line, and $\mathbf{Z}[T]$ with the solidification is like the closed unit disc. That was kind of the interpretation that I started with.

But let's do an example again in non-archimedean—let's base change to a non-archimedean field and see what happens. So let's take another example above. Let's look at $\mathbf{Q}_p[T]$, so that's functions on the affine line. Now we want to restrict to the closed unit disc in the sense that we've just described, so we take the T-solidification of this.

This is an instance of this functor here, and I just said this functor has all the properties in the world. It commutes with colimits, so I can again take the $1/p$ to the outside. Sorry, it also commutes with limits, so I can take the limit over n of $\mathbf{Z}/p^n\mathbf{Z}[T]$, T-solidified, and then $1/p$ at the end.

Now we're applying this functor there to $\mathbf{Z}/p^n$, but that's just two copies of $\mathbf{Z}$. So you get the same answer as for $\mathbf{Z}$, it's just discrete. This is just inverse limit over n of $\mathbf{Z}/p^n\mathbf{Z}[T]$, $1/p$, or in other words, it's $\mathbf{Z}\langle T\rangle$, p-completed, $1/p$. These are the functions on the closed unit disc, kind of as desired.

Okay, so maybe I'll take a five The complement, and the complement was the open unit disc, or rather, the open unit disc moved to infinity. So that's this thing, and that corresponds to $\mathcal{D}^\circ = \mathrm{Mod}_{\mathbf{Z}((t))}$.

I think someone asked before, but I forgot that you had the solidification, which is $\mathbf{Z}[T]$. There should be another on the other side. Wait, wait, wait...

So then, this is just the natural inclu—well, it's a forgetful functor. A priori, you're forgetting the extra module structure, but it's in fact a fully faithful inclusion because of the idempotency. The idempotency of this thing means that there's at most one $\mathbf{Z}[T]$-module structure on any $\mathbf{Z}((T^{-1}))$-module structure on an $\mathcal{E}_{\mathbf{Z}}$-module.

So this is actually like a localization sequence, or what have you. This is the symmetric monoidal quotient of this by this thing, which is kind of an ideal in there. And what kind of adjoints do we have, and which of them are well-behaved, and so on?

Well, we know that this one has a right adjoint, which is given by the inclusion. But it also has a left adjoint. And the left adjoint is given by sending M to, I guess, the fiber. So you take M, and then you base change it to infinity, and you take that cone diagram here. So it's the fiber of M mapping to $M[T]$ over $\mathbf{Z}[T]$ with the $\mathbf{Z}((T^{-1}))$.

And this left adjoint is, in a sense, better behaved than the right adjoint, the naive inclusion. By the measurement that it's better behaved, well, they both commute with colimits, but this one satisfies a projection formula with respect to this fundamental functor, which is the symmetric monoidal functor. So this left adjoint is kind of linear over these two symmetric monoidal categories. So this is better.

I'm going to call this one j^*, and this one $j_!$, and this one j_*, this inclusion here. And on the other hand, for here, we have this functor here, which I'll call i_*, the inclusion there. So it has a left adjoint, also i^*, which is just the base change functor, which is symmetric monoidal. So that's kind of the more fundamental one from this perspective. But it also has an adjoint $i^!$, which is given by some arimum, but it's less well-behaved again, because here, this one satisfies a projection formula with respect to this one. So this functor is linear over this symmetric monoidal category, just like this functor is linear over this symmetric monoidal category.

"$*$ is an adjoint, the inclusion has ! on it." "It does, but that one we don't talk about." "Yeah, now this was the question I..." "Yeah, yeah." Because already this one's not as nice as, I mean, the good adjoint is actually up here.

So, the interpretation that this suggests—this is exactly like if you have a topological space X, and then you have Z a closed subset, and then U the complement open, then you get, if you have ∞-categories of sheaves, you have exactly the same thing. Where you have the open over here, you have this thing satisfies a projection formula, you happen to have this other guy, but it's not as well-behaved. And then you have i_* from $\mathcal{D}(Z)$, i^*, and then $i^!$. So formally speaking, it behaves exactly the same way. And actually, they're both special cases of the same thing, which is: you have a symmetric monoidal category, and you have an idempotent algebra in it, and it generates the whole situation.

In this case, you have this category and this idempotent algebra. Here you have this category, and then the pushforward of the structure sheaf from the closed sub, or pushforward of the constant sheaf from the closed subset. An You said that the rational opens are closed.

Yeah, for example a distinguished open is just the ring with the function inverted - that's quasi-compact.

The direct image of the structure sheaf is quasi-compact.

Okay, so now... Yeah, by the way, we're going to be guided by this sort of thing in setting up the definition of analytic stack and so on. One of the things we discovered is that when you move to this condensed/solid context, you actually get six functor formalisms in large generality on derived categories of "quasi-coherent sheaves". They really have nice interpretations.

For example, if you think of this thing as being what sits at infinity, then it makes sense that this is extension by zero. You're taking your sections and killing the ones that live near the boundary. It all plays quite nicely, and we showed how to give proofs of things like c-t-ness using these formalisms.

So we take this perspective seriously - the derived categories and functors between them are going to dictate to us what the geometry looks like.

There was another thing I remembered in the break that I forgot to mention. I said that this functor is t-exact and preserves limits and colimits, but I want to caution you that t-solidification from solid Z[T] is not t-exact. It's only off by one. I said it was given by RHom from this object which has a 2-term compact resolution. So it's only off by one from being t-exact.

It sends everything connective to something connective, and everything anti-connective goes to at most a 1-shift of something anti-connective. So t-solidification is not t-exact, but it's very controlled. The first map in the composition is t-exact, but the second is not. It's a bit funny.

Alright, so now I want to motivate what I'm going to do in the rest. Where are we probably going to go? We want to look at solid rings, i.e. commutative algebra objects in this tensor category solid Z. Generalizing our discussion when R is Z[T], we want to see subsets like closed/open unit discs. Of course, when you have a big algebra, you'll get many more such subsets.

We want to organize what you see in a nice way. It's not necessarily the most general thing, but basically we're going to take the things you see over Z[T] and base change them along all possible maps Z[T] $\to$ R, i.e. all possible functions in R.

What we'll end up with is the statement that the derived category of solid Z-modules localizes along the valuative spectrum of the underlying discrete commutative ring of this solid ring. You take the valuative spectrum of R viewed as a discrete ring.

The valuative spectrum is a residue field and valuation. The valuation doesn't have to be integral on R, just any valuation. It has a topology which induces the topology on things like Spa, but it's more general - sometimes one gets points which are not in Spa.

This space is similar to the usual spectrum of a commutative ring. It's a spectral space with a basis of quasi-compact opens, and even a particularly nice basis analogous to distinguished affine opens in algebraic geometry. While a distinguished open is parameterized by a single element, here you have to take a bit more data.

The basic opens are the so-called rational opens. You take finitely many functions $f_1, ..., f_n, g$ and form $\{x \mid |f_i(x)| \leq |g(x)|$ for all i$\}$. The interpretation is that you invert g, so we're inside the distinguished open for g, but then shrink further by requiring $|f_i| \leq |g|$.

Right, and so I'm saying that this localizes on this meaning. You have actually a sheaf of categories, a sheaf of symmetric monoidal categories. And what you're going to attach to this thing is a version of the solid theory. So you look at those M in $D(\mathrm{Mod}_R)^{\blacksquare}$ such that, well, first you want to say that multiplication by g on M is an isomorphism. And second, you want to say that if you take this internal hom from P to M, and you take $f_i^* \otimes [-1]$, this should be an isomorphism for all i. Or in other words, f_i/g. Sorry, so in other words, you want—so you think of all these guys as maps from say $\mathrm{Spec}(R)$, so to speak, to the affine line. Then you want that g lands in the standard codimension one locus, and you want that the f_i/g land inside the closed unit disc.

Okay, so I'm not going to go into details about that. But where should I go now? I don't know, maybe... Here, this $|P|_B$ just refers to the local correspond to... I mean, the only... I don't... You can think of it as a locale, but it's also a topological space. And the points have a nice description and so on.

Okay, so what do I want to do today? Well, or partly do today. So in particular, I'm claiming this category localizes along the space. And in particular, you get a structure sheaf on the space. And what I want to do in the next bit, so goal for rest of lecture, is make this structure sheaf explicit and compare, well maybe probably start to compare, to Huber's theory. So we're eventually going to produce this data by very easy formal means, but it requires some language and setup. So we can't do it yet, but I want to make at least this part of it explicit already at the beginning.

Okay, so let's see. Let me make a... So let's start with this generality. We have a solid ring, and let's take an element f in R, or really I should say in the underlying discrete ring of R in case there's ambiguity. And that in particular, well, that gives you a map from $\mathbf{Z}[T]$ to R which sends T to f. And then we can sort of see how these loci that we've identified can be, or correspond to, properties of R.

So let me make a definition. f is topologically nilpotent if this map, factors, I should say, through the power series ring. And f is power bounded if... Well, I want to say that if this map, well the map, geometrically factors through the closed unit disc. But the way to say that is, if—so let's say if R is actually, so R is an algebra over $\mathbf{Z}[T]$. In particular, it's a module over $\mathbf{Z}[T]$, and we can ask that it be solid.

In the first definition, the factorization is unique. Yes, it's unique if it exists because of this idempotency. So that's an interesting fact, actually. This is also the free module on a null sequence. And so even though there are sort of Hausdorff solid abelian groups that have like non-Hausdorff behavior, still, this limit is unique if it exists.

Okay, so basically in the definition of solid, you're imposing that certain limits exist uniquely, even though you have non-Hausdorff behavior. Okay, so that's the same thing as saying that if you take $Hom(P, R)$, $f^* \otimes [-1]$, that this is an isomorphism. That's just... Oh, someone's talking. Hello?

Yes, yes, the first condition is saying that it's a null sequence which is the p^n go to zero. So it's literally... Yeah, it's... I mean, I was going to explain the relation with classical definitions, but it's quite immediate for this one that it's the same as the classical definition. So maybe I'll just repeat what Peter said. So this is Sure, let me fix that for you:

Imprecise earlier, thank you. Yeah, in the sense of $\mathbf{Z}[T]$-modules is enough, or I'm not sure. I don't think so. Thank you for the comment. Yeah, just as rings, okay.

So, all right, here's a lemma giving basic properties. So, we write R° (this is again kind of standard notation in Huber's theory) for the set of power-bounded elements, and $R^{\circ\circ}$ for the set of topologically nilpotent elements, which is a subset of R. Well, yeah, I'm going to prove that.

The lemma is that R°, inside this ring here, is an integrally closed subring, and $R^{\circ\circ}$, first of all, is contained in R°, and it is a radical ideal.

So I'm still confused about this N-sequence. Something is an N-sequence in the sense that the map extends to another sequence, then you don't know that it factors as a ring map, that this is a ring map? That seems correct to me, yes. Okay, so it doesn't mean topologically nilpotent in your definition, in your sense. But on the other hand, maybe if you have something that comes from a Hausdorff topological ring, for example, if the target is quasi-separated, then independently of asking about the algebra structures, the limit is unique if it exists. Yeah, quasi-separated was this analog of Hausdorff in the condensed setting.

So still, if you start with something Hausdorff, then it is automatically a ring map if it is an N-sequence, yes, because of density. Okay, so we are not sure whether "topologically nilpotent" in the N-sequence sense is always the same as this other notion. Okay, Hausdorff, yeah, all right.

So, proof. Why is this a subring? Well, I guess maybe the first thing to check is that it has a unit (that's part of what I mean by "subring"). But if you look at the definition of "solid", that's one way of saying it. Put $f = 1$ here, okay.

Now, I want to show that if f and g are in there, I'll prove that it's a subring by showing that if you apply any polynomial to f and g, then you're still in there. So how can we do this? Maybe there are different ways, but I think the cleverest one is: We can look at the map from the polynomial ring in two generators to R which sends X to f and Y to g. Our hypothesis is that R is a solid $\mathbf{Z}[X]$-module and a solid $\mathbf{Z}[Y]$-module, and what we want to conclude is that for any map from the polynomial ring in one generator T, R is a solid $\mathbf{Z}[T]$-module, okay.

So first, resolve R by its resolution. It's definitely solid as an abelian group, so we can resolve R by direct sums of compact projective generators. But we know that R is a solid $\mathbf{Z}[X, Y]$-module. So if we solidify with respect to X, then what does this turn into? It turns into a direct sum of products of copies of $\mathbf{Z}[X, Y]$ by the properties of derived solidification that I proved earlier. But then it doesn't change R, so that's still a resolution of R. Then we solidify with respect to Y and we get a direct sum of products of copies of $\mathbf{Z}[X, Y]$ again by the same reasoning. So in total, we see that R can be resolved by these guys, but each of these is clearly solid over $\mathbf{Z}[T]$, because it's a colimit of limits of things which are solid over $\mathbf{Z}[T]$. This is solid over $\mathbf{Z}[T]$ because it's discrete, and then this is a product and that's a direct sum, so in total it's solid over $\mathbf{Z}[T]$.

We conclude that R is solid over $Z[X]$. Oh, I switched to T somehow. Switch to X somehow.

We conclude that this composition R is actually solid as a $Z[T]$-module. Oh, I didn't prove... I forgot to prove it's integrally closed. I'm sorry.

Let's do that. It's again a very similar argument. Let's say that we have an equation of the form $\sum c_i t^i = 0$ where all c_i are power bounded. Again, we just make the universal thing. We have $Z[x_0, \ldots, x_{n-1}]$ and over that we have the ring where you adjoin another variable, let's call it t, and then you set the equation $\sum x_i t^i = 0$.

We have our solid ring R and by hypothesis we have a map here such that when we compose to here, R becomes a solid module over each of the x_i variables. What we want to show is that when you compose here, it becomes a solid module over this t variable.

I'll just say it quickly in words. You use the same trick. You resolve R first as just a $Z[x_0, \ldots, x_{n-1}]$-module in solid A_{inf} groups and you solidify with respect to each of the variables. You find yourself built out of products of copies of this ring, but you're also a module over this, so you can tensor up to this. But that's a finite free module over that ring, so then that will just go inside the products and you find that you're resolved by a direct sum of products of copies of these guys. Because this individually is solid and solid is closed under limits and colimits, you deduce that that's solid as well. The key here is just that this is finite as a module over this, so that tensoring with it you can bring inside the product. Finite free, yeah.

Although, well, that's not really necessary. The ring's Noetherian and you can resolve... finite would be enough in fact.

I'll leave the rest to you. It's completely analogous arguments, like why it's an ideal and why it's a radical ideal even. Fun exercises in that style of argument.

So now we can describe this structure sheaf. Suppose we have again a solid ring R and then $g, f_1, \ldots, f_n$ in R^{solid}. The claim is that there exists a universal or an initial solid ring $R^{\text{solid}}_{g,f_1,\ldots,f_n}$ with a map from R such that first of all g becomes invertible in there, and secondly f_i/g is power bounded in $R^{\text{solid}}_{g,f_1,\ldots,f_n}$ for all i. That kind of encodes the idea I was talking about, where you want f_i/g to go to the closed unit disc, you want g^{-1} to be invertible.

The proof is you can just construct the guy. You could first invert g, but maybe I'll invert g at the end. What you can do is take R and adjoin polynomial variables $x_1, \ldots, x_n$ and then solidify with respect to all of them, which recall does something when R is not discrete. Remember we had the example of one variable and R was $\mathbf{Q}_p$, then this gave us the Tate algebra.

Then we can say that these variables are supposed to be f_i/g, so maybe $gx_i = f_i$ for all i. It doesn't matter at which point you invert g, but let's do it at the end. This kind of obviously satisfies the correct universal property. First we freely adjoin solid variables, then we impose the relations which guarantee that kind of thing. There's one small thing to check, which is that after you take this in which these are definitely solid and then you do these operations, you need to see it's still solid. But that's because it's all colimits.

So this is the kind of thing you're looking at. Now I want to make a caution here. This is not That means what you're producing a priori is a derived sheaf, not an ordinary sheaf. So that's okay, but now I'll say, in most practical cases, almost all practical cases, all $\pi_i = 0$ for $i > 0$. So in practice it doesn't seem to cause trouble, but it's an important thing to keep in mind.

The second warning—even if R is a very nice kind of Huber ring, then this quotient may not be quasi-separated. So it's a kind of an analogue of restricted power series in general. Yes, exactly. This will always be the usual Tate algebra in many generators. That follows basically from the arguments we gave for any sort of Huber ring R. But then when you take this quotient by the ideal generated by these elements, it might not be closed. So in principle you could be having a non-Hausdorff quotient here.

So when you say Huber ring, you mean complete Huber—yes, I mean complete Huber ring, thank you. But this theory is defined for complete Huber rings as well, or no? This theory is only defined for complete Huber rings, the theory I'm discussing, because you cannot define the condensed set of any topological thing. You can define it, but it won't be solid unless the thing is complete in general. Ah okay, you want it solid. So you want complete, and then the other guys, when you do this, it is still solid but doesn't come from—it's kind of an analogue of, yes.

Yes, but again, in almost all practical cases, or maybe all practical cases, it is quasi-separated, i.e., Hausdorff.

So what I was trying to aim for is the relation to Huber's theory. I guess I probably won't get there. So if we—I think we'll definitely discuss this in more detail later, but just as a preview: if R is a Huber ring (and don't worry if you don't know what that is, it's just a certain nice kind of topological ring that people use in non-archimedean analysis), and let's say complete—no, just Huber, I'm just doing the rings now, not the pairs. And if the ideal generated by $f_1, \ldots, f_n$ inside R is open, which is the condition that describes rational opens in the space of continuous valuations as opposed to the space of arbitrary valuations, then in this case Huber defines the ring of functions here. And you could ask what the relationship is with this thing that comes from the solid theory.

This is the quasi-separification of this more generally defined thing that we have here. So the solid thing, or maybe for emphasis I should put the $\pi_i = 0$ as well—you can get the Huber thing functorially from this more general thing, in particular from our structure sheaf. But they're not necessarily equal in general. In all practical cases, though, they are equal. That's the general outline of the story.

So in general, if you always use the structure sheaf, it always satisfies the—yes, so it's always a sheaf. Yeah, so maybe that's an important point to mention. There was this little fly in the ointment in Huber's theory, that in the general setting he defined a structure sheaf, except it wasn't a sheaf, it was only a presheaf. In all practical cases it was a sheaf, but still, the general theory was kind of missing something for that reason. That's fixed by this. If you no longer care about things being quasi-separated and non-derived, then you get a good plain old structure sheaf. And you get even more, you get the derived category of quasi-coherent sheaves which localizes. Also you get the possibility of defining all of these things even without this condition being present.

Okay, I think probably I'll stop there. Thank you for your attention.

Yes, do we have any use of discontinuous valuations? Any use? Yes, I know Scholze likes them, so he came up with this notion of adic spaces, and those correspond to certain—yeah, but you don't really use them. I think he used them, but you—oh me? Well look, I just build the theory, I don't—he calls them adic spaces, I don't think he developed the theory, he just kind of did it in an example. So he wanted to exactly include things like this open unit disc.

So this—yeah, things like the

This non-quasicompact thing, to fit it inside Huber's theory—but Clausen advocates that sometimes it's better to just work with this thing as if it were fine, and that's something that our theory easily accommodates.

Yeah, there are people who did—I forgot the name of this—and constructed some notion of rigid, I forgot the name. Some draw some paper on this, on like a variant of... I forgot the name of the... There was some paper some time ago, but I'm not sure.

Okay, any other questions? Yes?

So every solid $\mathbf{Z}$-algebra... Yes, so you're... Yeah, so this notion of analytic ring, we haven't defined it yet. It kind of organizes a lot of discussion, but indeed, if you have a... If you have a solid... If you have an algebra in solid $\mathbf{Z}$-modules, you get what we call an induced analytic ring structure. So it's just all... You take all, I mean, the module category you have, and that sits inside condensed $\mathbf{Z}$-modules, and that is an analytic ring. Yeah, but in the case of $\mathbf{Z}$, we had something more natural—more natural, it's arguable, it's more complete. There are two things. They exist. They do, I mean, you want them both, I think, and can be extended... Can be extended to any discrete ring, right?

Yes, that's right. Not in general, not... Not general solid $\mathbf{Z}$-algebra. Well, for a general solid $\mathbf{Z}$-algebra, I don't necessarily know of any, like, completely canonical... Well, like, maybe... I mean, one thing you could do is you could take all of the power-bounded elements and force all of them to be solid. That would be kind of the maximally complete thing that you can get via the stuff that we've developed. But there could be further completions of that, as far as I know. I mean, I...

You're welcome. Yes, with the definition of solid analytic rings we used in the very first lecture, if we use this instead of a regular ring here, do we get something similar to like Huber's theory for Huber pairs instead of just regular? Could you repeat the question? I'm not sure I understood.

In the very first lecture, we defined solid analytic rings. Yes, with the ring and the direct category. If we try applying something similar here to analytic rings, what do you mean? Ah, you mean like this discussion of this thing here? Oh, yeah, yeah, yeah. You can do that. But do we recover, like...? Oh, yeah, you recover the theory with the R^+ in there as well, yeah. So elements R^+, you require solidity. Exactly, not for all of the guys. Exactly. So you can, you can... Yeah, and we will discuss this. You get... You can add that extra flexibility into the picture, yeah. And the fact that $R_{\mathbf{Z}_0}$ is this is automatic, that those are the... They're automatically solid, exactly.

Yes, okay. Yeah, so it actually fits remarkably well with Huber's theory. Like, you take tensor products of analytic... You have to first solidify one another, have to take... Yeah, yeah, usually. Yeah, so here it's still the same. Here you don't have... Well, these solidifications all commute with each other, so there you don't need to... Yeah, they're all given by R mapping out of, out of something, and any two R mapping out of commute with each other.

Yeah, other questions? Okay, well, see you on Friday or next Wednesday or whenever.

8. Huber pairs and analytic rings (Scholze)

https://www.youtube.com/watch?v=dIwBTJNN7a0&list=PLx5f8IelFRgGmu6gmL-Kf_Rl_6Mm7juZO

Unfinished starting from 0:00

Right, so last time Dustin started talking about the relative solid theory and the relation to adic spaces. I want to kind of continue with that.

Okay, so I guess I want to talk about the relation between these basic objects that appear in Huber's work, that are called Huber pairs, and the kind of basic objects that appear in analytic geometry.

Motivation: we've seen several examples of a pair of a "present" notion of "complete" and the "same". The first one we discussed was the integers ($\mathbf{Z}$) and solid $\mathbf{Z}[1/p]$-modules inside of all condensed $\mathbf{Z}$-modules. But then last time, Dustin discussed the example where you take the polynomial algebra over $\mathbf{Z}$ in T, and then within all condensed $\mathbf{Z}$-modules, you can somehow just take the ones that are complete in the sense of this T. So you're taking modules over this algebra inside of solid abelian groups.

But then Dustin argued that it's actually quite natural to also isolate a stronger condition, giving the solid $\mathbf{Z}[T]$-modules. Geometrically speaking, this corresponds maybe to some kind of line, and this corresponds to the unit disk. In these cases, the condensed thing was actually just the classical thing.

But this notion of completeness is still interesting. You could also take, I don't know, $\mathbf{F}_p$ or $\mathbf{Q}_p$ or whatever, and then take solid $\mathbf{F}_p$-modules or solid $\mathbf{Q}_p$-modules. In those cases, these are actually just the ones where the underlying condensed group is solid. There's no meaningful way to strengthen this.

Okay, so the notion of an analytic ring captures this situation.

On the other hand, if you learn the adic stuff, then you run into this definition. I mean, the basic objects there are these Huber pairs. So let me recall what these are. These are Huber's definition, although of course Huber used different names.

A Huber ring is a topological ring A with an open subring A^+ and an ideal $I \subset A^+$ such that there exists some finitely generated ideal $J \subset I$... Because now it's not clear what "ideal" means, because there are two rings in place, J is an ideal of A^+, a subset $A_0 \subset A$ that has the same I-adic topology.

Let me give examples in just a second. Let me just finish the definitions.

And then Huber has this notion of a ring of integral elements. This is an open and integrally closed subring $A^+ \subset A$ containing A°, the power-bounded elements. Second...

And third, a Huber pair is the pair (A^+, A) satisfying these conditions.

Okay, so what are the examples to keep in mind? Let me first give some stupid examples:

Any discrete ring A is Huber, so in this case you can take $A^+ = A$ and $I = 0$. Any ring that itself is an adic ring for a finitely generated ideal I is Huber, so in this case you take $A^+ = A$ and $I = I$.

Maybe actually interesting is when you have something like a Banach algebra. So for example, $\mathbf{Q}_p$ is Huber, or any non-archimedean field is Huber. In this case, you take $A^+ = \mathbf{Z}_p$ and I generated by p, which really is only an ideal in $\mathbf{Z}_p$ and not in $\mathbf{Q}_p$.

So basically, the idea is that Huber rings are basically certain kinds of localizations or something like this of such adic rings with some adic topology.

And so also whenever you have any

Kind of Banach algebra over $\mathbf{Q}_p$ or some other non-archimedean local field. There's also always Huber rank. As a zero, you can take the unit ball in your Banach algebra, and as the ideal, you can take the one that comes from a uniformizer.

Remark: The completion of any Huber ring is again a Huber ring. We will generally only consider complete examples now. In the classical sense, allow all convergent sequences mod $\mathfrak{m}^n$.

If you start with a so-called ring of definition, one which has the I-topology for some finitely generated ideal I, then it will be the case that the completion of A^+ will be an open subring of the completion of A. This is just the definition of a Huber pair.

At least when one uses Huber rings and Huber pairs to do adic spaces, then the adic spaces associated with a Huber ring and its completion are definitionally the same. In this sense, non-complete Huber rings have at most a technical role.

We will only consider complete ones. So from the following, whenever I say Huber ring, Huber pair, and so on, I always assume that the underlying Huber ring is complete. Okay?

And so, last time, Dustin discussed some notions of topologically nilpotent elements, power-bounded elements, and so on. This can also be defined here.

If you have a Huber ring, then you can define topologically nilpotent elements and power-bounded elements in A. This is the set of all $f \in A$ such that $f^n \to 0$ as $n \to \infty$, which once A is assumed to be complete, is really just a condition.

And these are all the elements such that the set of its powers is bounded. It's actually equivalent to saying that it's contained in some ideal of definition, some ring of definition.

Actually, a different way to think about this set: First of all, you have these canonically defined objects, A^+ and $A^{\circ\circ}$. But on the other hand, we used these things in the definition that there is some ring of definition and some ideal of definition of the same. These will always consist of power-bounded elements, and the ideal consists of topologically nilpotent elements.

For example, in the case of $\mathbf{Z}_p$, $\mathbf{Z}_p$ is actually the A^+, and (p) is actually the $A^{\circ\circ}$. In this case, these inclusions are equalities. Of course, this can't in general be true, because you could also take as ideal of definition the ideal generated by p^2, but then you don't have all topologically nilpotent elements.

But in some sense, the A^+ gets, in fact, the collection of all such A^+'s, and the collection of all such I's. They form filtered collections, and A^+ is actually the colimit of all possible such rings of definition, and similarly, $A^{\circ\circ}$ is the union of all ideals of definition.

Alright, this was part one of the definition, together with a little bit of discussion. Part two was—oh no, sorry, there is no part two. I wanted to define, I wanted to say that if I also have a ring of power-bounded elements, I can define an A^+, but it's kind of weak because—sorry, okay.

So when you learn Huber's theory, at first, I think it's extremely hard to appreciate the significance of this ring of integral elements A^+. It is somewhat necessary to set up the theory, but it's kind of hard to feel why it's necessary.

But it actually turns out that the theory that we develop using the condensed mathematics gives you a very good understanding of what it actually does. Namely, precisely—here's an example.

I actually have several possible examples. For instance, you could take $A = \mathbf{Q}_p\langle T\rangle$, and in this case, A^+ is just $\mathbf{Z}_p\langle T\rangle$. For example, you could take just

So maybe I should give this definition of analytic rings. By the way, sorry, maybe I can make one notation remark. Huber uses the single letter A to denote the whole pair consisting of some topological ring and a ring of integral elements. We will follow him.

Also, when I discuss analytic rings, I want to use single letters to denote my analytic rings. But then they will have an underlying condensed ring, that's $\underline{A}$, and we needed some symbol to denote that underlying condensed ring. We didn't come up with anything good, so we chose to just follow Huber's lead. Okay.

So let's say $A^{\triangle}$ now some lightning bolt, and then I want to say what is an analytic ring structure on this thing. So what is an allowed class $\mathcal{C}$ of complete A-modules? Okay, here's the definition. It's equivalent to the one that Dustin gave in the first lecture, but presented from a slightly more elementary perspective.

Okay, so it's a full subcategory $\mathcal{C}$ of $\mathcal{D}(A^{\triangle})$, the category of condensed $A^{\triangle}$-modules, together with an A-module structure. That's the data I just said. Now I will make a lot of conditions on this, but those are conditions that we had already seen before, twice. We stated that solid $A^{\triangle}$-modules have a lot of nice properties, and it was a long, long list. Sometimes, because we don't want to state this list all the time, we make this definition.

So first of all, $\mathcal{C}$ should be stable under kernels and cokernels. But it's also stable, in fact, under all limits and colimits. All extensions, so if you have an extension of two things in $\mathcal{C}$, it should also be in $\mathcal{C}$.

Then there is a Tor-amplitude condition you want $\mathcal{C}$ to check. It's also stable under all $X \mapsto X^{\oplus I}$ for some set I. And $\mathcal{C}$ contains $A^{\triangle}$ itself.

[Dustin:] So can I ask a question? Does it imply, maybe one can prove from this in some way, for example, that $\mathcal{C}$ is a Grothendieck category? The condition that I allude to is the existence of a set of generators. Is it automatic under these conditions?

[Peter:] Yes, yes. Did you hear his answer?

[Dustin:] Yes, he said yes. And does it imply that the Ext groups in the subcategory are the same as the Ext groups in the full category?

[Peter:] No. I don't know what he said, but the answer is yes. I know the answer is yes, but you didn't hear... I mean, in this presentation, the derived category might not be in degree zero. So if you really phrase it at the derived level, you have to be slightly careful when you say that, right? Because it might not be the case that the thing I will define, the derived category of $\mathcal{C}$-modules, is the derived category of $\underline{A}$-modules. It's not. Dustin, do you hear what I say?

[Dustin:] Yes, I hear what you say. You said that in the category, the thing I will define as a certain triangulated category or stable ∞-category as a full subcategory of condensed $\underline{A}$-modules with some properties, which is the correct one. But in general it will not be the same thing as the derived category of $\underline{A}$-modules.

[Peter:] Yes, this is the heart of a t-structure. This is those whose homological groups are in this. So it doesn't imply that the Ext's are the same.

[Dustin:] It does not. And sometimes it does, sometimes it is true.

[Peter:] But like in most practical cases it will end up being true. In full generality, no. Okay. So I can proceed?

[Several people:] Proceed.

[Peter:] Now Dustin put me...made me confused. So I want to claim that there is automatically a left adjoint to the inclusion. So the claim is that the left adjoint—which I will write as sending a module M to its base change from A_∞ to A—mod A is just purely notational for now, but I will think of it as the modules over this analytic ring A.

This base change functor has kernel, the $\otimes$-ideal in $\mathrm{Mod}(A_\infty)$, and $\mathrm{Mod}(A)$ acquires a unique symmetric monoidal structure making the base change a symmetric monoidal functor.

Let's sketch the proof. We already discussed the existence of the left adjoint, which is formal nonsense. If it's not, just make it part of the definition. The question is about this kernel being a $\otimes$-ideal.

So what does it mean to be a $\otimes$-ideal? The left adjoint F definitely preserves colimits. To show it's a $\otimes$-ideal, we have to show that if we have something M in the kernel and N is anything, then $M \otimes N$ is still in the kernel.

Assume a module $M \in \mathrm{Mod}(A_\infty)$ such that $F(M) = 0$, meaning it has no maps to any A-module. We want to show that for all $N \in \mathrm{Mod}(A_\infty)$, we have $F(M \otimes N) = 0$.

This means showing that for all $L \in \mathrm{Mod}(A)$, $\mathrm{Hom}(F(M \otimes N), L) = 0$. By definition of F being a left adjoint, this is equivalent to showing $\mathrm{Hom}(M \otimes N, L) = 0$.

Using the Hom-tensor adjunction, this is the same as $\mathrm{Hom}(M, [N, L])$, where $[N, L]$ is the internal Hom. But we assumed $\mathrm{Mod}(A)$ is stable under all limits, in particular internal Homs. So $[N, L] \in \mathrm{Mod}(A)$. Then again $\mathrm{Hom}(M, [N, L]) = 0$ because $F(M) = 0$ by assumption.

The symmetric monoidal structure on $\mathrm{Mod}(A)$ has to be given by taking the tensor product in $\mathrm{Mod}(A_\infty)$, seeing this as a colimit in modules, and then completing again. The question is whether this makes the base change functor symmetric monoidal.

To check this, for all $M, N \in \mathrm{Mod}(A_\infty)$, we can either first tensor M and N and then apply F, or we can first apply F to both of them and then tensor in $\mathrm{Mod}(A)$. This has to give the same result.

I think a better statement is that if a map in $\mathrm{Mod}(A_\infty)$ becomes an isomorphism under localization, then tensoring it with anything else also makes it an isomorphism. This follows from the same type of argument, by mapping into the subcategory and using the internal Hom. I did this in the solid case, and it's the same argument here.

Then the point is that, for example, f is from M to its completion, which becomes an item of localization because an important operation. And so if I– this within some– the same– stage it.

So this is some structure you automatically have on a triangulated category C of modules, some kind of localization of condens– the underlying ring. And requires in terms of product. And now we pass– D– let not just say that a structure on an underlying lightens string a triangle.

Then undine the of a modules, the full sub-subset for all Z of all– let me still just call them N, so compx of modules. So group– let me think homologically, all the homo groups line. Define for Jesus.

And okay, so here– here's already the warning: there is a natural comparison function from the of mod A, but it's not always. And in essentially all, I mean basically, yeah, all cases I'm aware, it will come out to be an equivalence, but it's just not a general effect.

But yeah, so the good thing we are to focus on is the thing that we simply call D. And so the previous proposition has an analog on dou.

So E of A triangle triang– so I mean, probably in one or two lectures we will probably switch to the infinity categorical language, where we would say "stable infinity category" instead. For now, it's not really requir–, so let me just TR more classical terms.

Stable under all– so again, in stable infinity case we could say "stable under all limits and colimits", but general limits and colimits are not well behaved work Str categories. But you can say something equol– and stable under all s– and for product, which are well behaved to what they're supposed to do.

I'm trying to say, right, the inclusion again has left on, that I will call the dve S, the product. And again, this has a property that if you have something, it becomes an isomorphism here. Then if you T– it with anything else, it's still the same. And because this is now a triang c–, you can actually phrase this equ– in terms of the co–. So if you have something in the kernel of this, then you tend up with something sa– the.

And then again, this the T– here. So if one wants to to do the previous type of argument for this fact, then one lends into the question of whether– of course there is internal or in the full derived category by unbounded and so on. But is the question is whether if you have internal oh– from anything to something in $D(A)$, then it lies in $D(A)$, right? And this is not– because of unboundedness, I don't– of course you have a bounded complex, you have a spectral sequence. I mean, you still have to to to work with that. Here you have unbounding in both directions, I can see in one direction you have IND light conding is still repete. So the derived category is left complete and so I think you can control control the question. Okay, let let me do this in a second when I come to the pro s– the product maying face chang–.

All right. So see, you have– I don't know, M' to M, M', M'_1, M', and there a modules. And let's assume two of them, and by shifting it doesn't matter which to, are $D(A)$. Then we want to show that M is also to show. But for this we just look at long sequence. So we have $H(M')$ have H, and these are A. So if I have some quent here and have some cur– kernel right? And this is a kernel of the thing which a mod is a qu– of a modules. So this of both a modules and then this one is an extension, right?

Here we use stability on the kernels and cions, and then we use stability on extends. No I think standing here actually realize okay. So and the directed to the H, and not standing realize that possibility the product, countable ones, they definitely reduce to.

And then the uncountable ones? That was okay when we were working in the full condensed setting and preparing the lecture. I overlooked that there might be an argument to do here.

Dustin, should I just assume that there is a claim on the level of the categories? I'm sorry, I was busy with the chat. What's going on? Why is it stable under all products?

Why is what stable under all products? The subcategory of complete ones? Oh, all products! Instead of just countable products, right? Oh, all products exist, but is it exact in your category? Yeah, this is a problem. ∞-limits and all products are not exact. Yeah, this is a problem.

Okay, we'll have to think about this. It's not an actual issue in some sense, but I screwed up the definition. So we should ask for the existence of left adjoints. I mean, Dustin did in the first lecture and I just threw it out when I prepared the lecture. For existence of left adjoints. The definition, I definitely want the admitted left adjoints.

Um, sorry. All right, so now I made this next thing actually part of the definition that exists.

So, let's say M is complete. And then, is any condensed... No, sorry. What I want to show is N is in $D^-(A)$. And then there anything... Then you have to show that for all L in $D(A)$, $\underline{\mathrm{Hom}}(L, N)$ are complete.

And first of all, because ∞-sites are what's known as replete, this means it's closed under countable limits of surjections. This notion was introduced in my paper with SAG, but on the pro-side. And one thing we saw there is that this implies that any such... Sorry, for all K, I didn't use the letter K, right? So for any K which is, for example, a condensed abelian group or module, K is isomorphic to the derived limit of its truncations in degrees at most n. Some kind of limit, usually of abelian groups.

I mean, it's somewhat true, right? When you truncate up to some degree and then just take a limit of these things, you're somewhat stabilizing to the correct answer. In general, that's an issue because you're taking a countable limit here. In general, countable limits need not exist. But under this assumption, you can control them.

So this means that I can certainly assume that L is bounded here, right? What I need... So first of all, and to show this, I can again use the adjunction. And I assume that M has sheaf completion. So it suffices to show that the internal hom in condensed abelian groups from $\underline{L}$ to $\underline{M}$ is... Sorry, it's not complete, because then you can rewrite this as a hom from L into this guy. But I assume that M has trivial completion, so it doesn't have to do anything.

Okay, so this I want to reduce to the level where I kind of had the statement that if L is in $D^{-}(A)$ and M is in $D^{+}(A)$, then all the internal homs are in $D^{+}(A)$. The issue though, as already pointed out, is that here we need to ask this condition for all possibly unbounded complexes. That's why I mentioned this fact. So this at least allows us to assume that L is in $D^{-}(A)$.

So I can assume L is in $D^{\leq 0}(A)$. I usually put this going to the right. Because also all truncations, they are still in $D(A)$. But because the condition was just on the other hand, N can be written as a colimit of the truncations to the left. I mean, this is always true, that there's a colimit of truncations $\tau_{\geq -n}N$. And $\tau_{\geq -n}N$ is in $D^{\geq -n}(A)$.

This is much easier because colimits forgetful to direct sums are always good. And similarly, you can pull the colimit into a limit. And because we know Okay, I think that's fine. Once you have that, the existence of the tensor product is just the same formal diagram chase that I didn't execute previously, but did earlier.

Another thing I should have mentioned as part of the general theory, but didn't, is that $D^{leq0}(A)$ has a natural t-structure, making it a stable
$infty$-category. The left adjoint is not generally t-exact, as we've already seen that solidification could turn something unbounded on the left. Still, this left adjoint preserves connective objects.

A t-structure is where you have a notion of truncation of complexes, a notion of complexes which live in certain non-negative degrees or certain non-positive degrees, and they satisfy all the usual properties. We definitionally made this a triangulated subcategory which is stable under all the different functors.

So this inclusion is t-exact, and it's a completely general fact that if you have a left adjoint to a t-exact functor, at least it preserves the connective part. Let me check whether this maps to anything which is concentrated on the right, but this is a left adjoint, so you can compute the
$mathrmMor$ in the larger category. But then this is still in this category.

In particular, you can talk about the heart. The heart is also definitionally just A. If you take this and pass to the heart, this is A. If you take the tensor product and pass to the heart, in this sense the derived and abelian level are compatible.

Then there's the other question: if you start here and just animate all these constructions to
$infty$-categories, do you recover those constructions? This is just not true in general. In general, you don't even recover $D(A)$. Even if you do, there are separate questions about whether you recover the correct functors, and again, not in general. I think if you do recover the correct categories, you also recover the correct functors by functoriality. But the tensor product is a bit subtle. Again, in practice it is true that $C(A)$ is just the animation, and all these functors are correct.

With that out of the way, I'm almost done with my lecture, unfortunately.

Okay, back to the comparison. When we had Huber rings, we had these topologically nilpotent elements and so on. Dustin already gave a variant of this. First of all, Huber rings themselves are Huber pairs (A, A^{+}). These condense to rings of course. All this is actually fully faithful.

Actually, I should denote these as $A^{\blacksquare}$ and $A^{\blacksquare +}$ where the $\blacksquare$ means solid. Last time, Dustin already gave a definition that for a solid ring $A^{\blacksquare}$, we can define subsets $A^{\blacksquare\circ}$ and $A^{\blacksquare\circ\circ}$ of the underlying condensed ring.

Let me recall, $A^{\blacksquare\circ}$ was the set of elements in the underlying ring such that the corresponding map $\mathbf{Z}[T] \to A^{\blacksquare}$ factors through $\mathbf{Z}\langle T\rangle$. There was some discussion about how much structure you need to check here. The condition was that it factors as condensed rings. It's actually enough to check that it factors as condensed modules over $\mathbf{Z}$.

Then $A^{\blacksquare\circ\circ}$ was defined as the set of elements f such that there is a sequence $(f_n)_{n\geq 0}$ with $f_0 = f$, $f_{n+1}^p \in f_n A^{\blacksquare\circ}$ for all n, and $f_n \to 0$. This comes together with a smallness condition that f times the shifted sequence makes it I-adically Cauchy.

If you apply this to the case where this solid ring $A^{\blacksquare}$ arose from a Huber ring, then this is precisely the set of topologically nilpotent elements, and those define the $\circ$
$circ$-elements. When you regard

Is precisely the same thing as $H \subseteq A$ being power-bounded. Dustin showed last time that this is always an integral statement.

This here is always a what's the definition? Yeah, sorry. Given f, I can again—let me write again why this means I can "a" and then the condition is, I'm already speaking of modules over C, modules over the closed unit disc, as we motivated last time. This means that $|f(a)|$ should be at most 1. So it should be also power-bounded. Okay.

But now, I can also make the point: Assume A is an analytic ring structure on a solid R-algebra. Then I can also define an A^+. I realized I didn't define this, so let me do this in just a second. Such that the map from $D(T) \to A$ is the same as always.

This map induces a map of analytic rings, from $\mathbf{V}$ (the corresponding solid module). Yeah, that's precise.

So something that I should have said previously but forgot. Let $\phi\colon R \to S$ be a map of condensed rings, M an S-module, and N an R-module. Then a map of modules $f\colon M \to \phi_* N$ (where ϕ_* denotes restriction of scalars) is equivalent to a map of condensed S-modules $\phi^* M \to N$ (where ϕ^* denotes base change). In this case, you can pass to the left adjoints on the level of derived categories, because you can check it on the level of modules.

Once you pass left adjoints, the left adjoint to restriction of scalars (i.e., what I term the base change functor) becomes the left adjoint here. So you get it also left adjoint here, which is base change. If you want, you can compute it by first base changing as condensed modules and then completing it. You also get a derived functor.

Okay, so the claim is that first of all, once you have such an analytic ring, you can get data as in Huber. I will immediately check that this actually automatically satisfies his list of conditions that he puts on his ring of integral elements.

Conversely (I'm not sure if I have time, I hope I can say it), whenever you have a ring of integral elements in Huber's theory, you can actually produce an analytic ring which is in some sense the initial one.

Okay, so right. First of all, I can also rewrite this power-boundedness condition. It's all those s such that $1 - sT^*$ (which is an endomorphism) maps $D(T)$ into A^+.

Changing notation: P was this "P" called—it's always the spectrum based on the normed R-algebra. And we characterized being solid over $\mathbf{Z}[[T]]$ by, well, being solid (but this we already asked), and that $1 - sT^*$ is a morphism on this projective generator.

So what's actually to ask is that if I look at this thing here (an object in $\mathcal{D}(A)$), then this is a "why". If I admit such a map, then this already happens here—like, already here, $1 - sT^*$ becomes an automorphism of this object (definitionally). But on the other hand, because this precisely characterizes the solid modules here, you can also show the converse.

Like, if I want to show that I have such A^+, I need to show that all complete A^+-modules here restrict to complete A-modules here. But being complete precisely meant that if I tensor $1 - sT^*$ into there, it becomes an isomorphism. And so just translates.

So basically, whenever you have any element s of your underlying ring, you can ask this condition that $1 - sT^*$ becomes a morphism of P. And this will define for you an analytic ring structure by general theory. Yeah, basically whenever you have an endomorphism of your complete projective object P, declaring that this should be an "as morphism" for all s in the ring structure—so you can take Modules... But then probably it's equivalent to what you have written, if you think about it, but in the derived way. I just want to confirm that the two versions are equivalent. Yes, right? Because you can actually detect analytic R-structures on the level of modules. It's enough to check it on the underlying level, the ind-level. That is also true, yeah. Okay, so it's equivalent.

I'm... Uh, yeah, so with derived I would be more confident, but I think the argument just sketched goes through. So here... Right, so the point is that this subset A_L automatically satisfies the conditions. It always contains the topologically nilpotent elements, which are always an open subset. $\{u \in R \mid \sum_{i\geq 0} a_i u^i$ converges$\}$ in particular, this is open. When R comes from $\mathbf{Q}_p\langle T\rangle$, it's always containing the power-bounded elements, and it's always integrally closed.

So why is that? Well, if $f \in R\langle T\rangle$ is an element, then we actually get a power series... That's almost... Yeah, if I have a module that's actually $\mathbf{Z}_p[[T]]$-module in Solid_A, that's automatically p-torsion-free, actually. I mean, this proof is exactly the same as last time, just because all I used was arguments about modules being solid over one ring implying they're solid over another, and so on. Yes, let me just state it again. Okay.

So these are actually all there. But this means that whenever I have a module here over A, it in particular becomes a module here, which is solid. Everything is solid. So it must be an underlying solid ring.

If A^+ is integrally closed, then I get a map from here, $\{a \in A \mid \forall M \in A\text{-Mod}, M \text{ solid} \implies aM \subseteq M\}$, to A. But in particular this means that A^+, the underlying condensed ring which we always assume is a complete $\mathbf{Z}_p$-module, by restriction... So if f is an element in A, right, so this group scheme inclusions $A^+ \to A$... And therefore, something that's the same argument. So in fact, yeah, the argument that Dustin gave there was already talking not just about a triangle, but about any module. And so if you just run his argument, to see this is what he actually...

Okay, so... Right. Therefore, if A is any triangle, I have the solid analytic R-structure $\mathcal{A}_1$ which lives over $\mathbf{Z}_p\langle T\rangle$ and consider this morphism of rings of integral elements. I just gave you a recipe here that was taking some such analytic ring structure here and produced a ring of integral elements A^+ in here.

And it's actually functorial. So you can actually show that $f : A \to B$, one arrow is contained in the other. Yeah, I mean, if you have... And you get an inclusion of the analytic rings, and this actually has a left adjoint, I assume.

So whenever I have a ring A, so I can produce an $\mathcal{A}_1$-ring structure on the solid A-modules. Yes, so Solid_A admits an $\mathcal{A}_1$-ring structure. It's unique because the ring of endomorphisms is unique, and then it's just a condition. It's just a condition that all the A-modules are actually solid.

A different way to phrase this is to ask that $1 - \varphi$ is an automorphism of $A^+ \otimes \mathbf{Q}_p$. It doesn't matter.

All right, so I wanted to say that it has a left adjoint. So if I have a morphism of pairs $(A, A^+) \to (B, B^+)$, then I can send this to the ring B associated where mod B^+ by definition, all those

Usually there's just one or two elements, something where you really have to check. All the same thing as those modules, and only for those two elements.

So yeah, to connect to the beginning, for example, like $Z[Z]$-pairs, these are just $Z[Z]$-modules. If I take Z, that's the pair. So if I only put Z here, then I'm only asking that it's Z-solid over Z. So I take all $Z[T]$-modules and Z-solid Z-modules. But then when I take this, it becomes R_0 for A^+. So let me just state one last proposition.

So when you start with H, and then go to A^+, then if I go back, this actually matches back to A^+. So if I write, I have an analytic ring and then I can take its plus ring. So this is actually a plus equivalence, the left adjoint functor, from R_0 to A^+.

So all brings, but they're all—and yeah, I mean, I'm really still quite struck by how closely the theory of solid analytic rings really matches Huber's classical theories. If you restrict to analytic rings where you only allow yourself to put conditions that one minus some element times the shift operator on T becomes an isomorphism, then you're precisely getting those analytic ring structures that are induced by rings of integral elements in Huber's sense. So which is kind of very strong aposteriori motivation for this definition.

All right, I should stop.

9. Localization of solid analytic rings (Clausen)

https://www.youtube.com/watch?v=lJTLj8gYAtg&list=PLx5f8IelFRgGmu6gmL-Kf_Rl_6Mm7juZO

Unfinished starting from 0:00

Welcome back, everyone. Today, we're going to be doing a little bit of localization in the setting of solid analytic rings. But first, I want to start with a recap of last time.

Last time, we defined the notion of an analytic ring, which is a pair $(R, \triangle R)$ where R is the whole object and $\triangle R$ is the category of R-modules. This $\triangle R$ is a full subcategory of the category of R-modules, satisfying some closure axioms. Specifically, $\triangle R$ is closed under all limits, colimits, and extensions. Additionally, for any M in $\triangle R$ and N in $\operatorname{Mod} R$, the internal Hom from M to N also lies in $\triangle R$. Finally, the unit R itself lies in $\triangle R$.

There were a couple of technical points that came up last time that I'll take the time to address now. One was regarding the existence of left adjoints. There was a claim that this was automatic, justified by a theorem of Adamek and Rosický called the "reflection principle." This states that if C is a presentable category and D is a full subcategory of C closed under all limits, and if there exists a regular cardinal κ such that D is closed under all κ-filtered colimits, then D is presentable and the inclusion $D \to C$ has a left adjoint.

I also want to make another remark concerning a technical point that came up in the last talk. Recall that we were discussing the derived analog of this definition, where we defined the derived category of $\triangle R$ as the full subcategory of the derived category of $\operatorname{Mod} R$ consisting of those objects whose homology groups all lie in $\triangle R$. We wanted to show that this derived category satisfies analogous closure properties under limits and colimits. The key point for proving closure under limits was to show that the product of a family of objects in $\triangle R$, viewed as objects in the derived category concentrated in degree 0, still satisfies the condition of lying in $\triangle R$. The subtlety here is that while countable infinite products are exact in the setting of condensed abelian groups, arbitrary infinite products are not. So the product functor has right derived functors, and we need to ensure that the product lies in $\triangle R$ for all degrees. This can be proved by considering the direct sum instead of the product.

Alpha, then this guy is a retract of $\mathbf{R}^I$ product α in M, because termwise it's obviously this system is a retract of this system here. But this is just the same thing as $X \in \operatorname{Hom}_M(\oplus_I \mathbf{R}, M)$. Okay, and what about product of infinite complexes, unbounded below complexes? Yeah, the argument given in the last lecture, I mean this was treated in the last lecture, ah, because there are kind of it's possible, so that you can reduce to countable. Exactly, okay.

So that's that, and I think also that I don't know, maybe you know the reference. So if you have in general a category $\mathcal{C}$ and subcategories $\mathcal{C}_i$ that are closed under coproducts and filtered direct limits, then in the derived category when you consider objects whose cohomology lies in the subcategories, this is also present. This, I think, can be shown. I don't know the reference for this, I don't know. Okay, but this is just an elementary thing and you can do it for complexes, and I think this can be used to give a less...I mean, once you know this, which actually one can formulate in terms of every complex being the limit of smaller ones with some bound, then this can slide well.

Yeah, I don't think, yeah, to this without using the, I don't think it's necessary to use this. I mean, actually you can kind of explicitly construct the left adjoint on the derived level just by taking left derived functors of the left adjoint you have on the abelian level. Well, maybe it doesn't quite work like that, but you can. Well, yeah, I'm willing to believe that the ∞-category version can be avoided anyway. Let's... you can first... the actually presentable, because you're just... that all the... right, right, right, right, yeah, category. Yeah, that's a good argument, yeah, because this is not necessarily the derived category of Mod_R, but what Peter said was that there's a general principle about presentable categories being closed under limits and the category of categories as long as you have functors that commute with colimits, and the homology functors commute with colimits, so then you can see that this thing has to be presentable, and then the ∞-categorical adjoint function theorem, the more naive version proved by LRI, would give you the left adjoint. Okay, anyway, enough of that.

Right, uh, let's move on to some real math. Okay, so and when you did before without light, you did everything, so is there a close relation between the notions in the light and the general set, that is, so a condensed ring in the light gives one in the general, and subcategory, subcategory, and so on, maybe, or is it more twisted? Well, the first statement is completely accurate: light condensed rings embed fully faithfully into all condensed rings. But when it comes to the analytic ring structure, it's a little bit more subtle.

In general, R^+ is an integrally closed subring containing all the topologically nilpotent elements. We saw that if you have a solid analytic ring structure on a solid ring, such that every module over it is solid as an underlying Abelian group, then the collection of f for which this is an isomorphism for all M is actually an integrally closed subring containing all the topologically nilpotent elements. So you could always just throw in these guys, take the subring generated by them, and take the integral closure, and that will not change the theory.

Okay, and then there was an example. If this is a Huber pair, meaning this is a Huber ring, then the integrally closed subrings R^+ like this are the same thing as the open integrally closed subrings, and these are exactly the R^+'s in Huber's theory. So the general setup we have for an arbitrary solid ring of the possible choices of R^+ when you specialize to the case of a Huber ring, it recovers exactly the choices of R^+ you have in Huber's theory.

From now on, whenever I say Huber ring, I'll mean complete Huber ring unless otherwise specified. There is also sometimes people use derived complete things, and if you have a derived complete thing, it also seems to give a condensed thing, and then it's also possible to say that this is also solid. Yes, actually I might talk about that fun story later in the lecture.

Moreover, in the Huber case, the R^+ is actually recovered from the analytic ring structure—it's basically equivalent to the $R^\triangle$ in Huber's theory. In the general case, I didn't quite claim that if you start with an integrally closed subring satisfying these conditions and you form that theory, there might for some other reasons also be other things that f that satisfy this property for all your M, but in the Huber case, you can show that there aren't—we didn't do it last lecture, but it was done in a previous lecture.

Okay, questions. Now we're going to discuss localization. Let me make a remark which might be a bit shocking at first glance, but it's actually trivial. If R is a solid ring, and we have this R^+ satisfying these conditions, and again you can feel free to assume it's an integrally closed subring containing the topologically nilpotent elements, note that this condition defining the analytic ring structure is just a condition that you're imposing for all f in R^+, and R^+ was by definition a subset of the underlying discrete ring R. So all the data that you're using to define the analytic ring structure actually already appears at the discrete level.

Then you get another pair, just with the same R^+ and the power-bounded elements in the discrete case are just all the elements, so certainly it's still going to be power-bounded inside there. And this observation shows that if you're interested in solid modules over your original R, R^+, you can take the ones over where you have a discrete ring, and then that already has all of the information about the analytic ring structure. And all that remains is to observe that R will be a commutative algebra object in here, and you just kind of abstractly take R-modules in this Abelian category. It's important to note that since we're doing condensed modules, even when you have a discrete ring, you have a huge amount of

Is actually base changed from the discrete case in this completely naive way. Okay, so we're going to discuss localization. How these categories glue, but it's actually going to be sufficient to treat the discrete case, because if you understand how this category glues, then you could just put the R-module structure on top of that and you'll understand how this category glues. And I want to stress from the beginning that I'm talking just about one kind of example of gluing—I'm not claiming this is the most general, but it is nonetheless quite general. It's just a certain framework for gluing, you can call it.

So now, let me make an analogy. So we're going to be in the world of discrete rings now. If R is a commutative ring, then we have its usual derived category of R-modules. This localizes over the Zariski spectrum of R, and I'll say more precisely what that means in a second.

What is this $\operatorname{Spec} R$? It's the set of prime ideals, and there's a basis of quasi-compact opens closed under finite intersection—these are the so-called distinguished opens. Let's say U_f are the set of prime ideals P such that f is not in P, so f maps to something nonzero in the residue field. There's a structure sheaf, and its value on this distinguished open U_f is R_1/f, the localization of R at f. It's important to note that neither U_f nor R_1/f determine f, but they do determine each other. In fact, U_f gets identified with $\operatorname{Spec}(R_1/f)$, and this matches up the distinguished opens.

If V is contained in U, then you get a base change functor. The theorem, which is completely classical, is that this pre-sheaf is actually a sheaf of ∞-categories. In this setting, you also have a sheaf of abelian categories, and even a hypersheaf, but I'll just focus on the derived categories here. The key difference is that in the setting I'm about to discuss, the localization maps won't be flat in general, unlike in the discrete case.

So, this is in L also. First of all, what I said should be correct, I think. I'm sorry, my brain is a little not working very well right now. I actually zoned out while you were talking. My apologies. Do you have any object, and you have any object, in the right category of the topos restricted to you? Val, in the sense, yes, then this is a hypersheaf, no, chief, yeah, hypersheaf, well, why would it automatically be a hypersheaf?

No, I think I was able to do it in some classical, more classical formulation, but it's a... Is it? So, what do you know about this statement? Oh, yeah, the usual derived category. I maybe it is, maybe it is a hypersheaf, yeah. I don't know, I don't know. I mean, I don't know the statement. It's, I guess, now that I think about it, it sounds plausible, but I mean the hypersheaf, but there's certainly Lurie proves it's a sheaf, yeah, and hypersheaf, maybe, I don't think proves it in one of his books, yeah, I'm sure.

Yeah, there is also something that I saw that you mentioned, I mean, in some text that they found on the internet, instead of looking at the derived category, you can look at Fun_∞ or Shv and this is the same as the derived category of the hypersheaves, if you do hypersheaves with values in the derived category of abelian groups, that's the same thing as the derived category of the category of sheaves of abelian groups. Yeah, and this is also proved, I assume, somewhere in Lurie.

Now, I'd like to move on. So, maybe we have this discussion at another time. Okay, so that's one part of the analogy. And the second part is, so now we have this $\mathbf{R}$, $\mathbf{R}^+$, a discrete Huber pair. So, that just means $\mathbf{R}$ is an ordinary commutative ring and $\mathbf{R}^+$ is an integrally closed subring.

Well, then we've assigned to this this $\mathcal{D}_{\mathbf{R},\mathbf{R}^+}$ solid, and the claim is that this localizes on something else, on the valuative spectrum of this pair $\mathbf{R}, \mathbf{R}^+$. Okay, so what is this?

So, that was the set of prime ideals, and the kind of purpose of a prime ideal in this setting is to let you know where functions vanish or don't vanish, so kind of you could think of it that way, so kind of a binary condition of whether you're zero or nonzero. And in the valuative spectrum, you are allowed some more refined information, not just information about whether a given function vanishes or doesn't, but given two functions, you can ask whether one is bigger than the other. And the way you can measure that is by means of evaluation, so this is a function from $\mathbf{R}$ to $\Gamma \cup \{0\}$, where Γ is an abelian group written multiplicatively. And then there are axioms, like multiplicativity, $v(fg) = v(f)v(g)$, and the non-archimedean condition, $v(f+g) \leq \max\{v(f), v(g)\}$. And we also involve the sub

Okay, so the point here is that we now have a much bigger category, and there's more flexibility for how to localize. It connects with this classical discussion of valuations. If you've never seen this before, then you can look, for example, at the rational numbers. Maybe you know the classification of valuations there. There's the trivial valuation, which I guess corresponds to equality, where for every prime ideal, you have the trivial valuation, where it's zero if your element is zero and one otherwise. But then also for every prime p, you have a p-adic valuation.

So you have the generic point of $\operatorname{Spec} \mathbf{Z}$, you have the special points of $\operatorname{Spec} \mathbf{Z}$, but then you have these things in between, which are nearby p but not equal to p, these p-adic valuations. Oh, sorry, I was talking about $\mathbf{Z}$ not $\mathbf{Q}$. But then the fact that you can classify those is kind of misleading, because once you add an extra variable, then all of a sudden things explode, and there's many different kinds of valuations, basically because in a surface, you can have lots of different kinds of curves passing through a given point, and you have valuations of so-called higher rank, which introduce additional complications into the theory.

I'm not going to go too much into this, but yeah, so I'll stick to mostly formal aspects for now. Okay, so let's continue the table of analogies.

So we had $\operatorname{Spec} R$, and we had this particularly nice basis for the topology, quasi-compact, closed under finite intersections, and each of them was also of the same form as the global guy, just for a different input datum, R_1 over F. And we have the same thing here. We have a basis of quasi-compact opens, closed under finite intersection, and these are called the rational opens in this case. They depend on the choice of some elements in your ring. You choose $F_1, \ldots, F_n$ and G inside your ring, then you can form this thing, and what is it? It's the set of those valuations V satisfying all of these conditions, such that moreover $V(G)$ is non-zero, and $V(f_i) \leq V(G)$ for all i.

So in some sense, it lives inside the distinguished open, the Zariski open, given by just deciding G should be non-zero, and then we use this extra flexibility of we can also impose inequalities, so we're shrinking this Zariski open a little bit using some inequalities, and we still get an open subset. Okay, continuing.

So there's a structure sheaf, but actually, there are structure sheaves. On this $F_1, \ldots, F_n$ over G, you have one thing which just takes the Zariski localization, but then you also get a choice of integral elements, and that you get by it's going to be, it has its going to be a subring of here, and you get it by taking the integral elements you had before, or rather their image in there, and then adjoint, and then looking also at these elements $F_1/G, \ldots, F_n/G$, and then that might not be integrally closed, so you take the integral closure. Basically, you just look at all of the elements which the valuations in your open subset think should be less than or equal to one, so you've kind of already have it for this by fiat, and you forced it for these, and then the collection of those things is an integrally closed subring.

And then again, you have this nice recursive property that $U(F_1, \ldots, F_n/G)$ is just the same thing as the valuative spectrum of $\mathcal{O}_U^+$, and this matches up rational opens. And here is another place you can see the kind of necessity of including the data of this $\mathcal{R}^+$ in the general theory, because if you could have said, "Okay, well, I want a bigger

Let me put this: If U is contained in V, then we get the pullback map. In fact, there's a map of analytic rings from $\mathcal{O}_U$ to $\mathcal{O}_V$ in the sense of the previous lecture. So we have a map of condensed rings, which is just in this case a map of discrete rings, such that if you have a complete module here, then when you restrict scalars, it's also complete here. That's the kind of forgetful functor, and then that always has a left adjoint, which is this base change functor. Explicitly, you get it by taking your module here, abstractly tensoring up from this ring to this ring, and then re-completing in this theory here.

The theorem—I've kind of run out of space, but maybe I'll put it. This precedes that one over there is a sheaf of ∞-categories, and I'll put the warning that this is not true on the level of abelian categories. In contrast to the classical case, these pullback functors are not t-exact in general, because the pullback involves a solidification, which, as I said, is not a flat operation and does bound topological dimension. Yes, it does, it'll be bounded by n, the solidification is bounded by—I mean, the homology is zero up to n, yeah.

Okay, so I think we'll take a 5-minute break before I get to the proofs. The proof—is it complete or not? Probably not in general, but there's an abstract result that if the space has finite cohomological dimension, then hypercompleteness is automatic. This is sometimes useful. If I start with a solid ring, I can take its underlying set, which is discrete, and we mentioned it's the same as the module versus the condensed thing. Is there a case where this is actually a point of this algebra? Hmm, I don't think so. So these tend—for example, like, I don't know, $\mathbf{Z}$. We showed that the $\mathbf{Z}$ power series T is idempotent over the $\mathbf{Z}$ polynomial T, but this discrete ring is going to be way too big. I think this is not going to be idempotent; there's no extra reason why this should be, I mean, I didn't think about it carefully, but I would assume the answer is no.

Okay, so I've stated the theorem, and now I want to explain the proof. But to motivate it, I'll give a certain proof of this classical theorem here. There are many different possible arguments in the classical case, especially because these localizations are flat, there's lots of flexibility in how you set things up. But I want to describe a particular argument for this claim here, which will kind of translate over without too much difficulty to this case here. So let me erase some boards. There was maybe one remark that one can make in both settings that I forgot to make. I said I defined a sheaf of ∞-categories on these rational opens. Okay, not every open subset is rational; they're just a basis for the topology. But there's this general result that when you have a basis for the topology closed under finite intersections—that condition being actually necessary in the ∞-context—then a sheaf on that basis uniquely extends to a sheaf on the whole space in the naive manner of taking limits of an arbitrary open. Yeah, so we're only describing this sheaf of categories on the rational opens, but after the fact, you get also a category attached to an arbitrary open, whether or

Okay, this is a very simple example of two elements which generate the unit ideal inside this ring. The claim is that if you want to check something as a sheaf, you only need to check the sheaf condition in this one specific situation. This was originally in Quince's proof of the—anyway, it's easy, but there was something of Qu and he proved the cell conjecture.

Okay, it reduces to the fact that if you have some vector bundle on a fine space over a ring which is derived over a local ring, then it is extended from the ring, and he did it by reducing to this, and it was a bit trickier. Quince is a clever guy, so let's give the proof.

Well, I said you know we can describe algebraically the covers. If you, in general, the covers would be described like this: you take F_1 up to F_N in O generating the unit ideal, such that there exist X_1 to X_N in O with $X_1F_1 + \cdots + X_NF_N = 1$. And the general cover is the U of F_i—oh, darn it, I can't believe I didn't think of that, will not be okay, because you cannot generate the empty set from non-empty things. Damn it, I should know better by now.

But, um, plus empty cover of empty set. Okay, checking the sheaf condition there just means you check that the value of your sheaf on the empty set is the terminal object in the category that is the target of your sheaf. Okay, so that usually can be done without much difficulty.

Okay, any question or comment from Bon? No? Okay, but note that this cover here is refined by another cover where you take $F_i * X_i$. This is a smaller distinguished open, and those still generate the unit ideal because of the same expression. So then we can assume just that $F_1 + \cdots + F_N = 1$, and then you can do an induction on N.

Here, we have an object in the derived category and an object in the DED category, and then you give yourself extra data of an isomorphism between them. But it's not an isomorphism in the usual derived category, it's an isomorphism in some infinity version. So you can imagine, for example, if this is represented by a complex of projective objects, then you'd actually want to give a chain homotopy equivalence between their images.

Let's say they're bounded above just for simplicity, and then you make an infinity category out of that. So you define some notion of chain homotopy there, and so on.

Right, so then what does essential surjectivity mean? It means you can glue in the derived category. If you have a chain complex here, a chain complex here, and an explicit identification between them, maybe you choose some quasi-isomorphic models and make a chain homotopy equivalence between them. Then that collection of data uniquely comes from an element here, up to quasi-isomorphism. So the point being that you actually have to specify the data of the chain homotopy equivalence here in order to get the well-defined object there. That's the essential surjectivity.

The fully faithfulness says something else. It says that if you have two objects here, and you want to know the homs between them, so you can think of calculating Ext groups, for example, the Rhoms between two objects here, you can get it by base changing here and taking Rhoms, base changing here and taking Rhoms, and then doing a homotopy pullback of those complexes for Rhoms.

Okay, so that's kind of how to think about this result. It lets you glue objects that are defined locally in a derived sense, but it also lets you do global Ext calculations by localizing.

Okay, but how do you formally prove such a statement? Note that each base change functor has a right adjoint, which is just the forgetful functor, from the derived category of R_1 over F to the derived category of R. And then it actually follows formally that this functor also has a right adjoint.

You can explicitly describe what this right adjoint is. If you have a pair M, N, α where α is an isomorphism, you just apply the right adjoints to each of these objects and then take a limit. So you take M crossed over N with M_1 over F, which is the same thing as N_1 over F.

Okay, so the trick to get used to conversing by two op is just to have an easy diagram, yes, in principle you could also do this argument without doing the reduction, but it's certainly easier to talk about it this way, because it's finite many intersections.

I think in the end, once you get to the statement we're trying to prove with the valuative spectrum, then you really probably don't want to - well, I don't know, maybe you could organize it cleverly, but I think doing the reductions makes it much easier.

Okay, right. This is good news, I mean, this is the great thing about proving something as a sheaf of categories—you have an automatic candidate for the inverse, it's some right adjoint. So you have a functor you want to prove is an equivalence, you have a right adjoint, that means you have a unit and a counit you need to check are isomorphisms. So then you need to check, one of them will be a map in this category, and one of them will be a map in this category.

For example, for the unit, you need that if M

Okay, so then the proof of the solid analog. You use the fact that when you invert $1 - f$, this is the intersection of the two, that is, those which are—yes, yes, yes, yeah, you want to know that when you pass to right adjoints, this thing is just the intersection of those two things as well. Yeah, maybe I should have added that to the list. Um, yeah, thanks.

Well, we have to understand it in a sensible way, I guess you're right. And of course, we have to know the language of $\mathcal{L}$ to make it precise. Yes, so it's not—maybe to well, I mean, it's the right adjoints are fully faithful, so it really is just kind of an object-wise condition you could say, just—yeah.

Okay, so what's the analog? So again, we have the site of rational opens U in this Valuation spectrum, and the Grothendieck topology, open covers. And then we have a lemma that this topology is generated by—the empty set cover, and for all rational opens U and all f in $\mathcal{O}_R$, we have to take care of two different kinds of covers. So, we have U covered by $1/f$ and $U \setminus V(f)$. This is a refinement of the Zariski cover we had previously, where you just inverted f and $1/f$, and that was a cover. This is a smaller, a refinement of that, which still covers.

The last thing I think—one, it's enough to do it when f is in $\mathbf{R}_+$. Okay, I don't think that will be helpful, but uh, it's—that's nice to know. Yeah, it's like in rigid geometry, in Tate's original work, where he proved a simplicity theorem. He reduces to—he didn't have rational domains, but he has this two types of covers, for which you can prove acyclicity, and it turns out that it generalizes to the analytic case.

I will not give the argument for this, it's—it's just it's a bit more complicated, because well, the Valuation spectrum is more complicated than Spec, but the idea is basically—well, the idea is somewhat similar, you could say, but it's actually a somewhat complicated argument. So, but it's—Uber, there is maybe a statement.

That it's enough to have a rational $f_1, \ldots, f_n$ generating the unit ideal, and then you can check just for those. Yes, yes, yes, you can do some little bit of work to reduce to exactly, exactly, exactly, yes.

So, Huber shows that every G-cover is refined by one of the following form: take $f_1, \ldots, f_n$ generating the unit ideal, and then look at $U(f_1, \ldots, f_i^\wedge, \ldots, f_n)$. The collection of these U's is a refinement of the usual Zariski cover you get when $f_1, \ldots, f_n$ generate the unit ideal, but you can check just on valuations that it still covers the valuative spectrum. Then you do something similar to what we did previously—you can assume one of the elements is equal to 1. You keep playing and playing and eventually you get the desired thing.

Okay, so now what are we reduced to, analogous to there? If (R, R^+) is a discrete Huber pair and we take $f \in R$, then we need...

There were two different kinds of covers: $U(f)$ and $U(1/f)$. So I'll do the first one. $D(R, R^+) \to D(R[1/f], R^+ + fR^+)$ is just the solidification, T-solidification, from $\mathbf{Z}[T] \to R$ with $T \mapsto f$. By definition, these are both analytic ring structures on the same ring, and the only difference is that in the second case, we've enforced the extra condition that $x \in R^+$ is solid in $R[1/f]$. This gives us the category we want.

For the second cover $U(1/f)$, this is $D(R, R^+) \to D(R[1/f], (R^+ + R^+f)^{\mathrm{int}})$. This is first inverting f, then T-solidifying for $\mathbf{Z}[T] \to R$ with $T \mapsto 1/f$. The modules here are a full subcategory of $R[1/f]$-modules where fx is invertible, with the extra condition of being "solid".

I claim that this whole process of inverting f and then solidifying with respect to $1/f$ is also described by just an $\mathbf{R}$hom. Namely, take $\mathbf{Z}[T] \to R$ with $T \mapsto f$, not $1/f$, and take $\mathbf{R}\mathrm{hom}_{\mathbf{Z}[T]}(-, R)$. This is the localization which kills the idempotent algebra $\mathbf{Z}[[T]]$ in solid $\mathbf{Z}$-modules.

So, this is kind of the new notation fitting it in the general framework. Before, for Uber pairs, you wrote the $\mathcal{D}R^+$ integral clause. Okay, you wrote $\mathcal{D}$, so let me explain what the point is here. This localization was supposed to be given by first inverting F and then doing the solidification. But again, this solidification—the first claim is that this functor already inverts F.

If you have something F-torsion, it's going to be killed by this, and the reason is you're killing this whole guy. And therefore, in particular, you're killing any module over this guy. But everything T-torsion is a module over $\mathbf{Z}[[T]]$. So this automatically inverts F, because anything T-torsion is a $\mathbf{Z}[[T]]$-module.

So if you have a solid Abelian group, which is a filtered colimit of things killed by powers of T, then it is a $\mathbf{Z}[[T]]$-module. That's just a condition, so you can reduce to checking for something which is uniformly killed by some power of T, but then it's obviously a $\mathbf{Z}[[T]]$-module because it's a module over the truncated power series ring.

So then it would be the same thing to write this formula where you invert T, but then if you do that, it's exactly the same thing as T-solidification as described by the previous formula. Inverting F and then solidifying $1/F$ is just the same thing as doing this here.

Okay, so basically all you need to check now, if you look at those conditions, most of them we already know. It's a localization, kind of by construction, the localizations commute, because they're both given by $\mathcal{R}$-homing out of some object, and any two functors $\mathcal{R}$-homing out of an object commute with each other, just because the tensor product by a functor and the tensor product being commutative.

So what does this translate to in terms of these idempotent algebras which determine these localization functors? It translates to a simple condition on these idempotent algebras. If you take $\mathbf{Z}[[T]]$ and tensor it in solid $\mathbf{Z}$-modules over $\mathbf{Z}[[T]]$ with $\mathbf{Z}((T^{-1}))$, you get zero. If you have something that dies on $\mathcal{R}$-hom out of this and dies on $\mathcal{R}$-hom out of that, then by messing around using this condition, you conclude that it just has to be zero.

What's the interpretation here? You can think of this as localizing away from the open unit disc, and this was localizing to the closed unit disc or away from the open unit disc centered at infinity. The reason those two cover intuitively is because if you take the closed unit disc centered at infinity and the closed unit disc centered at zero, then their union is the whole space. But in terms of the complements, that's saying if you take the open unit disc and the open unit disc at infinity, then they don't intersect, and that's exactly the algebraic translation of that fact.

Similarly, for the second kind of cover, you need that $\mathbf{Z}[[T]]$ tensor

We assigned to this discrete Huber pair. But if you then want to get a sheaf, you send a rational open. You have to send that to modules, our modules in this D-O-of-U, discrete... I don't, maybe I want to say, so O for the discrete ring, okay?

So this recovers the topology in the G-model, nisr, okay, yeah. So the and in particular, so what is the unit object in this category? So you take R, and then you invert G, and then you derive-solidify with respect to all the FI over G's.

So necessarily when you do this object for a completely general solid ring, you're going to end up with some derived phenomena here. I was hoping to get to it today, but we'll probably discuss exactly how that happens later. I want to also make another remark, which is that this sheaf, Dr-r+, actually lives over a much smaller subset, a closed subset. Let's say SP-R-star-R-plus-r. If you remember the set of topologically nilpotent elements, then here you add the condition that if F here is topologically nilpotent, the valuation has to be strictly less than one.

So for an individual F, that's a closed subset, and then it's a big intersection of such things that's a closed subset of this topological space. My claim is just that if you take this sheaf of categories and you restrict it to the open complement, you just get zero, so it's really living over this closed subset here.

On the other hand, Huber considers Spa-R-r+, which is the continuous valuations less than or equal to one on r+, and that's the same thing as a potentially smaller subset, generally smaller subset, satisfying a stronger condition, saying that if you're topologically nilpotent, then for all gamma in gamma, there exists an N in N such that the valuation of f to the N is less than gamma. So there's some subtlety here.

The space that Huber localizes over is actually smaller than the space that we localize over. But Huber shows, and I should say that this Dr-r-plus does not live over, but there does exist a retraction, which is actually a quotient. By definition, it was a subspace, but you can actually realize it as a quotient, and then you do get a sheaf of categories on Spa, and that's the correct way to get a sheaf of categories on Huber's topological space. It's the retraction that's kind of the good map in the sense that this is the quasi-compact map. So in general, you get more flexibility for localization using this picture than with Huber's picture, and the kinds of extra things you get are something that we already discussed, like the so-called functions on the closed unit disc, which arise from the structure sheaf in this general setting but don't arise from the structure sheaf in Huber's setting. You can analyze these things, but I think I've now said enough. Thank you for your attention.

Rational opens here you can actually parameterize it by similar data, but with an extra condition that these things generate an open ideal. And then if you pull those rational opens back here, you get exactly the corresponding rational opens as expected. But if you take a general rational open here not satisfying that condition, and then restrict it—no, no, if it does satisfy that condition and you restrict it, you get the correct thing. But if it doesn't satisfy that condition and you restrict it, you get something new which is not necessarily even quasi-compact, not a rational open. So you have to write it as a union of rational opens, and this is kind of like taking the open unit disc and writing it as a union of closed unit discs, which is a typical example of that phenomenon.

You said something about getting a structural shift last time. Can you comment on this, or will it come later? I was hoping to get to it again today, but I didn't. So the structural shift would just be you take R, which is living globally, and you apply the localization functor to get something living here instead. And that's this object here, and one can analyze it and so on and so forth. And it's also some center of a certain category. Is there a way to think of it as a center of a center? I don't know, but these are your symmetric monoidal categories, so it's just the unit, I mean, the unit of the symmetric monoidal derived category in this sense.

You claim that when you take $\mathcal{M}od_R$ of this, this is actually a good thing, which so it is associated to the—in good cases it is associated to the rational—except that sometimes you have to do. So this is, I mean, this will also correspond to an analytic ring, but you know, in the derived sense. So you have to—the notion of analytic ring that we've discussed so far, you had an ordinary condensed ring in a full subcategory. Here you need to not just remember that ordinary derived, you need to remember some derived enhancement of it as well. But then it is enough to just remember the ordinary abelian category of modules over the ordinary thing.

And besides the derived stuff, there's also a quasi-separated issue, where the value of the structure sheaf might be different from Huber's, even if it lives in degree zero, it might—the quotient might not be by a closed ideal, and so it might still differ from Huber's. But again, in practical cases, that doesn't show up. And I guess even inverting G can introduce non-quasi-separated behavior in general. We'll discuss this in coming lectures, all of these.

In the so, considering those two spaces in $\mathcal{U}$-theory where you have a retraction, which I think he maybe used a slightly different notation, but anyway, this $\mathcal{S}^{p,v}$ and $\mathcal{A}^i$. So you have this subspace living in a slightly bigger thing, and there is a retraction which is spectral. So you have shifts, you can consider shifts on both things. What I said, I think, is correct, that the restriction to the subspace is like the direct image. Okay, then you have a shift, like in this shift of categories and some sense on the full $\mathcal{S}$, and then you take the direct image yes to the subspace, which is like restriction.

Something I probably, but the question can also be asked about, so you have particular sheaves on the full $\mathcal{S}$, which are direct images by the inclusion of sheaves on the subspace. So the question is, like, in this context, so you have your, let us say, you have a rational open in the figure $\mathcal{C}$, and you consider its intersection with the smaller $\mathcal{S}$. You said that you can write it as the union of $\mathcal{C}$, so you can evaluate your shift by in this way, by inverse limit of those. Is it equivalent to the shift to the value on the original thing in the big $\mathcal{S}^{p,v}$? So like, whether the shift of categories is the direct image of its restriction to the subspace, which—so you have on $\mathcal{S}^{p,v}$ $\mathcal{R}$ plus

10. Analytic adic spaces (Scholze)

https://www.youtube.com/watch?v=YLQt_tV4tHo&list=PLx5f8IelFRgGmu6gmL-Kf_Rl_6Mm7juZO

Unfinished starting from 0:00

Good morning. Today I want to talk about what is called analytic geometry.

I immediately want to point out that there is some conflict of terminology here. Basically, using what we've said so far, all rigid analytic spaces give rise to analytic spaces in our sense. But within the world of rigid analytic spaces, there was already a qualifier called "analytic."

Within the context of any space, what is this conflict of terminology? I didn't come up with a good name for them, but there's one name that's already to some extent used in this world, so I will call them "T" in this lecture. Peter, could you write a bit bigger, as it's a bit unclear.

I don't want to overuse the word "analytic" in this.

Here's the definition where this name is already used: it's called the "affinoid" case. This is terminology that's already in use. If it has a topologically nilpotent unit, it's also called "adic."

The example to keep in mind is, for example, $\mathbf{T}_p$, where p is a topological unit. In fact, any non-archimedean locally complete field will do. We always assume completeness. Anything above you will also be called an "affinoid space."

This is usually called "analytic" if it is covered by such affinoid subsets. We didn't really discuss the full definition of rigid analytic spaces, but we discussed the algebra that their rings have, and there's some way of doing them which will not be all that important for us right now. Basically, we told you what the open subsets are, and then you kind of do the obvious thing.

It's actually equivalent to a property that the speaker discusses in the green text, which I won't go into.

The speaker also clarifies that we did not define "analytic spaces" in our sense, and that there is a subtlety about sheafiness in the classical theory of rigid analytic spaces.

The speaker then mentions the question of whether in the theory of localization and Dustin talked about last time, when you do this, you get some condensed thing, possibly over a rational domain, and it is unclear if this always gives the right thing.

The speaker then explains that if you have any space, you can associate to each point a completed residue field, which can either be the residue field or a complete valued field, and it is asked that there are no points where this is a discrete valuation ring.

Basically, the setup is that the regular fields are always of these forms. And when you have such a complete non-archimedean field and there is some unit in there, you can lift this to a small neighborhood, and that's why these are called "adic."

The intuition is that there is a world of schemes, which sits on some world of formal schemes, maybe with some adjectives. These all sit with the context of rigid analytic spaces. But then these "adic" spaces are some of the ones where you start with a formal scheme, but then remove all the scheme-like points, so they are at the other end.

The speaker then mentions something like $\mathbf{T}_p^0$ that can also be considered a kind of $\mathbb{SP}_1$, but says the generic fiber in this sense is unclear.

Finally, the speaker wants to say a few words about the structure of such things.

Definitions. So, let's say π is a topological unit. You can then take the ring of definition, because you can always assume that they contain any given power-bounded element. I mean, some particular power-bounded π. You could also do it such that you take any π, and then some power of π will anyway be a unit. And then actually, it's automatically the case that $\mathcal{O}_K/\pi$ is a zero ring.

That's an actual exercise in looking at the axioms of the topology. So you might think that it could have some rather more subtle topology, but actually, it's always supplementary. And then you can actually write $\mathcal{O}_K$ with Banach algebras. For example, you could say that the norm of a function f is $2^{-\max\{m | f \in \pi^m \mathcal{O}_K\}}$. This means that the norm of π is $1/2$ the norm of π^{-1}. Of course, there's some kind of arbitrary choice that I made here, both in uniformizing π and in the number $1/2$ here.

The trivial counterexample is the zero ring, where the norm of π would be zero. I don't think it has a topological unit, or maybe that's a counterexample to show that anything that is an algebra over $\mathbf{Z}$ is probably okay. So, whatever you figure it out.

There is an equivalence between certain p-adic and algebraic geometry of rings. The objects are p-rings admitting a p-valuation such that the norm of π is basically between 0 and 1, and the norm of the inverse is also 1. But as a moment, you actually only care about the topology used by the valuation, and everything here is completed p-ring and continuous. Of course, the zero ring is still a counterexample to what happens to zero.

Okay, so these are the basic algebras that we consider here. You could also talk about this in the language of p-algebras, but you shouldn't really fix the norm, but you should just ask defined by some norm with this property, because we have...

Right, so now I want to state a theorem that I proved some 10 years ago or so, and the theorem that I found extremely striking and surprising at the time. As was discussed, there are all these issues that for general Huber rings or Huber pairs, what Huber defines is not always actually a sheaf. But let's just say that A^+ is quasi-coherent if what Huber defines is a sheaf of Huber rings. And I did not at all expect that in those cases where you can prove that something is a sheaf, you can probably also prove other nice properties, because then that's probably the reason that things are well-behaved. What I did not at all expect is that you could prove any non-trivial theorem whose only hypothesis is that something is a sheaf, and then other good things happen, because just asking for the sheaf condition is the first thing that could break, but I didn't expect that if you're in a situation where it is a sheaf, why should other...

But here, I proved the following statement: If A is a quasi-coherent sheaf of finite projective modules over a Huber pair (A, A^+), then any finite projective module over A can be extended to a sheaf of finite projective modules over A^+. And I mean, A is just a direct module, so certainly this is still a sheaf. It's a sheaf of finite projective modules, and actually, it's one that's locally free.

A locally free $\mathcal{O}$-module is called a vector bundle. Whenever you have a ringed space, you can always talk about these modules which are locally free or of finite rank. These are called vector bundles with respect to that ringed space. It's easy to see that this is a functorial construction, but the highly nontrivial, surprising fact is that there is actually an equivalence. When you can glue vector bundles, you can define the category of vector bundles. Somewhat later, they also proved that there is some version of this that works for some kind of coherent modules, but I don't want to state the precise result because it's extremely...

Let me just say there is a result that if the ring is nice enough, then you recover the expected result that you can do this even for rings, but you have to be careful because localization might not preserve all the properties you want.

All right, so this is a very nice result. Basically, the aim at the time was just to prove that on perfectoid spaces you can define vector bundles, but the argument actually worked whenever the space is quasi-compact and quasi-separated. I could kind of follow their argument line by line, but it's a rather tricky argument where you really have to do some changes of bases to make things better. It's actually an analysis argument.

Today I want to explain different proofs that we can give using our general theory of solid modules. I should maybe say that some 10 years ago, when this result came out, I was also talking to K and he was already telling me that if you work in the derived category and look at some kind of $\mathbb{I}^{\mathbb{N}}$-up modules, then you can just glue. I was like, okay, but what does it even mean? We have all these problems, I think it's not a structure sheaf, and it seems highly non-obvious how vector bundles and so on would work. So what does this extremely general result actually tell you about such constructions? I don't know whether he ever figured that out, but the goal of this lecture is to show that this is not just some fancy, abstract result, and to get down to something more concrete.

Now, the idea of continuous valuations means that when I form such a rational subset, I should assume that the ideal generated by 1/a is an open ideal. But actually, once you have a topology where 1 is a unit, then a must be equal to 1.

First of all, H is given by taking the completed power series algebra in variables G_1 to G_N, and then modding out by the ideal generated by $dG_1 - F_1, \ldots, dG_N - F_N$, and taking the closure.

This is basically why that works on this open subset. But first of all, you didn't invert G, because now the ideal generated by G contains F_1 up to F_N, but they generate the unit ideal. So G has become invertible even without taking the closure.

And then on this, you have T_1 which is F_1/G and all its completed power series. This is what could happen on this subset where this is less than or equal to that.

But I mean, Huber was always working with complete topological rings, so when you take a quotient by something, you also take the quotient by the closure. And then, for example, this is a B-algebra, and the quotient by any closed ideal is still a B-algebra, so it's still okay.

This is maybe not so hard to show, and you all recall the endomorphism part. The image of a is just the thing that was defined last time. You define a category ring structure by asking for this completion for all these elements, and then you somehow look at what the completion of the unit actually is. This is what the S uses, and it has a very similar formula.

But then, take the derived quotient, where here or and elements I want to R. This derived quotient signifies the derived base change from only algebra, but they just modify all the variables X_i, so basically you're setting X_i also to zero, but not just in the stupid way, but in the derived way. And completely, this is computed by a possible complex, and in the middle there are some Ext there, a cotangent differentials.

And this is just by taking the standard resolution of $\mathbf{Z}$ over here. It will be an animated condensed thing, and that's why I will consider this. I don't really want to talk about animated things today, but yeah.

So, it's certainly some kind of algebra. Let me just give a sketch of it, and for the second part, so you can write A^1/d as $A^1 \bmod d$, and now it's not really necessary whether I take the derived or not, but it's also turn the derived S because what I will write down is a regular sequence.

And then you solidify, solidify, solidify these, so some exact operations category. This thing is just computed by a possible complex, which is a complex of finite complexes of some C thing. Just understand what happens when you solidify these things, but this is exactly what Dustin already computed that something.

Okay, and so certain things that saying it's the same thing as stating all, which is a context, but zero is just a usual structure. And then you take the C separated a small inter, the inclusion from light or not, it doesn't really matter, probably separated condition set inside of all condition set, it hasn't left the join. I'll take any x to the quotient separated portion, and this is making the operation of taking the maximum h

Has a good effect that if x has some kind of algebraic structure like being a group or ring, and so on, and the functor that is a preservation of final product is available both for the adic setting and all condensed sets, yes. I mean, also, if you start with the adic condensed and then from the maximal condensed also adic and so on. So, thank you.

Right, so completely if you write X as a condensed set, it's telling you to compute the joint, and you can always write any condensed set as a quotient of a discrete set. Just take different profiles depending on it. Then, there exists the minimal compact injection $\overline{R}$ of R that is also an equivalence relation and containing R. So, it's in some sense taking the closure of R intuitively speaking. But sometimes this adds new things where the relation is not spreading, you add that as well, and then you do some kind of transfinite induction or you just intersect all possible containments.

Then X is this quotient, this is $X \mathrm{mod} R$, because this inclusion relation was called the compact injection. So, for example, if X happens to be a group, then you can always also find a surjection from the separated condensed to the group, and then the relation is just the quotient by the subgroup. In that case, you're really just taking the quotient by the closure of the subgroup, the closure in the sense of small injections.

Okay, so here's one other thing I should recall, which is what do compact injections have to do with spaces? Let's say X is any sequential space. To recall, it's a topological space for which the condensed set is fully faithful in the closed subsets of X. Relatively to the compact injections, taking any closed subset here, why is it actually a further compact injection? While to check that, the definition is that whenever you pull back to some profinite set, then the preimage is a compact injection. But this precisely means that the other way is also compact, and so the preimage in this case is just the closed subset of this profinite set by the closed subset here.

Okay, and so back to the sequence, what is this? Well, this is just the thing, and then you take the extension. Okay, and then if you take a separated quotient, well, this guy is already separated, and then by the above, and because this here is actually sequential, comes from a sequential topological space, this precisely means that I should just replace this one here by the closure, which is what.

Right, and so this gives us some relation, but we're interested in a somewhat more precise relation, which is the following theorem, potentially due to Lurie, except that he didn't use this language to phrase it, but I mean the proof is really his. That a p-adic completion is p-adically complete if and only if all p-power series all have bounded denominators.

Let me give the proof. This is clear because by the general descent results from the last lecture, we have the sheaf of categories, but in particular, you get a sheaf of rings, and we just take the units. And the non-trivial and I think somewhat surprising result is that if it so happens to be a sheaf, then actually it is the right thing. Let me get the other direction. And before I start, note that to check this property that all p-power series are p-adically separated, it suffices to show that this is true on a further refinement of the cover, because you can always recover it by all the smaller values

To get the good properties of all these views, we need further refinement. I don't want to go through all the commutative refinement, but just note that some following concepts are also always refined.

We can always cover any space with a Zariski-type cover, with at most one and at least one nonempty intersection. So the other type of cover is not necessary when you look at new units. This reduces to the following key steps.

Assume we have a sequence of x_n that is Cauchy. Actually, what does "Cauchy" mean here? I mean, this is a sequence of elements in $\mathbf{R}$, so let me denote it as a_n. This satisfies the Cauchy condition, which says that for any $\epsilon > 0$, there exists N such that $|a_m - a_n| < \epsilon$ for all $m, n \geq N$.

This is always true, just because any element in the limit can be written as a sum of f^n and $1/f^n$, where the f^n come from one side and the $1/f^n$ come from the other side.

Assume this Cauchy condition, which is always true for a Pro-H space, because it's just one specific instance of the Cauchy condition.

Then, the derived object by f and the other thing is also the identity. In other words, it agrees with what we defined earlier. What does "the D" mean? It means that modification by $-f$ is actually injective here and has closed image.

Let's prove this. What you have to see is that if you look at the map, and then multiply by f or $1/f$, this is injective with closed image.

But actually, because of the star condition, if you want to show injectivity, let's assume you have something in the kernel that also still lies in the kernel when you replace a by one of those two rings. But then over those two rings, it's easy to see that this map is injective.

And similarly, this map actually has closed image, because the image is precisely the kernel of the next map. Using this, you can also check that if it has closed image over those rings, it must also have closed image here.

So it's enough to check it when we replace a by those two rings. In other words, we can assume that either the absolute value of f is less than or equal to 1, or the absolute value of f is greater than or equal to 1.

If f is less than or equal to 1, then $T - f$ can be shown to be a closed ideal, because you can just successively "peel off" the highest coefficient. Something similar holds in the other case.

Basically, if you have an element here, you can really just by looking at coefficients see what happens. And also, one of them becomes a torsion module anyway, because after not doing any further localization, it's only the other one you have to check.

So this is a funny argument that K found, that if you just assume the Cauchy condition, then you can reduce this problem to the simple open covers where you just take a portion by one element. Because then the question of what the R-portion is is really just the question of whether this one M here is injective and has closed image.

Okay, so this finishes my discussion about the Cauchy condition and the relation between the H-structure and so on.

To the second part—so, here are some definitions. What does the "me" mean? Let R be a ring with a certain condition structure for now. Then, something like this definition S_6, I believe. If it can be represented by complexes up to some degree n, but $\mathbf{Z}$-graded, then all these terms must be finitely generated. You could also say, try to project this with negative degrees as well, because I want to say something else in a second.

And then, importantly, if your ring was an $\mathfrak{a}$-adic ring, then this will just be the condition that each group is a coherent module—the case just finitely generated module. But if you're not in an $\mathfrak{a}$-adic range, then there's some coherent module structure form in some category, usually. And because you didn't ask, the relations between the relations are finitely generated, and so some of the infinite complexes they capture the idea that you have modules which are finitely generated with as many relations, as many further relations.

So the point is that over in the $\mathfrak{a}$-adic case, it's a good class of finitely generated modules behaving nicely; you don't have that over a general ring, but the ring still has a nice class of complexes. And let $\mathcal{P}$ be perfect if there is such a representation which just wanted to find it—just a finite complex, projective modules.

Then the theorem is that, let's say $\mathcal{A}^+$ is any H-module. Then sending any A to the following things: so, on the one hand, you can take perfect complexes, all the ones or you can take two coherent ones that are, say, in degrees greater than or equal to zero or greater than or equal to n for any n. We could also take all perfect complexes, but you could also take those perfect complexes which have such a representation in some interval. So, perad means those that can be represented using a complex sitting in certain degrees.

These are all functors that take a real subset to the infinity category, and they all agree, and I claim that they all share some nice properties on the right. I'm always just thinking of A as a cochain complex being a reason; there's no dependency on you on the right-hand side. I did not, thank you.

And I guess finally, I'm now extending all these notions to animated rings. And so, okay, so either you know what all these things mean when this guy is just an animated ring, then it's true as stated, or you secretly that you maybe in the $\mathbf{C}$ case where it's just $\mathbf{C}_0$, and then I just told you what they are. Either way, these are nice properties, in particular, the $\mathbf{C}$ case, if I expect the vector bundles, this cover.

Yes, Peter, is there a reason you didn't state the like pseudo-coherent $\mathcal{A}$-module? No, okay. You have to be careful that the good way to make $\mathcal{P}$ restrictions on $\mathcal{C}$ and a perfect complex are different. Yeah, for perfect complex, you assume that there is a representation as an actual complex that's in some range of degrees. For coherent sheaves, you can just make a more naive thing like bounded below, bounded above, although no, there is actually a reason. If I no, there is a reason because look, I don't think localizations are flat in any

Okay, this will have a meaning at some point when this is animated. We didn't discuss this yet, so if you feel more comfortable, just assume this sits at zero, and this is a sheet of any categorical and so this certainly contains fully faceful, just the modules over the underlying $\mathcal{S}$ ring, which is always true for any anticyclic ring whatsoever.

And this certainly contains all these other subcategories. But yeah, basically some kind of a finite solution here. And so by virtue of this being fully faceful, it means that the only thing we actually have to prove in all these settings is that if you have an object here, which locally happens to lie in all of the sub-categories, and actually so globally–maybe at the expense of pretending that I can replace all of you by $\mathcal{A}$ again–that this is such that over the base change from $\mathcal{A}$ plus, one of the sub-categories, then so does the conditions that we put.

Okay, and so maybe the first idea you might have is that well, let's first check that this condition of being a discrete module in the sense, and then the success conditions. But this actually does not work. Warning, this is a warning that Dustin already made, but I want to reiterate it because it's important. The condition does not just say globally, it's do locally, which will not be globally. Let me actually quickly sketch one example. I'm not sure it's the easiest example, but somewhat instructive.

Let's consider the curve, so it takes the $\mathbf{T}_m$ spheres over, let's say, $\mathbf{P}^1$, and then you can take the quotient by, let's say, other units. This what's called a table, different with the analog of taking $\mathbf{C}^\times$, complex space $\mathbf{C}^\times$, and $\mathbf{P}^1$ by some topological element. And then this becomes an adic curve, complex numbers do similar, or whatever rigid geometry started. And let's assume that our $\mathcal{A}^+$ is some large open subset here, so basically remove a small disc around the origin. But large enough so that there is non-vanishing global monodromy. But should really be sub-only and not this. And then there is $\mathcal{C}$, the projection that–yeah, ring. And then you can find what I will call a lower streak of sub here. What is this? Well, locally, this map is just split, and so locally on the base, like the $\mathbf{P}^1$ are just discs, a union of copies of the base taken by the integers, and then the sheaf is just a direct sum of copies of this same. And so this is how it's defined locally. And well, it's obviously a sheaf, and so it glues to some sheaf on the base. So this defines for you an object $\mathcal{A}^+$-solid, because locally it does. And it is locally discrete because locally, as I said, this cover of splits, and this is just a bunch of copies of your base. But you can check that globally it is not split.

Okay, so we can't hope to use this category instead. We will use two other notions. We said you can first also define this kind of pseudo-coherent, where now I mean, yeah, it's again the complexes which are represented by something that's bounded to the right, so goes to the left, and the cohomology are in this case, I just say they are

And you actually just use—I first of all being bounded to the right is a property that globalizes because you're locally bounded to the right, and then globally you're just some kind of finite limit of that, so you're still bounded to the right. And so then you need to check this x condition, and the only thing you need is that these localizations have the finiteness amplitude. Otherwise, there would be some issue, but during that localization, it's fine.

So this finiteness condition on the big category—this can be checked, and then actually once you have the finiteness condition, the thisness is also something that can be globalized. But this actually needs to be proved, and some equivalent conditions need to be checked.

So the issue there is that it's in some sense easier to work with the second subcategory of n-nuclear objects, and these are somewhat more general than just the B-nuclear objects, but not the C-nuclear ones. Let me just give a quick definition here. So all the B-nuclear ones will be modular, but things are somewhat more general than that. In particular, they contain all the B-nuclear objects, but not the C-nuclear ones.

The name is inspired by this class of nuclear vector spaces defined in functional analysis. It's not a completely precise translation, but in spirit it is. The condition is that the internal Hom to P factors through a trace-class map. This in some way encodes the idea that all maps from the dual of B tensor with something are trace-class.

In general, the nuclear modules are generated by shifts of just B-module functions. You can generate them by full limits, and in general, referring to more general analytic rings, by where these are all trace-class, meaning they come from elements in the dual. Whenever you have an element in the standard pro, you can produce an M from that, and these are all trace-class. And then if you take a sequential form of trace-class maps, you can check using that internal Hom commutes with limits that they always have, and conversely, when you try to present nuclear objects in terms of how they're generated by compact projectors, the map will actually factor through a trace-class map.

Okay, and so yeah, in particular, over $\mathbf{Q}_p$, this is stuff generated by B-nuclear objects, so this is still a rather large class of guys. It will actually, I mean, there are actually C-nuclear and so on. One very nice property of this nuclear module category is that it forms a universal L^1 approximation in some sense to the right-hand side, and using this, you can also define the L^1 spaces.

So back to here, you have the subcategory of n-nuclear objects, and the finest class that is globally closed. And so you want to check that if you have some C that satisfies this condition locally, then you want to check it globally. And for this, the ideal situation is that just the two sides of the morphism can localize—the right-hand side definitely localizes, as it's just some tensoring. The question is, does the left-hand side also localize? And it actually does, and the key thing is that you can use the same argument that was used last time to show the different localizations commute with each other, because they actually show that all the localizations are given by internal Hom from some object.

Strictly speaking, for an algebraic localization by inverting an element, it's not literally an internal hom from some object spaces and rational subsets, like inverting an element is not a rational subset. But in general, there's a suitable localization. In any case, you can always refine further so that it is.

I just wanted to mention that, in case there was some confusion. Actually, with other types of localizations, it's also commutative with elements, because it limits.

So, now we have two classes of things that are in the sense of a special category, and the question of commutative localization seems to suggest that the sketchy argument given doesn't use that you can put anything, not necessarily compact, instead of P. This commutative localization is contrary to what we have for schemes, where you have to take the underlying hom.

However, in this case, the localizations commute with all products, because they have a left adjoint, which is extremely important for the theory of compact SP-coherent modules. In these situations, the pullback functor has a kind of coherent lower shriek functor, which is the left adjoint. So the pullback preserves all limits, but it also satisfies the projection formula. This is precisely equivalent to the internal hom, so it's true for anything instead of P.

For the specific localization we're interested in here, to Zariski open subsets, this is actually always true. We made two steps in our proof: we isolated two different classes of modules that can be glued, and the last step is to isolate the intersection and show that the two central modules over S are just an inclusion. They are certainly in the SP category, and this is actually called...

So then we finally show that we can glue these two ones, and there's still a little bit to show that all the other super- and subscripts I put also glue, but this is actually easy.

So far, I didn't do anything that seems remotely like analysis. But for doing vector bundles, there is actually some new curious power series that you have to undertake somewhere. So somewhere, there has to be some actual work.

Let me show how it's done. We can assume we have some complex, and by shifting, you can assume it starts at $\mathbf{Z}$. I also said you can assume that these are all just...

Okay, you have such a complex. How do we know that it is nuclear? Now I certainly have a map from $\mathbf{P}$ to here, which is just the identity here, and my complex is just zero elsewhere. This is a map from $\mathbf{P}$ into C. Being nuclear, it means that it's trace class. If you actually have a section in here, then again you can factor this over some approximation to C, and when you think a little bit about what this means, you realize that there exists a diagram where G is PR, but then this F can actually be split back. You get a morphism here, and what you see with this is that the map from $\mathbf{P}$ to the complexes over the morphism from G to $\mathbf{P}$ for some G of $\mathbf{P}$ is just some playing around with the fact that $\mathbf{P}$ is projective and so on, and the definition of nuclearity reduces this.

So in the end, you see that there is actually a way to put a unit here, and then it's actually enough to show that this guy here is actually perfect. Certainly, this representation doesn't show that, but they claim, as far as the Particular compact operator, and then any perturbation of a Fredholm operator by a compact operator is still a Fredholm operator. Some have properties that are related to the kernel and cokernel dimensions.

This is an argument that you just have to execute slightly more carefully in this situation. So, there's some Fredholm operator on this like base, something like this. And then, you can show that, yeah, the cone on the Fredholm operator is a perfect complex, which is a better way of thinking about it as the K-theory generator.

This is the only place where you actually have to play with some, I mean, you have to have a matrix representing the map, and then you play with that. It's easy, but there's this step where you actually feel like you're doing a little bit of analysis.

Okay, before you erased the blackboard, you had a factorization where P. You said that you factorized it through P, and one map is trace class, right? But it seems to me that you have to use the Hom space. I mean, the data will not give you that the map from P to P is, you need some homotopy to do it. I mean, I think the data is probably that where it's executed, look at the complex geometry, not where we do precisely the same argument like this linear vector space.

So, it's an unfortunate thing that I'm probably screwing up if I try to do it right here. But in any case, the data probably means that the map from P to the complex is homotopic to a map that factorizes through a finitely generated free module, up to homotopy. And then you can use this finitely generated free module to modify the complex, so that now the degree zero part becomes zero, and then you can just keep going.

11. Solid miscellany (Clausen)

https://www.youtube.com/watch?v=87-wuqGA8GE&list=PLx5f8IelFRgGmu6gmL-Kf_R1_6Mm7juZO

Unfinished starting from 0:00

Okay, so I'd like to start. Welcome back, everyone. We're about ready to move on to a new topic, but I just want to finish off our discussion of the solid theory with some miscellaneous facts.

Recall that we had this procedure: if we had a pair, say A and A^+, where A is some solid ring and A^+ is a subring of power-bounded elements, then we associated to this a certain solid analytic ring specified by A/A^+. This was obtained by forcing all the elements in A^+ to become solidified variables.

Recall also that we had this on the level of the derived category. If A^+ is not a subring of $\mathbf{Z}$, then when you enforce this condition, you might lose A, and it might no longer live in that category, so you'd have to apply localization and get a new A, a sort of completion in some sense.

We also had this defined as a full subcategory of the derived category of A consisting of those objects whose homology lies in A^+ for all i. We showed that you have a nice left adjoint with the same properties as the left adjoint here: symmetric monoidal, and so on. But there was this subtlety that this is not necessarily, in the full generality of an analytic ring, equal to the derived category of this. The basic reason is that if you take some basic object here and apply derived solidification, it might not live in degree zero.

In fact, the generator here is the compact projective generator you take from the solid $\mathbf{Z}$-theory and then tensor it, in the sense of the solid $\mathbf{Z}$-theory, with this solid ring A. We know that it doesn't matter whether you take the derived tensor product or the ordinary tensor product, because as Peter showed in one of his lectures, this is flat in solid $\mathbf{Z}$.

So if you did the derived analog of this definition, it would literally just be the derived category of this $\mathcal{A}$-linear category. But then what's the generator of this derived category? You take this object here and you localize it with respect to this derived localization functor derA^+-solid.

I want to explain that this lives in degree zero, which means it's a compact projective generator for the $\mathcal{A}$-linear category $\mathrm{Mod}_{A^+\text{-solid}}$ and the derived category we defined there. As long as your underlying solid ring is not derived in any way, the whole theory is, so to speak, flat and determined on the $\mathcal{A}$-linear level.

Does this change from what was said before, or was it not clear in the previous talk? I mean, it was not clear that this was the case.

Okay, so how do you access this derived A^+-solidification? Being A^+-solid means that you're solid for any variable mapping in. What I want to claim is that this derived A^+-solidification can actually be written as a filtered colimit. It's all happening elementwise in A^+, and you can write A^+ as a union of rings which are finitely generated over the integers, let's say $R \subset A^+$ with R of finite type over $\mathbf{Z}$. Then you can treat M, which is a priori an A-module, as an R-module and do the derived R-solidification.

The point is, well, there are two things to check. First, you need to check that this is indeed derived A^+-solid. And you also need to check that in this functor, if you have something which is...

That if you map out to anything derived $\mathbb{A}^+_{\text{solid}}$, then it doesn't see the difference between this expression and this expression. Why is it derived $\mathbb{A}^+_{\text{solid}}$? Because derived $\mathbb{A}^+_{\text{solid}}$ is the same thing as derived $\mathbf{R}_{\text{solid}}$ for all $\mathbf{R} \subset \mathbb{A}^+$ of finite type, just because it's simply an elementwise condition on the algebra.

So certainly the $\mathbf{R}$ term in this thing is derived $\mathbf{R}_{\text{solid}}$, but also any further term in the filtered colimit after the $\mathbf{R}$ term is also going to be derived $\mathbf{R}_{\text{solid}}$ because it's even has an even stronger property of being derived $\mathbf{R}_{p',\text{solid}}$ where $\mathbf{R}'_p$ is bigger than $\mathbf{R}$. So there's a cofinal system of things in this filtered colimit which are derived $\mathbf{R}_{\text{solid}}$ for any fixed $\mathbf{R}$, and we know that a filtered colimit of $\mathbf{R}_{\text{solid}}$ things is $\mathbf{R}_{\text{solid}}$, so it's going to be $\mathbf{R}_{\text{solid}}$ for all $\mathbf{R}$ and therefore it's going to be $\mathbb{A}^+_{\text{solid}}$.

And then mapping, if you map out to something $\mathbb{A}^+_{\text{solid}}$, then in particular it's $\mathbf{R}_{\text{solid}}$ for every $\mathbf{R}$, and by a similar argument you see that there's no difference on maps from M to this or this filtered colimit to this.

Okay, so it suffices to analyze, to show that if you take this product $\mathbf{Z} \otimes_{\mathbf{Z}} \mathbb{A}$ and then you derived $\mathbf{R}$-solidify, this lives in degree zero. But this, we can do it in two steps. So we can take, we can see this as you first base change to the solid $\mathbf{R}$ theory, so you take this thing and then you derived $\mathbf{R}$-solidify, and then you tensor in the derived tensor in the solid $\mathbf{R}$ theory with $\mathbb{A}$.

So it suffices to see that, well, first of all, that product $\mathbf{Z} \otimes_{\mathbf{Z}} \mathbf{R}_{\text{solid}}$ derived $\mathbf{R}$-solidify, well, this lives in degree zero and is flat with respect to our solid tensor product.

Okay, by the way, let me note before I continue with the, so basically we have to analyze $\mathbf{R}_{\text{solid}}$ when $\mathbf{R}$ is finite type over the integers and prove analogues of what Peter already explained for $\mathbf{Z}_{\text{solid}}$. So we have to calculate this

Solidification and so on, it always just throws the variables inside the product instead. Here, we recall that if you do the $\mathbf{Z}[T]$-solidification of a module tensor over $\mathbf{Z}$ with $\mathbf{Z}[T]$, this commutes with limits in M, and it sends $\mathbf{Z}$ to $\mathbf{Z}[T]$. We're also using that the different solidifications commute, so that you can actually do the solidification with respect to all of them by just doing them one by one. When you do the X_1-solidification and then X_2-solidify, it'll still remain X_1-solid. You also use that if it's X-solid and Y-solid, then it is solid for everything generated in the subring.

So, to solidify for R, it's enough to solidify for all the variables in R. In the general case, if we have a quotient $X_1 \dots X_n \twoheadrightarrow R$, then the $\mathbf{Z}$-solid tensor R can be derived. You can get it in two steps: first, solidify with respect to a generating set for R, and then do an algebraic base change from this ring to the ring R. The derived tensor product is then the product of $\mathbf{Z}[T_1] \dots \mathbf{Z}[T_n]$, and we're left with analyzing this and proving that it's equal to a product of copies of R.

The product of $\mathbf{Z}[T_1] \dots \mathbf{Z}[T_n]$ is R-flat, and this can be shown using the fact that R can be resolved by potentially infinite resolutions of finite free modules. For each of those finite free modules, it's clear that you just bring it into the product. This argument works in the derived category, and we don't need to worry about resolving the original module.

So, we've proved that this derived solidification lives in degree zero, and it's flat with respect to the tensor product. Now, we want to analyze the structure of solid R-modules in more detail.

From this, being a compact projective generator is again that solid R is just the end category of its compact objects, which are the finitely presented ones. These are the things that are cokernels of maps between the compact projective generators.

But this second claim is—you could say—coherence. This subcategory is an abelian category, is closed under kernels, cokernels, and extensions. This is the same as—the first part is obvious, or sorry, well the first part follows from the fact that we have a compact projective generator of this category. Then the compact objects will all be built from finite colimits from that, and they will be generators, and it follows formally that it's the end category of that.

So, what we really need to check is that we have this—this is an abelian subcategory. Now, let me remind you a bit about some classical commutative algebra. So, a commutative classical commutative ring is called coherent if you have the analogous property in the setting of discrete modules, so that the finitely presented R modules form an abelian subcategory. But you can check by playing around with short exact sequences that you only need to—there's only really one thing to check, which is that every finitely generated ideal is actually finitely presented. And those same arguments in this setting show that it suffices to check that every finitely generated subobject of a product of copies of R is finitely presented.

So, for coherent rings, now I'm going to use a bit this notion quasi-separated. Note that if you have any subobject of a product of copies of R, well, this is quasi-separated, i.e., Hausdorff, and any subobject of anything quasi-separated is also quasi-separated. So, this is also quasi-separated. So, it suffices to show that every finitely generated quasi-separated solid R module is actually finitely presented. So, let me make a lemma.

If M in solid R is quasi-separated, then the following are equivalent: (1) M is finitely presented, (2) M is finitely generated, and (3) M is an inverse limit of M_n's, where each M_n is a finitely generated discrete R module, and the transition maps are projective. The category of these is just the countable pro-category of the category of finitely generated R modules, so the maps between such inverse limits are just the maps of pro-objects.

The non-trivial implications are (2) implies (3) and (3) implies (1). For (2) implies (3), if M is finitely generated by definition, we have a surjection from a product of copies of R, and then there's some kernel. But since M is quasi-separated, it follows that this inclusion is a quasi-compact map of condensed sets, which means that this object is quasi-separated, so it's just some filtered union of compact Hausdorff spaces. And this means that when you restrict this inclusion to any of those compact Hausdorff spaces, you get a closed subset, but this is one of those sequential spaces, metrizable spaces in fact, and so being a closed subset on any compact subset is the same thing as just being a closed subset in the topological sense with the product topology. This closed subset is also a module, the same associated condensed module that we are talking about.

So, then we can look at the projection onto the first n coordinates, and let K_n be the image. It follows that K is the inverse limit of

Because if we know that this guy is finitely generated, then it fits into something like this, and we just prove that the kernel is of the same form, and then it will follow that it's finitely presented.

So, but then this is actually elementary. You have like M_1 surjecting onto M_2 surjecting onto M_3, you can make some finite free module rejecting onto M_1, and then you have some kernel of this map here, and then you can make some R-direct some D_2 rejecting onto that, and then you can make a system R-direct some D_1, R-direct some D_1 plus D_2, and in the inverse limit you get a product of copies of R rejecting onto M.

So, in fact, if you're wondering about surjection on the level of condensed objects, in fact you can even get a topological splitting for the map of topological spaces, so by kind of section here, make a compatible section there and so on, if you're not worried about linearity, which you're not, then you can get this quite easily.

Okay, so that's the proof of the Lemma. So, the quasy separated objects are just these kind of classical R-modules, and that also proves the coherence: extensions are easy, extensions are formal, yeah, it doesn't require anything, because it's projective, projective generator, so you can lift exactly.

Actually, because you have this compact projective generator, you're equivalent to the category of modules over a ring, and so it's really, you can actually just quote the usual theorem from module theory, not commutative algebra, sorry, thank you.

Wait, we're still proving the theorem. Ah, well, it was used in order to say that this was the compact projective generator, so when we were—so a priori, you take product $\mathbf{Z}$ tensor up to R and then solidify with respect to R, and only in the finite type over $\mathbf{Z}$ case when you had this surjection from the polynomial ring, yeah, but for any noetherian ring, you can take the product of copies of, yeah, you if you just say that you build a category that has this as compact projective generator with the endomorphisms, what they have to be, then the rest of the arguments here work for an arbitrary noetherian ring.

Okay, and we're not quite done with the first theorem because we still need to show that the product of copies of R is flat. So now we make a claim that if M is finitely presented in solid R, then if you take M derived tensor product R solid with product of copies of R, you just get a product of copies of M, and in particular, this lives in degree zero.

No, and that implies that this theorem or this claim implies that this guy is flat, because the derived tensor product with any finitely presented object lives in degree zero, but every object is a filtered colimit of finitely presented objects, so the derived tensor product with any object will live in degree zero, which is the same thing as saying you have flatness.

So, incidentally, to define the right-derived tensor product, maybe we didn't spend time on this, but in the usual approach, you need to know there are enough objects to relate to $\mathcal{T}$ to define classically $\mathcal{T}$. So here, what is the approach, what's the meaning assigned $\mathcal{D}_{\mathcal{T}}$ Definition: It's not actually necessary to know anything about flat objects. For condensed abelian groups, we do know there are enough flat objects, so that's not needed to discuss them. But I was just speaking off the top of my head about how I think about it. Yes, free objects on a condensed set will themselves be flat.

Okay, so where was I? Ah, we need to prove this claim. The proof is: we can make a surjection where the kernel is finitely generated but also quasi-separated, so it will be finitely presented. We can then continue to make an infinite resolution, which reduces us to the case where M itself is a product of copies of R. Then we can remember that this was a product of copies of $\mathbf{Z}$ tensored with R, and that solidifies to a product of copies of $\mathbf{Z}$.

This nice object we were calling $\mathbf{P}$, the $\mathbb{N}$-indexed union, has the property that its product with itself is isomorphic to itself in the expected way.

Okay, any other questions? This works because the product of copies of a projective object is projective. But you're right that this requires a classical treatment - I couldn't have used that fact without defining the derived category of the solid category first.

You're absolutely right that I should have added the claim that the derived solidification is the left derived functor of solidification, and that the derived tensor product is the left derived functor of the tensor product. This all follows from the flatness and living-in-degree-zero properties we've established.

To define the derived tensor product without solidification, you can just work in the derived category of modules over a commutative ring, and use the standard construction there, without needing flat objects. The key is to have a way to do derived tensor products in the underlying category, which you can then solidify.

Okay, let me spend a bit more time on the homological algebra of solid R. Peter also explained something else in the case of solid $\mathbf{Z}$, that every finitely presented module has an infinite resolution by products of copies of $\mathbf{Z}$.

But it actually has a two-term resolution by products of copies of $\mathbf{Z}$, so that at least from the perspective of finitely presented objects, you have sort of homological dimension one for solid $\mathbf{Z}$, just like you have for usual $\mathbf{Z}$. I want to present a generalization of that.

Theorem: If R is of finite type over $\mathbf{Z}$ and is a regular ring of dimension d, then if you have M in solid R finitely presented and N in solid R arbitrary, the $\mathrm{Ext}^i(M, N)$ vanish in degrees bigger than the dimension.

A corollary of this will be that if you have a finitely presented module, then you actually get a projective resolution of length at most d or $d + 1$, depending on how you define length. And then we already saw that the projective objects are flat, so this also implies a bound on the Tor groups.

However, this statement does not extend to non-finitely presented M. Unlike in the case of discrete modules over a regular noetherian ring of finite dimension, where you actually have the Ext vanishing for all modules, the argument for going from the finitely presented case to the general case does not work in this context. In fact, I think you can have arbitrarily high non-vanishing Ext^i even for solid $\mathbf{Z}$.

Let me give a fun exercise you can try to do. Take two distinct prime numbers p and q, and consider the module $\mathbf{Z}/p\mathbf{Z} \oplus \mathbf{Z}/q\mathbf{Z}$. Then there is a non-zero Ext^2, obtained by switching the roles of p and q. This suggests that there can be arbitrarily high non-vanishing Ext^i groups, even in ZFC.

The issue is that when going from the finitely presented case to the general case, you need to analyze derived inverse limits along arbitrary filtered systems, and the cardinality issues come into play. If you are in a model with the Continuum Hypothesis, then perhaps you can get a bound. But in general, it seems that arbitrarily high non-vanishing Ext^i groups are possible.

Let's now try to prove the theorem. The first case is when N is a classically finitely generated module over the noetherian ring R, and M is a quasi-separated, finitely presented solid R-module. In this case, the claim is that the $\mathrm{Ext}^i(M, N)$ groups vanish for i greater than the dimension of R.

Let's just say discrete is the same thing as the filtered colimit of xt_i over R, so this is R-solid xt_i over R from m to n. Didn't you prove that presents imply quasi-separation?

No, certainly not. In solid $\mathbf{Z}$, something like $\mathbf{Z}_p/\mathbf{Z}$ will be finitely presented but not quasi-separated. So what was proved is that if you're quasi-separated and finitely generated, then you're finitely presented. But if you're finitely presented, you are not necessarily quasi-separated. For example, $\mathbf{Z}_p/\mathbf{Z}$ is finitely presented.

So if a map from a product of copies of R is not coaccessible, that does not necessarily imply it's not quasi-separated. That's right, I misunderstood this. That's important.

So we need to show that we can pull the inverse limit out of the $\mathbf{RHom}$. By the way, the claim for internal $\mathbf{RHom}$ follows from the claim with underlying sets, just replacing n by continuous functions with values in n for some profinite set S.

We have the Mittag-Leffler resolution, some kind of usual identity minus shift sort of thing, or maybe it's a shift times F or some kind of shift map. This distinguished triangle tells you that the question of pulling $\mathbf{RHom}$ the inverse limit out is the same as the question of pulling out a product and turning it into a direct sum. We can resolve $\prod_n M$ by $\prod_n R$ to reduce to $\mathbf{RHom}(\prod_n R, n) \cong \bigoplus_n \mathbf{RHom}(R, n)$, which follows from R being projective.

So from this, it follows that we get the desired vanishing of xt_i if n is a classical discrete R-module and M is finitely presented and quasi-separated. Now if M is arbitrary finitely presented but not necessarily quasi-separated, you can always resolve M by a product of copies of R, and then the kernel will be both finitely presented and quasi-separated. Analyzing the long exact sequence, you would get the desired result but with a loss of one degree of homological dimension: xt_i would vanish for $i > D+1$, where D is the Krull dimension of R.

So this naive argument only gives $xt_i(M,n) = 0$ for $i > D+1$. We have to do a little bit of extra work to improve this to the conclusion that $xt_i(M,n) = 0$ for $i > D$.

We didn't use regularity here, but I should maybe recall the classical fact that if R is regular of Krull dimension D, then the classical xt groups between discrete modules vanish in degrees greater than D. So the question for quasi-separated things in the solid context reduces to the classical question in discrete commutative algebra, and that's where we use regularity.

We're going to do a less naive argument. Instead of just a product of copies of R, we can surject again from a product of copies of R, and now at this point we take the kernel. This is still quasi-separated, and it suffices to show $xt_i(M,n) = 0$ for $i > D-2$. We bought ourselves one extra drop in the vanishing bound because we continued the resolution one step more.

This will work assuming $D \geq 2$; the case $D = 1$ can be handled by similar arguments in low dimension. Now we want to find a nice expression of n as an inverse limit of finitely generated R-

It's a closed submodule here. We can always just look at the first method. We analyze this map here. This is a map from a product of copies of R to a product of copies of R again. That's the same thing as a map on the pro-system. If you restrict to any initial chunk of this, then there's some corresponding initial chunk here where the map projecting onto that factors through projecting onto this chunk, and then just a map of discrete modules there.

If you reindex and allow finite free modules here instead, then you can assume that this map here comes from an inverse limit of compatible maps from the nth initial chunk here to the nth initial chunk here, just by reindexing. And then if you take the kernels of those maps, we can let n be the inverse limit over n of n_n. But the other thing we can do is we can let n'_n, which will be contained in n_n, be the image of n mapping to the inverse limit of f_n mapping to f_n.

Number one has some good and some bad properties. The good news is that you have this $x_i - 2$ vanishing, $x_i n_n x = 0$ for all i greater than $d-2$. But the bad news is no guarantee that this system n_n is Mittag-Leffler, which is needed for $x_i n x =$ filtered colimit over n of $x_i n_n x$.

In the argument I presented, the case where the transition maps were surjective, the argument really only used that it was a Mittag-Leffler system to get this resolution. And in the second case, the situation is opposite: Mittag-Leffler, but no guarantee that $x_{d-1} n'_n x = 0$.

We know that n is the limit of both in the condensed category, limiting the condensates of both of these systems. But you can produce an example of a map from $\varprojlim n$ to $\varprojlim n'$ for which the kernels would not be Mittag-Leffler.

What we have to do is find something in between that has the good properties of both. The reason we have this nice property here is that it's a kernel of a map between projective objects. But for this n'_n, you don't know that it's a kernel of a map between projective things, you only know that it's sitting inside a projective thing. So you get vanishing of x_d one better than for a general module, but you wouldn't get vanishing a priori in degree $d-1$.

We're going to find n'_n sitting in between them, such that we get x vanishing and n'_n is Mittag-Leffler. If we get this, then we're done, because the inverse limit, we would get the desired x bounds for the inverse limit of the n'_n, but that's sandwiched in between these two things which have the same inverse limit, which is the thing we're interested in.

The obstruction is a question of depth. The Auslander-Buchsbaum formula tells you that for a regular Noetherian local ring, the depth is the codimension.

Projective dimension of a module plus the depth of the module is equal to the dimension of the Ring. Because we have the one better estimate on the projective dimension, the only rings that are going to give us obstruction are the local rings at prime ideals, which are actually maximal ideals. So the local ring has maximal dimension, and the only obstruction is going to be moving from a situation of depth one at that maximal point to depth two.

At a maximal ideal, the module only has depth one. Yes, yes, yes, many closed points, and you know that it lies inside the other one because the other one is reflexive. And then you have to establish, but only at the closed points, the classical argument works. That's correct, yes.

Okay, maybe I'll just repeat what was said. So what you can do, if you let's say for simplicity you only have a problem at one maximal ideal (in general you'd fix a problem at finitely many), and then if you increase the number of them, eventually the situation would stabilize by an argument. So let's say there's a problem only at one maximal ideal. Then you can look at the inclusion of Spec R minus that closed point into Spec R, and then you replace the module by the extension-restriction of the module. That receives a map from the module, but it's in fact an inclusion due to the depth one assumption on the module.

But then, on the other hand, by this procedure of this extension and restriction, depth is characterized in terms of local cohomology at the maximal ideal, and that's exactly what comes up when analyzing the difference between this and say the derived version of this. And using that, you can see that this improves the depth to depth two at the closed point. On the other hand, if you were to perform the same construction for the original module, since we already know this has depth two, you wouldn't have been changing it. So you really are producing something sitting in between which fixes the problem at a given closed point. Then there's the noetherian property which tells you well, it's only finitely many closed points that are going to be involved, so you can, by the same procedure, fix all the problems for the module in a compatible manner in the tower. Okay, so that's how you've got it to be depth two at all the closed points, which gives you the desired vanishing for these modules.

And then what about the Mittag-Leffler condition? This tower of modules sits in a short exact sequence with the module and the quotient module. To show that this is Mittag-Leffler, you need to know that the quotient is Mittag-Leffler, and that is Mittag-Leffler because the transition maps are surjective. But this one here is supported at finitely many closed points and is a finitely generated module over the ring, and that implies that it's finite as an abelian group. Because remember, our ring was finite type over $\mathbf{Z}$, the maximal ideals all have residue fields which are finite fields, and a compactness argument shows that any inverse system of finite abelian groups satisfies the Mittag-Leffler property.

Okay, so that gives the vanishing. To sum up, we've seen that if the module is finitely presented and solid over the ring, and if the object X is a discrete module over the ring, then we get the desired vanishing.

Now, suppose X is quasi-separated and finitely presented in solid R. We can write X as an inverse limit of X_n with surjective transition maps. Then the R-homs into that will just be the inverse limit of the derived inverse limit of the R-homs into each of those terms. In principle, you might get one worse again because there could be a $\lim^1$ in the last inverse system, but because the transition maps are surjective and D is the largest degree at which you have nonzero vanishing, you actually see that on the X-dual, you get surjective maps, so there's no $\lim^1$ potentially giving you something in degree $D+1$. So you get the claim for D.

And then for X arbitrary finitely presented, you resolve it by, and the X with values in X can only be better than the X with values in these two, so you get that situation as well. And then

Arbitrary, so if you write X as a filtered colimit of X_i, where the X_i are finitely presented, then $\underline{Hom}(M, X)$ from any finitely presented M to X is the filtered colimit of $\underline{Hom}(M, X_i)$, by the pseudo-coherence of M. So you can resolve M by a product of copies of R, and then this follows from products of copies of R being compact projective.

What about $\underline{X}$? It's the same—from this point on, this claim for an arbitrary discrete module implies the same claim for $\underline{X}$, because it's the same as the S-valued points of this thing, which is the same thing as $\underline{Hom}(M, S)$ for continuous functions from S to this discrete thing, which is just another example of a discrete thing. So then you get this there, and then from that point on, all of the arguments actually work at the internal level.

Okay, including the Lim 1 argument, yes, because you reduce it to showing that a Lim 1 vanishes in condensed abelian groups, and the terms in the system are discrete and the system is Moeglin, and those properties are preserved by taking continuous functions with values in that—it's still a system of discrete things and the transition maps are still Moeglin or subjective.

In the condensed abelian group, again, you have only up to Lim 1, because you can compute it termwise. Yes, because countable products are exact. Okay, and you can compute the Lim 1 term by—no, maybe this is not, you can compute Lim 1 on each object, not necessarily projective.

You don't have projective computations like in any site where it's etale, you cannot compute, but here you can compute the Lim 1 on any test condensed set. Uh, no, no, for some things, yeah, so certainly for this $\underline{P}$ object you can, because that's projective. And then that's enough for solid, because we know that the solidification of $\underline{P}$ generates. So that's one argument you could give. I'm sure there are other arguments as well.

But you know, the Lim 1 is always just going to be the sheaf of the, you know, the condensed Lim 1 will always be the sheaf of the naive sectionwise Lim 1, and so if you prove the vanishing of the Lim 1 sectionwise, that's enough to prove. Yes, I missed where coherence comes from. Ah, right, so that comes from the claim that the finitely presented objects form an abelian category, which has as a corollary that for any finitely presented objects, you can build an infinite resolution where all of the terms are products of copies of R.

You're welcome. And to have non-quasi-separated finitely presented objects, the ring has to be of dimension at least two, or non-quasi-separated finitely presented has to have dimension at least one at least one. Yeah, so if you have a, if you're over a finite field, then every finitely presented object is quasi-separated, but once you move to say the integers, then $\mathbf{Z}_p/\mathbf{Z}$ is an example of something that's not quasi-separated but is finitely presented.

Also, the step of reducing from quasi-separated finitely presented to discrete is easy—it's not Lim 1 here. Like, which step? Sorry, this step here. Yeah, you see that there's no Lim 1 because the only Lim 1 that can contribute to degree $D+1$ is the Lim 1 of the X_D's, and because on the level of these guys, we know that there's nothing, there's no X for any discrete module in degree $D+1$. You see that if you have a surjective map, with then X_D's into it will also be surjective, because the obstruction is an X_{D+1} of the kernel, which vanishes, so then the

Ways to localize the same category. This is by no means the most general possible localization. It's the one we discussed because it's the one that's most closely related to Huber's Theory. And our goal in this class is to explain the basic definitions in our Theory and their relations to more classical theories of analytic geometry.

But I just want to point out very briefly that there's also a whole different avenue you can go for localizing this, which even more radically, I guess, departs from Huber's formalism. So we already saw a small departure in that we were allowed to do slightly more localizations of a Huber ring than before, because of this difference between valuations that are less than one on topologically nilpotent elements and ones which are valuations which are continuous in Huber sense. But kind of even more drastic things are possible.

So, in fact, if $C \subset \operatorname{Spec} R$ is any constructible subset—so that means an intersection of a quasi-compact open with the complement of a quasi-compact open, so it's some locally closed subset of $\operatorname{Spec} R$ which is sort of finitely presented—and of those, a finite union. Yes, yes, yes, yes. So the basic objects which sort of form a basis for the constructible topology would be something like you take $D(g)$ intersect the complement of the common zero set of finitely many functions.

To this, you can assign an idempotent algebra in $\mathrm{D}(R\langle\langle z\rangle\rangle)_{\text{solid}}$, namely, to this basic object, $D(R[1/g]) \cap V(F_1, \ldots, F_n)$, you assign the thing you get when you take R and then you invert g and then you derived complete along $F_1, \ldots, F_n$, which this final object only depends on the constructible subset. And you know, by the way, if R is an Artin ring, then this derived completion is just the usual completion. But I want to emphasize that I'm taking this derived completion in the derived category of solid Abelian groups, so I'm, so to speak, putting the inverse limit topology on this derived inverse limit here.

No, such things then you say that you ah you view your constructive subset as a union of locally closed, you also look at the intersection, so on. So for each of them, each finite intersection, you do this, and then you take the what? Okay, you take the limit of this type thing, and it won't be concentrated in homology, they'll have some cohomological degree, yes. So is it the case that the intersection of two basic things, what you get, is a tensor product? Yes, okay, so it is. Intersection, oh yes, yes, that's right, sorry.

Right, so maybe I shouldn't actually say that you can assign idempotent objects to any constructible subset, maybe I should just say that you can assign a idempotent object to any basic constructible subset. So you want to take the kind of the derived inverse limit of that idempotent object associated to all basic things inside the given thing, but then you have a problem to prove properties of this. I don't know if it follows that it is an algebra, or you're looking at those subsets, what? Yeah, constructible, locally closed, is if it's constructible, locally closed, so take $\mathrm{R}\Gamma_{\text{cts}}$ of this, so it will, could have some cohomological degree depending on the number of α and γ to cover, yes, yes, yes, yes. So that's—but thank you, Peter, that's better.

Yeah, so then the only derived behavior comes from the union of principal opens, thanks. Yeah, that's what I should be saying. Okay, so now why are these idempotent? So recall that the solid tensor product, they, if me, idempotent means that, let's say that this is idempotent, means that a tensor over R solid derived a is the same thing as derived complete $\mathrm{R}\langle\langle z\rangle\rangle_{\text{solid}}$ a. Is derived complete, so to check

So, you have a ring, and you have finitely many elements, or any number of elements, I suppose. And the derived solid tensor product of connective objects, so in homological degrees, will still be derived complete, why? Because we proved it, or yeah, we—well, Peter gave an argument for the special case of like p, and then the general case is quite similar, so he gave the heart of the argument of the general case.

You have to write, yeah, you have to, you have to do some work, you have to do some analysis, but it's not um, yeah, it's something we've discussed in previous lectures.

Okay, so now, and then, so now the claim that I would like to make is that if C_1 up to C_N are locally closed, constructible, and if their union, set-theoretically, is all of $\operatorname{Spec} R$, then these guys cover, let's say, these the corresponding A_i these idempotent algebras, cover $\mathrm{D}_{\text{ét}}(R^{\text{zt}})$ solid, in what sense? So in the sense that if you have M in the derived category of R^{zt} solid, such that $M \otimes A_i = 0$ for all i, then $M = 0$.

But, yes, thank you, thank you very much, yeah. So, and this implies that you get $\mathrm{D}_{\text{ét}}(R^{\text{zt}})$ solid is some limit over—so in the first term, you have a product of product over i of A_i modules in $\mathrm{D}_{\text{ét}}(R^{\text{zt}})$ solid, and then you have a product over i, j less than j, modules over the tensor product $A_i \otimes A_j$ in the same thing, and then so on, you'll have some finite check, uh, thing. What, sorry, cover if they, what?

I like that, they're giving it a big hug, that cover. Uh, yeah, so if they love $\mathrm{D}_{\text{ét}}(R^{\text{zt}})$ solid, okay. Anyway, and the basic idea, idea of the proof is that it suffices to show that R is generated by A_i modules for varying i.

So, for if I just take the example of Spec R is D_f union $Z(f)$, then you use, then $r_{\frac{1}{f}}$ is okay, because it comes from here, and then the difference between R and $r_{\frac{1}{f}}$ is, uh, so generated in a finite manner, finitary manner. But this is the union of like R/f^N with some shifts, uh, this is actually the fiber over here, and this is actually an R_f-complete module, so it lives in $Z(f)$. So, if you take all modules over R_f^{-1} and all modules over R_f-complete, and then generate things just by triangulated category nonsense, eventually you're going to hit R, and then tensoring with anything, you'll see that you hit anything, and then using that, you can

Inverting and it famously matters in which order you do this. But it all comes from this sort of commutative situation nonetheless in this way of setting it up here. I want to make the point that these things are like kind of like higher local fields that people have studied, and they tried to study them from their perspective of topological rings and topological fields. And there were just terrible problems. Matthew's not here right now, but he even wrote a paper explaining that everything is horrible. But if you put them in the condensed world, then they're just perfectly well-behaved objects that you can work with. It arises from this natural procedure and it's idempotent even in the ∞-category sense over this ring.

So, I invite you to try to draw pictures of these covers to get an understanding for what's going on with these kinds of constructions. But okay, I think I'll stop here.

Yes, for finite-type $\mathbf{Z}$-algebras R, the solidification of the countable product of $\mathbf{C}$ and $\mathbf{R}$ is just a countable product of $\mathbf{C}$ and $\mathbf{R}$. This is also true for something like the ring of powers of $\mathbf{Z}$, which is not finite-type, but it's also not discrete. So, yeah, it's determined by the topological ring structure.

You could also take a finitely generated ring and an arbitrary ideal, and look at the completion, and then you get the same statement. The basic compact projective generator is just the product of copies of the I-adic completion of R. And that's the same time if the ring is not finite-type. You also said it's the union of finite-type algebras. Is it easy to see for this? No, but $\mathbf{Z}[[T]]$ — sorry, that was a statement about discrete rings. So, in general, if you have some ring that's complete with respect to a finitely generated ideal, you'll get the good answer if your ring modulo that ideal is finitely generated. Maybe this is a good way to understand it.

If the ring modulo the ideal is finite-type over $\mathbf{Z}$, then you'll get the naive answer, I think. Yeah, sounds reasonable at least. Solidification is determined by the underlying topological ring structure.

Yes, so the same formula doesn't work for $\mathbf{Z}[[T]]$. $\mathbf{Z}[[T]]$ is a discrete ring, yeah. As a ring, you're looking at $\mathbf{Z}[[T]]$ as a discrete ring, yeah. Well, then it doesn't hold. No, but if you look at $\mathbf{Z}[[T]]$ non-discrete modules, the solid theory is the same as the solid theory in $\mathbf{Z}[T]$.

Once you've decided to be complete, then it's enough to solidify stuff modulo that ideal you're complete along. Okay, yeah, so does that address your concern?

Okay, so what again? You take a discrete ring and take the associated condensed ring, and you consider solid relative to the elements of the ring. Okay, just each element of the ring. And then you solidify, so if you consider this condensed ring with the product topology, you could either solidify every element in the underlying discrete ring or you could just solidify T, and it's the same. In fact, you don't even need to solidify T. I mean, you Proposition for finite type. So if I just, for example, like $\mathbf{Z}_p$, the p-adic integers, yeah, was there a computation? Oh, you're wondering about these homological dimension results for $\mathbf{Z}_p$, yeah? So there you get, uh, yeah, now I have to remember. I think, um, yeah, there you should get also, it should just only be X_1, uh, yeah. That's actually even easier to show than in the case of solid $\mathbf{Z}$, because $\mathbf{Z}_p$ is compact, so you can directly see that all the finitely presented objects there are also quasi-separated, and it's the they're also all those objects are quasi-separated, and they're just like, yeah, inverse limits of finite $\mathbf{Z}_p$ modules, uh, countable inverse limits of finite $\mathbf{Z}_p$ modules, and that makes the whole analysis much easier.

To summarize, so for let us say R is a field, yes, then the finitely presented are quasi-separated. In fact, they are all given quite simply by you just have the either infinite or final product of the $\mathcal{A}_\bullet$. So that's okay for our domain, except the fact that you can have a, the torsion group is maybe not trivial, but still it's a product of, uh, you have a resolution, you have product copies of R, and then the Koszul is something like product of maybe, ah, actually you can use the the Artin-Rees Lemma to get rid of this anyway. So you can, it's always this, this okay. So it is a resolution of length 2, like like before, because we don't have to worry. And so we get, and then in dimension at least two, you do your, the pro, the, uh, that we discussed, yes. Okay, that's the, yeah. And then there's if you wanted to move to non-discrete rings, then you'd have to do more analysis and so on.

Yeah, yep, okay, thanks everyone.

12. What happened so far? (Scholze)

https://www.youtube.com/watch?v=VMgZSP9sRdo&list=PLx5f8IelFRgGmu6gmL-Kf_Rl_6Mm7juZO

Unfinished starting from 0:00

Good morning. For the people here, you can fill out the survey there now or anytime this week or next week.

Alright, so where are we in the course? Let's try to take a little bit of a broader view. What happened so far? We started with this idea of condensed sets, the being cured, and so on—condensed mathematics. We tried to make the point that this is a good framework for combining homological algebra and functional analysis, with the goal of using this framework to develop a general, good notion of geometry.

At some point, we wanted a notion of completeness. We went in a certain direction, namely concentrating on solid modules—first over the integers, but then over more general rings of finite type, as discussed last time. This is closely related to a specific framework we discussed, which is extremely closely related to Huber's theory of adic spaces.

There were certainly some conceptual ideas that emerged from this, namely that the notion of completeness shouldn't be something that you define once and for all, but rather is an extra datum that is part of the data of what a commutative ring is in the setup. Completeness is a more relative notion.

So what are we trying to get through in the course? We have this category of analytic rings, and for reasons we already saw a little bit, when you localize, some of the structure might become not well-defined anymore. We might change the precise definition here a little bit to work with derived rings from the start, but I don't want to go into that today.

We want to start from this category of analytic rings and, as in usual algebraic geometry where you start with commutative rings and then build schemes by gluing the spectra of commutative rings, we want to have some procedure where we start with this category of analytic rings and then do some kind of gluing to produce some notion of analytic spaces, or maybe we'll directly go to some category of spectra.

Within this world, we want to find all sorts of examples. We definitely want our theory to accommodate geometry over the real or complex numbers, not just non-Archimedean geometry. So first, we would like to see more examples.

Some key questions are: How do the real numbers fit into this? Can we put a natural structure on the real numbers that is suitable for doing complex or real geometry? And can we also do non-Archimedean geometry using solid modules? What is a meaningful way to combine the two settings?

I want to develop one way of thinking about this, with the following example in mind. Maybe the most prime example of what we should be able to do in some world of geometry is the famous Tate elliptic curve.

It's actually, if you look at Fargues and Fontaine's paper, I think it's the one called "The Geometrization of the Local Langlands Correspondence," there really is also one of the first applications of a Serre's ideas, again discussing like the "tilting" of Deligne-Mumford stacks. This is a generalization to higher dimensional Beilinson-Bernstein varieties.

So, you have the étale spectrum of like $\mathbb{W}_{\mathbf{R}}$ series. Over there, you can look at some of the Fargues-Fontaine curve, which is an analytic space over $\mathbf{R}$. Informally speaking, that's all—there's some coordinate T here, and T should have the property that the absolute value of T is bounded between some $\mathbf{Q}_p$-valued elements.

And then you look at the part of the multiplicative group where the absolute value of T is bounded by a power of one of those $\mathbf{Q}_p$-valued elements, and then acting on this, you have multiplication by a character. This is actually a totally discontinuous operation because it multiplies the absolute value of T by the absolute value of the $\mathbf{Q}_p$-valued element, which is something between zero and one. So, it's actually a free and totally discontinuous action. And so, in the world of étale spaces, you can really pass to the quotient, and the taking this quotient is the nicest kind of quotient, because otherwise you just get where the quotient is like locally split, and locally the space just looks like this.

And so, you can define the Fargues-Fontaine curve to be this analytic Deligne-Mumford stack, and then you quotient by the $\mathbf{Z}$-action. And I mean, how does it look like? You started with, I don't know, $\mathbb{A}^1_{\mathbf{R}}$, and then by modifying by the $\mathbf{Q}_p$-valued element, it's moving everything towards the origin, but then when you take a quotient, this somehow is the same thing as taking some annulus of radius one of this $\mathbf{Q}_p$-valued element and then identifying the boundary.

And so, using this, you can actually see that this is some—yeah, it's actually proper, so it's proper and separated, and it's also smooth because it's locally just a Deligne-Mumford stack. It's a proper, smooth, connected analytic curve over $\mathbf{R}$. And actually, also, I mean, there's a group structure on the Fargues-Fontaine curve, given by some subgroups. It actually has a group structure.

And basically, in any world of analytic geometry, there should be a theorem that something that's proper, smooth, and one-dimensional is always algebraic, because you can always find an ample divisor by just taking any closed point and then taking the corresponding inverse ideal sheaf. This gives an ample line bundle, and then if you have any kind of version of GAGA or your favorite theorem which should always show that they imply the properness and algebraicity of the thing, and so it's definitely true here.

So, there—actually, so here, there implicitly there is some GAGA for over a nice noetherian base, Fargues and Fontaine's pairs, but also the statement about relative dimension one,

The coefficients of Q to the n in here are just polynomial in n. When you do this geometric series, the new coefficients still stay polynomial in n. This is a funny observation, because it implies that after just defining something formally as a power series, when you actually write down the equation, you realize that this is something you can specialize to any Archimedean value between 0 and 1.

You can certainly take $\mathbf{C}^{\times}$ as a complex analytic space, as a complex manifold, and for any complex number between 0 and 1, this picture makes literal sense—you can literally take that portion and you get an actual torus. So you can do that, and the analytic description makes perfect sense.

We would really like to have a way to do geometry so that we can perform this kind of construction of $\mathbf{Z}[Q]$ not just over the full power series algebra, but really over some subalgebra where we put some growth condition on the coefficients, so that we can also later on specialize to more general parts. The precise growth condition that you get here is quite a bit stricter than just the observation that it converges when the absolute value of Q is less than 1.

This convergence is just a subexponential growth of the coefficients. It's pretty clear that geometrically, there should be a direct geometric way of seeing that this power series should converge when Q is less than 1. There's some kind of geometric reason for the coefficients having at most subexponential growth, but it's not so clear how you would see that they actually have at most polynomial growth.

Let me actually talk a little bit about the geometry of the space of continuous valuations on $\mathbf{Z}[[Q]]$. You have this topologically nilpotent unit, and this is an extremely convenient structure to have, because it allows you to compare absolute values of all other functions against the absolute value of Q. There is a unique map from this space to the Berkovich spectrum, where the absolute value of Q is sent to some pre-specified element between 0 and 1, say $1/2$. For most of the valuations we care about here, this map will be injective, but there are some rank 2 valuations where there's a little bit of extra information in the geometry that's not remembered by this map.

Next, let's consider a set of all "absolute values", but now they really take values in $\mathbf{R}/\{0\}$ instead of just $\mathbf{R}$. Here, you ask the following things: First, there are some basic properties, like the norm of 1 is 1, and the norm is multiplicative. Second, the valuations satisfy a strong triangle inequality, so it considers the usual triangle inequality, but also requires that they are all bounded by the specified norm on $\mathbf{R}$.

This naturally leads to the product of copies of $\mathbf{R}^{>0}$ enumerated by all elements x in $\mathbf{R}$, endowed with the Subspace topology. Actually, because of this construction, you can always replace $\mathbf{R}^{\geq 0}$ here by the interval from $|x|$ to ∞, and then an arbitrary compact product of compact $\mathcal{H}$-spaces is still compact $\mathcal{H}$. So this is a nice compact $\mathcal{H}$-space.

The only difference between this space and the p-adic spectrum is the possibility of having higher rank things in the p-adic spectrum, but these just give rise to some minimal changes in the space. Implicitly, I'm endowing the integers here with a norm where the norm of 0 is 0 and the norm of all non-zero elements is 1, since they all contain a unit for the ring I'm considering.

So I still want to understand a little bit about this geometry. It's more or less the same thing as the Berkovich space of the integers, which has a much richer geometry than the usual spectrum of the integers. Here's a proposition: The norm on $\mathbf{Z}$ I'm considering is the one where the norm of 0 is 0 and the norm of any non-zero integer n is 1, since they all contain a unit. There is also a more natural norm you can put on $\mathbf{Z}$, which is just the absolute value. For each prime number p, you have the p-adic absolute value, which also satisfies all the required properties, but with a choice of what the absolute value of p is. This gives rise to a "ray" for each prime, and the inverse limit of joining these rays corresponds to maps to complete p-adic fields, including the usual rational numbers $\mathbf{Q}$ with the standard absolute value.

All right, let me also already discuss the side variation of this. You can also take the $\mathfrak{B}$-space of the integers with the usual absolute value, so $|n| = |n|$, the positive version of it.

This has the same pictures. The difference between these two things is just the boundedness condition. Previously, the absolute value of 2 could never be 2 like it would be for the real numbers. But now, the absolute value of 2 can be 2.

So this is actually some of the same picture. There's again this thing, for each $\mathbf{F}_p$ number part, but now there's something extra—there is some kind of Tate interval. This corresponds to the map from $\mathbf{Z}$ to $\mathbf{R}$, and then you take the usual absolute value of r raised to any power α, where α goes from 0 up to 1.

It's better to think of this if you take the usual absolute value, Peter, yes, can I vote that you use α instead of p when talking about raising an absolute value to the p power? Thank you. Just in time, to the α. If I parameterize this in terms of some fixed absolute p, where $|p| = 1/p$, then this line here corresponds to the α where now α can be anything from 0 to infinity. But for the real numbers, you fix the usual absolute value, then you can raise that to any real power, and it will definitely satisfy the conditions, but actually, if you want the triangle inequality to be satisfied, you realize that this happens only if α is at most 1. So in this sense, this line for the real numbers is actually some stops in the middle compared to the others.

Okay, right. So where are we? We are trying to understand a little bit about the geometry of what the spectrum of $\mathbf{Z}[\frac{1}{q}]$ actually looks like, and it's basically the same as the $\mathfrak{B}$-spectrum, and this is fiber over the $\mathfrak{B}$-spectrum of the integers. Now let's actually try to understand what are all the fibers of this map.

For example, if you take the fiber at a prime p of just $\mathbf{F}_p$, well then it's just one point. Then, as usual, some of the formation of the closed space commutes with some kind of fiber products, and so the rings $\mathbf{F}_p[\frac{1}{q}]$ are just a point, because they're already non-fields, and you fix the value of q to be the half.

In characteristic p, the thing has just one fiber, which is this kind of the wrong series $\mathbf{Q}_p$. Maybe I should have said, I mean, this proposition is basically just 0, right, because of the absolute values. But then you can also have this spectrum of the integers, and then for each p, you have this half-line of $\mathbf{F}_p$s. If you base change this whole half-line of $\mathbf{Q}_p$s, someone looking at, you're in some sense taking, now this is some kind of punctured open unit disc, and now you're this base change to $\mathbf{Q}_p$ will actually be some kind of punctured open unit $\mathbf{Q}_p$, so this will actually be a punctured disc, and this map here to this line 0 to infinity, depending on how you parameterize it 0 to 1 or whatever, this should be an incarnation of the radii matter, but actually in a slightly funny way, it's say 1 over the log of the radius or something like this, I won't get it straight.

So there is a whole punctured unit disc here, which has an origin and a boundary. Whenever you fix a specific point on here, then this fiber will be some specific annulus in here, where the absolute value is fixed. And now you can wonder what happens as you move towards the invisible, anyway, but I'm always tempted to try to see the image is this circle here, and then when you move towards

Whereas when you move upwards on this Ray, go towards $\mathbf{Q}$, then you will get other points, and they will get closer to the origin. Okay, so what does this thing actually look like?

There is one special point which is $\mathbf{Q}$ and then there are special points which are $\mathbf{Z}$. And on the way there, you have the punctured open disc. The region in the middle, the punctured open disc, and the slightly mind-bending thing is how the different parts are glued to each other. I mean, the whole thing is a compact Hausdorff space, so it makes sense like to ask when you go in this direction, where you end up.

And so, if you move towards the puncture of this open unit disc, you move towards this point, and you move towards the boundary, we end up towards this point. So this whole space is a space that has p-adic regions for each p, each p-adic region is a function open $\mathbf{Q}_p$, and then they glue to each other in this funny way, where for each one, if you go towards the center, you will end up at the common point, which is is a kind of generic point, and for each one, when you go towards the boundary, you end up at the characteristic." Now it has some kind of function open over the rational numbers, both p-adic places and the archimedean place. And everywhere when you go towards the center, you always end up with the center point of the picture.

The slightly awkward feature of this picture is that I had to now specify the absolute value of $\mathbf{Q}$ in advance, and then as the archimedean part of this picture, this punctured open just stopped at the boundary, although from the perspective of the p-adic curve there was no reason for stopping at all.

Right, so then one—but like this is precisely the kind of picture in which we would like to combine non-archimedean geometry. I mean, this space has parts which are literally complex analytic or real p-adic analytic, but they sit together in one job. And so one kind of PR version of the question about existence of analytic $\mathbf{R}$-structures is now like $\mathbb{K}^1$ and this guy or more any algebra for the vector—was a natural, and this has been a question that was very much in our minds back when we first found out about the solid theory and then tried to really go further.

It actually turns out that to define this, it's slightly better to work with not just precise the Hahn convergence condition, but functions which converge on some rate, some slightly larger dis. That's more techn. Generally speaking, for any kind of $\mathbb{B}$ offering which has a topological unit in the usual way, you can produce this kind of liquid analytic, and the resulting theory will be extremely close to $\mathbb{B}$ which theory, and this is something that we want to discuss at some point today.

However, I want to also talk about something else, something that we only found out a few weeks ago. Once you have this liquid and structure over $\mathbf{Z}$, maybe greater than a half, you can literally repeat the construction of the p-adic curve that I did in the beginning over this ring. And then someone show that the p-adic curve is definitely defined over this ring, and then okay, in the end you could also make a half larger and larger and would get that it's defined over the whole open unit dis, but you would not get this way the strange Tooumi draws bound on the coefficients.

There is actually a description of the "liquid" structure of the three modules. Yes, let $S = \bigoplus_{i=1}^{n} \mathbf{Z}$ as usual. Then in this ring, we take $\mathcal{H}(Q) = \mathbf{R}$, and this S defines the three "liquid" modules on S over this ring. The hard part is to prove that this actually gives an "analytic" ring structure defined as follows.

Just like $\mathbf{R}$ is the union of all intervals $[a, \infty)$ for $a > 0$, also the "free" modules are this union over intervals $[a, \infty)$. But then also, as in the free discrete case, we have this union over all size values, and this time I maybe want to index them by some real numbers $c > 0$. But then once you fix the radius and size, you're just taking an inverse limit of such free modules of the part where the norm is at most c.

A similar situation where you can show that if you bound the norm here and give this a certain kind of topology, this actually becomes a compact $\mathcal{H}$ thing, the limit is still compact. This naturally suggests itself when you try to define some notion of "complete" modules over this ring, because the p-adic norm on this ring is defined just this way for one element, and then of course if you have a free module, you're just summing the absolute values.

Let me actually just give a concrete example. In the beginning of my lecture today, I was asking two questions: first, is there a natural structure on the reals, and second, is there one which allows you to combine things. Now I'm starting to answer them in the opposite order.

First I said that there's something that combines them. Let me now also give the answer to the first question: what are the "analytic" ring structures on the reals? You can specialize from $\mathbf{Z}[t^{1/2}, t^{-1/2}]$ to $\mathbf{R}$ by sending t to some number T here less than $2^{1/2}$. This defines a point of the $\mathcal{S}p$ space that maps to the $\mathcal{B}$ space of $\mathbf{Z}$, and so must actually correspond to some power of the usual absolute value on the reals.

So inside the $\mathcal{B}$ space of $\mathbf{Z}$, you have this half-interval for the reals, where α from 0 to 1 corresponds to the absolute value on $\mathbf{R}$ to the α power. And now this map is actually realizing some isomorphism between these two things, where the value of T is mapped to some real number R, and this can be made explicit: $T = R^2$.

Okay, so the value of T determines some α, and I could have just told you the formula $\alpha = \log_2(T)$. But this would seem slightly curious—what does it mean? The meaning is that you have a point in the $\mathcal{S}p$ space that maps to the $\mathcal{B}$ space of the integers, and you get some point there. This is the α.

So we have a structure here, and we get one here. But the "complete" modules

The norm $\|x\|_\alpha$ is a sum of $|x_i|^\alpha$. Note here that $0 < \alpha < 1$, so this is not one of the usual kinds of norms that you would usually consider in real functional analysis. This is not locally convex. Usually when you put L^p norms, the p lies between 1 and ∞, but here we're going to the left and using this non-locally convex norm.

This norm does satisfy the triangle inequality, but it doesn't satisfy the usual scaling by real numbers. You raise it to the α power, but usually you'd ask that it satisfies the scaling with respect to the usual real numbers, which this does not do.

However, if you just look at the unit balls in a 2-dimensional vector space, they have a very peculiar shape, something like that. So you might be tempted to think that this is a stupid way to put a ring structure on the reals, and you should do something else. But this does work.

I guess that the completed modules on $\mathbf{R}$ should be the bounded measures on $\mathbf{R}$. One way to define them is as the dual to continuous functions, but that does not work. The way to describe them would be to do a similar construction but with one more parameter α. This would be closely related to the usual theory of solid modules, in about as clean a way as the usual theory of linearly complete modules.

We were really hoping that this α-norm structure on the reals would be impressive, but it isn't. However, it's related to some interesting things in classical functional analysis. One thing is that the category of complete modules is not stable under extensions, which was realized around 1918. But once you allow some locally convex vector spaces into the picture, the theory works again, though you're forced to use strictly convex norms.

I should also discuss one other thing quickly. If we specialize this construction to the p-adic numbers, you might expect that we're just trying to extend over the "missing part" at the real numbers, so the p-adic part wouldn't change much. But that's not true—you actually get new liquid structures on $\mathbf{Q}_p$, with an α parameter that can be anything between 0 and ∞. These α-liquid modules on $\mathbf{Q}_p$ can be described in a similar way to the real case.

You need them because the Slo Theory already works with you. So I want to ask a small question just to clarify possible confusion. You have the L_β for different β, which are again defined in the same way, just by some powers of the β, without taking one over β. This is a slight mismatch. What I know is that if I literally specialize the definition of A to ∞, I would be summing the supremum norms, which is not what I'm doing when I do this ∞ here. The supremum here is the actual...there's a little bit of a mismatch in notation between here and here, but I think it's okay.

But what is the when you take the union over $\beta < \alpha$? I think usually it was a filtered union. That is, is it the case, because here you have to be slightly careful that the way you index the constants, some should change when you increase α. So why the limit, the union over β, what are the inequalities between the L_β here, the Tate unit over all $\mathcal{C}$? And then, is it literally the case that these spaces get larger and larger as you increase β?

Okay, think of it like this: in some sense, you can think of a whole series of series that go from 0 all the way to ∞, where you put some norm here. At this end, you have solid modules, and then at each point α here, you have the α-liquid ones. In terms of the class of modules, being solid is a much stricter condition than being α-liquid. So the class of modules that you allow here becomes larger and larger as you make α closer to 0. And as you go to $\alpha = 0$, you have all condensed objects, because if you naively put some kind of L_0 norm there, meaning that there's only a bounded number of coefficients that are non-zero, and then you put some bound, this actually recovers a free condensed module without any competition by variant of what I said at the beginning about the integers.

Okay, I guess it's time for the new thing. Right, so back then we were trying to look, find natural candidates for how what the free compact modules could be, and then it was some hard matter of proving that they actually define the existence of being an antintic brain. But in this course, we have a different mindset. It is that to the $\mathcal{F}$ and the structure, we find something natural, the morphisms of this projective object P that you want to be morphisms. The free outno sequence should come after completion. And because in life being groups, this P has these very strong properties, like being compact and internally projective, this will always define a drink structure, so it has become extremely easy to produce analytical instrues.

Here's an example: it turns out that to define the adic curve, you only really need two things. The first is that P should be topological, it's new, and should be unit clear, and both of these have clear meaning in terms of the underlying cont ring. I mean, so being new means that you have a map from basically this guy and up with the natural ring structure, sending 1 to $\mathcal{C}$. But then you need some completeness for your modules, you need to be able to some certain sequences. And really the only thing you need is that $1 - Q \cdot$ shift acting on this projective P is an isomorphism. And it's clear that there is a universal example of such an antic ring. I mean, you just take the free generated by a unit, that's just a condensed ring, and then this condition puts some ring structure on this ring also, which is good to much. The key is that there is an initial such example. Existence is easy, the hard part is a description. Hey, so it's a pair of a triangle, and you can actually describe a triangle, and well, let me describe the underlying ring. This is precisely those sums such that the PO and thus, if you like, the claim is that we can do analytic geometry as usual over such rings. And so you can just repeat the construction to the curve, and then you will see for geometric reasons,

Already, the free complete guy—a triangle, for example, would be a free non-complete guy, but a triangle is a free complete guy. It is the union over all $a > 0$ of something. So, as usual, this is a limit of a sum of something.

I mean, of the part of the free module, let's say, where the coefficient of Q^n has ℓ^1-norm at most $n+n^k$. In the future lecture, I'll give a more precise description. But basically, you're asking for a similar description as before, but now you're asking for some polynomial growth conditions on the coefficients.

The condition here is that the coefficient of Q^n can grow at most like a polynomial in n of degree k, but I also have to allow the presence of negative coefficients. Anyway, then the limit as n goes to infinity has ℓ^1-norm at most $n + n^k$.

If you want the ring to be the thing where you have coefficients that grow at most polynomially, and you think about a way to encode that on the modules, then this is what you would be doing. Peter, do you really mean ℓ^0-norm or ℓ^1-norm? Because it's about a free module on $\mathbf{Z}$, so let's say ℓ^1-norm.

Within doing analytic geometry over this space, you will get a geometric way to construct the Tate curve really over the kind of smallest ring, which is actually the sum of something. This also means that if you specialize back to p-adic numbers, then the Ganga series is very close, but not quite at zero. It's a fun exercise to take this description of the free modules and base change it to $\mathbf{Q}_p$.

Let me start now.

13. Generalities on analytic rings (Clausen)

https://www.youtube.com/watch?v=38PzTzCiMow&list=PLx5f8IelFRgGmu6gmL-Kf_Rl_6Mm7juZO

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In the last lecture, Peter was—well, we spent a lot of time talking about solid analytic rings. In the last lecture, Peter started to introduce different kinds of classes of analytic rings which also work in the archimedean context, so these "liquid" and "gaseous" analytic rings.

What I want to do today is I want to talk about some generalities on analytic rings, partially in service of the story Peter is telling, and partially because it sort of should be done at some point. It's going to turn out that when discussing these new examples of analytic rings, it's kind of nice to have a good handle on the category of analytic rings and how to manipulate it. So I'll just be presenting some various facts about analytic rings and their category.

Let me start by recalling the definition. So an analytic ring is a pair $(R, \mathcal{M}_R)$, where R is a condensed, commutative ring, and $\mathcal{M}_R$ is a full subcategory of the category of condensed R-modules, closed under limits, colimits, and extensions. Additionally, any $\mathcal{X}$-group in $\mathcal{M}_R$ mapping to any condensed R-module still lies in $\mathcal{M}_R$. Finally, the unit object, the underlying ring itself, should lie in this full subcategory. Let's call this last condition "$\star$".

Now I'll make another definition. A "non-complete analytic ring", or "pre-analytic ring", is a pair as above, except we don't require the $\star$ condition. This concept arises when trying to produce an analytic ring structure on an interesting condensed ring, as the $\star$ condition can be quite complicated to verify. Often, there is a simpler ring that maps to the condensed ring, and it's easier to produce a pre-analytic ring structure on that simpler ring. Then, there is a completion procedure that allows one to go from a non-complete analytic ring to a complete, honest analytic ring.

We've seen examples of this when discussing rigid spaces. Recall the solid R^+ theory—when specifying the category attached to a rational open, we took the category of R-modules and imposed some conditions, which actually made it into a pre-analytic ring. But the $\star$ condition was not necessarily satisfied, so one would need to change the $\mathcal{M}_R$ to basically the structure sheaf to get an honest analytic ring.

In general, what you want to do with these non-complete analytic rings is to complete them to get something that is an analytic ring in the honest sense. That's the procedure I want to discuss right now.

A map of pre-analytic rings is just like a map of analytic rings—a map of underlying condensed rings such that the restriction of scalars of any module in the domain category lands in the module category of the codomain.

The completion function, the R-completion going to mod R, and then you have the S-completion going to mod S. So, this is a completion functor exhibiting this as a localization. And similarly here, this condition here is the same thing as saying that if you go here and then you complete, that actually factors through here necessarily uniquely because this is a localization. So, it's saying that if you have a map here which is kill, which goes to an isomorphism here, then it goes to an isomorphism here. So, then you get a symmetric monoidal base change functor from mod R to mod S.

So, that defines the category of pre-analytic rings. In other words, there's a fully faithful inclusion of the category of pre-analytic rings into the category of analytic rings, and this inclusion has a left adjoint. Moreover, the left adjoint sends the triangle mod R to—and some explanation will be required, but we take our triangle and we complete it with respect to this pre-analytic ring structure here. So, we apply the left adjoint to the inclusion from mod R into mod R triangle, and then we take, so to speak, the same mod R.

So, first, we need to make sure that this is of the appropriate type. I mean, I need to explain how mod R is actually a full subcategory of mod R triangle hat, in order for this to parse as a claim. And then I need to check that this setup satisfies the same axioms as this setup, so it actually defines a pre-analytic ring.

So, first of all, this mod R triangle hat is a commutative condensed ring. This is a sort of a completely general phenomenon. We have this mod R triangle, and this localization functor to mod R, this completion functor, this R-completion, and this is symmetric monoidal and cocontinuous, and it has a right adjoint, which is the inclusion in this case. But when you have a symmetric monoidal functor with a right adjoint, then the right adjoint is automatically lax symmetric monoidal. And in particular, if you take the unit object here and then you apply the right adjoint, it gets a commutative algebra structure. So, mod R triangle hat is a commutative algebra in this category, in this tensor category, and therefore a fortiori in condensed abelian groups, so it's a condensed commutative ring.

Moreover, in this general situation with the right adjoint to a symmetric monoidal functor, if you take any object here and apply that right adjoint, it gets the canonical structure of a module over this commutative algebra, again just purely formally. So, there's this natural factoring. We have mod R including into mod R triangle, and this actually factors through mod R triangle hat modules, and then the forgetful functor. And the next thing I should check is that this is actually fully faithful, so that we can indeed view mod R not just as a full subcategory of mod R triangle, but of mod R triangle hat.

Completion is idempotent. You find that this is indeed just a triangle M tan, okay?

So, we have to verify all of the axioms, and this is actually a little bit more subtle because we need to make sure that the calculations are going to work out correctly. Let me remark that this argument for full faithfulness will also work for X, provided that the derived completion on our triangle is the same as the completion on our triangle.

Recall that we had this notion of the derived category of an analytic ring, which was a full subcategory of the derived category of our triangle, and we had the derived completion functor, which was not necessarily even on something sitting in degree 0. Its π_0 or H^0 is the same as this, but it could have higher homology. For example, in this situation with these weird non-sheafy adic spaces, we don't have that. But in general, we don't have that. So what we can see is that everything works if we replace our triangle hat by our triangle hat, the derived thing, and use the notion of a derived analytic ring. Meaning that if you ask not to give an analytic ring structure on this ordinary ring, but an analytic ring structure on this derived enhancement, then using the same kind of formal argument, you'll be able to check all of these properties.

So, let's introduce a notion of a derived analytic ring. An analytic E_∞-ring is a pair $R_\triangle, \mathcal{M}_R$, where $R_\triangle$ is a connective condensed E_∞-ring, and $\mathcal{M}_R$ is a full subcategory of $R_\triangle$ that is closed under limits, colimits, and such that the internal $\mathcal{H}om$ from anything in $\mathcal{M}_R$ to anything in $R_\triangle$ is still in $\mathcal{M}_R$.

The proposition is that for any connective condensed ∞-ring $R_\triangle$, there exists a bijection between the set of pre-analytic ring structures on $R_\triangle$ and the pre-analytic ring structures on just the condensed commutative ring which is gotten by taking π_0. This projection on the level of pre-analytic ring structures restricts to a bijection on the level of analytic ring structures. And what is this gotten by? Here, you have this $\mathcal{M}_R$, and you send that to just those things in $\mathcal{M}_R$ which happen to lie in the heart.

As $D = 0 \oplus \pi_0 R\triangle$, so you have a module concentrated in degree zero over some derived ring, that's just the same thing as giving a usual module over the π_0. And in this direction, it's given by sending Mod_R to the set of those M in $D \geq 0R$ such that H_*M is in Mod_R for all $i \geq 0$.

So, is this definition that $D > 0$ might not be closed on the limits? Didn't I ask it to be a whole category? I can't repeat the question, but it goes to the greatest than minus one. I mean, yeah, I still have, but we gave the argument fixing that. So when you have this assumption, you can see that that holds. When you take an arbitrary direct sum of copies of the unit, then you'll see that products of a fixed, even higher derived products of a fixed, given fixed element will be still in the category. And then if you have an arbitrary product, then you can write it as a retract of a product with a fixed element by taking the direct sum of all the elements.

So, let me make a remark. In a special case where $R\triangle = \pi_0 R\triangle$, we see that the two definitions are consistent, meaning in the case where the notion of rings overlaps, namely where your derived ring is just concentrated in degree $\mathbf{Z}$, so it is a classical ring, the derived definition of what an analytic ring structure or a pre-analytic ring structure is matches up with the naive definition we gave on the level of ∞-categories.

The second remark is this finishes the proof of the proposition on completion, because what we can do is we can just... So there's no obstruction to proving the proposition on completion in the derived setting, and then we can move it down to the $\mathcal{A}n$ setting by going from the analytic ring structure on this derived thing that we produced to the analytic ring structure on its π_0, just by applying this procedure.

Monoidal category, then you can look at commutative algebra objects in it, and there's an ∞-categorical version of that, which implicitly involves things like ∞-operads or something. But really, an E_∞-algebra over $\mathbf{Z}$ will just be a commutative algebra object in the derived category of $\mathbf{Z}$ in that ∞-category, with the usual derived tensor product.

So if you believe in this ∞-category mumbo jumbo, you don't need to think about it in being built in terms of topological spaces and operads explicitly. You can kind of just plug into some simple categorical formalism.

Okay, so that's the notion of completion. Maybe the good thing to say is that you have an analytic ring over here, which gives you an analytic ring over here, such that the analytic ring structures here are the same thing as the analytic ring structures here, and such that each π_i, not just $\pi_{\mathbf{Z}}$, is complete in this category here.

I'm not going to prove the proposition, as this was proved in one of the older lectures. You can look there for the argument. Now I want to move on to the next topic, which is colimits in the category of analytic rings, although maybe I should make another remark.

It's important to note that the derived categories can change when you take an analytic ring structure on π_k. It gives you an abelian category, but we also argue it gives you a derived category. That derived category is not necessarily going to be the same as the derived category you get on $R^\triangle$ when you move along this equivalence, because the higher homology in $R^\triangle$ can make things a little different.

So when we're doing this completion on the level of naive analytic rings sitting in degree zero, it's important to note that the derived category is not the correct one. If $R^\triangle/R$ is the completion of $R^\triangle/R$, then it's not necessarily true that the derived category of this thing is the same as the derived category of that thing, unless you use derived completion on the left-hand side. If this ring that you calculate has higher homology, then you really should be considering a derived analytic ring instead of an ordinary analytic ring.

Okay, so now we have this notion of completion of analytic rings, and this lets us discuss colimits of analytic rings. The procedure is to take the colimit in pre-analytic rings and then complete. What is this colimit in pre-analytic rings? It's actually quite naive. If you have a diagram $R_i^\triangle/R_i$, then the colimit is just the tensor product of the $R_i^\triangle$ modulo the tensor product of the R_i.

Is the co-limit of condensed rings given by taking the co-limit of condensed commutative rings? No, sorry, the co-limit of R_i mod r_i is the pair of the co-limit over I of the R_i and that's the co-limit in condensed commutative rings. Then you take a certain full subcategory, so-called mod cimit I ini I R_i, where this is the those m in mod co-limit R_i such that when you restrict scalars to R_i, m lies in mod r_i. It's just an intersection of a bunch of categories where those categories are just required when you restrict scalars, you lie in the corresponding complete thing.

This is quite straightforward to see from the definition of the category of analytic rings. By definition, a map is a map of rings satisfying a certain property, so you certainly want the co-limit of the rings here, and this definition is just tailored so that you have the correct property. Checking that this satisfies the axioms of a pre-analytic ring is also not difficult. Closure under limits and colimits is elementary, and the closure under the axioms is also not so difficult.

Let me now take some examples. The claim is that for filtered co-limits in analytic rings, the completion is unnecessary. The filtered co-limit of R_i mod filtered co-limit of r_i is already complete. That means that for every I, you lie in mod r_i, but from I on in this filtered co-limit, you lie in mod R_i by definition, and so from I on, that's a co-final collection, so you can calculate this as a filtered co-limit of things which satisfy the condition, so it also satisfies the condition.

An example of this is that if R is a discrete commutative ring, then solid R is the filtered co-limit of solid r_i, i in I, if R is the filtered co-limit of the r_i.

If you understand filtered co-limits, then the next thing you should try to understand is pushouts. If you have an $R \to A \to B$ diagram of analytic rings, then you need to complete the pre-analytic ring which is a tensor over R of B, and then the full subcategory of those m in mod A tensor R B such that as an A-module, that lies in mod A and as a B-module, it lies in mod B. It is important to complete here, because this thing has no reason to lie in mod A or in mod B. The completion functor here a priori involves iterating an A-completion and a B-completion, alternating it one category or the other. The exact same remarks apply in the infinity category if you know the notion of derived analytic ring.

In general, you'd have to take this thing A-completed as an A-module, then B-complete that as a B-module, then A-complete that and B-complete that, pass to a sequential co-limit, and that would be the description of the completion functor. In practice, it's usually not so bad, but that's a priori what you need to do.

You enforce all of the relations described by your rational open. So if it was $\mathcal{F}_1$ over G, then you require that gx is invertible on your modules, and that $\mathcal{F}$ over G is a solid variable with respect to all of your modules. And then the pushouts—so a new analytic ring, I hesitate to call it $\mathcal{O}(U)^+$, but okay, I'll do it anyway. Maybe. And then if you take $\mathcal{O}(U)^+$ tensor over R with some other $\mathcal{O}(V)^+$, then what you're going to get is the same thing for the intersection of these rational opens: the analytic ring corresponding to the intersection of these rational opens. I guess in the discrete case, our discrete it literally is just this.

So the notion of pushout in analytic rings is corresponding to intersection of rational opens here, and that just follows from the definition of this. So yeah, these pushouts are in general kind of the most important construction, because they're geometrically what's supposed to correspond to pullbacks. And this business of having to complete them makes for an additional subtlety compared to the usual algebraic geometry.

Okay, so questions about that. Is the procedure easier when you just want a finite product? No, the case of relative tensor products is no easier. The case of absolute tensor products is no easier than the case of relative tensor products. So in fact, I could have—I didn't really need in this first part to say filtered, sifted is enough if you know what sifted means. And then general colimits, just as general colimits can be decomposed into filtered colimits and pushouts, general colimits can also be composed with sifted colimits and coproducts. So concretely, the completion functor for this relative tensor product is just the same as the completion functor for the absolute tensor product.

Okay, now I want to have a little bit of fun, well, depending on your definition of fun. I want to prove the following theorem. Suppose R is an analytic ring. Then the Frobenius map, which goes from R to R/P, induced by $x \mapsto x^p$, is certainly a map of condensed rings. But this is actually a map of analytic rings, from R to R/P. And the R/P-modules are just the R-modules that are complete when viewed as R-modules.

You should probably assume R has characteristic p, in which case it's really just a map from R to R. What do we need to do to prove such a claim? Well, according to the definition, what we need to do is to see that if M is in Mod_R, then the Frobenius pushforward of M, which lies in $\mathrm{Mod}_{R/P}$, actually lies in Mod_R. This is not so obvious how to check. We have these maxims of analytic rings, they're all about closure, categorical closure properties in linear algebra, like limits and colimits and so on, they say absolutely nothing about the Frobenius. They give no kind of hint as to why this should be true. However, let me make a remark: there is another perspective on maps of analytic rings.

To be a map, a map of analytic rings, you need to check a priori for all S-modules. But, by co-limits, it's enough to check it for a generating class. In fact, you know, so for these guys, in fact you can even take T to be the countable set if you like, so that this is the thing that you actually get, is that this is an R-module. So the map from the free R-module on T to it factors through RT, and this is functorial in S, functorial in T.

Moreover, well, there's a small extra condition that in particular is satisfied by them. So, if you do have a map from T to R-triangle, a map of condensed sets from T to R-triangle, then there's two things you can do. One, you can make a map from $R[T]$ to R-triangle, an R-triangle linear map, because by definition of analytic ring, this thing is complete. You go to R-triangle $[T]$ and then you go to $R[T]$. But then you also have this map here that we've assumed exists from $R[T]$ to $S[T]$, but on the other hand, you can compose this to S-triangle, and you get this thing here. So this is just a map of rings, this is the thing we posited to exist, and this exists for the same reason this exists, and that square will commute when you have a map of analytic rings.

So I want to claim—conversely, if $R \to R$-triangle to S-triangle is a map of condensed rings, such that there exist these maps from RT to ST satisfying the conditions above, then $R \to S$ is a map $R \to S$.

This is nice because you don't have to explicitly think about how you'd build $S[T]$ from $R[T]$-modules if you just have the maps that would kind of indicate it. Then you can actually do it. Let me maybe give a hint of the argument—it's in the notes from a previous iteration of this. A hint of the argument would be that Mod_R is monadic over condensed sets, so you have a forgetful functor from Mod_R to mod_R-triangle, which in turn forgets to condensed sets, and that satisfies the hypothesis of Barr-Beck. It's basically a localization, or the right adjoint of a localization, followed by a forgetful functor. And so this category can be understood as the category of whatever they call it, algebra over some monad here, which is kind of the free R-module monad. And then if you want to show that every S-module is an R-module, it would be enough to produce a map of monads from the free R-module monad to the free S-module monad. Well, the very first step in producing such a map of monads would be giving the map from the free object here to the map of the free object here, and then there's a condition you need to check, compatibility with the monad structure. And if you play around enough, you can reduce what you need to check to just this commutative diagram here.

Okay, so let's continue discussing Fenu's. So now what are we reduced to? We need, for every compact Hausdorff space T, a

Recognize that each of those cross terms is a norm from $M \otimes \mathbf{P}$, so it's a nice exercise if you've never done it.

Sorry, continue over here. I should say you have a group homomorphism—that's the special thing that happens here.

And what is the relation of this construction with the Frobenius on a ring? If $M = R$ is a commutative ring, then you have this thing which goes from R to $R \otimes_{\mathbf{P}} \mathbf{T^P}$ and π_0 of that. But then you can use the multiplication map from $R \otimes \mathbf{P}$ to R, to go to this where now the $\mathbf{T^P}$ action is trivial. Here you're using that the ring is commutative, so that the multiplication is a $\mathbf{T^P}$-equivariant map from $R \otimes \mathbf{P}$ to R. But now since the $\mathbf{T^P}$ action is trivial, this is just the same thing as the norm map, which is just summing over the action of the group, but the group is trivial, so you're just summing over $\mathbf{P}$ copies of 1. So this really is just $\mathbf{R_P}$. And if you trace through this, this is the Frobenius.

Okay, and now if M is an R-module, then this map, the so-called Frobenius map from $\mathbf{T^P}$, is a map of abelian groups. In fact, it is a map of R-modules if you Frobenius twist on the right-hand side. In other words, this abelian group level Frobenius is kind of linear over the ring level Frobenius, in the appropriate sense.

What this suggests is that we should try to apply this abelian group version of Frobenius and see what happens. So now we take T a profinite set, then we get $R[[T]]$, and we can always do this construction. Now we're viewing $R[[T]]$ as a sheaf of abelian groups on profinite sets, and we can apply this construction here. We tensor it $\mathbf{P}$ times, and we get $\mathbf{T^P}$, and then we take π_0. So that's all just happening at the level of sheaves of abelian groups.

We can always map this to any further completion we like, and in particular we can map it to $\pi_0 R[[T]]$, which is the free R-module on the profinite set T. We have this sort of Künneth formula, so the tensor products of the free modules are just the free modules on the product, and then $\mathbf{T^P}$ is acting here as well.

We wanted to make a map from $R[[T]]$ to itself which is Frobenius-linear, and we've gotten to $R[[T]]$ to the $\mathbf{P}$. How do we compare them? Well, we can put $R[[T]]$ into this via the diagonal embedding, and that's also equivariant for the $\mathbf{T^P}$ action if you make $\mathbf{T^P}$ act trivially here.

The claim is that this map is an isomorphism. If you buy that, then you're basically done. $\mathbf{T^P}$ is acting trivially here, so the Tate construction is just modding out by $\mathbf{P}$, so this is indeed $\mathbf{R_P}[[T]]$. And for similar reasons, the composite is actually going to be Frobenius-linear and give you the desired construction. You also have to check that condition, but the full argument is in the notes

Well, we're going to more directly use them now. So the proof is: the first claim is that R[SCP] is actually mapping injectively into R[S], and the reason is that any inclusion of light profinite sets has a retraction. This was a fact that Peter proved in the second lecture of this course. So if you apply it to the inclusion of CP and S, you find there's a retraction. So then if you hit it with any functor, it'll still be injective.

So what does this imply? This implies by the long exact sequence in take-chology that it suffices to show that if you take R[S] mod R[SCP], this thing has vanishing take-chology.

The second claim is that generally, if you have T inside S, then $R[S] mod R[T]$ only depends on the locally compact space, the locally light profinite space which is the complement $S \setminus T$. It's not like a group, where you can choose.

Okay, so that's not a precise claim. What's the more precise version of the claim? If you have S' mapping to S, and then you have T included here, and you form the pullback to get T', and if this is an isomorphism over S minus T, so you're sort of blowing up T, so to speak, or you're choosing a different compactification of S minus T, then R[S'] mod R[T'] maps isomorphically to R[S] mod R[T], and this holds because this square here is a pushout in condensed sets, which is a little exercise you can see using the definition of the Grothendieck topology.

And then the third thing is that if X is a sigma-compact, totally disconnected, locally compact Hausdorff space, and CP acts with no fixed points, then X is actually isomorphic to some coproduct of CP many copies of Y, where CP is acting by permuting those copies. Sigma-compact means countable union of compacts.

Okay, so if you combine two and three, what do you deduce? You deduce that in that setting, R[S] mod R[SCP] is actually a direct sum of CP many copies of some guy X, where CP is acting on this set.

Copies of some guy and then you compact a in a different way by just compactifying Y and then taking the $\mathbf{CP}^n$ many disjoint union copies of that to get a compactification of X and then use that to calculate the quotient here. Then you'll find that the module is induced, and an induced module has vanishing Tate cohomology.

Okay, so this fact that $\mathcal{F}$ is a map of analytic rings, it's not just a cute fact. I'll make a remark, but I don't think I'll go into the details. This theorem on $\mathcal{F}$ implies that if our triangle mod R or let's do it in this setting of a derived analytic ring or a pre-analytic ring, and we give our triangle the structure of an animated commutative ring or condensed ring, which gives new functors like derived symmetric powers on $D^{\geq 0}_{R}$, which are the things used to build free animated rings or the monad describing animated rings over our triangle, then these symmetric powers descend. If a map $M \to N$ goes to an isomorphism in $D^{\geq R}_{R}$, then its symmetric powers also go to isomorphisms there. This allows you to do normalization or completion of pre-analytic rings in the animated context, giving a good category of animated analytic rings.

Okay, so one last topic. I'll call it "killing algebra objects". Recall for motivation that we presented the solid $\mathbf{Z}$-theory as the analytic ring structure on the integers where M is in solid $\mathbf{Z}$ if and only if the internal Hom from P to M, shifted by -1, is an isomorphism. This is equivalent to saying that $\mathrm{Hom}(A, M) = \mathbf{Z}$, where $A = P/(T-1)$. This has an algebra structure in the ambient category $D^{\mathrm{cond}}_{\mathbf{Z}}$.

In general, if C is a symmetric monoidal category, we can consider "killing algebra objects" A in C, and the category of modules over A in C. The condition that a map $M \to N$ goes to an isomorphism in the localization $D^{\geq R}_{R}$ allows you to extend symmetric power functors from this thing to that localization.

Let's say we have a monoidal, presentable, stable infinity category, and let's include that the tensor product commutes with co-limits in each variable. Let's say A and C is an algebra object, and it really, I only need it in some kind of weak sense. So let's say that we have a multiplication map, literally just A tensor A goes to A. We have a unit, so there's the unit object in the symmetric monoidal category, and then we require that the multiplication is either left or right unital.

So let's say one is the unit for A, and then the multiplication is an isomorphism. You said you don't require any associativity or nothing, yeah, it's really, really weak. Okay, then we can define D subset C to be the full subcategory of those M in C such that the internal Hom, which implicitly is an RHom here, from A to M equals zero. So we're killing A, we're declaring that A should be equal to zero, but also in an internal Hom sense. And then the goal will be, in favorable situations, to give a formula for the left adjoint to the inclusion, to the inclusion. Sorry, is K the name of the mathematician or just "killing"? Killing, killing, killing means not the mathematician responsible for the killing. I'm that this kind of seem, probably he didn't do it, yeah.

Okay, maybe I should give some examples. Well, there was the solid example that I just described. So there was also solid ZT, which was also obtained by killing some endomorphism of, or requiring some endomorphism of P to go to an isomorphism, but that endomorphism of P was also just given by multiplication by some element with respect to the ring structure on P, so it's the same thing as killing the cofiber, just like this. There's also kind of pure algebra examples. So for example, if you take the usual derived category D of R, and then you look at R mod f, so that's called D of R, then what is D? Well, D is D of R1 over F, so it's kind of inverting f in some algebraic sense, is an example of this. Another thing you could do is call D of R, then you could take this algebra instead, R bracket f inverse in D of R, then what is D? D is the sort of like f-complete derived subcategory, and we're looking for a formula for the derived F completion.

Okay, so let's define a functor F from C to C by the following. F of X is the internal Hom from C to X, and there's a natural transformation from X to F of X because this is the same thing as internal Hom from 1 to X, and by construction, C maps to 1, so you get a map in the other way on the internal Hom. I want to claim that this is a first step towards constructing the required localization. Claim: if M is in D, then applying P to M to this map is an isomorphism. So Homing to M living in D doesn't see the difference between X and F of X.

Okay, so for the proof, it suffices to see that the fiber of X mapping to F of X is an A-module. The reason for that is to show that Homing out of this map to M being an isomorphism is the same thing as saying that the Homs from the fiber to M should be zero. But by definition, internal Homs from anything in A to

M is zero. Therefore, if it's an A-module, then it's actually a unital—maybe I should say it's a retract of A tensor

And one should actually potentially be a bit careful about which map one writes down here. What I want to do is I want to take F applied to the previous map, as opposed to taking the instance of the previous map with X replaced by $F(x)$.

Okay, and then you can continue on like this: $F(f(f(x)))$. And then I claim that, so let's define $F_\infty(x)$ to be the colimit of this sequence. Then I claim that, if either (1) this colimit stabilizes for all X, so if for example every map from here onward is an isomorphism, or (2) the internal Hom functor from C to C commutes with colimits, or really maybe only needs sequential colimits, then $X \mapsto F_\infty(X)$ is the left adjoint to the inclusion of D inside C.

For the proof, you have to check two things. One, you have to check that if you map from X to anything in D, that's the same thing as mapping from $F_\infty(X)$ to anything in D. But we just proved that claim for X going to $f(X)$, and this thing is given by hitting an instance of that map with the functor F. But the functor F is going to preserve the property used in the proof here that the fiber is an A-module, because internal Hom to an A-module will still be an A-module. So the exact same argument shows that each of these maps also satisfies the same property that internal Hom out of them doesn't see the difference between one guy and the next. And then you have a colimit, and internal Hom-ing out of that is just an inverse limit of internal Hom-ing out of all the other ones. So that formally just passes through to the colimit.

Okay, so in principle, for example, you can just you could at least attempt to use this formula to compute solidification. It's all actually quite explicit. In that case, this Hom-ing out of C is just...

It's also fun to try to unwind these cases and see that you recover the classical formulas. For example, the well-known formula for M_1/F can be rewritten as a sequential colimit, which is exactly the same as this sequential colimit formula.

Here, it looks different because it's an inverse limit, but what's actually happening is that this inverse limit is just the first iteration of $f(x)$ applied to M, and all the maps after that are isomorphisms. So it's a way to understand the left adjoints using this formula.

I should also mention that this situation occurs when a is idempotent. But this is not the case here, so taking the completion would not give you the same result. You'd be doing something else, not just algebraically inverting F.

I went a little over time, so I apologize for that. Thank you, and see you next week.

14. Gaseous modules (Scholze)

https://www.youtube.com/watch?v=krq6jCy-dhE&list=PLx5f8IelFRgGmu6gmL-Kf_Rl_6Mm7juZO

In Lecture 12, we discussed the Tate elliptic curve, and introduced a new kind of analytic ring structure, the "gaseous" analytic ring structure. Today, we will pick up there and analyze this gaseous analytic ring structure. To do this, we will need to use some of the results from Lecture 13.

The plan is roughly:

- say something about the "gaseous base ring" $\subset \mathbf{Z}(\!(q)\!)$, which is a certain version of arithmetic power series
- talk about the corresponding theory over the real numbers the "gaseous real vector spaces"
- the motivation for this was coming from the Tate elliptic curve curves. We claimed that the gaseous analytic ring structure would be good for that, and I want to start a little bit of explanation for that. Eventually this will require us to do some more serious analytic geometry, and that will be a point where we then start to develop a more general formalism to really carry that out.

14.1. The gaseous base ring.

Recall: the motivation was to find a "minimal" analytic ring $(A^{\triangleright}, \mathrm{Mod}_A)$ over which the Tate elliptic curve E_q can be defined. The Tate elliptic curve is an example of an elliptic curve, and the universal one would be the moduli space of elliptic curves. But this a priori won't do—we really want something that is of the form $\mathbf{G}_m/q^{\mathbf{Z}}$, in some analyic sense.

Desiderata 14.1. For the sought-after analytic ring A:

(1) there should be a topologically nilpotent unit $q \in A^{\triangleright}$
(2) let $P \otimes A$ be the free A-module on a nullsequence, then $1 - q \cdot \mathrm{shift} \curvearrowright P \otimes A$ should be an isomorphism.

On (2): this is similar to how we defined the solid analytic ring structure, where we had $q = 1$ instead. And of course, then we didn't ask for it to be topologically nilpotent. This is a weaker condition because we are saying that for any sequence $(a_n)_n$ in A, we can form $\sum_{n\geq 0} a_n q^n$. Note that

$$\frac{1}{1 - q \cdot \mathrm{shift}} = 1 + q \cdot \mathrm{shift} + q^2 \cdot \mathrm{shift}^2 + \cdots$$

which applied to $(a_0, a_1, \dots)$ gives

$$\left(\sum_{n\geq 0} a_n q^n, \sum_{n \geq 0} a_{n+1} q^n, \dots \right)$$

so we get the desired sum in the 0th component. In particular, we can form some kind of geometric series in q with the coefficients from an nullsequence.

We certainly want some condition here, because if we didn't ask any kind of completeness condition, then we would just work with all condensed A-modules, and it wouldn't be completed in any sense. But we do want to be able to form some kind of infinite series.

Proposition 14.2. *There is an initial analytic ring with these properties. In fact, there is an initial animated such, which turns out to be static.*

Before the proof, let's introduce some notation.

Definition 14.3. Denote the free condensed ring on a topologically nilpotent unit by

$$\begin{aligned} \mathbf{Z}[\hat{q}] &:= \frac{\mathbf{Z}[q^{\{\mathbf{N} \cup \infty\}}]}{q^{\infty}} \\ &= P \text{ as cond. ab. gps} \end{aligned}$$

As a condensed abelian group, this is the same as the module P defined earlier. The monoid structure of $\mathbf{N} \cup \{\infty\}$ induces a ring structure on P. But it will be useful to keep P as a module separate from $\mathbf{Z}[\hat{q}]$ as a ring.

Proof. By definition, (1) is equivalent to asking for a map

$$\underbrace{\mathbf{Z}[\hat{q}][q^{\pm}]}_{:=A_0^{\triangleright}} \to A^{\triangleright}.$$

Then we can form

$$P \otimes_{\mathbf{Z}} A_0^{\triangleright}$$

which is acted on by $1 - q \cdot \text{shift}$. We define our subcategory

$$\text{Mod}_A \subset \text{Mod}_{A_0^{\triangleright}}$$

by

$$\text{Mod}_A = \left\{ M \in \text{Mod}_{A_0^{\triangleright}} \,\middle|\, \begin{array}{r} 1 - q \cdot \text{shift} \curvearrowright \underline{\text{Hom}}(P \otimes_{\mathbf{Z}} A_0^{\triangleright}, M) \\ \text{is an isomorphism} \end{array} \right\}$$

This is extremely analogous to how we defined solid modules. In that case $A_0^{\triangleright}$ was just $\mathbf{Z}$, and we asked for the same condition with $q = 1$. And because this $\underline{\text{Hom}}(P, -)$ has such excellent properties, it's immediate to check (and this is what we did for solid modules) that this subcategory has all the stability properties that we ask for for an analytic ring.

Then the pair $(A_0^{\triangleright}, \text{Mod}_A)$ is a pre-analytic ring (c.f. Definition cite me of Lecture 13). It satisfies all the properties of an analytic ring, except for the property that the ring itself is complete: $A_0^{\triangleright} \notin \text{Mod}_A$.

But conditions (1) and (2) are equivalent to asking for a map from $(A_0^{\triangleright}, \text{Mod}_A)$ to an analytic ring, and in the previous lecture we saw that there is a left adjoint from pre-analytic rings to analytic rings, so we can complete this pre-analytic ring. So the completion of $(A_0^{\triangleright}, \text{Mod}_A)$ does the job. ☐

As explained last time, when you have a general analytic ring, it's best to complete it in the sense of an animated analytic ring, because a priori the derived completion might not sit in degree zero, and in that case the better object to consider is the derived completion. In this case, it will turn out to be the case that this derived completion actually sits in degree zero, so there will not be a difference. So, that's one reason that we somehow switch in this discussion about completion of analytic structures.

The reason for the final stretch of the previous lecture was that we now want to compute this analytic ring structure. For this, we saw in Lecture 13 some general recipes for computing this completion functor in some cases.

Theorem 14.4. *The following describes the initial analytic animated ring $A = (A^{\triangleright}, \text{Mod}_A)$ as above; it turns out to live in degree 0:*

(1) $A^{\triangleright}$ and all $A[T]$, $T \in \text{Pro}_{\mathbf{N}}(\text{Fin})$ sit in degree 0, and are quasiseparated.

(2) the underlying ring $A^{\triangleright}(*)$ is a subring of the Laurent series ring $\mathbf{Z}(\!(q)\!)$, given by

$$A^{\triangleright}(*) = \left\{ \sum_{n \gg -\infty} a_n q^n \in \mathbf{Z}(\!(q)\!) \,\middle|\, |a_n| \text{ has at most polynomial growth} \right\}$$

As for the condensed structure, we have

$$A^{\triangleright} = \bigcup_{\substack{k>0 \\ n>0}} \prod_{m \geq -n} \left(\mathbf{Z} \cap \left[-(m+n)^k, (m+n)^k\right]\right) q^m$$

Basically, k is giving the polynomial growth, and n is giving the order of the pole of the Laurent series. Each term in the product is a finite set, so the product is a light profinite set. Taking the union, this describes $A^{\triangleright}$ as a condensed set (and hence as a condensed ring).

(3) for a light profinite set $S = \varprojlim_i S_i \in \text{Pro}_{\mathbf{N}}(\text{Fin})$, we have

$$\begin{aligned} A[S] &\subset \mathbf{Z}(\!(q)\!)[S]^{\blacksquare} \\ &= \left(\varprojlim_i \mathbf{Z}[\![q]\!][S_i] \right) [q^{\pm}] \\ &= \mathcal{M}(S, \mathbf{Z}(\!(q)\!)) \end{aligned}$$

for the free A-module on S. So in particular, we only need to describe $A[S]$ as a condensed set, since it inherits the module structure from the target. It is given by

$$A[S](*) = \left\{ \mu = \sum_{m\in\mathbf{Z}} \mu_m q^m \in \mathbf{Z}(\!(q)\!)[S]^{\blacksquare} \;\middle|\; \begin{array}{l} \mu_n \in \mathbf{Z}[S] \subset \mathbf{Z}[S]^{\blacksquare}, \\ |\mu_n| \textit{ have at most} \\ \textit{polynomial growth} \end{array} \right\}$$

Once again, this is describing the underlying module of $A[S]$. For the condensed structure, we have

$$A[S] = \bigcup_{\substack{k>0\\ n>0}} \varprojlim_i \left(\prod_{m\geq -n} \mathbf{Z}[S_i]_{\leq (n+m)^k} q^m \right)$$

We will not prove this completely, but want to explain where the polynomial growth condition comes from. This is the most surprising aspect, since we didn't build this into the definition anywhere.

Proof sketch. We will use the formula from the end of Lecture 13 cite me, which told us how to compute completions given by asking that $\underline{\mathrm{Hom}}(A, -)$ from some algebra object A is 0. Note that asking for

$$1 - q \cdot \mathrm{shift} \curvearrowright \underline{\mathrm{Hom}}(P \otimes_{\mathbf{Z}} A_0^{\triangleright}, M)$$

to be an isomorphism is equivalent to the following. Let

$$B := \frac{P \otimes_{\mathbf{Z}} A_0^{\triangleright}}{1 - q \cdot \mathrm{shift}}$$

Now we will go back on what we said above, and remember that P has a ring structure. So we can write

$$= \frac{\mathbf{Z}[\hat{x}, \hat{q}][q^{\pm}]}{1 - qx}$$

Then the isomorphism condition is equivalent to $\underline{\mathrm{RHom}}_{A_0^{\triangleright}}(B, M) = 0$.

This B also has the condition that $\underline{\mathrm{RHom}}_{A_0^{\triangleright}}(B, -)$ commutes with all colimits, which follows from the same property for P. Thus, the derived completion in pre-analytic rings $(A_0^{\triangleright}, \mathrm{Mod}_A)$ is given by

$$D(A_0^{\triangleright}) \ni M \mapsto \varinjlim \left(M \to \underline{\mathrm{RHom}}(I, M) \to \underline{\mathrm{RHom}}(I^{\otimes 2}, M) \to \cdots \right)$$

where I is defined by the fiber sequence $I \to 1 \to B$.

Note that when you're algebraically inverting an element f, correspondong to a fiber sequence $fR \to R \to R/f$, this is the usual colimit formula for $R[f^{-1}]$. However, using this formula for solid completion turns out to be extremely confusing.

It will turn out that only the colimit is well-behaved: it will be concentrated in degree 0 and quasiseparated. However, the individual terms $\underline{\mathrm{RHom}}(I^{\otimes \bullet}, M)$ will have higher homology which is probably not quasiseparated.

How do we understand this? Well, I is built from 1 and B; the identity 1 is easy to understand, and B is just built from P. So to understand what's happening here, the key is:

Key. Compute $\underline{\mathrm{Hom}}_{\mathbf{Z}}(P, A_0^{\triangleright})$, and more generally $\underline{\mathrm{Hom}}_{\mathbf{Z}}(P, A_0^{\triangleright}[S])$.

Note also that we can proceed without inverting q in all of this; will do so. This means we need to understand

$$\underline{\mathrm{Hom}}_{\mathbf{Z}}(P, P) = \underline{\mathrm{Hom}}_{\mathbf{Z}}(\mathbf{Z}[\hat{x}], \mathbf{Z}[\hat{q}])$$

Lemma 14.5.

$$\underline{\mathrm{Hom}}_{\mathbf{Z}}(P, P) = \bigcup_{n>0} \varprojlim_m \left((\mathbf{Z}[q]/q^m)_{\leq n}[x^*] \right)$$

where

$$(\mathbf{Z}[q]/q^m)_{\leq n} := \left\{ \sum_i a_i q^i \in \mathbf{Z}[q]/q^m \;\middle|\; \sum_i |a_i| \leq n \right\}$$

More generally,

$$\underline{\mathrm{Hom}}_{\mathbf{Z}}(\mathbf{Z}[\hat{x}], \mathbf{Z}[\hat{q}][S]) = \bigcup_{n>0} \varprojlim_{m,i} \left(\mathbf{Z}[q]/q^m)[S_i]_{\leq n}[x^*] \right)$$

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Is our goal given by asking that the integral cohomology derive from some algebra? Note that this condition, that the integral cohomology is equal to the point, let me call it c. So this is the triangle modulo $\mathbf{R}$. But now I actually want to think about $\mathbf{P}$ as a ring. I didn't want to do that, and I want to keep my notation separate. But at this point, I do want to remember that $\mathbf{P}$ admits a ring structure.

It's actually a free $\mathbf{P}$-module on the topological generator X, and $\mathbb{A}$ was also a free $\mathbf{P}$-module. And then okay, so I had T over $\mathbf{Q}$, and then one minus the shift operator, which is multiplication by X, so this is actually a commut[ative]. So the condition is that the $\mathbf{R}$-linear map from B (because this $\mathbf{R}$-Hom is literally just the cone of the Homs, and the Homs are in degree zero because of the excellent properties of $\mathbf{P}$), and also B has the property that the internal Hom from B commutes with all limits, the same property for $\mathbf{P}$.

So this was one of the conditions under which Dustin explained the formula for the $\mathbf{R}$-completion. The $\mathbf{R}$-triangle $\mathbb{A}$ is given by taking a module M which is in the derived category of $\mathbb{A}$-triangle modules, and taking the colimit of the following diagram. This formula is quite useful in situations where you're just algebraically inverting some element F. But trying to use this formula to compute solid confusion turns out to be extremely confusing.

Anyway, so how do we actually go about computing this? The key is to understand the internal Hom from $\mathbf{P}$ to B. If we don't invert $\mathbf{Q}$, then this is really just the free $\mathbf{P}$-module, which is just $\mathbf{P}$. So we need to understand the internal Hom from T to $\mathbf{P}$, where we think of T as $\mathbf{Z}[X]$.

So these three objects, which are some kind of completed pro-groups on some pro-sets, are always given as a union over all n of things which have bounded norm at most n. And this will commute with some internal Hom, so this will also be a union over $n > 0$ where now I'm taking the part of this algebra where some of the norm at most n terms are allowed to appear.

But then when I do that, the new object, where I'm allowed to do an overall n, which is like the power of $\mathbf{Q}$ dividing by n, and then that's a direct sum of $\mathbf{Q}_p^n$, but then inside this you take sums of at most n basis elements, you take sums $\sum_i a_i \mathbf{Q}^i$ where the sum of the absolute values of the a_i is at most n, and then you take polynomials in a variable X.

There's a similar formula for the internal Hom from $\mathcal{P}$, which again is a pro-set, except with the adjoint you have some sort of profile sets, and it's just the same sort of structure, we take a union of all $n > 0$ and take an inverse limit over all n's, and then you take $\mathbf{Q}_p^n \oplus$, but inside there again this part that some of at most n sums are difference of at most n basis elements, and then I take a polynomial in the dual variable.

Anything on the limits over the exponents of the X, or the Y? I didn't want to call it X because it's the dual of X, but of course maybe I should have used a different notation to make that clear. The star didn't have any specific meaning here.

In particular, one consequence of this internal Hom from $\mathcal{P}$ is that it's actually just the subspace of $\mathbf{Z}[[Y]]$ with a certain kind of funny condition, where the coefficients of the power series in Y are bounded. Similarly, the internal Hom from $\mathcal{T}$ to this is also a subring of $\mathbf{Z}[[Y]]$ where each coefficient of Y satisfies a certain boundedness property.

Transition turns out to be just Q minus one, and so this sits in degree zero, and this sits in homological degree one. And if you do the similar computation with more variables, y_1 to y_k, then you get a similar picture but just the k of the variables y. This is computed by a complex in the y variable. So instead, what you take is you take $\mathbf{Z}$ and then you join y_1 up to y_k, and then again you take such power series in Q. And then, I mean, some of this was the derived notion just by one variable, the y, and now you take a derived notion by $Q - y_1$ up to $Q - y_k$. This corresponds to the Tate filtration. And I mean, you can do the same with with a with a sets, right? So if you have a profile sets somewhere, then you just plug the profile sets here in the middle and then get the same.

So now we have a somewhat concrete formula for these things, right? So if you want to compute the underlying ring, we literally just have to understand the colimit of all k of these things. So the ring we're interested in is just the colimit over all of this. Take y_1 to y_k, and if you found it, then you take the derived and here it turns out to be a case where it's really very important that each individual level to take the derived $\mathcal{D}$fied colimit, because there will be high homology.

Then here, what are the transition maps? A transition, sorry, yeah, I should have said, so this is similar question to what the transition matrix that Deroy wrote down last time. I mean, each of these is a subring of the next one, where you just don't have the new variable. It's not a, sorry, all these things are actually not rings because if you multiply two things, there's some confusion, but let me not try to say whether they are or not, but certainly there are submodules.

So far, there was no apparent polynomial growth condition at all, there was just some kind of boundedness that appeared for more transparent reasons. But now, let's analyze the H_0 of this, or rather, the separate H_0. I mean, this for simplicity, you can do the full analysis, but it takes a lot of concentration, so I think there's already enough interesting stuff going on without these additional technicalities.

So, what is the H_0? You really just take this ring and then quotient out by $Q - y_1, \dots, Q - y_k$, or if you take a separate quotient, really by the closure of all these things. In other words, here, I'm formally allowed to replace any occurrence of any of these variables by 1, it's really just set to Q.

So this, there's a question: what is the image of this thing mapping towards the power series where all the y_i are? Let me again recall what this was. This was the union over all n of the limit over M of the part of this thing which was of norm at most n. So this wasn't literally written as a finite free module, but I mean, you take a basis by giving $\mathbf{Z}[1/Q]$ to $\mathbf{Z}[1/Q]$ and then you append the variables by one. But in particular, it will actually be sufficient to analyze the part where all of these are say just equal to 1, $\mathbf{Z}$ or 1. So here, we're just allowed to take some kind of bounded sum, but then we have all these y_1 through y_k here at the disposal, but for each of them individually, I can take, I can take a, I mean, just each coefficient individually is bounded. So for example, if I like, for each n, we have to analyze what the image is, but then which multiples of Q which like integer times Q can I achieve? Well, this integer times Q, like, for each occurrence of Q, I can decide whether I want to keep it a Q or whether I make it a $y_1, ..., y_k$, but for each $y_1, ..., y_k$, I can use it at

The coefficient of Q to the m can lie in $\mathbf{Z}_n^* \times \mathbf{Z}_{n+k}$. Okay, I would have to be slightly more careful here about the precise notation, but I mean basically something like that. And so the question is, how does this grow? How does this grow in m and in M? This is precisely a polynomial of degree k, right? And then this is just the polynomial function of...

Yeah, so whenever I have something that grows where the coefficients grow in m just like a polynomial, then because I have all these monomials here at my disposal, I can somehow split up all the individual summands to make them bounded. There are a lot of choices about how you go about doing this. And when you want to prove that this complex actually ends up being in degree zero, you somehow have to show that a different way of arranging the terms here differs from the other one by something bounded, some bounded thing in the next term of the complex. And this will actually not be true for a given k, but when you allow your k to increase a little bit, then you can do it.

There's a question in the chat about whether you can interchange the order of $\mathbf{Z}, Y, S$, and indeed you can interchange the order. The kind of argument you have to do here to see that this is well-behaved is a little reminiscent of the arguments you have to prove that liquid cohomology is well-behaved, but it's several orders of magnitude easier. It's again a situation where you have some kind of system of complexes where each term has some kind of norm, and then you have to see that if you have something with differential zero, you can bound the preimage. But here, you can really just do it.

14.2. **Gaseous real vector spaces.**

Definition 14.6. A (light) condensed $\mathbf{R}$-vector space V is *gaseous* if

$$\underline{\mathrm{Hom}}_{\mathbf{Z}}(P, V) \curvearrowleft 1 - \tfrac{1}{2} \cdot \mathrm{shift}$$

is an isomorphism.

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All right, so this is all I wanted to say about the proof of this box formation. Now, I want to talk briefly about Gassar-Greenberg real vector spaces. Let me condense or light... We started emitting the light at a couple of times, so throughout this course we're always working with this light filling, and it might not always be mentioned.

If I take the internal Hom from, say, P which was always the free abelian group on the integers here, and then I choose one topological unit in the real numbers, you might actually wonder, doesn't it matter that I chose 1/2 here? And it doesn't. So in this setup of liquid analytic — of this liquid drink structures on the reals, there was this extra parameter that you have to choose, how non-convex you allow your vector spaces to be. Here, it seems you also have to make a choice, but actually it doesn't matter. The condition is independent of what number, like 1/2, you choose. You could also take a negative number, any topological unit.

Actually, why is that? It's easy to see, and it's intuitively believable, that if you make this number smaller, then the conditioning becomes weaker, because here some of the series you're trying to sum, they decay even faster. But on the other hand, if you're trying to sum a geometric series where the coefficients are like 1 over 2 to the n, then you can always — the finite sum where you sum the first few terms and then have... Well, not really telescope, but there's another question in the chat that I actually missed, so I can't read it.

By the way, general question: non-condensed sets will not be used anymore in the setup originally, right? There was a question about how originally, extremely disconnected sets played quite a crucial role in

Over the place, that's only from this, maybe related questions like: Can you also define the sum of liquid real vector spaces that worked not just in the p-adic setting, but in the full condensed setting? You can ask the same question about the Gaussian orthogonal structure—whether you can also define that on all condensed modules. I think you can, but the argument we gave here does not work because we used the internal projectivity of $\mathbf{P}^1$, so you would have to argue slightly more carefully to see that there is a group structure on real vector spaces in the full condensed setting.

Generally, this is only a telescopic argument. If you wanted something more, you can always rewrite like a finite sum and then a new sum where you still have a sequence of coefficients and powers of q to the n. Here, anything that's p-liquid for any p or α-liquid for any α is always a condensed vector space. This is an extremely general class of condensed vector spaces, but it's still a workable class for many purposes.

So, you can now wonder what are the free real vector spaces corresponding to this. Here's a proposition: Our guess, which is the analytic $\mathbf{R}$ corresponding to the Gaussian orthogonal group, is real vector spaces. Again, it's clear that this defines a group structure because of the good properties of $\mathbf{P}^1$. This is just obtained by taking the one we had, completing it, and then modding out by the group of roots of unity.

Setting $q = \frac{1}{2}$, you take this completed ring and then mod out by the group of roots of unity, and you actually get the real numbers. These are like functions on the real part of this thing, and if you set $p = \frac{1}{2}$, you just get the real numbers.

In particular, this tells you what the free Gaussian vector spaces are: you can compute them by taking the free Gaussian vector spaces here and then taking the p-adic points. But now the description becomes a bit confusing because when we wrote down the Gaussian conditions that we allow here, it was some kind of polynomial growth condition in q, but now q has become a constant, so it's not immediately clear how you phrase these Gaussian conditions directly on the real numbers. But you can do it, and here's the description.

The free Gaussian real vector spaces have the following crazy description: we take a union over some cases corresponding to the polynomial growth conditions we had previously, where q is now a constant. Then, I take a limit over i and then a certain bounded subset in the free real vector space on the finite set S_i, which satisfy a condition on how large the i's are allowed to be. The condition is that if you sum the size and measure it in terms of certain functions F_k that I will introduce, they are at most some constant C.

Formally, this is very similar to what the free liquid vector spaces look like, and there the norms you were taking were like raising to some power. Generally, the functions F_k that you would like to put there should be increasing, because larger values of x should be penalized more. On the other hand, you want the transition maps to be well-defined, and for this you need the functions to be concave. It turns out that these F_k should measure how large a real number is in terms of some real number greater than or equal to zero, and they should be increasing and concave.

F_K is any increase in compressive functions such that for small X, it's given by the following: it's the absolute value of $\log X$ to the power K. I'm saying it's this way because in principle, I'd just like to take this function and in the neighborhood of zero, it's a nice increasing concave function. But at some point, it becomes convex, goes to infinity, and then does some nonsense, because if you say $x = 1$, $\log 1$ is zero, and 1^{-K} is some nonsense.

So, I can write down this function for all X, but I can do it for small X, and then at some point, I just arbitrarily extend it, and it doesn't do me any good.

Okay, so this is maybe a little bit hard to just visualize, so let me give a sort of diagram. In the liquid story, there's this function $F(x) = x^{\beta}$, where β is some number between 0 and 1. So, there's some function like that, and it's always increasing and concave, so I can just always use it.

And then the locus where the sum of the x^β in the two-variable case will be some kind of locus, like a sling or a convex region. In the general case, it's some function that's extremely steeply ascending near zero, so even if it's just a tiny number, it's often treated as rather large by the function F_K. Then, the actual function that I wrote there would make a turn and then go up. Instead, I can just make it linear if I want to.

And then if I look at the set where the sum of the x^β is finite, this will similarly be something that's extremely closely tied to the coordinate x. You're allowed to take certain infinite sums, but basically, you have to ensure that the coefficients have exponential decay, but not quite that actually.

From the liquid story, we can form some sum, and there exist some α such that the sum of the x^β is finite. So, in principle, we'd like to just ask for usual stability, but as I said, this doesn't really work. But when you slightly restrict the class of sums for which they ask, such that they are always part of the structure of a vector space, then it works. In the general case, you have much more spring.

So, yeah, the liquid series. The liquid series depends on an extra parameter, and the theory can only form those sums where the x_n have exponential decay, meaning that there exists some $\epsilon > 0$ and some constant $C > 0$ such that the absolute value of x_n is at most C times $2^{-n\epsilon}$. If I didn't put the ϵ on here, then this would be some kind of exponential decay.

It's enough to have some fractional power of n here in the exponent, and if they satisfy some decay condition like this, then you're allowed to sum. So, why does this expression appear here? Well, if you take $2^{-n\epsilon}$ and then apply this kind of procedure, then the log turns this into $n^{-\epsilon \cdot K}$, and this becomes summable when $\epsilon \cdot K$ becomes very small.

So, this is a rather crazy type of growth conditions from getting real numbers, but it's still a workable thing because in complex geometry, at least all

The goal with this discussion of complex geometry would be just as good. So the claim is that you can do this computation of the three Grassmann elector spaces from the one I did over the variable Q. It takes a little bit of unraveling, but it's not that hard.

Finally, let me try to do some geometry. The goal is to define the GU. Okay, let me again record. The idea is something we've already used a couple of times. We have this fixed t, and an additive B, which we can use to normalize absolute values. So when you want to understand how large some other function is, you can see how large this is compared to Q. We can use Q to measure the size of any other function.

Basically, you're wondering how the powers of the absolute value of F from $\mathbf{C}$ to $\mathbf{P}^1$ behave. One way to organize this information is in the following setup. I will start to use this formal analytic language that this whole course aims to introduce. This language is really convenient to phrase things, because I could give a more down-to-earth version, but it would be much more confusing. It's a really elegant way of packaging the information.

Let me explain a little bit about this. We have a morphism T from some F-algebra A to $\mathbb{A}^1$. On the fine locus, you have the coordinate function. In particular, when you want to do something for any function, you can do it for the universal function, which is the function T on $\mathbb{A}^1$. This gives you a map from the spectrum of A to $\mathbb{A}^1$, sending your function T to your given one. So to define something for any function, it's enough to do it for $\mathbb{A}^1$.

There is some kind of absolute value of T function, measuring against the absolute value of Q. This can actually be infinite, because some could be some kind of unbounded function. A better way to think about this is that you have $\mathbf{P}^1$, and if you have the point with homogeneous coordinates $(x : y)$, this goes to $|x/y|$.

The implicit assumption here is that something as down-to-earth as this closed interval will have a certain very crazy incarnation in this analytic setup, and this is required to make sense of this. Again, the idea is that if you really want to go from 0 to ∞ here, I need to normalize the absolute value of Q. What I always do is to - but it doesn't matter here again, and I'll just assume we're in a formalism where something like this makes sense, where we have some way of measuring the absolute value on this.

Then we can define what I call the analytic Checkers space, which is the preimage of the open part. Intuitively, this is the union over all n of the locus where the absolute value of T is bounded between some powers of the absolute value of Q. This was something that already entered at the beginning of my last lecture, that this is what you just start with if you want to define the T-adic cohomology.

Then you still have a $\mathbb{G}_m$ action on this, by multiplication by 2. This map here is actually some kind of multiplicative map. If the absolute values multiply, the product of the absolute values, where this happens to be zero and the other is infinity, then there's no claim being made. But in particular, on this locus, you have this analytic Checkers space mapping to $[0,\infty]$, and this corresponds to multiplication by $1/2$ here. This map here is actually proper, because $\mathbf{P}^1$ is proper, $[0,\infty]$ is proper, so the map is proper.

Proper, and then if you pass the equation, then you have $\mathbb{G}_m/\mathbf{Z}$. I mean, this must be totally discontinuous. I mean, free and totally discontinuous is actually because it's, it is here, right? So this then is just $\mathbf{F}^\times/\mathbf{Z}$, which is a copy of the circle S^1.

So the $\mathbb{G}_m$ is locally isomorphic to S^1, but now S^1 is again proper. And so this means that this one is proper, but of course it's also locally isomorphic to this one, which is an open subset of $\mathbf{P}^1$, and $\mathbf{P}^1$ ought to be smooth, so it's a curve. And this is the $\mathbf{D}$, right? And so this is how we want to argue, how we think to construct this curve.

Questions? $\mathbf{Z}_\infty$ is also defined over this a. You can base change it to a, you don't have to write it. And this always–there will be a functor from condensed sets towards analytic spaces, and so $\mathbf{Z}_\infty$ I consider as a closed set, and this will have an incarnation as an analytic space. This will actually mean that the theory of analytic spaces will be related to the condensed story in two ways: one, because the analytic rings themselves are founded on condensed things, which is the role that condensed objects have played so far. But suddenly there will be another role that the condensed sets play, because the way this thing will be realized in the analytic space world, it actually just uses the adic rings, it will not use any interesting condensed structure on the ring level.

14.3. **The Tate elliptic curve.**

15. Stacks (Clausen)

https://www.youtube.com/watch?v=EEH_0QhrgEg&list=PLx5f8IelFRgGmu6gmL-Kf_Rl_6Mm7juZO

The topic of today's lecture will be stacks.

So far, we've discussed the theory and examples of "analytic rings". Next, we will explain how to use these to build geometric objects, the "analytic stacks". We already saw a clue of what phenomena we want to include in this world of analytic stacks in the previous lecture, namely the Tate curve over the gaseous base ring.

Today we won't give the precise definition of analytic stack, but will provide motivation from algebraic geometry. The paradigm here is you have commutative rings, and you want to think of these as describing some sort of basic geometric objects which are called affine schemes.

$$\text{commutative rings} \rightsquigarrow \text{affine schemes}$$
$$R \mapsto \operatorname{Spec} R$$

These two categories are anti-equivalent. Then you want to allow yourself to glue these affine schemes together to make more general geometric objects.

There are two aspects to this gluing:

(1) What gluings are allowed?
(2) How to identify the results of different gluings? More generally, what are the maps/what is the category we get from these formal gluings of affine schemes?

A classic example is the projective line

$$\mathbf{P}^1 = \mathbf{A}^1_+ \cup_{\mathbf{G}_m} \mathbf{A}^1_-$$

where we glue together a "plus" version of $\mathbf{A}^1$ and a "minus" version of $\mathbf{A}^1$ along the multiplicative group $\mathbf{G}_m$. But we want this object to have symmetries like PGL_2, which don't respect this decomposition. So there has to be something said about what are the maps between different gluings.

It's better to think of (2) first: specify an ambient category containing the category of affine schemes, and then single out a full subcategory by specifying allowable gluings of affine schemes in the larger category. This is one way to describe a class of geometric objects in algebraic geometry.

$$\begin{tikzcd} \text{category} \arrow[dr, phantom, "\rotatebox{-45}{$\supseteq$}"] & \supseteq & \text{affine schemes} = \mathrm{CRing}^{\mathrm{op}} \arrow[dl, phantom, "\rotatebox{45}{$\subseteq$}"] \\ & \text{single out } \{*\} & \end{tikzcd}$$

In the classical approach to schemes, you take the ambient category to be locally ringed spaces, so a commutative ring gives you $\operatorname{Spec} R$, and then a scheme is a locally ringed space which is locally isomorphic to some $\operatorname{Spec} R$. So the allowable gluings between affine opens are gluings along open subsets, in terms of viewing $\operatorname{Spec} R$ as a locally ringed space.

There's a more modern approach which says we shouldn't try to be clever about choosing an ambient category—we don't have to find the concept of a locally ringed space. We can just formally build an ambient category based on the category of commutative rings, and then work there. The category where you're allowed to glue arbitrary affine schemes is called the **category of presheaves** on affine schemes, Psh(Aff). This is the universal category in which you can glue; more formally, it's the initial category-with-all-colimits with a functor from Aff, and giving a colimit-preserving functor out of Psh(Aff) is the same as giving a functor out of Aff. There's a set-theoretic technicality here, so caution is needed; we'll discuss that later. The category Aff is not a small category, so one has to be careful when taking functor categories out of it.

Now you've formally allowed yourself to glue, but you haven't explained how you should identify the results of two different gluings. If you pass to sheaves for some Grothendieck topology, that explains how to identify gluings; more generally, how to map between two of these formal gluings. The more covers you put in your Grothendieck topology, the more maps you're going to be making, which might not be so evident from the perspective of the cover. For example, the automorphisms PGL_2 of $\mathbf{P}^1$ may not be apparent. But you don't want to add too many elements to your cover, or else you might destroy information by identifying too many things; so there's a delicate choice to be made in the Grothendieck topology.

For example, you could take the Zariski topology, and then schemes is a full subcategory of Zariski sheaves on affine schemes such that they're locally representable, in the sense of open covers.

$$\text{schemes} \subseteq \mathrm{Sh}^{\mathrm{Zar}}(\mathrm{Aff})$$

You can define the notion of an open inclusion in $\mathrm{Sh}^{\mathrm{Zar}}(\mathrm{Aff})$ just by reduction to the case of Aff: a map $X \to Y$ in $\mathrm{Sh}^{\mathrm{Zar}}(\mathrm{Aff})$ is an open inclusion if for every $\operatorname{Spec} A \to Y$, the pullback

$$\begin{tikzcd}
\operatorname{Spec} B \arrow[r] \arrow[d] \arrow[dr, phantom, "\lrcorner", very near start] & X \arrow[d] \\
\operatorname{Spec} A \arrow[r] & Y
\end{tikzcd}$$

is an open inclusion $\operatorname{Spec} B \to \operatorname{Spec} A$ in Aff (this includes the condition that the pullback is in Aff).

This is the perspective we're going to take on defining analytic stacks from analytic rings. However, we don't want to take schemes as a model, because more general geometric objects come up and are relevant, namely *stacks*.

15.1. **Why stacks?** Often moduli spaces in algebraic geometry have the form X/G, where X is a variety and G is a group variety or scheme acting on X. So we have a quotient of some variety by some group of automorphisms.

Example 15.1 (Moduli of elliptic curves)**.** Consider the moduli stack of elliptic curves $\mathcal{M}_{\mathrm{ell},\mathbf{Z}[\frac{1}{6}]}$, where we invert 6 to simplify the discussion. Then this is X/G with

$$X = \operatorname{Spec} \mathbf{Z}\left[\tfrac{1}{6}\right][A,B][\Delta^{-1}]$$
$$G = \mathbf{G}_m$$

where Δ is the discriminant.

This parameterizes elliptic curves with affine equation in Weierstrass form

$$y^2 = x^3 + Ax + B.$$

It then turns out that the only isomorphisms between two elliptic curves given by the above equation are given by scalar multiplication with certain weights on the x and y variables, and that gives the $\mathbf{G}_m$ action.

There's also many other ways of presenting the same stack. For example, you can add level structure and then mod out by a finite group,

$$\mathcal{M}_{\mathrm{ell},N}/\operatorname{GL}_2(\mathbf{Z}/N)$$

If N is sufficiently large, $\mathcal{M}_{\mathrm{ell},N}$ will be represented by a variety, and then you quotient out by the finite group $\operatorname{GL}_2(\mathbf{Z}/N)$. The Grothendieck topology that we choose should be such that this is identified with $\mathcal{M}_{\mathrm{ell}}$ our category, so we should at least allow étale covers into the story for this, and that is indeed the classic choice.

These quotients exist in the category of schemes, and they give $\mathbf{A}^1_{\mathbf{Z}[\frac{1}{6}]}$, the so-called "$j$-line", implemented by the j function. However, this is not a good quotient. One way of measuring that is: on the moduli stack of elliptic curves, there's a natural line bundle ω,

$$\omega = \operatorname{Lie}(E)^*,$$

which is the dual of the one-dimensional vector space of tangent vectors at the origin. In other words, it's the cotangent space of the universal elliptic curve.

This means you can write down a line bundle on $\operatorname{Spec} \mathbf{Z}\left[\frac{1}{6}\right][A,B][\Delta^{-1}]$ which is equivariant for the $\mathbf{G}_m$-action, or a line bundle on $\mathcal{M}_{\mathrm{ell},N}$ which is equivariant for the $\operatorname{GL}_2(\mathbf{Z}/N)$-action. However, this line bundle doesn't descend to the quotient A^1, so it's a bad quotient in the sense that you can have equivariant objects on the top, but they don't come from something on the bottom. Even more basic, the universal elliptic curve over $\mathcal{M}_{\mathrm{ell}}$ can't be defined over $\mathbf{A}^1$.

The problem in the above example is that the action is not free. There are some elliptic curves with extra automorphisms, and because the action isn't free, the naive quotient in schemes is collapsing too much. The solution is to take the quotient in a more refined (2-)category: (sheaves of) groupoids.

Working in groupoids, there's a notion of groupoid quotient

$$X /\!\!/ G$$

where:

- the objects of $X /\!\!/ G$ are the elements of X
- $\mathrm{Hom}_{X/\!\!/G}(x, y) = \{g \in G \mid gx = y\}$

There's always a map $X \to X /\!\!/ G$, and the fibers are all isomorphic to G, where "fiber" means a pullback

$$\begin{tikzcd} X_x \arrow[r] \arrow[d] \arrow[dr, phantom, "\lrcorner", very near start] & X \arrow[d] \\ \{x\} \arrow[r] & X /\!\!/ G \end{tikzcd}$$

So this sort of allows us to pretend that every action is free; the map $X \to X /\!\!/ G$ is always like the total space of a G-bundle.

The only trick is you have to interpret fiber product in the sense of 2-categories: in a pullback diagram of groupoids,

$$\begin{tikzcd} A \times_C B \arrow[r] \arrow[d] \arrow[dr, phantom, "\lrcorner", very near start] & B \arrow[d, "g"] \\ A \arrow[r, "f"'] & C \end{tikzcd}$$

an object of $A \times_C B$ consists of a triple (a, b, γ) where $a \in A$, an object $b \in B$, and γ is *an isomorphism* between $f(a)$ and $g(b)$ in C.

So it's a way of taking a quotient such that you don't really care about the difference between a free action and a non-free action. And it's such that you formally have that line bundles on $X /\!\!/ G$ are the same as equivariant line bundles on X and so on and so forth.

This leads to notions such as Deligne-Mumford stack, or more generally, Artin stack. These are all full subcategories of étale sheaves on affine schemes.

$$\text{Deligne-Mumford stacks, Artin stacks} \subseteq \mathrm{Sh}^{\text{ét}}(\mathrm{Aff})$$

In the discussion of schemes, we had Zariski sheaves that we asked to be locally representable, where "locally" is in the sense of open covers. We also could have used étale sheaves, it doesn't change the resulting category of schemes. For Artin stacks, you more or less require that you have a smooth cover by affine schemes, and maybe some technical things you want to put in there as well.

The basic example is, let X be an affine scheme, for simplicity let's say over some base S. Let G be a smooth group scheme over S which acts on X. Then you pass to the quotient in the stacky sense, $X \to X /\!\!/ G$.

So these are more general geometric objects, and this is really good for moduli theory. The theoretical justification for that is the **Artin representability theorem**, which gives concrete criteria for when a functor is represented by an Artin stack. But it's still constrained by the need for a smooth cover; in particular, this means finitely presented. So if you had some non-finite type group scheme acting on something, you wouldn't necessarily be able to take the quotient in Artin stacks.

Now you might say, why do you care? It turns out there are many $X \in \mathrm{Sh}^{\text{ét}}(\mathrm{Aff})$ which are geometrically relevant, but are **not** Artin stacks.

Example 15.2 (Formal schemes)**.** Let R be a noetherian ring, and $I \subset R$ an ideal of R. We want to consider the formal spectrum $\mathrm{Spf}(R_I)$. There are different options as to how to encode this thing, and what it should mean. One we've already discussed is Huber's theory, which includes formal schemes as an example, and is based on viewing R as a topological ring. Grothendieck's theory of formal schemes is also based on viewing R as a topological ring, but localizing along a smaller subset than in Huber's theory.

Another way of looking at it is it's just the union of all the nth order infinitesimal neighborhoods of $\mathrm{Spec}(R/I)$ inside $\mathrm{Spec}(R)$,

$$\mathrm{Spf}(R_I) = \bigcup_n \mathrm{Spec}(R/I^n).$$

So giving some (finite-type) data on a formal scheme should be the same as giving compatible collections of data at all of these finite levels. For example, vector bundles on $\mathrm{Spf}(R_I)$ should just give a compatible collection of vector bundles in the usual sense (finitely-generated projective modules),

$$\mathrm{Vect}(\mathrm{Spf}(R_I)) = \varprojlim \mathrm{Vect}(R/I^n).$$

This is a very different gluing from what you think about with schemes, and even when you think about Artin stacks. There are no smooth covers in sight here; instead, you're taking some union of infinitesimal thickenings and getting a new object.

There are other examples: for some moduli problems, you really are quotienting by an infinite-dimensional group. One example that's dear to the hearts of both homotopy theorists and number theorists is the moduli of one-dimensional formal groups, the **Lubin-Tate space**. In this case the group you have to mod out by is coordinate changes on a one-dimensional formal scheme, and then there's infinitely many coefficients you have to specify. So you have an infinite-dimensional group you have to mod out by, and that doesn't fit into the framework of Artin stacks.

This suggests using a different Grothendieck topology, say fpqc instead of étale, if you want to accommodate infinite type phenomena in your covers. But in addition to these, there's also a very remarkable class of examples started by Carlos Simpson.

Simpson: "Every linear algebra category is QCoh(some stack)"

Simpson didn't literally say this, and it's much too strong to be true; this is just an slogan interpretation of his work. His work gives lots of examples of this phenomenon, where you have a natural linear-algebraic category, and then it turns out you can write down some stack whose quasicoherent sheaves are that category. There are fun examples of this already in the world of Artin stacks.

Example 15.3 (Representations)**.** Let G be an algebraic group over a field k. Then a special case of a non-free quotient is a point $* = \mathrm{Spec}\, k$ with an action of G, so

$$BG = * /\!\!/ G.$$

A quasicoherent sheaf on BG should be a G-equivariant quasicoherent sheaf on the point; but a quasicoherent sheaf on a point is just a k-vector space, and the G-equivariance exactly means you have a representation of G. So we get

$$\mathrm{QCoh}(BG) = \mathrm{Rep}_G(k\text{-vector spaces}).$$

Example 15.4 (Filtered objects)**.** We can also consider $\mathbf{A}^1/\mathbf{G}_m$ for the natural action of $\mathbf{G}_m$ on $\mathbf{A}^1$ by scalar multiplication. This is a funny stack, because there's an open locus in this stack, corresponding to a $\mathbf{G}_m$-invariant open locus in $\mathbf{A}^1$, namely $\mathbf{G}_m$ itself. On that open locus, you're taking $\mathbf{G}_m/\mathbf{G}_m$, which is a point. So this has a point as an open subset, and the closed complement is $0/\mathbf{G}_m = B\mathbf{G}_m$. In this case, (flat) quasicoherent sheaves are given by filtered k-vector spaces.

$$\mathrm{QCoh}^{\mathrm{flat}}(\mathbf{A}^1/\mathbf{G}_m) = \{k\text{-vector spaces equipped with a } \mathbf{Z}\text{-indexed filtration}\}$$

The restriction to flat modules is just to stay in the abelian world rather than going derived.

Note that at the origin, we have

$$\mathrm{QCoh}^{\mathrm{flat}}(B\mathbf{G}_m) = \{k\text{-vector spaces equipped with a } \mathbf{Z}\text{-indexed grading}\}$$

These are Artin stacks, so there's nothing exotic there. Here's a more interesting example.

Example 15.5 (de Rham stack)**.** Let k be a field of characteristic 0, and let X/k be a smooth variety. Then you can form, and Simpson did, what's called the **de Rham stack** of X. There is a presentation

$$X \twoheadrightarrow X^{\mathrm{dR}}$$

but this is not quotienting out by a group action, it's just some by equivalence relation. The equivalence relation in question is that which identifies two points if they're "infinitesimally close" to each other. Equivalence relations are supposed to live in the product $X \times_k X$, and what you do is you take this product and you formally complete along the diagonal,

$$(X \times_k X)_{\widehat{X}}.$$

Then X mod this equivalence relation gives X^{dR}. The formal completion should be taken in this sense of Example 15.2, i.e. the union of the different scheme structures that are available on the diagonal as a closed subset.

(The definition of X^{dR} makes sense in arbitrary characteristic, but some things we're about to say will not be true in positive characteristic.)

What are quasi-coherent sheaves, let's say vector bundles, on X^{dR}? This is the same thing as vector bundles on X equipped with some kind of descent datum, but that descent datum exactly amounts to a flat connection.

$$\mathrm{Vect}(X^{\mathrm{dR}}) = \{\text{vector bundles on } X + \text{flat connection}\}$$

That's Grothendieck's interpretation of what is a flat connection. It's exactly giving descent, identifying infinitesimally close points.

There's also cohomology of the structure sheaf. This gives de Rham cohomology, which is the natural notion of cohomology in the world of vector bundles with flat connection.

$$R\Gamma(X^{\mathrm{dR}}, \mathcal{O}_{X^{\mathrm{dR}}}) = R\Gamma_{\mathrm{dR}}(X/k)$$

This is not even close to being an Artin stack either, and for different reasons from the moduli of formal groups. Here we're modding out by some formal scheme giving an equivalence relation.

Example 15.6 (Prismatization)**.** More recently, Bhatt-Lurie [?, ?] cite me$F$-gauges and Drinfeld cite medefine stacks whose QCoh capture coefficient systems for various p-adic cohomology theories in characteristic p or mixed characteristic. For example, there's a stack capturing de Rham characteristic p, but it is not the one of Example 15.5. You have to use the divided power envelope of the diagonal instead of the formal neighborhood of the diagonal.

So there are stacks capturing prismatic cohomology, de Rham cohomology, and crystalline cohomology, as well as filtered versions of these. Moreover, the comparison theorems in prismatic cohomology between all of these various cohomology theories can be explained, so to speak, "geometrically" in terms of the stacks. (It's arguable how geometric these kinds of stacks are :)

Maybe the better way to say it is that, a priori these comparison theorems are about comparing linear algebra categories, e.g. vector spaces. But it turns out there's a more fundamental explanation, which is that you have an isomorphism of *stacks*. You then deduce comparison theorems of cohomology theories by passing to quasicoherent sheaves. So you promote a comparison of cohomology theories to an isomorphism of stacks.

I want to give another example of this phenomenon. We've seen de Rham cohomology in characteristic zero, some p-adic cohomology theories as well, but what about Betti cohomology? Here's a fun example which actually has quite a bit of relevance for the course, so that's why I'm going to mention it.

Example 15.7 (Betti stacks)**.** "Betti cohomology" is the algebraic geometers' term for, if you have a complex variety, then you take singular cohomology or sheaf cohomology with constant coefficients on the underlying topological space with the analytic topology. For example, if you have a compact Hausdorff space S, then I claim that you can make a stack.

How do you do it? You use the old familiar idea: you find a surjection from a profinite set T, and then you have some fiber product $T \times_S T$.

$$T \times_S T \rightrightarrows T \twoheadrightarrow S$$

Since $T \times_S T$ is a closed subset of a product of two profinite sets, it'll also be a profinite set. Write $T_0 := T$ and $T_1 := T \times_S T$.

So your compact Hausdorff space S is a quotient of an equivalence relation in the category of profinite sets. We can then apply $C(-, \mathbf{Z})$ (continuous $\mathbf{Z}$-valued functions) followed by Spec to get a groupoid in the category of schemes, in fact in the category of affine schemes. We define the Betti stack of S as the quotient of this equivalence relation in the category of sheaves for the fpqc topology on affine schemes.

$$\operatorname{Spec} C(T_1, \mathbf{Z}) \rightrightarrows \operatorname{Spec} C(T_0, \mathbf{Z}) \twoheadrightarrow S^{\mathrm{Betti}}$$

You could also replace $\mathbf{Z}$ with a commutative ring k.

What are quasi-coherent sheaves on Betti stacks? These are just usual sheaves of abelian groups on the topological space S.

$$\mathrm{QCoh}(S^{\mathrm{Betti}}) = \mathrm{Sh}(S, \mathrm{Ab})$$

That's a fun exercise. So, coherent cohomology on the Betti stack S^{Betti} is just usual topological cohomology of the topological space S. More generally, we can embed condensed sets into stacks via the above procedure, using the presentation of a condensed set via profinite sets.

(something I didn't hear)

What you have to check to see that is that, if you have a surjective map of profinite sets, then it goes to a faithfully flat map on the level of continuous functions. That's not that hard to do: $C(T_i, \mathbf{Z})$ are filtered colimits of continuous functions of finite sets, which as rings are copies of products of $\mathbf{Z}$. In fact, for *any* map of profinite sets $T_1 \to T_0$, the induced map $C(T_0, \mathbf{Z}) \to C(T_1, \mathbf{Z})$ is flat, and then if it's surjective it's faithfully flat. You also need to check that fiber products in profinite sets correspond to relative tensor products; this follows again by a reduction to finite sets.

So now we're faced with this somewhat baffling array of different stacks, some of which don't resemble Artin stacks in the least. But we want them because they're convenient ways of encoding different linear-algebraic and geometric phenomena.

Question: how to define a reasonable subcategory of $\mathrm{Sh}^{\mathrm{fpqc}}(\mathrm{Aff})$ containing all these examples?

Answer: $\mathrm{Sh}^{\mathrm{fpqc}}(\mathrm{Aff})$ (modulo set theory)

In other words, it's not clear that there's any other answer to this question than the entire category $\mathrm{Sh}^{\mathrm{fpqc}}(\mathrm{Aff}, \mathrm{An})$. So there is content in the answer, but it didn't necessitate introducing anything wasn't present in the question.

15.2. **Desired examples of analytic stacks.** This is also the approach we will take in defining analytic stacks. We will define a Grothendieck topology on $\mathrm{AnRing}^{\mathrm{op}}$ and then take sheaves with respect to it (again modulo set theory). Before getting into the details of exactly which Grothendieck topology, and these set theoretic technicalities as well, let's see what kind of phenomena we want to capture, so what the examples should be. In some sense, we've already seen some.

Example 15.8 (Adic spaces)**.** We certainly want that any adic space in the sense of Huber should give rise to an analytic stack. We already explained how the basic ingredient in adic spaces, namely Huber pairs (R, R^+), give rise to analytic rings, and we explained something about how the formalism of analytic rings lets you glue. But we didn't quite discuss how you can use that to then glue these analytic rings together to get some kind of analytic stack.

We saw that at least you can localize the category of modules over that analytic ring along Huber's spectrum, but we didn't quite discuss how you can use that to then glue these analytic rings together to get some kind of analytic space. But we certainly want the kind of gluing that shows up in Huber's theory, gluing along rational open subsets in the topology defined by a basis of rational opens, we want that kind of gluing to be allowed and to give you an analytic space.

Example 15.9 (Complex analytic spaces)**.** We also want any complex analytic space to give an analytic stack, say over $\mathbf{C}^{\mathrm{gas}}$ or $\mathbf{C}^{\mathrm{liq}_p}$. So the kind of gluing allowed should also incorporate gluing along open subsets in complex analytic geometry.

Example 15.10 (Algebraic stacks)**.** Another even more basic thing is we want the world of analytic geometry to be a generalization of the world of schemes, and even of algebraic stacks (in some sense—maybe not precisely the fpqc topology discussed above, but a slight modification of that). These should live over the universal base $\mathbf{Z}$, with trivial analytic structure ($\mathrm{Mod}(\mathbf{Z}) = \mathrm{CondAb}$). So universally, over any analytic ring, if you have some algebraic object, you can get an analytic object.

Example 15.11 (Banach rings)**.** Any Banach ring R should also give rise to an analytic stack. This in some sense matches Berkovich's theory, in the same way that the affinoid analytic stacks coming from pairs (R, R^+) match Huber's picture. There's a small interesting tidbit here, which is that the stack that we'll assign to a general Banach ring will actually not be affinoid, it will really be a stack. So, if you take $\mathbf{Z}$ with the usual archimedean norm, it will go to an actual stack that's not affinoid; instead, it's a stack which in some sense corresponds to $\mathcal{M}(\mathbf{Z})$, the Berkovich spectrum of $\mathbf{Z}$, so that's a fun little twist.

Example 15.12 (Coefficient systems)**.** As above, there should be analytic stacks whose QCoh capture various coefficient systems for cohomology (sheaves of abelian groups, vector bundles with connection, prismatic F-gauges etc.)

Example 15.13 (Tate curve)**.** We want to be able to define the Tate curve as well as its uniformization over $\mathbf{Z}[\hat{q}^{\pm}]^{\mathrm{gas}}$. We also want to have machinery to prove it's algebraic. So we have this curve that we define via uniformization. We take the analytic $\mathbf{G}_m$ and we quotient by the multiplication by q, and we get something which Peter argued was a smooth proper curve, and it has an identity section. So then, if you have some Riemann-Roch theorem, then you can see that you have a projective embedding. And if you have some GAGA theorem, then you'll be able to see that it has to be algebraic. So we want Riemann-Roch, and we want GAGA.

Example 15.14 (Comparison theorems)**.** On the theme of GAGA, we want that various linear algebraic comparison results should promote to isomorphisms of stacks. GAGA is one such.

GAGA is a general principle which applies in different contexts. But for example, in the world of complex analytic spaces, it says that if you have a proper algebraic variety X over the $\mathbf{C}$, so it has an analytification X^{an} which is compact, then it says that coherent sheaves and their cohomology in the algebraic and in the analytic sense agree.

So that's a question about making a comparison between two linear algebraic categories. It's saying algebraic coherent sheaves are the same as analytic coherent sheaves. And one thing we would like our formalism to do is to promote that to an isomorphism of stacks.

Another example, again in the complex analytic context, would be the comparison between Betti cohomology and de Rham cohomology. This should also promote to an isomorphism of stacks.

15.3. **Analytification.** Recall that adic geometry in the solid context really got started once we noticed that there's a nice subset of $\mathbf{A}^1_{\mathbf{Z}_\blacksquare}$, namely the closed unit disc, which we were thinking of as open. Or you could think of it in terms of its complement. or the translation of that back to the origin. In the end, algebraically speaking, this came from an idempotent algebra

$$\mathbf{Z}[\![T]\!] \in \mathrm{Solid}_{(\mathbf{Z}[T],\,\mathbf{Z})}$$

Once we had this idempotent algebra, then we could move it to infinity via the change of variables $T \mapsto T^{-1}$, and then that gave us the closed unit disc, and then that let us tie into the (R, R^+) theory, where we could ask that the elements in R^+ actually land in the closed unit disc as opposed to just being maps to $\mathbf{A}^1$.

Similarly, over $\mathbf{Z}[\hat{q}^{\pm}]^{\mathrm{gas}}$, you can define a "subset" of $\mathbf{A}^1_{\mathbf{Z}[\hat{q}^{\pm}]^{\mathrm{gas}}}$, corresponding to an idempotent algebra of "functions which are convergent in some (unspecified) disk around the origin". So it's germs of functions defined at the origin, so to speak. Formally, it's going to be the filtered colimit

$$P[q^{-1}] := \varinjlim \left(P \xrightarrow{q} P \xrightarrow{q} P \xrightarrow{q} \cdots \right)$$

In the solid case, P was itself idempotent and turned into $\mathbf{Z}[\![T]\!]$. In the gaseous setting, P is not idempotent in $\mathrm{Mod}_{\mathbf{Z}[T]}(\mathrm{Mod}_{\mathbf{Z}[\hat{q}^{\pm}]^{\mathrm{gas}}})$, but it becomes idempotent after we take this colimit to shrink the open unit disc down to the origin.

The idempotent algebra $P[q^{-1}]$ satisfies many of the same properties as $\mathbf{Z}[\![T]\!]$, and it again lets us import Huber's theory of pairs to this context. What you do is you take this idempotent algebra functions of germs at the origin in the affine line, you move it to infinity and you get germs at infinity.

Let R be of finite type over $\mathbf{Z}[\hat{q}^{\pm}]^{\mathrm{gas}}(*)$, and assume that $\mathbf{Z}$ is bounded in R. Then you get an analytic ring structure on R, namely $(R, \mathrm{Mod}_R(\mathrm{Mod}_{\mathbf{Z}[\hat{q}^{\pm}]^{\mathrm{gas}}}))$. Every element $f \in R$ gives a map

$$\mathrm{AnSpec}(R) \xrightarrow{f} \mathbf{A}^1,$$

and we pass to the "subset" where all such maps land in the locus "away from ∞". This gives a new analytic space $\mathrm{Spec}(R)^{\mathrm{an}}$, the "analytification" of $\mathrm{Spec}(R)$.

Example 15.15. When $R = \mathbf{Z}[T^{\pm}]$, we get $\mathbf{G}_m^{\mathrm{an}}$. After base change to a ring where 2 is bounded, this is the object of the previous lecture, the thing we quotiented out by to get the Tate curve.

So now we have two different contexts (solid and gaseous) in which you can import Huber's theory of pairs into the world of analytic spaces. It turns out that they can be glued together, and so can the idempotent algebras. First notice that the gaseous theory makes sense over $\mathbf{Z}[q]$: all we had to do to define the gaseous theory was we had to write down the endomorphism $1-qt$ of P, and then ask that it become an isomorphism in our theory. And to do that, you didn't need to require q to be topologically nilpotent. So this gives an analytic ring $\mathbf{Z}[q]^{\mathrm{gas}}$.

- if we set $q=0$, we get the uncompleted $\mathbf{Z}$ theory
- if you set $q=1$, you get the solid theory, $\mathbf{Z}^{\blacksquare}$
- if you require q to be topologically nilpotent and a unit, you get the gaseous theory, $\mathbf{Z}[\hat{q}^{\pm}]^{\mathrm{gas}}$
- if you work away from the locus where q is topologically nilpotent, then you are working over a theory where you force both q and q^{-1} to be gaseous, and that implies that 1 is gaseous, which means you're in the solid theory, so you get $\mathbf{Z}[q^{\pm}]^{\blacksquare}$.

Then we can define a certain quotient of

$$\mathrm{Spec}(\mathbf{Z}[q^{\pm}]^{q\text{-gas}})$$

which more or less parameterizes choices of a notion of "analytification". So, anytime you have this variable q which you've declared to be gaseous, you can form this colimit and you'll get an idempotent algebra at zero, and you can move it to infinity, and then you get a notion of analytification, as discussed.

However, different choices of q can give rise to the same thing. If two choices of q differ by a bounded unit, so something in $\mathbf{G}_m^{\mathrm{an}}$, then you'll get the same algebra. So the quotient we want is

$$\mathrm{Spec}(\mathbf{Z}[q^{\pm}]^{q\text{-gas}})/\mathbf{G}_m^{\mathrm{an}}$$

where $\mathbf{G}_m^{\mathrm{an}}$ acts by multiplication on q.

This is a different role of stacks in the theory. Here we have a stack which is a quotient of (AnSpec of) an analytic ring by an equivalence relation, and which is in some sense parameterizing choices of analytic geometry over a given base ring. Let R be an analytic ring with a map

$$\mathrm{Spec}\, R^{\triangleright}(*) \to \mathrm{Spec}(\mathbf{Z}[q^{\pm}]^{q\text{-gas}})/\mathbf{G}_m^{\mathrm{an}},$$

which we call a "gaseous structure" on $\mathrm{Spec}\, R^{\triangleright}(*)$ (and again assume $2 \in R$ is bounded). Then we get two functors from $R^{\triangleright}(*)$-schemes to analytic stacks over R, plus a natural transformation.

Say $X = \mathrm{Spec}\, A$, where A is an $R^{\triangleright}(*)$-algebra. We can view this as an analytic stack over the uncompleted $\mathbf{Z}$, and then you can base change that to R to get $X_R := X \times_{\mathbf{Z}} R$. But then you also have a subset

$$X_R^{\mathrm{an}} \xrightarrow{\subseteq} X_R,$$

the analytification over R, given by requiring that all the functions land in the part of $\mathbf{A}^1$ that's away from ∞. Now there's a general theorem.

Theorem 15.16 (GAGA)**.** *If $X \to \mathrm{Spec}(R)$ is proper (and finitely presented), and every element $f \in R(*)$ is bounded, then $X_R^{\mathrm{an}} \to X_R$ is an isomorphism.*

some discussion between Peter and Dustin about whether finitely presented is really necessary. It might not matter for GAGA, but could for some other things.

Remark 15.17. This implies completely formally that $D(X_R^{\mathrm{an}}) = D(X_R)$, which is some form of GAGA. Non-formally, maybe with some more conditions on R (but satisfied in practice), this implies that $\mathrm{Vect}(X_R^{\mathrm{an}}) = \mathrm{Vect}(X_R)$, which is classical GAGA.

Basically you can do algebraic geometry over the uncompleted $\mathbf{Z}$ theory, and then that means you can do algebraic geometry over any analytic ring just by base change.

One of the things that maybe I should have been emphasizing before launching into this whole discussion is that over the completed $\mathbf{Z}$ theory, you can basically do algebraic geometry. And then that means you can do algebraic geometry over any analytic ring just by base change. For example, we were considering $\mathbf{A}^1$ over an arbitrary base ring. It's not analytified yet, but when you have extra structure on your analytic ring, then that picks out a choice of what it means to analytify an algebraic variety.

All the classical GAGA theorems are special cases:

- complex-analytic GAGA after Serre (using $\mathbf{C}^{\mathrm{gas}}$ or $\mathbf{C}^{\mathrm{liq}_p}$)

- Grothendieck's formal GAGA: look up statement of formal GAGAif you have again complete noetherian ring R, and a proper scheme over it, then coherent sheaves on that is the same thing as coherent sheaves on the formal scheme you get by formally completing or, in other words, compatible collections of coherent sheaves on all the various n potent thickenings there.

  In terms of Huber pairs, you would work over

$$\mathrm{Spa}(R_I^\wedge, R_I^\wedge) \to \mathrm{Spec}(\mathbf{Z}^{\blacksquare})$$

  so $\mathrm{Spa}(R_I^\wedge, R_I^\wedge)$ inherits the notion of analytification from $\mathbf{Z}^{\blacksquare}$ based on the closed unit disc.
- non-archimedean GAGA over $\mathbf{Q}_p$ or any complete non-archimedean field. For this you would take your analytic ring to be $\mathbf{Q}_p^{\blacksquare}$. But you don't put the gaseous structure on it which factors through the map to $\mathrm{Spec}\,\mathbf{Z}^{\blacksquare}$, rather you put the gaseous structure on it which corresponds to

$$\begin{gathered}\mathrm{Spec}(\mathbf{Q}_p^{\blacksquare}) \to \mathrm{Spec}(\mathbf{Z}[\hat{q}^{\pm}]^{q\text{-gas}}) \\ q \mapsto p\end{gathered}$$

  That's the one that picks out the notion of analytification that corresponds to usual analytification of algebraic varieties over your non-archimedean field.

Remark 15.18. You could try to use the other gaseous structure on $\mathrm{Spec}\,\mathbf{Q}_p^{\blacksquare}$ where you factor through $\mathrm{Spec}\,\mathbf{Z}^{\blacksquare}$. But GAGA won't apply in this setting, since then $1/p$ will not be bounded, and if you make it bounded in the solid setting you'll kill everything.

Nonetheless, there is still a different gaseous structure on $\mathbf{Q}_p^{\blacksquare}$ obtained by factoring it through $\mathbf{Z}_p^{\blacksquare}$. In a sense the above gaseous ring structure corresponds to some kind of overconvergent version of rigid geometry, while the gaseous ring structure that factors through $\mathbf{Z}_p^{\blacksquare}$ corresponds to usual rigid geometry.

15.4. **Addressing set-theoretic technicalities. Unfinished starting from 1:29:22**

things that maybe I should have been emphasizing before launching into this whole discussion is that over the completed Z Theory you you basically you can do algebraic geometry um and then that then that means you can do algebraic geometry over any analytic ring just by just by base change so that's um so that's kind of like considering the apine line like I was considering the apine line for example over an ariary base ring it's not it's not analytify yet but when you have extra structure on your analytic ring then that uh then that picks out a choice of what it means to analytify also in algebraic variety um and so uh and I'm also conf exit are fine or yeah when I was describing kind of explicitly what it is I was looking at the case where X is apine but in general both things glue and so you then then you can Define uh you can doesn't need to be it doesn't need to be Aline no yeah um so so all the all classical Gaga theorems uh are special cases so there's um so for example groen's formal Gaga well the complex analytic Gaga um so Sarah's original Gaga um take for example C gaseous or P liquid or whatever you like um uh there's also formal Gaga uh which is you know if you have a a again complete nean ring and then you have a a proper a proper uh scheme over it then coherent sheaves on that is the same thing as coherent sheaves on the formal scheme you get by formally completing or in other words compatible collections of coherent sheaves on all the various n potent thickenings there um then you would take a spff well maybe I should say Spa so in terms of Huber pairs you would work you would work over this um which lives over a solid Z and therefore inherits the notion of analyics based on the the closed unit dis um it also includes like non- archimedian GMA Gaga uh say over I don't know over QP or any complete non- archimedian field so there you would take your analytic ring you would take to be a spec of say QP solid um but you wouldn't put the gaseous structure on it which factors through the map to spec Z rather you'd put the gaseous structure on it which corresponds to mapping topc of Z q hat plus or minus one Q gasas uh where Q goes to uh Q goes to P say so St say standard choice of topologically mil potent unit um that's the one that picks out the notion of analy ification that corresponds to usual anal analy ification of algebraic varieties over your non- archimedian field um you could have also chosen the other gashes structure on this guy where you factor through uh spec Z solid um and that would give you a different statement of Gaga in fact a different theorem so yeah so we're Mark and three could also choose uh the solid z uh gaseous structure on uh on solid QP that gives a an a priori different Gaga theorem oh the the analy ification of A1 will then just be the open unit or the closed unit dis so um yeah so there's so let me illustrate H that cannot beis it has what sorry dation sorry1 ofp canale so they must also1 so iation of A1 in that context is oh wait ah you're right you're right you're right I'm sorry you're right I'm sorry yeah that's that that open your dis is when you just force T to be analytic but there's no reason why uh that should also Force something like uh T over P to be analytic yeah no ah same Gaga thank you yeah I miss uh I I was not the same guys it's just no it is the same isn't it because you you would make you should always assume that your whole ah you're right I forgot that Axiom shoot yeah so I'm sorry you need ah oh boy than uh thanks again so you need uh to assume uh that every element in here is is bounded uh uh yeah so I was kind of making the same mistake again over and over um yeah I'm sorry uh so you don't get a Gaga statement at all in this context because um one over p uh is never going to be if you try to make one over P bounded in that sense then you're just going to kill everything so in in the solid QP setting but nonetheless um there's a point I wanted to make which is that there is still a different gaseous structure on solid QP obtained by taking um uh obtained by factoring it through solid zp and in a sense the difference between this gaseous ring structure and this gaseous ring structure is sort of the difference between usual rigid geometry and um and some kind of overon convergent version of rigid geometry um yeah uh okay so is there something weaker than n something weaker than n could you explain the question a result that would put uh identify x r with some less uh regular could you explain uh okay maybe okay yeah this assumption that this uh bound it yeah yeah so what is the issue if it's if you don't assume that it's empty like the if you don't assume that everything in here is bounded then when you try to do the analytics that I described then in particular you're always forcing all of the scalers IE elements of of here uh you're forcing them to be bounded anyway so if you don't if you don't have this assumption then your analyics changing your your base so we have so a special case is when you take x equals Spec R and you try to analytify that what this assumption is saying is basically that then it doesn't change it like specr is its own analytify ring where it's where this thing has been forced to be bounded but in the case of solid QP if you try to force with that with that with that gaseous structure factoring through solid Z if you try to force all of the scalars to be bounded um you're just going to get the zero ring because yeah yeah um right yeah um so yeah so that was the uh yeah x equals specr was giving a problem there my apologies um okay uh right so that was um kind of explaining one example of where a linear algebraic comparison result uh can be promoted to an isomorphism of stacks um more generally we want relations um so relations between the various kinds of stacks uh various examples so for already in Peter's talk he had this map from the T curve uh with parameter Q to the topological Circle so and we want this to make sense as a map of analytic stacks so where I already explained how such a thing is an algebraic stack and algebraic Stacks will embed into analytic Stacks even over the the initial analytic ring um and then we have the Tate curve and so this kind of analytic space should map to that kind of analytic space but also if x is a complex analytic space uh then you want um X to map to uh its underlying topological Space X of c um and if say R is a banak ring then you want its well sort of its liquid spec uh to M map to the burkovich Spectrum um so uh yeah so there's all these we're going to have all these various different classes of analytic spaces and we want the category to be such that we can make maps between them when we expect comparisons um so for example it's well known that the coherent chology of a comp complex analytic space localizes on the underlying topological space um that's kind of more or less by definition in a lot of ways um and that kind of linear algebraic relation is supposed to be explained by the existence of such a map in this in this large category of analytic spaces um so all right um I think uh maybe I'll say one word about the set theoretic technicalities just to get it out of the way um so so addressing uh um so um well let's go back to our uh algebraic analog so so this was a Comm the opposite category of commutative Rings um so we have fpqc sheaves on there but again this is not a legitimate definition because this is not a small category and this by definition is a full subcategory of functors from uh commutative Rings uh to say sets or groupoids or whatever you like um and there's well-known problems with such things that the morphisms in this funct category involve a some more than a sets worth of data so you have morphisms that aren't sets but are some sort of bigger entity so that's a real pain um so what is the fix um uh consider so instead of instead of pre- shees uh on on F consider uh what are called accessible prees um which are the same thing as accessible functors uh from commutative rings to sets and what are accessible functors uh if and only if there exists a cardinal Kappa regular Cardinal uh such that uh fun F such that f commutes with with Kappa filtered coletes um so what does this mean uh the the first choice of Kappa is Alf not um um and in general when you think of a regular Cardinal you shouldn't try to think about what it is as a set what you should really think of is the collection of sets that are of smaller cardinality that's what Kappa is really indexing it's don't think of it as the cardinality of some set think of it as indexing the so this corresponds to finite sets um those are the sets of cardinality smaller than Kappa and then the notion of Kappa filtered Co limit in that case is just the usual notion of filtered Co limit so if your funter commutes with filtered colimits you're okay but if Kappa gets bigger uh you have fewer uh Kappa filtered Co limits so to be a so for example if you take Kaa equals lf1 then you're indexing the countable sets and you're only then a Kappa filtered Co liit would be one where every countable set has a cone as opposed to just every finite set so there are fewer kapper filtered colimits that means there's more examples of functors which commute with Kappa filtered colimits um and uh uh Kappa um so so the nice thing uh is that every uh for every X in F uh the un uh the on its image under the on embedding is always accessible and in fact accessible is equivalent to being a small co- liit of representable functors hxs so if we think of these apine schemes is the basic building blocks and Co limits as our gluing procedure then we're saying kind of an obvious thing we're only allowed to glue a sets many worth of things together at a time and that's exactly what's captured by this notion of accessible functor and moreover uh fpqc shif foration uh preserves accessible sheaves or pre- shees sorry that was actually that's a theorem of Waterhouse um so that um you can um yeah so you can also impose the sheath condition at will without running into set theoretic technicalities um but this accessible PR is not presentable right it's not a presentable C never there AIC yes okay yeah we have we have to prove it yeah yeah yeah it has to be proved so what what makes this work so these are not kind of in some sense non trivial claims the claims made here so what makes this work so the first thing is that this category that we're working with uh is presentable in fact compactly generated uh that's what makes the theory of accessible prees work so it makes that the condition they're commuting with Capa filtered col is the same as being a small cimit of representes um and then there's something about the gro de topology which is that every uh fpqc cover um uh in F is a a filtered is a there exists a Kappa such that um and in this case it's lf1 is a is a Kappa filtered limit of Kappa comp compact uh cap fpqc covers so every uh every flat cover of a ring is a alf1 filtered Co limit of countably presented flat covers of that ring um and this kind of condition having some a priori bound on the basic fpqc covers I cardinality bound is what you can use to um to check this kind of fpqc shic result and um so well so maybe I'll say then so then at least I we haven't explained what our gr de topology is but we' least explained what the category of analytic Rings is so so there's a theorem um so analytic Rings is uh presentable in fact it's Kappa compactly generated for Kappa uh true to the ALF not plus so so so that you Capa small means cardinality less than Continuum um so the uh that's what lets you kind of formally avoid all of these uh set theoretic difficulties involved with taking pre- sheaves and sheaves on a um a big category uh maybe yeah oh no I won't add anything that's fine okay thank you for your attention this category of analytic Rings is naturally ened of a condensed animal right I mean mapping mapping home mapping space is a condens condens animal yes so we could also consider ened seeds instead of usual SE it it seems also natural to me but you take non-enriched we take non-enriched yeah and um it's confusing the it's confusing um what can I say um so in particular this analy the category of analytic uh Stacks is not um no it's not that's right yeah um right different way because also okay but how do you use that to get an enrichment uh I mean yeah well maybe there's some adjoint funter is there I mean you're saying I mean okay it's yeah it's not even clear it's enriched over itself right well I mean I don't know yeah but no I guess I guess you're right you're you're saying you just take your profinite set or whatever and then you cross it with X and map it to y yeah no yeah that's right yeah so there is an sorry thanks yeah so you you could take take so in in analytics tax is enriched over light condensed sets because you can define an s-v valued point to be just a a map like this um but uh it's different from the enrichment there's essentially no relation with the enrichment you had on analytic Rings because we imported these things via kind of very trivial analytic Rings just continuous functions and it um yeah it's all all in all it's a little bit confusing and um yeah so for example I think in some recent Works in in with ptic coefficient systems and like like V vishan was telling me that she wants to work with like sheaves of solid aelan groups on the proall side site and then you know on the protol site you already have profinite sets there so you're somehow you have you're doing condensed in two orthogonal directions at the same time but apparently that's the correct thing for her and it's just it's it's all a little bit confusing but yeah yeah I want to explain how this space be seen as an analytical space um well this by this I just meant the topological space um and maybe I make some assumptions to make sure it's compactly generated otherwi I mean sorry metrizable otherwise I have to modify a little bit what I mean maybe but um yeah just so the same way that uh that this thing was a a topological space and therefore an analytic stack so but this you could think of as providing an analytic stack structure on this a non-trivial analytic stack structure on this this this topological space here yeah um what do you mean by two years Bond what do I mean by by one CU is bounded two is bounded two is bounded ah yeah so what I meant was that the yeah so we have some Spec R and I was looking at the apine line and then so two is a a function on this well if you have any element in the ground ring it gives you a section of the projection and you can ask that that section be away from the the local ring at Infinity so so we had this ring of of uh ring living at the origin this filtered cimit of P along scaling the coordinate by Q then we can put it at infinity and we can ask that this F not meet that Locus at Infinity in the sense that well in the sense that if you that if you take some relative tensor product like R mod f and then that that ring that lives there then you get zero um so um uh right and that if that happens we say that f is bounded or that f is of analytic so um it doesn't yeah it doesn't get too close to Infinity um so you have to ask that condition for the scaler two um in order for some of the claims I made to hold um yeah and for the Gaga you have to ask that condition for all elements in the Basse ring um yeah I feel particularly silly about forgetting that because I mean basically we we already basically wrote up the argument in the previous lectures on complex analytic spaces and that aim was explicitly included there so I just kind of forgot about it um okay other questions maybe at Point you said so you wrote what what was the G structure on Spec R on Spec R yeah uhhuh said this amount to Spec R G GM did it say did I say spec RQ plusus one I meant spec zq plusus one ah okay spec oh I'm sorry I'm sorry I meant Z I meant Z I'm sorry but is it possible to elaborate or maybe or maybe well I mean the point is that if you have a map to that stack then you get to Define such a Ring The Ring of germs at zero um uh which lets you run this machine of of building analytic so yeah but so why you divide by G because you don't really want to specify exactly you don't want to specify Q so for example you know if you have like a a tate ring or whatever an analy T Tate Huber ring or whatever then there exists a topologically no potent unit but you know it doesn't play any role I mean so or at least up to some bounded difference it doesn't play any role so that's exactly what this quotient is doing it's saying you have some say topologically nil potent unit um but it shouldn't it should only matter up to some bounded difference and so you define this GM analytic and then you say that if you have q and you multiply it by something bounded both both away from zero and away from Infinity then that should play the same role in particular it should give rise to the same item poent algebras and the same theory of analytic and so on by the way I don't see you multiply by something about it I mean rather that you can mully by something one or you raise to raise to some power but this gets confus s model is not really antic gen at all uh it is I mean you can have something that's like Q nor itself but if you multip by something bound that's too large then you would actually not have something anymore yeah but we that's true um that's true wait uh there but it's not really I thought I wrote the thing down um but maybe we can just sort I thought I wrote this thing down um just a second ah okay sorry yeah maybe it's not the GM maybe it's the uh so you can also Define the locus where uh the coordinate is bigger than or equal to one or sorry some in some over convergent sense and the locus for T is less than or equal to zero maybe it's the thing that's uh that lies in between there um that's not a yeah that's a group yeah um we will discuss later in the course yeah but now I'm concerned okay anyway okay we'll discuss it later in the course thanks everyone

16. Analytic stacks and 6 functors (Scholze)

https://www.youtube.com/watch?v=BV0-dlAuS3U&list=PLx5f8IelFRgGmu6gmL-Kf_Rl_6Mm7juZO

Unfinished starting from 0:00

So, last time we were starting the discussion of – and we really didn't get into the actual definition that we used, but we were trying to give an overview of what the definition should look like and what kind of examples it should accommodate. And so today, I want to go more into – like, I want to talk about analytic spaces. But something that I think is important is how we set up this definition. It's actually a series of six functions.

Okay, so last time, we saw some motivation for making a definition of – I want to be in some category, just – this definition, modulo some set-theoretic issues that I'll explain in a moment. And then, in that series of – some other things related to sheaves, Hyper-sheaves, and so on. I will probably also say something at the end of this lecture.

So, we would like to define our geometric object by which gives us our basic building blocks, the spaces, and then specify some topology that tells us how we're allowed to glue them together. The key question is, which Grothendieck topology is appropriate to put on this? There was some motivation for this definition, and lots of examples were mentioned that we will be taking up again once we have the proper definition of the category.

Before I get there, let me remark that what we're doing here is kind of very, very close to something we did at the beginning of the course. We defined condensed sets, or "lightly condensed sets," on the category of profinite sets. And there, we were playing a similar game, where we had some basic objects that we started with, and we're building a larger category by allowing ourselves to glue them in a certain way. The gluing that we allow was specified by the Grothendieck topology that we chose. And there, we chose a rather general Grothendieck topology, allowing all, in particular, all surjective maps of profinite sets.

And now, we're repeating this game with a much broader class of basic geometric objects, corresponding to these analytic rings. These are able to model all sorts of different geometries – they can model aspects of just algebraic geometry, they can model some p-adic geometry, some complex geometry, whatever. And all these different kinds of geometry are somehow built, you can put them together in this world of analytic stacks and build spaces out of all of these things together.

So, the key question is, which Grothendieck topology are we going to put on this? Here are some things that we want. The most important variant – if you have a triangle of analytic animated rings, then the primary invariant that we're interested in is its derived category of complete modules. Recall that this was defined as the full subcategory of the derived category of the condensed animated modules, such that all the homology groups are... Complete. Okay, so this is the primary variant, and we want that some a met the v a is a sheet. But this is, it's a drive. The only sensible way to phrase this is that it's an ∞-category. So we actually need to treat this as an ∞-category from now on.

Let's start with something we want. We want this, so this already means that for any analytic stack x, we will be able to define the ∞-category of PD-crystals on x. This is just the limit over all a that belongs to something. That's good, but actually we want something flatly more. This is some business with the six functors.

So we also want x to have a structure of the six functors. Let me say a little bit about what this is. There are six functors: there is always a !-functor, which has some kind of right adjoint, which is an internal Hom; then there is a pullback functor, which has a right adjoint, which is the pushforward; and then there will be two more that we would like to have, you would have to like a !-functor and the right adjoint.

So this structure should exist: each $\mathrm{D}(x)$ should have a symmetric model structure, and then whenever you have a model, you can ask for an internal Hom object, which has the usual relation to the model. If such an internal Hom exists, it's called a closed symmetric structure.

Then whenever you have a morphism f from y to x, there should be a pullback functor, and this should actually be compatible with the tensor product, should be a tensor functor. And this should have a right adjoint, which is the pushforward. This is something we just get automatically because, like when you have a morphism of rings, you can base change modules, and also then the same functor on $\mathrm{D}(x)$ and $\mathrm{D}(y)$ will be compatible with base change.

But then there are these lower shriek functors, not shriek functors, and so this is a more delicate kind of structure which only exists for certain f from y to x. We want the functor that lifts this cohomology, this compactly supported cohomology, and this should satisfy two properties:

One is the base change property, so whenever you have a morphism Y and you have any base change of this, first of all the base change should be again in the !-setting where this works, but secondly, there should be a natural isomorphism between taking first the !-functor and then pulling back, versus first pulling back and then taking the !-functor.

And another thing that it should satisfy is a projection formula isomorphism, that for a in $\mathrm{D}(x)$ and b in $\mathrm{D}(y)$, you're trying to say how these lower shriek functors interact with the existing structure, and so the first thing is that with a pullback they should just commute in this sense, and then with a tensor product, you have the following property that when you have a and b, then taking the !-functor of the tensor product is the same thing as the tensor product of the !-functors.

This is the kind of structure that arises in a lot of different contexts in mathematics. The most classical, in some sense, is if you take some nice topological space, something locally compact, then this has a structure for morphisms where this is literally the compactly supported cohomology. And then you can derive that f what with setting.

Actually, the first time this was developed, not for this case, but it was for D-modules on schemes, I think, but then it was realized that there's, I don't know, you can also do this for D-modules, and you can also, there are lots of different settings where you can do this. One setting where it was however not so much developed is the setting of varieties, now some kind of coherent settings or quasi-coherent settings, where usually you don't really have a notion of compactly supported cohomology. There is this appendix of Deligne's "Residues and duality" where he kind of

Don't say "um", but actually, our goal will naturally go through a series of c_c that behaves rather well, and we absolutely want to have it.

Before going on, let me make a remark, because I think Gouvea will complain in a second that there's a completely imprecise definition here of what the six-functor formalism is. When I write this isomorphism here, there is no natural comparison between the two—these are just two random functors. But they should naturally be identified. So you have to supply this isomorphism. But once you start supplying this isomorphism, then you run into trouble, because now I don't know—you can like base change twice, and then there are comparison maps here, comparison maps here, and one for the composite. Of course, you would hope that they commute, but then you start to wonder how many different such things you can write down and which kind of compatibilities you have to enforce.

I think for a while, it was some kind of open question what a really good and minimal way to encode all the data that is present in a six-functor formalism. There has been work by Liu-Zheng where they do this in the world of ∞-categories, and there's been work of Gaitsgory and Rozenblyum where they actually've Centede, although their approaches have different names and so they don't really talk about complexes with compact support at all. In the classical setting, they've set up some kind of notion of what such a six-functor formalism is. That treatment is however ∞-categorical, and so that's difficult. Then, maybe, Lucas Mann really isolated a key structure that you need.

So, yeah, there is a goal. I gave a course about this last winter, so let me simply refer you there. But something I was also personally taking away from this course is that if you're just interested in the six-functor formalism, then this kind of dictates everything, including the growth topology.

Okay, right. So, before starting the discussion in our specific case, let's recall the usual definition of such an ∞-functor. You do this by specifying two collections of morphisms: there's a class of proper morphisms, and here an ∞-functor will actually just be a functor. But if you want to make this definition, you better check that it satisfies the properties that an ∞-functor should satisfy—you need proper base change and the projection formula to hold. This time, the situation is better, because when you take this to be a functor, there's actually always a natural base change transformation. So here, I'm not supplying data, I'm just asking for conditions. When you want to declare a morphism to be proper and an ∞-functor to be an ∞-functor, there's something you can simply check—whether the natural base change transformation is an isomorphism in the case of this morphism f, and similarly for the projection formula.

There's also a class of open immersions, where instead you ask that the right adjoint of f for open immersions is the left adjoint of base change. Again, you need to check that this is a reasonable definition, so you need to check that it satisfies base change and projection formula. But this time, again, if it's a left adjoint, there is a natural comparison map between these two things going in the other direction, and you ask that these things are satisfied.

The general morphisms f that are ∞-stackable are the ones for which this ∞-functor is defined. They're taken to be the composites of an open immersion and a proper morphism. We have open and proper that go far, and the maps that you can choose are the ones which you can somehow compactify, and then you want to declare that the ∞-stack is compactly generated.

The definition of properness comes up with a really big caveat: when you write down this as a definition, it's not really a definition, because it chooses the compactification. In general, there are many possible ways to compactify. So you have to show that this definition is independent of the choices, canonically, because you really want to get a final structure. You don't just need to show that it's unique up to isomorphism, but even the isomorphism is unique up to higher isomorphism, and so on.

Fortunately, there is a theorem that was essentially proved by X, and then slightly streamlined in this formulation by Y. Under really minimalistic assumptions on the classes of proper maps, open embeddings, and some general finite maps (so essentially just what I said, except, for example, you want that a composite of finite maps can still be compactified), the theorem is that Z can always produce such a compactification. The precise theorem is in these lecture notes or in Y's work.

In particular, these assumptions include no condition whatsoever on uniqueness of compactifications. You don't assume something like the two compactifications can be dominated by a third one. The question in the chat is whether we can use this formalism for characteristic classes, index theorems, and so on. Yes, that's the kind of things we hope to do with this.

Now, let's apply these general ideas to our setting. We start with the category or ∞-category C, which is the category of analytic rings, or "affinoid analytic spaces" if you want. We will generally call an object in here the analytic spectrum of some A, but the underline does not mean it's a topological space—it's just a symbol to say we're taking a geometric object and passing through the opposite here.

We need to figure out which morphisms here should be proper, which should be open embeddings, and at this point we should forget any preconceived notion of what a proper morphism in algebraic geometry is, and really just look at what the formalism tells us. It turns out that a morphism of analytic rings from B to A is proper. This notion of properness is different from the usual one you use in algebraic geometry.

Context: He was talking about a pair of animated condensing light condensing and the subcategory of the connective derived category satisfying some conditions.

Right, so whenever you have any analytic geometry and just an animated algebra over the underlying animated condensed algebra, then you can always endow this one with an analytic ring structure where completeness is just completeness where you restrict the relation to the condensed one. Whenever we have any map of analytic rings, there's an induced one, and then there is some kind of localization where you're just taking the same ring but then doing a formal completion. This geometrically corresponds to the process of compactifications, although this may or may not be an open embedding.

Here's a proposition: if a proper map of analytic Huber pairs satisfies the projection formula, then it is an isomorphism. The only reason this seems at all sensible is because it matches the notion of properness that applies to formal schemes. In the case of formal schemes, there's always this completion sequence, and if you have a map of formal schemes where the completion upstairs is the smallest possible thing determined by the completion downstairs, this is already proper and satisfies all the good properties that a proper map should satisfy.

The projection formula says that for a proper map of analytic Huber pairs $f : A \to B$, and any M over A and N over B, we have $f_*M \otimes_B N \cong f_*(M \otimes_A f^*N)$. This should be intuitively true, since base-changing M from A to B and then tensoring with N should be the same as tensoring M with the base-change of N. The key is to be careful about the precise meaning of the symbols and check the details.

Actually, the thing must be the same. All right, and so, in general, you just have to do the same argument more carefully. Namely, I mean, in general, you can just take $n = B$; then the left-hand side becomes the base change from M to B in the sense of analytic rings, but the right-hand side is a base change from M from A to B purely in the sense of complete A-module. And so, that these agree is precisely the assertion that it's in very—

So, you get this equation where the T has these meanings, and that they agree that the base change in the sense of analytic is just the tensor of A is precisely saying that this must be induc because for the IND ener structure is precisely how you comp to base change in general when you compute the base change for rings. And first, you do the base change for juices, so you do this, but then afterward, you would still have to make it complete as a B-module. But here, it's saying that you don't actually have to do it.

Okay, was a question? I'm not sure what the question is, like the map from the fine line to a point is not proper in usual algebraic geometry, but if you pass through this word, it's counted as a proper here because it satisfies the projection formula and also satisfies proper base change and so on. I mean, actually, in classical base change for complex geometry, you don't need to ask for proper; you only need to ask for quasi-compact and quasi-separated.

So, Dustin would be able to give a really good solution to this. This is good. So, it's just a class of proper maps, it's on the base stack, and proper base change and the projection formula for proper base change, same base change, or it tells you that after any base change, the projection formula still holds. But also, proper base change—he would be able to check if you haven't induced any the structure, then you can just unravel what all the symbols mean, and it comes out right.

All right, so let's get to the class of open immersions, and again, I ask you to forget any preconceived notion of what an open immersion is. I'll give examples in a second. Let me actually call it $\mathcal{J}$, just for psychological comfort. Merge, if the P map B of modules $\mathcal{A}$-dit, the left three, set the projection from—

So, let me give a first example and then a non-example. A non-example is any open immersion in algebraic geometry that's not also a closed immersion, open-closed immersions. Okay, there, I don't know, they're kind of stupid, but like, if you take $\mathbb{G}_m \times \mathbf{Z}$ inside of some $\mathbf{Z}[t]$ with like, trivial ring structure, this is not an open immersion because usually, in algebraic geometry, taking the tensor of modules virtually never commutes with any infinite products, right? If you want left joint, it should mean that the pullback should commute with products.

But here's a key example: if you take the Spec of $\mathbf{Z}[p^{-1}]$ solid, this will be an example, or also sometimes more primitive because this one can be realized as the base change of this one, not quite, but essentially. The key thing to know is that if you take these joint solid and advance this into the induced one solid, this is open. This is something that already came up in

In terms of this, from this funny object, this thing should receive a map from M. Indeed, this complex it gives us a homological degree in $\mathbf{Z}$. This complex, which on cohomology gives us this junction, I think if you look back at Dustin's lecture on the certification of a $\mathbf{Z}$-joint, this is the formula he gave. This means that actually, if you tensor with this object, then this would become the left joint.

It is $\mathfrak{m}$-injective in this one $\mathfrak{S}$, and here this means the comp. And so this means that this left joint exists on the image of Jappa, but Jasta is essentially surjective. This also says that this left joint is given by tensoring some module, which is exactly what you need to check that projection.

Why is this reasonable? Why does this have anything to do with the intuitive idea that these lower $\mathfrak{S}$-functors should be some kind of compact support? In particular, like of the structure sheaf of $\mathbb{X}$, the upper stalk of $\mathfrak{S}$ should be like the compactly supported coherent cohomology in some sense of the affine line.

Well, what should it take to give a compactly supported thing? You should vanish near infinity. Giving a function, that's an element of $\mathbf{Z}[[T]]$, and now you want to say that vanish at infinity. But infinity is going to get functions the $\frac{1}{T}$ series, and so you would like to take functions that vanish near infinity.

The key thing making this work here is that you get some localization with these properties. The key thing you need to check when you want to check that, for example, this is what the completion does, is that $\mathbf{Z}[T^{-1}]$ is an important algebra. It's an object if you tensor it with itself, it's still itself. This is in fact the general description of these open immersions.

Given some X, the open immersion J from some Y into X are equivalent, or anti-equivalent, I'm not sure right now, to idempotent commutative algebra C such that the internal Hom from C preserves some contivity assumption. Here, J maps to the following. You can check $J^\dagger$ of the unit, and this will always map back to the unit, and the cone of that map is always an idempotent algebra.

The projection formula implies that the étale module is really just the stalk of the unit and the rest. This means that this thing is completely determined by the right module. So this means that the right module is actually just given by the internal Hom from the unit. Because the left adjoint tensoring with some object is our Hom from the unit, we know that the completion is really just given like this. This means that, in particular, this new analytic geometry structure is completely determined by this object, and then you just have to specify, or equivalently by this co-map, because then we can recover it by taking the fiber from one to C.

Then you just need to supply the conditions on this so that this completion really determines the analytic R-structure. For this, you need to check two things: that this completion commutes with all co-limits, and that it should preserve connectivity. All the other properties of the analytic R-structure are just some formal procedure to check.

Basically, when you have an open immersion, there is always this item but commutative algebra describing a space at infinity. In the sense of this open immersion, you have this open immersion, and then there's some kind of complementary closed subset determined by some algebra which is functions at infinity. This is an important algebra. The general idea is that whenever you have an open immersion, there is a complementary thing at infinity which is described by some algebra.

As a corollary of this, you can check that the class of étale maps is stable under base change. So we've isolated what the proper open immersions are. This leads us to a stable map. If you factor open and proper, we already know which proper map to take because I already told you about this canonical characterization of any Mori structure and then some kind of localization. This F it always specifies some canonical $\overline{F}$, in this case the compactification is canonical, which is nice.

This J always exists, and it will also always have the property that J does for the factful. The real only condition is that you have the left adjoint, the projection formula. An interesting example here is if you take the solid structure on $\mathbf{Z}$, which is a complexification, but precisely this one over s_0. More generally, if you have any M-finite type algebras and take the solid relative ring structure, then these maps will all be étale, or if you have a map of pairs, then being étale is exactly this condition that Huber calls being of $+$-weakly finite type, which just means that the subring of integral elements A^+ is generated by just finitely many new elements.

Huber was defining these kinds of finite notions also for $\mathbf{p}$-adic spaces, but now they are also the ones where you can define relative étale functoriality for proper open immersions as well. Yes, that's next. There is a little bit to check, like if you take a composite of two étale maps, that's true, basically just by composition on the base change. And then for the $\mathbf{s}$-functor, there's one thing you have to check, which is some interaction between the lower and the lower star functor for open immersions and proper maps. Again, that's a very straightforward check.

I didn't discuss in this lecture all the little X's you have to put to make this work. There are some little bits to check, but each of them is a really simple check. As a proposition, basically a corollary to this general construction of success, some the fall re, you can say that this data, like of proper maps, open maps, satisfies all the required conditions to get enough étale cohomology on our category C, which I recall.

Now, a general question I had: Given a functor $f : X \to \mathcal{C}$ from some category $\mathcal{C}$, one may want to pass to a larger category of geometric objects built by gluing objects in $\mathcal{C}$, just like schemes are built from affine schemes. This was maybe the original question that Lurie and Joyal were interested in when they wanted to extend from schemes to ∞-stacks or higher ∞-stacks.

I proved some general results about how one can go about extending a functor from one category to another. This was also used by Lukas Main, who started rephrasing it, and when I got to Scholze, I again slightly rephrased what they did. In my notes on six functors, I tried to analyze this question and pin down the best topology for such extensions.

I didn't completely settle on a very precise combination of this topology, as it's slightly ugly at one point. But the takeaway is that the covers should basically be those that satisfy "universal star descent" and "universal codescent." I'll explain what I mean by this in a moment.

Let me just state a theorem that in the case we're in, you should ask for universal star and universal codescent, which seems like a lot to check. But actually, you need to check much less—to some extent, it's always any six functor, but I think it seems better here.

Even stronger, it satisfies to the end. So, if you're not thinking about—usually we have a ring and you think about the category of modules, but now we can go one categoric level higher and think about presentable stable ∞-categories linear over $\mathcal{D}(A)$, and it turns out that asking for Schröder descent is equivalent to asking for descent at a 2-categorical level, which is actually how the proof is obtained, but this is probably not one I want to do now in the last five minutes.

Let me just end by giving the definition of the gr topology. That was the first step to fall on. Well, generally, on the one hand, just by finite just unions. So, whenever you have a gr topology, it's just a union of several, then it's covered by those. This includes the empty cover of the empty set.

And these satisfy some compatibilities, $\mathcal{F}$, so this is a rather general class of things. It's actually very, very close to related to this notion of descendability that we defined, which is some nice notion of descent that's satisfied for virtually all faithfully flat maps, at least all those that are countably presented, which will be another motivation actually for us to at one point switch to the ℓ_{tx} setting, because there, things are countably generated.

Particularly, if we restrict to proper maps, then this condition of Schröder descent is precisely the condition that the map of algebras descends. Yeah, have that, and then, okay, they, like in the beginning of my lecture, I said that there are two small wrinkles—one about set-theoretic issues, so again, we should look at accessible things, and for this, we should check that this gr topology has some approximation properties, which means that specification of something accessible stays acceptable.

The other, some hypercompleteness issues, is that we actually don't want the thing that's just sheaves on this, because also in the ℓ_{tx} setting, we're actually considering hypersheaves. So, we actually want to allow a certain class of hypersheaves, but it's the same ideas that go into the classical notion of hypersheaves.

Okay, basically, ℓ_{tx} will be hypersheaves on $\mathcal{F}$, $\ell_{\mathrm{tx}}^{\mathrm{fin}}$. All right, let me take a question. What do you mean by finite unions? Well, I mean you can have like $\mathcal{F}$-ℓ_{tx} objects, they have finite unions. I mean, like rings and finite products, and so whenever I take such an $\mathcal{F}$-finite union of $\mathcal{F}$-ℓ_{tx} objects, it should stay in $\ell_{\mathrm{tx}}^{\mathrm{fin}}$, which is like it's covered by the individual ones.

Right, no, just this is a cover whenever I is a finite set and x_i are in $\mathcal{F}$, then this is also an

It's an extremely wonderful structure that Jacob Lurie defined, this category of presentable Infinity categories with colimit-preserving functions. In there, you have a subcategory of stable ones, or also, whenever you work over some ring, you have some categories which are linear over some base, e.g. the derived category of a ring. These are, loosely speaking, DG categories, where the functions are colimit-preserving. This has a well, maybe a tangent structure.

Now you can also ask whether the association that takes such an algebra or the spectrum of it to this Infinity one or Infinity two category, it doesn't matter which one you choose for this purpose of asking for descent, whether this satisfies descent. By modular categories, I mean objects in this same Jacob Lurie's formalism.

For example, the derived category of something over a ring A is in this category, as is the derived category of an A-algebra. If you glue such things locally, then you are given the descent data. And why is it Infinity? The hom between two such things is viewed as an Infinity one category, actually, because you can't forget about the non-invertible 2-morphisms, and then you get an Infinity one category.

For the question of descent, it turns out that the 2-categorical structure of this thing doesn't matter much, and the non-invertible 2-morphisms will automatically satisfy the descent condition. For these questions, I would really recommend reading the paper of Artem Yurievich Belorussov.

17. !-descent continued (Clausen)

https://www.youtube.com/watch?v=rN_iM7Z8vdE&list=PLx5f8IelFRgGmu6gmL-Kf_Rl_6Mm7juZO

Unfinished starting from 0:00

Okay, let's get started. I will continue the discussion that Peter started last time on !-descent.

Let me recall the setup. We have analytic rings, and we are going to build some geometric objects on them, somewhat in the model of scheme theory. The first thing we do is define what the affine things are - we formally define them to be the opposite category of analytic rings. An object in this category, let's call it X, corresponds to an analytic ring, which we can denote as $\mathcal{O}_X, \mathcal{D}_X$. An analytic ring consists of an animated condensed ring and a certain full subcategory of modules over that, in the full derived category of modules over that ring.

I will denote the first coordinate as $\mathcal{O}$ of X, and the second coordinate as $\mathcal{D}$ of X in this notation. Here, X is really just a formal symbol.

In the derived context, there is a way to define the full derived category. For some purposes, it's easier to just restrict to the non-negative part, as the two formalisms are equivalent - you can go back and forth between them. But for the discussion today, it's actually much better to consider the full derived category as the primary object. The reason has to do with a descent phenomenon.

It turns out that the full derived category will have this !-descent property, but the non-negative part will not. In other words, for an analytic ring, we have a nice t-structure on this category, but for a general analytic stack, there won't be a t-structure on the derived category of the global object, which is obtained by gluing these local derived categories.

For an analytic ring, the category $\mathcal{D}_X$ is by definition a full subcategory of the derived category of $\mathcal{O}_X$-modules. This has a natural t-structure, and the claim is that this t-structure is detected all the way down in the condensed Abelian groups, and it induces a t-structure here as well.

We singled out the notion of a śhablem̌ap, which means it can be factored as an open immersion followed by a proper map. For a proper map $f : X' \to X$, this means that an element m in $\mathcal{D}_{X'}$ lies in $\mathcal{D}_X$ if and only if its image in $\mathcal{D}_Y$ lies in $\mathcal{D}_Y$, where Y is the target of the map. In other words, you just take the class of complete modules on the analytic ring X' and inherit the notion of completeness up to X.

We claimed that the composition of such maps is also śhable; but I forgot the details now.

Peter claimed it last time, I believe. But he didn't give the argument. It's quite straightforward, though.

Let me continue the discussion. The open immersion means that the functor $J_!$ always exists. This should be a localization, and the kernel should be just modules over some idempotent algebra. It will necessarily be a compact object because, by the definition of a map of analytic rings, the right adjoint of this commutes with colimits.

This is just in the pure category theory sense. There is a notion of localizing by a multiplicative set in usual categories. But these are presentable categories, and the good definition is what Peter said: the right adjoint should be a fully faithful functor.

We already know, as part of the discussion of analytic rings, that the right adjoint exists. So this implies that there is a left adjoint.

Let me continue the discussion. We had a claim or a theorem. The remark is that for a proper map π, the right adjoint π_* is nice: it commutes with colimits, satisfies the projection formula (i.e., it's $\mathcal{D}(Y)$-linear), and commutes with base change.

For an open immersion J, there exists a left adjoint $J^!$ or $J^\natural$, which has similar nice properties.

The theorem discussed last time was that there exists a six-functor formalism on $\mathcal{D}$ such that the class of "shabable" maps $f : X \to Y$ is characterized by having the property that for f proper, $f_! = f_*$, and for J an open immersion, $J^!$ is the left adjoint.

This class of shabable maps has good closure properties: it is closed under composition and base change.

If $F_1 : X_1 \to Y$, $F_2 : X_2 \to Y$, ..., $F_n : X_n \to Y$ are maps with the same target, then f_i is shtuka-able for all i if and only if the map F from the disjoint union over i of X_i to Y is shtuka-able.

And these disjoint unions in this category, those finite disjoint unions in this category, they correspond to finite products in this category of analytic rings, and they're kind of just naively defined coordinate-wise.

So for finite finite coproducts, maybe I'll do just a reminder of some example of this six functor formalism. Let's take Y to be Spec of the solid $\mathbf{Z}$-theory, and let's take X to be Spec of the solid $\mathbf{Z}[T]$-theory, with the natural map from X to Y. Then we can take X' to be Spec of the solid $\mathbf{Z}[T, T^{-1}]$-theory.

So this is a proper map, and this is an open immersion. And the complementary algebra is equal to this $\mathbf{Z}[[T]]^{-1}$. And then, for example, $j_!(\mathcal{O}_X)$ is this two-term complex. And then that's also the formula for $\pi_!\pi_*$, which is just the forgetful functor, forgetting that you have a module structure over $\mathbf{Z}[T]$. Intuitively speaking, this is functions on the affine line, and this is functions localized near infinity, localized near the missing point. And so this is like functions on the affine line which vanish near infinity, so to speak, because you're taking a fiber. So that's kind of compactly supported cohomology of the structure sheaf on the affine line, so to speak.

And this is what six functor formalisms are supposed to be doing in general: they're supposed to be specifying some notion of compactly supported cohomology, relative compactly supported cohomology, which behaves well in families. The key property of this $j_!$ is that it commutes with base change in complete generality.

So the algebraic objects are proper, even if you're dealing with something like the affine line, which is not proper in traditional algebraic geometry, but it's kind of compensated by the existence of this solid theory, where you have a new version of the affine line, the solid affine line, which is not proper anymore.

Okay, so a shtuka-able map satisfies Čech descent. If you take $\mathcal{D}_Y$, $\mathcal{D}_X$, and $\mathcal{D}_{X\times_Y X}$, and then continue like this, using the $f_!$ functors to define this diagram, this induces the $\check{\mathcal{C}}$ limit of the $\mathcal{D}_X$ to the $\mathcal{D}_{X/Y}$. The naive thing would be to consider the star descent, you This condition, what about the composition of Shri? Does it involve interchanging proper and open, or is it quite straightforward? The essential reason it's straightforward is that there's a canonical candidate for the X' in the factorization. Your X contains this algebra $\mathcal{O}_X$, the structure sheaf, and then you can just take X' to be $\mathcal{O}_X$ and then $\mathcal{D}(X')$ should just be the full subcategory of $\mathcal{D}(\mathcal{O}_X)$ consisting of things whose image in $\mathcal{D}(\mathcal{O}_Y)$ lands in $\mathcal{D}(Y)$.

So you have an open immersion and then a proper map, and then you claim that this, in étale spaces, the... Okay, so you use the other... Yes, okay, and you check that this is canonical.

The second goal for today is to be able to give examples of Čech covers, so that you can then apply these descent results. I don't remember if it was defined in the previous talk I saw, but what the six functor formalism is exactly. I think he referred to Lurie, so I'm going to actually discuss it today, because the precise way it's encoded will help give some arguments. So it's in fact the next topic that I'm going to turn to.

The existence of this six functor formalism on $\mathbf{S}$ follows rather immediately from work of Lurie, as reinterpreted by Gaitsgory and Rozenblyum. Following Gaitsgory, Rozenblyum, and Lurie, Gaitsgory encodes the six functor formalism in terms of span categories. Let me now set this up.

Suppose we are given an ∞-category C with all pullbacks, and a class of maps S in C stable under composition and pullback. Then we get a span category $\mathrm{Span}_{C,S}$. The idea is that this is going to encode the six functor formalism, where you have shriek maps defined for maps that lie in S, and you have star maps like upper star defined for any map whatsoever.

This has objects the same as in C, but maps $X \to Y$ given by diagrams $X \leftarrow M \to Y$, where the right-hand map lies in S. The composition is given by pullback. A two-functor formalism on C with respect to S is just a functor from $\mathrm{Span}_{C,S}$ to some category of categories.

Suppose you are given a functor $F_!$ from the derived category $\mathcal{D}(X)$ to the derived category $\mathcal{D}(Y)$. Here, this functor $F_!$ is giving you a functor F^* from $\mathcal{D}(X)$ to $\mathcal{D}(Y)$. These functors are stable under composition and include having the identity.

Yes, it does. This is what happens when you compose an empty set's worth of composable maps. If you have a pair of composable maps, the functoriality amounts to the base change formula for the $F_!$ functors. The fact that $F_!$ commutes with F^* when you have a Cartesian square in your category.

So, a two-functor formalism versus a six-functor formalism, but two is the essence of six in this case. The other functors are: a base change formula, compatibility of F^* under composition, and it also encodes compatibilities between the base change formulas and the compositions that you have on these things, so there's higher-order data implicit in this.

Provided this admits a right adjoint, then you get the right adjoint as well. And the remaining functors are some tensor product operation you have defined on each of these, and some adjoint to it, some internal Hom. If you have this and it satisfies a certain property, then it has some adjoint.

To encode this tensor product and the expected interaction of it with the functors here, namely projection formulas, we use a symmetric monoidal structure on this span category, induced by the Cartesian product in C. We need to assume that C has a terminal object and pullbacks, so that we have products.

This symmetric monoidal structure on the span category is not the Cartesian product in the category anymore, it's just some symmetric monoidal structure. We then request that your functor $\mathcal{D}$ from this span category be lax symmetric monoidal with respect to that tensor product we just defined and the symmetric structure here given by the Cartesian product of categories.

The basic data is that you have $\mathcal{D}(X) \otimes \mathcal{D}(Y)$ should map to $\mathcal{D}(X \times Y)$, plus some compatibilities which are conveniently encoded in this being a lax symmetric monoidal functor. In particular, $\mathcal{D}(X) \to \mathcal{D}(X)$ using the diagonal, and then you can probably reconstruct this by pulling back over Y. But then one can wonder why you need the product in the category—it's not enough to specify $\mathcal{D}(X) \to \mathcal{D}(X)$ for every X in some coherent way, you need to encode the compatibility with the $F_!$ functors somehow.

Object, oh boy, yeah. I'm sure it comes for free from, so it lacks a unit. I mean, there should be some unit.

Yeah, yeah. Okay, so I have to advance this story a bit. I said that if you just have these three functors satisfying certain properties, then you automatically get all six functors. And there's a very convenient way to organize the passage from 3 to 6, and that's a bit of higher categorical magic defined by Lurie. This is Lurie's magic category called PRL.

I have to say a little bit about this and how it works. Here, the objects are the presentable ∞-categories. "Presentable" means you have all small colimits and you're in some sense controlled by a small subcategory, in the sense that there's a small subcategory such that the whole thing is gotten by formally adjoining some sufficiently filtered colimits. So you should be κ-compactly generated for some κ. The morphisms are the colimit-preserving functors, and that's equivalent to admitting a right adjoint.

So let me tell you a bit more about this magic category. PRL has all small limits, and the forgetful functor PRL to the ∞-category of categories preserves them. That's nice–limits in this category exist and are completely naive. But also, PRL has all small colimits, and this is the really remarkable thing: you can also access these colimits in a completely naive way. There is a constant version of the forgetful functor where you take a category C and send it to C, but then you take a functor from C to D and send it to its right adjoint. You can make this into an honest functor, and this functor preserves colimits, which translates to limits in the ∞-category of categories.

So also the colimits in Lurie's magic category are just calculated as naive limits of the underlying categories, but with respect to passing to the right adjoints of the functors in your diagram. This is quite magical, because in the ∞-category of categories, colimits can be very difficult to calculate. But this makes it easy.

In particular, the condition of shriek descent for maps of fiberable maps of abelian groups is actually the same thing as cocartesian descent for the lower shriek functors in PRL, which is a much more convenient way of thinking about it.

There's more magic in PRL–there's also a symmetric monoidal structure, which is maybe the main theorem in Lurie's book. It's a tensor product characterized by a universal property, just like you would expect of a tensor product. Maps in PRL from C tensor D to E are the same thing as functors from the product that commute with colimits in each variable separately. And this tensor product on PRL also commutes with colimits in each variable separately, so you get a very nicely behaved tensor product on this category.

In fact, the internal Hom from C to D is just the ∞-category of colimit-preserving functors from C to D.

Of course, we only remember isomorphisms. So you can somehow recover the full mapping category by using this tensor structure. That's one way of recovering it at least.

In principle, PRL should be considered as an Infinity 2-category because there's a whole category of maps between two objects. But you don't really need to remember the Infinity 2-categorical structure because it's just the internal equivalence or isomorphism in this theory.

There was a reference to a paper on Infinity 2 in some places. So do they define Infinity categories and all of this in general? I haven't kept up with that literature; so far, I've been fine with just Infinity 1.

Okay, I want to continue a little bit more. If you have a PRL, it is now a tensor category, a category with a symmetric monoidal structure. You can ask what is a commutative algebra in this tensor category, meaning an object equipped with some symmetric multiplication and higher coherences about it being sufficiently commutative. What this means is that C is a symmetric monoidal presentable Infinity category, and the tensor product on C commutes with colimits in each variable. Some people call this a presentably symmetric monoidal Infinity category.

A basic example for us would be D of X for an algebraic variety X. You can then consider modules over C in PRL, which are certain presentable Infinity categories tensored over C. This allows you to say they are enriched over C in some sense. You have a C-internal hom from M to N, which is characterized by a universal property relating maps from C tensor M to N.

The category of modules over an algebra R1 tensor D R2 should be the same as the D of the tensor product, unless you're in characteristic 0. In that case, you get something bigger. But let me continue the story a bit more.

Mod C PRL also has all limits and colimits, and the forgetful functor Mod C PRL to PRL preserves them. It also has a tensor product which is a tensor product over C, a standard relative tensor product.

If you have C in PRL and then commutative algebra objects R and S in C, you can consider left modules over R or S, which will be in Mod C PRL. Then Mod R C tensor over C Mod S C is just Mod R tensor S C.

Course, you say, "C" tends to him, but of course this comes with, yeah, there's more coherencies and there is a good way to formulate this system of coherencies. In this, we have to read some higher algebra. You have to read higher algebra, but you know, yeah, you, yeah, it's not easy to read higher algebra, I know, but it's possible. People have done it, and someone even wrote it, which is even more amazing, yeah.

Okay, so Tser products. Okay, so now I want to connect this to analytic rings. So, note, there's a functor or, well, maybe, so, yeah, so now I can say the good way to encode, or a good way at least to encode a six functor formalism, is a lax symmetric monoidal functor from Span(C's) to PRL with this tensor product here. So, we're no longer using the Cartesian product on Cat(Infinity), but this tensor product on PRL. But it really just amounts to a condition on the kind of formalism we had in the other sense. It's just the condition that when you look at this D of X cross D of Y going to D of X cross Y, that that should commute with colimits in both D and X and D and Y separately.

So, it's just a condition on this formalism here, but it's best to think of it as being a formalism with values in PRL, okay. Now, I want to connect with our specific example. So, note, analytic rings map to PRL by sending our triangle, D of R, to D of R. Well, in fact, as I already said, it maps to C alge(PRL), but, in fact, it maps to C alge(PRL) over a certain other commutative algebra object, namely, the derived category of condensed Abelian groups. So, everything by its nature lives over this derived category of condensed Abelian groups. So, we have a presentably symmetric monoidal category with a functor from this presentably symmetric monoidal category, and then I claim this functor commutes with colimits and detects isomorphisms under, yes, under, thank you. We're in the world of algebra, so I should say "under".

Yeah, we have this category here, and then we have an object in this category, and this notation means that you consider an object in this category together with a map from this object to that object. So, it's this slice category or "under" or "over" category or something. Condensed, every condensed abil, co, gives you, by pullback, some guy in of, okay. I mean, it's just, yeah, I mean, the initial object here, I want to say, it always SS to see over over IM of the addition, ah, okay. That's an analytic ring to what's the initial object, Z. Okay, so for Z, you get D of, condensed, so you don't need them, ah, okay, CLA this F, just, yeah, just a sec, just a sec.

In particular, so if you want to, the derived category of a pushout, so, let's say, a, so this is pushout in analytic rings, which was this kind of slightly subtle operation in this perspective on analytic rings because you had to complete some pre-analytic ring structure and so on, but actually on the level of the categories, it's quite naive. So, it is just this Lurie tensor product, this relative tensor product in PRL. So, the proof is not so difficult. Let me indicate what's going on in the proof, just in this special case, which is really all we need. In the case where A and B are both proper over R, I'm not assuming any Schreier ability, but still, let me use this language of proper maps. In the case when A and B are proper over R, it's just an instance of this general fact here. Did you say that commutative and now got confused, all this? So, somehow you get, you said the modules as limits and colimits, and then did you say that symmetric? Oh, I didn't say that C alge has colimits. I should have maybe discussed it, but it does. And, as usual, pushouts are calculated by relative tensor products. Pushouts in C alge(PRL) are calculated by relative tensor products in PRL. Okay, and it has limits also. Those

You can still factor any map of analytic rings as a proper map followed by a localization. For localizations, it's quite easy to check the universal properties to compare the two sides of this. Granting the case of proper maps, you then just have two different quotients of your category that you're trying to identify, and you can identify them by looking at the descriptions.

For a proper map, the algebra over the bigger ring is the algebra of the source. However, for general analytic maps, the algebra over the localization is not necessarily the algebra of the source. But you can still use this reduction to the proper case. Every map of analytic rings still factors as a proper map followed by a localization, though this localization won't have a left adjoint.

This functor is not fully faithful for a technical reason. To be fully faithful, you'd have to be able to recover the triangle just from the data of the D of R with its tensoring over condensed abelian groups. You can certainly recover the underlying condensed abelian group of the triangle, but there's not quite enough structure here in the animated ring context to recover the full animated ring.

As a corollary, if you take Y in f and consider f-shriek over Y, the category of X mapping to Y via a schable map, then the six functor formalism applies. We can look at the span category of this, but now we don't need to restrict to schable maps anymore, because every map in this category is schable. This span category is a priori lax symmetric monoidal, and in fact, it is symmetric monoidal.

For X to Y schable, the object D of X in modules over D of Y in PRL is dualizable with respect to the relative tensor product over D of Y, and in fact, it is canonically self-dual. With respect to this self-duality, the dual of the pullback map from X to X' over Y should be a map from the dual of X to the dual of the dual of D of X to the dual of D of X'.

So, you just get a map in the other direction from $D(X)$ to $D(X')$. This map is none other than the lower shriek functor. The proof is that you can check this universally in any span category. We have a symmetric monoidal functor, so if you want to, the image of a dualizable object will be dualizable, and if you exhibit a duality pairing here, you get one there and so on.

So, why is every object dualizable in this Span category self-dual? You have a unit, which is a counit, that is the same thing going in the reverse direction. Then you just check the triangle identities; it's completely straightforward. And then you can see that with respect to this self-duality of every object, passing to the dual is just the same thing as transposing the span.

So, we have a proposition: if $f : X \to Y$ is a schematic map, then the following are equivalent. Peter said this last time but didn't give the proof, and now we're going to give the proof. One is that f satisfies shriek descent. Two is that for all M in $D(Y)$, you have descent for M. In other words, what Peter equivalent—oh, yes, yes, yes. By definition, shriek descent means that you have descent with respect to this weird pullback, these real weird shriek pullback functors on the D's. But the conclusion here is that you get it also automatically for the star pullback instead.

And then the third condition is kind of a categorified version of star descent, studied by Grothendieck and Rosenberg, which is a remarkable thing. If you take this whole category, then that assignment, assigning to Y this category, also satisfies descent in the sense of ∞-1, but if you have—I'll explain more or less why it's the same, requiring this for ∞-1 and for ∞-2, but I'm not going to try to say I know what an ∞-2 means.

So, do you have also a shriek version of two? Yes, you have a shriek version of two, but that's completely formal from one. Let's get into the proof, and then it'll become more clear. Or maybe let me state a corollary first. A corollary is that a pullback f satisfies shriek descent, implies any pullback also does. You can see that from two and using this symmetric monoidal that we discussed, but also, I think in the course of the proof, a simpler explanation will arise for why shriek descent is closed under pullbacks, which is important for using this to define a Grothendieck topology in the first place.

Okay, why the P? Because the D of the—yeah, because the pullback in analytic rings is already calculated by a relative tensor product. So, if you took, for example, M to be $D(Y')$, then this would give you star descent for the pullback, but you could take M more generally to be any $D(Y')$-module. And then you see that condition two is stable under pullback, under base change.

So, we have star shriek descent, but I explained that the best way to think of that is that you have this colimit in PRL, but I also explained that colimits in module categories are the same as on the underlying—so that's the same thing as modules over

Now, let's take the internal Hom out to M for any M in $\mathrm{Mod}_D(Y)$. Then, on the right-hand side, we're taking the internal Hom from the unit to M, so we just get M, and it's being identified isomorphically with some limit. So, the colimit pulls out of the internal Hom limit of the internal Hom in $D(Y)$ with M.

But I then explained over here that this thing is self-dual, it's dualizable, and in fact, self-dual. So then, you can actually move it over here with a tensor product. So, this is the internal Hom in $D(Y)$ with M, and I said that the manner in which it's self-dual is such that it converts shriek functors into lower shriek functors into upper star functors. So, then this is exactly a proof that one is equivalent to two.

Of course, you have to verify that the construction you get is really the expected one in some higher sense. You have to produce the data making this coherent identification of the dual of this self-duality and the dual of this, identifying with this, universally in the span category, and then map it into modules over $D(Y)$. I haven't actually done that, but there's also another independent argument for this which doesn't require such things. I just thought I would present this because it feels the clearest to me, even if it's slightly difficult to make technically work.

Okay, and then two implies three. We have to check this statement here. The map has a right adjoint, which is basically you take a system of modules here and you forget them all to $D(Y)$, and then you take the limit in this category here. So, it's like taking a system of modules M_n and then you take the limit of the M_n. And you want to check that the unit and the counit of this adjunction are isomorphisms.

For the counit being an isomorphism, it's true after you base change up to X. This is because $D(X)$ is dualizable over $D(Y)$, which implies that you can pull the limit out. And then, the covert is split on pullback to $D(Y)$ or $D(X)$, and there's an obvious base change property.

We are completely okay then. Let's see, because it's dualizable, any limit commutes with it. But this is actually not correct from the start. Oh, I should change the whole system from D of Y to D of X, yeah, that's good. So then, thanks.

After base change to D of X, in other words, you take this whole collection here and you base change to D of X, the limit can commute. So you can do it in the system after tensoring, and after tensoring you get the system of the Adic categories for the fiber product space, a fine space, so to speak. Because we said that the D of the fiber product is D of the tensor product of the D for fine things, yes or no?

Okay, and then the covering is split, and so by the usual argument, this means that you can play with some Amitsur cohomology. Okay, then what is point three, the base change property? Oh, that's so that when you tensor the thing up to D of X, you can identify that system with the corresponding system for the pullback of the cover to D of X. So it's purely formal.

Well, actually, I think this argument is in Kirill Mathews' paper, and maybe it's also in Gates-Goresky-Rosen-Bloom, and so on. I mean, which you referred to as the original reference for descend ability, it's like the reference I wrote down last time.

Okay, so then this collection, and then the image of this, and then this, those two things are the same, isomorphic, after you tensor to D of X. But then two also implies that tensoring D of X detects isomorphisms. Done. Okay, it's a bit of magic, but you also get now a strong form of three with the two infinity.

Let me quickly explain the argument for three implies two, which is kind of more or less explaining why you get a strong form of three. If you take mapping spaces, if you have a what's that, Peter said. Right, you're right, that this argument shows that two is exactly the claim that the unit is an isomorphism. So clearly, three implies two, but I was going to give a different argument which maybe also explains why you get an infinity 2 categorical three implies two, because three means that this functor is an equivalence, but this functor has a right adjoint. So being an equivalence is the same as the unit and the counit being isomorphisms, but when you unwind what it means for the unit to be an isomorphism, you get exactly two. So clearly, three is stronger than two.

The idea is to go from Δ^n to this, and then if you let N vary, you recover the whole well-completed simplicial space associated to this category, just in terms of mapping spaces.

I'm saying that what you get a priori is the space of objects in this ∞-category that you're interested in, but if you then vary the source by tensoring with presheaves on Δ^n, and that tensoring kind of commutes with everything, then you get not just the set of objects, but the set of morphisms, the set of composable pairs of morphisms, the set of, etc., etc.

Yes, I was just giving it a little more explicitly. Δ^n is a finite category, and I have to put it in this big world of Prl, so I just take presheaves of whatever spaces. If I want this to be a tensor over $\mathcal{D}$ of Y, then I should do presheaves with values in $\mathcal{D}$ of Y, and Δ^n is the simpli[cial] finite category or opposite.

Okay, so this was roughly half of what I wanted to do today, so let's go for another two hours, shall we? No, I'm kidding. We're taking a little break. I guess the next lecture is scheduled for the 10th of January, which is a Wednesday, so I'll be picking up and actually finishing what I intended to do today.

This was some consequence of Čech descent. Maybe just mention one more corollary of this, which is that the topology of Čech descent is subcanonical, so the Čech ∞-category fully faithfully embeds into the sheaf category. So those are some consequences of Čech descent—you get some very strong descent results. And next time, I'll talk about how you produce examples of Čech covers.

18. !-topology (Clausen)

https://www.youtube.com/watch?v=vRUmXU8ijIk&list=PLx5f8IelFRgGmu6gmL-Kf_Rl_6Mm7juZO

Unfinished starting from 0:00

Let's begin. So I'll be talking more about this Zariski topology, but it's been a little while since we've last met, and so maybe I'll start with a recap.

Recall—well, let me start at the very beginning. We have this category of analytic rings. The objects in here are pairs—there are different choices about exactly what sort of data you want to put in the second piece. I'll take the sort of full derived category instead of some connective derived category or some abelian category, where this is a condensed—and in general, we want to say animated ring, so we're allowed to have some derived phenomena. This is a certain full subcategory of what you could call D of R-triangle, which are triangle modules in derived condensed abelian groups, satisfying certain nice closure axioms for this, which I won't recall right now.

And then we define the category of affinoid spaces to just be the opposite category. And we singled out two—well, three, I guess—classes of morphisms of affinoid spaces.

So f from X to Y in $\mathcal{F}$ is proper if the pullback map from $\mathcal{O}_Y$ to $\mathcal{O}_X$ has a good right adjoint. Good means that the right adjoint commutes with pullbacks and also with colimits, and satisfies the projection formula.

The maps called open immersions are those where the pullback map $\mathcal{O}_Y \to \mathcal{O}_X$ is a localization and has a good left adjoint, again in the same sense as before. For such an open immersion, there will always be some idempotent algebra in D of Y which is somehow the "functions on the closed complement" and determines this situation.

Called shable if it factors as $X \xrightarrow{f} Y$, where f is proper and $Y \xrightarrow{g}$ is an open immersion. There are good closure properties for all three classes of maps: they are closed under composition, closed under base change, and contain all isomorphisms. Furthermore, if you have a map like this and a map like this, and they're both shable, then any map between them making the triangle commute is also shable.

We then get a six-functor formalism on $\mathcal{F}$, where the class of maps for which the important functor, the lower shriek functor, is defined is exactly the class of shable maps. The lower shriek functor $f_!$ is f_* for f proper, and $f_!$ is the left adjoint to f^* if f is an open immersion. This can be obtained by passing to the diagonal, as the closure properties imply that the classes are closed under passage to diagonals.

We then had a definition: a shable map $f : X \to Y$ in $\mathcal{F}$ is a shriek cover if the map from $\mathcal{D}(Y)$ to the limit of the Čech nerve is an equivalence, where we use the upper shriek functor for the transition functors. We then had a result that for shable f, the following are equivalent: (1) f is a shriek cover, and (2) we have a Čech-type equivalence, where we use the lower shriek functors and take the colimit in the category PRL (presentable ∞-categories with colimit-preserving functors) or in modules over $\mathcal{D}(Y)_{\mathrm{PRL}}$. Here, $\mathcal{D}(Y)$ is a presentably symmetric monoidal category, so the tensor product commutes with colimits.

Colimits in each variable make it a commutative algebra object in PRL with respect to L's tensor product. Then you consider modules, so it's a presentable ∞-category which is tensored over $\mathcal{D}(Y)$ in a kind of colimit-preserving way.

The third condition was the "shriek cover" condition. You have this "shriek descent," where you use this funny twisted pullback. But it turns out that it implies that you get descent with the star pullback, and in fact it implies you get the same result with coefficients for all M in $\mathrm{Mod}_{\mathcal{D}(Y)}(PRL)$.

If you take M to be $\mathcal{D}(Y)$ itself, then you see that this condition is just the usual descent, that is, descent with respect to pullbacks. But in fact, you can even tensor that with any module, and you still have descent.

The fourth condition was this kind of "two-descent," this categorified version, which says that the whole category of possible M's satisfies descent. Here, the only thing that makes sense is star descent, but I'll emphasize it's just some naive pullback, where the base change functors are just relative tensor products in PRL.

One can ask, instead of just the fpqc topology in algebraic geometry, using quasi-compact, whether you have descent for an arbitrary collection of maps, not necessarily finite. We think that is true, that you could try to make an analogous infinitary version of the Grothendieck topology, and it will end up being finitary anyway. Peter checked this carefully, and the answer is yes - every cover will have a finite subcover.

In the case of just finite sets, if you have the conerter set and take all morphisms from **P** to the conerter set, which is big, this is universally submersive. This means that when you cross with any topological space, to check if something is open, you can check the pullback.

Of course, you can ask whether you have the right descent properties, which is not so clear. But at least for opens, it seems to work. Peter discussed this with some logicians, and they had some conclusions. It can happen that you have some profinite set and some infinitary cover by other profinite sets which satisfies the descent, at least in some examples, using the perspective of the derived "shriek" descent condition. Equivalently, along the "lower shriek," you can show that it is finitary, but it's really something that only works when you ask for "star descent," not for "shriek descent."

So those are the equivalent conditions for being a "shriek cover." Now, I want to talk about how you can check these conditions. That image doesn't have to have any closure properties whatsoever. It doesn't have to be a stable subcategory or anything like that. But then you can take the thick subcategory generated by it. This means a closure under just finitary operations like cones, retracts, and shifts.

So, you get a priori a bigger category. You can ask whether it's the whole thing, or whether it just contains the unit. Let me write 1_Y for what you would normally think of as the structure sheaf of Y, the unit object in this symmetric monoidal category with respect to its tensor product.

The condition again is that this unit object can be written in a finitary manner in terms of things which are lower shriek from $\mathcal{D}(\mathcal{X})$. Can it be written in an infinitary manner? Yes, because it's a sheaf, it follows.

Oh, you're saying that if I say this condition but with infinitary, does it automatically imply the same condition but with finitary? I don't know what the relation between those is. I think the key point is that in the finitary case, this is a compact object in $\mathcal{D}(Y)$. So I think indeed they should be equivalent.

Maybe it's even better to say that if it's a proper, universally locally acyclic map, then you satisfy this condition. And the converse holds, provided either f is proper, or f is universally locally acyclic. Let me explain what that means: it means that $f_!$ is "good", which means it commutes with pullbacks, colimits, and satisfies the projection formula.

Those properties of f are not true in general, and they're not equivalent - there are cases where some hold and others don't. There may be some non-trivial implications, but the projection formula clearly implies that $f_!$ commutes with colimits, so I didn't need to write that separately.

The main claim is that the "universally locally acyclic" condition is kind of like an equisingularity property for $f : X \to Y$, where the dualizing object is compatible with pullback. But the converse fails in general. Let me give a counterexample...

Category. So let's call this mapping by J, and this mapping by I. This induces a map from the disjoint union. Then J lower shriek of the unit is this compactly supported thing, which is the fiber of the homotopy fiber of ZT going to the series T-inverse. But I lower star, well I lower shriek of the unit there, I is proper, it's a closed immersion, so this thing just gives a Zariski series T-inverse. It's clear that you can build Z of T from this fiber and this guy by just one cofiber sequence.

Then the lower shriek from X the disjoint union will just be given by restricting to Z. Take the lower shriek there and restrict to Z, take the lower star there, and then take the direct sum of those two objects. So you'll get this guy direct sum this guy. Therefore, by closure under retracts, if you allow closure on retracts, you get each of them, and then you get ZT individually.

So it is not a sheet cover, because for the cover you just get Y and Z separately, somehow you get the derived of Y cross the derived of Z, not the derived of Y. Okay, here you can have some X or something that is not the same in Y, and then you lose some Z.

So this condition is something like just set-theoretic surjectivity on underlying sets, or maybe it's like a cover in some constructible topology or something. And then if you want to go from that to some honest descent, you need to assume some properties. This is analogous to, in topological spaces, there's descent for open covers, but there's also descent for finite closed covers or proper descent, but you can't mix open and closed and still expect to get descent.

So this is some sort of set-theoretic cover condition, and then if you assume either that you're some generalization of open, which is this étale, or some generalization of closed, which is this proper, then you get honest descent. But you can't mix them.

So the proof - let's make explicit what this shriek descent according to the definition means. We're using these upper shriek functors, and there's some standard category theory which tells you that this comparison functor itself will then also have a left adjoint, which is given by taking a colimit in this category D of the G lower shrieks of them. So in particular for fully faithfulness, this amounts to the claim that if you take this colimit, it should be an isomorphism in D of Y for all M in D of Y.

If you take M to be the unit in Y, this is a geometric realization, it's a colimit over this Delta op. You can filter that, you can always write this as a colimit over the natural numbers and then a partial totalization. And this is a finite colimit, that's a nice fact about the simplex category. But then, since the unit object is compact, if you write it as a filtered colimit of something...

Let's look at sets of cardinality less than or equal to D. Since the unit interval Y is compact, we can deduce that it is a retract of some partial totalization, where each of these lies in the image of the shriek functor from D of X to D of Y. All of these structure maps from these iterated fiber products factor through D of Y. This is equivalent to a finite colimit, a colimit over a finite simplicial set.

In the stable setting, the difference between this for D and this for D-1 is just given by one of these objects up to a shift. The successive cofibres are described in terms of these individual objects. So in the stable setting, some kind of rewriting simplifies all of this.

A finite colimit is defined in a higher topos. This is not more general than a colimit over a usual category, but you have to be careful, as a finite category in the usual sense might not be finite when considered as an infinity category or a simplicial set.

For part two, the hypothesis implies that every M are generated by things in the image of the shriek functor, using the projection formula. We want to deduce at least the fully faithfulness. We can assume that M is of the form F shriek N, and then with a base change result, we can reduce to a split situation.

And all of these maps G are like compositions of pullbacks of f, so if F has one of these two properties, then all of the maps G will as well.

In the proper case, $G_!$ equals g_*, and the base change follows from the $f^*g_! \cong g_*f^*$ base change by passing to adjoints. And in the proper case, we have a sort of G_* is G_* of 1 tending to G^*, and the base change follows more directly from again the $f^*g_* \cong g^*f^*$ base change. So there is some—you have to make sure the two base change comparison maps that you have are equal, but okay.

In order to conclude from one being an isomorphism that the other one also is, but okay.

So that was that—proves fully faithfulness for two. So that gives full faithfulness in the !-descent, but the essential surjectivity, or so the other adjoint, or the unit or the counit, or whichever it is, the one going from here to there and then back up here again—that's proved in the same way, handled similarly using base change to reduce to the situation where the cover is split. So you're pulling back to X where the cover is split, so we have a functor where we want to claim is an isomorphism. We've identified an adjoint to it, and what I've just explained is that if you do the functor and then the adjoint, that's the identity. And you'd also need to check that if you do the adjoint and then the functor, you get the identity, and I'm claiming that's handled similarly using base change along X going to Y, where it happens for formal reasons because the cover is split.

Okay, so we're part of the way towards—so now we've, this is kind of a more concrete thing that you might hope to be able to check. So you have to be in one of these two situations. Let me explain some special cases.

Special cases. So one will be closed covers, finite closed covers. We defined a notion of a proper map and we defined the notion of open immersion, but we didn't define the notion of closed immersion. So if f is proper and, well, one way of saying it is that the pullback from $\mathcal{D}_Y$ to $\mathcal{D}_X$ is a localization—the right adjoint exists, and the right adjoint is fully faithful. Nothing more, no, but a localization in category theory—in which context is defined now for categories—of which kind I forgot. I know that people like in Gabriel and some—I don't remember now what the. So let me say that—so this is a funtor which admits a right adjoint, so when the right adjoint is fully faithful, which is what I'm calling localization, it follows that you have a universal property for limit-preserving functors out of here, namely they're the same thing as limit-preserving functors out of here which kill every object, or sorry, which invert every map which is sent to an isomorphism by this functor. So this is an analog of localization for triangulated categories, for example.

Algebra: So tensors are the product of two copies of itself. $\mathcal{D}_{\mathscr{Y}}$ is itself again via the multiplication map.

Since f is proper, let me make a warning. On the level of these ∞-topoi, it's not generally true that closed and open immersions are in bijection, with the same target. So an open immersion is not necessarily going to have a closed complement, and a closed immersion is not necessarily going to have an open complement.

It's close to being true that an open immersion has a closed complement. The only...Let me expand on this. Given an open immersion $U \to \mathscr{Y}$, we get an idempotent algebra a in $\mathcal{D}(\mathscr{Y})$ such that $\mathcal{D}(U) = \mathcal{D}(\mathscr{Y})/a$. But it's not true in general that a lives in $\mathcal{D}^{\geq 0}(\mathscr{Y})$. This is the condition needed to get a closed immersion in Aff. If you start with just an idempotent algebra, it doesn't necessarily correspond to a closed immersion in Aff because it doesn't necessarily have a correct underlying animated ring. It's only the connective ones that correspond to animated rings, not the non-connective ones.

In the case of usual schemes, can you recall what are the open... Well, it depends on which functor from schemes to analytic spaces you're using, so we'll go into it. But I want to say that this is analogous to some complementary phenomenon in scheme theory, where for an affine scheme, you have a closed immersion, but the open complement might not be affine. It might not correspond to an open immersion in affine schemes, but it's still a scheme. And it's kind of similar here, even in situations where this is not connective, usually you will get a closed complement which is an analytic space, it just won't be an affine one.

In the case of schemes, like taking the complement of $\operatorname{Spec}(A)$, you get $\operatorname{Spec}(A)$, which is important. But does this correspond to a closed immersion in this setup? When you take $\mathcal{D}(a)$, what do you get? Do you get $\mathcal{D}$ of some...which you call an open...a closed here? But I'd rather let me again get to the comparisons with the classical theories a bit later in the lecture, although this is going much slower than I anticipated.

I could give an example. If you look at $\mathscr{Y} = \mathbb{A}^2_{\text{sol}}$ and U to be $\mathbb{A}^1_{\text{sol}}$, then I invite you to do the very good exercise of figuring out what this idempotent algebra is, and then the corresponding a has a nonzero $\mathcal{H}^{-1}$.

The situation on the other side is somehow even worse. Given a closed immersion, well, actually Peter described the condition required for there to be a complementary open. A closed immersion corresponds to an idempotent algebra in the ≥ 0 derived category of $\mathscr{Y}$. Then you need for there to be a complementary open in Aff, you need that the internal $\mathcal{R}\mathrm{hom}$ from the fiber of the unit of $\mathscr{Y}$ going to a, which is a functor from $\mathcal{D}(\mathscr{Y})$ to $\mathcal{D}(\mathscr{Y})$, you need this commutes with filtered colimits and preserves $\mathcal{D}^{\geq 0}(\mathscr{Y})$. That's the formula for what would be the localization functor to the complementary open, and you need that that actually defines an analytic ring structure in our sense, which amounts to these conditions.

Right, so as I said, some of this will be fixed by allowing general analytic spaces, not necessarily

Okay, I was talking about finite closed covers right as an example of descent. So, suppose we have finitely many closed subsets $Z_1, \ldots, Z_n$ with closed immersions into X, and all of these are in the image of some map f. When do we get a cover? Well, the disjoint union of the Z_i mapping to X is a cover if and only if the structure sheaf satisfies the most naive form of descent.

This is actually going to terminate at a finite stage because it's a finite closed cover. The condition is that the structure sheaf is a sheaf, which is given by the tensor product of the item-potent algebras associated to the closed immersions.

Why is this the criterion for being a sheaf cover? If this is satisfied, then we are in the image of the lower-star functor from the disjoint union of the Z_i, since each of these is closed. This implies $\mathfrak{f}$-descent, which in turn implies the fancy $\star$-descent, giving the structure sheaf condition.

Now, what about open covers? It turns out that every open cover has a finite refinement. So let's consider the case of a finite open cover $U_1, \ldots, U_n$ mapping to X by open immersions. The claim is that the disjoint union of the U_i is a sheaf cover if and only if the tensor product of the corresponding item-potent algebras in $\mathcal{D}(X)$ is zero. The reason is that the unit object is compact, so this condition is equivalent to one of the algebras being zero, which happens if and only if the cover is a cover.

For simplicity, let's focus on the case $n = 2$. Then the $\star$-descent condition gives that...

If you look at what $\star$ descent means and use the formula for the upper $\star$ functor, which is this kind of localization formula, then you find that the claim of $\star$ descent is exactly the claim that you have a pullback of this form.

Wait, I'm sorry, I'm getting myself awfully confused right now. No, I'm getting myself very confused. This is the $\star$ descent for the closed complement. I'm sorry, I'm sorry.

Of course, if something is a module over $A_1 \otimes A_2$ and it goes to zero on the U_i, so it goes to zero in each stage of the simplicial diagram, so it goes to zero apparently in this limit in the ∞-categorical sense. So if this condition is not satisfied, then there is a $\star$ descent.

I apologize, I kind of assumed I would be able to do this off the top of my head and I didn't think about it carefully.

Let me say, the $\star$ descent for this cover, where you have two elements and both of them are mapping by monomorphisms into X, then the descent, which a priori involves some Čech nerve which is some infinite thing, it actually reduces to some Mayer-Vietoris. As is kind of standard, it's the same thing as $D(U_1 \cap U_2)$ being the pullback of $D(U_1)$ and $D(U_2)$, with the upper $\star$ maps. Then you can check that you have the map functor from this to the pullback, and again it has this left adjoint, so the claim for the unit gives that the unit of X receives an isomorphic map from $J_1^\star$ of the unit on U_2 or U_1.

You have some kind of Mayer-Vietoris sequence like this, so this is a cofiber sequence. And then you have formulas for everything. I apologize for not explaining this very well, but you have formulas for all of the functors involved in terms of the corresponding idempotent algebras. If you work it out, it's just going to amount to the condition that $A_1 \otimes A_2$ is equal to zero, meaning that this condition will be directly equivalent when you write down what everything means to $A_1 \otimes A_2 = 0$.

Open immersions are special, so $J^\star$ is one for open immersions. The $J^\star$ of this, how is it given in terms of the algebra? This term, for example, will be the fiber of the unit mapping to A_1. I made a mistake by not preparing this properly, because I thought it would just come to me, but yeah, I'm sorry for messing this up.

Let's give some examples now. The first example is Zariski covers. Note that there is a functor from the usual category of commutative rings to the category of analytic rings, which sends a commutative ring R to the pair $(R, \mathrm{D}(R))$, where $\mathrm{D}(R)$ is the full derived category of R modules in the category of condensed R-modules.

Viewed on the level of opposite categories, and maybe a remark is that this functor commutes with fiber products. In fact, it also sends the terminal object to the terminal object. In other words, relative tensor products in commutative rings are also relative tensor products in analytic rings, which follows from our discussion of relative tensor products in analytic rings.

Moreover, the relative tensor product—I mean, the derived one—covers f a map to f a shriek covers. But now it's occurring to me that I forgot to remind what this means. So, note that we get a Grothendieck topology on F by saying that a sieve over X in F is a shriek cover if it contains finitely many Y_i mapping to X, such that the disjoint union $\coprod Y_i$ maps to X as a shriek cover in the previous sense.

The key behind this, besides the obvious properties of finite disjoint unions, is that if you have a shriek cover, then any base change is also a shriek cover. This is a consequence of the discussion of colimits in PRL. Basically, the base change functor on the level of Mod-PRL just commutes with colimits, so if you have the condition there, then base changing, you get the condition.

The proof is simple. Indeed, Zariski covers go to closed covers in the sense just discussed. If you have a principal open in Spec R, given by inverting some element, that inverting an element gives you an idempotent algebra, which defines a closed cover on the level of these guys. And the condition we had to check is just usual Zariski descent.

The whole formal neighborhood of that, and this then it acquires some fuzz. I claim that what you should really think is that the fuzz is making this thing really behave more like an open subset, and the Zariski open should really be thought of as closed, and it should have some kind of tubular neighborhood. So it should really be a closed subset, and then the formal neighborhood is the open complement. That's the picture I would like to suggest.

And when you go to the solid world, then you can again have an open version of puncturing. So you can name this boundary, maybe something like $\mathbf{Z}\langle\text{series } T\rangle$ base changed along $\mathbf{Z}[T]$ to $\mathbf{R}$ where T goes to F, something like that. You can name the boundary and then you can remove it, so it's going to be a closed subset that you can remove, and you get an open subset. Then you're back to the usual way of thinking of having a Zariski open.

But then it's not—you take $\mathbf{R}[T]$, $\mathbf{Z}$, and then $\mathbf{Z}\langle\,\rangle$. So I'm saying once you move to the solid framework, then you can name this boundary here, which before was just heuristic, and its name is this. That will give it an input in algebra in $\mathcal{D}$ of $\mathbf{R}[z]_{\text{solid}}$, and the complementary open is like $\mathcal{D}$ of $\mathbf{R}[1/f(z)]_{\text{solid}}$, so $\mathbf{Z}[1/f(z)]$.

I mean, there are two different ways you can embed schemes into analytic spaces—one is based on sending R to $R\langle z\rangle$, and the other is based on sending R to (R, R), and this is the one I'm discussing right now, where you base change to the solid $\mathbf{Z}$. And then the Zariski opens look closed, but if you use this one that corresponds to always removing the boundaries, then the Zariski opens actually go to open immersions.

So what do you mean by boundary? I don't really know what I mean by boundary, if I don't just mean the formal neighborhood minus the middle. Intuitively, I'm claiming you're removing kind of an open piece from this chunk, and then the closed complement should intuitively have some boundary. I mean, there will be boundaries at infinity too if your thing isn't proper, and what's happening at infinity is also important, but let's pretend we're in a proper thing or something.

In particular, we get a functor from Zariski sheaves on derived schemes to sheaves on $\mathbf{F}$, which is in fact the pullback of some topology. So it commutes with colimits and finite limits, which is a consequence of this. This is one way of embedding usual algebraic geometry into what I haven't quite defined what an analytic space is, but it's close enough for practical purposes. We're going to have some kind of hyperdescent condition we want to impose as well, which I thought I was going to get to today but I clearly didn't. But this is basically analytic spaces or analytic stacks, modulo a couple of technicalities.

Conditions and when you analyze this, you use the weaker one with which the Serre-Swan theorem covers, although if you did a stronger one, it would still give the same claim, because it would be just a further localization of this. On the other hand, yes, well, I'm not going to touch the other side - one could, but the risk is that it also depends on whether you have... Yes, yes, it does. But I'm not going to care too much about that side. It was said in some talk, I don't remember who, that maybe Peter said that there would be something intermediate between Čech descent and full hypercover.

I was going to discuss it today, but it's not going to happen. Okay, so the other subtlety is a set-theoretic subtlety, because the category of commutative rings is not small, so you have to be careful considering presheaves and sheaves on it. And the same if you take only those which are accessible - yes, yes, exactly. And then you have to prove that sheafification preserves this accessibility and so on and so forth. But okay, I don't think in the remaining 7 minutes I could do justice to the next topic, which was going to be adic spaces. I could rush through it right now, but I don't think that's a good idea, so I'll stop here.

I wanted to know what, for example, should cover... Oh, oh, oh, oh, sorry, I didn't understand your question. I'm sorry. Whether in a tall cover also gives you a sheaf cover - yes, it does, it does. Right, so this is something I should do in a few minutes.

So this was all motivated by Matthew Emerton's theory of descendability. So we were talking in AF, and then we were saying we have this derived category of anything in AF, and it's built on this condensed framework. But it's clear that the discussion is very categorical in general, and we could just try to make the exact same definitions in the world of ordinary algebraic geometry instead, using the usual derived category of a ring, and see what kind of definitions that gives.

If you take the same definitions, but with the pair R and DR, where R is a usual commutative ring or maybe derived, and DR is the usual derived category, then some simplifications happen. First, every map is proper, which is quite clear - well, maybe that's the main simplification that happens, that every map is proper. This implies that every map is also sheafifiable, and then the proposition I discussed earlier also holds in this setting, where there's no condensed thing involved.

The upper shriek and lower shriek here are not the same as the ones in Grothendieck duality theory - they're just some categorical adjoints. Nonetheless, it turns out to be useful in this descendability discussion, because every map is sheafifiable and every map is proper. And then we deduced that a shriek cover, X to Y, is the same thing as saying that the unit in Y lies in the image of F lower star D of X to D of Y, and this is exactly the definition of descendability in Matthew's work, or one of the equivalent characterizations he gives.

What about the classical Grothendieck flat and faithfully flat descent? Is it related? Let me give some examples. Actually, every kind of purely formally descendible map, R to S in an ∞-category sense, gives a sheaf cover in the usual sense via this functor. And Matthew gives many examples of descendable maps, such as a tall cover.

So, this is kind of funny. You need this, well, we apparently probably really need this countably presented hypothesis. That means that you have a map from A to B which is faithfully flat, but also B is presented as an A-algebra, or maybe even just countably generated is enough, so B has a presentation as an A-algebra with countably many generators.

The condition is that on $\pi_i\mathbf{Z}$, it is faithfully flat and the π_i are countably generated. This is related to some limit, higher limit. I mean, if it is a limit of A_n's, that's fine too, but in practice it's the same thing. If it is only countably generated, then you don't get it. You need it to be countably presented, because you need to be able to reduce to a countable base ring.

The point is, in a lot of situations where you have classical descent, you get this even stronger Čech descent, which also gives you descent and much else besides. But also, there's some issues if you have $R \to R/I$ where I is a nilpotent ideal. The Čech descent is defined on the level of derived categories, so the descent you get for this does not imply that $D(R)$ is the same as $D(R/I)$ obviously.

The reason is that when you do the descent, you're doing everything on the derived level, and you end up with terms like this in the limit diagram, and those are not the same as R/I. Going to $\pi_i \mathbf{Z} R$, does it have the same property as $R \to \pi_i \mathbf{Z} R$? No, you can have a polynomial algebra with a degree 2 generator, and this has a module which by inverting that degree 2 generator goes to zero when you mod out by X, but is non-zero.

So, the analog for simplicial rings of a nilpotent ideal is that you should ask that the ring be truncated, so it has only finitely many homotopy groups. Then going to $\pi_i \mathbf{Z} R/I$ would work.

More generally, if you have a proper map of derived schemes, then you also get Čech-descendable. That's a generalization of this. And more generally, any faithfully presented cover, that's kind of a combination of this and that. There's a huge class of very, very much, but you do have to remember that in non-flat cases, the descent involves a higher topos and so on. All of these kinds of things will go to Čech covers in our setting.

Question: Was it equivalent to have a map of commutative rings be descendible in the ordinary commutative algebra Matthew sense, or for the image under this functor to be a shable map, a strict cover in our sense?

So, what's the argument? This is important to understand in this kind of relative sense. The descent and this pro-object formalism mean that this pro-object is just an old discrete pro-object. Okay, yeah.

Peter was pointing out that there's another characterization of descendibility, which is in terms of just the rings - like A, B, B tensor A B, and so on. You have to have descent, you have to get a limit diagram. But then you have to get even more: you have to get a very stable limit diagram. It has to be a pro-isomorphism between the tower you get from this co-augmented thing, the n-index tower, and that pro-object should be pro-isomorphic to the constant pro-object A.

In that condition, it's clear that it's independent of which framework you put it in, because the pro-category - the usual D(R) sits fully faithfully inside D(condensed R), and therefore Pro of usual D(R) sits fully faithfully inside Pro of D(condensed R). So for the pro-thing, it's enough to work in the homotopy category - it's enough to work in a weaker framework.

What you can do is pass to the fiber, and then you want to know about a tower being pro-zero. It's enough to look at it in the homotopy category. Okay, thank you again for your attention.

When a tower is pro-zero, is it then the case that it is uniformly pro-zero? That is, because there is some finite domain, it's enough for any stage to add a fixed number? Exactly, yes, in Matthew's work. And it works with Simpson, who works in a very general setup, just like commutative algebra objects in presentably symmetric monoidal categories. He only looks at the proper case, so every map is proper in our sense.

So the setup is a symmetric monoidal category C, and then you consider an algebra A in C. He defines descendibility in this context. It specializes to the condensed world but only discusses the proper maps, not the arbitrary shable maps.

19. Analytic stacks (Scholze)

https://www.youtube.com/watch?v=T9XhPCI8828&list=PLx5f8IelFRgGmu6gmL-Kf_Rl_6Mm7juZO

Unfinished starting from 0:00

So today, we will finally define what analytic stacks are. It's not so difficult. Recall that we have this category of analytic R-spaces. This was actually, I think, presentable. That's been proven - it has all co-limits, and it's generated by a set worth of the usual analytic rings and spaces.

We can make the following definition. I don't expect this to be accessible, but the word "accessible" is just there to deal with some set-theoretic issues. From analytic rings or Fr??chet analytic spaces, toward... Recall, the word "accessible" here, there will be a condition in just a second. This means that it commutes with filtered colimits for sufficiently large U. It's such that, sorry, let me rephrase the conditions as follows.

We have some open cover U of A, and then some hypercover U of A, for which the colimit of the nerve of this cover is isomorphic to A. Also, X of A is isomorphic to the spectrum of the colimit of the sheaves on this nerve. By the way, I write "unspec" where Dustin just wrote "spec" - I don't think we've settled on a final notation, just not to get it confused with the usual spec, let me write "unspec".

This is some form of descent that's somewhere strictly between... Can I ask just a minor technical question concerning the notion of hypercover? In usual algebraic geometry, do you hear me? Yes, I hear you.

So let's consider the case in usual algebraic geometry when you have, for example, an fpqc cover or something like this. There could be two meanings of fpqc hypercover. One meaning is that the map from X0 to X and the map from Xn to the coskeleton are fpqc. The other meaning is that there are coverings for the fpqc topology, which is H. Sometimes it causes some concern, what is the meaning.

In our context, you use the strict meaning, I suppose. That everything, I mean, you... Yes, so I want all maps to be fpqc, but then there is no difference between, if it's refined by a stable cover, then it's already a cover because of the S condition. Ah, okay, so here it is satisfied that if some, if A is dominated by it's already fpqc cover, so okay, thank you.

Right, and I also need to say that commuting finite products, so commuting products including the empty product, geometrically just means that X evaluated on a disjoint union is the product.

So this condition is, I mean, up to issues, and it's something that's somewhere between 2 and 3. For Čech sheaves, you would only consider some Čech nerves of covers, and those would, by definition of what a Čech cover is, always satisfy this condition, so for Čech nerves of Čech covers, you always have this condition, so it's always a Čech sheaf. But for Čech hypersheaves, you would ask this condition for all Čech hypercovers, whether or not they satisfy this condition, but we definitely want our derived category to satisfy, to be, some abs property, so we need to restrict to classifying the Čech for which this holds.

Let me just state this as a remark and not prove it. Sheafification of an accessible pre-sheaf is successful. This is the analog of the theorem of Waterhouse. This means that you can actually form colimits in this category, because sheafification forms the colimit in the category of sheaves. But then you need again to enforce the strong sheaf condition, so you need to sheafify, and you can check that it preserves the set-theoretic assumptions.

Examples or remarks: For any... You can look at the functor which takes B to the maps from A to B. I have a small technical question concerning the sheaf-ification, which I believe you mean, sheaf-ifying it to satisfy this precise condition, that is not the Čech sheaf and not the Čech hypersheaf, but this intermediate condition. Yes, but then in order to construct this...

Unification—it seems that you need to know that the pullback of this kind of hypercover satisfying the condition on the ∞-category is also such a hypercover. But again, this follows from the same argument. So this is actually equivalent to $D(A)$ being the colimit along the Lourish maps of the $VA_\bullet$ in the ∞-category of presentable ∞-categories. This is a condition that base changes, because the ∞-categories base change well.

You use all of this—this was discussed in the previous talk. This is all magic about ∞-categories. Then you use it, it is the tensor product when you base change.

Okay, so first of all you have all the fine objects. For any affinoid ring A, you can consider the object $\mathrm{Spf}(A)$ which takes any B to the homomorphisms from A to B. This already satisfies all the required properties, like commuting with products.

Now you have a hypercover, some $B_\bullet$, and then you want to map to B. In particular, you want the limit of the $B_\bullet$ to be B. So you map to all the $B_\bullet$ and you also map to B itself, because it's the limit.

This brings up the analytic rings. This fully faithfully embeds the analytic rings into the ∞-stacks. The accessibility condition is precisely that you're a small colimit of objects in the essential image. This is similar to how we think about condensed sets.

So what is the ∞-category of analytic ∞-stacks? For any analytic ring, there is an object which is the analytic spectrum. All the others are built by some gluing procedure, by some colimit of these fine objects. The hypercover condition tells you the ways in which you're allowed to glue.

As will become clear later, we want this very general topology because it means that analytic spaces that seem completely different can actually be the same object, just represented in different ways as a colimit of fine pieces. Often there is a geometric picture that they should secretly be the same, and to say they're really the same, we need to use rather strong topologies.

Rings, and then there's a way to extend them to the full class, but some left extension. And then, I think this way can probably show that...okay, okay.

All right, so there is a fun from like derived schemes. The derived schemes, they also of course embed into like the so-called ∞-topos. We can go to taking particular, like any Spec $\operatorname{Spec} A$ for a ring A, and take a condensed thing, which is really just the and, um, and all condensed modules. And if you want, you can also, as Dustin discussed, you can basically put the fpqc topos in here, except you have to do this funny countability assumption, only one fpqc.

So basically, like any fpqc stack can also be mapped, and there's really not much to show here, right? I mean, you definitely just have to spin on rings, and then you just have to show that whenever you have a ∞-topos cover, it goes to such a strict cover, but that's basically by definition. And again, you could also use this funny thing between sheaves and hypersheaves here in the $\mathcal{C}$-topology here if you wanted to.

Right, uh, so ah, right, maybe I can mention that the pro-étale site is actually fully faithful into the condensed ∞-topos view. It's a full back where you actually uses the Tan^* functor. Do you do you need QC? I think you can get rid of this because you can certainly reduce to the quasi-compact case, and then there's this argument of offer that you can write as a limit of quasi-separated things. You can how do you reduce to the quasi-compact? Well, you can certainly, if you want to understand Homs from X to Y, you can cover X by, but you can X compact, but then comp-wise they can definitely comp. Oh yeah, okay, there other argument that you can make the compact so can.

Alright, so you have some algebraic geometry sitting in there. Uh, next you have Stein spaces sitting in there, which well, these are good from the fromology one, so these are a plus. Like when you talk about Stein spaces, you always assume that this is Stein, and so what I'm saying now, let me also assume this because otherwise I would run to some longer discussion about exactly what I want to do.

And so I can just restrict this to the one plus of the analytic ring A^+ solid. So note that this way we get a different fun, get two different functions. So there are actually two ways to embed schemes into analytic spaces. You can take a Spec A, you can also match this to $\underline{\operatorname{Spec}\mathcal{O}_A}$. And then for there's a question in the chat, Peter, it says for derived schemes you're mapping the trivial analytic ring structure, exactly here I'm currently using the trivial analytic ring structure.

Right, no. And so you can either now take this further and look at A-modules and solid $\mathbf{Z}$-modules, or you can look at relatively solid A-modules. And so now there, and if you wanted to, you could put it right here suitably formulated and put it then also put it derived here. So then there are like three functors from schemes or derived schemes to analytic stacks,

Of course, this immediately suggests a generalization: for any analytic ring A, we can consider the "relative" scheme over $\operatorname{Spec} A$, which can be viewed as analytic spaces over A. To do this, we need to consider the "condensed" version of A, which has an underlying discrete ring. We can then look at schemes over this condensed ring.

For example, we might have the complex numbers with their usual ring structure, and then consider the usual schemes over the complex numbers as a special case of this framework, by just taking the condensed version of the complex numbers.

Now, you mentioned something slightly puzzling about "derived Huber pairs". This refers to a derived version of the notion of a Huber pair, where we have a derived ring with a π_0 part and all the higher π_i parts. I don't want to go into the details of this here, but it is something that can be defined.

In terms of the fullness of the functor, we know that it is definitely fully faithful on the $\mathbf{F}_p$-linear case, because then it reduces to just maps of rings, and we know that Huber rings are fully faithful for analytic rings.

For the general case, we know it's fully faithful under some mild coherence assumptions. But we don't have a full fully faithfulness result, basically because we don't have a good version of an "analytic Grothendieck topology".

There's also the fact that, in contrast to the scheme case, the "adicity" functor doesn't commute with pullbacks. So you have to be a bit careful there. But if you restrict to the "Tate-adic" or "adic" case, then it behaves as expected.

There's also an interesting feature on the right-hand side: if you take a fiber product of f-analytic spaces, it's always just the usual tensor product of the corresponding rings. This is not true in the "adic" case, where you can have a fiber product of adic spaces that is not itself adic anymore. There's some subtlety there that can be explained in a nice way using some derived techniques, but I don't want to get into that here.

You can also do this "non-Archimedean geometry" over the real and complex numbers. For example, you can have complex analytic spaces mapping to analytic spaces over the Gaussian complex numbers. The main issue here is that complex analysts usually don't tell you what an "analytic space" is, but they could. Any complex analytic space can be written as a union of what we call "Stein" subsets, which behave very much like affine objects.

What is a Stein subset? For example, it could be the vanishing locus of some ideal inside a polydisc of complex numbers of absolute value at most 1. The algebra of functions we put on such a Stein set is the algebra of holomorphic functions defined in some neighborhood. It turns out that this Stein algebra is excellent, and if the Stein set is actually a manifold, it's regular and has all the nice properties you could hope for.

So when talking about coherent sheaves on a complex analytic space, you can really just talk about finitely generated modules over these Stein rings, for each Stein subset. There is a small caveat, which is that you could have an ideal such that the number of connected components of its zero locus is infinite, and this would prevent the Stein ring from being Noetherian. But for Stein sets that are actually the Riesz closure of a polydisc, the Stein algebra is Noetherian.

Politics is not arbitrary or compact. Okay, I remember that it was for another compact.

Right, but if you look at complex geometry, there is a very close analog of the notion of an étale subset. Everything really has an algebra, and everything is very similar to how you do rigid geometry. Similarly to the Cech complex, which is actually a cosimplicial object over the algebra of continuous functions on the spectrum of that algebra, endowed with the Gauß-Hecke complex norm. This has a natural topology, like uniform convergence on compact subsets. So this also has a natural topology. You could also define direct and ind settings, whatever. It's a so-called dual nuclear Fréchet space.

Again, this is fully faithful on the analytic structure. You view it as an algebra in the Gauß-Hecke theory, and then you just take the induced analytic structure. So you just check on the underlying ring, which defines a condensed ring that is actually an algebra over the complex numbers. Because it's dual nuclear, it's actually Gauß-Hecke, and you can just induce up the Gauß-Hecke analytic structure from the complex numbers to here.

Not every dual nuclear space is Gauß-Hecke, but these are actually even nuclearly Gauß-Hecke. What is projective? It means some locally closed immersion into projective space, so maybe like an open subset of a closed subset.

Before I go to the next example, there's actually nothing special about using complex manifolds. You can do similar definitions for real analytic spaces, smooth manifolds, or even C^0 topological spaces. You take the ring of continuous functions and do the same construction. In each of those cases, you can decide whether you want real or complex-valued functions, and they give you two different functors where one is just the base change of the other.

Now comes a more involved example. This is showing that all the usual theories of algebraic geometry and geometry that are known can be incorporated into this framework. You could also directly define some notion of stacks. You can imagine many possible notions of a complex analytic stack by taking sites with complex analytic spaces and defining a stack over that topology, maybe just open covers.

But now comes the final example, relating this back to condensed sets. Since everything has become a stack, let's take condensed non-étale sheaves. These actually map into the framework we've been discussing. This works even for just schemes, without needing to go analytic. There is something condensed on the right, namely the analytic rings that were condensed rings, but we're not really using this condensed structure here - it comes from the speed objects.

Right, so if S was a finite set, it would just be a finite disjoint union of copies of $\operatorname{Spec} \mathbf{Z}$. In general, as S is the limit of finite sets, this is the limit of these finite schemes. And so then you can access further to the $\mathcal{I}$.

And here's the reason that we chose this really funny version of between chiefs and hyper chiefs. So, by definition, they are hyper chiefs of a certain profin-topos, the one that we always use. And so to get this, you have to show that if you have any hyper cover here of a profin set by profin sets, then it goes to something for which you enforce the S here. It definitely goes to a hyper cover, and so then there's a small thing you have to check that it actually is the same condition on the $\mathcal{D}$-category.

Basically, the argument that factorially flat maps satisfy the S-condition also proves that countably presented sheaves satisfy it. So I should say that here, the lightness condition is again important, because it's always true that if you have a stative map of profin sets, then if you look at the corresponding map of continuous functions, it's always flat for any profinite space. But we need a descendible map for our business, and so we only know that countably presented factorially that maps are descendible, which is another reason that we have this lightness condition.

All right, so here's a remark. There are different ways of embedding schemes into ∞-stacks, and now there are also different ways of embedding something like topological manifolds into ∞-stacks or the complex or real numbers. Either you can do the thing where you use the algebra of continuous real-valued functions to do this, or you can treat the topological manifold purely as a topological space or factorially into condensed sets and then go from condensed sets to ∞-stacks. This gives you a different thing, which is actually defined over the integers, right, because this ∞-stack just has integer-valued functions. These are completely different incarnations of a topological manifold, but there's actually again a map between them.

So let's consider the following example. You can take the two-sphere as a topological space and then treat it as a topological manifold, and this gives you an ∞-space which, in the compact case, it really is just the UN of the algebra of continuous functions. And let me work everywhere over the complex coefficients, which category of, let's say, $\mathcal{C}$-sheaves. What else could I do? I could take S^2 and treat it as a real analytic manifold and again build an analytic space over the guess, the complex numbers. Or, you could think about all other possibilities, like C^∞, real analytic, and so now you would have here the algebra of–what's a good notation for real analytic functions? Ω, yeah, C^Ω sometimes it's called. And I don't know, think about all possible other algebras like C^∞ functions, C^k functions, between they would all also give ∞-spaces.

But then, you can also like S^2 is like one version of $\mathbf{P}^1$ of the complex numbers, so you can also treat this as a complex space. And well, then you want to get something with the guess, the complex numbers. This is not $\mathcal{O}_{\mathbf{F}}$ anymore, right? I, there are not so many global homomorphic functions here. It is what it is, it's not $\mathcal{O}_{\mathbf{F}}$, but it's some glued from two copies of $\mathcal{O}$ over the disc, over the holomorphic functions on the disc, right? So you can cover this by two discs, glued along the over-convergent holomorphic functions on the circle. And similarly, this would be glued, but from two copies of $\mathcal{O}$ over the disc, glued along the over-convergent holomorphic functions on the circle.

Okay, so clearly, like making it more

Take again just S^2 and S^3 just as a condensed set, and go to the space of functions on this complex. So here, I'm using the functor from Lecture 6. Basically, what happens is that here you want to take the locally constant functions on S^2. Well, there are none on S^2, but if you secretly think of S^2 as being the quotient of a profinite set by a profinite relation, which you can always do by this condensed perspective, then on those profinite coverings you do have locally constant functions, and then you can do this gluing.

Secretly, to evaluate what this is, you have to remember that secretly S^2 is a quotient of a profinite set by a profinite relation, and then you can find locally constant functions. Here you have algebraic functions, here you have holomorphic functions, here you have real-analytic functions, and here you have continuous functions—all of them give you different incarnations of what the two-sphere might be.

There's already one really funny thing, which is something that Dustin already mentioned a couple of lectures ago, that one incarnation of the GAGA principle is that it's actually an isomorphism of analytical spaces. So this is one kind of GAGA statement, which is now not just a statement about some coherent sheaves or some derived category of coherent sheaves or whatever, but really before you pass to linear algebra, on the level of spaces, there is an isomorphism.

I mean, this is an instance where you see how the way you build these things is completely different—one is built from just continuous functions, one from holomorphic functions, and the strong work topology that we impose is precisely there to ensure that you can have the possibility of such interesting results.

The C of modules, of course, is just the category of modules of the algebra C. A vector space with an action of the continuous functions. But as here, there are just sheaves of C-vector spaces on the topological space.

One can show that if X is maybe locally compact, then the sheaf has a question: Peter, why do we consider the gas analytic ring structure on the left?

The reason is that otherwise, this is unclear. For example, if you want functions from complex spaces towards analytics, you need the intersections to match up. If you have an intersection of compact line subsets, then the intersections should again be something compact Stein. And you want this to be mirrored on the side of analytics text in our picture. Which means that if you compute the corresponding completed tensor product of analytic rings of functions on D and the other disc, and then glue them together, you should get functions over convergent functions on S1. And this is a computation that comes out correctly in the gas analytic ring structure, but definitely doesn't come out for the usual analytic ring structure, because then you would just take the usual tensor algebraic tensor product, and this is just nonsense. So you need to complete the tensor product, and then it comes out right.

So for X-dimensional schemes, then you can look at the category of Tex, where A is any, and B changes to the spectrum of A. And this is just sheaves on X, sheaves on just X, some reasonable topological space. Those values, so yeah, so you can think of general sheaves as being some kind of functions on an associated space.

I realized before that I should have said that the functors that take any spectrum A towards the A-modules, or also towards presentable ∞-stacks, are linear over T, satisfy the send conditions, and hence induce functors on all things analytic. So for any stack, you can define the direct image and descent, and also you can define what is like a sheaf of categories over X. So here it is again the intermediate condition, but for this functor version, between the center type for any. This will actually both of these things will actually be part of some general six-functor formalism, and at some point we will talk more about that. And basically, all design criteria for the GR topology was actually precisely that, that for any analytic d, we definitely want to be able to talk about the C-sheaves. And at some point we also realize it's probably good to enforce that we can also talk about sheaves of categories, and then we are basically taking the strongest possible GR topology where this is true.

What is Perfil? That is the thing which is gotten by descent from the association Spec A goes to the category of A-modules in PERL, which is a 2-category, yes, a 2-category, but we don't have to care, we can just see it as an ∞-1 by neglecting the non.

Right, okay, so this is some general examples that maybe we want to cover in more detail in the remaining lectures, but I did want to come back to the point where we started discussing analytics, which was the construction of the T of the curve, and discuss this in a little more detail now, because now we have the language of talking about this.

So let me recall what we want to do. We have this universal ring, denoted Brr, endowed with the topological unit 2, such that when you take this usual Guy, which is a free Guy on a normal sequence, and you tensor it up to the string, then the operator that's 1 minus Q* shift, so shift is the endomorphism of P that just shifts all the integers on S, and this was this ring structure we introduced some weeks ago. And where an on-computation was that you could actually compute what's the underlying string of this was, and it was this algebra of Laurent series in Q, which has a certain funny to normal condition on their coefficients. So the underlying condensed ring is this algebra, but maybe the underlying ring of the underlying condensed ring, because now I'm just telling you a set, maybe.

Did I tell you the current condensed structure on this? Okay, so this is some funny ring of formal power series, integral series, with rather strong growth of the coefficients. And then we recall that the goal was to finalize the analytic curve over $\mathbb{A}^1$ or really some $\mathbb{G}_m^{\triangle}$.

This would be a scheme such that if I take the incarnation of $\mathbb{E}_Q$ as a scheme over $\mathbb{A}^1$, this can be written as a quotient of a $\mathbb{G}_m$ over $\mathbb{A}^1$ under multiplication by Q as an operation. And I already a couple lectures back gave the outline of how I thought it should go.

The first step is to see that there is a certain kind of norm, and I will talk about this more in just a second, which goes from the $\mathbf{P}^1$ array, but really the $\mathbf{P}^1$ incarnated as a scheme over $\mathbb{A}^1$, toward the infinity. Intuitively, this tells you how large a point is. Having a point here is basically having a point here, so maybe nothing, think from a field or something like this, and then an element of that residue field. So there's something like saying that on any residue field, there would be a norm $\mathbf{Z} \to \mathbf{Z}$, like the absolute value, but now this map here is really meant to be a map of analytic stacks.

The left-hand side is now an analytic stack. We can take $\mathbf{P}^1$, make it into an analytic stack by this general functor that gives it a trivial ring structure, but then we can base change it to $\mathbb{A}^1$. And maybe here I should write that I mean, so here you're incarnating this by treating it as a condensed set, and we could or could not base change it to $\mathbb{A}^1$. Yeah, we can. You have to choose some value for the absolute value of Q, like say q. So it should stand for some q, q is a section of this, and it's multiplicative in fact, is a unique such thing with the properties I will mention.

So it should be multiplicative in a sense I will make precise in just a second. So I won't talk about these norms second. Let's say, to make it unique, I should precisely specify where q goes. Meaning, like q is actually a section from the spectrum of $\mathbb{A}^1$ back into this, and this should go to a constant value, something like half. And then you can define the analytic $\mathbb{G}_m$ array as a subset of the $\mathbf{P}^1$ array as the preimage of an open subset $\mathbf{Z} \setminus \{\infty\}$. And then you still have q acting there, and then can take the quotient by $\mathbf{Z}$. And then, by an argument I already sketched last time, this is basically a projective curve. And then you have to prove the zero-dimensional algebraization theorem that would apply to all projective curves with a section, at least, that it's algebraic, so it is in the image of different schemes over the underlying ring.

Towards the end, I mean, you need to make the line, it's not clear even geometrically, but here it's okay. We discussed this, you need some GAGA or exact GAGA, and something which is again where is a soft, soft. Well, in our approach to GAGA, you don't really get SAGA, which is much more general than GAGA, but it doesn't really tell you anything like Riemann-Roch for something you don't a priori know is algebraic. I mean, so that's kind of a separate issue.

Right, so one key notion that comes into this is a notion of norm that we want to have here. So let me actually define this in general. It's a slightly awkward notion of a norm $\mathcal{A}$ that is not really a norm on the underlying set of $\mathcal{A}$ or anything like that, but it's rather

One other thing I want to mention is about the notion of "norms" for elements. We will also follow something, and here's how I want to phrase that. So inside the $\mathbf{P}^1$, I can first look at the locus $\mathbb{A}^1$ where I didn't really have just functions, but then I can look at the locus where the function is "near-potent", and that is some locus given by the zero locus of a certain polynomial. And then you join the two variable, so in other words, it's again just this $\mathbf{P}^1$ treated as an algebra. This maps to the algebra $\mathrm{UNP}(a)$, which is just another ring. Instead of taking the norm for some s, I want to say that this composite factors over the closed interval $[0, 1]$, not the half-open one. It turns out that the better notion is this one, for reasons I can't fully explain yet, but it's the one that behaves well.

I should also say that such a factorization here turns out to be really just a condition, because there's really a monomorphism, and similarly for these other conditions like zero goes to zero, I want to say that it factors over the subset $[0, \infty]$ again, that's just a condition.

Alright, so these are some first properties. But then when you have a "norm", you usually ask for some version of multiplicativity, and also some behavior with respect to addition. It turns out we just forget about the addition part, but we keep the multiplicativity condition. However, I'm not working on $\mathbb{A}^1$ but $\mathbf{P}^1$, and the reason I work with $\mathbf{P}^1$ is that even on $\mathbb{A}^1$ I would want to allow some functions that could have infinite norm, and if I'm allowing infinity on the target of this map, I might as well allow it on the source as well.

Now for multiplicativity, I have to be slightly careful about handling the zero times infinity cases. One way to do this is as follows: I consider the locus X inside $\mathbf{P}^1 \times \mathbf{P}^1 \times \mathbf{P}^1$ where $XY = Z$, and similarly there is a smooth surface xar inside $\mathbf{P}^1 \times \mathbf{P}^1$. On the open part, you can look at the incarnation on the level of schemes of this closure, and one incarnation just on the real numbers, and then you have X incarnated as a schematic morphism to $\mathbf{P}^1$, incarnated as a choice, and this matches to $\mathbf{Z}^3_\infty$, and inside here you have this xar.

I wonder if it would be the same as asking first that the map giving the norm is equivariant for inversion on both sides, and then just asking for multiplicativity when you restrict to $\mathbb{A}^1$. I think so, yeah, that was kind of the definition I thought we were using, but maybe...

Yes, but even on $\mathbb{A}^1$ you have to be careful, because $\mathbb{A}^1$ can map to $[0, \infty]$, it could map to ∞. Oh, I should have said that I should have said the pre-image of 0 instead of $\mathbb{A}$

Might the norm of two be unbounded? Usually, the absolute value of two is less than or equal to two, which implies a triangle inequality. However, there could be an example where the absolute value of two is infinite.

On this Gaussian analytic base, I will try to construct such a norm in the remaining minutes. In particular, we can then look at what the norm of two is. It's some function from the analytic spectrum of a Gaussian towards the extended integers, and it's surjective.

The method I already stated on the board is that there is a unique norm on the Gaussian space, taking some number between zero and one. It doesn't matter which one, because you can always rescale the target by some exponential and still be alright.

Let's say x is some kind of finite-dimensional locally compact space, and y is then I claim there's some kind of Tannakian duality describing what maps from y to x are. It turns out that to give such a map from y to x, it's just enough to give a functor from sheaves on x towards sheaves on y, with some property. But actually, you don't really need to specify the values of all.

Let me first see. This is the same thing as the functor F from the category of abelian groups on x towards the category of abelian groups on y, such that these functors are compatible with all pushouts and pullbacks, and they should be linear over the base field. The condition is that locally on y, the following happens: on x you have lots of important algebras, namely for any closed subset of x you can take the constant sheaf. This should be connected, and the image under the functor F should remain connected. In general, giving such a map, you can always do it locally, so the condition you have to enforce is just some strict local condition.

It turns out that this is the condition that locally on y, the pullback or the sheaf locally for the topology remains connective. And note that the full category of sheaves on x is actually generated by these guys on the closed subsets. So to define the standard functor, you really only have to declare these important algebras. Describing the map from y to some such guy here is completely determined by specifying, for each closed subset of x, an important algebra, and if they're all already connected, then you're good to go. The only thing you have to check somehow is that the Tor product behavior of these important algebras exactly matches the intersection behavior of the closed subsets of x, and that's what it means to be a tensor.

This is actually a set, even though a priori it's an analytic object. To realize this space over the appropriate category, we need to find some important object. Typically, you have to find something that should correspond to the pre-image of an interval from zero to some number R. If you know where these pre-images should go, then due to the compatibility on the involution, you also know where intervals going from somewhere to infinity should go. Everything else can then be written as some kind of colimits and intersections of those.

Really, to describe the SC norm, you just have to say what is the pre-image of the interval $[0, R]$, in other words, what's the locus where the norm is at most R. This should be the analytic spectrum of a certain ring, generated by sums of $n \cdot T^n$ that converge for $R' > R$.

To produce such an object, you can start with a sequence in A and multiply it by suitable powers of Q to make it satisfy the desired condition. This can be described as a colimit of T subject to a condition on R, which can be explicitly written down in terms of the Gauss algebra. The key idea is that the absolute value of Q should be $1/2$.

If you form certain tensor products of such algebras centered at 0 and ∞, the tensor products will behave in the way you'd hope, matching the intersection behavior of intervals from 0 to ∞. This is a computation that relies on working over the Gauss algebra.

I should probably stop here, as I'm running over time. Let me know if you have any other questions!

Theories, yeah? So my question is, can you define an analytic space theory using your language? Because if I wanted to put an additional structure in an analytic space, perhaps I want a general definition for all analytic spaces - put the structure there. That's exactly what we're doing; that's what an analytic stack is. So you can say, "I have an analytic space theory, and this reduces to each known case."

Well, I - are such analytic space? Analytic space theory? I don't know what - yeah, I have to know what you mean by that. I mean, like something that would generalize all the... Yeah, that's what we're - that's what we're trying to do with this concept of analytic stack. Yes, you shouldn't be scared, it shouldn't be put off by the fact that we change the name from "space" to "stack" - that's basically a technicality, but that's the goal.

So you have a definition, like "an analytic space is an analytic stack that..." Well, you could try. Yeah, that's my question - like, do you have a definition, like "an analytic space is an analytic stack satisfying some axioms"? Do you have... Well, we have too many - maybe. Well, what I mean is, like, do you have the ultimate one, kind of like the umbrella one? From - we we tried, but we could never...

I mean, you could just say that the functor of points takes values in sets. No, F-ones are not okay, sorry. What if you ask that it takes values in sets? And even fine analytic spaces are not - not fine. Well, yeah, but the classical ones are. So anyway, the classical, the F-line, like, takes a ring to its underlying... Because the test category consists of derived things, you can... Yeah, yeah, that's true, that's true. No, but anyway, you don't have a definition of analytic... But I mean, you could ask that it's the counit of F along monomials. That's the - sorry, what's the condition? You could ask that it's the counit of F along monomials.

Okay, okay, thanks.

20. Normed analytic rings (Clausen)

https://www.youtube.com/watch?v=wk_wInYTasQ&list=PLx5f8IelFRgGmu6gmL-Kf_Rl_6Mm7juZO

Unfinished starting from 0:00

Okay, so now continuing the discussion, I want to continue the discussion from last time. I want to start by maybe going over a little bit of what Peter said, perhaps adding some details or some explanations. The main topic is normed analytic rings.

We had this, and now we've officially defined this framework of analytic stacks that we're working in. We saw that there was an embedding of — well, at least a functor, we saw that there was a functor to analytic stacks, and it's induced by these things that are generated by profinite sets. So, say T, and to one of those you assign—I'm just going to write it, I know this is probably overloaded notation, but you just have this discrete ring of continuous functions on your profinite set with values in the integers. It's a discrete ring, and you can view it with the maximal analytic ring structure, so uncompleted. You look at all condensed modules over this discrete ring or derived condensed modules over that discrete ring, and that's the analytic ring that you associate. And then its spectrum is an analytic stack.

The thing that makes this functor nice is that this assignment here sends hypercoverings to Čech hypercoverings, satisfying Čech descent or descent. Apparently, we don't need to prove that these are equivalent, Čech descent and C-descent. This is something that Peter referred to in some explanation. The idea is that when you have a category, it's equivalent to the inverse limit by Čech or the direct limit by C, and those are purely formal facts about pro-limits, proved by Lurie, that don't need anything on the shape of the diagram or whatever.

So these were the things that defined the notion of analytic stacks, but it also preserves finite limits. So, for example, pullbacks—well, let me say finite limits. If you take a pullback of profinite sets, that's just a filtered inverse limit of pullbacks of finite sets, and then it goes to a filtered colimit of the same situation for finite sets. This reduces to finite sets, and for finite sets, it's kind of completely clear. And those two facts kind of imply that this functor here, the induced functor, is cocontinuous and finite-limit preserving, which is nice.

Now, I want to mention that there's a paper on the arXiv by Rok Gregoric, and he studied the analog of this with analytic stacks replaced by fpqc $\mathcal{C}$-sheaves, in usual sheaves, fpqc sheaves in usual algebraic geometry over a field. He wrote a nice article, which he recently posted on the arXiv, which goes into some detail about properties of—well, I mean he's working in a slightly different setting, but much of it is the same. I would recommend reading that.

Okay, and then the other example that Peter discussed was an example of a condensed set. Let's start with this setting. Let's say K is a compact Hausdorff topological space. How does one get the associated analytic stack from this?

Well, the underlying object is a light condensed set, but there is no well-defined notion of an underlying topological space of an analytic stack. You can map this to topological spaces, but the analytic stack itself is a generalization of affine schemes, and there's no direct way to extract a topological space from it. Once we introduce normed analytic rings, we'll see that there is a nice way of extracting an underlying topological space, but for arbitrary analytic stacks, it's not so straightforward.

Underlying topological spaces for things over that normed analytic ring, let me continue the story, having unsatisfactorily not addressed the question. So, if you have a compact, Hausdorff, metrizable, and finite dimensional space K, this is the same as saying that K is embedded as some closed subset of a finite product of copies of the closed unit interval. These things are very easy to imagine.

Then, we can view this as a condensed set, in particular, a condensed analytic space. And then, we get an associated analytic stack. Peter made a claim about the functor of points for this analytic space associated to K.

There are two claims. The first is that the most important invariant of an analytic stack is its derived category. We saw that étale descent implies that the derived category of a stack is well-defined by pullback. The derived category of the analytic stack associated to K is just the derived category of sheaves on K with values in derived abelian groups. More generally, if you base change this stack to an analytic ring R, you get the derived category $D(R)$.

The second claim is that if you want to map, say, Spec R for an analytic ring R to this K, this is equivalent to giving a symmetric monoidal, cocontinuous functor from sheaves on the condensed abelian group K to the derived category $D(R)$, such that, étale-locally on Spec R, the functor sends the connective part of the sheaves on K into the connective part of $D(R)$.

The finite dimensionality of K is not a crucial assumption here. You can define the analytic stack for any K without dimension restrictions. I will sketch the argument for this.

The key point is that both the left-hand side and the right-hand side are sets, not higher structures. In the world of analytic stacks, everything is implicitly a condensed object, which could introduce higher homotopy, but the pullback-preserving property of the functor ensures that it is actually a monomorphism in condensed analytic spaces. This means that maps can agree in at most one way, so we have a set, not a higher structure.

On the right-hand side, the space of functors is a set because it is $D(R)$-linear, so it is just the space of cocontinuous functors from sheaves on K to $D(R)$, generated by the representable ones. This is determined by where the functor sends the free things on these representables, and then passing to the complementary closed subsets, it is determined by where it sends the constant sheaves on those closed subsets, which should go to idempotent algebras in the target $D(R)$, subject to some natural conditions.

Construction. There's a, if you have a union of closed subsets, you can kind of use a Mayer-Vietoris to express the constant sheaf on the union in terms of the constant sheaf on the two pieces and the intersection. And that gives kind of an algebraic formula for what the value on the union should be, which you can write down just at the level of idempotent algebras. And you ask that that union goes to that algebraic construction you have here.

And then, and then you still want this condition here, but that's just another condition.

So this looks like a sheaf theory. This looks like the direct category sheaf on K, yeah. Yes, this uses the finite dimensionality, yes, that does.

And then the equivalence with sending various pieces—the equivalence of giving something on the closed subspaces, is it does it use? This doesn't use, no. So when you work with shifts or so far, I haven't used finite dimensionality. If you take this in the sense of Leray, then everything I say works generally, so for an arbitrary topological space K, but certainly for a compact Hausdorff space.

Okay, right. And so the point is that idempotent algebras form a, this is actually just a poset. Um, so there's a priori, it's an infinity category, but one checks that it's just a poset. So a map, if it exists, is unique, you can uniquely write it down. So then we just have, the right hand side is just you have, you just have to give a map of posets to specify all of this—a map of posets satisfying some simple conditions to specify such a symmetric monoidal blah blah blah blah blah functor and so on.

The next thing to note is that, okay, we should now—now I'm claiming these two sets are in bijection, and I should first write a map. And the map, the map in this direction is obviously going to send F to F^*, where F^* is—um, um. So well a priori, D(K), well D(K) is this thing, but we can restrict to the full subcategory where you require the values to be discrete, or I could say $D^{con}(Z)$-linear functors from sheaves on K with values in $D^{con}(Z)$. And then I, so but I should, that gives one of these things, but I should maybe explain why the uh, that's a derived category of condensed abelian groups, light, sorry, light, yeah. Okay, so you have a uh, uh, so F goes to, uh, spec, ah, by definition, spec out. Okay, is H a map from the first? You have to give a map on the ring, but this is part of this functor, the map on rings is is included in KN, the no, so what is it? Your F from Spec R to K means that you have some shriek hypercover of this, uh, and a map of that shriek hypercover to the hypercover of this by profinite sets. Oh, because K is not, okay, yeah, and uh, okay.

And if is totally disconnected, then it is the same as a map, if K is totally disconnected, yeah, so maybe, yeah, if K is profinite, then the left hand side is the same as a map of rings from continuous functions on K with values in Z to just the underlying ring of this, an analytic ring that's by construction. Um, that those are the same.

Okay, and this actually explains why this condition is satisfied, I mean for arbitrary K, because for arbitrary K you can surject from a profinite set T, and then this is a surjection, so by definition there will be some sheaf cover Spec R where you factor through a map to T, but once you factor through a map to T, then your F^* is just being induced by this functor, but this is just a filtered limit of, uh, of, uh, yeah, filtered colimit of C(like K,Z)'s where this is finite, and um, well what do I want to say, the, um, so the, the, so well what what I want to say maybe, yeah, I don't know if that's the right remark to make. So in the case when K is profinite, then, so you want to prove that the image of every conn

Profinite. So, in this case, the connectivity is automatic. And if you want to go backwards, given such a symmetric monoidal functor or such an association with idempotent algebras, it's enough to go backwards, assuming this condition, because you can work shriek-locally, since both sides are shriek-sheaves or satisfy shriek-descent.

Conversely, if you're given such a symmetric monoidal functor, on the connective level, what you can do is take a hypercover by profinite sets. Each of these will automatically be profinite sets because they'll be closed subsets of the product of two profinite sets. Then you get a corresponding diagram of commutative algebras in the derived category of $\mathbf{R}$-algebras. You just take $\pi_0\mathcal{R}\Gamma$ of the constant sheaf on π_1, π_2, and so on. These things are all going to be connective, and in fact, you have a formula that gives that this is the Čech nerve of just the first map.

Now, you can apply your symmetric monoidal functor F, so you can take F of this $\pi_0\mathcal{R}\Gamma\mathbf{Z}$. This is a commutative algebra object, concentrated in degree $\mathbf{Z}$. And then you can say it's actually an animated commutative algebra. Then you take the analytic ring defined by the F of $\pi_0\mathcal{R}\Gamma\mathbf{Z}$ modules, and just the induced analytic ring structure. You can check that this procedure induces a map from the Čech nerve of this $\mathbf{R}'$ mapping to $\mathbf{R}$, such that the underlying $\mathbf{Z}$-algebra maps to the constant sheaf $\mathbf{Z}$, and the complement is the cofiber.

Okay, so where are we? Ah, yes, so then what we get is a map from the Čech nerve of Spec R' mapping to Spec R, to the Čech nerve of t_0 mapping to Decay. And then we'll have produced a map by descent. We have produced a map from Spec R to K.

So as long as we know that this is a cover, okay? But note that this is a proper map by construction. We built it as something where the, so to speak, the completeness condition isn't changing when you go from R to R'. You're just extending the ring. So by the criterion we had for hypersheaves, it's enough to see that the unit object, which is our triangle, lies in the category generated by the image of the forgetful functor $D(R') \to D(R)$. And for that, by applying F, it's enough to see, using the lower shriek, which is the same as the lower star, so it's really just a forgetful functor, that the image, this means the image closing by cones and retracts and finitary operations, yeah.

Okay, then applying F, it's enough to see that the constant sheaf on K is in the subcategory generated by the lower star functor, so sheaves of t on T values in $\mathbf{D}(Z)$. Okay, or in Ila's language, we need to see that the lower star of the structure sheaf on t_0 is descendible.

Now, remember that we could choose this arbitrarily, so it's actually enough for me to produce a cover by a profinite set for which I can check this descendability. We can reduce, by pullbacks, to $K = \{0,1\}^n$, and then I'll discuss just the case $n = 1$ because it's simpler to write down.

You can also reduce to this case, yes, that's true. This means when you take the fiber, there's some... We didn't get into much details about how to make these sorts of arguments, but Ila gave some toolkits which are nice. So then in this case, you take the usual kind of Cantor set. So I'm going to produce my cover of the closed unit interval by the Cantor set. What I can do is take the two halves of the closed interval, and their disjoint union is a space mapping to the unit interval. And then we can do the same thing again, the base to the power of two, and so on.

Then we have a sequence of spaces mapping to the unit interval, where in the inverse limit, you actually just get the Cantor set, which is then forming a cover of the closed unit interval. So what does this mean on the level of these pushforwards? These pushforwards from the Cantor set will then be the sequential colimit of the pushforwards from each of these.

Now, there's a general fact about this descendability: if you have a sequential colimit, it's enough to establish descendability with uniform exponent of nilpotence for each of the finite ones. And for each of the finite ones, you have descendability basically because you have a Mayer-Vietoris cover for the closed subsets, and the reason you get a uniform bound on the exponent is because in each case, there's only double intersections that you need to be concerned about and no triple intersections. So in the end, you get something like exponent of $n + 2$ for each of these individual things, and then 3 or 4 for the colimit or something like this.

Okay, now let's come back to the question about E_∞ versus animated commutative... About E_∞ versus animated commutative algebra. My apologies, let's actually move on. I want to make a remark at the end of this.

Maybe it's good to say that at least in this example, we can do the H_e structure by hand, because you only have to do it for the sequential part. Yes, that was the argument I was going to suggest, and then I was thinking in my head about why for idempotent algebras it's automatic, and it wasn't quite clear to me. But I think you can do it by hand if you know the categorical structure well. We should think more carefully about exactly how to do this and report back next time.

But let's move on. I want to make a small remark before we move on.

Remark: By similar arguments, or basically the same arguments, if you have a functor like this, it's equivalent, a priori, to saying that you have this connectivity estimate on Spec R locally. But it's equivalent to ask that over a proper cover, a cover by proper maps, you get the connectivity. So you don't actually have to, when you're working over a ring R, give you a ring structure on R to get this connectivity.

Could you say that one more time, Peter?

Yes, then you can use this to define a new notion of complete modules, which are modules over the... Yes, I see. Indeed, yeah, yeah. But are all of the intermediate guys connective as well? Yeah, I guess they are. Well, okay, yeah, so maybe connectivity isn't even important there anyway.

Alright, oh yes, and another remark is that this will actually be a little bit useful now. I've not... Sorry, oh, we're over here in the setting of number 2 in the theorem here. Okay, so let's say you want to produce a map like this by this procedure. You want to produce a symmetric monoidal functor, and then you have to check this annoying condition that on some étale cover of Spec R, you have this connectivity condition. I'm saying, or, let's... No, I'm making a different claim. Sorry, that... Let's say you have a map like this, then you know that étale locally, you get this. But in fact, what we see in the proof is that after a proper map, even after just a proper map from a proper cover of Spec R, you can ensure this condition. This comes from after proving the equivalence, yeah, yeah, yeah, not a priori, exactly.

Another remark is that if you have Z closed inside K, or this is actually more general, but, and then U is the complementary open, then they are also each other's complements in analytic stacks. So these two do determine each other, the associated analytic stacks do determine each other in the naive way. I.e., if you're given Z and you want to know U as an analytic stack, its functor of points is you just map to K such that when you pull back to Z, you get the empty analytic stack. That's the same thing as mapping to U, and vice versa, so you can check on profinite sets where it's quite elementary.

Okay, so now let's get to... Sorry, why did I do that? How else did you want me to write them? Only what it means doesn't mean anything. It's just because the... Well, I wanted to write, for example, I wanted to write this one, this one right above this one, because I'm saying the these two end points map to the same point down here, so I wanted to stack them vertically like that. Maybe that's the reason, does that make sense?

Yeah, yeah.

Okay, so now we get to a topic that I think is fun, which Peter introduced last time: this kind of norms on analytic rings. So let's say that R is an analytic ring. Definition: a norm on R is a map of analytic stacks from the algebraic $\mathbf{P}^1$ over R, which is something you can build over any analytic ring by just base change from the trivial case with algebraic geometry, to the closed interval from 0 to ∞, which is a condensed set and thereby an analytic stack. Let's call this map n. And then I'm going to give some conditions, which are going to be different from the ones that Peter gave last lecture. So a priori,

Right. The first condition is that, when you restrict the norm function on $\mathbf{P}^1_R$ to $\mathbb{A}^1_R$, you get a norm function on $\mathbb{A}^1_R$. But then what does it mean when you have an element of R? Do you get a real number or an element in $[0,\infty]$? No, you do not get an element in $[0,\infty]$; you get a map.

Note that if you are given an f in R, that induces a section from $\mathbf{P}^1_R$ to Spec(R). Then you can compose that with the norm map to $[0,\infty]$, and what you get is a map from Spec(R) to $[0,\infty]$. Exactly.

Now, suppose you start from an obstruction. We said there are several ways to view it as an analytic object, and for each of those, you have a notion of G. On the other hand, you have a geometry, like in Berkovich or like this. Can you say what the relations are? We will discuss these things later, but I want to get the basic definitions and results in place first.

Yes, so a norm for an element of the ring, you don't get a real number; you get a map from $\mathrm{Spec}(R)$ to the non-negative real numbers plus infinity. You can think of this as consisting of a family of residue fields, and for each residue field, you get a real number, but they could be varying with the residue fields. Relatedly, if you have a norm on R and you have a map from R to R_P, you get a norm on R_P just by composition. So it's really a geometric thing; it's something you can pull back, and it still persists. That's important to realize.

Okay, right, so that's a prelude, and then the first condition is that the norm of zero, which is a map from $\mathrm{Spec}(R)$ to $[0, \infty]$, should factor through the terminal map to the terminal analytic stack, which is also the analytic stack associated to the condensed abelian group which is the singleton point, which is a subset of here. So this is a condition that the norm of zero is the constant function zero. By the way, in this geometric perspective, there's sometimes a question of whether the norm of one should be equal to one or zero for the zero ring, but this is avoided here because when you have the zero ring, the norm is both one and zero, because then this is the empty set, and the map factors both through zero and through one. And so in this geometrical perspective on norms, there's no way to get messed up.

Okay, wait, sorry, over here, there's no—we're not saying anything about what happens to $\mathbb{A}^1_R$, just let me finish.

The second condition is that the following diagram commutes: $\mathbf{P}^1_R$ maps to zero in the second and third conditions. I'm going to try to say the norm is multiplicative. The first thing I'm going to say is that if you have inversion, so here we have λ goes to λ^{-1}, which exchanges zero and infinity, we also have let's call the coordinate in $\mathbf{P}^1$ T, and we also have T goes to T^{-1}, exchanging zero and infinity, and I want this diagram to commute. By the way, the maps from anything to $[0, \infty]$ of a space like k or $\mathbb{A}$ is just a set, yes, and even with semi-compact ones, this was said before.

Okay, so conditions one and two. Now note that one and two imply that if you take the infinity section, so, let's say, the norm of infinity, this factors through infinity. Okay, now before I—yeah, and now, yeah, okay, maybe three. Set, I don't—it's one, right? Ah, does this already imply that?

Infinity. Then we have that if you take $\mathrm{A}^1_{\mathbf{R}}$ analytic cross $\mathrm{A}^1_{\mathbf{R}}$ analytic, and then you have the Norm. Oops, oh I guess this is just $\mathbf{R}$ but okay, I'll continue to write it as ∞. Infinity. Then here we can take the product, so and we still get something in the region from zero to ∞ which is contained in $\mathbf{Z}_\infty$ closed.

On the other hand, this we can map to $\mathbf{P}^1_{\mathbf{R}}$ cross $\mathbf{P}^1$ via multiplication. TS goes to St, or TS, this is the multiplication M on $\mathbf{P}^1$ is not defined in general. TS, the norm inverse. Why, I'm going to justify this afterwards, I mean, so yeah, so actually, well, yeah, that I'll justify why this map is well-defined at the end.

Right, so this map should commute, so that's saying that the norm is multiplicative, but as Peter is pointing out, one needs to justify that such a map indeed exists. Let me do that now.

So the claim is that $\mathrm{A}^1_{\mathbf{R}^n}$ is a subset, well, a submonomorphism admits a monomorphism to $\mathbf{P}^1$ by definition. Another thing that by definition admits a monomorphism to $\mathbf{P}^1$ is the algebraic affine line over $\mathbf{P}^1$, and the claim is that this one's contained in this one. $\mathrm{A}^1_{\mathbf{R}}$ is just the polynomial ring in one variable over $\mathbf{R}$, without any special structure in the category. It's an affine analytic stack, and it's given by keeping the same class of complete modules you already had in $\mathbf{R}$ and just adding the polynomial variable as operators.

The point is that we want to produce a map from $\mathrm{A}^1_{\mathbf{R}^n}$ to $\mathbf{P}^1$. We have to check it on the two charts of $\mathbf{P}^1$, one of which is already $\mathrm{A}^1_{\mathbf{R}}$, and the other one is the other $\mathrm{A}^1_{\mathbf{R}}$ to some inverse operator.

So here's what I'm going to say. We know that the Norm of ∞ is equal to ∞. This implies that the ∞ section of $\mathbf{P}^1$ is an algebra over the Norm upper star of the structure sheaf of ∞. One way to think about it is that, in the set of schemes where the scheme is endowed with the trivial analytic structure, there is a general statement about this.

Let me try doing it the way Peter was suggesting. Let's abstract a bit, let's move ∞ to zero and abstract a bit, do it for $\mathbf{P}^1_{\mathbf{Z}}$. It means you can this $\mathbf{P}^1_{\mathbf{Z}}$. Suppose given an analytic stack over A^1 such that if you pull back to the zero section, you get the empty set, so it misses the zero section. Then the claim is that this $\mathcal{F}$ factors through $\mathbb{G}_{\geqslant}$ over $\mathbf{R}$.

The reason for this is you can think of maps like this in terms of symmetric monoidal functors. If X is $D(\mathbb{A})$, then you can think in terms of the corresponding pullback functor from $D(A^1_{\mathbf{R}})$ to $D(\mathbb{A})$. And what do we know about this pullback functor? We know that it kills the structure sheaf of the origin. But then it's just a purely algebraic fact that if you kill the structure sheaf of the origin, then you factor through inverting the parameter. So the structure sheaf of the origin is just the structure sheaf on $\mathbb{A}^1$

"Yeah, so thanks Peter. I think that's a much nicer way of saying it." Everyone, okay. So, that's the claim, and that implies that this map is well-defined, because certainly the multiplication is well-defined on $\mathbb{A}^1$. But maybe then I could put the $\mathbb{A}^1$ here a priori, and a priori have only the $\mathbb{N}$ going here. But then a posteriori, if I require this diagram to commute, then it follows that this actually lands inside $\mathbb{A}^1_{\text{analytic}}$, because by definition that was the pre-image, because this map factors through ∞.

Okay, all right. I want to get to something fun. "Yes, please, no, no, please, please." The fact that we denoted $\mathcal{I}$ and the norms here should somehow correspond to norms in analytic ification. What? Yeah, so I'm going to give some of the motivation at the end, but let me finish with the axiomatics.

I remember last time, I think Roch said that Gaga was really like, he noted down Gaga as an isomorphism of stacks. And somehow that was some sort of analytic ification. So does that also correspond to a norm then? This question, I suggest you keep for later.

Okay, yeah, let me finish with the axioms. 1, 2, 3. Ah, so four. Okay, so axiom 4. Now, maybe now's the time to say a bit about motivation. So, what if you have an analytic ring? What we're going to try to do is we're going to try to say if you have an analytic ring, you want to try to build some geometry over that ring. But it's hard if you're just given an analytic ring and you don't know anything more about it or you don't have any extra structure on it—it's kind of hard to build analytic geometry over it. I mean, basically all you can do is you can do this trick of importing algebraic geometry for an arbitrary analytic ring, that's more or less all you know how to do.

What we're going to be doing, and what—well, one measure that you have some good analytic geometry is that you have some nice subsets of the affine line. And nice in the context that we're discussing here means, for example, sheafable, so that the six functor formalism works, and then it kind of really feels like you're doing geometry in some more or less traditional sense.

But again, on a general analytic ring, you don't know how to write down any interesting sheafable subsets of the affine line, so you have to give yourself some of them. And that's the point of this notion of normed analytic ring—we're giving ourselves basically discs of certain, of some arbitrary radius inside the affine line. And they will turn out to define sheafable subsets of the affine line, and then we can get started on doing geometry that resembles some sort of traditional analytic geometry based on open discs or closed discs or what have you. But you have to have this extra structure on your base before you can get started on that game. If you don't have a notion of a norm on your ring, you can't start talking about closed discs and open discs of certain radius. So that's what we're doing right now.

But we already have some sort of things that kind of seem to function as a—we've already seen certain versions of the unit disc. For example, we spent a lot of time discussing this

Measure concentrated at 1. Okay, you use the multiplication on $\mathbb{N}$ or you use the addition on $\mathbb{N}$ to give the multiplication on $\mathbb{PR}$, yeah.

So, right, so in particular you get a factoring like this. This is generally true that you have a diagram like this, where this is the canonical map. In most examples, this $\mathbb{PR} \to \mathbf{R}$ mapping is a monomorphism, an injection. So in most examples, this lives in degree zero and this map is an injection. This is some kind of sequence space.

What is the condition that the sequence satisfies? Well, in the abelian category, which is the heart of this discussion, in most examples this lives in the heart, and this also in most examples lives in the heart. I mean, I guess maybe I should be using this notation, but I'm being a little bit sloppy here.

Yeah, and then this is just an injection, so to speak. So, in most examples, $\mathbb{PR}$ is like the set of sequences $r_0, r_1, \ldots$ satisfying some summability condition, such that if you termwise multiply by a null sequence, you get a summable sequence. The notion of summability depends on the analytic ring structure.

But at the level of this discussion, you could imagine, for example, $\mathbf{R}$ being the real numbers and summable means usual absolute sum, the absolute values you get a finite number. That's not actually a special case, but it's close enough and it serves the purposes for this discussion.

And this is kind of just by the universal property of $\mathbb{PR}$ that this is the correct interpretation, because $\mathbb{PR}$ is maps out of $\mathbb{PR}$ to an $\mathbb{M}$ in the abelian category. These are supposed to correspond to null sequences in $\mathbb{M}$ by construction.

So, you know, it's the kind of thing which when paired with a null sequence, you get an element in $\mathbb{M}$, and so the idea is this procedure. Well, if you well, I'll let you maybe, maybe me trying to explain it is not as helpful as all that is any analytic ring $\mathbf{R}$, but this is not precise mathematics here.

Yeah, but in this norm business, $\mathbf{R}$ is just a discrete ring. In the norm business, $\mathbf{R}$ is an arbitrary analytic ring, $\mathbb{M}$ is then, $\mathbf{P}$ is a... Okay, so you say that usually it's, say, in an object in the category of $\mathbf{R}$, yeah, usually that's right. And this is the what you call $\mathbb{M}$, $\mathbb{M}_0$. Sure, well, I mean, yeah, I could, yeah, I mean, so I was saying that this is the interpretation you get in practice, where the notion of summability depends on the analytic ring structure, and the way you see that this is the correct interpretation is by thinking about what it means to map $\mathbb{PR}$ to $\mathbb{M}$.

And so, like, for example, the most basic thing was, it would be if you map $\mathbb{PR}$ to $\mathbf{R}^{\triangle}$, then that's the same thing as giving a null sequence. But then, if you think in terms of what happens when you restrict to here, you have some coefficients in a polynomial. If it terminates, if you have zeros after a while, then what you're doing is you're just summing the null sequence times those things to get an element in $\mathbf{R}^{\triangle}$, and then you imagine that that summing should make sense for something which is not necessarily eventually zero, and so this is kind of the interpretation you should give that.

Right, and what, so, and so if, for example, $\mathbf{R}$ is $\mathbf{C}$ with the Gauß or Liouville analytic ring structures, what you see is that, if you look at holomorphic functions on the usual ring of holomorphic functions on the closed unit disc, meaning they converge on the closed unit disc, then every one of those satisfies this summ

Okay, what's our goal with all of this analytic geometry? For me personally, one goal since 10 years or so has been—there's all this fancy geometry that has been developed p-adically, like perfectoid spaces, prismatic cohomology, and p-adic shtukas, and the geometrization of local Langlands. This all works quite beautifully over the p-adic numbers.

So, condition four is split into two parts. The first part is that if you have a map from D or D_r to $\mathbf{P}^1$ mapping to 0 and ∞ via the norm, you want this to factor through the map up to $[0, 1]$ that corresponds to this, saying that this notion of the unit disc is sandwiched between closed and open.

This is a stronger condition than just saying you have a map like this, and the nice thing about this stronger condition is that, first of all, you can check it in practice, and second of all, it's just a condition, whereas giving a map like this is a priori structure.

We can think of this norm as non-archimedean or archimedean at will, as we haven't enforced any compatibility of the norm with addition. Last time, this was a cosmetic change from Peter's lecture - he did not mention this axiom, and we're not sure whether it's a consequence of the other axioms. If the base solidifies to zero, like over the real numbers, then this condition can be proved from the other axioms. But for other bases, it's an open question.

So, what is a norm again? If you have a norm on an analytic ring, this gives you, for example, the constant sheaf $\mathbf{Z}_r$ on $\mathbf{P}^1_r$, which should be interpreted as the algebra of overconvergent functions on the closed unit disc of radius r. The overconvergence is built in, as this is the filtered colimit of the constant sheaves $\mathbf{Z}_{r'}$ for all $r' > r$, under the restriction maps.

Suppose we have a disc of radius R. Some version of this disc of radius R would be forced to be the over-convergent one, because of this property. And because it's a pullback functor, so it commutes with colimits.

Okay, so the data of these guys, given the axioms, this determines n because it's very easy to classify the closed subsets of the interval. You only need to know about things less than or equal to something and things bigger than or equal to something, but the axiom about inversion gives you one in terms of the other. So you just have to give these things, subject to some simple conditions, in order to specify a norm.

The next topic is classifying norms. I don't mean like as in classifying spaces or whatever, I mean how to classify norms on a given analytic ring. So, let me start with one lemma.

Given a norm on an analytic ring, you can just check that the usual algebra, geometry, or theory of the complex numbers does satisfy these conditions. So this way you can produce, by hand, such norms over $\mathbf{Q}_p$.

What does "over-convergent" mean? It means that an over-convergent function on a disc of radius R is a function which converges on some disc centered at the same point with larger radius. So it extends to a function on an open neighborhood of the closed disc.

Given a norm, you can look at the locus where the norm is strictly between zero and one. This projects down to Spec R, and I'll say that this is a cover in the graded topology we've been considering. Essentially, if we're willing to work locally, we can assume we have an element Q in the underlying ring such that the norm of Q lies strictly between zero and one.

In fact, the stronger claim is that if you take the preimage of $1/2$ under the norm, this is a cover in the graded topology. This is not necessarily an affine cover, but it can be refined to one. For simplicity, let's assume that this is affine, so there's an argument for getting the conclusion anyway based on resolving this by a profinite set and using the descent ability that was proved earlier.

Assume that this norm inverse of $1/2$ is connective, so it's affine and proper over $\mathbb{A}^1$. By the descent ability criterion, our triangle lies in the algebra generated by this. But I claim our triangle is actually a retract, directly a retract without any cones. The intuition behind this should be clear - this is some version of a Laurent series ring, and you can just pick out the zeroth coefficient of your Laurent series to get a linear map which splits the unit. But we have to make sure it holds in this completely abstract setting.

Setting here, so what you do is you take this structure sheaf of $\mathbf{P}^1$. Then you can take the norm upper star of $\mathbf{Z}$. Okay, so this is a pullback in $\mathcal{D}$ of $\mathbf{P}^1$, just because the constant sheaf on this interval is the pullback of the constant sheaf on $[0, 1]$, constant sheaf on $[1/2, 1]$ glued along the constant sheaf at the point.

Then we apply push-forward to $\mathcal{D}$ of $\mathbf{R}$. The cohomology of the structure sheaf on $\mathbf{P}^1$ is just $\mathbf{R}$ concentrated in degree $\mathbf{Z}$. So what we get is an $\mathbf{R}$-triangle. I'm going to use the same notation, or just push it forward to the other $\mathbb{A}^1$.

There's a pullback in $\mathcal{D}_{\mathbf{R}}$, but it's also a pushout in $\mathcal{D}_{\mathbf{R}}$. If I want to make a map from here to a certain place, for example to our triangle, I make a map here and a map here and make sure they agree there. So what I do is here I take evaluation at 0 from our triangle, which goes to $\mathbf{R}$ triangle, and here I take evaluation at infinity, which also goes to our triangle. They clearly both give the identity map when you restrict to $\mathbf{R}$ triangle, so this gives the map here which when you restrict back to here is the identity.

That produces the desired retraction. As for the connectivity, there's a proper cover by something affine, and not just proper but also descendible. The unit here is generated by the image of this, and it follows that the unit here is generated by the image here, which provides the desired refinement by an étale cover of affines.

Now let's give ourselves this extra data which we've assured can be gotten locally. Given a norm $\mathbf{P}^1_{\mathbf{R}}$, norm 0 to infinity, and a q in $\mathbf{R}$ such that the claim is that q is given by a map from $\mathbf{Z}[t]$ to our triangle, or to $\mathbf{R}$. This factors uniquely through this gaseous base ring. The proof is that spec $\mathbf{Z}[q]/(q^2 - 1)$ maps to the affine line over $\mathbf{Z}$, and this is a monomorphism. There's a contractible space of factorisations if one exists.

Exists. This is not obvious from the definition of the Gauss base ring, because by definition of the Gauss base ring, we took $\widehat{\mathbf{Z}}_q$ which was just $\mathbf{P}$, and then we inverted q and then we enforced this q being Gauss. And this is not idempotent, so not idempotent. So enforcing the condition of being Gauss, what defines a monomorphism on the level of analytic stacks, because it's just some quotient of categories. But because this is not idempotent, it's really not clear from that description that the composite map all the way to $\mathbb{A}^1$ should be idempotent, because it seems at first you have to choose something which is extra structure, namely a proof that q is topologically nilpotent, so to speak. But you can do it in the other way.

Instead, you can also think of this as $\mathbf{P}$-modules or $\mathbf{P}$-modules. You just take a polynomial ring in one generator, and make that generator Gauss. Oh, and then maybe I have to invert q too. Okay, just so. And then the claim is that $\mathbf{P}$ is idempotent in $\mathcal{D}(\mathbf{Z}_q[\frac{1}{q}])^{\mathrm{Gauss}}$. And this actually, so we want to check that $\mathbf{P}^1 \cong \mathbf{P}$, that's actually a claim that happens after base change to $\mathbf{P}$, so to speak. So it actually reduces to a calculation here, that if you take this $\mathbf{P}$ over $\widehat{\mathbf{Z}}_q[\frac{1}{q}]^{\mathrm{Gauss}}$, now we have both a q variable and then we have a t variable say, coming from the $\mathbf{P}$ here. So that if you take this and you mod out by $t - q$, you just get the underlying ring.

Yes, you should still write Gauss, because completion changes the... Oh, okay, sure, yeah, the underlying ring of the analytic ring structure. And Peter described this free module, and if you use that description, you will find yourself, you're just, you have to check a short exact sequence and you can do it. So that's the uniqueness.

As for the existence, well, let me just say it in words. It's fairly elementary from the definition of the norm. So because our norm is contained in here, this means that we're away from the inverse of 1 at infinity. And that in particular means that we're away from the $\mathcal{D}$ sitting at infinity. If you take this $\mathcal{D}$, the thing that was $\mathrm{Spec}(\mathbf{P})$, and you translate it to infinity on $\mathbf{P}^1$, then that's going to be contained in this locus. But then saying that you're away from the $\mathcal{D}$ sitting at infinity is exactly the same thing as saying that q is Gauss. But then on the other hand, the norm of q is contained in the closed interval $[0, 1]$, because of the axiom I'm referring now to the axiom 4 about the placement of $\mathcal{D}$ in this norm. This implies that q comes from $\mathcal{D}$, so q is topologically nilpotent. It comes from $\mathcal{D}$, so $\mathcal{D}$ was this Spec($\mathbf{P}$) mapping to the affine line, it therefore maps to $\mathbf{P}^1$, and then you can pull back that map under the automorphism of $\mathbf{P}^1$ which is the inversion map, and you get something abstractly isomorphic to $\mathcal{D}$, but the structure map to $\mathbf{P}^1$ is different.

Okay, I'm almost

The open interval (0, 1). Yes, it's not—oh, thank you. And this is by recording q and the norm of q. This map is an isomorphism, i.e., giving a norm on an affinoid ring plus an element of norm strictly between zero and one is the same thing as specifying what the norm of that element should be, which is this function here. And requiring that that q actually come from this $\mathcal{G}as_K$-theory that we discussed earlier.

Both sides are "sets", no—almost, well, except for the annoying fact that $\mathbb{A}^1$ is not a set. So the norms on q: the first thing we said is that the norms really is a set, because there is no—and the q of course is something in some set, no, no, the q is not in a set because this could be derived. So both of these map to $\mathbb{A}^1$, and like the fibers of these maps are sets, so they're as much sets as each other. But they're not each individually sets. Ah, so q is a derived—ah, okay. But you still think of q as choosing something in the zero part of the simplicial—yeah, yeah, yeah, yeah, implicitly the higher simplices are there also.

So let me give the proof. After a étale cover, we can assume q admits all nth roots, compatible nth roots for all n. This is because the étale cover is countable in the fpqc topology. So we're free to work locally. We're free to assume that q has all nth roots. Then the claim is that we have a morphism $\mathbb{D}_q \to \mathbb{A}^1$ given by multiplication by q. And then we have $\mathbb{D}_q$ here. This makes sense for any q in $\mathbb{G}_m$. Note that if $\alpha = q \cdot \beta$ or q is topologically nilpotent, then we get a map from $\mathbb{D}_\alpha$ to $\mathbb{D}_\beta$ induced by multiplication by q. Here we use that $\mathcal{P}$ is a Hopf algebra encoding multiplication.

Right, I may have gotten the ordering wrong. This is some disc of radius q^{-1} or some version of a disc of radius q^{-1}. It's not the correct one because it's not a monomorphism and it blows up along the boundary, but then we can fix that. So $\mathbb{D}_q$ or $\mathbb{D}_\alpha$ gives a disc of radius α^{-1}, and the maps above give the inclusions between such discs.

Now we apply this with $\alpha = q^{m/n}$ in the rational numbers, and we get discs of radius $\|q\|^m$. We can freely assume the norm of q is constant equal to $1/2$ by rescaling the norm. Then you use these to make the over-convergent versions for an arbitrary real number—you can look at all the rational numbers bigger than it, you have these kind of "fake" discs of that rational radius, and then in the colimit all these problems.

About blowing up along the boundary, it becomes the thing that has to be specified if you're given a norm on $\mathbf{R}$. When you make it over convergent, it will be forced to be equal to the thing that comes from a norm if you have a norm. So, that's more or less an argument why this is a monomorphism. And then, if you want to check that it's a bijection, you just have to produce a norm on here and you do it by following this procedure. You can even adjoin all the roots that you want.

So, you take this "fake disc," you expand it by multiplication by powers of Q, and then you build the inclusion maps between those discs. You make them over convergent, and then you check that you have all the axioms. Sorry for going over time, thanks for paying attention.

In this inclusion, we also add an analog and discuss Huber rings of power-bounded elements. If Q is power-bounded, it's not entirely clear in this general setting what the class of Q is for which you get such a map. For example, this argument here doesn't prove that you have an identity map when $Q = 1$, but obviously you can take $Q = 1$.

For roots of unity, we know that the norm is 1. So, if you have a $Q^{1/N}$, its norm has to be the Nth root of the norm of Q. This implies, in particular, that roots of unity go to 1.

Okay, see you on Friday. As for the intuition for not being able to do geometry unless you choose a norm, I don't want to make such a strong claim. But if you want to define a notion of geometry that resembles usual complex analytic geometry, which is based on open and closed discs, then you need something that measures the size of a radius of a disc, and that's what the norm does. You could choose different norms, but then you'd get something basically equivalent to the usual complex analytic geometry.

So what's our goal with all of this analytic geometry? For me personally, one goal since 10 years or so has been—there's all this fancy geometry that has been developed p-adically, like perfectoid spaces, prismatic cohomology, and p-adic shtukas, and the geometrization of local Langlands. This all works quite beautifully over the p-adic numbers.

So actually, one thing we're going to do is we're going to mod out by—at some point, we're going to mod out by these sort of exponential rescalings of the norms. When you do that, you can't talk about a disc of a fixed radius, but many other things still work okay.

Yes, we discussed this many times. So if you have an affinoid universe, you choose, let's say, the absolute value on **Z** and 1, then you get a norm on this. And in particular, in each residue field, you get a non-archimedean norm. Yes, and this is like choosing this collection of norms.

Yes, so it is not at all the same as, so, say, for another field, it is related to the previous national norm, but for a more global object, it is very far from, like, the Benvéniste space on just one, it's quite a different way to think of norms. This is the definition, yes. And so you, and presumably what they said is an equivalence to take Huber rings, the national norms, and your sense is the same as a way to, of course you can, should it be continuous?

Exactly, continuous on the vector of rescale a given one, so it is a torso under scaling of the fixed one, that's precisely correct. And it sort of, uh, sort of follows from this, uh, okay, this. Okay, so in this example, this is what you get. And of course, then you can, uh, and of course for other analytic Huber, just locally, you can do the, I mean, you can, and for non-analytic Huber, this is something different—well, it's kind of, it's ruled out actually. I mean, you have to be Tate, you have to be analytic. I mean, we were saying Tate instead of analytic in this class, but you have to be Tate by this result if you have a norm. I mean, then locally for discreet guys, you are not having any non-trivial, any of exactly. That's right.

But what we're going to see is that if you mod up by rescalings, then there's a way to—there's a way to extend this. So when you mod up by rescalings on the norms, they form an analytic stack, by the way. So it's a it fits the definition of an analytic stack, sending R or Spec R to the set of norms on R, that's an analytic stack. And you have to put covered by S, yeah, I just did. Okay, okay, yes.

And but, and it is such that, like, discreet Huber things can't, can't map to that stack, but there's an enlargement of the stack which also accommodates solid **Z** and anything living over solid **Z** and so on, which we'll probably discuss. So there was this thing, like in the theory of diamonds, that you want to get from an analytic—like in the p-adic setting, you get a diamond from analytic, but if you have a non-analytic, still you can look at maps of antic, and then get that kind of, there was this non-quasi, I forgot now how, maybe it's not related to this, but somehow looks, I'm not sure what you're referring to actually, because there was a diamond only allowed Tate-Tate objects, so even if you start with a discrete one, I still only remember how Tate things than, okay

21. 6 functors for analytic stacks (Scholze)

https://www.youtube.com/watch?v=R5JNomeHjtI&list=PLx5f8IelFRgGmu6gmL-Kf_Rl_6Mm7juZO

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The main thing I want to talk about today is the form of six funs for step. But before I get there, let me try to say something that Dustin kind of said last time and that I kind of also said last time, but we both didn't quite really do - to do the construction.

First, the construction of the norm on the SK fa. Conveniently, Dustin kind of already explained that we can assume we actually have all the roots of Q as well. And so then we want to define the algebra that corresponds to the locus where the absolute value of T is at most R and, or like the over convergent functions on this disc. As Dustin explained, the ex of Norm some tell you that this has to be like there's an if guess for what this might be. So this might be like this free algebra on the top element t or scale suitably so that the norm is equal to R, but that's not quite right, but it's almost right. But when you pass the over convergence thing, then it actually becomes exactly correct.

I will construct the norm for which the absolute value of Q will be a half. I want to take this here A and so I want to take a p a module on a top l element where I rescale T by some power of cube. And so this should be such that this definitely converges here. We want that we should secretly think that maybe the norm of t r, and then we want the norm of Q to the T * T this should be bigger than one. But okay, so the nor of Q is half, so a half T * R should be bigger than one. And now you can do the algebra. This means that 2 to T must be less than R, so you take the co liit of all t for which two to T is less than R.

This is a condition that if R is very large, say, then we should multiply by a large power of Q to make it somewhat small. Okay, was there a disagreement or just commiseration? Okay, thanks. So this has is the only possible choice is what you explained last time.

And so what we have to see is that these unimportant have all the required properties to Define well, first of all, you have to see this Al item and they plus the anog for G inverse and inin Define map. Okay, now you can replace T by T inverse and then the T supp Infinity.

Okay, so here are some ways to and I mean all of these are some kind of simp computations. Let me just check that the locus where it's at least R and the locus where it's bigger than equal to to S where R is less than S this would be empty, and for this you have to compute something like you take a and then join to T * T head and say noers. And then you can draw in T and the S time T head inverse. You want to show that such a thing is zero for if say, and one situation says R is less than one after rescaling, you can assume that some one is squeezed in between them. And so then you can assume that t is negative and S is also negative.

But in particular here, you have a map from two toping elements which are X goes to Q to the T3 and Y goes to Q F and say satisfies relation that if you multiply the two you get something the negative power of s, so Yus Q to the some axy is equal to zero where a is some positive number. But and in turn if you just remember the product, so then you also have this is an algebra over a Jo C Aus a * C, but this is precisely I mean a join Z head, this is precisely this projective generator that we always use, and then here you're doing oneus Q. To the a * shift, and if a was equal to one, then we precisely declared this to be an isomorphism on the projective generator in the gas is ring structure. I also explained somewhere that the power doesn't really matter when you do this, so this implies the pr fractional power. So, this is actually zero, and this is really by the definition of the right. So, the minimal thing we really need to get this map is that we know this, which shouldn't intersect, and this really exactly comes down to the thing we enforce and the guesses. Then, there are some other things you have to check, and none of the computations is hard.

So, those, and then you have to check they cover that the guys like the they need to check the cover, but again for this, you just have to show that. Actually, I will probably come to this later in the course again, but I mean basically, you just have to, if you say we SCT to the Fine Line, then you just have to show that. Then, you expect that there is some they form a cover, and so they expect that there is some St like sequence like functions on the fine line just pooms going to functions on a dis and functions on functions on the opposite dis and then functions of the overlap, and for all of these things, you can just write down what they are and then just check that you get an exact sequence, and this implies the covering condition because it implies that the polinomial are generated by in a finary way by modules over over the ah, okay. So, you just have to check a cyclicity like the analog of data cyclicity, you just have to check theity.

I mean, this is enough to check the covering conditions. By the way, the reason that we really want to use this growth topology that we chose, I think there's another perspective here that makes some of these things easier, which is like instead of using this free thing on a topologically nil poent, it's like the free think where you make t topologically nil potent, which is not really like a subset. You could use the subset where te becomes gaseous like an analytic ring that's not induced, but they're all sandwiched between each other, so for the over convergent thing, it doesn't matter. Then, at least you have honest subsets, and then it's like kind of easier to check which subset. Oh, like I'm saying, there's a instead of using this thing I was calling D as your basic dis, you could instead use an analytic ring which is the analytic ring gotten by forcing the coordinate T to be gaseous in the sense of like one minus t * shift. What was D? Oh, D was just the spec of the pr this array which is this U, let me so I keep my notation keeps this notation, which to the line and then projective line. So, that's one thing you can consider. On the other hand, you can also Define the following funny analytic ring. So, you can take the polinomial and then make t guessers in the sense that you enforce 1 minus t * shift as usual on Project generator to be Aus. What is sh here? I mean, on this t-basis change to this ring, and so this is also an analytic R, and you can also take the aspect of this, and this is actually a Subspace of of P1 over a, and it's again a Subspace that will behave like like dis behave, so it will be training Norm, it will be contained in the locus, so it is contained in Long inverse of 01 and it contains Norm inverse of the op of open inter relation don't each other. Well, what one you can't say contain, and you could even consider a third version which would be the intersection, so you can also intersect these two. Then, something that Dustin mentioned last time is that actually, if you intersect these two, this map becomes an embedding, and also, this one becomes an embedding. You can also consider a Jo T head guess, and this is also true here. So, all of these three things, there are some slightly different versions of what a dis might be, and there also sandwich between like the over converging close unit disc and the open unit disc.

In particular, one way to verify the item potency without really sort of without doing any calculations is to see that the this da and this npec with the T gases are are sandwiched in between each other in this co-limit, and then it formally, since for one of the terms, you always have a monomorphism, it follows for the other term, you mean, there one thing you really have to check which is this that these two do not intersect. I think Clever way to arrange the argument. You can avoid them, but yeah. Yeah, so if the radii are the same, they always intersect. Any disc around zero and one around infinity will intersect. Okay, for all versions of the story. Okay, yeah. I mean, it well, it depends on which base you're working over, okay, but over the universal base. Yeah. Okay, so all right. Um, yeah, so let me start by talking about six functors.

Let me first recall that on an analytic space, we had a functor that was taking an object a to $d(a)$ and we also considered a different functor which was to look at the representable stable infinity line over d. This was a functor from the category of rings towards symmetric monoidal infinity categories. In particular, the commutative algebra structure here has insisted on some T functor, and actually, there's also always in both cases they're closed, so there's an internal Hom, and it's also functorial in the ring. This gives you, in particular, for any map of rings, a base change functor, which geometrically is always f^* and f_*.

These are four of the six functors, and then we defined this class of free maps, and for these, we had also a functor of lower shriek and upper shriek. Why does he move discretely? Because of the, the, yeah, something, a one-picture instead, yeah, another high quality. Camera usually, like, you don't see, right, exactly. So, we, right, um, so one convenient way to package all this data is in the notion of an abstract six-functor formalism, which I call the following.

Whenever you see any category $\mathcal{C}$ and $\mathcal{E}$ some class of morphisms, let me always assume that my infinity category is all finite limits and colimits, and here is a class of morphisms stable under composition and base change, where composition includes the empty composition. Then, we can define a new infinity category $\mathcal{C}_{\mathcal{E}}$, where the objects are these spans, and the composition is given by taking two such objects and following the fiber product. It's a symmetric monoidal structure, and there's an obvious way of taking a product of two morphisms.

We have this definition that, well, first of all, a three-functor formalism, where the three functors here that I'm talking about are $f_!$, f_*, and $f^!$ when f is an $\mathcal{E}$ morphism. There's a correspondence between this category with the end of product and infinity categories with certain properties. In particular, I would like to discuss this example where these categories themselves are not presentable.

There's also a six-functor formalism, what is the just-Stein product, and the means in which direction there are two ways, like $d(x \times y)$, right? So, yeah, so "lights" here means that you only roughly speaking, you only have maps from $d(x) \times d(y)$ to $T(x)$, but they need not be equal; they just have maps. And if you apply this to the empty product, this also means that d of the final object has a distinguished object.

Like, from the empty product here, so from the final category, so the point, there's a map to the drive category of the empty product, so P of the empty set, and from there, from the empty set, you can go further to anything else by a pullback, so some particular, like all the, all the you have access here are pointed, which is giving you the unit objects, really.

All for point, I think it's of the empty, it's sorry, it's, yes, think it's than you. All right, and so, a this is, very difficult amount of data that's, it's not so clear how to construct it, but conveniently, like Le Jen proved some very difficult Comorbi recipe. Well, the recipe is extremely easy, the proof that it works is rather difficult, um, but that makes it extremely easy in practice to construct these things.

In particular, like for C brings up and E the streetable NS, um, sending a either to D of a or to crl of a, um, which I'll probably discuss in a second, because, okay, let me just dove this one for now, um, uh, is an example.

Um, and so, uh, but now this and then some to the category of analytics text, and we would like to extend, uh, this, maybe to also some larger class of fre maps, well, not just I mean the fre maps of aine, certainly you want also base changes, but maybe much more than that, um.

And so, uh, yeah, so here's the, here, um, there is a minimal cost of morphism, um, unique, um, morms, the following property. You mean there is the smallest, because any class satisfying the condition contains this, yeah, any other class that satisfies the following properties will contain this class, yeah. So unique minimal is slightly different in some conventions, because it means that the pos it as a, as a unique minimal thing, it doesn't mean that it is the smallest, because so it's, but this depends on your notions, but it's slightly, according to some definition, better to say smallest than unique minimal, but because minimal doesn't imply that it is smallest in for parti other sets, unique minimal doesn't imply it smallest.

Um, uh, properties, so, uh, first of all, the the class, uh, a class of morphisms of maps of analytic rings define maps, that's something we want. Um, also we want that if we have just a disint union of fre m recable, so um, stable under unions in the following sense, so if, uh, you have some f from some XI to some common y, uh, all of them, then F the dist Union of all the F from the over y, so from the of all x yint this able i, it's clear how you would define a l stre function in this case, I mean, the drive car of the dist a product, and then you just take the direct sum of all the streaks.

Um, it's this class is local on the target, uh, in the following sense, so if you have a morphism of analytics text, such that the exist some cover, sorry, I wrote X before, so stick to this, um, that exist some y Prime, subjecting onto y, for which, uh, F Prime, which is a base change of of f, uh, which fime isn't class, uh, the class, say, uh, I'm not quite sure whether I need to explicitly say it, so I definitely want this class in the end to be stable under compeition and base change, and I'm not sure whether I should, whether this follows at some point, but always assumption.

Um, all right, so, uh, right, so this says that to check whether something is triable, you can, uh, localize on the Target, and by the way, I mean, if you wanted to find the lower streak funter for such a guy, it's actually quite clear you would do it, because you already have this ler streak fun for this one, and you know that streak fun they always come with base change, so to define lower streak on y Prim on y, you can do it after this base change to Y Prime, but then all the further X. G here. The following properties that G already lies in each other and is of universal strength in the sense that I will explain in a second.

And the composite and the composite. MTH, so you should think that this is rather stacky, but now you've covered it by something more reasonable for which you already know that the composite map is is is needer then the original map. Maybe I should make some remark now.

Did you say what Universal strength descent here? I mean, strength descent after pullback to any fine, so because this map already has strength maps, you can always ask whether the like the r of X meaning strength maps to the r ex and all fiber products an equivalence, and I ask this not only over X but after pullback to any.

But Peter, doesn't it follow from the assumption that it's a cover in the gr topology? How do you know is very very true. Thanks, thank you Dustin.

Yeah, what is this? So, how do we know that this, how do we know that there is a, for those kind of, oh, it's the assumption that G is in this, well, G is an. No, if you mean no, no, no, but EA, we don't know that there is, the theorem say there is a minimal class, and wait, wait, maybe maybe I need the next condition to to justify it.

If you have an $m : X \to Y$ is an eer, and why is that fine, then I actually ask that you can always find a cover of X by things which are over Y which I, and fin, then all the fiber products are also. Because and all the, also, X_i is covering over X, and then you need the full simplification. So, if I if I if my base is fine, then I make a strong restriction on anything that's possibly allowed, then I always ask that I can always further find such a shable, I mean such a yeah, shable C, where all the X_is themselves, they lie in this original class of coverable maps.

Peter, don't you also need a variant of four with disjoint unions, like, or would this be strictly more general? Like, doesn't two say that I could always, no, but two doesn't say that X_i to the disjoint union is in E. In other words, if you want to localize by using a family instead of one object, then you need some argument to reduce to the one object, and it seems convenient instead of two to have also the fact, in addition, I guess I suppose in some places that $X_i \to X \sqcup Y$ probably it should be in your class because it's just, yeah, but this is just because it's locally in the class $X \to X \sqcup Y$. Why is it in the class? Because it's locally in the class. Okay, okay, okay, okay, okay, okay. Just sorry, and then the empty to the something is always in the class because it's why, empty to anything is in the class, may you have to add it. No, this might be the empty collection. Ah, okay, okay. No, the empty to something X is empty and then going to Y, this would be in the class, yeah, you can reduce to Y being $\emptyset$ by, by, yes, and then it is in E. Okay, okay, okay, okay, okay.

Okay, all right, I hope I'm not screwing up, but, right, so to, you see this Universal strength descent after pullback to fine for such a map, then, after pullback to fine, you're in the situation of this one, but then you always know that you can refine by cover by strength, find for which you know strength. And there's one last condition which is really one we're interested in, that the six fun or the three fun formalism than you, Neely, from CE, I mean, from analytic rings and one we have to analytic STs uniquely in the space. This you have to put it in ER to the prev, yeah, so for some of these Surs I'm implicitly using that, I mean, okay, let say that. Yeah, so this follows from results, from exra, I mean, this has nothing to do with a specific six dealm, this just an is all about six formalisms, and I've given some account of this in my notes on six funes using Min different setup, but I think the same argument works here. All right, so that's an extremely

Because it gives us this extremely structured formalism of six operations. Whether I say three or six, it doesn't really matter. Here, let me say six, because for this, these are all presentable categories, and the fun f is all co-limit. Then, as Dustin explained, we always have the right joints anyway.

So, you have these six funs. Now, for some classes of stacks, and if you want to check whether - and it's in practice extremely easy to check whether any map - um, has or, in practice, basically all morphisms have three funs. And using some of these criteria that you can localize on S and $Target$, it's also usually rather easy to check that this is the case. So, you can first simplify the target as much as you want by localizing, and then you just have to find some kind of presentation of your guy which makes it simpler.

All right, let me say a few words about how it's done. It's kind of hard to explicitly describe, but it's easy in practice. You can check that the given map belongs to this class. So, let me just give an example to illustrate this a little bit. What happens? This must be covered by, but then is it enough? No, then you iterate, you do that again and again and again. You iterate, working locally on the source and the target, and you have to iterate and transform this, because once you get new shakable maps, then working locally, you get new things that can be covered by those.

Let's assume you're in a situation where you have some EX, Y, where maybe the Y is something simple, just a point, and the X is something spey. But then you can find the cover where, for this, you already have street maps. Then, the D of X because of the cover, it's a limit of the street maps of EX and Er, but this is also the same thing as again the co-limit along the lower street funs. Now, taking in PRL, these are the upper street fun, but you have lower street fun in the opposite direction, and for all of these things, you already have lower street funs, and they are compatible with the transition maps. Thus, because it's a co-limit and this guy is presentable, all the M preserving, you get the unique L street fun.

Let me actually give an even more specific example. Let's consider the interval mapping to a point. This is not something which happens in condensed sets, but as we said, condensed sets can be embedded into analytic stacks. As s explained last time, there is this descendible cover of this by a contour set, and this map here is stable, because when you face change to some cover here, so you can check it locally. And if I base change this to the contour set, then the fiber product here becomes itself some live profile contour, and this map here is just a flat map, I mean, it corresponds to a faithfully flat map of fine things, and so this here is an EA. And also, this map here is already in E. And so, up prior in our world, we've only defined street maps here, and we didn't define what the street map would be for the general compact Hausdorff space. But then, it turns out that in fact, at $L^{\star}$, one can observe that actually F^{∞} happens to also be an $L^{\star}$, as you would actually expect, because this looks like a proper map.

Let me do yet another example. This way, you can automatically extend from profile sets to compact Hausdorff spaces, at least the finite dimensional ones. It also gives an extension to all compact Hausdorff, no, let me not say that, to the finite dimensional ones. But then, you also have locally compact Hausdorff guys, so, for example, you have the reals, and now maybe this is a point where Dustin said I should have taken this J somewhere. Oh, there it's okay.

This is covered by the disjoint union over all n of the interval from $-n$ to n, and because, why is it subjective? Because, from the fun points, as condensed said, this is a union of these things. So, this is definitely subjective, it's also, and one

This argument will generally work for any kind of locally compact, supportable, and five-dimensional thing. We get some FL streak, and you can actually check that it is a usual streak. You can observe the comp, and why? I mean, you can look at this definition here. The drive category of R can be written as a Coit of the graph carries on these minus n NS along the lower street maps. To describe the low streak, it just means the col presing fun, so it's enough to describe what it does on all these things, but here it is just chology which is compact for chology. This very abstract procedure actually recovers a lot of like upior geometric intuition about what a low-frequency function should be of.

When I discuss the t a look curve, I was mentioning that some should be proper and that it should be smooth. Let me say a few words about smoothness. There is an inductive definition, a morphism, and why? I actually wanted to be in the, and then let me say proper. Maybe weekly proper, it's not, it doesn't precisely match one something I call proper in the context of an abstract. Let me just say proper, and maybe Dustin will complain that he wants this for something more specific.

There's a question in the chat from Peter asking if we have any explicit description of f uper shriek of the unit object for FX to a point in the category analytics tax or at least in some special cases. This is a very good question. Let me refer to the discussion of Smooths coming up in just a second. If you look at like f low streak in these examples here, it was just usual f low streak, and so then the f uper streak is also usual dualizing complex. In virtually any situation where you already have a dualizing complex, our up streak will agree with what you say is dualizing complexes. Except, one thing you have to be careful about is that if you embed schemes into analytic stats in this way, in this kind of with a tri analytic ring structure and all maps become proper as we said, the f l streak is always f lower star, and the upper streak is then this funny r joint of a lower star that is sometimes considered but is not so well behaved in some ways. If you want the usual dualizing complex on a scheme of finite type or something like this, you should rather use this other embedding using relative s with this caveat.

Let me try to describe find prop morphisms. One case that I definitely want to be proper is that it's relatively representable on f lines, so meaning that after you pull back to any fine becomes fine, uh, repres FES. I'm sorry, the following and proper you want toor for all Prim y, y um X Prime, which is the final product F and X Prime to Y Prime is proper in the sense that we used before for F, so that it has structure. Sorry, Peter, we were just discussing a remark that in the setting of aine analytics, Stacks the prop, you know, this the proper Maps satisfied descent for the gro and de topology. Someone pointed out that whether I say, well, they don't, right, because they have some connectivity issue. That's true, but I think I pass once more to diag or something I can avoid this. So, let me make this, so therefore, it's not the same, not the same as as so as being locally no. Yeah, we should, so we really shouldn't ask for all why Prime mapping to. Yeah, yeah, they will be a second part of this definition. I would hope that, was the second part, it wouldn't matter what I said here, but okay, I didn't carefully check it, so let me maybe not. However, one thing that this implies is that if this so happens in case one, then you already know that f streak has a canonical equivalence with f star, that's given to you because in for these Maps, we Define the f streak to be the f star, then because you can Constructors ISM locally d canonically, get it. So basically, the class of program that's be the one for which a floor speak is a floor.

But, but, say it like this: it's not a condition, but I want to say something which is really just a condition. And one way to do this is to, oh yeah, like for these guys here have such an identification. And in general, you can ask a diagonal ΔF from X to X^*. This is why I'm saying it's an inductive definition.

So, this might be proper in the sense of one, or it might also be proper in the sense of two, in which case it passes the diagonal again, then hope those are proper. And once the diagonal is proper, you get an identification between ΔFL^{str} and ΔFL^{star}. And this allows you to construct a canonical comparison map from a floor F^{str} to a floor F^{star}. Maybe recalling the second where it comes from and induced, does it follow after? And actually, you might think you want to also ask this after any base change, it actually follows. And actually, something even weaker of F is F, speak of the unit, and then solve it after the new. So, does this lead to a transfinite process? Namely, suppose you have, you can use $\mathcal{F}$ many times, and then you can say, no, I want us to determinately find many things. Yeah, there's no way to get it to go, let us say, suppose that you have a cover Y' to Y by half. Ah, in this case, one is a terminating condition, and the other is just you can apply $\mathcal{F}$ many times, but at one point, you should run into the one condition.

Okay, so the joint union of countably many guys X_i to Y_i, which are proper in the $\mathcal{Y}$ sense, will not be proper if when the sense goes to infinity. No, no, sorry again. When I said there is a cover by such, I was allowing that it's a cover by a collection of such. No, no, no, but I'm asking, suppose X_i to Y_i is proper, then the disjoint union of X_i to the disjoint union of Y_i is not proper. And I think it follows, no, because if you have a sense one because of, ah, because. Okay, I see what you're saying, but maybe you could just take this as the terminating thing as a definition, but, okay, so maybe it's not proper in my sense right now.

Yes, okay, what's the problem? It's not the problem. There's a variant where you, because this is just simple induction, yeah, without going to Omega, yeah. So, suppose you have X. So, yeah, so maybe I should really make this, I should say the proper is first defined proper, both of these things are like $\mathcal{QQS}$, because I think this should be the case. And then, in general, if it's true after pullback $\mathcal{J}YF$ or something. No, no, because suppose you have $\mathcal{QCQ}$ things X_i to Y_i which are proper in the, but in higher and higher senses, you need higher and higher, this will not. All right, but I'm not exactly sure in this theory how much you can reduce to $\mathcal{QCQ}$, because anything is a cover, but this. Yeah, the the equivalence $\mathcal{R}$ itself is not $\mathcal{QQS}$ in general, so it doesn't, you don't have good reduction, because I, the proper case, these things I think should be $\mathcal{QQS}$ because here the basic ones are $\mathcal{ICPS}$, and then the diagonal is. And I think from this you can deduce that F must be $\mathcal{QU}$ compa, at least because it implies that the floor is talking to $\mathcal{SCHORE}$ limits. So, these things should be $\mathcal{QQS}$ in a very strong sense, these maps, all the maps of

Itself, then it's Asis funs and it's true of Base change, and again this is something you can find in my notes on six fun EX for. And so, in particular with this definition of what proper is, then, yes, so then this map here is actually proper in this non-generalized sense to any careful. However, that if you have some kind of H Cube mapping to a point, then I'm not sure whether it's actually in. DUS, do you know? In any case, it's not proper because there is some the lower star fun doesn't actually con with col because of some infinite ises.

Right, so you can Define this things, and you can also Define smooth. So, for smooth, for smooth system, I want that the upper streak is basically the same as the paper star, but in this case, you don't actually usually expect isomorph on the nose. You rather expect that there's a same some twist, and also for smooth Maps, you would never assume that this is a condition that's stable under passing to diagonals.

So, the definition of SMU is, for this reason, is a bit different. And here, I us call the things loic, and this now really matches the abstract thing defined for any six fun form is if.

And now, I really want to ask, after there any base, sorry, this CL maps on Bas change? So, there's the natural transformation from pullback Stander with f upper streak of the unit, what upper streak, and of unit which will be some object of X, is actually an inversal object tender and commute to space change. And so, I mean, you can form this F upper spe to dualizing complex for x or y, but you can also do it for a base change, but it should also be an invertible object, and I ask that the formation actually commute to space change. Again, you can somewhat reduce the amount of data you actually have to check.

Let me rather just briefly say where the center comes from. And so, again, I'm mapping to write a joint fun. So, what you try should try to do is in produce a m from the corresponding L stre, but then this is precisely a projection form your situation here, pull back of something. And so, then this thing, but then, it's just by a junction. So, this class is St on the base change, uh, composition. I mean, also problem St in the base change, uh, composition. It's in some sense local on the source if you smooth cover which smooth, and okay.

So, there are some ways like that to be able to talk about some Notions of proper, smooth maps, and I mean, they some have the expected consequences on the rough categories, and so you can, yeah, execute many arguments as usual. So, I just want to briefly now come back to ttic curve.

All right, so, a is now again this guest displacer, and so now we have the norm. And so, we can def find the analytic over that's a of. So, we have multiplication by two, all right. Now, we can make some assertions.

First of all, this St here, this is proper. Well, it's not representable in Fes, clearly, but the diagonal is, because it's just a diagonal P1 f, and you can check that p stre and p star the unit just agree, and this is actually just the I don't know, you can check that in algebraic geometry, proper, and it's also smooth.

And for this one, should I mean, I you can just really just check that directly, it's not hard. You can also do the following argument that, whenever you have like the morphism of six fun formalisms, so, for example, like schemes mapping to analytic Stacks, then any such morm of six fun fors will always preserve comically smooth Maps, because there's some kind of diagrammatic way of encoding it in the six fun formalism. And so, the smoothness that you somehow know in algebraic geometry automatically implies it, the algebraic version implies anal question.

This J here, it's actually also, I mean, it's also shable, and it's actually somehow, I mean, I didn't Define it, but I could have defined also in an open imersion. So, this just means that, see identity, and again, this is something you can basically reduce to just zero infinity and like the open guy. The close guy, very some everything produces the usual six funs on locally compact $\mathcal{A}$ spaces. These are, in particular, also like $\mathcal{J}$ lower streak, so $\mathcal{J}$ upper streak in this case. $\mathcal{A}$ jerar particular is also, so this guy is not proper, just like this, I mean it's not $\mathcal{F}$ compa. Okay, but now again, we can take since $\mathcal{T}$ the curve, which is quotient. Ah, but one thing that is proper right, so the proper Maps they satisfy the two out of spe property because they were Som defined by passing, and this map here is actually proper. And so it also follows that if you pass to this and $\mathcal{G}M$, then it's still proper over zero.

Infinity, there's a question in the chat, Peter. Yeah, are open immersions of analytics Stacks representable? Absolutely not representable by $\mathcal{F}$. I mean if they were represent by $\mathcal{F}$, in particular, would have to be qua complex, but this $\mathcal{M}$ is very much not quic complex, so we get much more General open Emer.

Right, but now the $\mathcal{S}$ tatic curve, it's a of analytic $\mathcal{Z}$, and so this to $\mathcal{Z}$ Infinity mod rescaling by po of two, which is a circle. And so like the base change to $\mathcal{Z}$ infin you see that this map is actually proper, but if you project as one to the point, this is also proper. And so you see that the $\mathcal{T}$tic curve is proper or the point, the point is now like everything.

So you see that this constru gives you an $\mathcal{A}$ntic $\mathcal{S}$t that's proper, but also, I mean this map here is locally split, and so this means also this is here Lo local is the same as this $\mathcal{A}$nalytic $\mathcal{G}M$ which was smoo, so also the sky here isical smoo.

I find it a bit remarkable how you can like combine intuition of about the properness of compa spaces with some really algebraic geometry kind of properness and so on, and it all works really well.

Right, so, yeah, also it comes with a section as a unit section $\mathcal{I}$ given by one and $\mathcal{G}M$.

So, there some of the claim that this $\mathcal{E}$qa curve, which is $\mathcal{A}$nalytic $\mathcal{S}$t over a limit $\mathcal{A}$lun from $\mathcal{S}$ches schemes over the underlying $\mathcal{N}$ice $\mathcal{N}$ essential image of the fully faceful embeding from schemes over the underlying ring and ring ration.

So, to execute this argument in a really nice way, I should prove a little bit more about this ring $\mathcal{A}$ here. In particular, classify like the dualizable objects in $\mathcal{D}$ of $\mathcal{A}$ that they are just perfect complex, which I think is true. Let me do it a little bit more by hand because it's fun.

Modle and I asked that it SL on the essential image of H. Me what, sorry, I because it might be derived. In my situation, I'm in everything, there's no derived structure, so it doesn't really matter.

I definitely want that there's noology and, um, assum have this situation, um, and it's not clear the difference between Poli. I mean, this, it's just, I'm just trying to say you have an an line bundle in some approximate sense. So, for example, I want that it's Global, it or repl by some tens power, it's globally generated, uh, and there's no higher chology of tens powers which you can arrange. And I also want that the global sections are not something, some just to the SC module, because I want an algebraic schem in the end. So then we can define something algebraic to be just the project of the. Is this AR GMA? Is it just H zero, or it have a positive? Yeah, I mean, if some X is actually derived, it could have positive stuff, let's not worry so much about it. So you can Define this thing here is a direct scheme over, and you automatically get a comparison that, uh, from this algebraic guy, analytic fied, and, because, uh, globally generate, you can actually check that this is really a qu comp guy.

Okay, this is the stand, but it's not enough to, it's not L, to I hope. Um, so something one can say in the situation is that, uh, so anything comes from algebraic geometry, as we said, it's automatically improper, so M actually automatically improper. Meod, but also, uh, a simple observation is that in this abstract setting, automatically, it will be the case that aest of the unit is the unit structure, she is a structure, she, um, why, because you can check this, uh, we can check after the pullback, uh, to f, f, x of x, uh, but there on global sections, because I'm the fine guy, like of CH is just of the global section, because it's e, um, uh, but, uh, but this inclusion here is really just given by algebraically inverting something, and so then this a star here is then just some some filter po Lim of inverting some function f, so this actually just reduces to, uh, to to the isor on sections that we know. I mean, so this is like the plus of some f, where f is some fun, some Global section of, and then, uh, you're really just getting Co liit over multiplication by F of global sections, sorry, the Al, I mean, either on the algebraic guy or on the on X, but some the AL guy was ripped, so that like the go section of the line bundle are the same on X and the albra what K, the co liit over K transition modication by some section of l, f section of L, the K or n, maybe I don't know. Uh, F was an unfortunate Choice, actually, because that was your map from X, oh, sorry, yeah, thanks, thanks, sorry, G, did I use G? Let, all right.

So, uh, right, so you have a proper map of analytics F, where FL St unit is unit, uh, this actually implies that it's a s active net, because, like, yeah, this this is a SE true after any pullback, because proper St stre any pullback, and so this means that after pullback to any Fon, you can get the algebra, uh, in terms of the image, and so this actually means that f is subjective. All right, and so what does it now take to show that F isomorphism, uh, see, I mean, this generality it won't automatic the case that it's an isomorphism, but, uh, to check that isomorphism, uh, it now suffices to show that the diagonal of f is an isomorphism, right, because if you have any subject M of sheets, it's Isis only diagonal Isis, uh, but this, the diagonal of X here, he want just this a pull back here, uh, actually this also implies something else I should have said, also implies that the pullback map from the garage category of this guy is actually inly Faithfully into X, because, um, because if I look at low star upper star, then by the proection formula, this is just

So, if it so happens that there is a map $\mathcal{D}$ of X is $\mathcal{F}$ on, and Δx of one is in the image of $f * \mathcal{F}$, we discuss to show that to give an example of a proper map, explain the $\mathcal{C}ur$ algebra.

Okay, so I claim that I only need to check that this diagram gives a diagonal commutes. But in practice, like the diagonal, I don't know, some kind of complex or something, this, this, some risky $\mathcal{C}los$ immersion. In particular, some kind of f math, and the $\mathcal{X}$ $\mathcal{S}$ of one is really just some coherent $\mathcal{S}he$.

I claim that it's actually surprising to see that this lies on the image $\mathcal{F}$ time. Then we win because of it $\mathcal{LIC}$ image of times upper star, and it's actually the same thing as applying f f lower star and then upper star again. But if you apply the low stars and you run through this diagram, you see that it's actually the same thing as a pullback of, which actually implies that it's equal to $\mathcal{T}imes$ of $\mathcal{D}el$ algebraic.

But then this means that, yeah, but then this means the some of $\mathcal{P}$ diagram. All right, so the upshot is, if you want to prove that some such analytic space, proper analytic space is algebraic, you have to produce an ample line bundle in that sense over there. Then you automatically get this kind of comparison $\mathcal{M}$ to some algebraic something algebraic.

The only thing that remains to check is to see that the structure sheet of the diagonal lies in the subcategory that comes from this algebraic.

My time is up, but these are things that are simple to check in the case of static $\mathcal{C}$. So, you can, for this $\mathcal{L}$, you just take the $\mathcal{A}$ mod given by the zero section, and then you can actually just compute what all the global sections are just inductively. The global sections of the structure sheets you can just compute the $\mathcal{A}R$ easily, and then the rest you just acquire copies of just the zero section in addition, which are just your base ring. So, this is okay, and well, and for the diagonal, you can actually use that this is a group, so you can move the diagonal to the $\mathcal{Z}$ zero section, and the zero section is easy.

All right, my time's up, so let me stop here. I want to clarify just from the Viewpoint of General scheme Theory some point here. You consider $\mathcal{P}ro$ of rings which are not assumed to be finally generated, that is, suppose that we don't have a derived ring, we just have the usual ring. For example, the graded ring of $\mathcal{H}$

zero on some scheme of powers of a line bundle. So, we just interpret what you said for normal schemes for Simplicity, because I'm not sure about.

Now, when you look at the notion of proper, like $\mathcal{R}otend$ defines finite type, separated, universally close, but of course, you people thought about the case of non-finitely generated. So, you can have a universally closed, maybe separated, but then this turns out to be a projective limit in the $\mathcal{Q}C$ $\mathcal{Q}S$ case, a projective limit of usual proper things. So, my question is, suppose you have a $\mathcal{P}$ of the type that you consider. What we know is that it's covered by Finly many opens, a standard basic principle opens, whatever. But is it the case that it is proper well in this universally closed sense?

The problem is that, of course, in the $\mathcal{T}$ litic $\mathcal{C}$, I think the algebra will be finitely generated, but I allow the case that the graded

Let's specialize the notion of properness I defined here. In that setting, you get the usual notion of properness. Okay, because it implies COMP. Okay, all right.

22. Berkovich spaces (Clausen)

https://www.youtube.com/watch?v=fnEPiDIF9_k&list=PLx5f8IelFRgGmu6gmL-Kf_Rl_6Mm7juZO

Unfinished starting from 0:00

"So, we're nearing the end of the course, actually. And, at the very beginning, in the first lecture, I kind of gave an introduction to what we wanted to have out of this theory of analytic stacks. In particular, I was putting some emphasis on the fact that we want to know that traditional frameworks for analytic geometry can fit into this perspective of analytic stacks.

We've sort of already discussed attic spaces and a little bit of geometry over this, the little bit of things like like tape curves. But what I want to add there was this issue of shifting, and then the idea was that you have to define some kind of derived attic spaces. This will not explain in full what is Uber Uber set up and in your formul. Maybe there are different ways to do it. I'm not sure it's possible. There are different ways to do it, yeah. So, this is yet in progress already. You, yeah, let's say that or something.

The it's in Juan Estaban's paper on the analytic the ramstack that's okay. So, there you go. All right, okay. So, what I want to start moving towards today is a relation with Berkovich spaces. But, before discussing that, I want to just do a little bit of setup.

First, I want to talk about different Notions of how to localize an analytic stack over a topological space. So, well, we've already actually seen one way. If we have an analytic stack, and if we have S, which is maybe let's say a metable, finite dimensional, compact Hausdorff space, or it could be a maybe a locally compact Hausdorff space, which is locally one of these, then we've seen that that can also be viewed as an analytic stack. We can also view it as an analytic stack, and then we could just ask for a map F from X to S in the category of analytic Stacks. We discussed kind of what structure you get on X, basically you get a bunch of idempotent algebras in D of X corresponding to the closed subsets of S, or you can think in terms of the complementary opens as well, and you get sort of some idempotent coalgebras which would be like the J lower shrieks of the constant sheaf on the open subset, as opposed to the like I lower stars of the constant sheaves, giving you these idempotent algebras. But, in any case, you get a whole bunch of algebra objects in here which kind of let you localize this category over the topological space S.

But, we're going to want to, when discussing the relation with Berkovich spaces, it's going to be too annoying to require these kinds of conditions, even though they're satisfied in practice. So, I just want to discuss something slightly more general, some more general notion of how to localize an analytic stack along a topological space and the relation with this one in the case where you do have a metrizable, finite dimensional, compact Hausdorff space."

See, when I make the definition that I'm interested in, it doesn't matter whether it's a set or not. Question: the monom, if I only consider an that's like taking connector component. Exactly, yeah, so it just means that if you do the fiber product, then the diagonal, I mean the diagonal map associated to this inclusion is an isomorphism.

Okay, right, so and then of course a topological space also gives rise to a local. But let me actually change perspectives and just say 'S' is a local instead of saying 'S' is a topological space. Then we can ask for a map of locals, Lo(X) to S. Whether or not this is a set, this is some data that makes honest mathematical sense, because you're just saying that for every open subset, you have to give such a monomorphism, and you know unions have to go to unions and finite intersections have to go to finite intersections.

In the literature about locals, I don't remember, I didn't, don't use much, but this is like the direction of M of too, but I don't know when they define M of locals, is it in the other direction or in this? Yeah, I don't know either, so I'm doing the geometric morphism, let's say a geometric morphism of locals. Well, morphism of sites goes the other direction, you, the morphism of sites goes the other direction. Then I mean, yes, so I mean, I'm talking about a geometric morphism, but also of goes in the direction. Oh, maybe, oh, I forgot, yeah, yeah, okay, yeah, okay, go in the same direction. Okay, not, it's not used much.

Anyway, okay, but we can also ask for a stronger property, so that each inclusion of open subsets, say 'V' inside 'S', maps to, so it's supposed to give some monomorphisms, so maybe I should give some name for this, like 'f' or 'Pi', maybe. So this goes to like 'Pi' inverse 'U' subset 'Pi' inverse 'V' subset 'X'. We could ask that these inclusion maps are actually open immersions from the perspective of the six funter formalism that Peter discussed last time, the extended six funter formalism on analytic Stacks. So we could ask that each of these inclusions be shable, and that they be well chromologic smooth.

But in the case of a monomorphism, then it's quite easy to see that the dualizing object is canonically just the unit, and it's the same thing as an open immersion. A general monomorphism, I mean, it could look like anything really, it could look closed, it could look open, it could look like some mix. But it's reasonable to ask for this stronger property, that what looks like an open immersion on the level of the topological space is also, is looks like an open immersion from the perspective of the six funter formalism.

In the case where 'X' is a metric, finite dimensional, compact, Hausdorff space, can ask for, as before, 'S' to be an analytic stack.

What does this imply? Well, basically, you can just check on the level of the analytic stack associated to such a guy that every open inclusion, um, actually is an open inclusion from the perspective of the six functor formalism. Peter more or less discussed this last time, that our six functor formalism on analytic stacks, when we restricted to this case, recovers the usual six functor formalism on locally compact Hausdorff spaces. And then this property of being an open immersion is stable under base change, so that's, um, uh, and uh, you get the, yeah, so can you say so you know that open immersion holds for, ah, it is stable under, um, of analytic stacks or, well, uh, pullback, yeah, it's stable under pullback.

Yeah, um, so yeah, so this is the strongest condition. So if you have this, you can ask whether this holds. You just check whether certain inclusions or open immersions, if you have this, you can ask whether this holds. That's, as we discussed, uh, the last couple of times, that corresponds to some connectivity condition on the, on the ítm potent algebra, as you see, um, okay. Let me give a small example.

The second one is just given off by, lo, is just one of open, opener, yeah, that's true, yeah. So, yeah, so right, so you could think of this in terms of there's, lo, local X, and then there's some quotient local which is like the local of just monomorphisms that are open versions with respect to the six functor formalism, and then the second bit of data is a map like this, um, yeah, thanks for that remark, Peter.

Uh, and is it the case that the union, maybe I go confus, union of open immersions is an open immersion or to make it an open, yeah, no, no, that that's true, that it's an open immersion, yeah, which is needed to, well, it's needed to, yeah, it's needed to have this local, I mean, yeah, so, or well, remote, I mean, depends on how your, whe, right, right, right. No, but it's true, so that if you have a, yeah, a union of open immersions, then it's still an open immersion, and that's an important point to check when you're discussing these things, um, and it follows from this extension procedure for six functor formalisms that Peter discussed.

So if you have a union along open immersions, then those are sheafable maps, and but it's also a cover, and so you can you can get a a lower shriek map defined on the union and then you can actually check locally that it's comically smooth, um.

Okay, uh, so let's give an example. Um, so, well, remember way back when when we had Hub Pairs and stuff like that, so let's see, um. Then, uh, we assign to this an analytic ring, R, R plus solid, um. Then, we can take its spectrum, um, and then we can look at the local associated to this. And what we essentially already saw was that this, this analytic stack localizes along the usual, um, usual Huber topological space of continuous valuations.

No, you, you, ah, now you consider the spec in your, in your, yeah, this is this is what this is, spec n. So, Peter, yeah, I'm lazy, so I, yeah, I'm lazy, so I just say spec and, and then, uh, spies in the old sense, yes, it's a topological space, yes. And then, okay, the local, okay, and then you have a map of, oh, yeah, this loc, this means for every open in the add space, you give a monomorphism of analytic stacks, and uh, this, of course, this involves passing to some derived pairs in some sense, yeah, so because you are not, but your original pair is is not right. So if on the level of rational opens, this is just going to give another apine, uh, analytic stack, which is the one where you enforce, uh, that F invertible, just in the world of analytic Rings, you enforce that F is invertible, and that G1 over F, etc., GN over F is solid, um, and that defines some, uh, that Can always refine any open cover here so that, so in fact, you get an open cover. So in fact, we showed, without having the L, without having introduced the language for it, we showed that any open cover or cover of a rational open in $\mathbf{R}^+$ pulls back to an open cover, can be refined to, pulls back to an open cover of, you know, the, pulls back under Π to an open cover in the sense of the six functor formalism.

No, so let me make a remark. So let me make an open cover to cover by monomorphisms, yeah. So let me, let me make a remark. In general, it's not true that a rational open pulls back to an open immersion. Is there a proof of this? Oh, yeah, I'll give an example right now.

So, I don't know, have two formal variables, maybe p and X. Oh, sorry, I should say just $\mathbf{Z}_p$. Yeah, that's, yeah, I don't need two variables. Thanks. So just one formal variable and then invert it. So what does this correspond to? On the level of analytic rings, we get this, and on the level of derived categories, this is just solid $\mathbf{Z}_p$ modules, and then this is just a, well, this is just algebraically inverting p. So the left adjoint here is just the inclusion of the full subcategory, or the right adjoint here is the inclusion of the full subcategory where p is invertible.

So this passing from $\mathbf{Z}_p$ to $\mathbf{Q}_p$ is just algebraically inverting p, yeah, p is a prime. And that actually, from the perspective of the six functor formalism, is a closed immersion, not an open immersion. That's a proper map because the, this hasn't the second variable hasn't changed, so we're just changing the underlying ring and not the analytic ring structure. But it's also a, yeah, proper monomorphism, it's a, it's a closed inclusion, it's closed, not immersion, not open.

So, I think we treated the case of some covering given by a function like f, like the Laurent cover, yeah, the one where f or $1 - f$ is less than or equal to mean, this kind of basic cover. And for those, is it the case? Yes, so then let me make another remark. However, if R is Tate, then indeed, this doesn't arise. So in general, the problem is exactly these open covers in the Huber sense, which are just given by algebraically inverting a function without enforcing any inequalities. But in the setting of a Tate Huber pair, then you know, if you want to invert something, it's, the subset where you invert something can always be written as a union of subsets obtained by forcing inequalities.

So, it is not quasi-compact in general, right? But dealing with something which is in the sense of the ADC space is quasi-compact because it is a rational domain. I think in the, yes, but I mean, it is Rush, so it is quasi-compact in the, yes, in the. This also has to do with the fact that we didn't directly, I was sliding one little thing under the rug here, which is we didn't really directly define this as a map. We didn't really directly define it like this, remember we actually had this valuative spectrum of all of these guys, and we actually defined this map and then we used Huber's retraction here. So this was not quite accurate, because that would be accurate on the level of Spa, but that's not exactly how we get it for Spa_a. Instead, we, just assume that the real Spa_a, yeah, okay, right, right, yeah. The idea is open, and then it's okay, yeah.

So, the point is that the rational opens in the Tate case can always be described by forcing inequality among the valuations. Okay, yeah, let me just try to finish what I was trying to say. It's like, I don't know, this is very, I just, just to give an idea of what's going on, I don't want to get too precise about it, but it's this kind of phenomenon, where these guys correspond to this kind of thing, which does correspond to an

So, sheets on this F is not there. In fact, you cannot algebraically invert F, but using the right-hand side. That's right.

Okay, and also, there is another topology on the same space, with the same constructible subsets. Where the open pulls back to open.

So, another fix, even outside the Tate case, is to choose a slightly different topology from the one Huber described, where both of these things are open. It's not the constructible topology, no. It's a topology which is incomparable to the Huber topology. For this subset here, $P \neq 0$, it will be closed from one perspective and open from the other perspective. It's open from Huber's perspective but will be closed from the perspective of this other topology.

It is a spectral space. To be open, you basically redeclare Zariski open subsets to be closed, which the Zariski open they are not.

For example, inverting P here, inverting P, is now closed in the new topology. We haven't discussed that element of synthetics so far, that's true. But let's leave that aside, I think there's already enough information being discussed.

Is it the opposite spectral space? No, it's not the opposite spectral space, because these ones are still the ones where you're enforcing inequalities like $f \leq 1$, that's still open in both cases. It is the opposite of one of the ones Huber describes in one of his papers.

Yes, we can make this more precise. I'm going to hesitantly say yes, it's obvious if you remember the definitions of everything. Maybe we need compact or not, because I mean, you want an input to correspond to it, but I'm not sure whether if it's not quasicompact, you have...

Yeah, so everything is defined by inequalities $f_i \leq g_n, g_n \neq 0$, and this one you replace by saying that is a closed condition for any G. Well, I mean, you have a rational open, so the conditions are satisfied, like the ideal generated by all of them is open. So, you can't just invert G if that doesn't correspond to a rational open in Huber's sense, but in situations where you can, it corresponds to a closed conclusion.

One last remark about this general setup: if S_i is some inverse system of compact Hausdorff spaces, then giving compatible maps from $\mathrm{Lal}\, X$ to S_i for all i is equivalent to giving a map from $\mathrm{Lal}\, X$ to the inverse limit. And it's the same for Lo_p. If all of those ones factor through Lo_p, then this one will also factor through Lo_p.

Every compact Hausdorff space is a limit of metrizable finite-dimensional spaces, potentially in many different ways, but in many situations, there's a natural way of doing it. We'll see this in the setting of adic spaces, and then this gives you a way of going from the case of metrizable finite-dimensional compact Hausdorff spaces where you can sometimes go from this perspective and you can go to this perspective. You're closed, you know, the situation kind of passes to inverse limits in a nice way, and um, but now you no longer have to worry about things being metrizable or finite-dimensional. Okay, that was my little um, just uh, is a map to s the same thing as a map from loal? No, because you have this connectivity condition, so it's the you still have to impose this locally that locally all the item poent algebras are connective, so yeah. I mean, as far as I, it's not like I produced a counter example, but I I I kind of believe that it's not the same, uh, okay.

Okay, so I'm always confused, is there really a t-structure def over D of that object? And no, no, there's no t-structure. So what I didn't, I don't have to say what I mean by connective. I have to say what I mean by connective locally, and what I mean by connective locally is that there exists a cover by apine things such that it's your object when you pull back to each of those apine things is connective. Now, once you're in the apine world, connectivity is a pullback of something connective, is you have a t-structure and the pullback of something connective is connective, so it's kind of. And also, if the pullback is connective, then it's connective or not, no, that that is not necessarily true, okay, okay.

Okay, so it's not like, so it's not a sufficient condition for having a. So this is like the for schemes that you have the the non-aine orens, but still, if you use as the flat or pqc, still if it's up upstairs, it's up downstairs, so that's. But in your topology, of course, you have much more stuff, yes, yes. So you can have these wheel-we things where, yeah, you have a map which is apine locally, but with an with a map with apine Target which is apine locally, but not globally, and this kind of, yeah, um, yeah, that's kind of unavoidable when you're doing analytic geometry.

So, uh, okay, um, where are we? Okay, so yeah, so um, so Banach spaces. So let me just start with a reminder on the definition, so we know where we're going. So, so, so, a this is a Banach ring. So what does that mean? It means that a is a commutative ring, and this Norm map from a to the non-negative reals, uh, satisfies, so first of all, uh, Norm of 0 equals 0, second of all, uh, the triangle inequality, Norm of x + y is less than or equal to the norm of X plus the norm of Y, and third of all, um, it's sub-multiplicative. So, um, and uh, probably have to put the norm of -1 is equal to the norm of one, which is e z, yeah, I should probably put that, yeah, so uh, either a equal Z or Norm of 1 equal 1 and Norm of us one, also do you need that? It does not follow, uh, I'm not sure how much it follows, but it, it's usually they want the triangle inequality Al with x + y, okay. I think you can, you can get it, you can give a crazy thing like the negative multiply by two, you can do something that still satisfy the a without, but of course, it would be equivalent something satisfying the a, this is probably not how to show, oh, okay, uhhuh, yeah.

Um, yeah, and then, so let's say that, and then a is a complete, uh, with respect to the norm, uh, um. So an example, yeah, there's, so example for, say, s is a compact hausdorff space, uh, you could take for a to be, uh, functions, say, I don't know, complex, uh, complex-valued continuous functions, and take the norm to be the sup norm, where you have the usual absolute value, um, and this, maybe this is kind of, uh, right. Another example would be if, uh, if R is a a Tate-Huber ring, um, and if you choose a pseudo uniformizer, uh, topologically nilpotent unit, uh, then you can define, uh, define a norm, uh, Peter actually wrote down the

Yep, this is multiplicative. It's not multiplicative in general.

Okay, so right, and then so to this data, so this to a and the Norm Huber assigned the space, which has a set Burkovich. Thank you, yeah, we're switching over now. Thought Burkovich, which as a set is given by the. Norm of f equals z if and only if f equals z, right.

So, I'm just using this notation X, so kind of it's just a decoration to. And this is now a multiplicative seminorm, with which is bounded by the given Norm you have on the the Banach algebra A.

Well, for example, in and you can take. If you require that the norm restricts to the usual Norm on the complex numbers, then in fact these are all the examples. So that's maybe Gelfand's theorem. So, those are basically the only examples in this situation. And multiplicative includes the of Zero element, that is the norm of one is one, yes, yes, yes.

Except, but this is maybe zero multiplication of zero numbers, yeah, we don't want that. So, we want these to be like points, so we want them to be non-empty. So, we want one to be different from zero, I don't know, I mean, yeah.

But, do you allow $\mathbf{Z}_R$? No, well, wait, I allow, okay, I allow $\mathbb{A}$ is the zero ring, when this will be the empty set.

Okay, so, there's the, so this is actually a compact Hausdorff space. So, I didn't describe the topology, but here it is. You can view it as a subset of the product over all F in A of the interval from zero to the norm of F, and it's actually a closed subset. And this map just takes a norm and records its value on F, and the topology is just the subspace topology, so it's a compact Hausdorff space.

Okay, so what we're going to do is, we're going to move this definition, we're going to try to make the same definition to make the same definition, but in the world of analytic stacks instead of topological spaces. So, we want to take the same idea, which is that we want to look at the set of all Norms, multiplicative seminorms on A, bounded by the given Norm on A, but we want to say that in the language of analytic stacks and that we've discussed, using the notion of norm on an analytic ring that we discussed earlier, yes, yes, yes, that's true.

Okay, so let me remind you about this notion of a norm. So, the definition of, well, we could say even more generally in an ∞-category stack. A norm on X is a map from the algebraic $\mathbf{P}^1_X$ to the closed interval from zero to Infinity, including Infinity, which is multiplicative away from Infinity. So, let's say on Norm inverse 0 to Infinity, and let me just write it suggestively like this. You really should write down the commutative diagram with inversion on $\mathbf{P}^1$, inversion of the coordinate on $\mathbf{P}^1$ and inversion of this extended real positive non-negative real axis here. And some condition on how $\mathcal{P}$ sits, p, p, p, yeah, so this is the p, oh t, t is the variable on $\mathbf{P}^1$, yeah, right.

Yes, what do you mean by how $\mathcal{P}$ sits? Yeah, so, remember $\mathcal{P}$ was this ring object you have over any analytic ring, which is the free guy on a topologically nilpotent element. And it's some version of a unit disc, and what we ask is that this Norm function gives

Small a map in the category of analytic Stacks. Yeah, so these objects are fixed, right? When you fix X, this is fixed, and this is fixed. Yes, that you know that there is a. Why was it? Well, we basically by definition, every analytic stack was a small co-limit of representable analytic Stacks, okay? You have a.

Because there was a notion of analytic ring where there is the category, yeah. Okay, there, there, you, you, you handle the problem of large sizes. I mean, because it's enough to check the condition some smoke, but then you have the analytic stack where you cover, but you don't know which covering you need to give a ism, so you have need all possible covers could be covers by bigger and bigger see, but you say the category is accessible. Yeah, so the, the, the main technical result you need to prove is that the sheifification of an accessible prief is still an accessible prief. That's the main technical result you need to prove. We did not discuss this at all, but that's what's underlying the resolution to these issues. It's like in Waterhouse.

Okay, so this, in fact, so there's a cover, so this gaseous base maps to n. In other words, you can write down a norm on this gous space tack, and it's, and this, so this, and this is actually the universal norm on an analytic ring with a variable Q in R such that norm of Q is precisely equal to 1/12. What did you write? NX is the analytic stack n, n is an analytic stack. Yeah, so the, so NX, the set of norms, is actually just the set of maps from X to some stack n.

Right, so yeah, so if you ask for a norm on an analytic ring and an element whose norm is exactly equal to 1/2, which is kind of a somewhat stringent condition, because a priori again, the norm map is norm of Q is a map from $SpecR$ to $[0, \infty)$, and you're asking that it factor through this, a priori, its image could be some interval or something, but you're asking that its image be exactly this singleton. The universal example of that is this guy, and moreover, the map to the base stack is actually a cover in the sense of our Gro de topology on analytic Stacks, because every norm on an analytic ring locally, you can find such an element. We had this argument, we discussed this two lectures ago. Right, everything here was like X was a *Spec* of an analytic rate. I mean, I made this definition for a general X, but it, I mean, it doesn't matter because the condition, I mean, the norms on an analytic ring, they satisfy descent basically. I mean, it's kind of follows from general nonsense, and so it automatically glues to say what a norm is on an arbitrary analytic stack, and it just unwinds to the same thing. So the cover also exists because it exists locally, like you GRE it. Well, the map, the map, no, the map exists because you can write down this norm, and then it's a cover because given any norm on an analytic ring, after a cover, you can find a q that satisfies this property.

Sure, do you need to do some base tank? I'm sorry, do you need to do a base change? What do you mean? Is it okay, or you still have a question?

Okay, so I want to, before finishing the discussion of or introducing the definition of this enhanced Berkovich Spectrum as an analytic stack, I want to explore a little bit about so, what does n look like? So, well, what do we have on N if you have a norm on an arbitrary analytic ring? So, if you have a norm $p : R \to [0, \infty)$, so as mentioned, for any section here, you get a function from $SpecR$ to $[0, \infty)$, but over an arbitrary analytic ring, the only thing we know exist are the integers. So, given n in $\mathbf{Z}$, we get a map which records the value of your norm on the integer n. So, what does this We'll also see that the fiber over any point in the image is non-empty, so that at least in this case, maybe it's a general fact - I don't know, at least in this case, it's kind of a theorem that the image is closed, so to speak. Okay, right, so now note that the more traditional thing is this Berkovich spectrum of the integers, so that was also by definition a subset of this product going from zero to infinity, and it was given by those Norms, yeah, so multiplicative, avoiding Infinity, satisfying the triangle inequality.

Let me remind you what this thing looks like, in case people haven't seen this before. Recall that $\mathcal{M}(\mathbf{Z})$ looks as follows: you have a point at the center, so to speak, which corresponds to the trivial Norm, meaning the zero Norm of Z is Z Norm, and the Norm of everything else is equal to one. Then, you have several branches. You have an archimedian Branch, so at the end of the archimedian branch, you have the usual Norm, the usual archimedian Norm, the usual absolute value. But then, for each prime P, you have another branch, which also ends at some point. To get the correct topology on embedding it into $\mathbf{R}^2$, you should probably make the branches get shorter and shorter and shorter, but okay, that's...

Yeah, but the situation with these p-adic branches is a little bit different. So, what's going on here? Here, you have the usual absolute value, and here, at the halfway point, you have the square root of the usual absolute value. And here, you know, then you can put any Alpha between zero and one, and you can kind of see from the intuitive perspective that this interpolates between the usual absolute value and the trivial absolute value. Here, what goes at the top is not the usual p-adic absolute value; the usual p-adic absolute value sits somewhere here, so normalized to say so that P equals 1 over p. And now, you can actually scale it to any positive real, and then there's also a limit as the scaling goes to infinity, and what that gives is the trivial Norm or the pullback of the trivial Norm on the residue field FP, so in other words, the Norm of any multiple of p is equal to zero, and the Norm of everything else is one.

Okay, what of course, no, I... Yeah, this is great. I think this was something like this, this is the first talk. There was a talk in this course, it was in this course or another, probably in this course, there was some discussion of, but maybe I could do this another thing. Yeah, so I'm trying to recall something which is well-known indeed, in order to set up the discussion of what's following here. So, but in particular, I want to emphasize this is a really big space, but the Subspace MZ is quite small, you know, one-dimensional.

So, now what we're going to see, so there, you allow the value to be in, but here, you don't. That's correct. So, here's going to be the claim: the image of the Norm is a larger subset. It looks like this: you have all the same ones as before, sorry, I'm trying to say that it stops there, trying to draw like a closed interval sign, but then also at the archimedian place, it gets extended. So, here, now the usual archimedian absolute value is also in the middle of the interval, and you can take arbitrary powers of it, so for any Alpha in R greater than zero, you can take powers of it. So, it'll go to there in one direction, and to the other direction, you get some really strange point, which corresponds to... So, it's a subset of there, so it's given by some maps from Z to the extended real line there, and it's given by Norm of n equals infinity if n is not equal to -1, 0, or 1.

To infinity and then compactified at the end. Yeah, I guess what I - you can try to write a formula for it. It's like the image norm is like - you take the Berkovich space of the integers. Ah, no, let me make a before I say this.

In particular, the triangle inequality can fail. Well, that's quite clear here - you have norm of one equal one, but norm of two equals infinity. Okay, that's a pretty drastic failure of the triangle inequality. But also, like for the square of the usual absolute value, which is a new thing, you have the triangle inequality fails as well.

So yeah, I guess another way of saying this is like the image of norm - you can get it from the Berkovich spectrum of Z. On the Berkovich spectrum of Z, you have an action of the real numbers greater than or equal to one. Wait, did I - yeah, or well, yeah. Um, or maybe, yeah, I don't know, but on this other thing, you have an action of the positive real numbers. You can kind of do this, and that doesn't do anything on the non-Archimedean branches, and then on the Archimedean branch, it extends it all the way, and then you compactify it - one-point compactification. So, I don't know.

Okay, right, so let's explore this and let's see what kind - what is kind of going on. So, let's consider just the map - what's that? Is the one-point, yeah, the one-point, yeah, yeah.

Question: The picture you seem to see this image gen topological space, but this is supposed to be something associated with Z, right? That's a good question. So, by definition, I made it a - I was saying it's a closed subset of the topological space. Now, you can view this as an analytic stack. We saw how to view this as an analytic stack, and you can take the product in the category of analytic stacks - that's perfectly legitimate. It's no longer finite dimensional - sorry, it is metrizable, it's no longer finite dimensional. So, we didn't quite talk about this thing, but you can still view this as an analytic stack, and you still do get a map of analytic stacks from N to this product, and this closed subset does correspond to a subanalytic monomorphism of analytic stacks here, and the map from N there does factor through that closed subset, and so you can view it in several different ways.

Okay, to explore this, so consider a fixed prime p, and consider just the norm of p. So, here's the first claim: we can understand the locus in this universal space of norms where the norm of p lives between zero and one. This is just the same thing as the stack parametrizing norms on which the variable p lives between 0 and 1 - let's call this N and Z less than absolute value of p less than one. This is equal to spec Qp, the Gauß version of the p-adic numbers, across this stack associated to the open interval from 0 to 1.

Yeah, well, in fact, this is quite easy to see because we know that the universal analytic stack equipped with some variable whose norm is between zero and one is spec of Zq hat plus or minus one Gauß cross 0,1. And then you just have to impose that that variable becomes p, so you just set q equals p or you mod out by q minus p, and then that, as Peter discussed when discussing this Gauß base stack, gives you some analytic ring structure on the p-adic numbers, and then the second variable doesn't really change.

And so, I'm claiming in particular that if you look at the universal - let's say we take the fiber over a point lambda in 0,1, then you get a normed analytic ring structure here. And it is - so what does that mean? It means that for every radius r, you have some notion of overconvergent functions on a disc of radius r, where the notion of on a disc of radius r is the usual one from non-Archimedean geometry, where you take the normalization of the absolute value on the p-adic numbers for which the absolute value of p is equal to this lambda. So, what we're seeing here is the kind of the interior of the p-branch here is kind of fairly... Straightforward to understand. So, next, let's look at the locus where another locus that's fairly easy to understand is the locus where p is between one and infinity, strictly between one and infinity, because that's the same thing as saying that p has to be invertible. The absolute value of p is away from zero, and then it's the same thing as saying that the absolute value of $1/p$ is between 0 and 1. We can again use the exact same argument to understand what this is, and what you get is you get $\mathrm{Spec}(\mathbf{R}) \times [0,1]$, and the argument is the same. As Peter explained, if you take this ring and mod out by setting $q = 1/p$, then you actually get the real numbers. You get a certain analytic ring structure on the real numbers.

Again, if you look at the universal norm with a fixed value of λ, the universal norm here is the usual one given by convergent functions in Archimedean geometry, say complex geometry, but with respect to the norm, which is a power of the usual absolute value, where α is such that the norm of $1/p$ exactly gets λ.

I'm not quite sure where the over-convergent functions show up. You had some formal ring with q, but then you specified that it is the usual over-convergent functions. The first thing to understand, of course, is why when you take this ring and specialize to $q = 1/p$, you get the real numbers. That has to do with some kind of base-p expansions.

Then you have to understand what the base change of the module p is. It should be some module over the real numbers, some sequence space with some summability condition. You can see that the summability condition is basically just some exponential decay, as Peter described.

The point is that this thing does sit between the usual ring of over-convergent functions on the unit disc and the ring of holomorphic functions on the interior of the unit disc. When you do this over-convergent business, the subtlety of exactly what ring it is and what summability property you have doesn't matter anymore, and it just returns the usual ring of over-convergent holomorphic functions on the disc, with some scaling of the usual absolute value.

In particular, this locus is independent of p, because the universal norm didn't really depend on p. There's some rescaling property of the norm, but it doesn't affect this subspace of the universal space of norms. This is the kind of thing you need to see to understand that you're getting the Berkovich space. You have some infinite-dimensional space, but the conditions on the various prime numbers are very tightly related to each other, just like in the classification of norms on the integers.

Other and is determined each is set of norms on z. Yes, not up to equivalence. No.

Question: all the point right? Yeah, why that would be. Is there some explanation why that would be for me?

It's a bit of a calculation. I mean, in our axium for normed analytic ring, we had no version of the triangle inequality whatsoever. We just had this that the norm should be multiplicative. But then you can look and see well, if you believe what I'm claiming, then kind of almost all of it does almost all of the space does satisfy the triangle inequality because you can just check on in terms of the rings of functions that are being assigned, and you can just verify that the triangle inequality holds. So whenever you're in the ordinary Banach space of Z, you actually satisfy the triangle inequality, and then okay, it's a you get some sort of quasi-norm if you move out in this direction, and this part is a little funny, but if you throw that away, so there's always some version of the triangle inequality that is automatically satisfied just as a consequence of multiplicativity, and that's kind of funny.

Yeah, in the usual, the like, in fact, was consider earlier, then I mean, it's I'm speaking now about Urs classification, the or maybe, so you can put the following condition. I don't remember which reference, instead of inequality you can put like x plus y val less equal to some constant times x y, and then one can prove that after a normalization by some power, you have the triangle inequality or even the.

Yeah, and so is it the case that here you can is related somehow where, yeah, in the locus where, so if you know that the priority that the nor some integer like or three not infinity, then you can get the triangle inequality by scaling it.

Yes, exactly. Yeah, so if, well, I have on everything now, I'm speaking about Bierz. Like in the big, let me make some further claims.

Claim: if you have a normed analytic ring with the norm of 2 say, doesn't matter, less than or equal to 1, automatically satisfies the non-Archimedean triangle inequality. A normed analytic ring with norm of 2 less than or equal to 2 satisfies the usual triangle inequality. Oh, I sorry, let me move this, is the that no of 2 two is equivalent to no of 3 three, yes, yes. Two is an arbitrary prime number here, about to 6, is it that's also equivalent, yeah, yeah. So I guess prime is not so important, yeah, two, yeah, two is an arbitrary integer bigger than one.

Okay, so, and then, where am I now? Oh, yeah, so a normed analytic ring with norm of 2 less than infinity always, there always exists a constant C greater than zero such that the norm of x + y is less than or equal to C * the norm of X, the norm of Y, but this is interpreted not in the sense of normal functions, but it's not. Yeah, it's some universal thing, like, you know, you write down, you have whatever you have, yeah, you have P1, like, yeah, the locus where the norm of the T variable is less than or equal to a, cross P1, you know, locus for the S variable is less than or equal to B, um, yeah, that this maps to, so it's a and b are less than infinity here, so it's this maps to P1 R, and then this maps to Z Infinity, but this should factor through zero, and then C * a plus b.

Yeah, so, and this is addition A1, yeah, they, it's well defined because this happens to live inside A1, which we already argued earlier.

Yeah, yeah, so there's always some version of the triangle inequality, um, yeah.

How do you prove these claims? By the way, these claims imply the claim about what the image of n is, because, um, so, yeah, so, and then this extended Berkovich space, the one I mean the specific one that I wrote down, the reason is that you can, you can look at the norm of two, which goes to zero infinity, ah, yeah, if you want to, if you want to know the image of something, you can actually work, you can actually work on stratifications of your topologic, you don't have to work in closed covers or open covers, you don't have to work locally Over the locus where 2 is less than or equal to 1, the locus where 1 is less than 2 is less than infinity, and then the locus where 2 is equal to infinity. We then have to take the union of the images we see there and here. If you believe this claim, then there you have the non-archimedean triangle inequality. So, here it's automatically a subset of just by some universal argument, the non-archimedean Berkovich spectrum of $\mathbf{Z}$. We're contained in the claimed locus there. This thing we already classified is contained in the archimedean locus, the archimedean locus. So, the last thing we need to do is to see that this locus consists of just one point.

We need for all n different from 0, 1, and -1. And that actually doesn't follow from the claims well. Yeah, well, the claim that these conditions are independent of 2 does give this as well. The last thing you say plus be, that's true. You can also directly argue that if 3, for example, wasn't sent to infinity, then you'd be in one of these loci, and then 2 wouldn't be equal to infinity.

To prove these kinds of claims, you just calculate. For the first part, we only know it's not larger than once, and it's not implied directly. Then you also need to see that if you take any point and then take that, there exists some normed analytic ring which has those values. We already saw it for the points in the interior of the rays in the Berkovich space, and it's also quite easy to hit the center because you just do some non-archimedean geometry over some Laurent series ring with $\mathbf{Q}$ with the trivial norm, or you can hit the points at the end by doing some non-archimedean geometry with $\mathbf{F}_p$. This gives the containment, but you can actually see the other inclusion by just exhibiting.

To prove, we can assume we have this $\mathbf{Q}$ with a norm of $\mathbf{Q}$ between 0 and 1, and then we're working over this Gauss to base. For the first claim here, the non-archimedean claim, what do we need to do? Well, then the universal case, given that we fix this data, we're just asking that the norm of 2 is less than or equal to 1, so it just lives over an idempotent algebra. This was because of the claim that you have a cover. You can check things like the non-archimedean triangle inequality after passing to the total space of a cover.

This idempotent algebra is just the ring of holomorphic functions, so to speak, the overconvergent version of the unit disc, and then you just mod out by $T-2$. This gives a power series or Laurent series with integer coefficients that converge to radius zero, which are converge on some unspecified open disc around the origin. This already shows that the condition is independent of 2, and then you can just look at the universal over this and check the non-archimedean triangle inequality.

The only other locus you need to worry about is the locus where you're... Between one and two, and then you're in this non-Archimedean branch, and we saw you get the usual thing: you have the triangle inequality there. And then, to prove this claim, it's actually enough to - I think it's easier to prove the root. The root is, it's easiest to prove this claim, and then that implies the claim, because this means that the only other possible point to consider, we're living in the Archimedean locus, so we're just some rescaling of the usual absolute value. And then, this weak triangle inequality, this quasi-norm triangle inequality, is actually satisfied.

To prove this, you again just do a calculation. It's sort of similar, except you're setting t equal to 2, or you're setting the inverse of t equal to some weird version of functions convergent on the open disc. And again, you just observe that it's independent of 2.

In this locus, your convergence property is shrinking down to zero. In this locus, the locus relevant to this claim, it's kind of the Laurent tails that are forcing the convergence out to the boundary of the unit disc.

As mentioned, there's no course this Friday, but please come here, and Peter will be here in person, and so will three other people giving talks, some of which are relevant to the material here. Next week, I'll continue this discussion of Berkovich geometry next Wednesday, and then the next Friday is actually going to be the last class, so we're really almost done here.

Regarding the question about the ontic stack sitting over the classical regions, and what it means if it sits over infinitely many points, it's a bit weird. The norm is quite different - it's an analytic norm, not the norm on a ring. This relates to the statement about the set of norms being like the rescaling of the norms and forming a stack, where any analytic space has a unique map to this stack. According to the website, we have two more weeks. However, we may have to change that because there was some... Apparently, on the IHS website, it says we have two more weeks. But I'll get it fixed because the number of talks was originally more than the actual...

We did decide that next week is the last week, right Peter?

Yes.

What's that IASS?

Here, I see. In BOND classes, officially end, that's why we're stopping.

23. Berkovich spaces II (Clausen)

https://www.youtube.com/watch?v=vXZC3WzKZgo&list=PLx5f8IelFRgGmu6gmL-Kf_Rl_6Mm7juZO

Unfinished starting from 0:00

This lecture is about Berkovich spaces, which is a topic I began discussing last time.

Let me remind you of the classical setup. You have a Banach ring, which is a ring equipped with a norm that is submultiplicative and satisfies the triangle inequality. The norm of 1 is equal to 0 or 1, unless R equals Z. And R is complete.

Berkovich assigns to this a compact Hausdorff space, $\mathcal{M}_R^{\text{norm}}$, which is a subset over the product over all elements in R of the closed interval from 0 to the norm of f. A point in this space is denoted as X, and it is literally an evaluation map from R to the non-negative real numbers, which is now multiplicative and satisfies the triangle inequality, with 1 being 1.

R is not complete with respect to this norm X. In fact, there could be many elements of norm zero. However, if you complete R with respect to any norm X, you get a complete valued field $\mathcal{K}_X^{\text{hat}}$, which is sort of the residue field in the sense of Berkovich theory.

There are basically three cases: archimedian, non-archimedian but discrete, and non-archimedian non-discrete. In the archimedian case, there is no real variety in complete normed fields, as it is either R or C. In the non-archimedian discrete case, you have the trivial norm. In the non-archimedian non-discrete case, the value could come from a discrete valuation, and there is an ambiguity in the normalization.

Now, the goal is to promote $\mathcal{M}_R^{\text{norm}}$ to an analytic stack, using the stack of norms, $\mathcal{N}$. A map from an analytic stack $\mathcal{X}$ to $\mathcal{N}$ is the same as a certain map from $\mathbf{P}^1_{\mathcal{X}}$ to $[0, \infty)$, satisfying multiplicativity.

Last time, we investigated the geometry of $\mathcal{N}$, and saw that it lies over the extended Berkovich spectrum of the integers. The triangle inequality is what made the difference between taking an arbitrary power α and having something that behaves like a norm, versus just a quasi-norm.

Plus, you have also the kind of the limit at, yes, there's also the limit at Infinity. So, that makes, yeah, so, and in this stack of norms, there's no triangle inequality imposed, and it turns out what you get is this non-strict triangle inequality where all powers of the usual Archimedean normal are allowed. Then, also, there's a some kind of very strange limit point where your Norm takes infinite values on natural numbers, and then you have the Archimedean ones, so two Tic absolute values which end here in a point which has both characteristic zero and characteristic P Behavior, but where the norm of two equals zero.

For the other primes P, we also kind of saw what the on the interior of these line segments you were getting the some, the gaseous R theory on the interior here you're getting the gaseous two Tic numbers, and then as you move along the normed, the norm is changing, but the kind of, so to speak, the analytic ring is not. In Q3 and so on, and then here you have things living, you have F2 living for example, you have things living in characteristic two there, living in characteristic three here, and here you have things living in characteristic zero.

In some sense, you can imagine that the points of this stack correspond to something like these complete valued fields or the minimal choices of complete valued Fields like you have real numbers, Adic numbers, you have discrete FP, you have discrete Q.

So, that's kind of a substitute for the notion of multiplicative valuation, but then we still have to input our Bon ring R into the construction in order to get something non-trivial. Here's the definition, and I don't know what good notation is, I'll write it like this.

This will be an analytic stack which will be a substack subset of stack of norms cross, and then some Affine analytic stack which is just we take R with a trivial analytic ring topology, I mean sorry, the trivial analytic ring structure. So, what I mean by this is that you take an R, R is a Banach ring, so it has a topology, but you can also view it as a light condensed ring. Using the topology, you consider those condensate, yes, and then trivial analytic ring structure that is all modules are allowed, yes, even not okay, it's the full, you know, condensed derived category of this condensed ring. In this map, so it's a condition that you can check in practice, and so is it the case that you have. Since R is cool, you can think of it as a limit of C sub Rings, which are account many elements. Yes, so you can probably reduce to, no, but it maybe, it's not Dimension. Well, I'm not sure, but no, no, but you can look at as a CO liit over finally generated sub rings, and then, and that's always embedded in a finite I mean, okay, okay.

Okay, yes, so that's one way. So, there is indeed a canonical way to write this as an inverse limit of finite dimensional metrizable spaces, but let's not do it. Let's just work with this setup here. And is it true that you have inverse limits in analytic stat? Yes, you have, you have all limits in analytic Stacks, yeah. Ah, okay, so you can syn of it as a limit over the stack, is a limit over the, well, this isn't a stack now. This is just, I'm just viewing this as, yeah, when you have those nice sings, you can construct.

Okay, so it's not, no, but then you can take for the nice subing, you can take the stock Associated to m in those limit, you get something which is, yes, you get something. Then you get something, the only problem I have with that, it's not a real problem, but just is that if you started with something which happened to be already be finite dimensional and metrizable, then you'd be non-trivially writing it as an inverse limit of other finite dimensional metrizable things, and I mean, you know, so let me just not, let me just not get into it.

Okay, yeah, one other question. When R is State, then we we also show that we can localize over the spa, and then there's a map from the spa to the. I mean, that's right, that's right, yeah. There's a commutative di, there's a yes, there's a commutative diagram that you can write down where here you have a map from, you know, the Huber space mapping to this, and then you have a some this thing, and then you have the the solid guy here mapping to that. And this this last Arrow, I I mean, no, no, yeah, when I I I'll discuss in more detail what this looks like in the Tate case, and then, and then you'll see.

Yeah, okay, so in particular, just I want to highlight that we get a structure chief on this topological space and even a structure chief of you know of infinity categories, so a theory of Quasi cerent shes on the usual burkovich space. I'll explain in basically the Tate case how it's pretty easy to calculate this structure chath and see what it's doing in the case of Rings like the integers. Well, the integers, you can kind of do it by hand, but it would be interesting to compare. So it's we I'll explain basically how you compare to the cases burkovich discussed, but it would be interesting to compare to Puu who kind of more or less by hand described structure sheaves in certain cases over the integers. So this is a different different approach where you define something which is a prioria structure sheet, and then you have to calculate it, which can be done in principle, but you know, takes a while. In Poo's case, he explicitly assigns the value, and then he has to maybe prove some, yeah, prove some descent results. So here we automatically get some sort of infinity descent, but then you have to calculate the value.

Okay, to like I know this and should be easy to say what I on a disc the structure sheet. Yeah, it does reduce to seeing what goes on on a dis, but to see what goes on on a dis, yeah, so I mean, I'll explain what you need to do to do these calculations, and I think you'll see that it is like, but probably there are like in the case of ho Rings, there are probably some derived phenomena, because when you want to look at the things like the algebra functions on close or open this, anyway, you you quau something by certain, I mean, you have non closed idea, so probably you need to work in some derived sense to get the right, to get what you get from your fan Theory, you should probably have the drinks which are complete. We do, we do, we do, okay, yeah, I mean, that's what.

Yeah, so I do believe that in cases like what Poo considers, where it's you're starting with The Universal Locus: Where the Absolute Value of T is Equal to 2.

So, if we pull back along this, then we look at the Spec Berk R, Norm, Norm, and then $SpecZ_q$ plus or minus 1, and then we get this Universal disc or some Universal annulus over here. Let me just call it y.

For this y thing, we've already got the norm, and we've kind of artificially adjoined an element of Norm 1/2. But the only thing we need to ensure to go from this to this is we need to give the second map, we need to give the map to Spec of R with a trivial thing, and we need to see that they agree, and we need to enforce the condition that this Norm condition that NF lands inside that part there.

So, to get y, you just take Spec of Z_q hat plus or minus 1 gas cross Spec R, and then pass to the closed subsets given by the idempotent algebra obtained by taking the norm F inverse of these closed subsets.

This Berkovich spectrum here has a fairly simple cover by an afine, and to calculate this afine, the most difficult part for a completely General R is already in the first step, that is making this product and calculating what analytic ring this is, and in particular, calculating what the underlying condensed ring is, because what this tensor product involves is you have to calculate the gaseous localization of R.

Once you have that, I'm going to explain that calculating these idempotent algebras is not that hard. So, once you know the ring you have here, then you're passing to certain idempotent algebras over it, and that's not the hard part of the calculation.

If you're in a situation where you can do this calculation, then it's quite feasible to calculate y, which is giving a presentation for this stack. We also saw that the Descent for this cover is quite simple; it happens at the zero stage, so the structure sheaf here will just be a retract of the structure sheaf there.

The map to the Berkovich Spectrum is kind of interesting. On the Spec Berk, you have the universal Norm, let's call it n, and then for every element in R, you have, by fiat, a map to the structure sheaf here. Then, we get a map from Spec Berk R to the product over all f in R of the zero Norm of f. But since we enforce that every element of R, the norm of that element is bounded by the prescribed Norm on our boning R, that in particular implies that the norm of 2 is less than or equal to 2, which means that by last time, the triangle inequality holds for n, and that means that this map lands inside the Berkovich Spectrum. This construction, we could also keep some of the which does not satisfy the triangle inequality, right? Yeah, I mean, I, um, yeah, it's it's natural from the perspective of this stack of norms that we've been discussing as we've seen to relax the triangle inequality, and it's, and you can do that. I mean, you could, I mean, the the formalism is quite general. There's, I the reason I I stuck to the classical thing is just because it's the classical thing you have for the triangle inequality. You assume that, yeah, you could require that there exists a constant such that, uh, you know, this, that's that's one thing you could do. Okay, but then if you want to, uh, ah, so this still defines a uniform structure, so you can say it's complete, and, uh, and it is not equivalent by slightly changing things to an actual norm if you have this.

Well, I don't know because for fields, just by power H, is that true? This wouldn't be, I think this is something, uh, may be, maybe it's true, yeah, yeah, but you could also conceivably allow the norm to take infinite values and try to, um, tried to build that into things as well. Yeah, I just, I wanted just wanted to stick with the classical thing.

Um, so there is a theorem about the scaling, no, if you have a norm with the kind of, I don't know how it's called, with the constant, yeah. So, so, the theorem that for fields, I think you only get the Pu classification with an alpha, I think, yeah, yeah, I think you're right, yeah, yeah, yeah, but for rings, you don't know that because it's it's delicate because if Y is small, this doesn't imply this condition doesn't imply that the norm of X and of X plus Clos, yes, yes, yes, I I agree, it could be subtle for a general ring. I I I don't want to make any claims. I actually want to, uh, stick to the classical setting.

Okay, so this was me explaining the general case, um, but there is, um, and in the general case, uh, why, oh, sorry, this, this, this thing, despite the notation with the spec, this, um, is not going to be affine. So, for general, um, this spec, Bur R is not affine, so it's not the spec of a of an analytic ring. Um, so for example, well, if we look at spec, Bur, uh, Z, and then the usual Archimedean absolute value, which is the, um, kind of the maximal, the every every norm you could put on Z will have to be less than or equal to this one, so this is kind of the the choice that gives you the biggest possible Berkovich spectrum. Um, uh, this is a, this is just this, uh, locus where, yeah, two, absolute value of two, is less than or equal to two inside this stack of norms, and it really is a stack as you can kind of see at the at the points that live at the boundaries.

Um, uh, okay, so however, uh, suppose, so let me make an assumption star, um, that, uh, so there exists a, let's say a pi in R, uh, such that the norm of pi is less than one, uh, pi is a unit in the ring, and I want it to be that it strictly multi-, like the norm strictly multiplies when you multiply by the norm is multiplicative with respect to multiplying by pi.

Um, so, uh, so this, uh, well, this condition is obviously not satisfied here, but it's satisfied quite broadly, so, so example, so any non, any non-discrete, yeah, I apologize again, non-discrete, I'll put it way over here so you don't think I'm saying non-discreetly valued field, but non-isotropically valued field, uh, uh, yeah, admit such a norm, sometimes they say nontrivially valued field, okay, yeah, yeah. Um, so you there you have multiplicativity for all elements, and then, uh, if it's not discretely valued, then there's something with norm between zero and one, and it that'll be a unit, and, yeah, of course there is a slight ambiguity in valued field because sometimes it refers to absolute value, sometimes to crude valuations, it could be higher rank, and then

Is $\leq$ say a constant, nor effect this. Define still upap on ver spaces. I suppose that your definition does not depend. That is, if let's say you have two Noes which are equivalent in this sense by constants, then all of this will be the same for the two Noes. I suppose, but well, is to check something not quite. So I mean, let me say what I can say, and then, yeah, sorry, I just, okay, yeah. But in, under this assumption, you mean or in general, I think, yeah, yeah, yeah, I agree with that. So that's why I want to postpone the discussion, yeah, a bit.

Okay, so in particular, I mean, the classical settings in Berkovich Geometry, you work over some fixed field which is not, well, often non-discrete, and you're working with Banach algebras over that field, and they will certainly satisfy this condition $\star$. But if you're working over a discrete ring, you know, you won't have this condition $\star$. So, so then I claim, well, let me give, yeah, we're also, yeah, so also any, any Kate Huber ring has a norm defining the topology, satisfying $\star$ with π a pseudo-uniformizer, so also some mixed characteristic examples exist.

Right, so, where am I? Oh, yeah, so then, claim, if $\star$ holds, then $\mathrm{Spec}_{\mathrm{Berkovich}}(R, \|\cdot\|)$ is affinoid and corresponds to an analytic ring structure on the condensed ring R. Moreover, this analytic ring structure only depends on the condensed ring R, not on the norm satisfying $\star$.

I think you're right, I think you're right, I think you're right, yes, yes, yes. So similarly for a morphism, if it is only with a constant, you could still apply the same thing. I think you're right, $\mathcal{O}_{\mathcal{E}/\mathcal{S}}$, yeah, so this, because in some, I remember that in some text, I don't know if the book of Ver, in some place they consider such things, which is more, apparently more natural, because I don't know the why, but it's probably, you can't, I think you're right that because our Norms are by definition multiplicative, I mean, these geometric Norms that you can, you can argue exactly as you suggested, yeah.

Um, yes, that's a good point, thanks. Okay, right, so, what doesn't depend on, well, on this thing, you have this universal Norm n, so the universal norm n on $\mathrm{Spec}_{\mathrm{Berkovich}}\, R$ does depend on the norm you choose on R, but any two choices are equivalent under some map α from $\mathrm{Spec}_{\mathrm{Berkovich}}\, R$ to the positive reals, so Norm passes to Norm to the α. For some map α from $\mathrm{Spec}_{\mathrm{Berkovich}}\, R$ to the positive reals, so there's a scaling action, I'm referring to the fact that there's a scaling action which sends a norm and a continuous function α to the norm you take the norm and you compose with the α map on exponentiation map on on zero Infinity exponentiation by α on Zero Infinity.

These... Okay, okay. So, the proof... Um, um. So, I'm going to explain how to produce this analytic ring structure on the Condens ring R. Um, so take Pi, as in star, um, then we get a map from $\mathbf{Z}_q$ to the Condens spring R which sends q to Pi. Um, but uh, Pi is of norm less than one, which implies that it's topologically nilpotent - its sequence of powers tend to zero. That implies that it factors through this ring here, and it's also a unit, um, by assumption. So, we get a factoring through this ring here.

Um, and then, uh, so, uh, we need to check, or we want to check, um, that R is gaseous. Recall that this Gaseous theory was a non-trivial analytic ring structure which was produced by taking, uh, by realizing that the category as a full subcategory of over this ring, um, and then there's this completion procedure which changes the underlying ring to this gaseous thing, but the category of modules was just described at this level.

Um, so, uh, so, and this is something very, uh, this is something very straightforward, because the definition of gaseous was that some map from $\mathbf{P}$ to $\mathbf{P}$, $\mathbf{P}$ being the universal null sequence, so to speak, namely $1 - t * q$, uh, should be an isomorphism on maps to R. But, um, when you map out to R from this null sequence space to your Banach space, um, you're just getting the space of null sequences in the Banach space, so that's equivalent to saying that if you look at the space of null sequences in R, uh, and then you have some $1 - q * shift$, this should be an isomorphism of condensed, uh, of Banach-Banach alien groups, say, um. But this is, uh, but it's easy to see what the inverse is supposed to be, and to write it down, you need to just, you need that if, uh, sort of F_n is a null sequence, uh, then you can sum, uh, uh, $F_n \pi^n$ and still get an element in R, um, and, uh, the condition on Pi and the usual triangle inequality stuff, uh, lets you write this down. It's just the limit of the Cauchy sequence, um, so it's quite straightforward to check that R is gaseous, um, and then we can just take the induced analytic ring structure.

So, what? Oh, I said I meant to, I keep saying liquid instead of gaseous, yeah, it is gaseous. Well anyway, this, uh, take induced, uh, analytic ring structure, uh, from $\mathbf{Z}_q \pm 1$ gaseous.

Now, recall that on, on the spec of $\mathbf{Z}_q$ hat plus or minus one gaseous, we have a universal norm, uh, with the norm of Pi strictly equal to, uh, this. Okay, now maybe let me say R is not the zero ring. It's the zero ring, I leave the claim as an exercise. So then, if it's not the zero ring, then this Pi will have to have norm bigger than zero and less than one, um, and then on, uh, this, we have the universal norm where the norm of Pi lands inside this singleton subspace, maybe I'll write it like that to remind you that this is kind of a subset and not a value.

Um, uh, right, and then we can, then, then by functoriality of norms, but because norms pull back, we get a, get a norm on, uh, on what is Pi, what? Ah, q, uh, Pi, Pi is q. Pi is our, we're fixing one of these guys, which exists by hypothesis.

The q, q, oh, thank you, thank you, thank you. I'm sorry, yes, yes, thank you.

Yeah, uh, right, so we get a norm on R with an induced, uh, analytic ring structure, um, and the claim, so this normed analytic ring, uh The Singleton value pi and it's easy to see just by tracing through the construction that this is universal with respect to that or with respect to those that structure. So what's the difference with the thing we're trying to compare to? It's that in instead of having a condition on just the norm of Pi, which we have here, we have a condition on the norm of every element. So we need that this condition on a norm is equivalent to the condition that Norm of f is contained in zero f for all F and R. So we need this equivalence.

One direction is quite easy. If you have this, then you apply it to Pi and to Pi inverse and you deduce that. The key is to see that just telling you what the norm of Pi is, then I know I've constrained the norm of every element in our offering. For that direction and because it's good to know, we will calculate the Universal Norm.

More precisely, what is a normed analytic ring structure? Recall that it amounts to specifying some item potent algebras over P_1. So we'll calculate the algebras. For all less than c, a norm in our Sense on an analytic ring is implicitly just telling you what the overon convergent functions are on a disc of arbitrary radius centered around the origin. My claim is it's going to be the usual thing from Berkovich Theory, so this is equal to filtered co limit of let's say radius bigger than C of you take the, I'll explain what this is afterwards, but kind of you can make a universal Bano ring where the norm is less than or equal to R.

Is this local or now? I don't know if I have fin time, how much time you need. Well, you take the time you need and when it's precise, ask again.

Okay, so this is like the formal series that when you replace each coefficient by the absolute value and replace T by the ab by R, you it converges, the sum is is finite. So this is the set of well, it's just the coefficients but let's say $R_N T^{T_N}$ such that some absolute value R_N. Okay, so this is the norm, this is the on. I was also making claims last lecture about calculations of the what these overon convergent functions were in various cases, so I'd like to explain how to make these calculations and it turns out there's a trick where you really don't have to do anything, it's just kind of purely formal.

There's a trick to calculating, of course this is in the condensed, now you have to view this as a condensed, yeah, this is Banach and then it's, yeah, so it's condensed, yeah, cond, yeah, and then this filtered colum it is taking place in the condensed category. Oh yeah, so well, what is it we need to calc, what is it that we're calculating here actually? We're taking what we're doing is we're well, we're trying to calculate we have the universal thing over this gaseous base, which we more or less wrote down, and then we have to tensor it with the gaseous tensor product so over this analytic ring with r where here Q goes to Pi and we have to, I mean actually you know a priori it's a derived tensor product but this is the kind of thing we need to do and if we're being too naive about it, it can look kind of tricky because naively what you'd do is you'd write this as as we've explained is some filtered co limit over copies of P, so that's kind of over convergence, and then you'd take you'd first calculate P tensor R and then you'd pass to the filtered co limit but actually it's not so easy to unwind what P tensor R is, in particular it's not so easy to see that it would be concentrated in degree zero, so let's use a trick.

Let's not use this approach that was how we produced this thing in the Universal case. Recall the idea was that the this P was some version of functions on the open unit disc and then when you have this q and maybe all of its fractional Powers you could scale that open unit disc and get some version of functions on an arbitrary disc and it wasn't the correct one but when you make it over convergent it doesn't matter, it'll be by some kind of. Sandwiching argument, okay? Alright, so the trick to calculate is some general category theory fact, so, so LMA is so. If so, so C is symmetric monoidal and's say infinity category, it's not too relevant, um, but then, uh, so if you have a tower, so X1, X2, X3, uh, in C, where each map is Trace class. So, so X to Y is Trace class means it comes from a map X, so from the unit to x dual tensor y or X dual, I'm not assuming X is dualizable, this is just the internal H from X to one, uh, sorry, yes, us closed, thank you, m. There's probably a way to, well, never mind, um.

Uh, where are we, um, ah, then, then, for all Y in C, uh, we can calculate the co-limit over N of X and dual, uh, tensor y. So we pass to the, we have a tower here, we pass to the Dual thing, which gives a sequence, and we take the co-limit over that sequence, um. Then this, uh, is the same thing as co-limit over N of the internal H from xn to y.

So, uh, this is Elementary, um. I'll leave it, I'll leave it just like that without giving the proof. You just have two systems, and you make two in systems, and you make maps backwards that go up a step using the Trace class hypothesis, okay? So, again, you, how do you know there is a, the limit makes sense?

Okay, well, this, this is actually an equality of IND objects, so, uh, I mean, it's an equality of end objects. Does it, doesn't it, doesn't matter, okay? As an in object, and you have to know what is the end for, for, uh, this is end for, for C in in C, which makes sense is an Infinity.

Okay, um, yeah, so in particular, if C has co-limits, you, you can just, you can remove the quotation marks, um. If C has co-limits, and tensor product commutes with co-limits, which is the case in our examples, then you can remove the, I mean, you can, yeah.

Okay, uh, right, um, okay, so, yeah, so what we're going to do, so, yeah, in particular, well, yeah, so, well, so what we're going to do is we're going to recognize, uh, so if you take y equals the unit, then, um, you know, and yeah, that this object here, um, coim of the internal H. Ask a technical question, yes, about the definition, it's nice. What do you mean by it comes from a map? It's that the, is given by this, ah, right, so if you're given a map like this, then you can tensor it with X, you get a map from X to X tensor x dual tensor Y, and X tensor x dual has an evaluation map to the unit, so you then get a map from X to y, yeah, yeah.

Um, right, um, so, so we're going to, we'll, so we, uh, will recognize, uh, C as a co-limit over now P, du, um, and apply this with Trace class transition Maps. So once we do that, then we reduce to, uh, reduce to, um, uh, some, to looking at null sequences in R again, and then just some, so some filtered co-limit of some space of null sequences in R, and, um, you can actually modify this to the thing where you require these to form a null sequence, and they wouldn't be the same at each term, but it's quite easy to be, they're the same when you take the filtered colum again, any two versions of the unit dis are kind of the same after you make them overon convergent, um, so, uh, yeah, and then the calc, the calculation is very easy, uh, once you once you do this, and for this, uh, uh, for that, we can use S Duality on P1, uh, so over, well, it happens to be over the liquid base, but it, I mean, the gaseous base, but it doesn't much matter, um, so St Duality on P1, this, if you, so, and the, and the six funter formalism, so, um, sidity on P1 The Dual on the other side, but we can do it with a trick more or less because, so this gives also, so this, this over con, this by the over convergence, you get that the the single Dual of the end object, sorry, sorry, we can then we can write this, this is a pro object, we can, we get, so yeah, we get, we can get this, we can view this over convergent thing as an end object, and it's dual will be a pro object, and it will be the pro object given by this thing where you increase the radius as well. But then now we can view that Pro object as an inverse limit of the over convergent things with the the non-strict inequalities over there, and then, and then use The Duality result in this direction on each of those.

There's some trick, trick with over convergence. This implies that if you take the the Dual of this, so if you view this as a pro object, because it's an inverse limit of the things where we have a yeah greater than, then the Dual of that Pro object is the end object we're interested in. And that gives a, that gives an expression exactly like this, that this guy is a co-limit of du of P's. We still need the trace class claim, but I claim that that also holds for soft reasons.

There's a general topology fact that if X is a topological space and Z and Z' are closed subsets such that there exists an open U which lies in between them, so two closed subsets which are separated by an open subset, then the map from the push forward of the constant sheaf, the Restriction map, is Trace class in the derived category of sheaves on X, with values in D of Z, which is not the category of X in general because it's right.

So then on the level of just this closed interval from 0 to plus infinity, then any of these transition maps will be Trace class for this reason, and then you can pull back, that's a symmetric monoidal functor, you get that Trace class, you get a trace class map in the derived category of P1, but it lives on A1, and then there's another trick to see that its image under the forgetful functor to the base is also Trace class. So there's pull back to A1, use another trick, and the conclusion is that the Restriction map, say from any of these over convergent guys, is Trace class. And then also for pres, yeah, then again by sandwiching different versions of the discs and changing the Radia, and using the trace class Maps as a two-sided ideal in all maps, then you get the presentation like this, which let you calculate, do what by hand? You want to write over functions on this disc, I mean the declanse is to use pH pieces, yeah, you can just system and see that transition has really dis, yeah, that there's, I believe you can do that. Certainly, I remember doing that in the complex case, and I assume it works over the gashes base too, but I thought it was nice to be able to do it without doing any calculations.

Okay, you still need to know that the way you presented is the way you scene presented. Yes, that's true, that's true. The remark was I wasn't being very careful here about writing what the filtered systems are and all this. You have to show that it is there.

Okay, so what was a bit of a digression. So what were we doing? We were calculating the normed analytic ring structure, and the conclusion was that the norm on Spec R gashes is given by usual over convergent functions on dis over r. So then you need to, so to see what did, what were we trying to show? We were trying to show that for this Norm here, this Universal Norm that we produced by fixing the norm of Pi, that automatically the norm of every other element is correctly bounded. So to show Norm of f contained in zero f for all f, it suffices to show it translates into, oh no, now we have bad notation, it's not clear a priority that it is finite, sorry, you have to know even the fineness is not a statement, that's correct. So we have to show that if you take this and you mod out by T minus F, you just get R, this. This is Elementary, and indeed this is Elementary, so the map giving this is of course setting T equals to F, and it's quite Elementary to see that you get the correct short exact sequence of banak spaces, so that the kernel of this map is.

T minus. f is of the. That mere existence of the m is mere existence of the map. Enough. Oh, that's a really good point. That's a really good point, Peter. Yeah, thanks. Yeah, so what Peter was saying?

Yeah, so what Peter was saying is that we know a priori that this thing is an idempotent algebra over A_1, so over the polynomial ring on one generator. Therefore, when you base change it along here, then you get an idempotent algebra over $R[T]/(T-F)$, which is R. So this thing is an idempotent algebra over R, and so is R itself. And if you want to show that two idempotent algebras over R are equal, it's enough to just produce an algebra map between them. Thanks a lot, that indeed makes it very. No, just, we already have the unit map. Yeah, we have a map in both directions indeed, but we already have the unit map.

Let's see. Because when you do it analytically with this, instead of over conversion, just conversion on the closed disc like before, and of course you have a map when f is less than or equal to C, you get them up. But to prove division, you still get to some convergence is not okay in the Archimedean case, and so it is, but it's not idempotent. It's not idempotent in that case if you don't do the overconvergent one, if you just do the one without the overconvergence. It's not going to be idempotent, as the argument would not work exactly. So somehow the proof of idempotent is you must have already done work similar to showing that.

In the non-Archimedean case, the thing with rigid algebras does work, and in this case, rigid algebras are important, or not, they are, yes. When you do it with the non-Archimedean, I mean, the everything non-Archimedean, then, why is it? I mean, we basically proved it when we discussed the solid theory, but maybe, if everything is not so, there you can use the solid, then it is, right? So, in any case, the map exists because you can evaluate at Tate's F, and as Peter points out, that's enough. Thanks, Peter.

So, the first part of the claim was that this thing, the Berkovich spectrum, this analytic stack which I'm calling the Berkovich spectrum, is pine when you have that assumption. The next claim was that the analytic ring structure is independent of the choice of the norm. Proof continued: we need that $\mathrm{Spec}(R_{\mathrm{gas}})$ is independent of the norm and π. If you have two topologically isomorphic rings, it depends on the condensation, exactly. Without the condensation, you could have one norm with this one π, another norm is π', exactly.

Let me give the independent description. The claim is that R_{gas} is the initial analytic ring with a map from R_{triv} to R_{gas} such that for all topologically nilpotent units π in R, the map $\mathbf{Z}_q^\wedge \oplus \pm 1 \to R$ given by $q \mapsto \pi$ factors through R_{gas}. This condition only depends on the topological ring R.

One can compare to Uber and get that it's the same. It is the topic of some, you can, but in the Aredian case of course there is this. In the Berkovich Spectrum, there is this condition that I mean, I think if you take the, you could also like try to make a modification of the Berkovich Spectrum, only thinking of R is a topological ring where you ask for these seminorms to be continuous, and then I think if you take that space and then you mod out by this exponentiation action by positive real numbers, then that will be the same as the Berkovich Spectrum for any fixed Norm, satisfying condition star.

No, no, no, because when Aredian, the non-Aredian things, yes, you don't because in the B space you don't identify no to its power, to its powers, so I don't see how you, no, but yeah, the identification is maybe not so obvious. It's kind of, well, I mean, I'm not, I'm not sure, but so you, because you, you, you want to claim that your, your, your Bëlovy spectrum of course it maps to the space, the Bel space as we said, yes, and but you don't claim here that the map is, because if you want to, claim the map is the same, you have to compare the average spaces, and this looks like a little bit tricky, at least in the away from the non-Archimedean case, we can understand it anywhere, I'm not sure.

Well, yeah, I don't, I don't know. Okay, so let me, okay, let me give the proof of this claim, which is kind of giving an intrinsic description of this Tate's analytic ring structure. So, note that if π is topologically nilpotent, then there exists an n such that after passing to some power, you have small Norm, in particular, you have Norm less than one. And let me note that this condition here, this is invariant under replacing π by any power, this is actually a remark that Peter made at some point, some point early on, so we can assume that π is Norm less than one.

But then, but then, for the universal Norm we built over our gaseous, sorry, well, sorry, I need to fix. Okay, so my claim is going to be, so certainly this R_{gas} that we built, we built it so that it satisfies a weaker version of this property, where you only demand it for a fixed π satisfying this condition here, and what we need to show is that, let's say, our gas was built to satisfy star just for some fixed π_0, and now we have to show that it's satisfied for all choices of π.

But then, so can assume $0 < \pi < 1$, and then that implies that the norm of π is in this interval from 0 to 1, which we already showed implies that π is gaseous, just from the axioms of a normed analytic ring.

Okay, what is $\pi_{\mathbf{Z}_R}$? For real $\mathbf{Z}$, what is it? It doesn't make sense, because I've only defined this when R is a Banach ring satisfying this condition star. Sorry, the other condition star about the existence of a π Norm between zero and one, etc. π_z, no, fixed π_0 is a, it's a, the way I built this was I took my Norm and I took a fixed π_0 satisfying condition star, and then I built my analytic ring and I built my Norm over it, and now I want to check that that thing satisfies this universal property, which means that so it was universally built to satisfy that, just for a fixed one, but then, and to have the correct Norm on there, but and then I want to argue that it automatically all of the other possible π's are also gaseous, and we can use the Norm to prove that, because the Norm is such that it, you know, yeah, well, such that we have this chain of implications.

Okay, so the last part, the last part is, right, that

With respect to this, but now with this one, it's just some arbitrary map to $0/1$. But that implies that with respect to the norm n, that if you take n of π', this is a map from Spec $\mathbf{R}_{\mathrm{Gauss}}$ to $0/1$. And then, it follows that there exists an α such that $n(\pi')^\alpha$ is equal to just a constant, the norm of π, because exponentiation acts simply transitively on 0.

So this is the thing we have to. Let me finish the argument, then we'll address Ofer's first point at the end of the argument. Then, n and n' are two norms on Spec $\mathbf{R}_{\mathrm{Gauss}}$, both with the same value on π'. By the classification of norms, they must be equal on π to some fixed real number between 0 and 1, and we showed that such a norm is uniquely determined when we proved this classification of norms.

Now, to address Ofer's question about whether this map α is pulled back from the Berkovich space. The question is whether the map $\alpha : \text{Spec } \mathbf{R}_{\mathrm{Gauss}} \to \mathbf{R}_{>0}$ is pulled back from the Berkovich spectrum. This is true if the norm of π' is, and that's true by construction, because the mapping to the Berkovich spectrum was exactly recording the norms of all the elements in $\mathbf{R}$, and in particular we're recording the norm of π'. So the answer to the question is yes.

I also think it should be true that any two choices of norms on your ring are equivalent up to exponentiation to a constant, in the sense of having a bounded ratio from one side to the power of the other, under some natural topological conditions on the ring.

Also, less than or equal to this, no. Probably, well, it will be equal to $1/2$. Yeah, okay, then it will be less than or equal to this, yeah. And then, okay, this is one idea, but then if you have got a. But by the way, for with this construction, you'd get only the triangle inequality, maybe up to some constant. Again, no, no, no, no, no. If I have what I claim is this, this is something that I check. So if you have a non-commutative, forgot now. If you have got a, in general, for uniform spes, they, they have three, three, uh, you need to, they work with three, but if you have an community group, you can do it with two. So I just claim first that if you have, if you have a topologic cian group with topology is defined by sequence of a symmetric neighborhoods of the origin, un, un plus one plus un plus, contain un, then you, you just get a metric by this by imposing that the guys in un know at most one over two to the n, so you have a metric in a generalized sense, it could be plus infinity for something. Then I will change it using the unit, my, my, uh, I will correct it using my sud uniformer, but at least I will get, uh, okay, maybe what I'm saying is a bit, uh, maybe I'm thinking too fast with some mistakes, but in any case, I think it will also come out that any two norms are, uh, in some sense, uh, yeah. But this I am not sure, any to, okay, maybe we can discuss later. Let me, let me just finish. I have only one more thing to say, and it's quite short, so, yeah.

Um, so, uh, right, um, the last thing is when I talk a bit about just say global globalization, only to say that it's trivial. Um, so we could make a definition, I don't know, I mean, a definition of a burkovich analytic space, I don't know, is a pair, um, X or triple x s, uh, Pi from, uh, local of opens in X to S, where X is an analytic stack, uh, S is a locally compact Hausdorff space, um, and Pi is a map of locals, uh, such that, um, such that, uh, locally on S for the topology, the open section topology, or the the local section topology, um, uh, it is isomorphic to spec Burke are, uh, uh, sorry, R Norm, Mr. Norm, uh, and then this canonical map Pi for some Bing R, um, okay. So this is completely trivial now, uh, to globalize, and, yeah, so the only point to note is that, uh, working locally on S, you're also automatically working locally on X, and that's because these, by definition of a map of local, is a a cover, and the open cover topology gives a cover in the sense of open covers of analytics stxs here, and those are covers in our Gro and deque topology that we use to Define analytic Stacks, they're even open covers in the sense of the six functor formalism for the what do mean local section to I mean the Gro topology on locally compact Hausdorff spaces, which is generated by open covers, okay, yeah. It is isomorphic to isomorphic to this basic object space, so you locally, yeah, so recall that these these aine ones are always compact Hausdorff, so you're not going to kind of if you if you say locally in too naive a sense, you're not going to get any examples because you know open covers, open subsets of say R, are usually not compact, right, so but if you do this usual thing of having a compact neighborhood of every point, then it's fine, but I, I, this looks it, I wonder about the derived nature of the of the r when you localize because it seems to me that, of course, you can make this definition, but then you can ask whether, for example, what does mean locally oness, if it is true, if you take D, do you have like, for sufficiently fine, for small open, it is let us say that is, if it is true for some cover, it sufficiently fine one, then probably you have to to pass to to to the ring Associated to some More sub here that the basic building rings, but then she of over conversion, so it's actually never B she of kind dis between the global Al start with, and no, but still that doesn't I mean that doesn't obstruct the claim that that there's a neighborhood base you know. Yes, I mean ofer's question was about a neighborhood base.

Yeah, that's that is a good point, and I mean you know you can modify these. You could also you know from instead of instead of these guys you could also pass to inverse limits, so you could starting with these apine guys you could pass to arbitrary inverse limits for example, like so inverse limits in the compact house door space and just filtered Co I mean inverse limits in the category of analytic Stacks, which in the Aline case is just you know Co filtered Co limits of analytic rings, and then these overon convergent things would also count is apine, and then maybe that's a little nicer uh to work with, and there's no harm in in doing that. Um, but say will not be B rings, but they will be condensed rings with certain. Yeah, they'll still they'll still correspond to analytics an analytic stack with a structure map to a compact house door space and so on. Okay, so you probably instead of B ring you can have a condensed ring with s proper, yeah, with with some Norm satisfying some properties and so on. Yeah, I mean we didn't we didn't try to give the best possible formulation, was just a just wanted to connect to the classical thing. Okay, so that's all. Thank you.

Sorry, can you again with it local section? Oh, yes, so on the on the category of locally compact house door spaces, you can define a Gro topology, where a you know a set of maps I mean it's a set of maps like x i to S forms a covering if uh for every point of s there's an open neighborhood of that point and an index I and a section of the pullback uh uh you know you pull back x i to that open neighborhood you should have a a section there. Um, and the map can be ar, yeah, the map can be arbitrary, the map doesn't have to be an open inclusion, but it's also the same thing as the gro dig topology generated by the the covering families, which are just the usual open covers. So if you look at just the usual open covers and say that you want sheath condition for that, you automatically get sheath condition for anything any any map that has local sections, so that's a so if you want if you, yeah, so the seeve will always be the same as the seeve generated by some open cover of s, and so yeah, but it's convenient when you want to talk about the sense in which a locally compact house door space is locally compact house DWF because it's not true in some naive sense, but it's true in this sense.

Okay, other questions. Another question, it seems to me if you take at least naively you take another p as the norm, you're supposed to be a constant one, uh, n Prime of Pi Prime was supposed to be a constant, but n of Pi Prime can vary over the burkovich Spectrum, but uh, I think with supposed to be a constant because is small than the normal Prim Prim inverse the same was it to be. Actually, no, see the norm Pi Pi Prime was adapted to P Prime was adapted to um, uh, absolute value prime, it wasn't adapted to absolute value, so it's not adapted, so this this n here wasn't such that it satisfies that property with respect to.

So how did we built this n from this absolute value here for which Pi had this property that Norm? So we had in other words, we had the norm of Pi inverse equals Norm of Pi inverse here, right, and then we built this n using this so that the the norm of every element would be bounded by the norm prescribed here, that implies that n of Pi has to be this fixed value, but it doesn't imply anything about n of Pi Prime because Pi Prime doesn't NE Pi Prime doesn't necessarily satisfy this property for this Norm, it only satisfies it for this Norm always.

Okay, thanks.

24. Outlook (Scholze)

https://www.youtube.com/watch?v=YKw1XaueLJY&list=PLx5f8IelFRgGmu6gmL-Kf_R1_6Mm7juZO

Unfinished starting from 0:00

Okay, so welcome to the last lecture. Today, I want to give some kind of outlook. With Dustin's lecture on Wednesday, we kind of finished what we promised in the first lecture. So today, I want to talk about some directions one could go in with the kind of machinery we developed.

Some years ago, I did a lot of pic geometry, and I always wanted to have a way to do this not just periodically but also with real numbers and over spy. But it was always clear to me that I really needed a completely new language to talk about these things. As I said already in my first lecture, this is the reason that I was really putting a lot of effort into this project. Finally, I have the feeling that we basically have now the language that we always wanted, and then now, it is a sensible question to just try to really use it to do a lot of things.

It appeared to me that there was this original goal that we maybe had in mind for what the series should do, but on the other, it's also good to look in other areas of mathematics to see how the theory might be useful. I'm not really competent, but I still want to give some vague ideas that I think might be worth looking at.

Okay, so here are some possible directions, and I will start from the most well-developed to the most speculative. First, we do have now a general theory of analytic sheaves, not just conditions imposed on the modules in analytic geometry in all flavors of analytic geometry. This unified theory is not just a formal thing but a full six functor formalism of six functor Street, which lets you play a lot. In particular, some things we kind of looked at using this formalism is that it's actually a non-trivial application of this general theory of sheaves without any Mysteron or otherwise hypothesis. For example, even for B spaces of finite type over the integers, we had to really work a lot to define things, but in our formalism, they just come with the structure, period.

There are also all sorts of Gaga theorems that you can reprove, but you can also prove various new sorts of Gaga theorems. There are various results about the sands of vector bundles, and for example, there's this famous paper of Greenfeld about infinite-dimensional vector bundles, where he proves some nice results, and I think our techniques could be useful for proving yet another variant of that kind of result.

We also know complex geometry, and we discussed things like off for complex manifolds, and such things are kind of one kind of approach using this formalism. One thing which we in some sense still haven't quite figured out but are quite optimistic that in principle could be done is to prove the Sing index theorem using our technology.

Related to these last points, they are, of course, very closely related to the notion of CAS theory. And one thing that was kind of missing for a while is the notion of the CAS of analytic spaces, which is defined first by Toal and then for general schemes by Thomason, and it really uses that you have a well-behaved category of coherent sheaves on. It and then maybe actually stable. Infinity category of per complexes and then you can define the case theory of that.

But going to analytic geometry, there was the issue that there is not a good enough category of modules of which you could then take, apply these categorical techniques and get some kind of case theory.

You can actually do it; you actually don't use exactly those CL shields as there, but some variant of nuclear modules. But this has not been analyzed to some extent, so this definitely uses the work of Sasha Eimo to define Cas of dualizable categories. And then if your analytic spaces are actually Ed space, this was worked out in the Ph.D. thesis of Andf.

We had some problems at first to define the complex numbers, but also some ideas how to do that. The Cas here might actually also help for this. There are actually also some other relations to the work of Sasim, so he has these very strong results about the category of localizing motives, proving that it's a rigid category, particularly dualizable itself. Using this, you can actually define certain refined variants of Clic hology, topological cyclology, and so on, that are actually not taking values in some kind of complete category as usually when you take as one6 points you get modules over some power series ring which are complete, but instead you can go to nuclear modules in our sense again.

Let me mention maybe that if it's okay, Dustin has a joint project with Brund, where they use some of this nuclear Cas theory to settle some like questions from homotopy theory. Oh yeah, but we figured out how to avoid it actually. Ah, okay, yeah, too bad.

I was a bit disappointed too, but all right, so that maybe where I'm kind of coming from, and where there had a lot of applications, and where really a lot of work has already been done is in the area of like thetical modation and so on.

There was a spetic hology, which was defined from former schemes, and one question that people had was how to really define it for rigid generic fibers. This then very much is related to analytic stats in our sense, so in particular, there is no what's called the analytic R st, defined by the work of Gu. It gives you a way to talk about six fun formalism on what's classically known as like dcat modules, so there's a certain completion of the ring of differential operators, giving some kind of differential operators of infinite order, defined by Aov and One SL. They suggested that when you work in analytic geometry, you should really look at these modes over these dcat modules and try to get a six formalism for that, but as usual, you run into something of function analysis issues doing so.

Using our analytic geometry, Ro G was able to write down, by specializing the six fun formul to specific St, a very general six fun formul for these dK modules.

This is related to D modules; there's also in the pic world the analog of like hi hi bundles, and some kind of Simpson correspondence, the first incarnation of which goes back to Dinger and Fings. These can also be interpreted as in terms of a stack, and so that's there so-called Analytics St.

This you can find in particular, there like, okay, maybe not yet using our technology, just essentially work of Aner and H, who recently obtained really strong results on the P correspondence.

This work on these hotate staks is also very closely related to what's known as geometric 10, which originally arose from Sensi, which is something about P go representations, and which has been used to very great effect by Lou Pun and also Ralo for applications to P points. Also, last Friday, Vin Pon gave a talk about his joint work with boxer, Kary, and G, where they proved modularity of Genus 2 curves of a Q in many cases, and some of the key technical parts of this proof is really using this kind of technology.

What this also is pointing to is really that there should be some version of theal for loc representations, in terms of some kind of geometric Lance of the center, so some version of what I did in my paper was long thought. I mentioned here something that's in some sense combining or just different the and St St, so is also from antic citation.

Thing. This is a proposal for something in this direction. I propose this by Helman.

By and by, in this direction. So, I mean this is of course maybe what my original interest is, that there was this work with SPK on consideration of local Langlands, and I would like to formulate all incarnations of local Langlands in such terms and eventually then also Global Langlands.

This is just to a large extent, I mean, at least work in progress, and this is probably already very speculative, but this is something that's very actively investigated.

Actually, a lot of progress on this was made during this house trimester that happened last summer here in Bonn, and in particular, we discussed a lot about these things. Then, at some point, we realized that now that we understand the Brau story really well and that we understand the kind of correct geometric language to phrase these things in, we can just basically one-on-one translate all the ingredients in the Brau world to the real world. So, there's a real analog of virtually everything that's happening in the Brau world.

So, there's also an analytic stack, and that's actually a funny version that is actually isomorphic to the Brau stack, as it so happens in this case. Where this is some incarnation of a real Hodge correspondence, you can also define an analytic P-space, which in this case actually maybe there's some kind of non-trivial JP. Okay, and again, there's some analytics stack that encodes the vector BCE, and it encodes some kind of periodic variations of F-structures, and so there's also an analytic stack including variations of twist structures.

Of course, this ties in very well with all the work of Simpson in this world, and then Maukie developed and really developed the Ser variations of F-structures, which are generalizations of variations of Hodge structures. There's a certain action of new1 on these things, and you want objects, and then it seems to be possible to synthesize everything and get also a formulation of real local Langlands correspondence for real Lie groups, for some kind of locally analytic representations.

Good. Can you produce the weight filtration too in the analytics stack? It's a very good question. Let me comment on this in a second. Not yet, but there's some kind of geometric Langlands on twist1, which is a kind of real analog of Brau.

I gave a talk in Muenster three months ago, where I was outlining the general form that we should take. I will give some three lectures actually in Princeton a month from now, and I'll say a bit more about how this is supposed to work.

Actually, part of this, it's maybe a small thing, but maybe not. Usually, when you talk about representations of G of R, you run into all sorts of functional analysis issues, and usually, you replace them by more algebraic notions of Banach modules by passing to the finite vectors and some compact subgroup. But then the theory somewhat less invariant because you need to choose this K, and it becomes more algebraic.

But in the real world, there's really not much of an issue of really encoding representations of a real group. We don't need those, but you have many representations which have the same Harish-Chandra modules because you can use different functions.

What we actually do is we will look at the real group. It's a real analytic, it's a group object in real analytic manifolds, and so you can do the kind of real analytic incarnation in the analytic stack. And so, this is a group object in the analytic stack, and you can take the classifying space of this. Let's say the stack to guess this complex number is not that, real. I mean, as usual, like the classifying space, there are something like representations of the group, but to realize them representations, you need to go back to the point where, and there's a functor. So, there's a projection from the point to here, and you can take either P or P upper star, and for any of these usual G-K modules, there's a canonical object in here, and then the P upper star, should I mean, this is something we...

The P should produce a minimal globalization. The P Street should produce a maximal globalization. So, what C you take, you take C with the gas. Yeah, you can take the gas as it's enough. And so, you have the.

Of course, this is closely related to some results on existence of analytic vectors in representations. It should; otherwise, you would not do, do you use such? I mean, the fact there are enough analytic vectors. Let me not try to say anything precise about this relation here, because it's something I still need to think much more about.

So, there was this question about weight structures. And I believe it's related to the following.

Now, we run into the speculative realm. Really, so for these things, I'm pretty confident that some version of this will work out. This is at this point more of a speculation, but I have a very strong belief in it.

Classically, in all sorts of questions about function analysis and complex geometry and everything, you often really need to put metrics on something. If you really want to prove the H decomposition, at some point you need to do some L2 stuff, put metrics on stuff, and so on. This is something that we cannot yet incorporate or what we cannot yet translate into our world everything that's related to metrics at this point. But I believe there is a clear way to look, namely, you should look at this extended version F that we had. I will connect this in a second to this question about weight structures.

So, we have this Berkovich space, and maybe the way to do it is WR. So, you have some kind of central point related to the real numbers, and then there was a rate for Q2, and then at the end of this, you have kind of F2, and then there was a rate for G3 at the end of the G, F3, all the other primes, and then there was also the Prim corresponding to the reals.

Usually, like in the Berkovich space, this kind of ends in the middle because you ask for a triangle inequality. But we don't have to do that, and so there is no point at infinity here. And there's this point at infinity, and our geometry kind of tells you that this point must be related to metric. And to some extent, you can already see that when you work with some fragments of it, but also by analogy here, like if you work topologically, then extending over this point precisely means that you put some kind of vector bundle over this thing, like a Z2 mod over the integral topological numbers. And so, extending over bundle places is definitely putting some metric on things, and it's very sensible to think that whatever exactly happens here, it must have something to do with putting metrics on some kind of real stuff.

Some question we have in our mind is whether there is a way to prove the Hodge decomposition, like for complex K-manifolds or something like this, using some geometry that will involve this extra point here. In this abstract language, this is kind of difficult for me to think about, but we actually have a very good analogy. Again, I mean, we can use this analogy between three and four. Periodically, if you work over this kind of part of this picture, then over this part of the Berkovich space, like over the open part, you can define a complex space in the sense called QP, locally analytic. This corresponds to the union over P, where Z is really just the analytic spectrum of the locally Euclidean function from DP to GP, so ones that are locally developable into power series expansion. And this kind of comes up very naturally in all these investigations there, and we know that this analytic space, which lives over the open part because it naturally has K coefficients, this has a canonical extension. What was the F2 point? And so, this is very much related to a theory of locally analytic power series representations, I mean, these are the coefficients of the groups of some PP, at least some fragments of which you can find somewhere in the literature.

Periodically, there is some kind of, yeah, so this canonical is actually a little bit subtle to write down, it has some divided powers. But it exists and is very important for this P story. And one thing this suggests, and which I don't yet know how to do, is that if you look at the real part of the picture, real, and then close on infinity, then... Over here again, you also have the real numbers, like as a real analytic space. So, the thing that's covered by, yeah, so there are as a real analytic manifold, and again, you locally, the thing with the functions are the real analytic functions. So, the ones that are locally developable and locally be developed to power series expansion, something that match to the base, and I mean, it match to the to the open part of the base, but like each, each ϕ over a point is this one, and it suggests that this should this should extend canonically over the function, yeah, canonically over close Point Infinity.

In a way, I don't yet know how to really think about, but this also suggests that if you have some, like I mean, if this could be done, and this would be some kind of ring object, then also this real analytic group would flips over like, oh yes, this should also should also expend.

So, here, you put, you use the $\mathcal{G}$ at all points, you use the $\mathcal{G}$ structure or use $\mathcal{L}$ for different, no, we don't. I will come to $\mathcal{L}$ structure later today for now, it's not needed for anything. So, you just use the same, the same at all points of the, yeah, so acting on this again, you have the rescaling action. So, there's a lot of different copies of the real numbers now, sorry for that. You, a raling action of the, every single $\mathcal{V}$, but but, and it maps to the $\mathcal{B}$ in in your sense, to the $\mathcal{B}$ space, I mean, to the analytic stack of the. So, this, yeah, so this, this maps to the space of norms, and it's an open Subspace of the space of norms, and over there, you have this $\mathcal{S}^{\mathcal{T}}$ which is like real of locally analytic, analytic thing, which lives up over the open part over the open Ray, but then there should be a way to canonically extent.

So, but properly speaking, you mean $\mathcal{RLA}$ cross $\mathcal{O}(1)$ right, or $\mathcal{Z}$. And so, I expect that whatever kind of group that is, in maybe representations of this have some kind of metric structure attached to them. I don't really know like that, if you want extend representation over the open part of the punct, I would expect this something to putting a metric on it, but I don't know. These are some objects that also exist, that I don't yet know.

All right, maybe let me mention, ah, I mean, also, I mean, this some like, and you can also just just try to understand like, we can look at analytics text over the space of Longs that really map to the close Point infinity and try to understand what the kind of geometry is there, and this is some very peculiar geometry where you, it's still about some kind of real complex manifolds or something like that, but you are able to localize much much more, you're able to really zoom in finely into your space, and so I think it's very interesting to try to investigate what geometry this point looks like. I can all agree, so we can zoom in and some, it's a very different.

So, I mean, you have to, for example, you have to use $\mathcal{RE}$ sphere, and then the bounded part of the $\mathcal{RE}$ sphere is actually just what seems to be just the Close unit disc, the bounded part is some kind of weird overconvergent, minimally over convergent neighborhood of the of the close unit disc. It's essentially just the $\mathcal{CL}$, so any real number that's bigger than one is an unbounded function on this thing.

So, I think it will take a while to figure out how, why do you picture the sphere? Sorry, Peter, why do you picture the sphere? I mean, actually, I mean, we were always looking at this, this normal like $\mathcal{T}^1$ right, $\mathcal{T}^1_\infty$, yeah, and and the Antincs

So, like any fine guy, I mean, we first associate just the drive category, but then we could also associate module categories over it. But then, as I was explaining last Friday, a series of present infinity n SP due to Stanage, um, and so you can just so you can do two PRL just continue forever. And I mean, so you can define n PRL on X for any X, and you cannot just define it, but again, there's some you have all the kind of six fun on it, and so you can just play with it.

The existence of this is not a speculation, but what comes next is definitely a speculation.

Okay, so I have a strange project with stars and Z where they're computing some Quantum CH Sim Varian, and I don't know what they are, but they seem to get certain power that seem to be very related to the kind of periodic structures I'm seeing. And for a long time, I'm trying to make sense of whatever they're doing.

But now, recently, I realized that I'm able to send about higher categories, and that well, the C hypothesis tells you that this kind of to fi series I should really just be certain higher categories and then try to understand which one they should be. And you can essentially say it, but it doesn't quite work.

So, here's what's called Quantum transus or something. This, I must be aware that I'm saying words that I don't understand.

Okay, so, so, so, to the extent that I understand anything, um, you have maybe start with a complete group and maybe Forlani simple or something like this, relevant. And then you fit what's called level. I mean, so there's a funny computation that if you look at maybe I don't know, let's see simply connected, so then the first interesting formology group of G is a third formology group, which I mean a simple assumption z, um, and yeah, so let's fix the level, which is just an element.

And so, here G is just considered a topological space, but then you also have kind specifying space of G. Um, I mean, g map to be Cub, so this, if you want the net from G cling space of z, um, this actually d loops it's map of groups automatically obstructing, so m from BG to B4 to the z z. Um, and then, but you can also restrict that to G as a real analytic thing.

Right, because in general, like I had to think that whenever you have a real analytic manifold, it maps to the incarnation of m, like is a condensed set kind of incarnation, and this basically what I'm using here. Uh, and so, but then there, like, okay, so then there's also like the exponential sequence analytic.

Yeah, thanks. Um, have exponential sequence, so everything's living here over, let's say, guess is complex numbers, um, and then a composite map to be to the four of GA venes because for here chology has trivial and so this actually lifts to be cubed of the analytic GM and of course the analytic GM fus.

So now, this sounds really like the enact. All right, but so, so, so, what does it mean to give a map to be cubed here? Well, this is too hard for me to think about, but um, so map to BGM, well, that's just the line model, right? So, BGM, that's just a classifying space for line bundles. Then, b square GM, uh, this is something giving you some algebra or something, so this giving you a Twist of the category of modules. And then the Cub GM, this means that you give a Twist of the category of categories is over over it.

Yeah, so this map called Alpha from BG a real GRP QM specifies a Twist of like PRLG, I some, let's call it just L or invertible, it's invertible object is a 2PR.

This, all right, okay. So, have this, uh, and you have the projection just to the point, or the point is like the guess complex numbers. And I think what people do is like, okay, so they, they want to look at like some family of G tsers like bundles with a flat connection or non-flat connection, whatever, um, and but sometimes Twisted by this Alpha. And so, I mean, this is somehow more or less governed by pulling back this L under Alpha, but then they want to integrate over the space of all G ches. So Somewhere in a three-category, so now we have one. So the question is, is this streetable, and as any expert would immediately tell you, this, there's no chance of working.

Because the space of conformal blocks, I believe it's called, you must put some holomorphicity constraint, and I'm kind of not doing that here. But I mean, in some sense, it's not so far, so, but it is true, duable, I think that's, that's, you don't need much, okay. So I didn't carefully check it, but I believe it is to dualizable, and to check that it would be S-realizable, and for S-realizability, you would need the following things:

You would need that π is kind of from proper and smooth, so the diagonal of π is proper, smooth, and the diagonal diag of P is proper, smooth. So there are six conditions to check, and only one of them fails. Okay, so more concretely, this is like the point, this one reduces to the group being proper and smooth, and this means that the inclusion of the point into this, so for all of these, you would need that they're proper, that they're smooth, any guesses for which of the six fails? Actually, the one you probably would expect the least is the smoothness of this map, the smoothness of the second, yeah, only.

Just a digression, the map to be, you said that it lifts to a map from B4Z to B3GM analytic, but you want to BGR locally analytic instead of BG, so the obstruction is something having to do with topology with coefficients in something like continuous topology, but it is in your language, so I'm not sure what it corresponds to, but it's just the étale topology of the stack, basically, the topology of the stack, but it is not true for BG itself, it lifts, or I mean, here would be the topology of kind of condensed set ST, which would actually be the étale topology of this funny ST is singular topology. So if you would go here to be for those complex numbers, you would just T the, this is a singular commod with a complex number, so this one wouldn't lift. You need to go to, the obstruction is has to do with maps to, to right? Yeah, so it's some kind of, I mean, up to this analytic, it's basically coherent, but on this classifying ST as a condensed set, the coherent topology is kind of singular topology. Ah, okay, okay, and it's because it's a compact group that it vanishes on the go to the lotic, yeah, but the compact is much more crucial for these kind of problem sols we can hear.

Okay, so this doesn't word, but in some sense, I feel like it was so close to working, so maybe you just need to tweak this, this here a little bit, and as I said in five, there's actually a canonical candidate where you kind of go to the Point Infinity, maybe that one works, very naive question, uh, doesn't word, how replacing by fiber, fiber of the extension first, and it seems weird that this should put the kind of correct holomorphicity constraint into the picture, but I think the formalism might just work out to do that, and there's also some structure you're not using with the like, like, um, like I think this map to B3GM that you build, it should really have a connection, I mean, like, I think that there, you know, could kind of be going to deLine topology and weight too.

Uh, yeah, so, so maybe this is not yet completely correct. I just want to point out that I mean, you can just play with these things, and you can hope that using if you would actually understand what you're really trying supposed to do, uh, you could write down something that would actually produce something special. So I mean, usually what you would try to do this, you would run to all sorts of issues that you always want to do functional analysis and high categories, and I mean, people manage to do a lot of things, but here you can just very naively try.

Alright, so this brings me to the last thing I want to talk about a little, and this one I actually want to go into a little bit more details, and actually, I mean, this was very fancy hyro, and now it becomes a bit more concrete again.

So this is about a theory of where fundamental proces now, it is we don't see very well, it's condensed, yes, cond, it'll resolve.

So, yeah, so The kind of theory where our situation where our series should be useful, and so I just try to see to what extent it is useful. Okay, so let's consider a fiber-like model situation that we may be interested in the following: Consider a fiber product of many M1, M2, next of let's say compact, oriented compact M, M1, M2. Feel free to assume that these Ms are closed immersions. I don't think that's relevant, but I mean, so yeah, consider, but which is of expected Dimension zero, in other words, like d M1 plus d M2. So then, if the transverse intersection turned out to be transverse, this X would just be a finite set of points, oriented points actually, and so you could count the number of intersection points.

This intersection is transverse and finite, and actually also oriented, so any point knows whether it should be counted as plus one or minus one. So you get a well-defined counted sign count of the elements of X. This is a variant under so long as you stay as intersection stays transverse. Okay, so then that's the setup. For, always a question, the question is, can you Define this sign count of X in general, I mean, without transverse intersection, purely intrinsically on X? Of course, every connected component will have a well-defined number, which is the part of the intersection number coming from this connected component.

Sure, but even these local ones you need to Define, right? The good situation, I know, mean you have two circles meeting transversely, and then this plus one, this minus one, intersection zero. But now, this might be generated, I, you might have two circles that I don't know, some situation like this where this might be, I know, this, this, might situation, it can be infinitely conic and Stu, I don't know, I mean, this might be tangent to infinite order somewhere, might be the same for, then cross, I don't know, all sorts of really funny behavior.

Okay, so if instead of compact manifolds, you have some kind of smooth projective varieties, then in this case, it's known how to do this, but also like the intersection cannot be as bad as for smooth manifolds, where the intersection, basically, as a topological space, has basically no structure whatsoever.

Okay, so let me first State Z, yes, compute the F product in our C, so in particular, X is some kind of deriv, and this time, it's actually somewhat critical to do this not over the gases real numbers, but over the liquid real numbers for some choice of id structure. This depends on a parameter that doesn't actually matter for this application. Yeah, so for everything else we did in our course, this kind of liquid structure that we once produced was very much effort, not so relevant, but here, I think it actually really is relevant for reasons I will explain in a second.

So what's the, so yeah, so we can Define kind of a notion of we can look at analytics texts in our setups that just have the property that locally, it is possible to write them such an intersection, and then, one can can only produce a virtual fundamental clause on. Okay, so but, there are some symplectic experts present at NP, in particular, Nate Botman and K Barheim, and I kind of discussed this a little bit with them already, and I want to continue those discussions. But in particular, they made me aware that, like, even in this kind of simple space, it's kind of surprising that this should work.

So, let me say why this is surprising. Let's consider the case where those M1 and M2 are Cur, there's my picture over there. And then, just assume you're in a local situation where you kind of have two things meeting at just one point locally. In the best possible scenario, this intersection is transverse, you just get a point count, I don't know, plus or minus one, depending on your orientations. I don't know, then the next worse scenario is if it's a square function, then okay, this to be zero, and if it's like a cubic function, there should be again, first one, and so on. And so, this tells you that if it's Vanishing to finite order, then basically, it's clear that we're okay, right, because we just need to remember the vanishing order of that function, and this will tell you what the this function crosses the line or it doesn't.

No, but now let's consider a situation. As you can have $\mathcal{C}^\infty$ manifolds, but there are more. Tangent to infinite order, something like $x-1, x^2$, but then you can have a very similar function which is $s(x)\cdot x$. And so what I'm claiming is that the locus where this function is zero, whatever that means, determines whether this function crosses a line or not. The claim is: the vanishing locus of these functions, just the vanishing locus, determines, I mean distinguishes, these two cases in particular, determines whether the function crosses a line or not in which category in our category.

Okay, so let me make it slightly more explicit. That, like, in the classical geometric picture, it's quite unclear what kind of structure you need to give the anything. I mean, the intersection point which knows it, it must, in some sense, must know more than all the derivatives of this function, because it will never be able to double from the derivatives, but it knows much, much less than the germ of this function. It really only knows eventually. So, classically, we would maybe try to use the whole germ of this function in order to remember whether it crossed or not.

Right, so what is the locus of say some $\mathcal{C}^\infty$ function from $\mathbf{R}$ to $\mathbf{R}$? Let's assume it really just has an isolated zero, Z. So this vanishing locus, in general, for this, is just the analytic spectrum of the $\mathcal{C}^\infty$ functions from the module generated by f, where f is this thing here. I mean, it is a liquid, special type of condensed $\mathbf{R}$, with the analytic ring structure. Analytic ring structure doesn't matter. It is a very funny one, though. I mean, if f would only manage to find out to order then, this would just be the usual polynomial algebra. Finish in, and so this is some nice algebra, but, if f vanishes to infinite order, then this is some funny non-separated thing, and so technically, you would have trouble and this with a topology, but in the condensed world, I mean, it has a natural condensed structure.

So, here's a proposition that tells you that the vanishing locus somewhat knows about this. If you have two functions, f and g, and there exists an isomorphism of liquid algebras between their vanishing loci, you mean, analytic rings with the liquid... Well, I mean, I don't need to put the analytic structure on them, because it's just induced one, so I can really just look at them as liquid algebras. Also, I mean, f is still a non-zero divisor here under this assumption I made, so this really still concentrating degree zero, just not Hausdorff. So, I don't have to say anything.

So, assume that these are isomorphic. The condensed set of $\mathcal{C}^\infty(\mathbf{R},\mathbf{R})$ means that you have to define smooth functions from $\mathbf{R}\times$ a profinite set to $\mathbf{R}$. You do it in the usual way, yes, the $\varprojlim$ construction, just completely internally in the condensed world, and it will produce the right condensed structure. Or, just remember that it's a $\mathfrak{C}$ space, topological thing, just pass to condensed after verfying those, give you the right condensed liquid thing. It doesn't change the condensed, liquid is just a condition. I mean, just say condensed.

Okay, so assume this, then, actually, f is g times u for some is an invertible function. And so, in particular, being an invertible function from $\mathbf{R}$ to $\mathbf{R}$ must either be everywhere positive or everywhere negative. And so, multiplying by such a unit cannot change whether the function crosses the line or not.

I believe like on nuclear nuclear fet spasis the liquid tensor product is doing the expected thing and so it actually is producing the C functions on the product, but it's not true for the overall G there would be some smaller thing. But concretely, what does it mean? A liquid is a set, I mean R is just the usual condensed ring, yeah, real numbers, but then you put different ring structure there which is much closer to asking that like the building blocks are some kind locally convex things. They're not quite locally convex, they're just α locally convex versus α might be slightly less than one, but you're very close to locally convex setting. And so when you form this T products, it basically allowing all locally convex combinations, and at least if you kind of have nuclear transition maps, then the precise kind of complexity you need actually it doesn't matter, one limit works okay.

So this is the deriv T of product, it would be works. So this is a non-computation that comes out right in the liquid World, and so from this you can deduce that if you take C^∞ functions from $\mathbf{R}$ to $\mathbf{R}$, that this is actually isomorphic over $\mathbf{R}$.

Uh, is a usual coordinate, yeah, so maybe isomorphism should be condensed our algebra with the coordinate in the proposition, otherwise I don't believe if you don't have the coordinate, so I mean I said that both of them have the isolated zero at zero, that's what I use, but I don't use anything more than that. Let's see what he's doing, but he's not.

All right, so let's analyzation. So let's assume we just have an abstract ASM. So for both of these, they admit unique map to the real numbers, because you can classify all the maps from $C^\infty(\mathbf{R})$ to $\mathbf{R}$, they just given by real numbers, a variation, that's some real number and only one of them, one of them f is equal to zero. So what is definitely true is that they have this unique evaluation to the real numbers, and this must commute, because in those cases.

All right, and now I have $\mathbf{R}$ joint key equal to zero, nothing to here, why is the usual coordinate. Right, I mean maybe I should $C^\infty(\mathbf{R}, \mathbf{R})$ here. And so I get there, and so where does this T go? Well, I mean, let's assume G is not just also Vanishing first order, then like I mean you have this unique map, and you also have to Unique first integral neighborhood, so this must be a function that it's not managing to first order here. Do you actually care about this? Maybe I don't care.

All right, so I have this map, but okay, I also have this compos map, and of course it has a quotient here, and if I want, I can also lift to here, right, because I just need to lift my element T from here to here and I can do that. Yeah, so you get it f is equal to G * unit up to a change of coordinate, so it's G times the change of coordinate compos a coordinate change times the unit change.

Okay, let me to this algebra here, just I mean let call this here A and this here B. So one I want to claim that this map from $\mathbf{R}$ joint key to B is a usual, there exists the unique extension which is just a condition. Okay, and uh, so is a module same, and there is a unique extension in which sense, in the sense abstract Rings or I mean this sense here, right, so it can be at most one extension, okay, saying. And I claim it exists, and for this I can, it's enough to check that it exists here, but I can classify all M from joint T to here, and they all do extend, right, okay, they do extend by just comp by comp. All right, and you really just need the extension is necessary, you need to just need to prove existence, but this map is given by some C^∞ function from $\mathbf{R}$ to $\mathbf{R}$, but then any other C^∞ function from $\mathbf{R}$ to $\mathbf{R}$ can just be evaluated At least, this property about F crossing the line is only deep crossing the line because so there is n's presentation, not saying it right, but let me make a point here. So, there are some existing theorems, a series of derived manifolds, but they proceed very differently, and in particular, they consider some kind of algebra of functions where, by definition, whenever you have a function, you can kind of compose with any C^∞ function from $\mathbf{R}$ to $\mathbf{R}$. This is some kind of data I put on the Rings, but this observation is telling you that these kind of funny derived potions that we're getting, they just have the property that whenever you have a function, you can always canonically extend to C^∞ function.

So, they acquire the structure that for any function, you can compose with a smooth function, but you don't need to put it in the beginning. Try to see if can Solage was trying to save. This is the canonical put that you use that probably, and I mean those things can be classified; these are just given by usual new function from $\mathbf{R}$ to $\mathbf{R}$. And so then, okay, so has a ization, and so maybe you have to put a reparameterization in, then just makes a similar, sorry, I want to say the same thing, it's faithful embedding.

H, so this just pullback by right, so yeah, okay, maybe up, okay, so right, but so some of the thing that makes this work is precisely this thing that you do get these unique extensions to C^∞ function, because otherwise, you cannot control the kind of C^∞ from here to the C^∞ section from here, you wouldn't be able to compare.

Alright, so let me now state some more objects. Let's say, as locally compact Hausdorff space, and let's assume also it's a five-dimensional, which is always satisfied when it's intersection-like that, plus a cheve of animated all together. So, that is locally isomorphic, but not with a given data, so this is just a condition, intersection.

Then, one can define a family, depending on S, of smooth manifolds, or just constant intersection of smooth manifolds. The intersection of smooth manifolds is constant locally or is it varying continuously? No, I mean locally, you can write open subsets of S can be written as the fin where the that's so coming from the point, yeah, ah okay, S itself is okay, S, S, this is okay. Then, one can define an expected Dimension, some C, it's a locally constant function, this integer coefficient, and an orientation, Ω_s, so this is a zero persistent on, right?

And, okay, so let me denote by π from S to $\star$ just the projection of locally contact po spaces and virtual fundamental T of like S equ structure sheet Al, also this depends on S, all of these depend on S, which is a global section on S of the dualizing complex. This makes sense, yes, this makes a lot of sense, and is because in particular, if the expected dimension is zero, as is compact, and choose an orientation, oh my God, fix, I mean, assume exist and fixed one, then you can define the Fundamental class, Global fundamental class, as some of the integral of the virtual fundamental class, Global, I know the count, signed count of SOS as the integral of the virtual fundamental class over S.

Okay, so I mean under G equal to zero in this orientation, this goes away, and then you just, and as compact, you have a trace map that goes back to the integers. Alright, so let me sketch the construction, and so basically, this is just the direct analog of the algebraic geometric construction. I think this was pioneered in the work of, and, then many people worked on this, and I'm not an expert on this at all. The paper where I learned it from, some kind of algebra, I mean, lination I know by a, but the ideas, I think, will maybe.

So, what they define first of all is this, what they, I think, called the intrinsic normal cone, and so this realizes.

That describes some of the fibers. Some of this corresponds to the cotangent complex. Of like over R, in our world, I mean, first of all, you can somehow define such a derived intersection of like smooth things. But also, if you compute what this kind of cotangent complex does, that can like abstractly be defined for C^∞ functions, basically for the same reason that the T-products came out right. The Čech complex also comes out right for C^∞ functions on a manifold. And then, for such a derived T-part, of course, you get an S. So this is actually a perfect complex.

What's the superscript on the L? Here, some kind, so actually I just want to define this here as a locally compact space. Let's say it's a condensed, some kind of stacky version, where some tangent directions give you stack directions. I mean, it's not really, you don't need to go all the way only to group, you don't need high, I don't need high.

For the L itself, it is concentrated in the okay, okay, yeah, sorry, perfect amplitude, amplitude 01. So, the already did it. Okay, for a compact space or a pro-finite set, if you want, T. Well, first of all, from T to S, you can just regard this as a map from the space of continuous functions from T to R to some, let me call it, unexpected SOS. So that you can, by locally the spectrum of this algebra, you can build an analytic stack in all sense. And then, if you evaluate this on just the continuous functions, you just get maps from T to the underlying set. But now, I want to define what are the maps from T to this intrinsic pH.

This can actually be defined as a map from a funny algebra. You take this guy, and you model it by zero. In other words, this is a tensor product of the continuous functions from T to R, over R, join Epsilon, where the degree of Epsilon is equal to 1 and some particular square to zero. But where the structure shift comes in, okay, it's so, here it doesn't really matter because you always factor over the continuous functions, but here's the R-structure that is focusing that to something. There is, okay, so in the first thing, you can replace it by the kind zero, and even by the homotopy class, because here, not, and just by the, you can analyze what does it take to lift the map from here to here, and it's just something in terms of the cotangent complex. And so, you can realize that the fibers here have some kind of geometric incarnation. You also have the cotangent complex, so all the tangent directions, they give you kind of stacky directions. And, yeah, and if there's some kind of actual obstruction, this is the non-smooth part, and there's actually also some vector bundle directions.

Yeah, some particular kind of smooth map, or can understand it. And so, and yeah, right, so there's a Čech complex, and like the perfect complex, so it has a kind of dimension, which is the difference between the vector bundle dimensions. And so, this already gives you the expected dimension. Just look at the rank of this guy. And this kind of situation here, this geometry here, will also produce the orientation data for you. Think about it.

So, actually, what you really need to produce is the global section, an element in this intrinsic normal cor, with values in the data you really need to produce. And now, there's this really funny thing that let's take R as a condensed set here down here, and then one can write down something which everywhere except at zero, it's just a point, but in this F at zero, you get this funny fullness. There's some kind of condensed form which lives over the real numbers and which everywhere except at zero is just a point, but then as you degenerate to the point, suddenly, there's very sticky thing, and you see this condensed thing here.

Corresponds to, but first of all, let me use G for safety because I'm not sure if I've used F before. Corresponds to a map from, like this should map to R. So, first of all, correspond to a continuous map from T to R.

But once you have that, you can look at maps from the continuous function from T2 R modul G, where, of course, if G is the zero maps, then this is just recovering what I told you before. If G is non-zero, well, then if equ equ by an invertible function, this is just zero, so you're not getting this is no data. So, you guess just get a point, but then if G is a function that's somewhere invertible, somewhere zero, then suddenly you have this kind of thing where is producing some kind of doing these things together.

And now, it's just some simple game with six fun just to produce a class because, basically, generically, you just have a canonical section here, it's just a point, and then you just degenerate that section.

Let me try to do it. So, consider the sheath F which is form. So, this guy here is streetable, and that's the only way I currently know how to check it is to use it. I assume that a der section of smth manifolds, and for manifolds, it's true, and then it's disable on fiber products. In principle, you could probably produce this canonic virtual fundamental class with much small assumptions. The only thing you really need to that this map here isable.

But then, you can Define this thing which is a sheet on followe, and then, okay, so you have an open subset J which is the cone where from zero, and then you have I which is exclusion from the cone, and then you have some excision sequence for F. Here, you have which is just I the of the p stre, and then, in particular, you get a boundary map here, and the boundary map just gives you the CLA you want. And that's always a degree shift, I'm getting confused about, but it did work out.

Because the ididentification the I F when you, yeah's a shift by one appearing here because I'm pulled back from, uh, yeah, so there's a shift by plus or minus one here, minus one because I take Z here on the real line and not pulled back from the point. And so, then you get a m from the X zero of R Z Z towards the H, and okay, this, and then you take a class here which is zero on one connected component and one on the other, and the image is what you're looking for.

By the way, is it true that this pie cone def is not just triable but smooth? No, I don't think so. I would have guessed okay, I don't think so. I so this cone of s, this is some crazy thing right? I mean, it's some kind of fiberwise, it's some kind of vector bule like thingy, but over this crazy s, I mean, this s is completely nasty in general. So, if this map was smooth and also like the F was smooth, but this has just no way of being smooth, I don't think.

Okay, so I don't think this thing that appears here is in vertical at all, but you can still can still degenerate the topological fundamental class on a point towards this cone using a little bit of six fun, and then, yeah, this Con difference from the original space just by some kind of fiber BS, which you can analyze and which give you both the dimension shift and the orientation I'm over time.

I want, I don't know if it obstruction. The well, the TR obstruction series isn't that just that there this quention complex that behaves right? Okay, that's just completely infil this six. Any further questions? Right. So, it stays a triangle on *St*. Okay, okay, okay. Okay, thanks, Peter. Yeah, all right.